JOÃO MARCOS

LOGICS OF FORMAL INCONSISTENCY

PhD Thesis

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LOGICS OF FORMAL INCONSISTENCY

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Ad parentes meos,

sine qui

non

But we never admitted the birth of logic among us.

Oswald de Andrade,
'Anthropophagite Manifesto',
May 1928.

Contents

Resumo

Segundo a pressuposição de consistência clássica, as contradições têm um cará[c]ter explosivo: uma vez que estejam presentes em uma teoria, tudo vale, e nenhum raciocínio sensato pode então ter lugar. Uma lógica é paraconsistente se ela rejeita uma tal pressuposição, e aceita ao invés que algumas teorias inconsistentes conquanto não-triviais façam perfeito sentido. As Lógicas da Inconsistência Formal, **LIF**s, formam uma classe de lógicas paraconsistentes particularmente expressivas nas quais a noção meta-teórica de consistência pode ser internalizada ao nível da linguagem obje[c]to. Como consequência, as **LIF**s são capazes de recapturar o raciocínio consistente pelo acréscimo de assunções de consistência apropriadas. Assim, por exemplo, enquanto regras clássicas tais como o silogismo disjuntivo (de A e $\langle$não-$A\rangle$-ou-B, infira B) estão fadadas a falhar numa lógica paraconsistente (pois A e $\langle$não-$A\rangle$ poderiam ambas ser verdadeiras para algum A, independentemente de B), elas podem ser recuperadas por uma **LIF** se o conjunto das premissas for ampliado pela presunção de que estamos raciocinando em um ambiente consistente (neste caso, pelo acréscimo de $\langle$consistente-$A\rangle$ como uma hipótese adicional da regra).

A presente monografia introduz as **LIF**s e apresenta diversas ilustrações destas lógicas e de suas propriedades, mostrando que tais lógicas constituem com efeito a maior parte dos sistemas paraconsistentes da literatura. Diversas formas de se efe[c]tuar a recaptura do raciocínio consistente dentro de tais sistemas inconsistentes são também ilustradas. Em cada caso, interpretações em termos de semânticas polivalentes, de traduções possíveis ou modais são fornecidas, e os problemas relacionados à provisão de contrapartidas algébricas para tais lógicas são examinados. Uma abordagem formal abstra[c]ta é proposta para todas as definições relacionadas e uma extensa investigação é feita sobre os princípios lógicos e as propriedades positivas e negativas da negação.

Palavras-chave: Lógica Universal, negação, paraconsistência, semânticas de traduções possíveis, modalidades, filosofia formal.

Abstract

According to the classical consistency presupposition, contradictions have an explosive character: Whenever they are present in a theory, anything goes, and no sensible reasoning can thus take place. A logic is paraconsistent if it disallows such presupposition, and allows instead for some inconsistent yet non-trivial theories to make perfect sense.
The Logics of Formal Inconsistency, **LFI**s, form a particularly expressive class of paraconsistent logics in which the metatheoretical notion of consistency can be internalized at the object-language level. As a consequence, the **LFI**s are able to recapture consistent reasoning by the addition of appropriate consistency assumptions. So, for instance, while classical rules such as disjunctive syllogism (from A and $\langle$not-$A\rangle$-or-B, infer B) are bound to fail in a paraconsistent logic (because A and $\langle$not-$A\rangle$ could both be true for some A, independently of B), they can be recovered by an **LFI** if the set of premises is enlarged by the presumption that we are reasoning in a consistent environment (in this case, by the addition of $\langle$consistent-$A\rangle$ as an extra hypothesis of the rule).

The present monograph introduces the **LFI**s and provides several illustrations of them and of their properties, showing that such logics constitute in fact the majority of interesting paraconsistent systems from the literature. Several ways of performing the recapture of consistent reasoning inside such inconsistent systems are also illustrated. In each case, interpretations in terms of many-valued, possible-translations, or modal semantics are provided, and the problems related to providing algebraic counterparts to such logics are surveyed. A formal abstract approach is proposed to all related definitions and an extended investigation is carried out into the logical principles and the positive and negative properties of negation.

Keywords: Universal Logic, negation, paraconsistency, possible-translations semantics, modalities, formal philosophy.

Introdução

> Deveis considerar principiada contra os índios antropófagos uma guerra offensiva que continuareis sempre em todos os annos nas estações seccas e que não terá fim.
> —Carta Régia de D. João VI, 13 de Maio de 1808.

Há cerca de 40 anos, uma abordagem lógica notável à pacificação da bravia noção de inconsistência foi inaugurada pelo paranaense Newton Carneiro Affonso da Costa. A presente monografia comemora esta empreitada ao atualizar e estender alguns aspectos escolhidos da abordagem dacostiana, centrados na faculdade de assegurar o comportamento clássico de algumas asserções feitas no interior de ambientes paraconsistentes. Mais especificamente, este documento investigará uma ampla classe de lógicas paraconsistentes inspiradas pelo trabalho de da Costa, a classe das *Lógicas da Inconsistência Formal*, **LIF**s, cuja característica fundamental consiste na capacidade que possuem de internalizar uma certa noção de consistência ao nível da linguagem objeto. Como será visto, tal capacidade expressiva abre como consequência a possibilidade de se efetuar a recaptura completa do raciocínio consistente a partir de uma **LIF** —uma lógica que, por concepção, falha necessariamente a pressuposição da consistência clássica (ou, equivalentemente, intuicionista).

A tese trata dos fundamentos teóricos da lógica paraconsistente em geral, e das **LIF**s em particular. Ela se compõe de prolegômenos e 8 artigos divididos em 4 capítulos, sobre os quais discorrerei brevemente a seguir. Cada um destes capítulos vem precedido de um resumo em português e um texto explicativo que situa os resultados dos artigos aí apresentados dentro da perspectiva geral da monografia, esclarece pontos relacionados, e relata concisamente a história da redação e da apresentação pública destes artigos.

O Capítulo **1** traça o mapa do território paraconsistente em larga escala. Tendo surgido a partir de uma proposta muito ambiciosa de entender bem o passado para poder reescrever o futuro da lógica paraconsistente *made in Brazil*, posso dizer talvez que a nossa abordagem já conta ao menos alguns sucessos, o menor das quais não terá sido a conquista de novos adeptos, como será anotado logo adiante, para um certo modo de se fazer lógica com uma motivação semântica precisa e um olho na formulação abstrata das estruturas e princípios com que se está a trabalhar, sem ao mesmo tempo fugir muito dos formatos sintáticos já tradicionais. O artigo **1.0**:

> Walter A. Carnielli and João Marcos. A taxonomy of **C**-systems. In W. A. Carnielli, M. E. Coniglio, and I. M. L. D'Ottaviano, editors. *Paraconsistency: The Logical Way to the Inconsistent,* Proceedings of the II World Congress on Paraconsistency, held in Juquehy, BR, May 8–12, 2000, volume 228 of *Lecture Notes in Pure and Applied Mathematics*. Marcel Dekker, 2002, pages 1–94. Preprint available at: `http://www.cle.unicamp.br/e-prints/abstract_5.htm`.

se trata do único artigo em co-autoria desta tese. A noção precisa de consistência com a qual pretendo trabalhar é ali cuidadosamente introduzida, e as definições precisas de **LIF**s, **C**-sistemas e **dC**-sistemas são apresentadas neste artigo pela primeira vez. Os princípios lógicos relevantes à nossa abordagem são ali estudados de um ponto de vista abstrato, e diversas formas de explosão são ilustradas. Novos e velhos cálculos paraconsistentes são exibidos como exemplos de lógicas de cada uma das supra-mencionadas classes, e um diligente levantamento é feito da literatura relacionada. Problemas ligados à frequente falha da propriedade de substitutividade em nossas lógicas são comentados e novos resultados são formulados a respeito deste problema, algumas vezes estendendo resultados anteriores, de outros autores. Uma classe de **C**-sistemas que são maximais com relação à lógica clássica é submetida à apreciação do leitor, e os problemas relacionados à algebrização dos **C**-sistemas e **dC**-sistemas estudados neste artigo são recordados ou mesmo generalizados. Tivemos a sorte de ter ótimos leitores e comentadores. Nem por isso nos livramos, contudo, dos erros técnicos e conceituais. Uma **Errata** contendo uma lista dos apontamentos que colhi nos últimos dois anos, desde a publicação de **1.0**, é também apresentada aqui, no fechamento deste primeiro capítulo da tese.

Na minha dissertação de mestrado estudei a aplicação de uma certa técnica semântica que possibilitava o uso de um conjunto de cenários para a interpretação de lógicas mais recalcitrantes. Os exemplos que ali estudei eram quase todos, como agora sabemos, amostras de **dC**-sistemas. No Capítulo **2** desta tese retomo o tema para mostrar como aquela abordagem continua viva e pode se aplicar a diversas outras lógicas paraconsistentes. O artigo **2.1**:

> João Marcos. Possible-translations semantics. In W. A. Carnielli, F. M. Dionísio, and P. Mateus, editors. *Proceedings of the Workshop on Combination of Logics: Theory and applications* (CombLog'04), held in Lisbon, PT, 28–30 July 2004. Departamento de Matemática, Instituto Superior Técnico, 2004, pages 119–128. Extended version available at:
> `http://wslc.math.ist.utl.pt/ftp/pub/MarcosJ/04-M-pts.pdf`.

foi publicado como resumo estendido no simpósio acima referido. A versão do artigo incluída nesta tese estende este resumo ao corrigir algumas de suas imprecisões e acrescentar as demonstrações de todos os seus teoremas. Trata-se aqui de definições novas e muito abrangentes de estruturas de traduções possíveis, como um estudo em Lógica Universal. Usam-se como arcabouços conceituais tanto lógicas com conclusão simples quanto lógicas com conclusões múltiplas. Estas últimas aparecem aqui pela primeira vez, no contexto desta tese, e provarão ser muito úteis em capítulos posteriores. O artigo **2.2**:

> João Marcos. Possible-translations semantics for some weak classically-based paraconsistent logics. Research report, CLC, Department of Mathematics, Instituto Superior Técnico, 1049-001 Lisbon, PT, 2004. Submitted for publication. Preprint available at:
> `http://wslc.math.ist.utl.pt/ftp/pub/MarcosJ/04-M-PTS4swcbPL.pdf`.

nasceu como um relatório de investigação escrito para registrar idéias e resultados para o uso de colegas, e a versão mais recente deste relatório se encontra presentemente submetida a publicação em um periódico internacional. Neste artigo, semânticas de traduções possíveis são oferecidas para uma coleção de lógicas paraconsistentes dedutivamente bem débeis, dentre as quais certas **LIF**s fundamentais baseadas na lógica clássica. Deve-se notar que este artigo é também o primeiro a oferecer uma axiomatização cuidadosa (e infinitária) para a lógica **mCi**, que se encontra na base de quase todas as **LIF**s apresentadas no capítulo anterior.

Muito se discutiu na literatura sobre o problema de se encontrar lógicas paraconsistentes nas quais valha a propriedade da substitutividade, e bastante se debateu também sobre as relações entre as lógicas paraconsistentes e as lógicas modais. O Capítulo **3** desta tese identifica os dois problemas (todas as lógicas modais usuais satisfazem a propriedade da substitutividade) e investiga **LIF**s que possuem semânticas modais. O artigo **3.1**:

> João Marcos. Logics of essence and accident. Research report, CLC, Department of Mathematics, Instituto Superior Técnico, 1049-001 Lisbon, PT, 2004. *Bulletin of the Section of Logic*, 2005. In print. Preprint available at:
> `http://wslc.math.ist.utl.pt/ftp/pub/MarcosJ/04-M-LEA.pdf`.

considera interpretações modais para os conectivos de consistência e de inconsistência, independentemente da presença de um operador de negação paraconsistente. Como resultado, uma nova leitura metafísica parece se impor para os conectivos anteriores, enquanto modalidades assertóricas de essência e de acidente. Alguns resultados de caracterização da débil linguagem modal relacionada são estudados neste artigo. No artigo seguinte, **3.2**:

> João Marcos. Modality and paraconsistency. Research report, CLC, Department of Mathematics, Instituto Superior Técnico, 1049-001 Lisbon, PT, 2004. In M. Bilkova and L. Behounek, editors. *The Logica Yearbook 2004*, Proceedings of the XVIII International Symposium promoted by the Institute of Philosophy of the Academy of Sciences of the Czech Republic. Filosofia, Prague, 2005. Preprint available at:
> `http://wslc.math.ist.utl.pt/ftp/pub/MarcosJ/04-M-ModPar.pdf`.

a linguagem modal 'inteira' é considerada, contendo não apenas conectivos de (in)consistência mas também interpretações modais para a negação paraconsistente em termos de 'não-necessidade'. O principal resultado deste artigo diz respeito à logica **D2** de Jaśkowski, que já fora caracterizada como um **dC**-sistema, na *Errata* ao primeiro capítulo da tese. Aqui ficamos sabendo que **D2** *não* é uma lógica modal usual tal como se poderia imaginar a partir da literatura relacionada: esta lógica não satisfaz a propriedade da substitutividade. Finalmente, no artigo **3.3**:

> João Marcos. Nearly every normal modal logic is paranormal. Research report, CLC, Department of Mathematics, Instituto Superior Técnico, 1049-001 Lisbon, PT, 2004. Submitted for publication. Preprint available at:
> `http://wslc.math.ist.utl.pt/ftp/pub/MarcosJ/04-M-Paranormal.pdf`.

aprendemos que *toda* lógica modal não-degenerada pode muito naturalmente ser caracterizada como um **dC**-sistema. Um cuidadoso levantamento da literatura relacionada é avançado, e uma alternativa simplificada é proposta para as restrições *ad hoc* que caracterizam algumas reconstruções modernas do quadrado das oposições aristotélico, substitutindo a relação de subalternação pela relação de dualidade. Mais um artigo baseado na noção mais simétrica de relação de consequência com (premissas e) conclusões múltiplas, **3.3** mostra como a noção de paracompletude surge como uma dual muito natural à noção de paraconsistência, e as **LUF**s despontam como duais às **LIF**s. Ainda mais importante do que isto é a caracterização intuitiva e mais ou menos informal oferecida neste artigo para o dito *Atributo Fundamental das* **LIF***s*, a propriedade que permite que estas lógicas recapturem o raciocínio consistente, mesmo em vista de seu desrespeito à pressuposição de consistência clássica.

Encerrados os dois últimos capítulos sobre semânticas para **LIF**s, o capítulo final desta tese começa por retomar a abordagem mais abstrata da Lógica Universal. O artigo **4.1**:

> João Marcos. On negation: Pure local rules. *Journal of Applied Logic*, 2005. In print. Preprint available at:
> `http://www.cle.unicamp.br/e-prints/revised-version-vol_4,n_4,2004.html`.

mais uma vez faz uso das conclusões múltiplas, desta vez para estudar várias propriedades positivas e negativas da negação, e suas inter-relações. Ainda outra vez é feito o levantamento da literatura relacionada, e vários deslizes de outros autores são apontados. A dualidade tem aqui um papel importante, até mesmo para a definição de várias regras aparentemente desconhecidas como duais a regras bem conhecidas da literatura. A proposta mais original deste capítulo, no entanto, diz respeito à própria definição de lógica e de constantes lógicas, definição esta que é aproximada aqui a partir de um prisma negativo, em aberto contraste com as abordagens usuais que pretendem caracterizar quais regras são comuns a todas as lógicas ou a todas as constantes de uma certa família. Finalmente, **4.2**:

> João Marcos. Ineffable inconsistencies. In J.-Y. Béziau and W. A. Carnielli, editors. Proceedings of the III World Congress on Paraconsistency. North-Holland, 2005. Preprint available at: `http://wslc.math.ist.utl.pt/ftp/pub/MarcosJ/04-M-ii.pdf`.

é o último artigo da tese. Aqui eu mostro mais uma vez como a escolha do arcabouço conceitual pode fazer toda a diferença. Usando conclusão simples pode-se dar uma receita para construir constrangedoras versões inconsistentes e 'paraconsistentes' de lógicas absolutamente usuais sem causar-lhes no entanto grande violência a partir do ponto de vista de suas relações inferenciais. O paradoxo é desmascarado se usamos conclusões múltiplas. Isto mostra, de um ponto de vista abstrato e semântico, como é preciso cuidar, por exemplo, para não acabarmos com exemplos improfícuos de lógicas e de lógicas paraconsistentes em mãos, imaginando que estamos fazendo algum progresso.

Um sinal a mais de maturidade da presente abordagem —na minha certamente tendenciosa opinião— será dado pela publicação em 2005, em colaboração com Walter Carnielli e Marcelo Coniglio, de um artigo intitulado 'Logics of Formal Inconsistency' como um capítulo da segunda edição do celebrado *Handbook of Philosophical Logic*. Eis um primeiro fruto apurado do trabalho desta tese. Esperemos por mais.

A proposta dacostiana para a lógica paraconsistente é tão rica e rebuscada que não seria nada menos do que uma temeridade imaginar que eu poderia tratar aqui de todos os seus aspectos. Na realidade, pouco ou nada será dito nesta tese sobre teorias de conjuntos paraconsistentes, sobre as relações entre paraconsistência, ontologia e pluralismo lógico, sobre a relevância filosófica da existência de uma infinidade de lógicas paraconsistentes 'puras' ou sobre os critérios de escolha que poderíamos empregar com o fim de escolher uma dentre tais lógicas para uma aplicação específica, sobre os detalhes das importantes demonstrações de maximalidade, ou mesmo sobre versões predicativas das lógicas aqui estudadas. Como explicarei mais adiante, já estudei alguns destes aspectos em outros artigos que aqui não foram incluídos; a outros aspectos ainda não pude contribuir, por incompetência ou desinteresse. Assim, ao invés de perseguir uma contribuição abrangente ou quiçá exaustiva à paraconsistência feita à moda da casa, apesar da diversidade e do alcance dos artigos contidos neste trabalho, deve-se ter em vista que me dedico aqui obsessivamente a estudar apenas um único aspecto destes desenvolvimentos, a saber, a possibilidade de expressão formal de uma noção específica de consistência que é capaz de recuperar o caráter explosivo de uma contradição, permitindo a expressão limitada de antinomias clássicas. Há vantagens em se poder contar com uma lógica paraconsistente assim expressiva. Haverá também desvantagens. Não chego sequer a propor aqui que a classe das **LIF**s seja de algum modo superior à classe das não-**LIF**s. O que se deve notar, de uma maneira ou de outra, é que estas duas classes podem ser objetivamente separadas, do ponto de vista técnico, e elas se diferenciam em geral por uma característica semântica muito precisa, a saber, a impossibilidade, em uma **LIF**, de se obter um modelo para todas as sentenças (e para todas as contradições) de uma determinada linguagem. Os paradoxos que ocasionam a trivialização a partir do uso de propriedades explosivas da negação dependem da pressuposição de consistência clássica. A opção entre uma **LIF** e uma lógica paraconsistente menos expressiva, para uma determinada aplicação, passará assim pela avaliação do quanto se deseja permitir a recuperação do raciocínio e da matemática 'clássicas', e do quanto se está disposto a ensejar (e arriscar) a recaptura da própria noção de consistência.

Prolegomena to Any Future Paraconsistency

Some 40 years ago, a remarkable logical approach to the taming of inconsistencies was pioneered by Newton Carneiro Affonso da Costa, in Brazil. The present monograph commemorates this endeavor by updating and extending some chosen aspects of the daCostian approach, centered around the possibility of securing the classical behavior of some assertions made inside a paraconsistent environment.

The birth of paraconsistent logic

> Ai, ai, ai, ai
> Have you ever danced in the tropics?
> With that hazy lazy
> Like, kind of crazy
> Like South American Way
> —Al Dubin e Jimmy McHugh, *South American Way*, 1930s.

Time often helps us separate the wheat from the chaff. With some luck, inconsequent ideas eventually are abandoned and forgotten. It would be a pity, though, that an important approach to a variety of non-classical logics, and one that is so close to our hearts and minds, would end up remembered only for the wrong reasons. An everlasting myth perpetuated by a considerable parcel of the literature on paraconsistency concerns the alleged origin of paraconsistent logic nearby Curitiba, Paraná, to wit, somewhere in between the pinelands and the sea of Southern Brazil. This section aims at debunking that myth, if only for the sake of intellectual honesty in the practice of the 'science of logic'.

Let's initially consider here two expository papers by da Costa and collaborators, namely [43] (1995) and [42] (1999). In [42], for instance, one can find the following assertion [here in my translation]:

> In fact, the first logician to have built paraconsistent systems having a full scope (propositional logic, predicate logic, set theory) is N. C. A. da Costa (cf. [35], [36]).

In a similar vein, [43] mentions, right from the start, "the creation of paraconsistent logic by the first author of the present paper [da Costa], more than thirty years ago", as having shown that it is "possible to develop a logic in which contradictions can be mastered, in which there are inoffensive or, at least, not dangerous contradictions".

In reality, the paper [43] proposes to tell us the history of the 'invention of paraconsistent logic', and to that effect it mentions the 'forerunners' of paraconsistent logic (according to the paper: Łukasiewicz, Vasiliev, Jaśkowski, Nelson, Smiley, but not Orlov), setting them at 'a great distance' from the 'discoverer' of paraconsistent logic (according to the paper: Newton da Costa). In the particular case of Vasiliev, the paper asserts that his work "was not really understood until the seventies, when the first author [da Costa] read an abstract of a paper of his written in English, and perceived that he had the intuition of paraconsistent logic. Then he suggested that one of his students, A. I. Arruda, investigate Vasiliev's works". On what concerns Jaśkowski, the title of 'discoverer' is denied in that same paper because he "has not constructed any discussive logic at the quantificational level. This was done by L. Dubikajtis and the first author [da Costa] in the sixties." Nelson and Smiley are merely mentioned by name, and their works are not commented upon. In addition, a great emphasis seems to be put on the allegation that da Costa developed his paraconsistent calculi "in a completely independent way from the works of Vasiliev, Łukasiewicz and Jaśkowski. At that time, in Brazil, the works of these logicians were inaccessible to him".

I cannot help but find the above statements utterly puzzling. In his initial thesis on paraconsistent logics, da Costa closes the introduction (p.5) by writing that [in my translation]:

> Our research had its origin in studies that we have previously published (see [32, 33] and [30, 31]). But, to the best of our knowledge, very little has been done on the topic, besides certain inquiries by Jaśkowski (see [55, 56] and [65, 66]); some studies by Nelson bear some relation to the object of this thesis, though the orientation of the North-American logician is very much distinct from ours (see [68], where you will find bibliographic references).

This paragraph alone already seems to seriously impair the last contention about da Costa deserving a special merit for having been a lone researcher with no access to the work of other logicians —as he does indeed seem to have had access to all relevant papers, at some point. At any rate, from a historical perspective, why should ignorance or lack of contact with the outside world be attached anything more than a sentimental value at the moment we are assessing one's contribution to science? A more balanced partial account of matters was presented by da Costa himself in his opening address (read by Itala D'Ottaviano) at the Stanisław Jaśkowski's Memorial Symposium, held in Toruń, Poland, in 1998 (cf. [40]):

> I was delighted to notice, in the early 1960's, that the work I had developed in Brazil by that time had close connections with Jaśkowski's. I recall, as if it were today, reading the English abstract of one of his papers, and realising that the two of us were independently producing

> works of a striking similarity. I then sent him a letter, and that is how my long term contact with the Polish community of logicians started.

Many papers produced by the 'Brazilian school', influenced by da Costa, try to maintain somehow that the latter "is actually the founder of paraconsistent logic" (e.g. [3, 51]). However, to rule out any possible doubt about da Costa himself not having recognized Jaśkowski's central role on that foundation (15 years earlier than the former author), it should be noticed that the same address by da Costa, [40], states that "it was not earlier than 1948 that Stanisław Jaśkowski, under Łukasiewicz's influence, would propose the first paraconsistent propositional calculus".

At first, and second, analysis, the distinction proposed in [43] between 'forerunners' and 'discoverers' of paraconsistent logic surely seems discretionary —one could even say inconsistent with all the information brought out by other papers. What are, after all, the criteria used by the authors of [43] in order to determine which researchers belong to each class? Well, on that very respect they defend that the birth of paraconsistent logic "corresponds to its appearance, strictly speaking, as a theory, i.e., as a mathematical theory, studied in itself in a systematic way, and scientifically acknowledged", and they situate this event after da Costa's 1963 thesis. Moreover, they say that "to really constitute a logic", a system of logic "has to be developed at least to encompass quantification and equality, given the role of logic in the articulation of conceptual systems". They conclude by saying that "to this extent, the first author [da Costa] seems to have been the first logician to have done so with paraconsistent logic".

There are several immediate problems posed by the above conceptual schema. Neither 'paraconsistency' nor 'logic' itself, as enterprises that together will allow us to build 'inconsistency-tolerating' 'reasoning mechanisms', seem to depend, for their definition, on anything beyond the propositional object-language level. First-order paraconsistent logics are certainly important for many applications, but that fact alone does not obligate logic or paraconsistency to involve first-order notions from the start. Moreover, as I have argued in [63], if a separation should really be drawn among, on the one hand, those 'forerunners' of paraconsistent logic who have merely advocated for a change of attitude towards the contradictions that would be present in our theories or who have only informally described reasoning mechanisms that would deal with such contradictions, and, on the other hand, those 'founders' of paraconsistent logic who have realized that the most important task to be accomplished was that of avoiding triviality or overcompleteness and, derivatively, the task of controlling the explosion principle of classical logic, then the first class would contain people such as Vasiliev, Łukasiewicz and Wittgenstein, while the second class would contain logicians who have actually built such logic systems, such as Jaśkowski, Nelson and da Costa.

About Jaśkowski (1948 and 1949), as a matter of fact, da Costa says in [40] that "to the best of my knowledge, he was probably the first to formulate, with regard to inconsistent theories, the issues connected with non-triviality". Though da Costa rarely ever mentions nor explains the investigations done by Nelson (1959), there should be no doubt either about Nelson's insight or the importance of his work. Indeed, in [68], a paper based on developments made a decade earlier (cf. [67]), Nelson writes that:

> In both the intuitionistic and classical logic all contradictions are equivalent. This makes it impossible to consider such entities at all in mathematics. It is not clear to me that such a radical position regarding contradiction is necessary. I feel that it may be possible to conceive a logic which does more justice to the uncertainty of the empirical situation insofar as negation is concerned.

Then, after developing in some detail a nice and well-motivated first-order paraconsistent logic with equality, Nelson asserts that "the system has been constructed, of course, to show that the logical operations may be interpreted in such a way that a mathematical system may be inconsistent without being overcomplete". Moreover, not only was this paper by Nelson written in English and was widely circulated, having been related to a number of advances made by then on the study of constructivity in mathematics, but this investigation also gave rise, not so much time later, to further developments by other authors, as a well-known study done by Fitch (cf. [53]) that considers Nelson's logic as a system to overcome paradoxes or a well-known thesis by Prawitz (cf. [70]) that studies a constructive naive set theory based on Nelson's logic. There is no reason thus for the paraconsistent community to continue neglecting, by and large, Nelson's approach. A modern account of Nelson's constructive logic can be found, for instance, in Wansing's [86].

Of course, the fact that Santos-Dumont was *not* the first to invent the airplane does not in any sense diminish the many deeds of Embraer. Analogously, the fact that Newton da Costa was neither the first nor the second author to develop paraconsistent logics should not count against the many interesting intuitions and approaches promoted by the 'Brazilian school' along the years. If, on the one hand, paraconsistent logics still remain nowadays as a rather marginal variety of non-classical logics, on the other hand the papers written by Newton da Costa on the theme (several of the initial studies having been done only in Portuguese and many of them having been published in places that did not render them much visible) have never been the most accessible or the most popular ones on the field, globally speaking. Promoting the approach of the 'Brazilian school' by way of biased historical revisionism would appear to be at most irrelevant: a misleading, pointless and unnecessary strategy. It would only prove that sin *does* exist beneath the Equator, and it would not help in making that approach more accessible or popular. I tend to believe that a better job would be done if we could only stop losing time trying to guarantee a historical or conceptual priority, and concentrate instead on technical and philosophical aspects of relevance. My work in the area aims to make a contribution to this second strategy.

Semantic intuitions

> Veritas? Quid est veritas?
> —Pontius Pilate (Joannes 18:38).

One of the advantages of saying that you are a 'formalist' is that you do not really have to *understand* the things you are doing. Formalism is often misconstrued in fact as mere 'blind manipulation of symbols'. In principle, a computer could do it better than you. Furthermore, a related confusion often to be found in discussions about formalism is betrayed by assertions to the effect that it makes no sense for a formalist to talk about the 'existence' of mathematical objects. I am certainly no expert in this matter, but *both* positions seem to me to constitute serious misconstructions of David Hilbert's Metamathematics. Anyone who carefully reads Hilbert's lecture 'On the infinite' (cf. [54]) —or, for that matter, several other texts by Hilbert, close collaborators like Bernays, and competent commentators—, will see that Hilbert did *not* defend the idea that mathematical objects 'have no meaning', *neither* did he attack the idea that mathematical objects could have some sort of 'real existence'. In fact, Hilbert fully acknowledged the usual meaning of numbers in number theory and the existence of points and lines in euclidean geometry. Hilbert did worry though about the alleged lack of meaning of some 'ideal elements' such as 'complex numbers' or the 'actual infinite', in spite of how useful these ideal elements have proved to be in mathematics, and stressed the importance of heeding proof theory as a safer way of checking the 'consistency' of our theories, proceeding by finitary steps and abstracting from the meaning of the objects and of the constants of mathematics and logic. I wonder why some people maintain that Hilbert defended a doctrine any stronger than this.

As we are up to this, one should perhaps notice that Van Quine seems to have been quite content with Hilbert's formalist approach to the existence of mathematical objects, as he made just a further small step beyond when formulating his famous ontological motto for mathematics: "To be is to be the value of a variable" (cf. [78]). However, Quine's ontological strictures, as it should be clear, aim not to show that something exists, but rather to tell us about our own ontological commitments when positing our theories. (To help choosing among competing ontologies Quine just suggested that we should accept the "simplest one that fits our experience".) Taking into account the modern proliferation of logical alternatives to classical logic, Newton da Costa suggested to update Quine's slogan by substituting it for: "To be is to be the value of a variable in a particular language with a given underlying logic" (cf. [37, 47]). Moreover, as we will see, if Hilbert took it to his heart that the non-contradictoriness of a mathematical object should count as a necessary and sufficient condition for its very existence, da Costa's approach to paraconsistent logic was soon to update that guideline by substituting Hilbert's 'consistency' by a more generous logical notion, that of 'non-triviality'. But much more will be said below about non-triviality and about (paraconsistent) logic and ontology.

In contrast to the rest of the present monograph, whose main approach to logic is more formal and abstract-oriented, here I want to start by a brief reasonably informal semantic-oriented motivation for paraconsistent logic and for the Logics of Formal Inconsistency. Some definitions of consistency, inconsistency, varieties of explosion and trivialization will be hereby illustrated. Whenever necessary, I will use '$\neg$' as a symbol for unary negation.

Let's consider some convenient set of sentences $\mathcal{S}$, a primitive set of states of affairs G, and two binary predicates defined over subsets of $\mathcal{S}$ representing two notions of consequence: A local notion of consequence $\Vdash^l$ associated to each state $l \in G$, and a global notion of consequence $\Vdash^g$. Given a sentence A and some state l, say that A can be inferred in l in case $\varnothing \Vdash^l \{A\}$. Denote this alternatively by writing $\Vdash^l A$, and in case A cannot be inferred in l denote this by writing $A \Vdash^l$. Given sets of sentences Γ and Δ, say that Δ can be locally inferred from Γ in l in case $\Gamma \Vdash^l \Delta$; in what I call the canonical definition of consequence, this will hold good exactly when there is some $A \in \Gamma$ such that $A \Vdash^l$ or some $B \in \Delta$ such that $\Vdash^l B$. Set-forming braces will often be omitted so as to streamline notation. Similar definitions can be proposed for the global consequence, with the restrictions that (a) given $\Gamma \cup \Delta = \Gamma$, then $\Gamma \Vdash^l \Delta$ iff $\Gamma \Vdash^g \Delta$, and (b) $\Vdash^l \subseteq \Vdash^g$. In the canonical definition of consequence, $\Gamma \Vdash^g \Delta$ holds good exactly when $\Gamma \Vdash^l \Delta$ for every state of affairs $l \in G$. In case $\Gamma \Vdash^x A$ for every $\Gamma \subseteq \mathcal{S}$, we say, if $x = l$, that A is acceptable in l, and we say, if $x = g$, that A is a thesis of G. If a similar thing can be checked about $A \Vdash^x \Delta$, for every $\Delta \subseteq \mathcal{S}$, we say, if $x = l$, that A is refutable in l, and if $x = g$ we say that A is an antithesis of G. Assuming that the above notion of state of affairs is intended to embody intuitive notions of truth and falsehood (you could read $\Vdash^l A$ by 'A is true in l' and read $A \Vdash^l$ by 'A is false in l'), the associated notions of consequence are intended to guarantee that truth is preserved from premises to conclusions and falsehood preserved from conclusions to premises.

Let's concentrate on the canonical notion of consequence. So, when I write hereon something like $\Gamma \Vdash \Delta$, for some $\Gamma \cup \Delta \subseteq \mathcal{S}$, I will mean $\Gamma, \Sigma \Vdash \Pi, \Delta$ for every $\Sigma \cup \Pi \subseteq \mathcal{S}$. Suppose that a particular logic corresponds to each choice of sentences and of a global consequence relation, as defined above. Say that a set of sentences Σ is explosive in case $\Sigma \Vdash^g$. There are many alternate ways in which a set of sentences Σ can explode. This Σ could for instance be a singleton (what I call a bottom particle), or it could be a pair $\{A, \neg A\}$ of contradictory sentences that only explode together, not in separate, defining thus a notion of negation-explosion. Or it could also be some gentle kind of explosion that demands the presence of a larger number of sentences depending exclusively on A. Perhaps we do not have explosion with respect to contradictions made with $\neg$, but we have a supplementing form of explosion with respect to contradictions made with some other primitive or derived negation symbol. Perhaps explosion can be somewhat controlled and $\{A, \neg A\}$ explode only for sentences A of a certain format. Explosion could also be partial, in allowing one to infer not just any sentence, but at least some sentences of a certain format that are not already theses of the underlying global consequence. By definition, paraconsistent logics should fail at least the most basic form of negation-explosion.

Call a state of affairs dadaistic in case it satisfies all sentences of $\mathcal{S}$, and call it negation-inconsistent in case it satisfies some pair of sentences of the form A and $\neg A$. We say that a logic $\mathbf{L}$ is consistent in case two requirements are met: (a) $\mathbf{L}$ admits of no dadaistic state of affairs (that is, $\mathcal{S}$ is explosive), and (b) not all sentences of $\mathcal{S}$ are theses of $\mathbf{L}$. The notion of negation-consistency of a logic $\mathbf{L}$ adds to those requirements the idea that: (c) any contradictory set of sentences is explosive. Obviously, (c) implies (a). In contrast, a paraconsistent logic, though, is (negation-)inconsistent, and in principle it could admit of dadaistic states of affairs. Decent paraconsistent logics will also admit of some non-dadaistic negation-inconsistent states.

There are now many varieties of inconsistency to be considered. A logic $\mathbf{L}$ is called trivial in case any set of sentences can be inferred from any other. With the canonical notion of consequence, that can only be the case if $G = \varnothing$. In case the logic $\mathbf{L}$ is not trivial but every one of its sentences is a thesis (and so, all states of affairs are dadaistic), then $\mathbf{L}$ is called absolutely inconsistent, or dadaistic; in particular, for any given negation symbol, all contradictions are inferable as theses of such a logic. Any logic $\mathbf{L}$ that has some pair A and $\neg A$ of theses is called dialectic. Most paraconsistent logics in the literature are not dialectic. A simple form of negation-inconsistency, for a logic $\mathbf{L}$, is obtained if one admits both non-dadaistic states of affairs (escaping thus absolute inconsistency) and negation-inconsistent states (invalidating explosion). A decent form of negation-inconsistency requires the admission of states of affairs that are at the same time non-dadaistic and negation-inconsistent. An expressively inconsistent logic is decently inconsistent, but it has in common with consistent logics the requirement that no dadaistic models may be admitted. Most important paraconsistent logics are decently inconsistent, but several of them fail to be expressively inconsistent. Finally, a gently inconsistent logic is one for which, for every sentence A, there is a certain number of things that you can say about this sentence so as to make it explosive, that is, there is a minimal set $\overline{\underline{\bigcirc}}(x)$ of sentences depending only on the sentence x such that $\overline{\underline{\bigcirc}}(A)$ (or maybe $\overline{\underline{\bigcirc}}(A) \cup \{A, \neg A\}$) cannot be satisfied by any state of affairs. Logics of Formal Inconsistency constitute a variety of gently inconsistent logics. In such logics the set of sentences $\overline{\underline{\bigcirc}}(A)$ to be added to $\{A, \neg A\}$ in order to make the latter set explosive is said to express the consistency of the sentence A.

On what concerns the above notion of consistency, and to quickly go back to the theme from the beginning of this section, one seems to have now two equally good choices of approach to the Logics of Formal Inconsistency: One can either be a formalist and construe consistency as yet another 'ideal element' to be justified by the role it plays in our structures, or one can adopt instead the semantic intuition that consistency is whatever a theory might be lacking so as to become non-trivially explosive.

The Fundamental Feature of LFIs

> No fairer destiny could be allotted to any physical theory, than that it should of itself point out the way to the introduction of a more comprehensive theory, in which it lives on as a limiting case.
> —Albert Einstein, *Relativity: The Special and General Theory*, chap. 22, 1920.

Agnosticism in logic. As in Poland, the development of mathematical logic in Brazil was strongly influenced by logical positivism, the doctrine that calls *meaningless* any statement that is neither verifiable nor refutable. This intellectual stance has often been taken too far, and it has been used to disqualify a good part of philosophy as 'purely speculative'. All in all, obscure metaphysical jargon was the preferred target of positivists. As an alternative, it became popular to do something that was dubbed 'scientific philosophy', always to begin with an analysis of language, and often to proceed by the use of even more impenetrable jargon. Ray Smullyan gives a humorous definition of a logical positivist as someone who rejects as meaningless anything that *they* cannot understand (cf. [81]). He also tells the story of a lady who, despite not having any formal education as a philosopher, lived in a house full of philosophy books. When asked for the reason of that, she replied that her ex-husband was a logical positivist, and added that it was logical positivism that broke up their marriage. How come? She explained that it was simple: Whatever she said, he told her it was meaningless!

Newton da Costa has since long (cf. [41]) been a faithful espouse of scientific philosophy and a professed supporter of a certain variety of Quinean platonism (check what I wrote about this just above, and check also the note 5 in **Chapter 1.0**). The criticism of the 'standard form of platonism' that da Costa presents in [39] is determinedly directed against the 'speculative character' of a doctrine that "presupposes that mathematical objects are grasped by a material intellectual intuition", a doctrine he rejects for being 'too nonscientific'. Moreover, on the import of logic to philosophy, da Costa insists in [37] that philosophical doctrines cannot be derived directly from logic, or from geometry, or from any other scientific field. On the indirect contribution of logic to philosophy he mentions though the possibility of using logic in "the elaboration of philosophical theories" or in "showing the formal inadequacies of philosophical inquiries". As an example of the former phenomenon, he mentions Tarski's researches on the notion of truth as having shown the tenability of the theory of correspondence, and as an example of the latter phenomenon he mentions Gödel's theorems as having promoted debates on the philosophical status of the formal sciences and a revolution in the domains of proof and of axiomatization.

In spite of his sympathy for realism, however, and the consequent rejection of fictionalism and instrumentalism (cf. [41]), and in spite of the alleged indirect contributions of logic to philosophy, da Costa wants to advocate the 'philosophical neutrality' of paraconsistent logic. In [46], da Costa and

Bueno write that, just as mathematics, logic "cannot justify by itself any metaphysical or, in general, 'speculative' position" [quotation marks by the authors]. Apparently concerned in their paper with 'dialetheist' interpretations of paraconsistency (although no paper on dialetheism is explicitly mentioned), they also write that they want to "stress that one *cannot* prove that 'speculative' philosophical interpretations of paraconsistent logic *cannot be true* (though it might be also difficult to show that they are)".

Dialetheism (cf. [76]) is a doctrine according to which "there are true contradictions" (in mathematics, presumably, or in reality). But, "just as empiricists (such as van Fraassen) are agnostic about (the existence of) unobservable entities in science", da Costa aims to be "agnostic about the existence of true contradictions in nature" (cf. [40]). A reason he presents for that is the so-called 'underdetermination argument': "There are always many paraconsistent logics which can be used to accommodate a given 'phenomenon' —whether it is an 'inconsistent' reasoning or an 'inconsistent' theory" [quotation marks by the author]. For da Costa (check [39], chap. IV.3), "reason, in the sense of a set of principles, does not coincide with any system of logic" [my translation]. In fact, while reason "constitutes itself historically, in harmony with the reality that surround us", each particular logic is supposed to have its own domain of application, as "the logical system underlying each rational context is a consequence of the pragmatic principles of reason, the nature of the context and the historical and cultural factors that shape reason" (id., ibid.). In the particular case of paraconsistent logics and dialetheism, da Costa maintains that "in the present state of science, it is not known whether the universe is consistent or not, in a strict sense, that is, if there are real contradictions" (id. chap. III.5). For "logic does not have a way of deciding, by itself, if there are real contradictions in the world. These can only be verified or refuted by way of experimentation, through the scientific method". Similar arguments are raised by da Costa and Bueno, in [45], against dialetheism and the idea that there would be 'one true logic' —and one of a paraconsistent character. According to the authors: (1) dialetheists have provided no evidence that their logic is the one true logic; (2) each domain has its own appropriate logic, to be chosen with the help of heuristic and pragmatic reasons. These arguments have been criticized, though, by Tanaka (cf. [82]), according to whom the evidence *has* been exhaustively provided, and it's up to da Costa and Bueno to reject it. Moreover, the mentioned heuristic and pragmatic rationale that would allow us to choose *this* logic instead of *that* never seem to be provided by da Costa and Bueno themselves.

From my own perspective, agnosticism might well be the most convenient wager. For the purposes of the present monograph on the foundations of paraconsistent logic, to take a position about dialetheism seems entirely immaterial. In defending agnosticism with respect to the existence of true contradictions, da Costa writes (cf. [37]) that "most systems of paraconsistent logic may also be treated as mere formalisms, by means of which we are able to systematize theories or systems of theories containing contradictions. But, in this case, contradictions are not interpreted as real, but as difficulties caused by the limitations of our knowledge". The point was more fully elaborated by Diderik Batens (cf. [10]), who argues that, even if the world is consistent, we might still need paraconsistent logic to be applied to our *theories*, rather than to the *world*. Moreover, as Batens recalls, "inconsistent theories may very well be the best among the theories available at a particular point in time". Inconsistencies do not have to be 'real', but they may arise, for instance, from conflicting observational criteria, from the language and the relations we choose for describing the world (check also [8]), or from the way we construct our scientific worldview, in which case "it is usually preferable to face an inconsistency rather than to neglect one half of it". Indeed, in the case where one of the conflicting theories is ignored, "if that theory turns out to prevail, one will be forced to reorganize one's worldview in a much more drastic way —the full bet was on the wrong alternative".

For my part, I cannot see what is wrong, in fact, about speculation *per se*, as long as it produces measurable results. Speculation does not presume mysticism, nor does it antagonize science. There is nothing wrong with metaphysics, either. Metaphysical assumptions underlie, in one way or other, any undertake of ours to understand the world. Besides, that technical concepts of formal logic might be invoked in the clarification and the study of such metaphysical assumptions should not be seen as anything like an unwelcome or an unexpected intrusion (check [52]).

A lot of philosophy can be done by 'discursive' means, that is, by sewing arguments through the use of reason rather than intuition or revelation. This is not to deny any rationality for intuition, and it is not to say that one's hunches and epiphanies should be altogether ignored, but only to say that these latter forms of access to knowledge should be used with extreme care, before one can get a better grip on how they are produced, and where they are leading to. Furthermore, discursive philosophy is often done more than well with the use of informal logic, critical thinking, and a more or less informal discourse. As opposed to that, and given the sort of problems I am to tackle in this monograph, what I intend to be doing instead is a kind of 'formal philosophy'.[1] I will not answer any of the great problems of philosophy, or of metaphysics, or even of philosophical logic. Auspiciously, however, I do hope to make some advances on some of the great philosophical problems of logic. Just logic.

[1] "Formal philosophy is called logic", writes Kant in the preface of *Fundamental Principles of the Metaphysics of Morals*, 1785. The use of the term 'formal philosophy' in denoting the employment of logic and formal methods in the study of language and grammar was championed in more recent times by Richard Montague (cf. [84]). The use of this same term, as referring to the use of formal methods in philosophical contexts, seems in fact to be experiencing a revival, nowadays. For the Northern winter of 2005, for instance, an 'International Conference on Formal Philosophy' is being organized by the Danish Research School in Philosophy, History of Ideas and History of Science and the Danish Network for Philosophical Logic and Its Applications.

Recipe for a certain programme in paraconsistency. The first paraconsistent logics ever built by Newton da Costa, the calculi C_n, $1 \leq n \leq \omega$, came to life in 1963.[2] The rationale presented by da Costa for that construction (where C_0 represents classical logic), in 1974, was:

> **The Calculi** C_n. As C_n, $1 \leq n \leq \omega$, are intended to serve as bases for non-trivial inconsistent theories, it seems natural that they satisfy the following conditions: (i) In these calculi the principle of contradiction, $\neg(A\&\neg A)$, must not be a valid schema; (ii) from two contradictory formulas, A and $\neg A$ it will not in general be possible to deduce an arbitrary formula B; (iii) it must be simple to extend C_n, $1 \leq n \leq \omega$, to corresponding predicate calculi (with or without equality) of first order; (iv) C_n, $1 \leq n \leq \omega$, must contain the most part of the schemata and rules of C_0 which do not interfere with the first conditions. (Evidently, the last two conditions are vague.)

In practice, the construction of those calculi was to make use of Kleene's axiomatization of classical logic (cf. [57]), except for the axiom that guarantees *reductio ad absurdum*, a rule to be necessarily failed by paraconsistent logics (check **Chapter 4.1**). Instead of that usual axiom, in general, restricted versions of it, related to requisite (i), were considered by da Costa: Calling A° the formula $\neg(A\&\neg A)$, *reductio* was guaranteed in C_1 as soon as A° could be assumed to hold, in C_2 the same guarantee would be valid as soon as both A° and $(A^\circ)^\circ$ could be assumed to hold, and so on. The result was the definition of an increasingly weaker hierarchy of logics: $C_1 \gnsim C_2 \gnsim C_3 \gnsim \ldots$

I will return below to each of the above requisites by da Costa in some detail. For the moment, it suffices to say that, as in any (decent) paraconsistent logic, requisite (ii) is obviously going to be satisfied by the logics C_n, for $n > 0$. Moreover, as I have argued in [27], C_ω, the weakest logic in the hierarchy, is in fact an intruder, as it does not share the main metatheoretical features of the other logics. On what concerns requisite (i) and the choice of the formula $\neg(A\&\neg A)$ to represent the 'principle of contradiction', we will see that it is an ill-advised move, to say the least. Moreover, as I have maintained above, requisite (iii) goes much beyond the mere *proposal* of paraconsistent systems, but it cares instead about their *use*. Finally, requisite (iv) is simply never respected by the logics from the above mentioned hierarchy. A thousand times repeated in the literature, I will argue that the above 'natural conditions' on the construction of paraconsistent logics neither determine in any sense the logics in question (once they are somewhat ill-advised, partially disrespected, and pretty vague), nor do they mention some of the most important features of those logics: Their capacity of expressing consistency and spreading it, and the possibility they open in a paraconsistent environment for classical reasoning to be fully recaptured.

[2]Though da Costa often claims to have started to develop his own brand of paraconsistency 'from 1954 onwards' (cf. [47, 43]) or 'from 1958 on' (cf. [37]), his own criterion for determining the date of birth of paraconsistent logic, as we have seen above, would force us to ignore such dates and stay with 1963, date of his first known publications on paraconsistent logic properly speaking (cf. [35, 34]). Check also the brief historical note at the end of this **Prolegomena**.

The fetish formula, and the Principle of (Non-)Contradiction. A lot of noise is often made in the literature about paraconsistency representing the 'effective derogation of the Principle (or Law) of (Non-)Contradiction'. Da Costa himself claims that "in paraconsistent logic, the principle of contradiction, in one form or another, is qualified or limited" (cf. [40]). We have already seen above that da Costa was worried from the start about the validity of (specific forms of) this principle, and, as a matter of fact, it is easy to see that he shared this preoccupation with many people. For instance, Jean-Yves Béziau writes in [15] that, "roughly speaking, a paraconsistent logic is a logic rejecting the principle of non-contradiction", and similar statements can be found in several other papers by this author (e.g., [17, 18]). Now, that might be true, but, as I show in **Chapter 1.0**, it all depends of course on how you read that principle.[3] Should the Principle of Non-Contradiction say that "no sentence can be true together with its negation", then one would still have to clarify, for instance, what 'true' means: Is it a 'local' or a 'global' notion? Does it mean 'satisfiable' or 'valid'? In the former connotations, the principle reduces to what I here call Principle of Explosion, a direct concern of paraconsistent logics; in the latter connotations the result is a much weaker principle, and one that is respected by the great majority of known paraconsistent systems. That I have decided to use the term in its second connotations does not prove anything definitive about the relation of the 'Principle of Non-Contradiction' to the making of paraconsistent logics. What *can* be proved, though, is that neither formulation of the above principle is related, in general, to the validity of the formula $\neg(A\&\neg A)$, which I shall hereby call the 'fetish formula' of (some) paraconsistentists.

As soon as a fetish formula of the form $\neg(A\&\neg A)$ was proved, da Costa called the formula A *well-behaved* (cf. [35, 36]). As we will see, that meant in practice that the formula A 'behaved classically'. As we have seen in requisite (i) above, such formulas could not all be proved in da Costa's original **C**-systems, under pain of making these logics lose their paraconsistent behavior. Why worry about this particular formula? In [43], da Costa and his collaborators allege that "there are mathematical reasons, related to the construction of the systems, to demarcate between well-behaved and non-well-behaved propositions, and that is why the constraint on rejecting $\neg(A\&\neg A)$ as a logical truth was advanced". They continue rationalizing by pointing to the requisites of da Costa's 1974 paper, mentioned above, and by defending this as a decision with no philosophical motivations: "As opposed to any particular philosophical concerns, the main consideration underlying such a proposal consisted in presenting a logical framework in which the presence of contradictions does not lead to trivialisation, meeting thus, initially at least, a mathematical (not a philosophical) demand". Now, that comment certainly sounds intriguing, as it gives the impression

[3] The theme is still popular. A collection of papers on the 'Law of Non-Contradiction' is indeed about to be published (cf. [71]), and there you can find a few papers that discuss the many possible versions of this law.

that any such a paraconsistent 'logical framework' would have obligated one to follow that proposal of rejecting the fetish formula. But there are many paraconsistent logics for which $\neg(A\&\neg A)$ is a theorem. One of these is a 3-valued maximal paraconsistent logic studied by da Costa himself, together with D'Ottaviano, a **C**-system[4] that they dubbed $\mathbf{J}_3$ (cf. [50]).[5]

The confusion involving the fetish formula and paraconsistent logic is not systematic. Béziau has denounced, for instance, in [16] and in [19] the misidentification of the acceptance of the Principle of Non-Contradiction with the validity of the fetish formula. However, while this same author has identified, as we have seen just above, the failure of the former principle with the very definition of a paraconsistent logic, he has also very often criticized the fact that the fetish formula is a theorem of some paraconsistent logics. In [13], for instance, Béziau criticized Igor Urbas for having proposed in [85] a dual-intuitionistic logic that validated the fetish formula while solving the problem of replacement (see below): "Even if this logic is algebraizable, it has also some 'abnormalities' which are even worse, from the point of view of paraconsistency at least, the fact for example that $\neg(A\&\neg A)$ is a theorem". Calling *full* any paraconsistent logic in which the schema $\neg(A\&\neg A)$ is provable, Béziau asserts in [19] that "it is not clear at all that the idea of a full paraconsistent logic is meaningful", and he adds that the question is still open whether we can find an 'intuitive interpretation' of a paraconsistent negation with respect to which the fetish formula can be proved. All that sounds however more like vague complaints. The only technical reason ever presented by Béziau for mistrusting full paraconsistent logics, in fact, seems to be the one from the paper [14]: That the negations of full paraconsistent logics cannot at the same time validate introduction and elimination of double negation and also satisfy replacement (for a generalization of this result, check Theorem 3.51(vii) from **Chapter 1.0**). The argument only proves, of course, that these classical properties are jointly incompatible inside a paraconsistent logic: Why should one insist on having all of them, though, knowing that paraconsistent logic is bound to throw some classical properties away?

In contrast to the 'Brazilian' inconsistent and inconstant reaction to the fetish formula, one could note for instance that Jaśkowski had already called this formula 'law of contradiction', pointed out that it is a theorem of his discussive logic, and observed that, in spite of the denomination that he adopted for this formula, it "has no close relation to the problem of the logic of inconsistent systems" (cf. [55]). The present monograph will return to this theme every now and then. Check in particular the brief survey of the various reactions to the fetish formula done in subsection 3.8 of the paper TAXONOMY, in **Chapter 1.0**.

[4] At least a **C**-system, *nota bene*, according to my present definition of the term —check sections 2.4 and 2.6 of **Chapter 1.0**.

[5] It should be noticed that those authors have never proved the maximality of this logic, arguably one of its most interesting features. Such a proof can be found in [61, 29].

The replacement property. A logic is said to enjoy the replacement property whenever it allows for equivalent formulas to be freely intersubstituted everywhere. In 1965, da Costa and Guillaume pointed out the failure of that property (and, in particular, the consequent failure of the global form of the rule of contraposition) for the paraconsistent logics from the original C_n hierarchy (cf. [48]). Replacement is an important property, as it constitutes a prerequisite for usual modal interpretations to be available, and it affects the implementation of usual algebraization procedures (in the definition of congruence relations). For quite some time, people were worried about that failure being some sort of structural problem of paraconsistent logics. It is not. Even if many authors have still failed to take notice, examples of paraconsistent logics satisfying the replacement property are known since many years —relevance logics are among the illustrations of importance, as well as dual-intuitionistic logics.

Many of the most well-known **C**-systems fail replacement. But less well known is the fact that Jaśkowski's logic **D2** (another **C**-system, according to my present definitions) also fails replacement, as it is proven, apparently for the first time, in **Chapter 3.2**. Such failure of the replacement property has originated many misdirected criticisms. For instance, according to Priest and Routley, in [77], the failure of contraposition in da Costa's logics C_n, $n > 0$, makes them somewhat problematical, as it "results in the general failure of the principle of the substitutivity of provable equivalents". As an argument, this constitutes already an abuse. As discussed in sections 3.3, 3.5 and 3.7 of **Chapter 1.0**, and illustrated all along **Chapter 3**, contraposition is much more than one needs in order to make a logic respect this substitutivity principle. But Priest and Routley continue, by saying that "this in turn implies that we cannot produce a Lindenbaum algebra for the **C**-systems in the normal way", and they proceed to mention Mortensen's well-known result on the logics C_n (cf. [64]). Here again, as the reader will see in **Chapter 1.0**, the underlying definition of '**C**-systems' that is taken for granted is just too restricted to be of any interest. Besides, it can be shown that there are many other **C**-systems with similar properties that are, nonetheless, perfectly amenable to several varieties of algebraization procedures —some of them quite 'normal'. At last, the authors finish their criticism by asserting that "the fact that there is no Lindenbaum algebra might not seem to be a substantial philosophical (as opposed to technical) problem but in fact it is. For it implies that there are no recursive semantics of a suitable kind. [...] There are well-known arguments for the fact that philosophically adequate semantics must be recursive". There is a lot to be rectified about such final arguments. As the reader will see in the **Chapter 2.1**, all of our **C**-systems possess adequate non-truth-functional bivalent semantics. There is nothing inherently 'non-recursive' about such semantics. Moreover, the competing possible-translations semantics offered in **Chapter 2.2** are entirely recursive, and in fact decidable, and so are the many-valued semantics from **Chapter 1** or the modal semantics from

Chapter 3. At any rate, the insinuated connection between 'Lindenbaum algebras' and 'recursive semantics' remains at best unclear.

On the issue of replacement, da Costa and Bueno write, in [46], that "it is usual to criticise certain paraconsistent propositional logics for not having relations of congruence involving all the connectives. [...] Instead of these logics, some specialists propose distinct ones, which present natural relations of congruence, but which satisfy the law of non-contradiction $\neg(A\&\neg A)$, a law that, *of course*, does not hold in the former ones" [my italics]. The authors seem to suggest a strange dichotomy here: Either the logic satisfies replacement or it validates the fetish formula $\neg(A\&\neg A)$. Now, while **C**-systems like C_1 fail both replacement and the fetish formula, other **C**-systems like $\mathbf{J}_3$ fail the first but validate the latter, and the paraconsistent version of the modal logic K (check **Chapter 3.3**, where the paraconsistent negation is set as a 'non-necessity' operator) satisfies the first while failing the latter. Finally, the paraconsistent versions of modal logics extending KT conform to both replacement and the validity of the fetish formula. So, why 'of course'?

All that said and done, how can the failure of the replacement property be justified, technically or philosophically? Given the multiplicity of paraconsistent logics that are available nowadays, da Costa and Bueno comment, in [46], on what concerns the justification of the success or of the failure of properties such as replacement or theorems such as $\neg(A\&\neg A)$, that 'pragmatic arguments and concrete motives' should be employed, if we are working 'in the domain of applied logic'. That seems a sensible advice. Unfortunately, however, not a single such argument is presented in their paper so as to illustrate how such a choice can be done, in practice.

One last remark. In the case of **dC**-systems (a special kind of **C**-systems, read about this below) satisfying the replacement property, given a classical negation $\sim$ one can often prove that $\sim(A\&\neg A)$ fails to hold (because it will denote the consistency, or 'good behavior', of the formula A). Moreover, the same can be said about $\sim(\neg A\&A)$, or $\sim(A\&(A\&\neg A))$, or any other expression in which $\sim$ applies to a formula equivalent to the inconsistency $(A\&\neg A)$. These formulas will all be equivalent —in fact, congruent. If we are talking about logics that do *not* satisfy the replacement property, however, such variants of the fetish formula can easily fail to be equivalent, to start with. As I point out in **Chapter 1.0**, in a logic like C_1, the formulas $\neg(A\&\neg A)$ and $\sim(A\&\neg A)$ are equivalent, and so are $(A\&\neg A)$, $(\neg A\&A)$ and $(A\&(A\&\neg A))$, but the fetish is monomaniac: While $\neg(A\&\neg A)$ indicates good behavior, seemingly harmless variants such as $\neg(\neg A\&A)$ or $\neg(A\&(A\&\neg A))$ do *not*. In that case, the mentioned logic reveals itself to be, in a certain sense, strongly asymmetric and too dependent on accidental syntactical formulations.

An extended account of the relations of paraconsistency with replacement can be found in **Chapter 1.0** (where replacement is dubbed Intersubstitutivity of Provable Equivalents) and in the introduction to **Chapter 3**.

Good behavior and formal consistency. The path of paraconsistency has never been an easy one to tread. But that was less because of any intrinsic difficulties posed by this variety of non-classical logics than because of, as Florencio Asenjo puts it, "the sterile prejudice of centuries against all contradictions" (cf. [6]). On the one hand, from a purely logical point of view, given the perfect duality between paraconsistent and paracomplete logics, it is hard to imagine an argument for the general rejection of paraconsistency that would not reject, say, intuitionism, for dual reasons; on the other hand, a full acceptance of paraconsistency does seem to require from us the development of a new 'theory of opposition' (or so I argue in **Chapters 3** and **4**). I will not try here to survey the extra-logical arguments that have been presented against paraconsistency. Disgracefully, both among those that work with paraconsistent logic and those that have no idea of what it is about (but still want to write about it) there is still a lot of confusion as to what it accomplishes. For one, Jerzy Perzanowski has adverted to that danger, in writing: "Notice first that the popular name 'paraconsistent logic' is, in a sense, misleading. It suggests that such logics are consistent in a special, weak sense. But, as we know, it is just exactly the reverse. They are simply inconsistent, but unlike the classical logic they are able to work with inconsistencies" (cf. [69]). I will not enter terminological discussions here. I will subscribe though to that same (still shocking?!) intuition about consistency: Consistent logics with a negation symbol are both explosive and non-trivial; paraconsistent logics are inconsistent yet also non-trivial.

According to Batens, "Aristotelian consistency tradition seems to reduce to sheer prejudice" (cf. [10]). Indeed, reasoning mechanisms such as those of classical or of intuitionistic logic unquestionably presuppose consistency as a sort of 'methodological requirement'. I shall here suggest nothing about the 'consistency of the world' or about our possibility of knowing it (or our passionate reaction about its possible failure). As we have seen, da Costa himself believes that it is not up to logic to decide whether the world is consistent or not. But he does say that (check [39], chap.III.3), on what concerns true contradictions, "the central problem consists in knowing [...] if the real world is consistent or not (it seems obvious that it is non-trivial)" [my translation]. (Notice how the 'obvious' part sounds quite 'speculative'.)

Moving to more practical matters, I should say that, after working a few years in the area, I find it ever more difficult to find any interest in da Costa's original hierarchy of logics C_n, $n > 0$, apart from its historical role in the development of paraconsistent logic in Brazil. It seems truly hard to point any technical or philosophical reason that would put these logics in advantage with respect to other competitors, with nicer features. I do believe though that these logical systems were based on remarkable intuitions that can and should be generalized. That belief gave rise to the definition of *Logics of Formal Inconsistency*, explored in the present monograph — logics whose most remarkable feature is the ability to recapture consistent reasoning by the addition of appropriate consistency assumptions.

Suppose you subscribe to the Whiteheadian motto 'one god, one country, one logic'. In that case, if new theories and knowledge happen to supersede old ones by refining rather than by refuting them, you might still want the latter to be preserved in situations in which they were known to work well. Now, suppose instead that you are a pluralist and believe in a 'multiplicity of rationalities', each with its own domain of application. In that case you will not want to commit yourself to the new theory any more than you did commit to the old one. Here again you might be happy that the former theory be capable to reproduce the safe conclusions of the latter, and only contribute to it by suitable localized updates. Non-classical logicians of all breeds will have their reasons either to desire or to abhor the possibility of recapturing classical logic inside their deviant (devious?) logics of choice. Andrew Aberdein classifies in [1] the different attitudes logicians might take with regard to the recapture of classical reasoning as produced by a new non-classical logic **L**: (1) claim that no suitable recapture constraint is expressible in **L**; (2) insist that such recapture of classical logic by **L** is irrelevant; (3) desire to maintain classical logic as a limiting case of its successor **L**; (4) characterize classical logic as a proper fragment of **L**. One would presume, of course, that as soon as a definition of 'recapture' is presented, there should be an objective procedure (if decidable at all) to check whether the logic **L** does or does not recapture classical logic.

Well, how can a theory be recaptured by another, as a 'limiting case'? What would it mean, for instance, to say that hydrogen can be recovered from water, or blue recovered from the sunbeams? That 'hydrogen' and 'blue' are somehow present in 'water' and 'sunbeams', and can be rescued through appropriate mechanisms and devices. Less poetically, what would it mean to say that euclidean geometry is contained in non-euclidean geometry, or that newtonian physics is contained in relativistic physics? That euclidean geometry is obtained from non-euclidean geometry by guaranteeing a certain notion of geometric similarity through variations in scale of the shapes, and newtonian physics is obtained from relativistic physics when the speed of the objects in question is sufficiently low so as to have neglectable effects. Now, how could classical logic be recaptured by a paraconsistent logic?

In [72] and [73], Graham Priest studies a 3-valued paraconsistent logic called LP that is characterizable by adding excluded middle to a De Morgan negation (in a language involving $\wedge$, $\vee$ and $\neg$). As a result, the logic turns out to have no definable implication connective respecting the rule of *modus ponens*. Priest insists though in introducing a sort of *quasi implication* (defined, as in classical logic, by setting $A \supset B \stackrel{\text{def}}{=} \neg A \vee B$) that makes his logic identical to the one that had been proposed by Asenjo, in [5]. Priest argues that LP has, as truth-values, the values 1 and 0 that it shares with classical logic, plus the 'paradoxical' value $\frac{1}{2}$. But, if one restricts the semantics of LP to the classical two-valued (consistent) codomain, he continues, *modus ponens* is respected by the above mentioned quasi implication. Rules and inferences that are validated only in case such a kind of restriction is made are dubbed, by Priest, *quasi valid*. Returning to the issue of his 3-valued quasi implication, Priest asks himself: "How can one reason [in natural language or in mathematics] without *modus ponens*?" (cf. [72], sec. IV.2). The author then exhibits a curious 'methodological maxim' he keeps up his sleeve: "Unless we have specific grounds for believing that paradoxical sentences [i.e. those receiving the intermediary value $\frac{1}{2}$ under some particular valuation] are occurring in our argument, we can allow ourselves to use both valid and quasi-valid inferences" (id., sec. IV.9). How is this maxim actually internalized by the logical machinery of LP? The answer is that it is not. For that the reader would have to wait several years, until the paper [74], where an inconsistency-adaptive (nonmonotonic) version of LP is investigated.[6] Priest wants his logic to somehow recapture classical logic and recover classical reasoning simply by avoiding paradoxical sentences.[7] The logic is, however, just not expressive enough to that end: It is impossible to *say* in LP that a certain sentence is provably 'non-paradoxical'.[8]

Since his first adventures in paraconsistent territory, da Costa has always strived to devise logical mechanisms that would not be contrary to classical logic, but would extend it in some sense. There are various ways of realizing that strategy, and some of them are illustrated in this monograph. Da Costa wants classical logic to be the logic of the 'well-behaved' sentences from the paraconsistent logics that he proposes. I expanded on that idea and made 'good behavior' a primitive 'consistency' connective of what I call **C**-*systems* (a particular case of **LFI**s). The logics in which such new connective can in fact be introduced through a definition in terms of more usual connectives are called **dC**-*systems*. The fundamental feature of all such systems inspired by da Costa's approach consist exactly in the possibility they open for us to recapture consistent reasoning for instance by the addition of appropriate consistency assumptions. So, while classical rules such as disjunctive syllogism (from A and $\neg A \vee B$, infer B) are bound to fail in a paraconsistent logic (because A and $\neg A$ could both be true for some A, independently of B), they can be recovered by an **LFI** if the set of premises is enlarged by the presumption that we are reasoning in a consistent environment (in this case, by the addition of 'consistent-A' as an extra hypothesis of the rule). A more detailed, if relatively informal, explanation of that mechanism can be found in section 1 of **Chapter 3.2** and section 2 of **Chapter 3.3**.

[6] For inconsistency-adaptive logics in general, check [11]; for a sharp criticism of Priest's adaptive strategy, check [9].

[7] Any valid classical inference that is failed by LP is quasi valid. So, if you erase the 'paradoxical' truth-value, you obtain just the classical matrices. This fact is not nearly as informative as it might seem at first look. Call *hyper-classical* any many-valued matrix with that property of defining only classical matrices when we restrict its semantics to the classical codomain. It is easy to see then, using the general abstract results from **Chapter 2.1**, that, given any logic **L** that shares the structural properties of classical logic, **L** is a deductive fragment of classical logic if and only if **L** is characterizable by some set of hyper-classical matrices.

[8] Check the criticism of LP by Batens in section 1.4 of **Chapter 1.0**, and in notes 24 and 25 of the same paper, in section 3.10.

Other aspects of paraconsistency

> Do I contradict myself?
> Very well then I contradict myself,
> (I am large, I contain multitudes.)
> —Walt Whitman, *Leaves of Grass*, 51, 1855.

In 1982, among the 'positive effects' of paraconsistency on the 'philosophical field', da Costa lists (cf. [37]):

1. Better elucidation of some basic concepts of logic, such as, for instance, those of negation, of contradiction, and of the role of the scheme of abstraction in set theory.
2. Deeper understanding of certain theories, specially dialectics and Meinong's theory of objects.
3. Proof of the possibility of strongly inconsistent but non-trivial theories; as a corollary, the common paradoxes are now coming to be seen from very new perspectives.
4. Elaboration of ontological schemes distinct from those of traditional ontology.

Among the 'negative effects', he lists:

1. Proof that some criticisms, formulated against dialectics, are unsound (for example, some critical remarks of Popper).
2. Proof that standard methodological requirements imposed on scientific theories are too stringent and could be liberalized.
3. Evidence that the usual conception of truth, à la Tarski, does not imply that the laws of classical logic (even of the first-order predicate calculus) must be valid.

In view of such a bold list of accomplishments or purposes, it would have been a temerity to think that I would be able to contribute to all fronts. My task in this monograph is much more modest, though. As I explained, the intention is to investigate a certain idea related to the possibility of internalization of the metatheoretical notion of consistency at the object-language level of our logics. It can be seen thus as an exploration of the foundations of paraconsistency made from the point of view of the universal logician, that is, from the point of view of General Abstract Logic.

There are some fundamental intuitions by da Costa and some important aspects of the daCostian systems, however, that were not touched by my present approach, and there are yet some other relevant aspects that I did work on, but that are not reflected in the present selection of papers. I will briefly mention some of these aspects in this section. Each chapter that follows these **Prolegomena** finishes by a **Brief history** where I report on the development of the thereby contained ideas and on some of the venues (congresses, seminars etc) in which I had the opportunity of presenting these ideas and receiving immediate lively feedback on them. This section will also complement that information where it is lacking.

On set theory. I risk being seriously unfair if I talk about da Costa's approach to paraconsistent logic and do not even mention the topic of 'paraconsistent set theory'. Since their very inception, daCostian paraconsistent logics relied heavily on the intellectual provocation brought about by the paradoxes of set theory. As da Costa sees it, this is "one of the most compelling motivations for the construction of paraconsistent logic: the possibilities it opens up in the foundations of set theory" (cf. [40]).

Russell's antinomy is formulated in set-theoretical terms with the help of an explosive negation plus the unrestricted postulate of separation, also known as comprehension scheme. To stay clear from its trivializing conclusions it would seem natural either to make negation non-explosive or to impose restrictions on separation. The classical solutions have all, in one way or another, elected the second alternative —an infinite number of weaker forms of separation is indeed possible and many have been tried. But the first alternative could in principle be naively realized by a paraconsistent logic.

Several paraconsistent set theories have been proposed along the years, many of them based on **C**-systems, notably by da Costa and collaborators (cf. [44]). One of their most reassuring properties is equiconsistency (or, more precisely, 'equi-non-triviality') with classical set theories (cf. [38]). Da Costa attaches a 'paramount importance' to paraconsistent set theories, even for the very definition of logic. In [45] he writes with Bueno that "given that logic is basically concerned with the study and systematization of certain conceptual structures, and that in order to formulate them we need, for instance, set theory, it seems reasonable to demand that a logic, to be taken as such, be developed at least up to this point". Da Costa believes that one of the most important features to be satisfied by a paraconsistent set theory is the provision of a way of recovering classical set theories and reconstructing traditional mathematics, while at the same time settling the foundations for a 'paraconsistent mathematics'.

I should open here a small parenthesis on a puzzling 'motivation' presented by da Costa for the 'devising of paraconsistent logic', namely, "the interplay between semantics and set-theoretic issues". As a matter of fact, the authors of [43] say that the use of classical set theory in the formulation of the semantics for paraconsistent systems is 'philosophically untenable', and they maintain that "there is, in a certain sense, no semantics for a paraconsistent logic without a paraconsistent set theory". Similarly, in [45] the authors write that "one should note that, at least on philosophical grounds, it is needed to have a paraconsistent set theory already articulated if one intends to have a reasonable semantics for paraconsistent logic (given that semantics shall be constructed within set theory)". In both papers they use those remarks in fact to justify why da Costa, "when first presented his paraconsistent systems, not having developed yet a paraconsistent set theory, formulated them in a syntactic, not in a semantic, way" (cf. [43]). The line of argument seems really baffling. So, the authors wish to defend a 'pluralist' outlook on logic, and want at the same time each logic to be dealt with exclusively by a set theory based on this very same logic? Is it really untenable to provide a semantics for a logic such as, say, da Costa's C_3 based on anything else than a set theory built over C_3 itself? What is non-classical about the set theory underlying the theory of valuations that da Costa has proposed for his logics C_n, $1 \leq n \leq \omega$? Or about the many-valued or modal semantics that are adequate for yet some other **C**-systems?

I do not share with da Costa the belief that logic starts at set theory, nor do I see any necessary connection between the foundational problems of paraconsistent logic and the foundational problems of set theory. It is true that paradoxes such as the one raised by Russell's antinomy can serve as a good motivation for the proposal of paraconsistent systems. But, as it is well-known, there are other set-theoretical paradoxes such as Curry–Shaw-Kwei's that can be derived from the unrestricted postulate of separation even when negation is not available, as long as the logic has an implication satisfying both contraction and *modus ponens* (cf. [80]). It is not enough to go paraconsistent so as to avoid such paradoxes. I am unaware of any outstanding progresses recently achieved by the 'Brazilian school' in the investigation of set theories based on the full postulate of separation and on paraconsistent logics weak enough so as to avoid such kinds of trivialization strategies. I myself do not have much to contribute here at present. For all those reasons, the theme 'paraconsistent set theory' is absent from this monograph.

On an infinity of logics. Da Costa started his work on paraconsistent logic already by proposing not one or two such logics, but a denumerable number of them. The present monograph shows only how this number can be multiplied. Is there anything to be learned from such a multiplicity of logical options?

As we have seen above, da Costa defends that each logic has its 'domain of application' and that the existence of various systems of logic only helps in showing that "the rational and logical activities do not coincide, even though any logical activity is, *ipso facto*, rational" (cf. [39]). In [46] he intends to defend a sort of fallibilist interpretation of paraconsistent logic that would seem to lie, as argued in [75], somewhere in between realism and instrumentalism. Da Costa and Bueno say that "given the proliferation of heterodox logical theories, especially the existence of infinite paraconsistent logics containing a considerable part of traditional logic, the defence of an extreme realist view becomes a difficult task". For these authors, "realist conceptions *à la* Frege and Gödel, according to which logic supplies the most general features of the universe, only seem to be defensible on largely speculative grounds". So much here for the philosophy of paraconsistency.

I assume in the present monograph that logic is about 'what-follows-from-what'. It should be noted that da Costa classifies that approach as 'applied logic' —for him, 'pure logic' is about model theory, formal languages, recursion theory, and the like (cf. [45, 46]). But that classification makes him adopt a very specific stance with regards to the choice of logical systems to apply to each specific situation. He asserts that, if one takes into consideration the plethora of non-classical logics that are available, one must conclude that there is 'considerable underdetermination' on what concerns that choice: 'Empirical constraints' and 'pragmatic considerations' should be taken into account "in the determination of the acceptable solutions to the problems under examination". That seems a sensible advice, but it is not clear to me how tractable the choice problem is. Which would be the heuristic and pragmatic commitments to be measured so as to help us in deciding whether a logic like C_3, say, is preferable over C_1, or over any other paraconsistent logic? I can only hope to count on the technical features of each system to help me decide on that.

On maximality. One way of attending requisite (iv) of da Costa's 1974 paper (recall the last section), the one that required that paraconsistent logics should preserve as much of classical logic as possible, is by the systematic search for maximal paraconsistent fragments of classical logic. Already in 1980, Batens denounced the fact that the notion of a maximal paraconsistent logic had not been given enough attention (cf. [8]). None of the logics C_n, $1 \leq n \leq \omega$, is even close to being maximal (though Sette's logic $\mathbf{P}^1$, as studied in [79], is known since long to constitute a maximal extension of the logics from the former hierarchy).

I studied the problem of defining **C**-systems that would constitute maximal paraconsistent fragments of classical logic a few years ago, and presented some preliminary results about that in my contribution to the II World Congress on Paraconsistency, as reported in the paper [62]. These ideas were soon extended into the research note [59], whose main results are reported in section 3.11 of **Chapter 1.0**. A different approach to da Costa's requisite (iv) is taken by inconsistency-adaptive logics (cf. [11]), in which maximality is pursued through nonmonotonic strategies, presupposing consistency by default.

On predicate logic. As we have seen above, and confirmed with requisite (iii) of da Costa's 1974 paper, arguments have been proposed to the effect that (paraconsistent) logic should be (at least) first-order. I have made it clear though that I see no need of requiring that much from the objects I call logics. Now, that surely does not mean that I ignore or refuse to study predicative versions of the Logics of Formal Inconsistency (check what I say about this at section 4 of **Chapter 1.0**). As it happens, given my objectives and the sort of problems I had to attack, not much is said or done about paraconsistent logics in the present monograph that goes beyond the propositional level. In other papers, however, the topic was important: A first-order version of the **C**-system $\mathbf{J}_3$ (under the name of **LFI1**) is used in the papers [29, 49], the latter having been presented by Sandra de Amo at the II International Symposium on Foundations of Information and Knowledge Systems (FoIKS'2002); a process that would allow first-order paraconsistent logics to be obtained by the appropriate combination of propositional paraconsistent logics and classical first-order logic was reported in [24], a paper presented by Carlos Caleiro at the IC-AI'2001, the 2001 International Conference on Artificial Intelligence —in that paper we show in fact how the method could be applied so as to 'first-ordify' the logic C_1.

On databases and mechanized deduction for LFIs. How 'strategic' is (paraconsistent) logic? It is not always easy to do basic science. If you want to get that grant you always dreamt of, and make your research topic count in the class of 'priority topics' of your financing agency, you had better show how useful and applicable it can be. You can also play a bit with that. Physics had its century. Biology is the science of the hour. Aristotle would be happy with that. I have already written a paper on 'taxonomies' of logics. What should the next one be on? Genetic sequencing of logics? Clones of logics? (Note, by the way, that clone theory is already a topic from algebra, and genetic algorithms are old fellows of neural networks.)

Long ago, in [83], the logician Alfred Tarski made a provocative comment on that issue that I cannot resist to quote here:

> I believe, nevertheless, that it is inimical to the progress of science to measure the importance of any research exclusively or chiefly in terms of its usefulness and applicability. We know from the history of science that many important results and discoveries have had to wait centuries before they were applied in any field. And, in my opinion, there are also other important factors which cannot be disregarded in determining the value of a scientific work. It seems to me that there is a special domain of very profound and strong human needs related to scientific research, which are similar in many ways to aesthetic and perhaps religious needs. And it also seems to me that the satisfaction of these needs should be considered an important task of research. Hence, I believe, the question of the value of any research cannot be adequately answered without taking into account the intellectual satisfaction which the results of that research bring to those who understand it and care for it. It may be unpopular and out-of-date to say — but I do not think that a scientific result which gives a better understanding of the world and makes it more harmonious in our eyes should be held in lower esteem than, say, an invention which reduces the cost of paving roads, or improves household plumbing.

On that matter, there is also the story by Michael Faraday, who, after a public demonstration of an electrical experiment, was asked what was the use of electricity. He retorted: "What use, madam, is a new-born baby?"

There might still be some resistance to be found in the philosophical community, but many people in computer science nowadays believe that paraconsistent logic is already running on cables. This monograph probably mentions not a single application of paraconsistent logic to real-life problems. And it will probably not make me rich. I have, nonetheless, worked elsewhere on the application of Logics of Formal Inconsistency, **LFI**s, to problems of computer science. Some initial investigation of ours on the mechanization of deduction for **LFI**s by way of tableaux is reported in [28], a paper presented by Walter Carnielli at the IC-AI'2001. Much more about that, specially on what concerns the many-valued case, can be found in [21], a paper presented by Marcelo Coniglio at the III World Congress on Paraconsistency, and the related papers [23] and [22], inspired on my early draft [60]. In [29] and [49] I have explored the application of certain **LFI**s to the study of 'evolutionary databases', databases that are endowed with inconsistent-tolerant logical mechanisms and that can evolve with time, allowing for some inconsistency to appear also among their integrity constraints. In 2003 I was invited to and participated on the Dagstuhl Seminar 03241 on 'Inconsistency Tolerance', at the Dagstuhl Castle (DE). There I talked about the mechanization and the use of dyadic semantics in providing automated decision procedures for logics that allow for reasoning under uncertainty.[9]

Advertising LFIs. This is not a job just like any other. I had the mission of convincing other people that **LFI**s could do them good —if only to test whether I was really right on that belief. I took the gospel to several places in the last few years, and specially after writing **Chapter 1.0**. Under invitation, I gave some quite general talks on the idea of **LFI**s and the internalization of consistency, at varying degrees of informality and detail, at the Theory of Computation Seminar of the Center for Logic and Computation of the IST, in Lisbon, in January 2002, at the Séminaire Interuniversitaire de Logique Mathématique, at the Université Libre de Bruxelles, in February 2002, at the School of Informatics of the City University of London, in March 2002, at the Institute for Logic, Language and Computation of the University of Amsterdam, in March 2002, and, in Poland, in September 2002, at the Department of Logic and Methodology of Sciences of the University Marie Curie-Skłodowska, in Lublin, and at the Department of Logic of the Institute of Philosophy of the Nicholas Copernicus University, in Toruń. Interestingly, I invariably learned from my audiences much more than what I was able to teach to them.

On the duality between inconsistency and undeterminedness. One of the basic semantic intuitions behind paraconsistency relates to its purported duality with intuitionism —or, more generally, with 'paracompleteness' (cf. [58, 12]). A renitent difficulty concerning the abstract characterization of that duality has always been the persisting use, by a good part of the 'Brazilian school', of old-fashioned syntactical mechanisms that privilege truth over falsehood. I like to compare this asymmetrical situation to all the fiction that surrounded the 'dark side of the Moon', whose first notorious visitor was Jules Verne in his 'Autour de la Lune', from 1869. From where we stand, on Earth, how could we even *know* that the Moon has another side, besides the one that is permanently visible from our planet? I am not

[9] Check
`ftp://ftp.dagstuhl.de/pub/Proceedings/03/03241/03241.MarcosJoao.Slides.pdf`.

suggesting that the Moon could have been some sort of Klein Bottle; but, from all we know, the Moon could have turned out to be, for instance, a half-sphere, or have some other strange 'lunoid' shape. The first photographs we ever got from the other side of the Moon were taken by the spacecraft Luna 3, in 1959. Did it have any trouble taking the pictures in the dark? None at all! Most people do not even think about it, but, while it is true that half of the Moon is always in shadow (for the Moon has its phases), the so-called 'dark side of the Moon' gets just as much sunlight on it as the side we do see from the Earth. Even though we are not looking at it, there may well be a lot of interesting things to see at the far side of the Moon. Why should one ignore one half of the Moon? Why should there be in logic a bias towards truth, while falsehood is ignored?

The adoption of a multiple-premise-multiple-conclusion framework, as in **Chapter 2.1** of the present monograph, allows for a very natural fix of the above situation, in restoring symmetry and clearly displaying the duality between paraconsistency and paracompleteness, as it is done in **Chapters 4.1** and **3.3**. Before I could arrive to that conclusion, I had already done some semantic investigations on duality, reported for instance at a talk I presented in October 2000 at the Institut für Logik, Komplexität und Deduktionssysteme of the University of Karlsruhe, in the scope of a ProBrAl project involving Brazil and Germany, at my contribution to the Joint Austro-Italian Workshop on Fuzzy Logics and Applications, held at the Università degli Studi di Milano, in December 2000, at yet another talk I presented in February 2001 at the Theory of Computation Seminar of the CLC / IST, in Lisbon, and at my contribution to the IV Flemish-Polish Workshop on the Ontological Foundations of Paraconsistency, held at the University of Ghent in December 2001. The work documented in the present monograph smoothly sprang from those early investigations, as soon as I managed to fix the right theoretical framework.

Paraconsistent mistakes. That paraconsistent logic admits of some inconsistencies should not mean that you can be as incoherent as you want if you work in this area. That you tolerate inconsistencies should not mean that you eagerly expect for them to be found. One of the most inopportune obstacles that paraconsistency has faced (and still faces) on its way to becoming more popular and well-accepted seems to be the attraction it exerts on practitioners of pseudo-science and other varieties of fashionable nonsense. I will not advertise their work here. In the long run, historians and sociologists of science might help me give support to the impression that this area of logic, more than others, has always been prone to such rubbish. I am more interested here instead in studies that have been produced by reasonably informed researchers but that, nevertheless, got impaired by deadly, and obviously unintentional, mistakes. Several such studies are mentioned in the present monograph, and the reader will see that I have put a lot of effort in fixing the spotted flaws whenever I was able to see the way out.

In August 2003 I organized a round-table called 'Contradictory and not: On the Philosophy of Inconsistency', at the XXI World Congress of Philosophy (XXI WCP), held in Istanbul, and I presented there a talk entitled 'The millionaire contribution of all mistakes' (making use of the nice expression by Oswald de Andrade, one of the heads of the Brazilian Modernist Movement). The talk was based on the exposure of some flaws committed by paraconsistentists-to-be, and on what we can learn from them. I will have certainly committed my own mistakes, here and elsewhere, and I only hope to risk committing even more (for that will indicate that I am, or at least I am trying to be, engaged in a productive scientific life). As a responsible scientist, though, I should try to minimize such errors, and I should be happy to have them pointed out and corrected, when that is the case.

The Future. After half a century, how mature and successful is the paraconsistent enterprise, in our days? On that respect, here is an event that da Costa likes to mention, and with a good reason: "In 1991, fifteen years after its baptism and twenty-eight years after its birth,[10] paraconsistent logic was accepted in the category of theories admitted by the mathematicians: a special section is created for it in *Mathematical Reviews*" (cf. [43]). Indeed, the Mathematical Subject Classification has created for 'Paraconsistent Logic', in 1991, the field 03B53 (where '03' stands for 'Mathematical Logic and Foundations' and 'B' for 'General Logic'). However, in 2000, the above classification changed its description, from 'Paraconsistent Logic' to 'Logics admitting inconsistency (paraconsistent logics, discussive logics, etc.)'. Now, what is there in 'discussive logics' that make them plural and diverse from 'paraconsistent logics'? And what sort of objective description of a class of objects includes an 'etc.' in it? That late change in description only seems to me to attest to the lack of coordination and deep understanding still to be found in the field of paraconsistency. Not maturity.

Another story. In September 1999 I have started a discussion group on paraconsistency,[11] that now counts about 80 members. It does not bear good testimony to this area of research, though, that the members of that group have been unable to or uninterested of pursuing any rich threads of discussion ever since, as one might have expected of researchers in a more consolidated area. Well, let's keep on waiting. . .

To the moment, our present approach to the Logics of Formal Inconsistency seems to have been reasonably successful in its objectives. It managed to instill a new breath of life into the Brazilian school of paraconsistency, and it has collected a few adherents worldwide (see the next section). No lesser sign of maturity of this particular topic, in my opinion, is given by the publication of a paper called 'Logics of Formal Inconsistency' as a chapter of the second edition of the Handbook of Philosophical Logic (cf. [26]). More is hopefully to come.

[10]The reader will recall from earlier sections that da Costa likes to date the birth of paraconsistent logic from his 1963's habilitation thesis.

[11]Check `http://groups.yahoo.com/group/paraconsistency/`.

Some contributions of the present thesis

> Usando do inglês como língua científica e geral, usaremos do português como língua literária e particular. Teremos, no império como na cultura, uma vida doméstica e uma vida pública. Para o que queremos aprender leremos inglês; para o que queremos sentir, português. Para o que queremos ensinar, falaremos inglês, português para o que queremos dizer.
> —Fernando Pessoa, 'Babel — or the Future of Speech', excerpt from *As Cinco Línguas Imperiais*, 1930s.

If you feel discomfortable with criticism, a life of science is not a life for you. If you are willing to receive it, howsoever, and to learn with it, you should put your work in a visible position and open to debate. Moreover, to minimize problems related to linguistic incompetence (of those who cannot read you), you had better write in the language of the empire. In order to write the present thesis in English at the State University of Campinas, Brazil, I was forced to make it a collection of more or less independent papers. The format has an obvious advantage: No much need to rewrite things after the job is done. But the disadvantages are numerous: A lot of effort should be put in relating those papers to one another, if that be the case; there will certainly be redundancies (and a lot of repetition too); terminology and notation will often vary along the text; there will be a profusion of bibliographical references; the result of late investigations might oppose results previously obtained; one's own linguistic incompetence will be more striking when writing in a foreign language. However, the extra effort might be compensated by a better presentation and understanding of the whole, while the pidgin English, the redundancies and the fluctuation on terminology might end up being no big deal for the perspicacious reader, and other differences in presentation might allow them to better follow the progress of a study and track the evolution of its main ideas. The present text will surely suffer from the former many defects, and enjoy the latter few virtues. The main difficulty one might have with studying such a text, specially if we take into account its number of pages, is in evaluating its original contributions, or even just finding them in the middle of so much else. I will thus briefly offer, in this section, a selection of some of the main contributions of the present study, according to my own preferences.

1. *A formal study of logical principles.* If you have ever heard about paraconsistent logics, you have certainly heard about how these logics allegedly defeat at least one among the so-called 'Principles' or 'Laws' of *ex contradictione*, *ex falso*, *pseudo-scotus* and non-contradiction, and possibly you have also heard about how these logics do respect the principles of non-triviality and non-overcompleteness. The formal framework set in **Chapters 1.0** and **4.1** allows us to distinguish between *all* such principles, and also a number of other principles. *Ex contradictione* and *pseudo-scotus* are related to a certain principle of explosion that needs to be failed by paraconsistent logics. Several distinct varieties of explosion (supplementing explosion, partial explosion, controllable explosion and gentle explosion) are still compatible with paraconsistency. Deviating from the bulk of related literature, my present version of the Principle of Non-Contradiction is also compatible with paraconsistency, yet, as it could be expected, its failure in non-overcomplete logics does require a paraconsistent environment. The fetish formula of paraconsistentists, $\neg(A\&\neg A)$, is shown to have nothing to do with paraconsistency, in general.

2. *Definitions of paraconsistent logic.* Of course, these will depend on how you define 'logic', how you define 'negation', and how you define 'paraconsistent'. Instead of fixing once and for all such definitions, I propose here a novel *negative* approach to them (check **Chapter 4**, but also **Chapters 1** and **3.3**). A logic is just a very general structure having an arbitrary set of 'formulas' as its domain and a convenient 'consequence' relation defined over it, intended to indicate what can be inferred from what. The main property of a *decent* logic consists in failing overcompleteness. A negation is in general a unary symbol aimed at embodying some general notion of 'opposition', and among the main negative properties of a *decent* negation are the rules I call *verificatio* and *falsificatio*, that will guarantee that negation is not a 'positive operator' —intuitively, they will make sure that negation inverts some truth-values. Finally, a *decent* paraconsistent logic should disrespect both *pseudo-scotus* and *ex contradictione*, so that it will not only have an inconsistent model, but a non-dadaistic such model (check **Chapter 4.2**).

3. *Coherence conditions for connectives, and perfect connectives.* Logical constants can often have their meaning set by groups of abstract complementary rules, that show how these constants can be introduced and eliminated, at the right or the left hand side of the consequence symbol. In **Chapter 1.0** I show how the deletion of some of those rules suggests the addition of further negative rules, so as to keep coherence and avoid idle —or 'indecent'— examples of connectives. I show in **Chapter 3.3** how the rules that are lost by paraconsistent or paracomplete logics can often be recovered by the addition of certain subsidiary connectives —such as the connectives of consistency or inconsistency— that complete the partial meaning of negation, restoring its lost perfection.

4. *Definition of* **LFI**s*,* **C**-*systems, and* **dC**-*systems.* The Logics of Formal Inconsistency are introduced in the first chapter and studied throughout the thesis. Their near-ubiquity in the realm of paraconsistency is repeatedly illustrated: Most interesting logics produced by the Brazilian school fit the definition, all non-degenerate normal modal logics can be recast as **dC**-systems, Jaśkowski's discussive logic **D2** and its close relatives also constitute **dC**-systems. A comprehensive survey of the related literature is provided, and the problems related to the algebraization of such logics and the possible validity or invalidity of the replacement rule are equally surveyed. The 'Brazilian plan' is completed in that I hint on how maximal logics obeying all of the initial requisites of da Costa on paraconsistency can be obtained. Examples of **C**-systems that are not **dC**-systems, of **LFI**s that are not **C**-systems, and of paraconsistent logics that are not **LFI**s are also presented. The *Fundamental Feature of* **LFI***s*, as reflected in the so-called 'Derivability Adjustment Theorems' or the translations that allow consistent reasoning to be recaptured inside the inconsistent environments of **LFI**s, is heavily emphasized.

5. *Duality.* A framework of abstract Gentzen-like multiple-premise-multiple-conclusion consequence relations is investigated in **Chapter 2.1**, **3.3** and **4**. That framework allows for a full symmetry to be established between premises and conclusions, and each inference can then be dualized just by reading it from right to left instead of from left to right, or vice-versa. In semantical terms, this corresponds to substituting truth for falsity and vice-versa in each given model. Paracomplete logics are then characterized as duals to paraconsistent logics, and the dual-**LFI**s constitute the so-called Logics of Formal Undeterminedness, **LFU**s.

6. *Definition of structures of possible-translations.* In **Chapter 2.1**, some very generous definitions of Possible-Translations Representation and of Possible-Translations Semantics are offered for the very first time, both in terms of single- and of multiple-conclusion logics. The theory of these structures is shown to extend the general theory of matrices and logical calculi. Several examples of possible-translations semantics as applied to some very weak **LFI**s that are not characterizable through finite matrices nor usual modal semantics are presented in **Chapter 2.2**.

7. *Modal* **LFI***s.* As already mentioned, paraconsistent logics are shown, in **Chapters 3.2** and **3.3** to have a significative intersection with modal logics. Jaśkowski's **D2**, however, is shown in **Chapter 3.2** not to constitute anything like a usual modal logic once it fails the replacement property. Many natural examples of **LFI**s satisfying the full replacement property are still presented, with the help of modal interpretations for paraconsistent negations and consistency connectives. A similar thing is done for **LFU**s.

8. *Logics of Essence and Accident.* Studied apart from the presence of a paraconsistent negation, the modal connectives of consistency and inconsistency are given the reading of connectives that qualify essential and accidental modes of truth. A poor modal language is set by adding such connectives to the classical language, and a minimal logic of essence and accident is adequately axiomatized in **Chapter 3.1**. Some initial results are presented about the definability of the usual modal language from the language of essence and accident as well as the characterizability of classes of frames with the help of the latter language.

9. *Many confusions and mistakes by other authors are pointed out right on the spot*, all along the thesis. Some of the flaws are repaired.

10. *Formal Philosophy.* The whole thesis is an illustration of how philosophical problems can be studied with the help of convenient logical tools, if only we can agree to fix a convenient formalization for the terms under discussion. Philosophy can also claim thus its laboratory and its measuring instruments.

Travelling Salesman

Navigare necesse est, vivere non est necesse.
—thus spake Pompey, according to Plutarch's *Life of Pompey*, 75 AD.

The development of the investigations hereby reported spanned several years of my life. Since I engaged in my PhD research, about 6 years ago, I lived for long periods in 4 different countries, and I received monthly stipends from 7 different academic institutions and foundations, having worked as a Research Fellow, a Research Assistant, and also as a Teaching Assistant. When I finished my Master's Thesis, I had not a single published paper, and I had only participated on a few congresses and workshops here and there. However, as a testimony of astonishing Brazilian scientific autism, I felt no pressure for a more active involvement in scientific life. Notwithstanding, after getting more acquainted with real science as it is done 'out there', I decided to keep away from parochial science the best I could, and to change my behavior and my history. In the course of the last few years, then, I took part directly on 23 different congresses, symposia, workshops and summer schools, in 14 different countries, where I presented 19 contributions. I can in fact add 2 more events and 1 more country to that list if I count the other 6 contributions presented by my co-authors. Besides, in the same period, I presented other 21 invited talks at 8 different countries, addressing audiences from 14 different institutions. Lots of advertise, indeed. And, best of all, I got paid for it.

Not everything you write gets published, and that is just how it should be. I did manage though to have 2 papers already published in international journals and 5 other papers were accepted and await publication in other journals in the near future, I published 8 other papers in books and proceedings of congresses, and 3 further papers of mine are scheduled to appear soon in other volumes. In 9 among the above studies, I had the opportunity to collaborate with 6 different co-authors. I currently have 3 papers submitted for publication in different journals, several preprints available on-line and a few other papers at varying degrees of completion. Not to count many other short abstracts published at the many congresses mentioned above. I was involved in various research projects. The above mentioned papers deal with several different themes. A good sample of those papers organized around a central theme on paraconsistency and Universal Logic builds up the present monograph.

Some further scientific work. I translated a book on modal logic and Gödel's theorems. I co-supervised the work of a student in Scientific Diffusion in mathematics and I co-organized 3 international events of different sizes. I officially refereed 17 papers and books submitted to international journals, publishing houses and journals. Having been no more than a neophite at the II World Congress on Paraconsistency, in 2000, I had the honor of being invited to join the Scientific Committee of the III World

Congress on Paraconsistency, only 3 years later. Best of all, my teachings did not fall on deaf years. They have helped other people make progress. I have collected in recent years a sizable list of papers that cite, refer to, or actively make use of my work, as it can be checked below. That is surely very rewarding for a scientist on the making. Perhaps you will also contribute to my collection?

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Short Addendum on Unnecessary Explanations

> If it was so, it might be;
> and if it were so it would be,
> but as it isn't, it ain't. That's logic.
> —says Tweedledee, in Lewis Carroll's *Through the Looking Glass and What Alice Found There*,1872.

Let me tell you one last thing. It might be shocking for you, so please sit down. Here it is. I do not assume logic to be an 'investigation of the laws of thought'. Nor do I buy it, otherwise, as 'a formula language of pure thought'. And do not even think that I understand logic as the 'pursuit of truth'. I certainly do not assume here that logic *by itself* is going to tell you much about the principles of rational thinking, the psychology of reasoning, natural language, epistemology, metaphysics or ontology. (Logic relates to metaphysics, for instance, just as mathematics relates to engineering.) Yet it might *help* you investigating any of those issues. Logic, in my everyday job, is but pure technology —a declaration that would not surprise Aristotle. It helps you in doing philosophy, mathematics, linguistics and computer science in much the same way a laboratory and a computer helps the standard natural scientist in their job. As any other man-made tool, or organon, it extends your power to realize the tasks that your intelligence proposes to you. It would be somewhat surprising that any of the above statements still needed to be made in the days we live. But here they are. For all I said in such a *via negativa*, this might seem all too elusive as a characterization. Oh well, I am not telling you now what I *do* take logic to be. Will you please just have a look at the papers that follow.

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NON SEQUITUR
    QUODLIBET
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. . . .

Late, and hopefully also unnecessary, warning

> I killed my logic teacher when I was 16. Alleging self-defense —and which defense would be more legitimate?— I managed to be absolved by five votes against two, and I went to live under a bridge of the Seine, though I have never been in Paris.
> —Campos de Carvalho, *A Lua vem da Ásia*, 1956.

In case you have not yet noticed, this thesis will be better read by those with a good sense of humor.

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You already know that I have travelled a lot. That I have read a lot of papers. Met a lot of people. To be fair, the number of people I would have to mention as having helped me in one way or other, by teaching me a few logical tricks, by showing me the way to go, or by just being there when I needed the most, well, that number is certainly LARGE. By way of expression, I would rather call it *innumerable*. But in that case I am forced to abide by the Cantorian dilemma, according to which any list I would try to make of those people would necessarily leave somebody out. So, to be perfectly fair with everybody, I will mention NO ONE here. (Yet many people will be mentioned by name in the next chapters!)

I do have to refer here, though, and in grateful recognition, to the financial support I received in the grants and fellowships that enabled me to survive through the last few years. This thesis would not have been possible had it not been, in this order, for a 24 months CAPES fellowship (at Unicamp / BR), together with a CAPES / DAAD allowance that financed my trip and stay in Karlsruhe (DE) for 4 months, working at the Institut für Logik, Komplexität und Deduktionssysteme, a 12 months Dehousse Doctoral Grant that I received while working as a Research Assistant in Ghent (at UGent / BE), an 11 months CNPq grant (at Unicamp / BR), and finally a 24 months support from the Fundação para a Ciência e a Tecnologia (PT) and FEDER (EU), namely via the grant SFRH / BD / 8825 / 2002 and the Project FibLog POCTI / MAT / 37239 / 2001 of the CLC / IST (at UTL / PT). I should also thank here (contradicting my policy of not mentioning names!) those who were my hosts while I enjoyed those grants, namely, the professors Walter Carnielli (BR), Peter Schmitt and Bernhard Beckert (DE), Diderik Batens and Erik Weber (BE), Carlos Caleiro and Amílcar Sernadas (PT), for providing to me the best working conditions they could throughout this odyssey (and no better place to finish an odyssey than Lisbon, having Ulysses himself granted to it its old name of Olisipo, or Ulyssipo).

Was the whole effort worth? I do hope so (se a alma não é pequena).

Crime is common. Logic is rare. Therefore it is upon the logic rather than upon the crime that you should dwell.
—Sir Arthur Conan Doyle, *The Adventures of Sherlock Holmes*, 1892.

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Appendix: Brief historical note

To give a fair account of the early development of paraconsistent logic in Brazil, Newton da Costa, 75 years old, was interviewed in February 2005 about the historical origins of his work on paraconsistency. Prof. da Costa tells us that he developed paraconsistent logic in between 1954 and 1958, presenting his results in seminars and conferences at the Federal University of Paraná (UFPR), at the State University of São Paulo (USP), and at the late University of Brazil. He says that he only exposed his results little by little, because they were revolutionary at the time, in contrast to what happens in our days. Da Costa recalls some early expositions he made on the material for the benefit of Mário Tourasse Teixeira at Rio Claro, São Paulo, and Constantino Menezes de Barros at Rio de Janeiro, in 1958 and 1959. He also mentions a course he gave at the Federal University of Rio de Janeiro (UFRJ) in 1961, followed by, among others, Mário Tourasse, Constantino de Barros and Max Dickmann, and he recalls his surprise for not having been told by anyone that he was crazy —Prof. António Monteiro, for one, is supposed to have told da Costa then that a violation of the principle of non-contradiction was simply inconceivable, as Heyting would have asserted. All that happened allegedly before da Costa got in touch with the abstracts of the studies by Jaśkowski and with the work of Nelson, which da Costa was to mention in his Habilitation Thesis on the theme, in 1963. Da Costa proceeded then to exchange letters, in separate, with both Jaśkowski and Nelson on issues pertaining to their common interests.

The first publications by da Costa containing more precise definitions related to paraconsistency and the axioms of his first paraconsistent logics appeared in 1963 (cf. [35, 34]). However, it should be acknowledged that in 1962 a short and relatively informal notice about these logics had appeared as an abstract of a contribution to the XIV Annual Meeting of the SBPC (the Brazilian Society for Progress in Science), held in Curitiba, Paraná, in July 8–14 of 1962. This abstract was published as:

> Newton Costa. Sôbre um subsistema do cálculo proposicional clássico. *Ciência e Cultura*, 14(3):139, 1962.

Here you can find the content of this abstract, *ipsissima verba*:

> O cálculo proposicional clássico não se presta para servir de base a sistemas dedutivos onde possa haver contradições. Alguns lógicos e matemáticos, como Kolmogoroff e Jaśkowski, procuraram, então, estruturar cálculos proposicionais com tal finalidade. O autor estudou um subsistema do cálculo tradicional, denominado **cálculo C**, que satisfaz, aparentemente, a exigência acima, e que possui as seguintes características: 1) em C no vale o princípio da não contradição; 2) em C, de duas proposições contraditórias, não se pode deduzir, em geral, qualquer proposição;[12] 3) grande parte dos esquemas e regras de dedução

[12] A formulação deste item é algo estranha e pode ser mal entendida se lida por um novato

> mais importantes do cálculo proposicional clássico valem em C; 4) a extensão de C a um cálculo funcional de primeira ordem é imediata; 5) acrescentando-se a C o princípio da não contradição, obtém-se o cálculo tradicional.

I also take the chance here to provide a first translation of this into English:

> Newton Costa. On a subsystem of the classical propositional calculus.
>
> The classical propositional calculus cannot serve as basis for deductive systems where contradictions can be found. Some mathematical logicians, such as Kolmogorov and Jaśkowski, have tried, then, to structure propositional calculi to serve such an end. The author studied a subsystem of the traditional calculus, called **C-calculus**, that would seem to satisfy the above requirement, and that has the following characteristics: 1) in C the principle of noncontradiction does not hold good; 2) in C, from two contradictory propositions, no proposition in general can be deduced;[13] 3) a great deal of the most important schemas and deduction rules from the classical propositional calculus holds good in C; 4) the extension of C to a first-order functional calculus is immediate; 5) if one adds to C the principle of noncontradiction, the traditional calculus is obtained.

. . . .

All that said and done, one should also acknowledge that it would have seemed that paraconsistency was in the air, in the Southern Hemisphere, in the early 1950s. In 1953, a young man called Florencio González Asenjo (nowadays Professor Emeritus of the University of Pittsburgh) delivered at the University of La Plata, in Argentina, a talk entitled 'La idea de un cálculo de antinomias'. Asenjo developed his views on inconsistency in that decade, but when he first published his results in more detail, in between 1965 and 1966 (cf. chap. X.2 of [20]), he was already acquainted both with the review of the first paper by Jaśkowski and with the first French paper by da Costa.[14]

Is Asenjo a 'forerunner' or a 'discoverer' of paraconsistency? Neither of these options? Both? That is yet another combat, not to be fought in the present thesis.[15]

no assunto. No entanto, do que se sabe hoje da lógica paraconsistente, deve-se supor que pela frase "não se pode deduzir, em geral, qualquer proposição" ter-se-á pretendido dizer que "não se pode deduzir, em geral, uma proposição qualquer" (nota minha).

[13] This item is awkwardly formulated already in Portuguese (see the last note). One should suppose, however, that the author wanted to say that one cannot deduce, in C, an arbitrary proposition from two given contradictory propositions.

[14] Prof. Asenjo tells us that he was very pleasantly impressed when he found da Costa's Comptes Rendus paper by accident in a library in Pittsburgh in the early 1964, shortly before he submitted, in the same year, his paper [5] to the NDJFL. Sobociński was the person who called his attention to the work of Jaśkowski, which also ended up cited in the latter paper. Asenjo also calls our attention to an early passage where the work he did in 1953 was mentioned, in a book published in Madrid in 1962 (cf. p.9 of [4]).

[15] I thank Décio Krause, Newton da Costa and Florencio Asenjo for their kind assistance in helping me clear up the above historical imbroglio.

Chapter One

Map of the Territory

The paper that constitutes this chapter was published as [12]. It is reproduced here with the kind permission of Marcel Dekker.

Resumo

As lógicas da inconsistência formal (**LIF**s) são lógicas paraconsistentes que nos permitem internalizar os conceitos de consistência ou inconsistência em nossa linguagem obje[c]to, introduzindo novos operadores para falar sobre tais conceitos e tornando possível, em princípio, separar logicamente as noções de contraditoriedade e de inconsistência. Apresentamos as definições formais de tais lógicas no contexto da Lógica Abstra[c]ta Geral, sustentamos que elas representam na realidade a maior parte das lógicas paraconsistentes existentes até o momento, se não ao menos as mais excepcionais dentre elas, e demarcamos uma subclasse de tais lógicas, os chamados **C**-sistemas, como aquelas **LIF**s que são construídas sobre a base positiva de alguma dada lógica consistente. A partir de caracterizações precisas de alguns princípios lógicos estabelecidos, mostramos que o ponto fulcral da lógica paraconsistente repousa sobre o Princípio da Explosão, ao invés do Princípio da Não-Contradição, e também distinguimos claramente estes dois princípios do Princípio da Não-Trivialidade, considerando a seguir várias formulações mais fracas da explosão e investigando suas inter-relações. Em seguida, apresentamos as formulações sintá[c]ticas de alguns dos principais **C**-sistemas baseados na lógica clássica, mostramos como várias lógicas bem conhecidas da literatura podem ser reformuladas como **C**-sistemas e estudamos cuidadosamente as suas propriedades e limitações, mostrando por exemplo como tais sistemas podem ser usados para reproduzir inteiramente as inferências clássicas, apesar de constituírem eles próprios apenas fragmentos da lógica clássica, e aventuramos alguns comentários sobre as suas contrapartidas algébricas. Definimos ainda uma classe particular dos **C**-sistemas, os **dC**-sistemas, como aqueles nos quais os novos operadores de consistência e inconsistência podem ser dispensados. O escrutínio dos métodos gerais apropriados para fornecer interpretações adequadas para estas lógicas, tanto em termos de semânticas de valorações quanto em termos de semânticas de traduções possíveis, pode ser encontrado em outros artigos. O presente estudo se propõe tanto a apresentar e caracterizar do zero o campo no qual ele se insere, apontando evidentemente as conexões com o trabalho de vários autores e anotando algumas questões em aberto, quanto a apontar algumas direções para continuação, estabelecendo de passagem um arcabouço teórico unificador para a investigação ulterior por pesquisadores envolvidos com os fundamentos da lógica paraconsistente.

Contents

> These ambiguities, redundancies and deficiencies remind us of those which doctor Franz Kuhn attributes to a certain Chinese encyclopedia entitled 'Celestial Empire of Benevolent Knowledge'. On those remote pages it is written that animals are divided into (a) those that belong to the Emperor, (b) embalmed ones, (c) those that are trained, (d) suckling pigs, (e) mermaids, (f) fabulous ones, (g) stray dogs, (h) those that are included in this classification, (i) those that tremble as if they were mad, (j) innumerable ones, (k) those drawn with a very fine camel's hair brush, (l) others, (m) those that have just broken a flower vase, (n) those that resemble flies from a distance.
> —Jorge Luis Borges, *The analytical language of John Wilkins*, 1952.

I will briefly highlight in what follows some of the most significative motivations and results of the hereby included paper, 'A taxonomy of **C**-systems' —henceforth referred to as TAXONOMY.

Byzantinisms

There are several inappropriate ways of depluming a biped, and several ways of rendering one's field of research harmless and uninteresting by way of an inappropriate definition or classification. Good ol' Diogenes would certainly have found rather amusing the classification of the paraconsistent logics produced by the school founded by Newton da Costa as 'Brazilian paraconsistent logics', 'positive-plus logics', 'non-truth-functional logics', and so on. For such 'paraconsistent definitions' are at the same time too restrictive and too general, and, even at an informal level, they leave too much of reality out, on the one hand, and put too much of it in, on the other. Our TAXONOMY aims at a methodic classification of several varieties of paraconsistency and purports to make a criterious selection of what should be inside the above kingdoms and phyla, but it also gets fine-grained enough so as to talk about some specific remarkable genera and species. The corresponding resulting class of 'Brazilian paraconsistent logics' —or at least the (arguably) most interesting among them, wherever they might be produced— will in the end comprehend those, and exactly those, logics that are able to express the notion of consistency, including the surprisingly large family of logics which have been (re)christened, in the TAXONOMY, **C**-systems.

The first label, 'Brazilian paraconsistent logics' (cf. [40, 35]), certainly sounds facetious. Or does anybody think that logics have nationalities? What next, requiring a visa for some Third World logics to travel from one place to another? Perhaps this is just an inner joke of the relevantist community, being already used to separate the world between 'US' (U.S.?) and 'them' (other improbable places, such as 'Australia'), and talking about the 'American Plan' on relevance logics in contrast to the 'Australian Plan'. By the way, given that Jaśkowski's logic **D2** can be characterized as a **C**-system on our current definition of the term, it is somewhat droll to realize then that 'Polish logics' are 'Brazilian'... Or is it the other way around?

The next label, 'positive-plus logics' (cf. [33, 23]), or, even more inelegantly, 'positive logic plus approach' (cf. [32]), is supposed to designate the "logics that augment classical or intuitionist [sic] positive logic with a non-truth-functional negation" (cf. [31], p.300). This classification seems to have gained quite a few adherents, perhaps because it made some people believe that they knew what 'Brazilian paraconsistent logics' were about. Most of the time, however, the denomination was simply used in order to refer to this other category of things that they call '**C**-systems', and by the term '**C**-systems' most people mean just the original daCostian hierarchy of paraconsistent logics C_n, $1 \leq n \leq \omega$ (cf. [15, 16]). From our present point of view, this label is a bad choice for various reasons, among them: (1) because there is nothing really special about the original C_n logics, and it is easy to imagine indeed several other similar hierarchies that could take their place (cf. [19, 28]); (2) because logics such as C_ω are indeed 'positive-plus' with respect to intuitionistic logic, but do not deserve to be called **C**-systems according to our present approach; (3) because there are logics that are perfectly truth-functional (see more about this below), such as $\mathbf{J}_3$ and $\mathbf{P}^1$, that have been proposed by the Brazilian school and are very different from the original C_n, but that do fit under our present definition of **C**-systems, being 'positive-plus' with respect to classical logic and being able to express consistency. To be sure, the Brazilian school is partly to be blamed for the confusion, as it never cared to make clear what it meant by the term '**C**-system', and used it always in a very loose way. It never ceases to amaze me that people will endlessly discuss the adequacy of the use of a certain term without even trying first to make clear what they mean by it! In the TAXONOMY we do our best so as to fix this situation, starting with a precise notion of an order of expressive entities to be called 'Logics of Formal Inconsistency'. Not by mere chance, the definition that we will offer for **C**-systems as a particular family of entities from that order will make sure that those systems (be they truth-functional or not) *are* in fact 'positive-plus' with respect to some previously given consistent logical basis —though intuitionistic and classical logics will certainly not be the only possible bases for that operation.

On what concerns the third unfortunate label, 'non-truth-functional logics' (cf. [31, 34]), it is not even clear whether the people that use it know precisely what they are talking about. In [34] one can read that: "The study of non-truth-functional systems was initiated by da Costa (who has also produced several other kinds of system [sic]). The main idea here was to maintain the apparatus of some positive logic, say classical or intuitionistic, but to allow negation in an interpretation to behave non-truth-functionally." Now, even if we decide to overlook the fact that 'non-truth-functionality' certainly has nothing to do with 'positive logic', we are still left with the problem of determining what is the precise underlying notion of 'truth-functionality' that should here come into play. It is somewhat unfortunate that still nowadays people will believe, without any particular technical justification and even without a philosophical justification, that some given logic can be distinguished from another given logic by means of the semantics that might be circumstantially associated to them. This seems in fact to be one of the underlying beliefs of the important recent chapter [31] of the Handbook of Philosophical Logic, where paraconsistent logics are presented and contrasted from the point of view of some preferred semantic presentations. The first trouble with this ill-advised approach is that the *same* logic (under most definitions of what it means to say that two logics are 'the same') can often be characterized through several different semantic presentations. Consider a particular example. In [30] the same logic LP is presented twice, by way of two different sorts of 3-valued semantics. It is not difficult to see, anyhow, that this same logic can also be characterized in many other ways: by means of a non-truth-functional 2-valued semantics, a possible-translations semantics, a society semantics, a modal-like semantics, dialogues, tableaux, and so forth. Should 'truth-functional' mean that the logic has 'at least one many-valued adequate semantics'? One who defends such a definition should then recall some well-known adequacy results from General Abstract Logics (a.k.a. Universal Logic) that can be used to show that *any* 'tarskian logic', LP included, has an adequate many-valued semantics, in fact, even a 2-valued one (check **Chapter 2.1**, further on). And what does it mean for a connective to 'behave non-truth-functionally'? Does it mean that it does not have a canonical modal interpretation satisfying the replacement property? In that case, again, it should be recalled that there are many **C**-systems, in the present definition of the term, that satisfy that property (check **Chapters 3.2** and **3.3** for all usual normal modal logics recast as **dC**-systems). Perhaps the term 'non-truth-functional' should simply be avoided by those who do not feel comfortable with the topic of General Abstract Logics (otherwise, a diagonal reading of [41] or of [42] or of [38] is always recommendable).

Fortunately, at least one of the authors of [34] seems to have now adopted a more reasonable appellation: In an interesting recent paper (cf. [39]), the expression 'Brazilian school of paraconsistency' is used. It is still somewhat problematic, however, to talk about 'schools of paraconsistency' as if they had nationalities. While the expression 'Belgian school of paraconsistency' nowadays might bring to one's mind the inconsistency-adaptive logics developed by Batens and his disciples, notwithstanding the fact that there are other people in Belgium that do paraconsistency with no affiliation to nor coincidence of interests with that school, the expression 'Australian school of paraconsistency' used so as to refer to some specific developments by Priest and Routley/Sylvan is pretty abusive: In that case one might easily think instead of other 'Australian' paraconsistentists, say, those that deal more with relevance issues, such as Meyer, Slaney, and Brady. Besides, is there anyone else alive in Australia, or in the world, willing (or capable) to defend exactly the same 'dialetheist' views on logic as those systematically defended by Priest since many years? If not, why bother to talk about a 'school' that has no pupils? Finally, in the case of the so-called 'Brazilian school of paraconsistency', influenced by the work of da Costa and collaborators, the situation is even more disconcerting. The trouble is not so much that there are always people from the 'Brazilian school' that do not live and work in Brazil, but that there are many many people working on paraconsistency in Brazil at any given time, and they cannot be said to belong all to the daCostian school, or even to share common interests and tools with that school.

All that said and done, the reader will observe that the present thesis, benefiting a little from the work of each school on paraconsistency and departing freely from the received traditions when necessary, is as 'Brazilian' as it can be.

The meat

The **Section 1** of the TAXONOMY brings an extensive and detailed introduction to the contents of the paper, and it is better that you read it than that I try to further condense it here.

Section 2 contains the material that I consider to be of more immediate philosophical significance. If we shall have logics as objects of research rather than auxiliary tools that come to help on that research, we need a rich metalanguage to talk about the inferential mechanisms of these logics. This is to say that the study of logics as mother-structures in the sense of Bourbaki (cf. [8] and [5]) will need to be set in a more or less formal framework such as the one in [13], in such a way that we can schematically quantify, for instance, over theories and formulas of a given logic. The study of General Abstract Logic is anything but new, but syntactical and semantic-oriented approaches to logic certainly have collected more adherents in the present times. This section of the paper was born from my intuition that neither syntax nor semantics provide in general the right level of abstraction for a number of logical properties to be expressed.

There is an awful number of papers in the philosophical literature discussing 'logical principles' that you finish reading without having any clear idea of what the authors even *meant* by such and such a principle. This is because typically the principles are not defined or stated with any degree of precision. No wonder there is so much disagreement then on the import of such principles: It seems everybody has their private understanding of a given principle, and they will refuse to formalize it a single bit to help other people agreeing or disagreeing over that understanding. Hopefully, this hand-waving way of doing science and philosophy will become more the exception than the rule in the near future, as serious and well-trained new generations of logicians take the scene.

This section of the TAXONOMY offers precise formulations of several logical principles, assuming a logic to be a schematic structure whose universe of discourse is a set of formulas in a signature containing a symbol for negation. The logical structure is also assumed to contain a relation that represents the notion of (multiple-premise-single-conclusion) consequence. All 'decent' logics are supposed to respect the Principle of Non-Triviality (PNT), and paraconsistency is equated to the failure of the Principle of Explosion (PPS), also referred to in the paper as Pseudo-Scotus or as *ex contradictione sequitur quodlibet*. I also offer here a very particular reading of the so-called Principle of Non-Contradiction (PNC), sharply distinguishing it from the Principle of Explosion: 'Dialectic' non-trivial logics are paraconsistent logics that fail (PNC) —being often non-structural logics, to that effect; the immense majority of paraconsistent logics in the literature however are not dialectic and do not disrespect (PNC). For logics that do respect (PPS), at any rate, the principles (PNC) and (PNT) are often interderivable (Fact 2.6). A few alternative abstract definitions of paraconsistent logics are surveyed and the conditions for their equivalence to be proved are emphasized (Facts 2.7 and 2.14). Several weaker varieties of 'explosion' that are compatible with paraconsistency are also surveyed. In particular, my formulation of '*ex falso sequitur quodlibet*' does not imply paraconsistency, as it is commonly assumed in the literature: *ex falso* and *ex contradictione* are simply two distinct principles, as it is (in the present framework) easy to check. A paraconsistent logic can also have —and often has— a contrary-forming negation operator. In that case, and in general in any case in which a negation with an explosive character is present, the logic is said to respect (sPPS), the 'supplementing' form of (PPS). The role that conjunction and implication might play on relating the previous principles is elucidated (Figures 2.1 and 2.2). Other important varieties of explosion are also formulated, including a 'partial' form (pPPS) according to which not all formulas of the logic are derived from a contradiction, but all formulas with a certain format (say, all negated formulas) are so derived. Usually, paraconsistent logics are required to be 'boldly paraconsistent', avoiding both the basic form of explosion and the other partial forms, that is, they are required to fail (pPPS). The **Errata** at the end of this chapter shows, among other things, that the logics we work with in the TAXONOMY *are* boldly paraconsistent. Yet another form of explosion, a 'controllable' one (cPPS), says that at least some contradictions explode, if not all. This form of explosion is almost inevitable: Fact 3.32 later on will show that already very weak paraconsistent logics are controllably explosive, if only they are sufficiently expressive.

A final fundamental variety of explosion introduced in this section should be highlighted: the 'gentle' explosion (gPPS). Inconsistent logics can be either trivial (absolutely inconsistent) or non-explosive (paraconsistent). Paraconsistent logics are thus non-trivial logics having a negation that lacks the 'consistency presupposition'. But some paraconsistent logics —those respecting (gPPS)— are expressive enough so as to internalize the very notion of consistency at the object language level. Such logics are called Logics of Formal Inconsistency (**LFI**s). As a consequence, despite constituting fragments of consistent logics, such **LFI**s can canonically be used to faithfully reproduce all admissible consistent inferences, just by adding to them, in each case, a convenient set of 'consistency assumptions' (recall the *Fundamental Feature of* **LFI**s mentioned at the **Prolegomena** of this thesis, or at the section 2 of **Chapter 3.3**, further on). Other ways in which our **LFI**s can recover consistent reasoning by way of direct grammatical translations are illustrated in the whole of **Section 3.7** and in Theorems 3.61 and 3.67 of the present TAXONOMY. **C**-systems are defined in the end of **Section 2** as those **LFI**s that can be constructed from the positive part of given consistent logics by the addition of a single new connective to represent consistency. With the exception of one **LFI** mentioned in **Section 3.10** —the logic that constitutes the deductive limit to da Costa's hierarchy or paraconsistent logics C_n, $1 \leq n < \omega$ (cf. [11])—, all the remaining **LFI**s studied in the TAXONOMY are **C**-systems based on classical logic. The near-ubiquity of **LFI**s among paraconsistent logics is illustrated at Fact 2.19.

A few other abstract definitions can be found elsewhere on the paper that would theoretically belong to the present section. Thus, the definition of **dC**-systems as a variety of **C**-systems in which the consistency connective can be introduced through a definition in terms of more usual connectives is to be found in **Section 3.8**. Well-known examples of **dC**-systems include the C_n, $1 \leq n < \omega$, and the logic $\mathbf{P}^1$. A well-known example of a **C**-system that is not a **dC**-system is given by the logic $\mathbf{J}_3$ (or **LFI1**). A well-known example of a logic that used to be informally included among the '**C**-systems' (or '**C**-logics') and that now falls outside this class is given by da Costa's logic C_ω. It should be mentioned that almost all of the above mentioned definitions are novel, at least at the present level of precision.

As not everybody seems to have this clear in mind, there are some further small generic results about paraconsistent logics that are worth mentioning: (1) That not all contradictions are equivalent in a paraconsistent logic (Fact 2.8); (2) that disjunctive syllogism in general cannot hold good (Fact 3.19); (3) that contraposition also fails, in general (Theorem 3.20).

Section 3 is much more practically-minded. I work there in the old-fashioned way, with Hilbert-style characterizations of a few simple **C**-systems, starting with **bC** in **Section 3.2**, and then I add more and more axioms until I arrive to a class of maximal paraconsistent fragments of classical logic in **Section 3.11**. There are several results (3.14, 3.17, and many others) showing how classical reasoning can be recovered from inside **C**-systems if only a sufficient number of 'consistency assumptions' are in each case added to the premises of our inferences. Several independence results related to the axioms that we consider are stated, and in spite of the fact that they are in general not that easy to prove if a decision procedure is not available, they are not really worth mentioning here. There are many results related to the failure of the 'intersubstitutivity of provable equivalents' (IpE) (or 'replacement property') inside many of the present paraconsistent logics, and some other results (such as Theorem 3.41 and Fact 3.81) that show that a partial form of (IpE) occasionally holds at least for some particular formulas of some of our logics. On what concerns the failure of (IpE), Theorem 3.51 is noteworthy, for summarizing results from several papers together with new ones and setting some very general conditions for (IpE) to be disrespected by paraconsistent logics. The problems with (IpE) eventually evolve into serious trouble in producing non-degenerate algebraizations for many of our present logics. A survey of what was known by then and a partial classification of our **LFI**s from the point of view of Abstract Algebraic Logic in the manner of Blok-Pigozzi (cf. [7, 22]) is done in **Section 3.12**. We are now sure, however, that replacement is not really out of reach, as there are indeed many paraconsistent logics that satisfy full (IpE) —again, on that issue, remember to check **Chapters 3.2** and **3.3**.

For the interested researchers, several open problems and directions for further investigation are listed in **Section 4**.

Parts that were promised and are missing, things that will change

The Taxonomy was intended to be entirely self-contained (and it seems to have been reasonably successful on that), but it also aimed at exhaustiveness, if that is at all feasible. Thus, we also intended to deal, for example, with the semantics of the **LFI**s thereby presented, and we left there the promise to do that in a future paper. That paper is now unlikely to ever exist, having been superseded by a number of better conceived papers. Of course, the semantics of several **LFI**s (among them the 3-valued ones from **Section 3.11**) is already presented in the Taxonomy, but many other systems were left untreated. At any rate, semi-automated algorithms for defining adequate bivalent semantics for all our **LFI**s exist at least since [6], so we would not have much to contribute here. Many of the more convoluted **LFI**s from the Taxonomy had already received adequate possible-translations semantics in [28], and the paper on **Chapter 2.2** of the present thesis now shows how several of our weakest **LFI**s, none of them finite-valued, can also be interpreted in terms of a combination of specific 3-valued scenarios. The papers on **Chapter 3** show how the consistency connective can be given an adequate canonical modal interpretation, and putting that together with a modal interpretation of negation we can now talk about fully modal **LFI**s.

The maximality of the 8K 3-valued logics from **Section 3.11** with respect to classical logic was also hinted at, yet the corresponding paper, [27], is still not ready. The reader can have a very good idea of how the maximality proof works, however, if he only consults the Ap. $\omega+\omega$ of [28] or else the paper [29], where the proof is done in detail for a few logics from the above mentioned class.

Some very interesting extremely weak **LFI**s that were mentioned only in the (final) **Section 4** of the TAXONOMY are the logics **mbC** and **mCi**. They were now, however, carefully taken into account as our most basic examples of **C**-systems based on classical logic plus 'excluded middle', studied in the handbook chapter [10], an important offspring of the present dissertation. The main axiomatic and semantic properties of those logics are also studied here, in **Chapter 2.2**.

The present single-conclusion approach to consequence relations is, in a sense, 'biased towards truth' and it does not permit one to take full profit of the above mentioned general abstract definitions. Further on, in **Chapter 4**, in a multiple-conclusion framework, I will show for instance how the above Principle of Non-Triviality can be generalized so as to regulate not just one but four degenerate examples of logics. Moreover, I will also show how that framework allows us to distinguish Pseudo-Scotus from *ex contradictione*, the former principle to be failed by non-trivial inconsistent logics in general, and the latter to be failed by the 'decent' paraconsistent logics among them.

Brief history

The TAXONOMY has a somewhat winding history. During the writing of my Master's Thesis (cf. [28]), in between 1998 and 1999, I had the chance of acquiring a significant knowledge of the literature on paraconsistent logics, and specially of the variants of such logics that had been produced in Brazil in the last 40 years or so. I had no particular interest on paraconsistent logics from the start —my interest at the time laid more on formal semantics and all-purpose logical tools, and the things you can do with them. It was on and about paraconsistency, however, that I found a wealth of notable logical problems to attack with the tools I had at the time, and thus I dug into it.

Just when I finished the thesis and had all those results in hands, we were starting to organize the II World Congress on Paraconsistency (WCP'2000), that would be dedicated to Newton da Costa and that was to congregate a very international audience in Brazil in the following year. Together with Walter Carnielli, my supervisor in my then initial doctoral developments, we decided to offer at the WCP'2000 a kind of survey of the paraconsistent logics 'made in Brazil', to wit, those logics directly developed by da Costa and collaborators or at least inspired by their approach. What could be the unifying framework for reconstructing decades of variegated work in the area in just 50 minutes? We frankly had no real idea of where to start. We would certainly like to recall and generalize some fundamental ideas by da Costa (and collaborators): his 'Tolerance Principle' (cf. [14]), his initial requisites on paraconsistency (cf. [16]), the theory of valuations and bivalued semantics (cf. [18]), the intuitions on duality with paracompleteness (cf. [26]), the agnostic perspective on the existence of 'true contradictions' (cf. [17]). On the top of that, we would also like to add some new results: a couple of interesting new logics that I had been tinkering with, their possible-translations semantics, and some recent notes on troubles related to the algebraization of such logics. The lecture was announced as 'The **C**-systems: Paleontology and Futurology', and was chosen to close the congress. But when it finally came about, in May 2000, we had already chosen some very specific paths to tread. We had decided to capitalize on the notion of 'consistency' as a primitive object language notion, generalizing da Costa's notion of 'good behavior' to a whole new dimension. The idea, from the start, was that of exploring the possibility of having paraconsistent fragments of classical logic that would nevertheless be capable of recapturing classical reasoning in a very natural way. The ability to express consistency helped neatly on that. We were content as we seemed to had attained by then the right level of generality: The chosen framework was able to put together in the same class of **C**-systems logics so diverse as da Costa's 1963 logics C_n, $1 \leq n < \omega$ (cf. [15, 16]), Sette's 3-valued logic $\mathbf{P}^1$ (cf. [37]), and D'Ottaviano & da Costa's 3-valued logic $\mathbf{J}_3$ (cf. [21]), besides, as we saw later on, also Jaśkowki's 1948–49 logic **D2** (cf. [24, 25]), and Schütte-Batens logic **CLuNs** (cf. [36] and [4]); at the same time, other less expressive logics were definitively excluded from that framework, such as da Costa's logic C_ω (cf. [20, 15]), Asenjo-Priest's logic *LP* (cf. [30] and [1]), or Batens-Avron's logic *Pac* (cf. [2] and [3]).

By early September 2000 the above ideas had been much more thoroughly developed, and I gave a detailed account on them to the group of Newton da Costa at the Faculty of Philosophy, Languages and Human Sciences of the State University of São Paulo (BR). There, the outlines of the Taxonomy were first appreciated and warmly welcomed to the world. Anyhow, it was not before I went to live in Germany, a few days later, with a Capes / DAAD grant for a ProBrAl project, that the serious development of those ideas jump-started. It would in the end take me at least 7 months of hard work and require much more reading and research that I would have dreamt of. About 40% of the paper was written during that first period, and the remainder was written after March 2001, when I took up a research position in Belgium, under a Dehousse doctoral grant. My boss during this second period was Diderik Batens, and it was only with his gentle permission and the generosity of his extremely careful reading of the final version of the paper that the job got finally accomplished. I am also grateful to the editors of [9], the volume in which the paper was to appear, for their willingness to consider this very late contribution and for their rewarding choice of referees. The present version of the paper would not have completely fulfilled Pindar's injunction and 'become what it is' had it not been for the help of a few careful commentators, including Chris Mortensen, Jean-Yves Béziau, Carlos Caleiro and Marcel Guillaume. My most sincere thanks to all of them.

On coauthorship

Given that the present chapter contains the most fundamental and the longest paper from the thesis, and given that I sign the paper only as its 'second author', the question of coauthorship has been raised.

For one thing, the paper would surely not have been possible without the continuous support (and the pressure) of Walter Carnielli, my coauthor in it and my supervisor in the present thesis. I am much obliged to his help and encouragement, to the countless discussions and comments he patiently exchanged with me on the subject by e-mail, to the many attempts he made on helping me to complete the paper's writing, and to his firmness in making me put a stop to the seemingly endless task. I am glad to have had someone like him as a coauthor, always encouraging as an enthusiast of the underlying project. I am also grateful, of course, to have now someone to share the responsibility for the mistakes that have been committed in the paper and that have been found so far (check the **Errata** at the end of this chapter).

It should be clear, at any rate, that failing to acknowledge my contribution in organizing and writing the paper, setting forth its main ideas, painstakingly double-checking the related literature, proposing its definitions and theorems, and finding out all the corresponding proofs is to risk being seriously unfair. That would not be too different from failing to acknowledge, say, the work of Paul Bernays in the 2 volumes of Hilbert & Bernays's *Grundlagen der Mathematik*, or failing to acknowledge the work of Bertrand Russell in the 3 volumes of Whitehead & Russell's *Principia Mathematica*. You wouldn't like to commit that mistake.

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A Taxonomy of **C**-systems ◦•

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Abstract
The logics of formal inconsistency (**LFI**s) are paraconsistent logics which permit us to internalize the concepts of consistency or inconsistency inside our object language, introducing new operators to talk about them, and allowing us, in principle, to logically separate the notions of contradictoriness and of inconsistency. We present the formal definitions of these logics in the context of General Abstract Logics, argue that they in fact represent the majority of all paraconsistent logics existing up to this point, if not the most exceptional ones, and we single out a subclass of them called **C**-systems, as the **LFI**s that are built over the positive basis of some given consistent logic. Given precise characterizations of some received logical principles, we point out that the gist of paraconsistent logic lies in the Principle of Explosion, rather than in the Principle of Non-Contradiction, and we also sharply distinguish these two from the Principle of Non-Triviality, considering the next various weaker formulations of explosion, and investigating their interrelations. Subsequently, we present the syntactical formulations of some of the main **C**-systems based on classical logic, showing how several well-known logics in the literature can be recast as such a kind of **C**-systems, and carefully study their properties and shortcomings, showing for instance how they can be used to faithfully reproduce all classical inferences, despite being themselves only fragments of classical logic, and venturing some comments on their algebraic counterparts. We also define a particular subclass of the **C**-systems, the **dC**-systems, as the ones in which the new operators of consistency and inconsistency can be dispensed. A survey of some general methods adequate to provide these logics with suitable interpretations, both in terms of valuation semantics and of possible-translations semantics, is to be found in a follow-up, the paper [42]. This study is intended both to fully present and characterize, from scratch, the field into which it inserts, hinting of course to the connections with other studies by several authors, as well as to set some open problems, and to point to a few directions of continuation, establishing on the way a unifying theoretical framework for further investigation for researchers involved with the foundations of paraconsistent logic.

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1 THOU SHALT NOT TRIVIALIZE!

> On account of the classical principle of [non-]contradiction, a proposition and its negation cannot be both simultaneously true; thanks to this, it is not possible that a theory which is valid under the philosophical (or logical) point of view includes internal contradictions. To suppose the contrary would seemingly constitute a philosophical error.
> —Newton C. A. da Costa, [46], p. 6–7, 1958.

In the dawn of the XXI century, debates on the statute of contradiction in logic, philosophy and mathematics are still likely to raise the most diverse and animated sentiments. And this is an old story, whose first dramatic strokes can be traced back to authors as early as Aristotle (for the defense of non-contradiction), or Heraclitus (for the contrary position). Be that as it may, the fact is that in the beginning of the last century essentially the same dispute was still taking place, this time contraposing Russell to Meinong. And so it could still proceed, for centuries, if only the philosophical aspects of the dispute were touched. Even on more technical grounds, logicians of caliber, such as Alfred Tarski, would eventually speculate about that (cf. [106]):

> I do not think that our attitude towards an inconsistent theory would change even if we decided for some reason to weaken our system of logic so as to deprive ourselves of the possibility of deriving every sentence from any two contradictory sentences. It seems to me that the real reason of our attitude is a different one: We know (if only intuitively) that an inconsistent theory must contain false sentences; and we are not inclined to regard as acceptable any theory which has been shown to contain such sentences.

Against such suspicions, the philosopher Wittgenstein, who had devoted almost half of his late work to the philosophy of mathematics and used to refer to it as his ‘main contribution’ (cf. the entry *Mathematics*, in [63]), would have had something to say. Indeed, he often felt puzzled about ‘the superstitious fear and awe of mathematicians in face of contradiction’ (cf. [109], Ap.III–17), and asked himself: ‘Contradiction. Why just this *one* spectre? This is surely much suspect.’ (id., IV–56). His point was that ‘it is one thing to use a mathematical technique consisting in the avoidance of contradiction, and another thing to philosophize against contradiction in mathematics’ (id., IV–55), and that it was necessary to remove the ‘metaphysical thorn’ stuck here (id., VII–12). In this respect, the philosopher described his own objective as precisely that of altering the *attitude* of mathematicians concerning contradictions (id., III–82).

The above passage from da Costa’s [46] could also be directed upon criticizing a position such as the above one of Tarski. The presupposition to be challenged here, of course, is that of an inconsistent theory obligatorily containing false sentences. Thus, if models may be described of structures in which some (but not all) contradictory sentences are simultaneously true, we will have a technical point against such suspicions of impossibility or implausibility of maintaining contradictory sentences inside of some theory and still being able to perform reasonable inferences from that, *instead* of being able to derive arbitrarily other sentences. This is sure to make a point in conferring to the task of studying the behavior of contradictory yet non-trivial theories —the task of *paraconsistency*— some respectability. And it *is* indeed possible to assign models for inconsistent non-trivial theories, even if these were to be regarded by some as epistemologically puzzling, or ontologically perplexing! Obtaining models and understanding their role is certainly an extraordinarily important mathematical enterprise: Enourmous efforts from the most brilliant minds and more than twenty centuries were required until mathematicians would allow themselves to consider models in which, given a straight line S and a point P outside of it, one could draw not just one line, but infinite, or no parallel lines to S passing through P, as in the well-known case of non-Euclidean geometries. In the present case, then, the problem will not be that of *validating falsities*, but that of *extending our notion of truth* (an idea further explored, for instance, in [28]).

At that same decisive moment, in the first half of the last century, there were in fact these other people like Łukasiewicz or Vasiliev who were soon proposing relativizations of the idea of non-contradiction, offering formal interpretations to formal systems in which this idea did not hold, and in which contradictions could make sense. And in between the 40s and the 60s the world would finally be watching the birth of the first real operative systems of paraconsistent logic (cf. Jaśkowski's [67], Nelson's [86], and da Costa's [49]). But the paleontology of paraconsistent logic will not be our main subject here —for that we prefer to redirect the reader to some of the following articles [6], [59], [55], and those in section 1 of [95], plus the book [26].

1.1 Contradictory theories do exist. Be them a consequence of the only correct description of a contradictory world (as assumed in [90]), be them just a temporary state of our knowledge, or again the outcome of a particular language that we have chosen to describe the world, the result of conflicting observational criteria, superpositions of worldviews, or simply, in science, because they result from the best theories available at a given moment (cf. [14]), contradictions are presumably unavoidable in our theories. Even if contradictory theories were to appear only by mistake, or perhaps by some Janus-like crooked behavior of their proposers, it is hard to see, given for instance results such as Gödel's incompleteness theorems, how contradictions could be prevented from even being taken into consideration. So it should be clear that the point here is not about the *existence* of contradictory theories, but about *what we should do* with them! Should these theories be allowed to explode and derive anything else, as in classical logic, or rather should we try to substitute the underlying logic, in (potentially) critical situations, in order to still be able to draw (if only temporarily, if you want) reasonable conclusions from those theories?

At this point it is interesting to consider the following motto set down by Newton da Costa, one of the founders of modern paraconsistent logic (cf. [47]):

> From the syntactical-semantical standpoint, every mathematical theory is admissible, unless it is trivial.

Da Costa designated that motto 'Principle of Tolerance in Mathematics', in analogy to the 'syntactical' principle proposed before by Carnap (cf. [35], p.52). According to this, the dividing line in between systems worthy of investigation and those that do not 'make a difference' (cf. [55]), nor convey any information (cf. [14]), should be drawn around non-triviality, rather than in the vicinity of non-contradictoriness. This will give us the first key to paraconsistency: if there are no contradictions around, then everything is under control, once we are inside of a consistent environment; but if contradictions are allowed, non-triviality should be the aim —but then what we must control is the *explosive* character of our underlying logic. Indeed, inside of a consistent logic we know that contradictions are dangerous in a theory precisely because they will give sufficient reason for this theory to explode, deducing anything else!

So, given a logic whose language includes a negation symbol $\neg$, let's call *contradictory* a theory from which some formula A and its negation $\neg A$ can be derived by way of the underlying logic. Let's also call a theory *trivial* if any formula B can be derived from it by way of the underlying logic, and call a theory *explosive* if the addition to it of any contradiction A and $\neg A$ is sufficient to make it trivial. The underlying logic, in its own right, will also be called *contradictory*, *trivial*, or *explosive* if, respectively, all of the theories about which it can talk are contradictory, trivial, or explosive. To be sure, any trivial theory / logic will also turn to be contradictory, whenever there is a negation available (anything is derived from it, in particular all pairs of formulas of the form A and $\neg A$). Inside classical or intuitionistic logic, and, in a general way, inside any 'consistent' logic (this will be defined in what follows), the contradictory and the trivial theories simply coincide, by way of their explosive character. *Paraconsistent logics* were then proposed to be the logics to underlie those contradictory theories which were still to be kept non-trivial, and what those logics must of course effect to such an end is weakening or annulling the explosive character of these theories.[1] So, all at once, paraconsistency comes and provides a sharp distinction in between the logical notions of contradictoriness, explosiveness, and triviality.

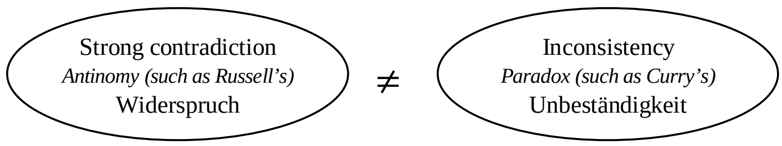
Anyone working as a knowledge engineer, assembling and managing knowledge databases, will be perfectly aware that gathering inconsistent information is the rule rather than the exception. And again, either if you assume, by some sort of methodological requirement, inconsistent theories to be problematic (cf. [14]) or not (cf. [90]), this does not prevent you from also assuming them to be, in general, quite *informative*, and wanting to *reason* from them in a sensible way. Consider, for instance, this very simple situation (cf. [40]) in which you ask two people, in the due course of an investigation, a 'yes-no' question such as 'Does Dick live in Arizona?', so that what will result will be exacly one of the three following different possible scenarios: they might both say 'yes', they might both say 'no', or else one of them might say 'yes' while the other says 'no'. Now, it happens that in neither situation you may be sure about where Dick really lives (unless you trust some of the interviewees more than the other), but only in the last scenario, where an inconsistency appears, *are you sure* to have received wrong information from one of your sources!

Our next point is that also the logical notions of *inconsistency* and of *contradictoriness* can and should be *distinguished* in a purely abstract way. Distinctions have already been proposed, in the literature, among the notions of *paradoxical* and of *antinomical* theories (cf., for instance, Arruda's [6], p.3, or da Costa's [51], p.194), the paradoxical ones being identified with those theories in which inconsistencies could occur without necessarily leading to trivialization, and the antinomical ones identified with those in which any occurring contradiction turns out to be fatal, as in the case of Russell's antinomy in naive set theory. Let us, here, insist on this distinction for a moment and stretch it a bit further. One first difficulty to be confronted with is that of some English technical terms: It is such a pity that techniques and results such as Hilbert's witch-hunt programme in search of a *Widerspruchfreiheitbeweis* for Arith-

[1] Surprising as it may seem, this would also have been the advice given by Wittgenstein on how to proceed in the presence of contradictions: 'The contradiction does not even falsify anything. Let it lie. Do not go there.' (cf. [110], XIV, p.138) For the relations and non-relations between Wittgenstein and the paraconsistent enterprise the reader may consult, for instance, [75], [64] or [77].

metic were to be eventually translated into the search for a ‘consistency proof’, given that what it literally means is something much more precise, namely a ‘proof of freedom from contradictions’! More often than not, German language indeed shows itself to be exceedingly precise, so that we should rather stick here to the literal meaning of *Widerspruchfreiheit* as non-contradictoriness, and associate inconsistency, if we may, with something like the term *Unbeständigkeit* (or any other synonym of *Inkonsistenz* together perhaps with some terms opposed to *Beschaffenheit* and to *Widerstandsfähigkeit*). Now, antinomies will be related to the presence of ‘strong’ contradictions —those with explosive behavior—, while paradoxes will be related to the presence of inconsistencies, which do not necessarily depend on negation, such as in the case of the well-known Curry’s paradox (cf. [45]). Let us try to summarize this whole story in a picture (maybe you do not agree on our choice of names, but we beg you to stick to our terminology for the moment):

Figure 1.1



The above distinctions, of course, are more illustrative than formal (nothing prevents you, for instance, from thinking of Russell’s antinomy as something not as destructive as it was, if you just change the underlying logic of its theory so as to make it only paradoxical, but the distinction between antinomies as involving the notion of strong negation, on the one hand, and inconsistencies, on the other hand, as something more general and in principle independent of negation, should be taken more seriously).

Now, whatever an inconsistency might *mean*, be it more general or not than a simple contradiction, we may certainly presuppose that a contradiction is at least an *example* of an inconsistency, be it the only possible one or not. Traditionally, as we have noted a few paragraphs above, the contradictoriness of a given theory / logic was to be identified with the fact that it derives at least some pairs of formulas of the form A and $\neg A$, while inconsistency was usually talked about as a model-theoretic property to be guaranteed so that our theories can make sense and talk about ‘real existing structures’. Of course, any trivial theory / logic, thus, given our assumption above that contradictions entail inconsistencies, will also be both contradictory and inconsistent. Now, if explosiveness does not hold, as we shall see, in the scope of paraconsistent logics, there is in principle no reason to suppose that the converse would also be the case, and that a contradiction would always lead to trivialization. How to reconcile these concepts then? Da Costa’s idea, when proposing his first paraconsistent calculi (cf. [49]), was that the ‘consistency’ and the ‘classic-like behavior’ (he called that ‘well-behavior’) of a given formula, as a sufficient requisite to guarantee its explosive character, could be represented as simply another formula of its underlying logic (he chose, for his first calculus, C_1, to represent the consistency of a formula A by the formula $\neg(A\wedge\neg A)$, and referred to this last formula —to be intuitively read as saying that ‘it is not the case that both A and $\neg A$ are true’—, as a realization of the ‘Principle of Non-Contradiction’, conventions that we will here, in general, *not* follow —neither will we follow, necessarily, the identification of consistency with 'classic-like behavior'). In fact, our proposal here, inspired by da Costa's idea, is exactly that of introducing consistency as a *primitive notion* of our logics: the paraconsistent logics which internalize the notion of consistency so as to introduce it already at the object level will be called *logics of formal inconsistency* (**LFI**s). And, given a consistent logic **L**, the **LFI**s which extend the positive basis of **L** will be said to constitute **C**-*systems based on* **L**. Our main aim in this paper, besides making all the above definitions and their multiple shades and interrelations entirely clear, will be that of studying a large class of **C**-systems based on classical logic (of which the calculi C_n of da Costa will be but very particular examples).

On what concerns this story about regarding consistency as a primitive notion, the status of points, lines and planes in geometry may immediately be thought of, but the case of (imaginary) complex numbers seems to make an even better comparison: even if we do not know what they are, and may even suspect there is little sense in insisting on which way they can exist in the 'real' world, the most important aspect is that it is possible to calculate with them. Girolamo Cardano, who first had the idea of computing with such numbers, seems to have seen this point clearly —he failed, however, to acknowledge the importance of this; in 1545 he wrote in his *Ars Magna* (cf. [87]):

> Dismissing mental tortures, and multiplying $5+\sqrt{-15}$ by $5-\sqrt{-15}$, we obtain $25-(-15)$. Therefore the product is 40. …and thus far does arithmetical subtlety go, of which this, the extreme, is, as we have said, so subtle that it is useless.

His discovery, that one could operate with a mathematical concept independent of what our intuition would say and that usefulness (or something else) could be a guiding criterion for accepting or rejecting experimentation with mathematical objects, definitely contributed to the proof of the Fundamental Theorem of Algebra by C. F. Gauss in 1799, before which complex numbers were not fully accepted.

To make matters clear, the basic idea behind the internalization of consistency inside our logics will be, in general, accomplished by the addition of a unary connective expressing consistency (and usually also another connective to express inconsistency), plus the following important assumption, that *consistency* is exactly what a theory might be lacking in order to deliver triviality when exposed to a contradiction.[2] Recapitulating: as we said before, triviality entails contradictoriness (if a negation is present), and contradictoriness entails inconsistency (or, to be more precise, contradictoriness entails 'non-consistency', for it may happen, as we will see, that consistency and inconsistency are not exactly dual in some of our logics, if we take both notions as primitive); now we just add to this the assumption that contradictoriness *plus* consistency implies triviality! We are in fact introducing, in this way, a novel definition of consistency, more fine-grained than the usual model-theoretic one: for a large class of logics (see FACT 2.14(ii)) it will turn out that consistency may be identified with the presence of *both* non-contradictoriness and explosive features.

[2] It is interesting to notice, by the way, that this assumption is remarkably compatible with Jaśkowski's intuition on the matter. As he put it, 'in some cases we have to do with a system of hypotheses which, *if subjected to a too consistent analysis*, would result in a contradiction between themselves or with a certain accepted law, but which we use in a way that is restricted so as not to yield a self-evident falsehood' (our italics, see [67], p.144). It is clear that we can give this at least one reading according to which Jaśkowski seemed already to have been worried about the effects of consistent contradictions!

Now, non-contradictoriness will be a necessary but *no more* a sufficient requirement for us to prove consistency. In the case of explosive logics, of course, the concepts of non-contradictoriness and non-triviality will coincide, so that non-contradictoriness and consistency are also to be identified. Paraconsistent logics are situated exactly in that terra incognita which lays in between non-explosive logics and trivial ones, and they comprehend exactly those logics which are both non-explosive and non-trivial (examples of such logics are provided by the whole of the literature on paraconsistent logics)! So, again, consistency divides the logical space in between consistent (and so, explosive and non-contradictory) logics, and inconsistent ones, and these last ones may, at their turn, be either paraconsistent (and so, non-explosive, and possibly even contradictory), or trivial.

1.2 Paraconsistent, but not contradictory! In fact, there is another point that we want to stress here, for it seems that much confusion has been unnecessarily raised around it. In general, paraconsistent logics do *not* validate contradictions or invalidate anything like the 'Principle of Non-Contradiction' (though there are a few that do). Most paraconsistent logics, actually, are just fragments of some other given consistent logic (such as some version of classical logic, or else some normal modal logic), so that they *cannot*, in any case, be contradictory! However, a good way of making this whole point much less ambiguous (even though still open to dispute, but now on a different level) is by considering formal definitions of those so-called (meta)logical *principles*.

Let us say that a logic respects the *Principle of Non-Contradiction*, (PNC), if it is non-contradictory, according to our previous definitions, that is, if it has non-contradictory theories, that is, theories in which no contradictory pair of formulas A and $\neg A$ may be inferred. Let us also say that a logic respects the *Principle of Non-Triviality*, (PNT), (a realization of da Costa's Principle of Tolerance inside of the logical space) if it is non-trivial, thus possessing non-trivial theories, and say that a logic respects the *Principle of Explosion*, or *Pseudo-Scotus*, (PPS), if it is explosive, that is, if all of its theories explode when in contact with a contradiction. It is clear now that all paraconsistent logics, by their very nature, must disrespect (PPS), aiming to retain (PNT), but it is also clear that they cannot disrespect (PNC) as long as they are defined as fragments of other logics that do respect (PPS)! The gist and legacy of paraconsistent logic indeed lies in showing that logics may be constructed in which the Principle of Pseudo-Scotus is controlled in its power, and this has 'in principle' *nothing to do* with the validity or not of the Principle of Non-Contradiction as we understand it. Yet a few logics exist which are not only paraconsistent, but that in fact disrespect (PNC). Such logics are usually put forward in order to formalize some dialectical principles, and are accordingly known as *dialectical logics*. Being able to infer contradictions, however, such dialectical logics cannot be fragments of any consistent logic, and in order to avoid trivialization they should also usually assume, for instance, the failure of Uniform Substitution, at least when applied to some specific formulas, such as the contradictions that those logics can infer (or else any other contradiction, and thus any other formula, would be inferable). Much weaker versions of the Principle of Non-Contradiction have nevertheless been considered in the literature, as for instance the following one, deriving from semantical approaches to the matter: a logic is said to respect the *Principle of Non-Contradiction, second* *form*, (PNC2), if it has non-trivial models for pairs of contradictory formulas. But then, of course, every model for the falsification of (PPS), that is, every model for a paraconsistent logic, would also satisfy (PNC2), and vice-versa, so that not only would (PNC2) be unnecessary as a new principle, but there would also be no principle dealing specifically with the existence of dialectical logics. Too bad! And, of course, there is a BIG difference in having models for some specific contradictions and having *all* models of a given logic validating some contradictory pair of formulas — this amounts, in the end, to the same difference which exists, in classical logic, in between contingent formulas, on the one hand, and (tautological or) contradictory ones, on the other hand…

The above definition of (PNC) will also prevent us from identifying this principle, inside some arbitrary given logic **L**, with the validity in **L** of some particular formula, such as $\neg(A\wedge\neg A)$ (as in da Costa's first requisite for the construction of his paraconsistent calculi —check the subsection **3.8**). But it *is* true that such a formula can, as well as many other formulas, be identified, in some situations, to the expression of consistency inside of some specific logics, such as da Costa's C_1! Let us, in general, say that a theory Γ is *gently explosive* when there is always a way of expressing the consistency of a given formula A by way of formulas which depend only on A, that is, when there is a (set of) formula(s) constructed using A as their sole variable and that cannot be added to Γ together with a contradiction A and $\neg A$, unless this leads to triviality. A gently explosive logic, then, is exactly a logic having only gently explosive theories, and we can now formulate (gPPS), a 'gentle version' of (PPS), for a given logic **L**, asserting that this logic must be gently explosive. Gently explosive paraconsistent logics, thus, are precisely those logics that we have above dubbed **LFI**s, the logics of formal inconsistency. In the logics we will be studying in this paper, we will in general assume that the consistency of each formula A can be expressed by operators already at their linguistic level, and in the simplest case this will be written as $\circ A$, where '$\circ$' is the 'consistency connective'. The **C**-systems (in this paper they will be supposed to be based on classical logic), will be particular **LFI**s illustrating some different ways in which one can go on to axiomatize the behavior of this new connective.

There are also some other forms of explosion, as the *partial* one, which does not trivialize the whole logic, but just part of it (for instance, when a contradiction does not prove every other formula, but does prove every other *negated* formula). We will let our paraconsistent logics also reject this kind of explosion. There is *ex falso*, which asserts that at least one element should exist in our logics so that everything follows from it (a kind of *falsum*, or *bottom particle*). There is *controllable* explosion, which states that, if not all, at least some of our formulas should lead to trivialization when taken together with their negations. And, finally, there is *supplementing* explosion, which states that our logics should possess, or be able to define, a *supplementing*, or *strong* negation, to the effect that strongly negated propositions (that we have above called strong contradictions) should explode. (There are also all sorts of combinations of these forms of explosions, and perhaps some other forms still to be uncovered, but these are the ones we will concentrate on, here.) All of these alternative forms of explosion can be turned into logical (meta)principles, and none of these rejects, by their own right, 'full' Pseudo-Scotus —all of them, nonetheless, can still be held even when the Pseudo-Scotus does not hold! The paraconsistent logics studied in this paper will, of course, disrespect Pseudo-Scotus, and in addition to that they will also disrespect the principle regarding partial explosion, while, in most cases, they will still respect the principles regarding gentle explosion, *ex falso*, supplementing explosion, and, often, controllable explosion as well. This will be made much clearer in section **2**, where this study will be made more precise, and the interrelations between all of those principles will be more deeply investigated.

1.3 What do you mean? Let's now briefly describe the exciting things that await the reader in the next sections (we will skip section **1** in our description —you are reading it—, but do not stop here!).

Section **2** is *General Abstract Nonsense*. No particular systems of paraconsistent logic are studied here (though some are mentioned), but most of the definitions and preprocessed material that you will need to understand the rest are to be found in this section. There is nothing for you to lose your appetite —you can actually intensify it, even if, or especially if, you do not agree with some of our positions. We first make clear what we mean by *logics*, introduced by their *consequence relations*, and what we mean by *theories* based on these logics, and on the way you will also learn what *closed theories* and *monotonicity* mean, and what it means to say that a logic is a *fragment* or an *extension* of another logic. This is just preparatory work. We then introduce the logical notions of *contradictoriness*, *explosiveness* and *triviality*, concerning theories and logics, and pinpoint some immediate connections between these notions. This already takes us to the subsection **2.1**, where the first logical (meta)-principles are introduced, namely the principles of non-contradiction, (PNC), of non-triviality, (PNT), and of explosion, (PPS) (a.k.a. Pseudo-Scotus, or *ex contradictione*). You will even learn a little bit about the (pre-)history of these principles, their interrelations, and some confusions about them which lurk around. Some of their ontological aspects are also lightly touched. The subsection **2.2** brings us to *paraconsistent logics*, formulated in two equivalent (but not necessarily so) presentations, one of them saying that they should allow for contradictory non-trivial theories, the other one saying that they must disrespect (PPS). After you learn what it means to say that two given formulas / theories are *equivalent* inside some given logic, FACT 2.8 will call your attention to the discrimination that paraconsistent logics ought to make between contradictions: they cannot be all equivalent inside such logics. *Dialectical logics*, being those logics disrespecting (PNC), are mentioned to fill the gaps in the general picture, but they will not be studied here. In the subsection **2.3** we start talking about finite trivializability, and look at some remarkable examples of this phenomenon, as for example the one of a logic having *bottom particles* —thus respecting a principle that we call *ex falso*, (ExF)—, and the one of a logic having *strong negations* —and respecting a principle we call *supplementing explosion*, (sPPS). We also consider some properties of adjunction (and so, of conjunction), and in the end we draw a map to show the relationships between (PPS), finite trivializability, (ExF) and (sPPS), noting that no two of these principles are to be necessarily identified (and, in particular, *ex falso* does not coincide with *ex contradictione*). Pay special attention to FACT 2.10(ii), in which all non-trivial logics respecting *ex falso* are shown to have strong negations. Subsection **2.4** considers what happens when one says farewell to (PPS) but still maintains some of the other special forms of explosion exposed before and hints are given as to some disadvantages presented by paraconsistent logics which disrespect all of those principles at once. Some other misunderstandings about the construction of paraconsistent logics are discussed, and the difference between contradictoriness and inconsistency is finally called into scene. Logics respecting a so-called principle of *gentle explosion*, (gPPS), are introduced as the ones in which *consistency* can be expressed, and even a *finite* version of gentle explosion, (fgPPS), is considered, as a particular case of finite trivialization. *Logics of formal inconsistency*, **LFI**s, are then defined to be exactly those respecting (gPPS) while disrespecting (PPS), and the great majority of the **LFI**s that we will be studying in the following will actually also respect (fgPPS). The new definition of consistency that we introduce is shown, for a given logic, in general to coincide simply with the sum of (PPS) and (PNT). Systems of paraconsistent logic known as *discussive* (or *discursive*) *logics* are shown to be representable as **LFI**s. In the subsection **2.5**, the principles of *partial explosion*, (pPPS), and *controllable explosion*, (cPPS), are finally introduced. A *boldly* paraconsistent logic is defined as one in which not only (PPS) but also (pPPS) is disrespected, and we try to concentrate exclusively on such logics. Classical logic respects all of the above principles, but for each of those principles, except (PNT), examples will be explicitly presented, or at least referred to, at some point or another, of logics disrespecting it. Multiple connections between those principles are exhibited not only along these lines but also in the section **3**. In this respect, pay also special attention to FACT 2.19 and the comments around it, in the subsection **2.6**, which show that the **LFI**s are ubiquitous: an enormous subclass of the already known paraconsistent logics can have its members recast as logics of formal inconsistency. **C**-systems are also introduced in this last subsection, and the map of the paraconsistent land presented in the subsection **2.4** gets richer and richer.

Section **3** brings a very careful syntactical study of a large class of **C**-systems based on classical logic. Each new axiom is justified as it is introduced, and its effects and counter-effects are exhibited and discussed. The systems presented are initially linearly ordered by extension, but soon spread out in many directions. The remarkable unifying character of our approach in terms of **LFI**s is made clear while most logics produced by the 'Brazilian school' in the last forty years or so are shown to smoothly fit the general schema and together make up a whole coherent map of **C**-land. Subsection **3.1** presents a kind of minimal paraconsistent logic (for our purposes), called C_{min} and constructed from the positive part of classical logic by the addition of (the axiom which represents the principle of) excluded middle, plus an axiom for double negation elimination. The Deduction Metatheorem holds for this logic and its extensions, and C_{min} is shown to be paraconsistent. Comparisons are drawn between C_{min} and one of its fragments, da Costa's C_ω, and the facts that no strong negation, or bottom particle, or finitely trivializable theory, or negated theorem are to be found in these logics are mentioned. You will also learn that, in these logics, no two different negated formulas are provably equivalent. Of course, as a consequence of these last facts, these logics cannot be **LFI**s, what to say **C**-systems, but the **C**-systems which will be studied in the following subsections are all extensions of them. We make some observations about versions of *proof by cases* provable in these logics, by way of excluded middle, and we adjust some of its axioms to better suit deduction. A way to turn these logics into classical logic, simply by adding back the Pseudo-Scotus to them, is also demonstrated.

In subsection **3.2**, we introduce the *basic logic of (in)consistency*, called **bC**, by adding a new axiom to C_{min}, and we show how to immediately extract from this axiom a strong negation and a bottom particle. We now have '$\circ$', the *consistency* connective, at our disposal as a new primitive constructor in our language, realizing the finite gentle explosion. The logic **bC**, which is, in fact, a conservative extension of C_{min}, is shown already to have negated theorems and equivalent negated formulas, but on the other hand it does not have any provably consistent formulas. Sufficient and necessary conditions for a **bC**-theory to behave classically are presented. The axiom defining **bC** define a kind of restricted *Pseudo-Scotus*, as obvious. Some related restricted forms of *reductio ad absurdum* which are also present are studied, and the elimination of double negation shows its purpose in THEOREM 3.13, where you will learn that some forms of partial trivialization are avoided by all paraconsistent extensions of **bC**. Restricted forms of *reductio* deduction and inference rules are shown to be present in **bC**, and some other rules relating contradictions and consistency are exhibited. No paraconsistent extensions of **bC** will contain the formula $(A\wedge\neg A)$ as a bottom particle (but some other **LFI**s, such as Jaśkowski's **D2** —at least under some presentations— do have it as a bottom particle). A formula such as $\neg(A\wedge\neg A)$ is also not provable in **bC**, but can be proved in some of its extensions, such as the three-valued maximal paraconsistent logics **LFI1** and **LFI2**.

Subsection **3.3** is mostly composed of negative results. It starts by showing that not many rules making the interdefinition of connectives possible hold in **bC**. The reader will also learn about the obligatory failure of *disjunctive syllogism* in vast extensions of the paraconsistent land, and the failure of 'full' *contraposition* inference rules in **bC** and all of its paraconsistent extensions, though some *restricted* forms of it had already been shown to hold, in the previous subsection. The uses of disjunctive syllogism and of contraposition to derive the Pseudo-Scotus had already been pointed out a long ago, respectively, by C. I. Lewis (and, much before, by the 'Pseudo-Scotus' himself), and by Popper. Some asymmetries related to negation of equivalent formulas are pointed out, and as a result it will not be possible to prove a *replacement* theorem for **bC**, which would establish the validity of *intersubstitutivity of provable equivalents*, (IpE), and the same phenomenon will be observed in most, but not all, of **bC**'s extensions. Reasons for all these failures, and possible solutions for them, are discussed.

In subsection **3.4** the problem of adding an inconsistency connective '$\bullet$' to **bC**, intended as dual to '$\circ$', making inconsistency coincide with non-consistency, and consistency coincide with non-inconsistency, is shown to be not as easy as it may seem, as a consequence of the last negative results. Some intermediary logics obtained in the strive towards the solution of this problem are exhibited, and hints are given on how this solution should look. A first solution, adopted, in fact, in the whole literature, is to make inconsistency equivalent to contradiction, and this is exactly what the logic **Ci**, introduced in the subsection **3.5**, does. It will *not* be the case, however, that consistency in **Ci** can be identified with the negation of a conjunction of contradictory formulas, that is, the consistency of a formula A will not be equivalent to any formula such as $\neg(A\wedge\neg A)$. New forms of gentle explosion and restricted contraposition deduction rules are shown to hold in **Ci**, and provably consistent formulas in **Ci** are shown to exist. Indeed, the notable FACT 3.32 shows that provably consistent formulas in **Ci** coincide with the formulas causing controllable explosion in this logic and in all of its extensions. Some more restricted forms of contraposition inference rules introduced by **Ci** are also exhibited, and the failure of (IpE) also for this logic is pointed out. In fact, as we already know that full contraposition cannot be added to **Ci** in order to get (IpE), some weaker contraposition deduction rules which would also do the job are tested, and these are also shown to lead to collapse into classical logic (see FACT 3.36). But there still can be a chance of obtaining (IpE) in extensions of **Ci** by the addition of even weaker forms of contraposition deduction rules, as the reader is going to see at the end of subsection **3.7**, where positive results for this are shown for extensions of **bC**. The connectives '$\circ$' and '$\bullet$' have a good behavior inside of **Ci**, and we show that any formula having them as the main operator is consistently provable, and that consistency propagates through negation (and inconsistency back-propagates through negation). In addition to that, schemas such as $(A \to \neg\neg A)$, which are shown *not* to hold in the general case, are indeed shown to hold if A has the form $\circ B$, for some B. All of this comes either as an effect or a consequence of the fact that a restricted replacement is valid in **Ci**, as proved in the subsection **3.6**, to the effect that inconsistency here can be really introduced, by definition, as non-consistency, or else consistency can be introduced, by definition, as non-inconsistency.

Subsection **3.7** shows how to compare the previously introduced **C**-systems with an extended version of classical logic, **eCPL** (adding innocuous operators for consistency and inconsistency). As a result, we can show that the strong negation that we had defined for **bC** does not have all properties of classical negation, but another strong negation can be defined in **bC**, which *does* have a classical character. In **Ci** these two negations are shown to be equivalent, but the interesting output of a strong classical negation is making it possible for us to conservatively translate classical logic inside of all our **C**-systems (THEOREM 3.46 and comments after THEOREM 3.48), so that any classical inference can be faithfully reproduced, up to a translation, inside of **bC** or of any of its extensions. About **Ci**, we can now prove that it has some redundant axioms, and the remarkable FACT 3.50, showing that only consistent or inconsistent formulas can themselves be consistently provable in this logic, and so these are the only formulas that can cause controllable explosion in **Ci**. An even more remarkable THEOREM 3.51 shows several conditions which *cannot* be fulfilled by paraconsistent systems in order to render the proof of full replacement, (IpE), possible. But paraconsistent extensions of **bC** in which (IpE) holds are indeed shown to exist, the same task remaining open for extensions of **Ci**.

Subsection **3.8** presents the **dC**-*systems*, which are the **C**-systems in which the connectives '$\circ$' and '$\bullet$' can be dispensed, definable from some combination of the remaining connectives. The particular combinations chosen by da Costa in the construction of his calculi C_n are surveyed, and we start concentrating more and more on general parallels of da Costa's original requisites for the construction of paraconsistent calculi (which does not mean that we shall feel obliged to obey them *ipsis litteris*). Criticisms on the particular choices made by da Costa and some of their consequences are surveyed. Again, in a particular case, that of da Costa's C_1, the consistency of a formula A is identified, as we have said before, with the formula $\neg(A \wedge \neg A)$, and the extension of **Ci** which makes this identification is called **Cil**. This system is shown still to suffer some strange asymmetries related to the negations of equivalent formulas (for instance, $\neg(\neg A \wedge A)$ is not equivalent to $\neg(A \wedge \neg A)$), and some partial or full solutions to that are discussed at that point and below. Connections between our **C**-systems and *relevance* or *intuitionistic* logics are touched. In particular, the problem of defining **C**-systems based on intuitionistic, rather than classical, logic is touched, but no interesting solutions are presented (because they seem still not to exist, but perhaps the reader will have the pleasure of finding them in the future).

In subsection **3.9**, **dC**-systems are put aside for a moment and the addition of an axiom for the introduction of double negation is considered, together with its consequences. Arguments, both positive and negative, for the 'proliferation of inconsistencies' that such an axiom could cause are presented, and rejected. Subsection **3.10** surveys various ways in which consistency (and inconsistency) can propagate from simpler to more complex formulas and vice-versa. One of these forms, perhaps the most basic one, is illustrated by an extension of **Ci**, the logic **Cia** (or else an extension of **Cia**, the logic **Cila**, which was recorded into history under the name C_1), and this logic will be shown to make possible a new and interesting conservative translation from classical logic inside of it, or any of its extensions. If all the reader wants to know about is da Costa's original calculi C_n, this subsection is the place (together with some earlier comments in the subsection **3.7**), but in that case be warned: You may miss most of the fun! Properties of the C_n are surveyed, and the problem of finding a real *deductive limit* to this hierarchy is presented together with its solution, so that the reader can forget once and for all any ideas they may have had about the logic C_ω having its place as part of this hierarchy. In particular, the deductive limit of the C_n, the logic C_{Lim}, is shown to constitute an **LFI**, though we are not sure if its form of gentle explosion can be made finite. Again, (IpE) is shown not to hold for the calculi C_n, so that *Lindenbaum-Tarski*-like algebraizations for these logics can be forgotten, but the situation for them is actually worse, for it has been proven that they just cannot define any non-trivial congruence, putting aside also the possibility of finding *Blok-Pigozzi*-like algebraizations for them. But several extensions of the C_n can indeed fix this last problem, and so we try to concentrate on some stronger forms of propagation of consistency which will help us with this. In particular, the logics C_1^+ (later proposed by da Costa and his disciples), as well as five three-valued logics, $\mathbf{P}^1$, $\mathbf{P}^2$, $\mathbf{P}^3$, **LFI1** and **LFI2**, proposed in several studies, are also axiomatized and studied as extensions of **Ci**. An increasingly detailed and clear map of the **C**-systems based on classical logic is being drawn.

In the subsection **3.11**, the last five three-valued logics are shown to constitute part of a much larger family of 8,192 three-valued paraconsistent logics, each of them proven to be axiomatizable as extensions of **Ci** containing suitable axioms for propagation of consistency. Each of these three-valued logics can also be shown to be *distinct* from all of the others, and *maximal* relative to classical logic, **eCPL**, solving one of the main requisites set down by da Costa, to the effect that 'most rules and schemas of classical logic' should be provable in a 'good' paraconsistent logic. We also count how many of those 8,192 logics are in fact **dC**-systems, and not only **C**-systems, and show many *connections* between them and the other logics presented before, all of them fragments of some of these three-valued maximal paraconsistent logics. Interestingly, $\mathbf{P}^1$ is shown to be conservatively translatable inside of any of the other three-valued logics, and all of these are shown to be conservatively translatable inside of **LFI1**. (IpE) is proven not to hold in any of these logics, but there are some other interesting connectives which they can define, as some sort of *'highly' classical* negation, and *congruences* which will make possible, in subsection **3.12**, the definition of non-trivial (Blok-Pigozzi) algebraizations of all of these three-valued logics. Indeed, the subsection **3.12** surveys positive and negative results regarding algebraizations of the **C**-systems.

Section **4** sets some exciting open problems and directions for further research, for the reader's recreation.

1.4 Standing on the shoulders of each other. It would be very unwise of us to present this study, which includes a technical survey of its area, without trying to connect it as much as possible to the rest of the related literature. But we pledge to have done our very best to highlight, wherever opportune, some of the relevant papers which come close or very close to our points, or on which we simply base our study at some points! As in the case of the famous legendary caliph who set the books of the library of Alexandria on fire, we could say that the relevant papers which are not cited here at some point or another are, in most cases, either *blasphemous* (meaning that there is no context here for them to be mentioned), or *unnecessary* (meaning that in general you have to go no farther than the bibliography of our bibliography to learn about them). Other options would be our total *ignorance* about such and such papers at the moment of writing this (about which we thank for any enlightenment that we might receive), or else because we felt it was already well represented by another publication on the same matter, or because it integrates our list of *future research* (that was a good excuse, wasn't it?). Or perhaps it was *not* relevant at all! (You wouldn't know that, would you?) Read the text and judge for yourself. We just want to mention in this subsection a few other papers whose structural or methodological similarity (or dissimilarity) to some of our themes is most striking —so that we can better highlight our *own* originality on some topics, whenever it becomes the case.

Our study in section **2** is totally situated at the level of a general theory of consequence relations, a field sometimes referred to as that of *General Abstract Logics* (cf. [111]), or *Universal Logic* (cf. [19]). There are a few (rare) papers dealing with the definitions of the logical principles at a purely logical level. One of them is Restall's [99], where an approach to the matter quite different from ours is tackled. Also starting from the definition of logic as determined by its consequence relation (even though monotonicity is not pressuposed), and assuming from the start that an adjunctive conjunction is available, the author also requires one sort of contraposition deduction rule to be valid for all of the negations he considers (something that, in most logics herein studied, does not hold), and fixes the relevance logic *R* in the formulation of most of his results. Several versions of the 'law' of non-contradiction are then presented, starting from the outright identification of this principle with the principle of explosion, passing through the identification of the principle of non-contradiction with the principle of excluded middle, or with the validity of the formula $\neg(A\wedge\neg A)$, and going up to some sort of difference in degree between accepting the inference of *all* propositions at once from some given formula $(A\wedge\neg A)$ instead of accepting *each* at a turn. It is clear that the outcome of all this is completely diverse from what we propose here. Some other studies go so far as to also study some of the alternative forms of explosion that we concentrate on here. This is the case, for instance, for Batens's [10], and Urbas's [108]. We are unaware, however, of any study which has taken these alternative forms as far as we do, and have studied them in precise and detailed terms. Such a study is presumed to be essential to help clarify the foundations, the nature and the reach of paraconsistent logics. There have been, for instance, arguments to the effect that the negations of paraconsistent logics are not (or may not be) negation operators after all (cf. Slater's [104] and Béziau's [23]). Béziau's argument amounts to a request for the definition of some minimal 'positive properties' in order to characterize paraconsistent negation really as constituting a *negation* operator, instead of something else. Slater argues for the *inexistence* of paraconsistent logics, given that their negation operator is not a 'contradictory forming functor', but just a 'subcontrary forming one', recovering and extending an earlier argument from Priest & Routley in [93]. Evidently, the same argument about not being a contradictory forming functor applies as well to intuitionistic negation, or in general to any other negation which does not have a classical behavior. Regardless of whether you wish to call such an operator 'negation' or something else, the negations of paraconsistent logics had better be studied under a less biased perspective, by the investigation of general properties that they can or cannot display inside paraconsistent logics. Some good examples of that kind of critical study may be found not only in the present paper but also in Avron's [9], Béziau's [18], and especially Lenzen's [70], among others.

In section **3** we investigate **C**-systems based on classical logic. In this respect, the present study has at least one very important ancestor, namely Batens's [10], where a general investigation of logics extending the positive classical logic (not all of them being **C**-systems!) is presented. This same author has also presented, elsewhere one of the best arguments that may be used to support our approach in terms of logics of formal inconsistency, **LFI**s. Criticizing Priest's logic *LP* (cf. [90]), Batens insists that:

> There simply is no way to express, within this logic, that B is *not* false or that B behaves consistently. (Cf. [13], p.216)

Asserting that 'paraconsistent negation should not and cannot express rejection' (id., p.223), Batens wants to say that it is not because a negated sentence $\neg B$ is inferred from some non-trivial theory of a paraconsistent logic that we can conclude that B is not also to be inferred from that, i.e. $\neg B$ does not express the *rejection* of B. From that he will draw several lessons along his article, such as that: (i) the presence of a strong negation (he writes 'classical negation', but this is clearly an extrapolation —see our note 15, in the subsection **3.7**) inside of a paraconsistent logic is not only a sufficient requisite but also a necessary one to express (classical) rejection; (ii) that one needs a controllably explosive paraconsistent logic (he calls it 'non-strictly paraconsistent') to be able to 'fully describe classical logic' (id., p.225); (iii) that the existence of a bottom particle is also sufficient for the above purposes, for it may define a strong negation (this appeared in an addenda to the paper). So, all at once, this author argues for the validity of three of our alternative explosion principles: (sPPS), (cPPS), and (ExF). From these, of course, we already know (see FACT 2.19) that our principle (gPPS) will often follow, so that according to his recommendations we are finally left with **LFI**s, instead of something like Priest's logic, which, again according to Batens, and for the above reasons, 'fails to capture natural thinking' (though it was proposed to such an effect), and does not provide sufficient environment for us to do 'paraconsistent mathematics'. Priest's response to such criticisms seems to us to be somewhat of a cheat, for he proposes to introduce such a strong negation using an ill-defined bottom particle (see note 25, in the subsection **3.10**). The only point where Batens goes too far to be right seems to be on his argument about paraconsistent logics not being adequate to be used on our metalanguage, because we would be in need of strong negations to complete any consistent description of the world. But now we know that an **LFI** would be more than enough to such an end, being able to fully reproduce all the classical inferences (THEOREM 3.46). And do remember that **LFI**s are especially tailored in order to *express* the fact that B behaves consistently, attending, thus, to Batens's requisite above (and also to his praxis, given that he has already been using in his articles, since long, some symbol to express inconsistency in the object language, be it just an abbreviation or some sort of metaconnective —check the symbol '!' in [15]).

The very idea of a paraconsistent logic still has, nowadays, as strong defenders as attackers. Though, as we know, many attacks are but misunderstandings, many defenses are also poor or unsound. We hope here to contribute to this debate, in one way or another, combining as much precision and clarity as we can. At a more fundamental philosophical level, also, paraconsistency has raised diverse excited opinions about the contribution (or damage) it makes to the very notion of *rationality*. An author such as Mario Bunge will on the one hand compare the Pseudo-Scotus with some sort of *cancer* (cf. [32], p.17), and on the other hand observe that 'a refined symbolism can hide a brazen irrationalism' (id., p.23). He asserts that paraconsistent logic is non-rational by definition, because 'it does not include the principle of non-contradiction' (id., p.24). About this same point other authors will concede similar verdicts, and yet arrive at different conclusions. As Gilles-Gaston Granger put it, paraconsistent logics can be seen as a 'provisory recourse to the irrational', for maintaining an indicium of the rational (sic), namely the principle of non-trivialization, while also maintaining an indicium of the irrational, namely the possible presence of contradictions, to be 'philosophically justified' (cf. [65], p.175). Yet some other authors, such as Newton da Costa, defended that, according to some pragmatic principles of reason which 'seem to be present in all processes of systematization of rational knowledge', this same rational knowledge can be said, among other things, to be both intuitive and discursive, to result from the interaction of the spirit and its environs, and not to be identifiable with a particular system of logic. About reason, on its own turn, he maintained that it is tied to its historical evolution, has its range of application determinable only pragmatically, and is always expressible by way of some logic, which, in each case, is supposed to be uniquely determined by each given context, as being precisely the logic that is most adequate to that context. To determine the concept of adequacy, finally, da Costa recurs again to pragmatic factors, such as simplicity, convenience, facility, economy, and so on (cf. [51]). This is in fact a very thought-provoking issue, and several other authors have advanced positions on the relations of paraconsistency and rationality, such as Francisco Miró Quesada (in [81] and [80]), Nicholas Rescher (in [98]), Jean-Yves Béziau (in [23]), and Bobenrieth (in [26]). The reader is invited to read those authors directly, if this is their interest. We will not venture here any further steps in this slippery slope, for our aim is much less ambitious. After reading this comprehensive technical survey, however, we hope that the reader will feel illuminated enough to risk their own rationally-based judgements on the matter.

2 A PARACONSISTENT LOGIC IS A PARACONSISTENT LOGIC IS…

> Logic is the chosen resort of clear-headed people, severally convinced of the complete adequacy of their doctrines. It is such a pity that they cannot agree with each other.
> —A. N. Whitehead, "Harvard: The Future", Atlantic Monthly 158, p. 263.

Many a logician will agree that the fundamental notion behind logic is the notion of 'derivation', or rather should we say the notion of 'consequence'. On that account, in our common heritage it is to be found the Tarskian notion of a *consequence relation*. As usual, given a set *For* of formulas, we say that $\Vdash\ \subseteq \wp(For)\times For$ defines a consequence relation on *For* if the following clauses hold, for any formulas A and B, and subsets Γ and Δ of *For:* (formulas and commas at the left-hand side of $\Vdash$ denote, as usual, sets and unions of sets of formulas)

(Con1) $A\in\Gamma \;\Rightarrow\; \Gamma\Vdash A$ (reflexivity)
(Con2) $(\Delta\Vdash A \text{ and } \Delta\subseteq\Gamma) \;\Rightarrow\; \Gamma\Vdash A$ (monotonicity)
(Con3) $(\Delta\Vdash A \text{ and } \Gamma, A\Vdash B) \;\Rightarrow\; \Delta,\Gamma\Vdash B$ (transitivity)

So, a logic **L** will here be defined simply as a structure of the form $<For, \Vdash>$, containing a set of formulas and a consequence relation defined on this set. We need not suppose at this point that the set *For* should be endowed with any additional structure, like the usual algebraic one, but we will hereby suppose, for convenience, that *For* is built on a denumerable language having $\neg$ as its (primitive or defined) negation symbol, and we will also suppose the connectives to be constructing operators on the set of formulas. Any set $\Gamma\subseteq For$ is called a *theory* of **L**. A theory Γ is said to be *proper* if $\Gamma\neq For$, and a theory Γ is said to be *closed* if it contains all of its consequences, i.e. if the converse of (Con1) holds: $\Gamma\Vdash A \;\Rightarrow\; A\in\Gamma$. Whenever we have, in a given logic, that $\Gamma\Vdash A$, for a given theory Γ and some formula A, we will say that A is *inferred* from Γ (in this logic); if, for all Γ, we have that $\Gamma\Vdash A$, that is, if A is inferred from any given theory, we will say that A is a *thesis* (of this logic).

Not all known logics respect all the above clauses, or only them. For instance, those logics in which (Con2) is either dropped out or substituted by a form of 'cautious monotonicity' are called *non-monotonic*, and the logics whose consequence relations are closed under substitution are called *structural*. Unless explicitly stated to the contrary, we will from now on be working with some fixed arbitrary logic $\mathbf{L}=<For, \Vdash>$, and with some fixed arbitrary theory Γ of **L**. Properties (Con1)–(Con3) will be assumed to hold irrestrictedly, and they will be used in some proofs here and there. Some interesting and quite immediate consequences from (Con2) and (Con3) which we shall make use of are the following:

FACT 2.1 The following properties hold for any logic, any given theories Γ and Δ, and any formulas A and B:
(i) $\Gamma,\Delta\nVdash A \;\Rightarrow\; \Gamma\nVdash A$;
(ii) $(\Gamma\Vdash A \text{ and } A\Vdash B) \;\Rightarrow\; \Gamma\Vdash B$;
(iii) $(\Gamma\Vdash A \text{ and } \Gamma, A\Vdash B) \;\Rightarrow\; \Gamma\Vdash B$.
Proof: (i) follows from (Con2); (ii) and (iii), from (Con3). □

Given two logics $\mathbf{L}1=<For_1, \Vdash_1>$ and $\mathbf{L}2=<For_2, \Vdash_2>$, we will say that **L**1 is a *linguistic extension* of **L**2 if For_2 is a proper subset of For_1, and we will say that **L**1 is a *deductive extension* of **L**2 if $\Vdash_2$ is a proper subset of $\Vdash_1$. Finally, if **L**1 is both a linguistic and deductive extension of **L**2, and if the restriction of **L**1's consequence relation $\Vdash_1$ to the set For_2 will make it identical to $\Vdash_2$ (that is, if $For_2 \subset For_1$, and for any $\Gamma \cup \{A\} \subseteq For_2$ we have that $\Gamma \Vdash_1 A \Leftrightarrow \Gamma \Vdash_2 A$) then we will say that **L**1 is a *conservative extension* of **L**2. In any of the above cases we can more generally say that **L**1 is an *extension* of **L**2, or that **L**2 is a *fragment* of **L**1. These concepts will be used mainly in the next section, where we will build and compare a number of paraconsistent logics. Just as a guiding note to the reader, however, we could remark that usually, but not obligatorily, linguistic extensions are also deductive ones, but it is quite easy to find in the realm of non-classical logics, on the other hand, deductive fragments which are not linguistic ones (like intuitionistic logic is a deductive fragment of classical logic). Most paraconsistent logics in the literature are also deductive fragments of classical logic themselves, but the ones we shall be working on here, the **C**-*systems*, are in general deductive fragments only of a conservative extension of classical logic —by the addition of (explicitly definable) connectives expressing consistency / inconsistency). A particular case of them, the **dC**-systems, will nevertheless be shown to be characterizable as deductive fragments of good old classical logic, dispensing its mentioned extension. But these assertions will be made much clearer in the near future.

Let Γ be a theory of **L**. We say that Γ is *contradictory with respect to* $\neg$, or simply *contradictory*, if it is such that, for some formula A, we have $\Gamma \Vdash A$ and $\Gamma \Vdash \neg A$. With some abuse of notation, but (hopefully) no risk of misunderstanding, we will from now on write these sort of sentences in the following way:

$$\exists A\,(\Gamma \Vdash A \text{ and } \Gamma \Vdash \neg A). \tag{D1}$$

For any such formula A we may also say that Γ is A-*contradictory*, or simply that A is *contradictory* for such a theory Γ (and such an underlying logic **L**). It follows that:

FACT 2.2 For a given theory Γ: (i) If $\{A, \neg A\} \subseteq \Gamma$ then Γ is A-contradictory. (ii) If Γ is both A-contradictory and closed, then $\{A, \neg A\} \subseteq \Gamma$.

Proof: Part (i) comes from (Con1), part (ii) from the very definition of a closed theory. □

A theory Γ is said to be *trivial* if it is such that:

$$\forall B\,(\Gamma \Vdash B). \tag{D2}$$

Hence, a trivial theory can make no difference between the formulas of a logic — all of them may be inferred from it. Of course, using (Con1) we may notice that the non-proper theory *For* is trivial. We may also immediately conclude that:

FACT 2.3 Contradictoriness is a necessary condition for triviality in a given theory. (D2) $\Rightarrow$ (D1)

A theory Γ is said to be *explosive* if:

$$\forall A\,\forall B\,(\Gamma, A, \neg A \Vdash B). \tag{D3}$$

Thus, a theory is called explosive if it trivializes when exposed to a pair of contradictory formulas. Evidently:

FACT 2.4 (i) If a theory is trivial, then it is explosive. (ii) If a theory is contradictory and explosive, then it is trivial. (D2) $\Rightarrow$ (D3); (D1) and (D3) $\Rightarrow$ (D2)

Proof: Use (Con2) in the first part and FACT 2.1(iii) in the second. □

2.1 A question of principles. Now, remember that talking about a logic is talking about the inferential behavior of a set of theories. Accordingly, using the above definitions, we will now say that a given logic **L** is *contradictory* if all of its theories are contradictory. (D4)

In much the same spirit, we will say that **L** is *trivial*, or *explosive*, if, respectively, all of its theories are trivial, or explosive. respect. (D5), (D6)

The empty theory may be here regarded as playing an important role, revealing some intrinsic properties of a given logic, in spite of the behavior of any of its specific non-empty theories (also called 'non-logical axioms'). Indeed:

FACT 2.5 A monotonic logic **L** is contradictory / trivial / explosive if, and only if, its empty theory is contradictory / trivial / explosive.

We can now tackle a formal definition for some of the so-called *logical principles* (relativized for a given logic **L**), namely:

PRINCIPLE OF NON-CONTRADICTION (PNC)
L must be non-contradictory: $\exists\Gamma\,\forall A\,(\Gamma \nVdash A \text{ or } \Gamma \nVdash \neg A)$.

PRINCIPLE OF NON-TRIVIALITY (PNT)
L must be non-trivial: $\exists\Gamma\,\exists B\,(\Gamma \nVdash B)$.

PRINCIPLE OF EXPLOSION, or PRINCIPLE OF PSEUDO-SCOTUS[3] (PPS)
L must be explosive: $\forall\Gamma\,\forall A\,\forall B\,(\Gamma, A, \neg A \Vdash B)$.

This last principle is also often referred to as *ex contradictione sequitur quodlibet*.

The reader will immediately notice that these principles are somewhat interrelated:

FACT 2.6 (i) An explosive logic is contradictory if, and only if, it is trivial. (ii) A trivial logic is both contradictory and explosive. (iii) A logic in which the Principle of Explosion holds is a trivial one if, and only if, the Principle of Non-Contradiction fails. (D6) $\Rightarrow$ [(D4) $\Leftrightarrow$ (D5)]; (D5) $\Rightarrow$ [(D4) and (D6)];
(PPS) $\Rightarrow$ [not-(PNT) $\Leftrightarrow$ not-(PNC)]

Proof: Just consider FACT 2.4, and the definitions above. □

A trivial logic, i.e. a logic in which (PNT) fails, cannot be a very interesting one, for in such a logic anything could be inferred from anything, and any intended capability of modeling 'sensible' reasoning would then collapse. Of course, (PPS) would still hold in such a logic, as well as any other universally quantified sentence dealing with the behavior of its consequence relation, but this time only because they would be unfalsifiable! It is readily comprehensible then that triviality might have been regarded as the mathematician's worst nightmare. Indeed, (PNT) constituted what Hilbert called 'consistency (or compatibility) principle', with which proof his Metamathematical enterprise was crafted to cope. Well aware of the preceding fact, and working inside the environment of an explosive logic such as classical logic, Hilbert transposed the 'problem of consistency' (that is, the problem of non-triviality) to the problem of proving that there were no contradictions among the axioms of arithmetic and their consequences (this was Hilbert's Second Problem, cf. [66]). By the way, this situation would eventually lead Hilbert to the formulation of a curious criterion according to which the non-contradictoriness of a mathematical

[3] Which was made visible by a reedition of a collection of commentaries on Aristotle's *Prior Analytics*, long attributed, in error, to Johannes Duns Scotus (1266-1308), in the twelve books of the *Opera Omnia*, 1639 (reprint 1968). The current most plausible conjecture about the authorship of these books will trace them back to John of Cornwall, around 1350. See [26] for more on its history.

object is a necessary and sufficient condition for its very *existence*.[4] Perhaps he would have never proposed such a criterion if he had only considered the existence of non-explosive logics, with or without (PNC), logics in which contradictory theories do not necessarily lead to trivialization! The search for such logics would give rise, much later, to the 'paraconsistent enterprise'.[5]

In classical logic, of course, all the three principles above hold, and one could naively speculate from such that they are all 'equivalent', in some sense. Indeed, they have all been now and again confused in the literature and each one of them has, in turn, been identified with the 'Principle of Non-Contradiction' (and these will not exhaust all formulations of this last principle that have been proposed here and there). The emergence of paraconsistent logic, as we shall see, will serve to show that this equivalence is far from being necessary, for an arbitrary logic **L**.

2.2 The paraconsistency predicament. Some decades ago, S. Jaśkowski ([67]) and N. C. A. da Costa ([49]), the founders of paraconsistent logic, proposed, independently, the study of logics which could accommodate contradictory yet non-trivial theories. Accordingly, a *paraconsistent logic* (a denomination which would be coined only in the seventies, by Miró Quesada) would be initially defined as a logic such that:

$$\exists\Gamma\exists A\exists B\ (\Gamma \Vdash A \text{ and } \Gamma \Vdash \neg A \text{ and } \Gamma \nVdash B). \tag{PL1}$$

Attention: This definition says *not* that (PNC) is not to hold in such a logic, for it says nothing about *all* theories of a paraconsistent logic being contradictory, but only that *some* of them should be contradictory, and yet non-trivial. As a consequence, following our definitions above, the notion of paraconsistent logic has, in principle, nothing to do with the rejection of the Principle of Non-Contradiction, as it is commonly held! On the other hand, it surely has something to do with the rejection of explosiveness. Indeed, consider the following alternative definition of a paraconsistent logic, as a logic in which (PPS) fails:

$$\exists\Gamma\exists A\exists B\ (\Gamma, A, \neg A \nVdash B). \tag{PL2}$$

Now one may easily check that:

FACT 2.7 (PL1) and (PL2) are equivalent ways of defining a paraconsistent logic, if its consequence relation is reflexive and transitive.

[(Con1) and (Con3)] $\Rightarrow$ [(PL1) $\Leftrightarrow$ (PL2)]

[4] Girolamo Saccheri (1667-1733) had already paved the way much before to set non-contradictoriness, instead of intuitiveness, as a sufficient, other than necessary, criterion for the legitimateness of a mathematical theory (cf. [1]). The so-called 'Hilbert's criterion for existence in mathematics' seems thus to constitute a further step in taking this method to its ultimate consequences.

[5] Assuming intuitively (cf. [46], p.7) that a contradiction could painlessly be admitted in a given theory if only this theory was not to be trivialized by it, even some years before the actual proposal of his first paraconsistent systems, da Costa was eventually led to trace the Metamathematical's problem about the utility of a formal system back to (PNT). At that point da Costa was even to suggest that Hilbert's criterium for existence in mathematics should be changed, and that *existence*, in mathematics, should be equated with non-triviality, rather than with non-contradictoriness (cf. [47], p.18). To be more precise about this point, da Costa has in fact recovered Quine's motto ([96], chapter I): 'to be is to be the value of a variable' —from which follows that the ontological commitment of our theories is to be measured by the domain of its variables—, and then proposed the following modification to it: 'to be is to be the value of a variable, in a given language of a given logic' (cf. [52], and the entry 'Paraconsistency' in [33]). This was meant to open space for the appearance of different ontologies based on different kinds of logic, analogously to what had happened in the XIX century with the appearance of different geometries based on different sets of axioms.

Proof: To show that (PL1) implies (PL2), use (Con3), or directly FACT 2.1; to show the converse, use (Con1). □

Say that two formulas A and B are *equivalent* if each one of them can be inferred from the other, that is:

$$(A \Vdash B) \text{ and } (B \Vdash A). \tag{Eq1}$$

In a similar manner, say that two sets of formulas Γ and Δ are equivalent if:

$$\forall A \in \Delta\, (\Gamma \Vdash A) \text{ and } \forall B \in \Gamma\, (\Delta \Vdash B). \tag{Eq2}$$

We will alternatively denote these facts by writing, respectively, $A \dashv\Vdash B$, and $\Gamma \dashv\Vdash \Delta$.

Now, an essential trait of a paraconsistent logic is that it does not see all contradictions at the same light —each one is a different story. Indeed:

FACT 2.8 Given any arbitrary transitive paraconsistent logic, it cannot be the case that all of its contradictions are equivalent.

Proof: If, for whatever formulas A and B, we have that $\{A, \neg A\} \dashv\Vdash \{B, \neg B\}$, then any A-contradictory theory, would also be, by transitivity and definition (Eq2), a B-contradictory theory. But if a theory infers every pair of contradictory formulas, it infers, in particular, any given formula at all, and so it is trivial. □

Once again, the reader should note that the existence of a paraconsistent logic **L** presupposes only the existence of *some* non-explosive theories in **L**; this does not mean that *all* theories of **L** should be non-explosive —and how could they all be so? (recall FACT 2.4(i)) Moreover, once more according to our proposed definitions, the reader will soon notice that the great majority of the paraconsistent logics found in the literature, and all the paraconsistent logics studied in this paper, are non-contradictory (i.e. 'consistent', following the usual model-theoretic connotation of the word). In particular, they usually have non-contradictory empty theories, which means, from a proof-theoretical point of view, that they bring no built-in contradiction in their axioms, and that their inference rules do not generate contradictions from these axioms. Even so, because of their paraconsistent character, they can still be used as underlying logics to extract some sensible reasoning of some theories that are contradictory and are still to be kept non-trivial. This phenomenon is no miracle, and certainly no sleight of hand, as the reader will understand below, but is obtained from suitable constraints on the power of explosiveness, (PPS). So, all paraconsistent logics which we will present here are in some sense 'more conservative' than classical logic, in the sense that they will extract less consequences than classical logic would extract from some given classical theory, or at most the same set of consequences, but never more. Our paraconsistent logics then (as most paraconsistent logics in the literature) will not validate any bizarre form of reasoning, and will not extract any contradictory consequence in the cases where classically there were no such consequences. It is nonetheless possible to also build logics which disrespect both (PPS) *and* (PNC), and thus might be said to be 'highly' non-classical, in a certain sense, once they *do* have theses which are not classical theses. Such logics will constitute a particular case of paraconsistent logics that are generally dubbed *dialectical logics*, or *logics of impossible objects*, and some specimens of these may be found, for instance, in [56], [83], [88], and [100]. We will not study these kind of logics here.

We shall, from now on, make use of either one of the above definitions for paraconsistent logic, indistinctly.

2.3 The trivializing predicament. Given (PL1), we know that any paraconsistent logic must possess contradictory non-trivial theories, and from (PL2) we know that these must be non-explosive. Evidently, not all theories of a given logic can be such: we already also know that any trivial theory is both contradictory and explosive, and every logic has trivial theories (consider, for instance, the non-proper theory *For*, i.e. the whole set of formulas). It is possible, though, and in fact very interesting, to further explore this no man's land which lays in between plain non-explosiveness and outright explosiveness, if one considers some paraconsistent logics having some suitable explosive proper theories. That is what we will do in the following subsections.

A logic **L** is said to be *finitely trivializable* when it has finite trivial theories. (D7)

Evidently:

FACT 2.9 If a logic is explosive, then it is finitely trivializable. (D6) $\Rightarrow$ (D7)

Proof: All theories of an explosive logic are explosive, in particular the empty one. Thus, for any A, the finite theory $\{A, \neg A\}$ is trivial. □

This same fact does not hold for non-explosive logics. In fact, we will present, in the following, a few paraconsistent logics which are *not* finitely trivializable, although these shall, in general, not concern us in this article, for reasons which will soon be made clear. Let us first state and study some few more simple definitions.

A logic **L** has a *bottom particle* if there is some formula C in **L** that can, by itself, trivialize the logic, that is:

$$\exists C\, \forall \Gamma\, \forall B\ (\Gamma,\ C \Vdash B). \tag{D8}$$

We will denote any fixed such particle, when it exists, by $\bot$. Evidently, no arbitrary monotonic and transitive logic can have a bottom particle as a thesis, under pain of turning this logic into a trivial logic —in which, of course, all formulas turn to be bottom particles.

It is instructive here to remember another formulation of (PPS) which sometimes shows up in the literature:

PRINCIPLE OF 'EX FALSO SEQUITUR QUODLIBET' (ExF)

L must have a bottom particle.

Now, if we are successful in isolating logics that disrespect (PPS) while still respecting (ExF) we will show that *ex contradictione* (*sequitur quodlibet*) does not need to be identified with *ex falso* (*sequitur quodlibet*), as is quite commonly held.[6]

We say that a logic **L** has a *top particle* if there is some formula C in **L** that is a consequence of every one of its theories, no matter what, that is:

$$\exists C\, \forall \Gamma\ (\Gamma \Vdash C). \tag{D9}$$

We will denote any fixed such particle, when it exists, by $\top$. Evidently, given a monotonic logic, any of its theses will constitute such a top particle (and logics with no theses, like Kleene's three-valued logic, will have no such particles). Also, given transitivity and monotonicity, it is easy to see that the addition of a top particle to a given theory is pretty innocuous, for in that case $(\Gamma,\ \top \Vdash B)$ if and only if $(\Gamma \Vdash B)$.

Let **L** now be some logic, let σ: $For \rightarrow For$ be a mapping (if *For* comes equipped with some additional structure, we will require σ to be an endomorphism), and let this mapping be such that $\sigma(A)$ is to denote a formula which *depends only on A*. By this we shall mean that $\sigma(A)$ is a formula constructed using but A itself and some

[6] And yet this separation between these two principles can already be found in the work of the Pseudo-Scotus (see [26], chapter V, section 2.3).

purely logical symbols (such as connectives, quantifiers, constants). In more general terms, given any sequence of formulas $A_1, A_2, \ldots, A_n$, we will let $\sigma(A_1, A_2, \ldots, A_n)$ denote a formula which depends only on the formulas of the sequence. Similarly, we will let $\Gamma(A_1, A_2, \ldots, A_n)$ denote a set of formulas each of which depends only on the sequence $A_1, A_2, \ldots, A_n$. In some situations it will help to assume this σ to be a *schema*, that is, that given any two sequences $A_1, A_2, \ldots, A_n$ and $B_1, B_2, \ldots, B_n$, we must have that $\sigma(A_1, A_2, \ldots, A_n)$ will be made identical to $\sigma(B_1, B_2, \ldots, B_n)$ if we only change each A_i for B_i, in $\sigma(A_1, A_2, \ldots, A_n)$ (this means, in some sense, that all these σ-formulas will share some built-in *logical form*). Usually, when saying that we have a formula, or set of formulas, depending only on some given sequence of formulas, we further presuppose that this dependency is schematic, but this supposition will in general be not strictly necessary to our purposes.

We say that a logic **L** has a *strong* (or *supplementing*) *negation* if there is a schema $\sigma(A)$, depending only on A, that does not consists, in general, of a bottom particle and that cannot be added to any theory inferring A without causing its trivialization, that is:

$$\text{(a) } \exists A \text{ such that } \sigma(A) \text{ is not a bottom particle, and} \\ \text{(b) } \forall A \forall \Gamma \forall B\, [\Gamma, A, \sigma(A) \Vdash B]. \qquad \text{(D10)}$$

We will denote the strong negation of a formula A, when it exists, by $\sim A$.

Parallel to the definition of contradictoriness with respect to $\neg$, we might now define a theory Γ to be *contradictory with respect to* $\sim$ if it is such that:

$$\exists A\, (\Gamma \Vdash A \text{ and } \Gamma \Vdash \sim A). \qquad \text{(D11)}$$

Accordingly, a logic **L** is said to be *contradictory with respect to* $\sim$ if all of its theories are contradictory with respect to $\sim$. (D12)

Here we may of course introduce yet another version of (PPS):

SUPPLEMENTING PRINCIPLE OF EXPLOSION (sPPS)
L must have a strong negation.[7]

Some immediate consequences of the last definitions are:

FACT 2.10 (i) If a logic has either a bottom particle or a strong negation, then it is finitely trivializable. (ii) If a non-trivial logic has a bottom particle, then it admits a strong negation. (iii) If a logic is explosive and non-trivial, then it is supplementing explosive. [(D8) or (D10)] $\Rightarrow$ (D7); [not-(D5) and (D8)] $\Rightarrow$ (D10); [(PNT) and (ExF)] $\Rightarrow$ (sPPS); [(PPS) and (PNT)] $\Rightarrow$ (sPPS)

Proof: (i) is obvious. To prove (ii), define the strong negation $\sim A$ of a formula A by stipulating that, for any theories Γ and Δ, we have (a) $(\Gamma, \Delta \Vdash \sim A)$ iff $(\Gamma, \Delta, A \Vdash \bot)$, and (b) $(\Gamma, \Delta, \sim A \Vdash \bot)$ iff $(\Gamma, \Delta \Vdash A)$. By (Con1), we have that $(\Gamma, \sim A \Vdash \sim A)$, and so, part (a) will give us $(\Gamma, \sim A, A \Vdash \bot)$, choosing $\Delta = \{\sim A\}$. But $\bot \Vdash B$, for any formula B, once $\bot$ is a bottom particle. So, by FACT 2.1(ii), we conclude that $(\Gamma, A, \sim A \Vdash B)$, for any B. Now, to check that such a strong negation, thus defined, cannot be always a bottom particle, notice that part (b) will give us $\Vdash A$ iff $\sim A \Vdash \bot$, choosing both Γ and Δ to be empty. So, if $\sim A$ were a bottom particle, $\sim A \Vdash \bot$ would be the case, and hence any A would be a thesis of this logic, which is not the case, once we have supposed it to be non-trivial.[8] To check (iii), just note that a non-trivial ex-

[7] A strong negation should *not* be confused with a 'classical' one! Take a look at THEOREM 3.42.

[8] In the presence of a convenient implication, for instance, obeying the Deduction Metatheorem (THEOREM 3.1) such an 'implicit' definition of a strong negation from a bottom particle can be internalized by the underlying logic as an 'explicit' definition (as in the case of intuitionistic logic). Check also our remarks about this matter in our discussion of Beth Definability Property, at the end of the subsection **3.12**.

plosive logic will come already equipped with a built-in strong negation, coinciding with its own primitive negation. □

FACT 2.11 Let **L** be a logic with a strong negation ~. (i) Every theory which is contradictory with respect to ~ is explosive. (ii) A logic is contradictory with respect to ~ if, and only if, it is trivial. (D11) ⇒ (D3); (D12) ⇔ (D5)

A logic **L** is said to be *left-adjunctive* if for any two formulas A and B there is a schema $\sigma(A, B)$, depending only on A and B, with the following behavior:

$$\text{(a) } \exists A \exists B \text{ such that } \sigma(A, B) \text{ is not a bottom particle, and}$$
$$\text{(b) } \forall A \forall B \forall \Gamma \forall D\, [\Gamma, A, B \Vdash D \;\Rightarrow\; \Gamma, \sigma(A, B) \Vdash D]. \qquad \text{(D13)}$$

Such a formula, when it exists, will be denoted by $(A \wedge B)$, and the sign $\wedge$ will be called a *left-adjunctive conjunction* (but it will not necessarily have, of course, all properties of a classical conjunction). Similarly, a logic **L** is said to be *left-disadjunctive* if there is a schema $\sigma(A, B)$, depending only on A and B, such that (D12) is somewhat inverted, that is:

$$\text{(a) } \exists A \exists B \text{ such that } \sigma(A, B) \text{ is not a top particle, and}$$
$$\text{(b) } \forall A \forall B \forall \Gamma \forall D\, [\Gamma, \sigma(A, B) \Vdash D \;\Rightarrow\; \Gamma, A, B \Vdash D]. \qquad \text{(D14)}$$

In general, whenever there is no risk of misunderstanding, we might also denote this formula, when it exists, by $A \wedge B$, and we will accordingly call $\wedge$ a *left-disadjunctive conjunction*. Now, one should be aware of the fact that, in principle, a logic can have just one of these conjunctions, or it can have both a left-adjunctive and a left-disadjunctive conjunction without the two of them coinciding.

To convince themselves of the naturalness of these definitions and the comments we made about them, we invite the reader to consider the following two more 'concrete' properties of conjunction:

$$\text{(a) } \exists A \exists B \text{ such that } A \wedge B \text{ is not a bottom particle, and}$$
$$\text{(b) } \forall \Gamma \forall A \forall B\, (\Gamma, A \wedge B \Vdash A \text{ and } \Gamma, A \wedge B \Vdash B). \qquad \text{(pC1)}$$

$$\text{(a) } \exists A \exists B \text{ such that } A \wedge B \text{ is not a top particle, and}$$
$$\text{(b) } \forall \Gamma \forall A \forall B\, (\Gamma, A, B \Vdash A \wedge B). \qquad \text{(pC2)}$$

Now, it is easy to see that:

FACT 2.12 Let **L** be a logic obeying (Con1)–(Con3). (i) A conjunction in **L** is left-adjunctive iff it respects (pC1). (ii) A conjunction in **L** is left-disadjunctive iff it respects (pC2). [(Con1)–(Con3)] ⇒ {[(D13) ⇔ (pC1)] and [(D14) ⇔ (pC2)]}

Proof: To prove that a left-adjunctive conjunction respects (pC1) and that a left-disadjunctive conjunction respects (pC2), use (Con1) and (Con2). For the converses, use (Con3). □

The reader might mind to notice that a conjunction which is both left-adjunctive and left-disadjunctive is sometimes called, in the scope of relevance logic, an *intensional* conjunction, and in the scope of linear logic such a conjunction is said to be a *multiplicative* one (also, in [9], this is what the author calls an *internal* conjunction).

We may now check that:

FACT 2.13 Let **L** be a left-adjunctive logic. (i) If **L** either is finitely trivializable or has a strong negation, than it has a bottom particle. (ii) If **L** is finitely trivializable, then it will be supplementing explosive. (iii) If **L** respects *ex contradictione*, then it will respect *ex falso*. (D13) ⇒ {[(D7) or (D10)] ⇒ (D8)}; [(D13) and (D7)] ⇒ (sPPS); (D13) ⇒ [(PPS) ⇒ (ExF)]

Proof: To prove (i), note that if **L** has a finite trivial theory Γ, one may define a bottom particle from the conjunction of all formulas in Γ; in case it has a strong negation, any formula in the form $(A \wedge {\sim}A)$, for some formula A of **L**, will suffice. Parts (ii) and (iii) are immediate. □

Consider now the *discussive logic* proposed by Jaśkowski in [67], **D2**, which is such that $\Gamma \vDash_{\mathbf{D2}} A$ iff $\Diamond\Gamma \vDash_{S5} \Diamond A$, where $\Diamond\Gamma = \{\Diamond B$: for all $B \in \Gamma\}$, $\Diamond$ denotes the possibility operator, and $\vDash_{S5}$ denotes the consequence relation defined by the well-known modal logic *S*5. It is easy to see that in **D2** one has that $(A, \neg A \vDash_{\mathbf{D2}} B)$ *does not* hold in general, though $(A \wedge \neg A) \vDash_{\mathbf{D2}} B$ *does* hold, for any formulas A and B. This phenomenon can only happen because (pC1) holds while (pC2) does not hold in **D2**, and so its conjunction is left-adjunctive but not left-disadjunctive, while $(A \wedge \neg A)$ defines a bottom particle. Hence, the fact above still holds for **D2**, and this logic indeed displays a quite immediate example of a logic respecting (ExF) but not (PPS).

To sum up with the latest definitions and their consequences, we can picture the situation as follows, for some given logic **L**:

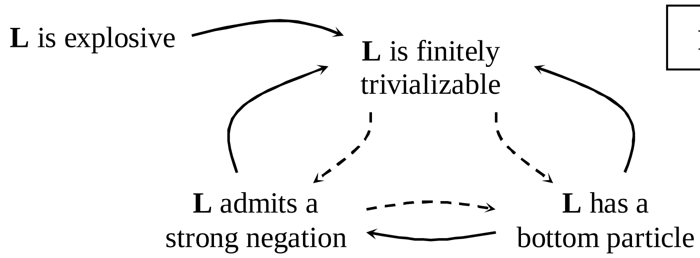


Figure 2.1

Where:

× ⟶ ✓ means that × entails ✓

× ⇢ ✓ means that × plus left-adjunctiveness entails ✓

2.4 Huge tracts of the logical space. Lo and behold! If now the reader only learns that all properties mentioned in the last subsection *are* compatible with the definition of a paraconsistent logic, they are sure to obtain a wider view of the paraconsistent landscape. Indeed, general non-explosive logics, that is, logics in which not all theories are explosive, can indeed uphold the existence either of finitely trivializable theories, strong negations, or bottom particles! (A rough map of this brave new territory may be found in **Figure 2.2**.) Logics which are paraconsistent but nevertheless have some special explosive theories, such as the ones just mentioned, will constitute the focus of our attention from now on, for, as we shall argue, they may let us explore some fields into which we would not tread in the lack of those theories. Some interesting new concepts can now be studied —this is the case of the notion of *consistency* (and its dual, the notion of *inconsistency*), as we shall argue.

Consider for instance the logic *Pac*, given by the following matrices:

$\wedge$	**1**	**½**	**0**
1	1	½	0
½	½	½	0
0	0	0	0

$\vee$	**1**	**½**	**0**
1	1	1	1
½	1	½	½
0	1	½	0

$\rightarrow$	**1**	**½**	**0**
1	1	½	0
½	1	½	0
0	1	1	1

	$\neg$
1	0
½	½
0	1

where both 1 and ½ are distinguished values. This is the name under which this logic appeared in Avron's [8] (section 3.2.2), though it had previously appeared, for instance, in Avron's [7], under the name $RM_3^{\supset}$, and, even before than that, in Batens's [10], under the name PI^s. It is easy to see that, in such a logic, for no formula A it can be the case that $A, \neg A \vDash_{Pac} B$, for all B. So, *Pac* is a non-explosive, thus paraconsistent, logic. Conjunction, disjunction and implication in *Pac* are fairly classical connectives: in fact, the whole positive classical logic is validated by its matrices. But the negation in *Pac* is in some sense strongly non-classical in its surrounding environment, and the immediate consequence of this is that *Pac* does not have any explosive theory as the ones mentioned above. If such a three-valued logic would define a negation having all properties of classical negation, the table at the right shows how it would look. It is very easy to see that such a negation (in fact, a strong negation with all classical properties) is *not* definable in *Pac*, for any truth-function of this logic having only ½'s as input will also have ½ as output. As a consequence, *Pac* will not respect *ex falso*, having no bottom particle (being unable, thus, as we shall argue before the end of this section, to express the consistency of its formulas), and once it is evidently a left-adjunctive logic as well, it will not even be finitely trivializable at all. One could then criticize such a logic for providing a very weak interpretation for negation, once in this logic all contradictions are admissible. This has some weird consequences and is certainly too light a way of obtaining a paraconsistent logic (this is also the central point of Batens's criticism of Priest's *LP*,[9] see [13]): if some contradictions will give you trouble just assume, then, that no contradiction at all can ever really hurt your logic! Under our present point of view, proposing a logic in which no single contradiction can ever have a harmful effect on their underlying theories is quite an extremist position, and may take us too far away from any classical form of reasoning.[10]

	~
1	0
½	0
0	1

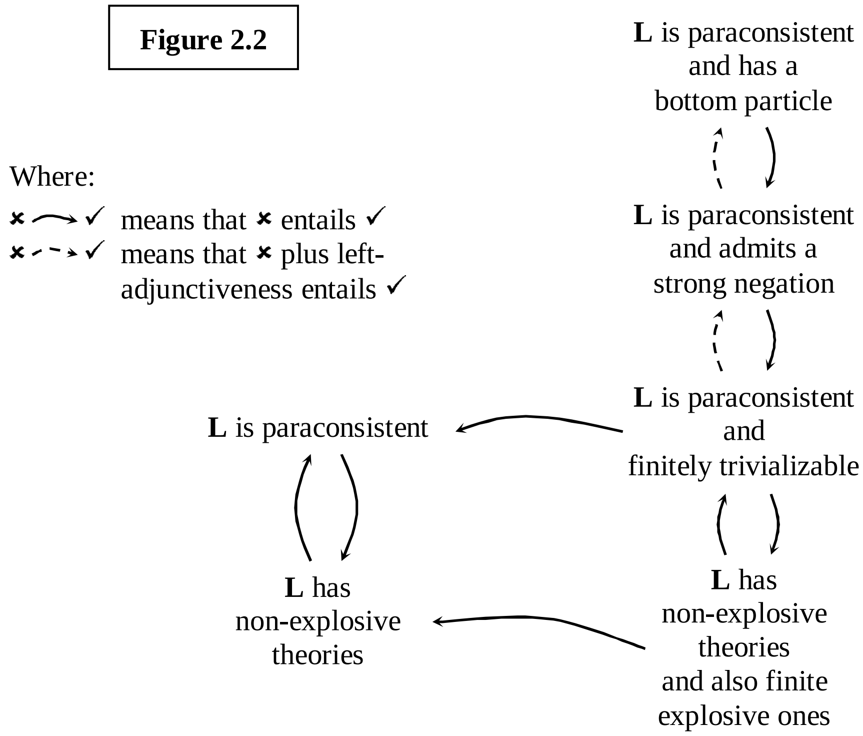
Now, if one endows the language of *Pac* either with such a strong negation or a *falsum* constant (a bottom particle), with the canonical interpretation, what will result is a well-known conservative extension of it, called $\mathbf{J}_3$, which is still paraconsistent but has all those special explosive theories neglected by *Pac*. This logic $\mathbf{J}_3$ was first introduced by D'Ottaviano and da Costa in 1970 (cf. [60]) as a 'possible solution to the problem of Jaśkowski', and reappeared quite often in the literature after that. The first presentation of $\mathbf{J}_3$ did not bring the strong negation ~ as a primitive connective, but displayed instead a primitive 'possibility connective' ∇ (see its table to the right). In [61] it was once more presented, but this time having also a sort of 'consistency connective' ∘ as primitive (table to the right), and in [44] we have explored more deeply the expressive and inferential power of this logic, and the possibility of applying it to the study of inconsistent

	∇	∘
1	1	1
½	1	0
0	0	1

[9] By the way, Priest's logic *LP* is nothing but the implicationless fragment of *Pac* (cf. [90]).

[10] This constitutes indeed the kernel of a long controversy between H. Jeffreys and K. Popper. The first author argued in 1938 that contradictions should not be reasonably supposed to imply anything else, to which the second author replied in 1940 saying that contradictions are fatal and should be avoided at all costs, to prevent science from collapsing. Jeffreys aptly reiterated, in 1942, that he was not suggesting that *all* contradictions should be tolerated, but at least *some*. Popper responded to this successively in 1943, 1959 and 1963, saying that he himself had thought about a system in which contradictory sentences were not 'embracing', that is, did not explode, but he abandoned this system because it turned out to be too weak (lacking, for instance, *modus ponens*), and he hastily concluded from that that no useful such a system could ever be attained. See more details and references about this dispute in [26], chapter VI.

Figure 2.2



databases, abandoning ~ and ∇ but still maintaining ∘ as primitive. As a result, we have argued that this logic (now renamed **LFI1**, one of our main 'logics of formal inconsistency') has been shown to be perfectly adequate, among other options, for the task of formalizing the notion of (in)consistency in a very strong and sensible way. But we will have much more to say about this further on.

The reader could now certainly ask himself: If paraconsistency is about non-explosiveness, why are you so interested in having these special explosive theories? Because our interest lies much further than the simple control of the explosive power of contradictions —we want to be able to retain classical reasoning, if only under some suitable interpretation of a fragment of our paraconsistent logics, and we also want to use these paraconsistent logics not only to reason under conditions which do not presuppose consistency, but we want to be able to take hold of the very notion of consistency inside of our logics! From this point of view, the paraconsistent logics which shall interest us are exactly those which permit us to formalize, and get a good grip on, the intricate phenomenon of *inconsistency*, as opposed to mere cut and dried *contradictoriness*.

Whatever inconsistency might mean, by our previous analysis, we might surely suppose a trivial theory to be not only contradictory but inconsistent as well. But yet, a contradiction is certainly one of the many guises of inconsistency! So one may conjecture that *consistency* is exactly what a contradiction might be lacking to become explosive —if it was not explosive from the start. Roughly speaking, we are going to suppose that a 'consistent contradiction' is likely to explode, even if a 'regular' contradiction is not. In logics such as classical logic, consistency is well established, and indeed all theories are explosive; therefore, in any given classical theory, a contradiction turns out to be not only a necessary but also a sufficient condition for triviality.

Now, based on the above considerations, let us suppose in general that a proposition *can* be contradictory and still does not cause much harm, in general, in a paraconsistent logic, if only its consistency is not guaranteed, or cannot be established. Thus, an 'inconsistent' contradiction will be allowed to show up with no big commotion, but still a 'consistent' one should behave classically, and explode! This is how we will put it in formal terms. Let $\Delta(A)$ here denote a (possibly empty) set of schemas depending only on A. We will call a theory Γ *gently explosive* if:

$$\text{(a) } \exists A \text{ such that } \Delta(A)\cup\{A\} \text{ is not trivial, } \Delta(A)\cup\{\neg A\} \text{ is not trivial, and}$$
$$\text{(b) } \forall A\,\forall B\,[\Gamma, \Delta(A), A, \neg A \Vdash B]. \qquad \text{(D15)}$$

The gently explosive theory Γ will be said to be finitely so when $\Delta(A)$ is a finite set, so that a finitely gently explosive theory will be simply one that is finitely trivialized in a very distinctive way. (D16)

Accordingly, a logic **L** will be said to be *[finitely] gently explosive* when all of its theories are [finitely] gently explosive. [(D17)] (D18)

Thus, in any such a gently explosive logic, given a contradictory theory there is always something 'reasonable' —to wit, consistency— which one can add to it in order to guarantee that it will become trivial. We may now consider the following gentle versions of the Principle of Explosion:

[FINITE] GENTLE PRINCIPLE OF EXPLOSION [(fgPPS)] (gPPS)
L must be [finitely] gently explosive.

So, according to the interpretation proposed above, what we are implicitly assuming in the above principles is that, for any given formula A, the (finite) set $\Delta(A)$ will express, in a certain sense, the *consistency* of A relative to the logic **L**.

Based on that, we may define the consistency of a logic in the following way. **L** will be said to be *consistent* if:

$$\text{(a) } \mathbf{L} \text{ is gently explosive, and (b) } \forall A\,\forall\Gamma\,(\forall B\in\Delta(A))(\Gamma \not\Vdash B). \qquad \text{(D19)}$$

It immediately follows, from these definitions and the preceding ones, that:

FACT 2.14 (i) Any non-trivial explosive theory / logic is finitely gently explosive. (ii) Any transitive logic is consistent if, and only if, it is both explosive and non-trivial. (iii) Any transitive consistent logic is finitely gently explosive. (iv) Any left-adjunctive finitely gently explosive logic is supplementing explosive.

[not-(D2) and (D3)] $\Rightarrow$ (D16); [(PNT) and (PPS)] $\Rightarrow$ (fgPPS);
(Con3) $\Rightarrow$ {(D19) $\Leftrightarrow$ [(D6) and not-(D5)]};
[(Con3) and (D19)] $\Rightarrow$ (D17);
[(D13) and (fgPPS)] $\Rightarrow$ (sPPS)

Proof: To check (i), just let $\Delta(A)$ be empty, for every formula A. This result evidently parallels FACT 2.10(iii), about supplementing explosive logics. To see, in (ii), that any given consistent logic is explosive use transitivity whenever you meet a non-empty Δ. Part (iii) follows from (i) and (ii), and part (iv) simply reflects FACT 2.13(ii). □

So, based on the above definition of a consistent logic and the subsequent fact, if we were to define a so-called *Principle of Consistency*, it would then simply coincide with the sum of (PNT) and (PPS), for logics obeying transitivity. We shall, therefore, not insist in explicitly formulating here such a principle.

We may now finally define what we will mean by a *logic of formal inconsistency* (**LFI**), which will be nothing more than a logic that allows us to 'talk about consistency' in a meaningful way. We will consider, of course, an *inconsistent* logic to be simply one that is not consistent. This assumption, together with FACT 2.14(ii), explains why paraconsistent logics were early dubbed, by da Costa, 'inconsistent formal systems', once all paraconsistent logics are certainly inconsistent in the sense of not respecting (D19), even though they are always also non-trivial and quite often they are non-contradictory as well. Those inconsistent logics which went so far as to be trivial, and thus no more paraconsistent at all, were dubbed, by Miró Quesada, *absolutely inconsistent* logics (cf. [80]). Now, an **LFI** will be any non-trivial logic in which consistency does not hold, but can still be expressed, thus being a gently explosive and yet non-explosive logic, that is, a logic in which:

(a) (PPS) does not hold, but (b) (gPPS) holds. (D20)

Classical logic, then, will not be an **LFI** just because (PPS) holds in it. *Pac* will also not be an **LFI**, even though it is paraconsistent, for *Pac* is not finitely trivializable. But D'Ottaviano & da Costa's $\mathbf{J}_3$ (and, consequently, our **LFI1**), which conservatively extends *Pac*, will *indeed* be an **LFI**, where consistency is expressed by the connective $\circ$ (see above), and inconsistency, as usual, is expressed, by the negation of this connective. Also, Jaśkowski's **D2** will constitute an **LFI**, as the reader can easily check, where the consistency of a formula A can be expressed by the formula $(\Box A \vee \Box\neg A)$, written in terms of the necessity operator $\Box$ of $S5$.[11]

'Only' **LFI**s —though these seem to comprise by far the *great majority* of all known paraconsistent logics— will interest us in this study.

2.5 DEFCON 2: one step short of trivialization. The distinction between the original formulation of explosiveness, its formulation in terms of *ex falso*, and its supplementing and gentle formulations offered above does not tell you everything you need to know about the ways of exploding. Indeed, there are more things in the realm of explosiveness, dear reader, than are dreamt of in your philosophy! Thus, for instance, a not very interesting scenario seems to unfold if contradictions are still prevented from rendering a given theory trivial but nevertheless are allowed to go half the way, causing some kind of 'partial trivialization'. So, a theory Γ will be said to be *partially trivial with respect to* a given schema $\sigma(C_1, \ldots, C_n)$, or σ-*partially trivial*, if:

(a) $\exists C_1 \ldots \exists C_n$ such that $\sigma(C_1, \ldots, C_n)$ is not a top particle, and
(b) $\forall C_1 \ldots \forall C_n\ [\Gamma \Vdash \sigma(C_1, \ldots, C_n)]$. (D21)

Following this same path, a theory Γ will be said to be *partially explosive with respect to* the schema $\sigma(C_1, \ldots, C_n)$, or σ-*partially explosive*, if:

(a) $\exists C_1 \ldots \exists C_n$ such that $\sigma(C_1, \ldots, C_n)$ is not a top particle, and
(b) $\forall C_1 \ldots \forall C_n \forall A\ [\Gamma, A, \neg A \Vdash \sigma(C_1, \ldots, C_n)]$. (D22)

Of course, a logic **L** will be said to be σ-partially trivial / σ-partially explosive if all of its theories are σ-partially trivial / σ-partially explosive. respect. (D23), (D24)

More simply, a theory, or a logic, can now be said to be *partially trivial* / *partially explosive* if this theory, or logic, is σ-partially trivial / σ-partially explosive, for some

[11] This needs to be qualified. Among the various formulations among which **D2** has appeared in the literature, it is not completely clear if its language has a necessity operator available so as to make this definition possible, or not. If this is not available, it may well be that **D2** is not characterizable as an **LFI** after all (even though a situation for a necessity operator would quite naturally appear, to all practical purposes, in the trivial case in which there is just one person 'discussing', or even more unlikely, a situation in which all contenders just agree with each other).

schema σ. We can now immediately formulate the following new version of the Principle of Explosion:

PRINCIPLE OF PARTIAL EXPLOSION (pPPS)
L must be partially explosive.

One may immediately conclude that:

FACT 2.15 (i) Any partially trivial theory / logic is partially explosive. (ii) Any explosive logic is partially explosive. (D21) $\Rightarrow$ (D22); (D23) $\Rightarrow$ (D24); (PPS) $\Rightarrow$ (pPPS)

A well-known example of a logic which is not explosive but is partially explosive even so, is given by Kolmogorov & Johánsson's Minimal Intuitionistic Logic, **MIL**, which is obtained by the addition to the positive part of intuitionistic logic of some forms of *reductio ad absurdum* (cf. [68] and [69]). What happens, in this logic, is that $\forall\Gamma\forall A\forall B\,(\Gamma, A, \neg A \Vdash B)$ is *not* the case, but still it *does* hold that $\forall\Gamma\forall A\forall B\,(\Gamma, A, \neg A \Vdash \neg B)$. This means that **MIL** is paraconsistent in a broad sense, for contradictions do not explode, but still all *negated* propositions can be inferred from any given contradiction!

It is something of a consensus that an interesting paraconsistent logic should not only avoid triviality but also partial triviality. Thus, the following definition now comes in handy. A logic **L** will be said to be *boldly paraconsistent* if:

$$\text{(pPPS) fails for } \mathbf{L}. \qquad \text{(BPL)}$$

Evidently:

FACT 2.16 A boldly paraconsistent logic is paraconsistent. (BPL) $\Rightarrow$ (PL2)

Now, let's tackle a somewhat inverse approach. Call a theory Γ *controllably explosive in contact with* a given schema $\sigma(C_1, \ldots, C_m)$ if:

(a) $\exists C_1 \ldots \exists C_m$ such that $\sigma(C_1, \ldots, C_m)$ and $\neg\sigma(C_1, \ldots, C_m)$ are not bottom particles, and (b) $\forall C_1 \ldots \forall C_n \forall B\,[\Gamma, \sigma(C_1, \ldots, C_m), \neg\sigma(C_1, \ldots, C_m) \Vdash B]$. (D25)

Accordingly, a logic **L** will be said to be *controllably explosive in contact with* $\sigma(C_1, \ldots, C_m)$ when all of its theories are controllably explosive in contact with this schema. (D26)

Some given theory / logic can now more simply be called *controllably explosive* when this theory / logic has some schema in contact with which it is controllably explosive. An immediate new version of the Principle of Explosion that suggests itself then is:

CONTROLLABLE PRINCIPLE OF EXPLOSION (cPPS)
L must be controllably explosive.

Similarly to the case of FACT 2.14, parts (i) and (iii), it follows here that:

FACT 2.17 (i) Any non-trivial explosive theory / logic is controllably explosive. (ii) Any transitive consistent logic is controllably explosive. [not-(D2) and (D3)] $\Rightarrow$ (D25)
[(PNT) and (PPS)] $\Rightarrow$ (cPPS); [(Con3) and (D19)] $\Rightarrow$ (D26)

By the way, we may also now emend FACT 2.9 so as to immediately conclude that:

FACT 2.18 Any finitely-gently / controllably explosive logic is finitely trivializable, and yet non-trivial. [(D17) or (D26)] $\Rightarrow$ [(D7) and not-(D5)]

This fact can be used to update and complement the information conveyed in **Figure 2.1**.

Now, there seems to be no good reason to rule out controllably explosive theories, as we did in the case of partially explosive theories by way of the bold definition of paraconsistency, (BPL). In fact, it seems that most, if not all, finitely gently explosive logics *are* controllably explosive, and vice-versa! We will see, later on, many examples of paraconsistent logics —indeed, of **LFI**s— which not only are obviously gently explosive, but are also controllably explosive in contact with schemas such as $(A\wedge\neg A)$, or such as $\circ A$, where $\circ$, we recall, is a connective expressing consistency (Jaśkowski's **D2**, for instance, may already be one of these, but the logic **LFI1**, on the other hand, explodes only in contact with the second of these schemas). There are even logics which controllably explode in contact with large classes of non-atomic propositions (see [78], and ahead, for a number of them). An extreme case of these, as we shall see, is given by Sette's three-valued logic $\mathbf{P}^1$ (cf. [103]), which controllably explodes in contact with *any* complex formula, and so can be said to behave paraconsistently only at the level of its atoms. It is also not uncommon for some paraconsistent logic **L** having a strong negation $\div$ to be controllably explosive. In fact, it suffices that such a logic is transitive and infers $\neg\div A \Vdash A$, and of course it will turn out to be controllably explosive in contact with $\div A$, or at least in contact with $\div\div A$ (see, for instance, FACT 3.76, or THEOREM 3.51(i) and FACT 3.66). Many **LFI**s will moreover be controllably explosive in contact with any consistent formula (see FACT 3.32). And so on, and so forth.

A range of variations on the above versions of the Principle of Explosion can be obtained if we only mix the ones we already have. We shall nevertheless not investigate this theme here any further, but only notice that the multiple relations, hinted above, between (sPPS), (gPPS) and (cPPS), the supplementing, the gentle and the controllable forms of explosion, certainly deserve a closer and more attentive look by the 'paraconsistent community' and sympathizers.

2.6 C-systems. Given a logic $\mathbf{L}=\langle For, \Vdash\rangle$, let $For^+\subseteq For$ denote the set of all *positive formulas* of **L**, that is, the *negationless* fragment of *For*, or, in still other words, the set of all formulas in which no negation symbol $\neg$ occurs. The logic $\mathbf{L}1=\langle For_1, \Vdash_1\rangle$ is said to be *positively preserving relative to* the logic $\mathbf{L}2=\langle For_2, \Vdash_2\rangle$ if:

$$\text{(a) } For_1^+=For_2^+, \text{ and (b) } (\Gamma\Vdash_1 A \iff \Gamma\Vdash_2 A), \text{ for all } \Gamma\cup\{A\}\subseteq For_1^+. \qquad \text{(D27)}$$

So, if **L**1 is positively preserving relative to **L**2, then it will in general be a conservative extension of the positive fragment of **L**2. Now, as an example of the ubiquity of **LFI**s inside the realm of paraconsistent logics, just notice that:

FACT 2.19 Any paraconsistent logic that is positively preserving relative to classical logic and has a bottom particle can be characterized as an **LFI**.

Proof: Just define $\circ A$ as $(A\to\bot)\vee(\neg A\to\bot)$, and check that, in general, $\circ A$ is not a top particle, $\{\circ A, A\}$ is not always trivial, and $\{\circ A, \neg A\}$ is not always trivial, but that, in any case, $\{\circ A, A, \neg A\}$ is indeed a trivial theory. This result actually holds for any logic having a *left-adjunctive disjunction*, that is, a binary connective $\vee$ such that $(B\vee C)$ is not a bottom particle, for some formulas B and C, and such that $\forall B\forall C\forall\Gamma\forall\Delta\forall D\ \{[(\Gamma, B\Vdash D) \text{ and } (\Delta, C\Vdash D)] \Rightarrow [\Gamma, \Delta, (B\vee C)\Vdash D]\}$ (for a particular consequence of this feature, see FACT 3.7), and having *modus ponens*: $\forall\Gamma\forall A\forall B\ [\Gamma, A, (A\to B)\vdash B]$. You just have to choose $\Gamma=\{A\}$, $B=(A\to\bot)$, $\Delta=\{\neg A\}$, and $C=(\neg A\to\bot)$, and notice that, in this case, both $(\Gamma, B\Vdash\bot)$ and $(\Delta, C\Vdash\bot)$, by *modus ponens*. □

This last result shows that any paraconsistent logic conservatively extending the positive classical logic and respecting either one of the principles of *ex falso* or of supplementing explosion will be finitely gently explosive as well, throwing some light on some hitherto unsuspected connections between (ExF), (sPPS) and (fgPPS), and consequently any such a logic can be easily recast as an **LFI** (take another look at **Figure 2.2**). Consequently, for all such logics, it amounts to be more or less the same starting either with a consistency operator, or with a strong negation, or with a bottom particle: each of these can be used to define the others. This does not mean, however, that 'only' such logics are **LFI**s (see the case of C_{Lim}, in the subsection **3.10**).

To specialize a little bit from this very broad definition of **LFI**s above we will now define the concept of a **C**-system. The logic **L**1 will be said to be a **C**-*system based on* **L**2 if:

(a) **L**1 is an **LFI** in which consistency or inconsistency
are expressed by operators (at the object language level),
(b) **L**2 is not paraconsistent, and
(c) **L**1 is positively preserving relative to **L**2. (D28)

Any logic constructed as a **C**-system based on some other logic will more generally be identified simply as a **C**-*system*. In the next section we will study various logics which are **C**-systems, and pinpoint some which are not.

Jaśkowski's **D2**, as we have already seen in the above subsections, *is* an **LFI** and *can* define an operator expressing consistency —at least under some presentations (see note 11). But, in order for it to be characterized as a **C**-system it would still have to be clarified on which logic it is based, that is, where does its peculiar positive (non-adjunctive) part come from! This same question arises with respect to all other logics that are left-adjunctive but not left-disadjunctive, as well as with respect to many relevance logics.

All **C**-systems we will be studying below are inconsistent, non-contradictory and non-trivial. Furthermore, they are boldly paraconsistent (though the proof of *this* fact will be left for [42]), and often controllably explosive as well, they have strong negations and bottom particles, and are positively preserving relative to classical propositional logic —so, that they will respect (PNC), (PNT), (ExF), (sPPS), (gPPS) and often (cPPS), but they will not respect neither (PPS) nor (pPPS). Let's now jump to them.

3 COOKING THE **C**-SYSTEMS ON A LOW FLAME

> Indeed, even at this stage I predict a time when there will be mathematical investigations of calculi containing contradictions, and people will actually be proud of having emancipated themselves even from consistency.
> —Wittgenstein, Philosophical Remarks, p.332.

Underlying the original approach of da Costa to the concoction of a propositional calculus capable of admitting contradictions, yet remaining sensible to performing reasonable deductions, laid the idea of maintaining the positive fragment of classical logic unaltered. This explains why his approach to paraconsistency has eventually received the inelegant label of 'positive (logic) plus approach' and, more recently, the not much descriptive (and in some cases plainly inadequate) label of 'non-truth-functional approach' (cf., respectively, [92] and [94]). Surely, competitive approaches do exist, like the one stemming from Jaśkowski's or Rescher & Brandom's investigations, which rejects left-disadjunction, and is usually referred to as a 'non-adjunctive approach' (cf. [67] and [98]), and which has more recently been tentatively dubbed, by J. Perzanowski, as 'parainconsistent logic'.[12] Another megatrend comes from the 'relevance approach' to paraconsistency, captained by the American-Australian school, whose concern is not so much with negation as with implication, giving rise to 'relevance logics' (cf. [3]). Still another very interesting proposal came from Belgium, under the appellation of 'adaptive logics', which do not worry so much about proving consistency, but assume it instead from the very start, as some kind of default (cf. [11] and [12]). Now, let us make it crystal clear that our concentration in this study on the investigation of **C**-systems, born from the first approach mentioned above, wishes not to diminish the other approaches, nor affirms that they should be held as mutually exclusive. Our intention, indeed, is but to present the **C**-systems under a more general and suggestive background, and from now on we shall draw on the other approaches only when we feel that as a really necessary or instructive step. To the reader particularly interested in them, we prefer simply to redirect them to the competent sources.

3.1 Paleontology of C-systems. All definitions and remarks made above were set forth directing an arbitrary consequence relation $\Vdash$, be it syntactical, semantical or defined in any other mind-boggling way. Once the surfacing of contradictions on a theory involves negation, and nothing but that, it is appealing to consider and explore the intuitive idea that an interesting class of paraconsistent logics is to be given by the ones which are positively preserving relative to classical logic, differing from classical logic only in the behavior of formulas involving negation. This is the idea into which we will henceforth be digging, by axiomatically proposing a series of logics characterized by their syntactical consequence relations, $\vdash$, and containing all rules and schemas which hold in the positive part of classical logic. Thus, let's initially consider $\wedge$, $\vee$, $\rightarrow$, and $\neg$ to be our primitive connectives, and consider the set of formulas *For*, as usual, to be the free algebra generated by these connectives. We will start our journey from the following set of axioms:

(Min1) $\vdash_{min} (A\rightarrow(B\rightarrow A))$;
(Min2) $\vdash_{min} ((A\rightarrow B)\rightarrow((A\rightarrow(B\rightarrow C))\rightarrow(A\rightarrow C)))$;
(Min3) $\vdash_{min} (A\rightarrow(B\rightarrow(A\wedge B)))$;
(Min4) $\vdash_{min} ((A\wedge B)\rightarrow A)$;
(Min5) $\vdash_{min} ((A\wedge B)\rightarrow B)$;
(Min6) $\vdash_{min} (A\rightarrow(A\vee B))$;
(Min7) $\vdash_{min} (B\rightarrow(A\vee B))$;
(Min8) $\vdash_{min} ((A\rightarrow C)\rightarrow((B\rightarrow C)\rightarrow((A\vee B)\rightarrow C)))$;
(Min9) $\vdash_{min} (A\vee(A\rightarrow B))$;
(Min10) $\vdash_{min} (A\vee\neg A)$;
(Min11) $\vdash_{min} (\neg\neg A\rightarrow A)$.

Here, by writing $\vdash_{min} (A\rightarrow(B\rightarrow A))$ we will be abbreviatedly denoting that:

$$\forall\Gamma\,\forall A\,\forall B\,[\Gamma\vdash_{min} (A\rightarrow(B\rightarrow A))],$$

[12] On his conference delivered at the Jaśkowski's Memorial Symposium, held in Toru , Poland, July 1998.

and so on, for the other axioms. The only inference rule, as usual, will be *modus ponens*, (MP): $\forall\Gamma\forall A\forall B\ [\Gamma, A, (A\rightarrow B) \vdash_{min} B]$. The logic built using such axioms, plus (MP) and the usual notion of proof from premises (we may now be calling *proofs*, *theorems* and *premises* which we have previously called, respectively, inferences, theses and theories) was called $C_{min} = \langle For, \vdash_{min}\rangle$ and studied by the authors in [39].

First of all, let us observe that the so-called *Deduction Metatheorem* is here valid:

THEOREM 3.1 $[\Gamma, A \vdash_{min} B \;\Rightarrow\; \Gamma \vdash_{min} (A\rightarrow B)]$.[13]

Proof: It is a familiar and straightforward procedure to show that the Deduction Metatheorem holds for any logic containing (Min1) and (Min2) as provable schemas and having only *modus ponens* as a primitive rule. □

Evidently, by monotonicity and transitivity, *modus ponens* already gives us the converse of THEOREM 3.1. This makes it possible for us to introduce all axioms as some sort of axiomatic inference rules, and this is what we shall do from now on. Moreover, using the Deduction Metatheorem and its converse, one could now equivalently represent, in C_{min}, the fact that (PPS) (the Principle of Explosion) does not hold by the unprovability of the theorem (tPS): $(A\rightarrow(\neg A\rightarrow B))$. And indeed:

THEOREM 3.2 (tPS) is not provable by C_{min}.

Proof: Use the matrices of *Pac*, in the subsection **2.4**, to check that all axioms above are validated and that (MP) preserves validity, while (tPS) is not always validated. This shows that C_{min} is a fragment of *Pac*, and so it also cannot prove (tPS). In fact, (tPS) is more than non-provable, it is *independent* from C_{min} (and *Pac*) for its negation is not even classically provable, and *Pac* is a deductive fragment of classical logic. □

As usual, *bi-implication*, $\leftrightarrow$, will be defined by setting $(A\leftrightarrow B) \stackrel{\text{def}}{=} ((A\rightarrow B)\wedge (B\rightarrow A))$. Note that, by the above considerations, $\vdash_{min} (A\leftrightarrow B)$ if, and only if, $A \vdash_{min} B$, and $B \vdash_{min} A$, which is the same as writing $A \dashv\vdash_{min} B$. So, bi-implication holds between two formulas if, and only if, they are (provably) equivalent (see (Eq1), in the subsection **2.2**). Nevertheless, as the reader shall see below, having two equivalent formulas, in the logics we will be studying here, usually does *not* mean, as in classical logic, that these formulas can be freely intersubstituted everywhere (take a look, ahead, for instance, at results 3.22, 3.35, 3.51, 3.58, 3.65, and 3.74).

Axioms (Min1)–(Min8) are known at least since Gentzen's [62] as providing an axiomatization for the so-called 'positive logic'. Of course, they immediately tell us, among other things, that the conjunction of this logic is both left-adjunctive and left-disadjunctive (just take a look at axioms (Min3)–(Min5)). Nevertheless, (Min9): $(A\vee(A\rightarrow B))$, which *is* a positive schema, is *not* provable even if one uses (Min10) and (Min11) in addition to (Min1)–(Min8) and (MP) (i.e. the logic axiomatized as C_{min} minus the axiom (Min9))! Indeed:

THEOREM 3.3 (Min9) is not provable by $C_{min}\backslash\{\text{(Min9)}\}$.

Proof: Use the following matrices (cf. [2]) to check that (Min9) is independent from $C_{min}\backslash\{\text{(Min9)}\}$:

---

[13] Read this kind of sentence as a universally quantified one —in this case, for example, it would be $\forall\Gamma\,\forall A\,\forall B\,(\Gamma, A \vdash_{min} B \;\Rightarrow\; \Gamma \vdash_{min} (A\rightarrow B))$

$\wedge$	**1**	**½**	**0**
1	1	½	0
½	½	½	0
0	0	0	0

$\vee$	**1**	**½**	**0**
1	1	1	1
½	1	½	½
0	1	½	0

$\rightarrow$	**1**	**½**	**0**
1	1	½	0
½	1	1	0
0	1	1	1

	$\neg$
1	0
½	1
0	1

where 1 is the only distinguished value. □

So, what is this thing that Gentzen (and Hilbert before him) have dubbed 'positive logic', if even a deductive extension of it is unable to prove all positive theorems of classical logic? Here is the trick: Gentzen referred of course to positive *intuitionistic* logic, and not to the classical logic! So, this logic $C_{min}\backslash\{(\text{Min9})\}$, which was proposed by da Costa (cf. [49]) and called C_ω by him, turns out to be only positively preserving relative to intuitionistic logic, and not relative to classical logic. In [39] we have proven that its deductive extension C_{min}, obtained by adjoining (Min9) to C_ω, is indeed positively preserving relative to classical logic, and moreover:

THEOREM 3.4 C_{min} does have neither a strong negation nor a bottom particle, and is not finitely trivializable.
Proof: PROPOSITION 2.5, in [39], shows that C_{min} does not have a bottom particle, and so, by left-disadjunction and FACT 2.13, it does not have neither a strong negation nor is it finitely trivializable. □

Moreover, in [39] we also proved that:

THEOREM 3.5 C_{min} does not have any negated theorem, i.e. ($\nVdash_{min} \neg A$).

Of course, both results above are valid, *a fortiori*, for C_ω. Indeed, as shown by Urbas (cf. [107]), these logics are very weak with respect to negation, so that the following holds:

THEOREM 3.6 No two different negated formulas of C_{min} are provably equivalent.
Proof: The THEOREM 2, in [107], shows that $\neg A \dashv\vdash \neg B$ is derivable in C_ω if and only if A and B are the same formula. It is straightforward to adapt this result also to C_{min}. □

Much more about the provability (or validity) of negated theorems will be seen in the paper [42], which brings semantics to most logics here studied.

THEOREM 3.4 shows that C_{min}, or C_ω, *cannot* be **C**-systems based on classical logic, or intuitionistic logic, once they are both *compact* (all proofs are finite) and not finitely gently explosive, so that they cannot be gently explosive at all, and thus cannot formalize 'consistency', in the precise sense formulated in the subsection **2.4**. We had better then make them deductively stronger in order to get what we want.

We make a few more important remarks before closing this subsection. First, note that (Min10): $(A \vee \neg A)$ was added in order to keep C_{min} and C_ω from being *paracomplete* as well as paraconsistent (let's investigate one deviancy at a time!), and this axiom can indeed be pretty useful in providing us with a form of *proof by cases*:

FACT 3.7 $[(\Gamma, A \vdash_{min} B)$ and $(\Delta, \neg A \vdash_{min} B)] \Rightarrow (\Gamma, \Delta \vdash_{min} B)$.
Proof: From (Min8) and (Min10), by *modus ponens*, monotonicity, and the Deduction Metatheorem (from now on, we will not mention these last three every time we use them anymore). □

It will also be practical here and there to use $[(A \to B), (B \to C) \vdash_{min} (A \to C)]$ (a kind of logical version for the transitivity property) as an alternative form of the axiom (Min2). Indeed:

FACT 3.8 (Min2) can be substituted, in C_{min}, by $[(A \to B), (B \to C) \vdash_{min} (A \to C)]$.

We shall often make use of both these forms without discriminating which.

In the next subsection (see THEOREM 3.13) we will learn about the utility of (Min11): $(\neg\neg A \to A)$, which was added by da Costa as a way of rendering the negation of his calculi a bit stronger, using as an argument the intended duality with the logics arising from the formalization of intuitionistic logic, in which usually only the converse of (Min11), i.e. the formula $(A \to \neg\neg A)$, is valid.

It is quite interesting as well to notice that the addition of the 'Theorem of Pseudo-Scotus', (tPS), to C_{min} as a new axiom schema will not only prevent the resulting logic from being paraconsistent, but it will also provide a complete axiomatization for the *classical propositional logic* (hereby denoted **CPL**). In fact, it is a well-known fact that:

THEOREM 3.9 Axioms (Min1)–(Min11) plus (tPS): $(A \to (\neg A \to B))$, and (MP), provide a sound and complete axiomatization for **CPL**.

Actually, the axiom (Min11) can be discharged from the above axiomatization, being proved from the remaining ones. Axiom (Min9) also turns to be redundant (take a look at the FACT 3.45, below).

3.2 The basic logic of (in)consistency. Let's consider an extension of our language by the addition of a new unary connective, $\circ$, representing *consistency*. Let's now also add, to C_{min}, a new rule, realizing the Finite Gentle Principle of Explosion:

(bc1) $\circ A,\ A,\ \neg A \vdash_{\mathbf{bC}} B.$ 'If *A* is consistent and contradictory, then it explodes'

We will call this new logic, characterized by axioms (Min1)–(Min11) and (bc1), plus (MP), the *basic logic of (in)consistency*, or **bC**. Clearly, thanks to (bc1), we know that **bC** is indeed an **LFI**, i.e. a *logic of formal inconsistency*, and so it is in fact a **C**-*system* based on **CPL**. A strong negation, ~, for a formula *A* can now be easily defined by setting $\sim A \stackrel{\text{def}}{=} (\neg A \wedge \circ A)$, and evidently we will have $[A,\ \sim A \vdash_{\mathbf{bC}} B]$, as expected. A *bottom particle*, of course, is given by $(A \wedge \sim A)$, for any *A*. For alternative ways of formulating **bC**, consider FACT 2.19 and the comments which follow it.

We can already show that THEOREMS 3.5 and 3.6 do *not* hold for **bC**:

THEOREM 3.10 **bC** does have negated theorems, and equivalent negated formulas (but, on the other hand, it has no consistent theorems, that is, theorems of the form $\circ A$).

Proof: Consider any bottom particle $\bot$ of **bC**. By definition, it must be such that $(\bot \vdash_{\mathbf{bC}} B)$, for any formula *B*, and so, in particular, $(\bot \vdash_{\mathbf{bC}} \neg\bot)$. But we also have that $(\neg\bot \vdash_{\mathbf{bC}} \neg\bot)$, and proof by cases (FACT 3.7) tells us then that $\vdash_{\mathbf{bC}} \neg\bot$. By the way, this result also transforms THEOREM 3.4 into a corollary of THEOREM 3.5 —if a reflexive logic has proof by cases and no negated theorems, then it cannot contain a bottom particle. Evidently, any bottom particle is equivalent to any other. To check that no formula of the form $\circ A$ is provable, one may just use the classical matrices for $\wedge$, $\vee$, $\to$ and $\neg$, and pick for $\circ$ a matrix with value constant and equal to 0. □

Now, it is easy to see that, in such logic **bC**, if the consistency of the right formulas is guaranteed, than its inferences will behave exactly like in **CPL**. Indeed:

THEOREM 3.11 $[\Gamma \vdash_{\mathbf{CPL}} A] \Leftrightarrow [\circ(\Delta), \Gamma \vdash_{\mathbf{bC}} A]$, where $\circ(\Delta) = \{\circ B : B \in \Delta\}$, and Δ is a finite set of formulas.

Proof: On the one hand, one may just reproduce line by line a **CPL** proof in **bC**, and when it comes to an application of (tPS) —see an axiomatization of **CPL** in the THEOREM 3.9— one will have to use (bc1) instead, and add as a further assumption the consistency of the formula in the antecedent. The converse is immediate. □

We know that **bC** is a both a linguistic and a deductive extension of C_{min}, once it not only introduces a new connective but has an axiomatic rule telling you what to do with it. But we know more than that:

THEOREM 3.12 **bC** is a conservative extension of C_{min}.

Proof: Indeed, if you consider the **bC**-inferences in the language of C_{min}, you can no more use (bc1) along a proof, and so you can prove nothing more than you could prove before. □

What we have then, in (bc1), is a sort of rough logical clone for the finite gentle rule of explosion. Now, da Costa, in the original presentation of his calculi, which guides us here, has never used a gentle form of explosion but used instead a gentle form of *reductio ad absurdum*:

(RA0) $\quad \circ B,\ (A \to B),\ (A \to \neg B) \vdash \neg A.$

'If supposing A will bring us to a consistent contradiction, then $\neg A$ should be the case'

Notice, by the way, that $((A \to B) \to ((A \to \neg B) \to \neg A))$ was exactly the form of *reductio* used by Kolmogorov and Johánsson in the proposal of their Minimal Intuitionistic Logic, mentioned above as an example of a logic which is paraconsistent and yet not boldly paraconsistent. Now, the reader might be suspecting that it would really make no difference whether we used (bc1) or (RA0) in the characterization of **bC**. They are right, but this assertion could be made more precise. Indeed, consider the two following alternative versions of these rules:

(bc0) $\quad \circ A,\ A,\ \neg A \vdash \neg B;$

'If A is consistent and contradictory, then it partially explodes with respect to negated propositions'

(RA1) $\quad \circ B,\ (\neg A \to B),\ (\neg A \to \neg B) \vdash A,$

'If supposing $\neg A$ will bring us to a consistent contradiction, then A should be the case'

and consider the logic *PI* (that is how it was called when it appeared in [10]), characterized simply by (Min1)–(Min10) plus (MP), that is, C_{min} deprived of the schema (Min11): $(\neg\neg A \to A)$. Then we can prove that:

THEOREM 3.13 (i) It does *not* have the same effect adding either (bc1) or (RA0) to *PI*. (ii) It *does* have the same effect adding to *PI*: a) (bc0) or (RA0); b) (bc1) or (RA1). (iii) It *does* have the same effect adding to C_{min} whichever of the schemas (bc0), (bc1), (RA0) or (RA1). (iv) **bC** cannot be extended into a $\neg$-partially explosive paraconsistent logic.

Proof: To check part (i), use the classical matrices (with values 1 and 0) for $\wedge$, $\vee$ and $\to$, but let both $\neg$ and $\circ$ have matrices constant and equal to 1 —this way you will see that (bc1) is not provable by the logic obtained from the addition of (RA0) to *PI*. Part (ii) is easy: use FACT 3.8 to prove (bc0) in *PI* plus (RA0), and to prove (bc1) in *PI* plus (RA1); use (Min1) and the proof by cases to prove (RA0) in *PI* plus (bc0), and to prove (RA1) in *PI* plus (bc1). We leave part (iii) as an even easier exercise to the reader (*hint*: use (Min11)). (iv) is an immediate consequence of (iii). □

So, this last result gives one reason for us to have our study started from C_{min} rather than from *PI*: we will be avoiding that paraconsistent extensions of our initial logic might turn out to be partially explosive with respect to negated propositions in general, as what occurred with **MIL**, the Minimal Intuitionistic Logic (recall the subsection **2.5**). This feature will help in making many results below more symmetrical. But, to be sure, this does not guarantee that all such extensions will be boldly paraconsistent as well!

The reader should notice that there are, however, some restricted forms of ‘reasoning by absurdum’ left in **bC**. For example:

FACT 3.14 The following *reductio* deduction rules hold in **bC**:
(i) $[(\Gamma \vdash_{\mathbf{bC}} \circ A)$ and $(\Delta, B \vdash_{\mathbf{bC}} A)$ and $(\Lambda, B \vdash_{\mathbf{bC}} \neg A)] \Rightarrow (\Gamma, \Delta, \Lambda \vdash_{\mathbf{bC}} \neg B)$;
(ii) $[(\Gamma, B \vdash_{\mathbf{bC}} \circ A)$ and $(\Delta, B \vdash_{\mathbf{bC}} A)$ and $(\Lambda, B \vdash_{\mathbf{bC}} \neg A)] \Rightarrow (\Gamma, \Delta, \Lambda \vdash_{\mathbf{bC}} \neg B)$;
(iii) $[(\Gamma, \neg B \vdash_{\mathbf{bC}} \circ A)$ and $(\Delta, \neg B \vdash_{\mathbf{bC}} A)$ and $(\Lambda, \neg B \vdash_{\mathbf{bC}} \neg A)] \Rightarrow (\Gamma, \Delta, \Lambda \vdash_{\mathbf{bC}} B)$.
Proof: Part (i) comes immediately from (RA0), part (ii) comes from part (i) using reflexivity and proof by cases, part (iii) comes as a variation of (ii), if you use (Min11). □

But we still have not mentioned some of the most decisive features of **bC**! We are now ready for this. Consider, to start with, the following result:

THEOREM 3.15 (i) $(A \wedge \neg A)$ is not a bottom particle in any paraconsistent extension of **bC**. (ii) $\neg(A \wedge \neg A)$ and $\neg(\neg A \wedge A)$ are not top particles in **bC**.
Proof: For part (i), just use left-disadjunction and THEOREM 3.2 (but the reader might recall from the subsection **2.3** that this formula *is* a bottom particle in some non-left-disadjunctive paraconsistent logics such as Jaśkowski’s **D2**). To check part (ii) use the following matrices to confirm that neither $\neg(A \wedge \neg A)$ nor $\neg(\neg A \wedge A)$ are provable by **bC**:

$\wedge$	**1**	**½**	**0**
1	1	1	0
½	1	1	0
0	0	0	0

$\vee$	**1**	**½**	**0**
1	1	1	1
½	1	1	1
0	1	1	0

$\rightarrow$	**1**	**½**	**0**
1	1	1	0
½	1	1	0
0	1	1	1

	$\neg$	$\circ$
1	0	1
½	1	0
0	1	1

where 1 and ½ are the distinguished values. By the way, the matrices of $\wedge$, $\vee$, $\rightarrow$, and $\neg$ are exactly the same matrices which originally defined the maximal three-valued logic $\mathbf{P}^1$, proposed in [103], and mentioned in the subsection **2.5** as a logic which is paraconsistent and yet controllably explosive when in contact with any non-atomic formula. □

As to the relations between contradictions and inconsistencies what we will find here are some variations on the intuitive idea that a contradiction should not be consistent (but not necessarily the other way around):

FACT 3.16 These are some special rules of **bC**, relating contradiction and consistency:
(i) $A, \neg A \vdash_{\mathbf{bC}} \neg\circ A$;
(ii) $(A \wedge \neg A) \vdash_{\mathbf{bC}} \neg\circ A$;
(iii) $\circ A \vdash_{\mathbf{bC}} \neg(A \wedge \neg A)$;
(iv) $\circ A \vdash_{\mathbf{bC}} \neg(\neg A \wedge A)$.
The converses of these rules do *not* hold in **bC**.

	¬
1	0
½	0
0	1

Proof: Use FACT 3.14 to prove (i), and left-adjunction to jump from this fact to (ii); play similarly to prove (iii) and (iv). To show that none of the converses of (ii)– (iv) are provable by **bC**, use the same matrices as in THEOREM 3.15(ii), substituting only the matrix for negation by this one to the right. □

The significance of stating both (iii) and (iv) is to draw attention to the fact that, in what follows, logics will be shown in which, due to some unexpected asymmetry, only one of their converses hold. This is the case, for instance, for C_1, the first logic of the pioneering hierarchy of paraconsistent logics, C_n, $1 \leq n < \omega$, proposed by da Costa (cf. [49] or [50]). As we shall see, the converse of (iii) holds in C_1, while the converse of (iv) fails, so that $\neg(A \wedge \neg A)$ and $\neg(\neg A \wedge A)$ are *not* equivalent formulas in this logic (in this respect, see also THEOREM 3.21(iii)).

As the reader will learn in the next subsection (THEOREM 3.20), the regular forms of 'reasoning by contraposition' cannot be valid in any logic which is, as **bC** and its extensions (cf. THEOREM 3.13(iv)), both positively preserving with respect to classical logic and not partially explosive with respect to negation. But there are some restricted forms of it that hold already in **bC**:

FACT 3.17 These are some restricted forms of contraposition that hold in **bC**:

(i) $\circ B, (A \to B) \vdash_{\mathbf{bC}} (\neg B \to \neg A)$;
(ii) $\circ B, (A \to \neg B) \vdash_{\mathbf{bC}} (B \to \neg A)$;
(iii) $\circ B, (\neg A \to B) \vdash_{\mathbf{bC}} (\neg B \to A)$;
(iv) $\circ B, (\neg A \to \neg B) \vdash_{\mathbf{bC}} (B \to A)$.

Proof: To check (i), let $\Gamma = \Delta = \Lambda = \{\circ B, (A \to B), \neg B\}$ and apply FACT 3.14(ii) to $\Gamma \cup \{A\}$, so as to obtain $\Gamma \vdash_{\mathbf{bC}} \neg A$. From this it follows that $[\circ B, (A \to B) \vdash_{\mathbf{bC}} (\neg B \to \neg A)]$. Part (ii) is similar to (i). For parts (iii) and (iv) apply FACT 3.14(iii). □

Now, may the reader be aware that rules such as $[\circ A, (A \to B) \vdash_{\mathbf{bC}} (\neg B \to \neg A)]$ *do not* hold in this logic!

3.3 On what one cannot get. If 'logic is about trade-offs', as Patrick Blackburn likes to put it, let us now start counting the dead bodies to see what we have irremediably lost, up to now. The connectives $\wedge$, $\vee$ and $\to$ of **bC**, for example, show up as quite independent from one another, and cannot be interdefined as in the classical case:

THEOREM 3.18 The following rule holds in **bC**:

(i) $(\neg A \to B) \vdash_{\mathbf{bC}} (A \vee B)$,

but none of the following rules hold in **bC**:

(ii) $(A \vee B) \vdash_{\mathbf{bC}} (\neg A \to B)$;
(iii) $\neg(\neg A \to B) \vdash_{\mathbf{bC}} \neg(A \vee B)$;
(iv) $\neg(A \vee B) \vdash_{\mathbf{bC}} \neg(\neg A \to B)$;
(v) $(A \to B) \vdash_{\mathbf{bC}} \neg(A \wedge \neg B)$;
(vi) $\neg(A \wedge \neg B) \vdash_{\mathbf{bC}} (A \to B)$;
(vii) $\neg(A \to B) \vdash_{\mathbf{bC}} (A \wedge \neg B)$;
(viii) $(A \wedge \neg B) \vdash_{\mathbf{bC}} \neg(A \to B)$;
(ix) $\neg(A \wedge B) \vdash_{\mathbf{bC}} (\neg A \vee \neg B)$;
(x) $(\neg A \vee \neg B) \vdash_{\mathbf{bC}} \neg(A \wedge B)$;
(xi) $\neg(\neg A \vee \neg B) \vdash_{\mathbf{bC}} (A \wedge B)$;
(xii) $(A \wedge B) \vdash_{\mathbf{bC}} \neg(\neg A \vee \neg B)$.

Proof: This is much easier to directly check after you take a look at the semantics and decision procedure of **bC**, in the paper [42]. But it also comes as a consequence from the fact that this is already valid for C_{min}, as we have proved in [39], and that **bC** is a conservative extension of it (THEOREM 3.12). □

Notice that any uniform substitution of a component formula C for its negation $\neg C$, or vice-versa, will not alter the fact that the above rules hold or not in **bC**. That is to say, for instance, that $(A \to \neg B) \vdash_{\mathbf{bC}} (\neg A \vee \neg B)$ does hold but $(\neg A \vee B) \vdash_{\mathbf{bC}} (A \to B)$ does not. Of course, the failure of a rule such as $(A \vee \neg B) \vdash_{\mathbf{bC}} \neg(A \wedge \neg B)$ was already to be expected from the fact that $(A \vee \neg A)$ is provable (it is (Min10)) but $\neg(A \wedge \neg A)$ is not (see THEOREM 3.15(ii)).

Now, it should be crystal-clear that the above fact is only about **bC**, and that it does not necessarily carry on to stronger logics. In fact, it is not hard at all to check, for instance, that the three-valued maximal logic **LFI1**, whose matrices were presented in the subsection **2.4**, both extends **bC** and validates all the rules above, except for (ii) and (vi). Once more, the non-validity of (vi) is barely circumstantial, for there are logics extending **bC** in which it holds, such as the above mentioned $\mathbf{P}^1$ (see also the results 3.68 and 3.70, below). Still and all, there *is* a very good reason for the failure of (ii)! Indeed, this is a consequence of the following fact:

THEOREM 3.19 The *disjunctive syllogism*, $[A, (\neg A \vee B) \vdash B]$, cannot hold in any paraconsistent extension of positive (classical or intuitionistic) logic.

Proof: Assume that it held. From (Min6), we would have that $[\neg A \vdash (\neg A \vee B)]$ and so, ultimately, we would conclude, by the transitivity of $\vdash$, that $[A, \neg A \vdash B]$. □

Finally, as we have already advanced above, 'full' contraposition is lost (cf. [54]):

THEOREM 3.20 The regular forms of *contraposition*, such as $[(A \to B) \vdash (\neg B \to \neg A)]$, cannot hold irrestrictedly in any paraconsistent extension of **bC**. Furthermore, they cannot hold in any extension of the positive classical logic which happens to be not $\neg$-partially explosive.

Proof: If the above rule held in a logic **L** that extends the positive classical logic, from (Min1) we would obtain $[B \vdash (A \to B)]$, and from (MP) we obtain $[(A \to B), \neg B \vdash \neg A]$. These two rules would ultimately lead to $[B, \neg B \vdash \neg A]$, and so **L** would be partially explosive with respect to negated propositions. If we assume **L** to be **bC**, then a particular case of $[B, \neg B \vdash \neg A]$ would be $[B, \neg B \vdash \neg\neg C]$, taking A as $\neg C$, and (Min11) would then give $[B, \neg B \vdash C]$, and so it would not be paraconsistent at all. Indeed, this addition of contraposition to **bC** would simply cause the collapse of the resulting logic into classical logic (by THEOREM 3.9). Still some other forms of this contraposition rule, such as $[(\neg A \to B) \vdash (\neg B \to A)]$, could be ruled out even without recurring to (Min11), or to partial explosion. □

The use of the disjunctive syllogism (THEOREM 3.19) constitutes indeed the kernel of the well-known argument laid down by C. I. Lewis for the derivation of (PPS) in classical logic (cf. [73], pp.250ff), and this was, in fact, a rediscovery of an argument used by the Pseudo-Scotus, much before.[14] The use of contraposition (THEOREM 3.20) to the same purpose was pointed out in an argument by Popper (cf. [89], pp.320ff). Of course, in a logic where both the disjunctive syllogism and contraposition are invalid derivations, these arguments do not apply as such.

[14] See Duns Scotus's *Opera Omnia*, pp.288ff. Cf. also note 3.

The failure of contraposition gives us a good reason for having doubts also about the validity of the *intersubstitutivity of provable equivalents*, which states that, given a schema $\sigma(A_1, \ldots, A_n)$:

$$\forall B_1 \ldots \forall B_n\ [(A_1 \dashv\vdash B_1) \text{ and } \ldots \text{ and } (A_n \dashv\vdash B_n)] \Rightarrow [\sigma(A_1, \ldots, A_n) \dashv\vdash \sigma(B_1, \ldots, B_n)]. \tag{IpE}$$

Now, as a particular example, if we had (IpE), from $A \dashv\vdash B$ we would immediately derive, for instance, $\neg A \dashv\vdash \neg B$. But this is not the case here. Indeed, in what follows we exhibit some samples of that failure in **bC**:

THEOREM 3.21 In **bC**:

(i) $(A \wedge B) \dashv\vdash_{\mathbf{bC}} (B \wedge A)$ holds, but $\neg(A \wedge B) \dashv\vdash_{\mathbf{bC}} \neg(B \wedge A)$ does not;

(ii) $(A \vee B) \dashv\vdash_{\mathbf{bC}} (B \vee A)$ holds, but $\neg(A \vee B) \dashv\vdash_{\mathbf{bC}} \neg(B \vee A)$ does not;

(iii) $(A \wedge \neg A) \dashv\vdash_{\mathbf{bC}} (\neg A \wedge A)$ holds, but $\neg(A \wedge \neg A) \dashv\vdash_{\mathbf{bC}} \neg(\neg A \wedge A)$ does not.

Proof: The parts which hold are easy, using positive classical logic. Now, to check that none of the other parts hold, even if axioms and rules of **bC** are taken into consideration, use the same matrices and distinguished values as in THEOREM 3.15(ii), changing only the values of $(1 \wedge ½)$ and $(1 \vee ½)$ from 1 to ½ (but leaving the values of $(½ \wedge 1)$ and $(½ \vee 1)$ as they are, equal to 1). □

COROLLARY 3.22 (IpE) does not hold for **bC**.

The reader should keep in mind that this last result is, initially, only about **bC**, and that some deductive extensions of it may fix some or even all the counter-examples to intersubstitutivity. Now, given that (IpE) holds for classical logic, it will obviously hold for the positive (classical) fragment of **bC** as well, that is, for the set of formulas in which neither $\neg$ nor $\circ$ occur. Adding contraposition as a new inference rule, it is easy to see, by the transitivity of the consequence operator and the Deduction Metatheorem, that one could extend (IpE) from positive logic to include also the fragment of **bC** containing negation. But then (bold) paraconsistency would be lost, as we learn from THEOREM 3.20! What happens, though, is that the contraposition inference rule is much more than one needs in order to obtain intersubstitutivity for the consistencyless fragments of our logics. In fact, any of the following ‘contraposition’ deduction rules would of course do the job equally well (cf. [107] and [105]):

$$\forall A\, \forall B\ [(A \vdash B) \Rightarrow (\neg B \vdash \neg A)]; \tag{RC}$$

$$\forall A\, \forall B\ [(A \dashv\vdash B) \Rightarrow (\neg B \vdash \neg A)]. \tag{EC}$$

It is obvious that (EC) can be inferred from (RC), and Urbas has shown in [107] that the paraconsistent logic obtained by adding (EC) to C_ω is extended by the paraconsistent logic obtained by the addition of (RC) to C_ω (and both, of course, are extended by classical logic). So, it *is* possible to obtain paraconsistent extensions of C_ω (and also of C_{min}, for Urbas’s proof of non-collapse into classical logic by the addition of (EC) also applies to this logic), but then these new logics can all still be shown to lack a bottom particle (as in THEOREM 3.4), constituting thus no **LFI**s! The question then would be if (IpE) could be obtained for *real* **LFI**s. The closest we will get to this here is showing, in THEOREM 3.53, that there are fragments of classical logic extending **bC** for which (IpE) holds, but then these specific fragments turn out not to be paraconsistent in our sense. At any rate, for various other classes of **LFI**s we will show that such intersubstitutivity results are just unattainable, as shown in THEOREM 3.51 (see also, for instance, FACT 3.74).

To be sure, one does not need to blame *paraconsistency* for these last few negative results. As the reader will see below, the eccentricities in THEOREM 3.21 can be fixed by some extensions of **bC**. As for THEOREM 3.20, one could always throw away some piece of the positive classical logic in an extreme effort to avoid its consequences. This is what is done, for instance, by some logics of relevance. This could, however, have the effect of throwing the baby out with the bath water —most such logics, if not all, will also dismiss the useful Deduction Metatheorem or, regrettably enough, *modus ponens*. Now, suppose that, driven by itches of relevance, one was taken to consider logics such that (A, $B \nVdash A$). This would definitely mean, thus, that their consequence relations would be no more than 'cautiously reflexive'. If one still insisted that ($A \Vdash A$) should hold, then the logics produced would be non-monotonic as well. This would mean, of course, that many of the results that we attained in the last section would not be immediately adaptable to such logics (and this remark also applies to adaptive logics, once they are also non-monotonic, even if for other reasons). These are not problems of actual relevance logics, nevertheless, as they are usually relevant only at the level of theoremhood (always invalidating ($A\to(B\to A)$)), while in some cases still validating ($A\to A$)), but still not at the level of their consequence relations, as conjectured above (see, for instance, [3] or [9]) —and of course, in all such cases, the Deduction Metatheorem cannot hold. But yes, we had better push our exposition on, instead of scrubbing this matter here any further.

3.4 Letting bC talk about (dual) inconsistency. The reader may find it a bit awkward, indeed, that we would be calling **bC** a logic of formal *inconsistency*, since it only has a connective expressing *consistency*, but not its opposed concept. So, for *us* to be more consistent, let's now consider a further extension of our language, this time adding a new unary connective, •, to represent inconsistency. The intended interpretation about the dual relation between consistency and inconsistency would require exactly that each of these concepts should be opposed to the other. But how do we formalize this? Consider the following additional axiomatic rule:

(bc2) $\quad \neg\bullet A \vdash_{\mathbf{bbC}} \circ A$. 'If A is not inconsistent, then it is consistent'

This is surely a must, but in fact it does not represent much of an addition. Indeed, consider its contrapositional variation:

(bc3) $\quad \neg\circ A \vdash_{\mathbf{bbC}} \bullet A$. 'If A is not consistent, then it is inconsistent'

The lack of contraposition (see THEOREM 3.20), despite the presence of some restricted forms of it (such as in FACT 3.17) can be partly blamed for the fact that **bC** plus (bc2) can still not prove (bc3). Indeed:

THEOREM 3.23 (bc3) is not provable by **bC** plus (bc2).

Proof: Just consider three-valued matrices such that: $v(A\wedge B)=0$ if $v(A)=0$ or $v(B)=0$, and $v(A\wedge B)=1$, otherwise; $v(A\vee B)=0$ if $v(A)=0$ and $v(B)=0$, and $v(A\vee B)=1$, otherwise; $v(A\to B)=0$ if $v(A)\neq 0$ and $v(B)=0$, and $v(A\to B)=1$, otherwise; $v(\neg A)=1-v(A)$; and the matrices for the non-classical connectives are the ones demonstrated on the right. 0 is the only non-distinguished value. □

	∘	•
1	0	1
½	0	1
0	½	0

So, let us now, for the sake of symmetry, define the logic **bbC** as given by the addition of both (bc2) and (bc3) to the basic logic of (in)consistency, **bC**. This is still not much… for consider now the converses of these rules:

(bc4) $\bullet A \vdash_{\mathbf{bbbC}} \neg{\circ}A$; 'If *A* is inconsistent, then it is not consistent'
(bc5) ${\circ}A \vdash_{\mathbf{bbbC}} \neg\bullet A$. 'If *A* is consistent, then it is not inconsistent'

Will these hold in **bbC**? The answer is once more in the negative:

THEOREM 3.24 Neither (bc4) nor (bc5) are provable by **bbC**.
Proof: Consider the same three-valued matrices for the binary connectives as in THEOREM 3.23, but let now negation be such that $v(\neg A)=0$ if $v(A)\neq 0$, and $v(\neg A)=1$, otherwise. The non-classical connectives will now be defined by the new matrices to the right. Once more, 0 is the only non-distinguished value. □

	$\circ$	$\bullet$
1	0	1
½	½	1
0	0	1

In reality, the situation is even worse than it may appear at first sight, though predictable. It happens that, once more, it is not enough to add just one of (bc4) or (bc5) to **bbC** —the other one would still not be provable. Indeed:

THEOREM 3.25 (i) (bc4) is not provable by **bbC** plus (bc5); (ii) (bc5) is not provable by **bbC** plus (bc4).
Proof: Consider now the four-valued matrices where $\wedge$, $\vee$, $\rightarrow$ and $\neg$ are once more defined as in THEOREM 3.23 (only that now they have a wider domain, with four values). For part (i), let $\circ$ and $\bullet$ be given by the matrices to the right. For part (ii), just modify $\circ$ so that ${\circ}(⅔)=⅓$ (and no more ⅔); modify also $\bullet$ in the contrary sense, so that $\bullet(⅔)=⅔$ (and no more ⅓). In both cases, only 0 should be taken to be a non-distinguished value. □

	$\circ$	$\bullet$
1	1	0
⅔	⅔	⅓
⅓	0	⅔
0	1	0

Taking the above results into account, we will now define the logic **bbbC** to be given by the addition of both (bc4) and (bc5) to the preceding **bbC**.

It is important to note that the last theorem above also shows that it is ineffective trying to introduce the inconsistency connective in the logic **bC** simply by setting, by definition, $\bullet A \stackrel{\text{def}}{=} \neg{\circ}A$. The reason is that, even though this would automatically guarantee that $\bullet A \dashv\vdash \neg{\circ}A$, and that $\neg\bullet A \dashv\vdash \neg\neg{\circ}A$, and so on, just by definition and reflexivity, this would *not* guarantee as well that, for instance, we would have ${\circ}A \vdash \neg\bullet A$. Indeed, to check this you may here just reconsider THEOREM 3.25(ii). So, the relation between $\circ$ and $\bullet$ cannot, in the cases of **bC** and **bbC**, be characterized by a simple definition. Despite this, one may now establish new presentations for some previous facts and theorems, just slightly different from before:

THEOREM 3.26 The results 3.11, 3.14, 3.15, 3.16, and 3.17 are all valid for **bbbC**, and are still valid as well if one substitutes any occurrence of $\circ$ for $\neg\bullet$, and $\neg\circ$ for $\bullet$.
Proof: This is routine, just using (bc2)–(bc5). For 3.15 and 3.16 remember to add a matrix for $\bullet$, just negating the matrix for $\circ$ presented in THEOREM 3.15(ii). □

So, could the relation between $\circ$ and $\bullet$ be characterized by a definition, now that we have **bbbC**? Another NO is the answer. For if a definition such as $\bullet A \stackrel{\text{def}}{=} \neg{\circ}A$ were feasible, this would mean, given (bc5): ${\circ}A \vdash_{\mathbf{bbbC}} \neg\bullet A$, that ${\circ}A \vdash_{\mathbf{bbbC}} \neg\neg{\circ}A$ should hold just by straightforward substitution. But, as it happens, this last rule does *not* hold in **bbbC**:

THEOREM 3.27 Neither $\circ A \to \neg\neg\circ A$ nor $\bullet A \to \neg\neg\bullet A$ are provable by **bbbC**.

Proof: Consider once more the same three-valued matrices for the binary connectives given in THEOREM 3.23, 0 as the only non-distinguished value, but now let the unary connectives be those pictured to the right. □

	$\neg$	$\circ$	$\bullet$
1	0	½	½
½	1	0	1
0	1	0	1

Evidently, the above matrices must also display the non-provability by **bbbC** of the schema $A \to \neg\neg A$, the converse of (Min11). But if the validity of $A \to \neg\neg A$ clearly implies the validity of the two schemas in THEOREM 3.27, the validity of those schemas certainly *does not* imply the validity of $A \to \neg\neg A$. Indeed:

THEOREM 3.28 $A \to \neg\neg A$ is not provable by **bbbC** plus $\circ A \to \neg\neg\circ A$ and $\bullet A \to \neg\neg\bullet A$.

Proof: Consider the same matrices and distinguished values as in THEOREM 3.27, only that now $\circ$ is constant and equal to 0, and $\bullet$ is constant and equal to 1. □

Now, if we added to **bbbC** the axioms $\circ A \vdash \neg\neg\circ A$ and $\bullet A \vdash \neg\neg\bullet A$ this would only shift our problem to proving that $\neg\bullet A \vdash \neg\neg\neg\bullet A$ and $\neg\circ A \vdash \neg\neg\neg\circ A$ hold, and so on, and so forth. Of course, these would be all guaranteed if we now defined **bbbbC** by the addition to **bbbC** of an infinite number of axiomatic rules, to the effect that $\neg^n\circ A \vdash_{\mathbf{bbbbC}} \neg^{n+2}\circ A$ and $\neg^n\bullet A \vdash_{\mathbf{bbbbC}} \neg^{n+2}\bullet A$, where $\neg^m$ denotes m occurrences of negation in a row. We could also solve all of this at once by fixing $A \vdash \neg\neg A$ as a new axiomatic rule, but we argue that it is a bit too early for this last solution —indeed, there is a gamut of interesting **C**-systems in which this axiom does *not* hold, and we would rather explore them first. So, let us study first, in what follows, some other forms of obtaining the intended duality between $\circ$ and $\bullet$ using a finite set of schemas, and without yet incorporating $A \vdash \neg\neg A$ as a rule.

3.5 The logic Ci, where contradiction and inconsistency meet. While strengthening **bC**, we have been trying to keep up with the intended duality between consistency and inconsistency. But, given the new version of the FACT 3.16 obtained in THEOREM 3.26 (which also applies to **bbbbC**), we know that in any of the logics **b**(**b**(**b**(**b**)))**C** a contradiction implies an inconsistency, but not the other way around —so, *this* situation has still not been changed. Now, the distinction between contradiction and inconsistency is a contribution of the present study, and we are unaware of any other formal attempts to do so in the same way as we do here. What will happen then if we now introduce new axioms in order to finally obtain the identification of contradiction and inconsistency, getting closer this way to the other paraconsistent logics in the literature? Let's do it. Consider the two following axiomatic rules:

(ci1) $\bullet A \vdash_{\mathbf{Ci}} A$; 'If A is inconsistent, then A should be the case'

(ci2) $\bullet A \vdash_{\mathbf{Ci}} \neg A$. 'If A is inconsistent, then $\neg A$ should be the case'

Given the classical properties of conjunction, these two rules will evidently have the same effect as the following single one:

(ci) $\bullet A \vdash_{\mathbf{Ci}} (A \wedge \neg A)$. 'An inconsistency implies a contradiction'

So, let's call **Ci** the logic obtained by the addition of (ci1) and (ci2) (or, equivalently, the addition of (ci)) to **bbbC**, that is, the logic axiomatized by (Min1)–(Min11), (bc1)–

(bc5), (ci), and (MP). In **Ci** we finally have that $\bullet A$ and $(A \wedge \neg A)$ are equivalent formulas, and we shall see that this will make a BIG difference on **Ci**'s deductive strength.

First, let us note that, even though we now have, in **Ci**, the converse of parts (i) and (ii) of FACT 3.16, the converses of parts (iii) and (iv) still do *not* hold. Indeed:

FACT 3.29 This rule does hold in **Ci**:
(i) $\neg\circ A \vdash_{\mathbf{Ci}} (A \wedge \neg A)$,
but the following rules do not:
(ii) $\neg(A \wedge \neg A) \vdash_{\mathbf{Ci}} \circ A$;
(iii) $\neg(\neg A \wedge A) \vdash_{\mathbf{Ci}} \circ A$.
Proof: The first part is obvious. For the following ones, consider, for instance, the three-valued matrices such that:
$v(A \wedge B) = min(v(A), v(B))$;
$v(A \vee B) = max(v(A), v(B))$;
$v(A \to B) = v(B)$, if $v(A) \neq 0$, and $v(A \to B) = 1$, otherwise;
$v(\neg A) = 1 - v(A)$;
$v(\circ A) = 0$, if $v(A) = v(\neg A)$, and $v(\circ A) = 1$, otherwise;
$v(\bullet A) = 1$, if $v(A) = v(\neg A)$, and $v(\bullet A) = 0$, otherwise,
where 0 is the only non-distinguished value. The attentive reader might have noticed that these are exactly the matrices defining the already mentioned **LFI1**, in the subsection **2.4**. □

So, this last theorem reminds us that, even though in **Ci** we *do* have an equivalent way of referring to inconsistency using just the classical language, this does not mean that we should also have an immediate **CPL**-linguistic equivalent manner of referring to *consistency* as well (but confront this with what happens in the case of the **dC**-systems, in the subsection **3.8**)! There are, however, many other things that we *do* have. For instance, in **Ci** the THEOREM 3.15 is still entirely valid. Indeed:

THEOREM 3.30 $\neg(A \wedge \neg A)$ and $\neg(\neg A \wedge A)$ are not top particles in **Ci** (also the formula $(A \to \neg\neg A)$ is still not provable).
Proof: Use again the matrices of $\mathbf{P}^1$ (in THEOREM 3.15(ii)), adding a matrix for '$\bullet$' by negating the matrix for '$\circ$'. □

We also have in **Ci** some new ways of formulating gentle explosion and the *reductio* deduction rules:

FACT 3.31 The following rules hold in **Ci**:
(i) $\circ A, \bullet A \vdash_{\mathbf{Ci}} B$;
(ii) $\circ A, \neg\circ A \vdash_{\mathbf{Ci}} B$;
(iii) $\bullet A, \neg\bullet A \vdash_{\mathbf{Ci}} B$;
(iv) $[(\Gamma, B \vdash_{\mathbf{Ci}} \circ A)$ and $(\Delta, B \vdash_{\mathbf{Ci}} \bullet A)] \Rightarrow (\Gamma, \Delta \vdash_{\mathbf{Ci}} \neg B)$;
(v) $[(\Gamma, B \vdash_{\mathbf{Ci}} \circ A)$ and $(\Delta, B \vdash_{\mathbf{Ci}} \neg\circ A)] \Rightarrow (\Gamma, \Delta \vdash_{\mathbf{Ci}} \neg B)$;
(vi) $[(\Gamma, B \vdash_{\mathbf{Ci}} \bullet A)$ and $(\Delta, B \vdash_{\mathbf{Ci}} \neg\bullet A)] \Rightarrow (\Gamma, \Delta \vdash_{\mathbf{Ci}} \neg B)$.
Proof: Part (i) comes from (ci) and (bc1), parts (ii) and (iii) come from part (i) if you use (bc2)–(bc5). Rules (iv), (v) and (vi) are variations on FACT 3.14(ii), using the previous rules. □

Parts (ii) and (iii) of FACT 3.31 simply show **Ci** to be controllably explosive in contact either with a consistent or with an inconsistent formula. In fact, in **Ci** one can go on to prove a much more intimate connection between consistency and controllable explosion, and this will reveal some even stronger consequences of the new axiomatic rule, (ci), that we now consider:

FACT 3.32 A particular given schema in **Ci** (or in any extension of this logic) is consistent if, and only if, **Ci** is controllably explosive in contact with this schema.

Proof: To show that $[(\Gamma \vdash_{\mathbf{Ci}} \circ A) \Rightarrow (\Gamma, A, \neg A \vdash_{\mathbf{Ci}} B)]$ just invoke axiom (bc1) and the transitivity of $\vdash$. For the converse, note that, from (ci) and (bc3), one may obtain $[\neg\circ A \vdash_{\mathbf{Ci}} (A \wedge \neg A)]$, and so, from the supposition that $(\Gamma, A, \neg A \vdash_{\mathbf{Ci}} B)$ it follows that $\neg\circ A$ is a bottom particle. One may then conclude, as in THEOREM 3.10, that $\vdash_{\mathbf{Ci}} \neg\neg\circ A$, and, by (Min11), that $\vdash_{\mathbf{Ci}} \circ A$. □

FACT 3.33 These are some special theses of **Ci**:

(i) $\vdash_{\mathbf{Ci}} \circ\circ A$;
(ii) $\vdash_{\mathbf{Ci}} \neg\bullet\circ A$;
(iii) $\vdash_{\mathbf{Ci}} \circ\bullet A$;
(iv) $\vdash_{\mathbf{Ci}} \neg\bullet\bullet A$.

Proof: Parts (i) and (iii) come directly from FACT 3.32 and from parts (ii) and (iii) of FACT 3.31. For (ii) and (iv), use (bc2) and the previous parts. □

This last result (check also [54]) implies that **Ci** will not have consistency or inconsistency appearing at different levels: both consistent and inconsistent formulas are consistent (in contrast to what happened in the case of **bC** —see THEOREM 3.10—, where no formula was provably consistent), and none of them is inconsistent (check also FACT 3.50 for a much stronger version of the last fact in **Ci**).

The reader will recall from the subsection **3.3** that contraposition inference rules not only did not hold in **bC** but could not even be added to any paraconsistent extension of it (THEOREM 3.20). **Ci** can be shown to count, nevertheless, with more restricted forms of contraposition than **bC** (compare the following with FACT 3.17):

FACT 3.34 These are some restricted forms of contraposition introduced by **Ci**:

(i) $(A \to \circ B) \vdash_{\mathbf{Ci}} (\neg\circ B \to \neg A)$;
(ii) $(A \to \neg\circ B) \vdash_{\mathbf{Ci}} (\circ B \to \neg A)$;
(iii) $(\neg A \to \circ B) \vdash_{\mathbf{Ci}} (\neg\circ B \to A)$;
(iv) $(\neg A \to \neg\circ B) \vdash_{\mathbf{Ci}} (\circ B \to A)$.

Proof: To check (i), let $\Gamma = \Delta = \{(A \to \circ B), \neg\circ B\}$ and apply FACT 3.31(v) to $\Gamma \cup \{A\}$, so as to obtain $\Gamma \vdash_{\mathbf{Ci}} \neg A$. This will give the desired result. Alternatively, one could use directly FACT 3.17(i) and note that $\circ\circ B$ is a theorem of **Ci** (this is FACT 3.33(i)). The other parts are similar, and we leave them as easy exercises to the reader. □

Note that all rules in the last result continue to be valid if one substitutes any '$\circ$' for '$\neg\bullet$', and any '$\neg\circ$' for '$\bullet$'. On the other hand, rules such as $[(\circ A \to B) \vdash_{\mathbf{Ci}} (\neg B \to \neg\circ A)]$ *do not* hold in this logic!

Now, we have learned from COROLLARY 3.22 that the intersubstitutivity of provable equivalents, (IpE), does not hold for **bC**. The same result is true for **Ci**, and still the same counter-examples mentioned before can be presented here:

THEOREM 3.35 (IpE) does not hold for **Ci**.

Proof: Add to the matrices on THEOREM 3.21 one matrix for $\bullet$ such that $v(\bullet A) = 1 - v(\circ A)$, and check that all the new axioms, defining **Ci** from **bC**, still hold. □

Now, in order to go one step further from the actual absence of contraposition in **Ci**, let us recall that in the subsection **3.3** it has been pointed out that the addition of some of the deduction 'contraposition' rules (EC) or (RC) would have been equally sufficient for obtaining (IpE) for consistencyless fragments of our paraconsistent logics. It seems, nevertheless, that obtaining (IpE) will not be an easy task, after all:

FACT 3.36 The addition of (RC): $[(A \vdash B) \Rightarrow (\neg B \vdash \neg A)]$ to **Ci** causes its collapse into classical logic.

Proof: From (ci1) and (ci2), plus (bc3), one obtains, respectively, that $\neg\circ A \vdash_{\mathbf{Ci}} A$, and $\neg\circ A \vdash_{\mathbf{Ci}} \neg A$. Applying (RC) and (Min11) one would have then $\neg A \vdash_{\mathbf{Ci}} \circ A$ and $\neg\neg A \vdash_{\mathbf{Ci}} \circ A$. But then, using the proof by cases, one would conclude that $\vdash_{\mathbf{Ci}} \circ A$, that is, all formulas would be consistent. Looking at THEOREM 3.9 and (bc1), one sees that this was exactly what was lacking in order for classical logic to be characterized. □

So, (RC) must be ruled out as an alternative in order to obtain (IpE), in the case of **Ci**. As for (EC), its possible addition to **Ci** will be discussed below, in THEOREM 3.51, FACT 3.52, and the subsequent commentaries on these results.

The new restricted forms of contraposition in FACT 3.34 are, in any case, strong enough for us to show that **Ci** has some redundant axioms as it is. Indeed:

FACT 3.37 In **Ci**: (i) (bc2) proves (bc3), and vice-versa. (ii) (bc4) proves (bc5), and vice-versa.

Other interesting consequences of (ci) are those that we shall call 'Guillaume's Theses', which regulate the propagation of consistency and the back-propagation of inconsistency through negation:

FACT 3.38 **Ci** also proves the following:

(i) $\circ A \vdash_{\mathbf{Ci}} \circ\neg A$;

(ii) $\bullet\neg A \vdash_{\mathbf{Ci}} \bullet A$.

Proof: From (ci) and (bc4), we have that $[\neg\circ\neg A \vdash_{\mathbf{Ci}} (\neg A\wedge\neg\neg A)]$, from C_{min} we have that $[(\neg A\wedge\neg\neg A) \vdash_{\mathbf{Ci}} (A\wedge\neg A)]$, and from FACT 3.16(ii) we know that $[(A\wedge\neg A) \vdash_{\mathbf{Ci}} \neg\circ A]$. So, ultimately, we have the rule $[\neg\circ\neg A \vdash_{\mathbf{Ci}} \neg\circ A]$. By (bc3) and (bc4) we prove part (ii) of our fact. Part (i) comes from this same rule, by an application of FACT 3.34(iv). □

This last result will provide us with some other forms for the theses in FACT 3.33, such as:

FACT 3.39 These are also some special theses of **Ci**:

(i) $\vdash_{\mathbf{Ci}} \circ\neg\circ A$;

(ii) $\vdash_{\mathbf{Ci}} \neg\bullet\neg\circ A$;

(iii) $\vdash_{\mathbf{Ci}} \circ\neg\bullet A$;

(iv) $\vdash_{\mathbf{Ci}} \neg\bullet\neg\bullet A$.

It will also be useful to note that here we have (contrasting with THEOREM 3.30, which informed us, among other things, that $[A \nvdash_{\mathbf{Ci}} \neg\neg A]$):

FACT 3.40 Here are some more special theses of **Ci**:

(i) $\circ A \vdash_{\mathbf{Ci}} \neg\neg\circ A$;

(ii) $\bullet A \vdash_{\mathbf{Ci}} \neg\neg\bullet A$.

Proof: These will follow directly if you apply FACT 3.34 twice. The reader might remember that we lacked these forms in **bbbC** (this was THEOREM 3.27). □

Now, do we obtain in **Ci** that intended duality between consistency and inconsistency? The answer is YES. This is the topic for our next subsection.

3.6 On a simpler presentation for Ci. The logic **Ci** provides us with a sufficient environment to prove a kind of restricted *intersubstitutivity* or *replacement* theorem. While we know from THEOREM 3.35 that full replacement for the formulas of **Ci** does not obtain, our present restricted forms of contraposition, nevertheless, will help us to show that intersubstitutivity *does* hold if only we are talking only about substituting some formula whose outmost operator is '$\circ$' by this same formula, but now having '$\neg\bullet$' in the place of that '$\circ$', or if we will substitute some formula whose outmost operator is '$\bullet$' by this same formula, but now having '$\neg\circ$' in the place of that '$\bullet$'. In simpler terms, what we are saying is that we can now take just one of the operators '$\circ$' and '$\bullet$' as primitive, and define the other in terms of the negation of that first one. So, we will now show that:

THEOREM 3.41 An equivalent axiomatization for **Ci** is obtained if we consider only axioms (Min1)–(Min11), (bc1), (ci), and (MP), and set one of these two definitions:

(i) $\bullet A \stackrel{\text{def}}{=} \neg\circ A$;

(ii) $\circ A \stackrel{\text{def}}{=} \neg\bullet A$.

Proof: Consider part (i) to be the case. This means that we can take (bc3): $\neg\circ A \vdash_{\mathbf{Ci}} \bullet A$ and (bc4): $\bullet A \vdash_{\mathbf{Ci}} \neg\circ A$, for granted, simply by definition. Now, (bc2): $\neg\bullet A \vdash_{\mathbf{Ci}} \circ A$, will be the case if, and only if, given the definition of '$\bullet$', $\neg\neg\circ A \vdash_{\mathbf{Ci}} \circ A$ is the case —and it is, because of (Min11). As to (bc5): $\circ A \vdash \neg\bullet A$, it will be the case if, and only if, $\circ A \vdash_{\mathbf{Ci}} \neg\neg\circ A$ is the case —and it is, this time thanks to FACT 3.40(i). An alternative, and much simpler way, of checking that (bc2) and (bc5) should hold here is by taking FACT 3.37 into consideration. The axiomatic rule (bc1): $\circ A,\ A,\ \neg A \vdash_{\mathbf{Ci}} B$ is already in the 'standard form' (we are here eliminating all occurrences of '$\bullet$'s and leaving only '$\circ$'s), and the rule (ci): $\bullet A \vdash_{\mathbf{Ci}} (A\wedge\neg A)$ can be exchanged, by the definition of '$\bullet$', that is, by (bc3) and (bc4), for $\neg\circ A \vdash_{\mathbf{Ci}} (A\wedge\neg A)$. Now we have shown that all occurrences of '$\bullet$' in the axioms of **Ci** can be substituted by an occurrence of '$\neg\circ$', and all occurrences of '$\neg\bullet$' in the axioms of **Ci** can be substituted by an occurrence of '$\circ$'. So, if you would have proven a formula in which, respectively, an inconsistency connective '$\bullet$' or its negated form '$\neg\bullet$' appears at some point, you can now rewrite the proof using the new versions of the axioms above and what will appear in the end will be, respectively, a negated consistency connective '$\neg\circ$', or simply the connective '$\circ$'. For part (ii) the procedure is entirely analogous, but now use FACT 3.40(ii), or FACT 3.37 again, when necessary. □

So, this last result provides us with a restricted form of replacement theorem for consistent formulas, and guarantees the intended duality between $\circ$ and $\bullet$, which could not be obtained in the subsection **3.4**, within **bbbbC** or its fragments. With such a result in hand we need make no big effort to verify that formulas such as $\neg(\circ A\wedge\neg\circ A)$, $\neg(\circ A\wedge\bullet A)$, $\neg(\neg\bullet A\wedge\neg\circ A)$ and $\neg(\neg\bullet A\wedge\bullet A)$ are all equivalent, which could, otherwise, be quite a non-trivial task!

The reason why we can obtain this new axiomatization, as the reader will make out after he is introduced to the semantics of **Ci**, in [42], is that, truth-functionally based on the non-classical behavior of negation, both the consistency and the inconsistency operators of this logic will work quite 'classically'.

3.7 Using LFIs to talk about classical logic. At this point, working with **Ci**, perhaps the question would arise as to how far we are from classical propositional logic, **CPL**. The answer is: a lot —and just a little bit. As we are not presupposing any kind of doublethinking, let us then reformulate a few things for the question, and its answer, really to make sense.

To start with, it is hard to compare two logics if they 'talk about different things', and are so disjoint that none of them is an extension of the other. For C_{min} was a conservative extension of positive classical logic, but it was a fragment, in the same language, of 'full' **CPL**, as we know, and **bC** was a conservative extension of C_{min}. Thus, **Ci**, which is a deductive extension of **bC**, happens to be written in a richer language than that of **CPL**, but it does not contain all classical inferences, and so these two logics are hardly comparable. Now, this is easy to fix. Let us also conservatively extend **CPL** by the addition of connectives for consistency and inconsistency, whose matrices will be such that $\circ$ takes always the distinguished value 1, and $\bullet$, on the contrary, is constant and equal to 0. We will designate this 'new' logic, obtained by such an extension of **CPL**, *extended classical logic*, or **eCPL**. Of course, **eCPL** can be easily axiomatized by the addition of an axiomatic schema such as:

(ext) $\vdash_{\mathbf{eCPL}} \circ A$ 'Every A is consistent'

to any axiomatization of **CPL**, like the one mentioned in THEOREM 3.9. The inconsistency connective, $\bullet$, can be here introduced as a definition: $\bullet A \stackrel{\text{def}}{=} \neg\circ A$, just as in THEOREM 3.41(i). In this way we obtain an extension of classical logic which looks as a logic of formal inconsistency (see (D20)), having an operator expressing consistency, and of course an axiomatic rule such as $[\circ A, A, \neg A \vdash_{\mathbf{eCPL}} B]$, expressing finite gentle explosion, will hold in **eCPL**. But, as it happens, given axiom (ext), we know that **eCPL** is not only finitely gently explosive (and so, non-trivial), but explosive as well. It is, in fact, a *consistent* logic (see (D19)), instead of an **LFI**.

Well and good, but is **Ci** now to be characterized as a deductive fragment of **eCPL**? Indeed! Just check that all axioms of **Ci** are validated by the matrices of **eCPL**, and that's it. So, **Ci** is in fact a fragment of an alternative formulation of classical logic, and this of course will guarantee that **Ci** is a non-contradictory logic (once **Ci** is not explosive, but it is a fragment of **eCPL**, and **eCPL** is still at least as explosive and non-trivial as **CPL** was, and consequently it cannot prove a contradiction). Is that all to it? No, because we will now see that we can still use **Ci** to reproduce in a very faithful way every inference of **CPL** (or of **eCPL**)!

How can this be done? Remember that **Ci** has a strong (or supplementing) negation, which can be defined, as in the case of **bC**, by setting $\sim A \stackrel{\text{def}}{=} (\neg A \wedge \circ A)$. But, in **bC**, even though this negation had the power of producing (supplementing) explosions, it could not still be said to have all properties of a *classical negation*. Indeed:

THEOREM 3.42 The strong negation $\sim$, in **b**(**b**(**b**(**b**)))**C**, is not classical.
Proof: Just consider once more the classical matrices for the classical connectives, as in the above definition of **eCPL**, but now exchange the matrices of $\circ$ and $\bullet$, letting $\circ$ be constant and equal to 0 (and not to 1, as before), and letting $\bullet$ be constant and equal to 1 (and not to 0, as before). It is easy to see that all axioms and rules of

b(**b**(**b**(**b**)))**C** are validated by such matrices, but (ci) and (ext) are not, and consequently formulas such as $(A\vee{\sim}A)$ and $(A\to{\sim}{\sim}A)$ (recall the definition of ${\sim}A$) are not validated as well, being independent from all logics we have exposed previous to **Ci**. □

This is an interesting result that shows that being explosive is not enough to make a negation classical.[15] But what would be enough? Well, given the axiomatization of classical logic in THEOREM 3.9, we know that any connective $\div$ added in an axiomatic environment where (Min1)–(Min9) hold and which is such that:

(Alt10) $\vdash_{\mathbf{Alt}} (A\vee\div A)$;
(Alt11) $\vdash_{\mathbf{Alt}} (\div\div A\to A)$;
(Alt12) $\vdash_{\mathbf{Alt}} (A\to(\div A\to B))$,

also hold, should behave as the classical negation. So, all we have to do now is to show that (Alt10)–(Alt12) hold in **Ci** if one substitutes $\div$ for the strong negation ${\sim}$. We could here make use of an auxiliary lemma:

LEMMA 3.43 These are some theorems of **Ci**:
(i) $\vdash_{\mathbf{Ci}} (A\vee\circ A)$;
(ii) $\vdash_{\mathbf{Ci}} (\neg A\vee\circ A)$.
Proof: For part (i), observe that, from (Min6), $[\circ A\vdash_{\mathbf{Ci}} (A\vee\circ A)]$, and, from (ci1), $[\neg\circ A\vdash_{\mathbf{Ci}} A]$, so, once more by (Min6), and transitivity, $[\neg\circ A\vdash_{\mathbf{Ci}} (A\vee\circ A)]$. Using the proof by cases one finally concludes that $[\vdash_{\mathbf{Ci}} (A\vee\circ A)]$. Part (ii) is similar to (i), but you should now use (ci2). □

THEOREM 3.44 The strong negation ${\sim}$, in **Ci**, is classical.
Proof: To check that (Alt10) holds for ${\sim}$, that is, that $[\vdash_{\mathbf{Ci}} (A\vee(\neg A\wedge\circ A))]$, notice that this last schema is equivalent to $[\vdash_{\mathbf{Ci}} (A\vee\neg A)\wedge(A\vee\circ A))]$, by positive classical logic, and the latter is provable from (Min10) and LEMMA 3.43(i), using (Min3). Now, (Alt12) is immediate, by the very definition of ${\sim}$, and to check (Alt11) you might just notice that by reflexivity we have $[{\sim}{\sim}A,\ A\vdash_{\mathbf{Ci}} A]$, and from (Alt12) we have $[{\sim}{\sim}A,\ {\sim}A\vdash_{\mathbf{Ci}} A]$; so, using a new form of proof by cases obtained from (Alt10) and (Min8) (as in FACT 3.7), we conclude that $[{\sim}{\sim}A\vdash_{\mathbf{Ci}} A]$. □

So, **Ci** is strong enough to endow its strong negation with all properties of a classical negation. This result has some immediate consequences. For instance, we could use it to show that (Min9) is redundant in **Ci** (and all other logics extending it). Notice, of course, that the two last results did not really need to use the whole positive *classical* logic, but that its *intuitionistic* fragment (which does not contain (Min9)) would have been enough. Confront the following fact with THEOREM 3.3:

FACT 3.45 The schema (Min9): $(A\vee(A\to B))$ is redundant in the axiomatization of **Ci**.
Proof: From reflexivity and (Alt12) we have that $[A\vdash_{\mathbf{Ci}} A]$ and $[{\sim}A\vdash_{\mathbf{Ci}} (A\to B)]$. But, of course, either A or $(A\to B)$, by (Min6) and (Min7), imply the above schema, $(A\vee(A\to B))$. So, using (Alt10) once more to provide a proof by cases, we are done. □

[15] There seems to be, at any rate, a widespread mistaken assumption in the literature to that effect (despite the example of intuitionistic negation, strong but not classical). Yet in some other studies, as for instance Batens's [13], note 11, a 'classical' negation, $\div$, in a paraconsistent logic is assumed to be one which is not only strong but it should also be the case that $[\div A\vdash\neg A]$ holds (as in axiom (bun), in the subsection **3.8**). This *is* the case, however, for **bC**'s strong negation ${\sim}$, but now we know that it is still *not* classical (it just has some kind of intuitionistic behavior). Ten years before, nevertheless, this same author (see [10], page 224) had put things more precisely, and required for that definition that $[\vdash (A\vee\div A)]$ should also be the case.

We shall not list the properties of ~ in **Ci** at this point, but only mention the fact that 'it is a classical negation' when necessary, and then use any property that derives from this fact.

Now, this strong (classical) negation will give us a very interesting result. We already knew that the other binary connectives worked as their classical counterparts, and we were informed above that **Ci** comes also equipped with a negation which works like the classical one; so why don't we use **Ci** to 'talk about classical logic', that is, use **Ci**'s own stuff to reproduce any classical inference? One intuitive procedure to bring forth such an effect would be to pick any classical inference and just substitute any occurrence of a classical negation by an occurrence of a strong negation, and leave the rest as it is. And this indeed works:

THEOREM 3.46 The following mapping conservatively translates **CPL** inside of **Ci**:

(t1.1) $t_1(p)=p$, if p is an atomic formula;

(t1.2) $t_1(A\#B)=t_1(A)\#t_1(B)$, if # is any binary connective;

(t1.3) $t_1(\neg A)={\sim}t_1(A)$.

So, it is the case that $[\Gamma \vdash_{\mathbf{CPL}} A] \Leftrightarrow [t_1[\Gamma] \vdash_{\mathbf{Ci}} t_1(A)]$.

Proof: Given THEOREM 3.44, we know that, by way of the above transformation, a counterpart to **CPL**'s axiomatization can be obtained inside of **Ci**. □

COROLLARY 3.47 We also have a conservative translation of **eCPL** inside of **Ci**.

Just extend the above mapping by adding:

(t1.4) $t_1(\circ A)=\circ\circ t_1(A)$.

Proof: This comes from the above theorem, **eCPL**'s axiom (ext) and FACT 3.33(i). □

The above recursive translation just substitutes one negation for another, thus giving rise to a *grammatically faithful* (cf. [61], chapter X) way of reproducing classical inferences inside of **Ci**, and inside of any other logic deductively stronger than it, as the ones we will be studying below. Of course, other logics may provide yet some other sensible ways of translating classical logic inside of them (see, for instance, COROLLARY 3.62).

To be sure, we already had, in **bC**, a way of reproducing classical inferences (recall THEOREM 3.11), but at that point we had to introduce further premises in our theories —to wit, the premises that some of our propositions were consistent). A natural question which may arise then is whether this was really necessary, given that from THEOREM 3.42 we know that the 'canonical' strong negation of **bC** was not a classical one, and would then not allow the above translations to be performed inside of **bC**, or could it perhaps be the case that all strong negations are indeed strong, but some are stronger than others? This last option is indeed what occurs, for it can be easily shown, if we just recall FACT 2.10(ii), how one can define a classical negation inside of **bC**, despite the weakness of this logic, thus being able to talk about classical logic already inside of the most basic **C**-system we here present:

THEOREM 3.48 The logic **bC** does have a classical negation.

Proof: From (bc1), the axiom that realizes finite gentle explosiveness, and from left-adjunctiveness, we know that $(A\wedge(\neg A\wedge\circ A))$ is a bottom particle, for any formula A —let's choose any of these conjunctions and denote it by $\bot$, as usual. Inspired by FACT 2.10(ii) and using the Deduction Metatheorem we then define a new strong negation, $\dot{\sim}$, on **bC** as $\dot{\sim}A \stackrel{\text{def}}{=} (A\to\bot)$.[16] To check that *this* negation is

[16] This was indeed one of the many 'negations' set forth by Bunder in [30], though this author seems not to have completely understood their properties (see below the subsection **3.8**).

classical, we just need to prove that $(\div\div A \to A)$ is a theorem of **bC**. To such an end, first note that $[\vdash_{\mathbf{Ci}} (A \vee \div A)]$, given that this is $[\vdash_{\mathbf{Ci}} (A \vee (A \to \bot))]$, a form of axiom (Min9), and this gives us a new form of proof by cases, as in THEOREM 3.44. Next, notice that $[((A \to \bot) \to \bot), (A \to \bot) \vdash_{\mathbf{Ci}} \bot]$, by *modus ponens*, and $[\bot \vdash_{\mathbf{Ci}} A]$ by definition of the bottom particle, but also $[((A \to \bot) \to \bot), A \vdash_{\mathbf{Ci}} A]$. Thus, the new form of proof by cases will immediately give us $[((A \to \bot) \to \bot) \vdash_{\mathbf{Ci}} A]$. $\Box$

It is easy then to transform THEOREM 3.46 into a grammatically faithful translation of **CPL** already inside of **bC**, but it would be less easy to find a non-trivial analogue of COROLLARY 3.47, the translation of **eCPL**, given that **bC** is already known to have no consistent theorems (recall THEOREM 3.10) —that is, no theorems of the form $\circ A$. As to the status of the two different strong negations presented above inside of the stronger logic **Ci**, one can easily go on to show that:

FACT 3.49 In **Ci** the two strong negations above, $\sim$ and $\div$, are both classical, and are in fact equivalent, in a sense (but not all strong negations are classical in **Ci**).

Proof: That they are both classical is an obvious consequence from THEOREM 3.44 and THEOREM 3.48. To see that they are equivalent in **Ci**, remember, on the one hand, that $[A, {\sim}A \vdash_{\mathbf{Ci}} \bot]$, by definition, and so $[{\sim}A \vdash_{\mathbf{Ci}} (A \to \bot)]$, that is, $[{\sim}A \vdash_{\mathbf{Ci}} \div A]$, by the Deduction Metatheorem. On the other hand, we have both that $[(A \to \bot), A \vdash \bot]$, and thus $[(A \to \bot), A \vdash_{\mathbf{Ci}} {\sim}A]$, and that $[(A \to \bot), {\sim}A \vdash_{\mathbf{Ci}} {\sim}A]$, so the form of proof by cases offered by THEOREM 3.44 will allow us to conclude that $[\div A \vdash_{\mathbf{Ci}} {\sim}A]$. The reader will be right in thinking that all classical negations extending a positive classical basis are equivalent, but it is still the case, nonetheless, that **Ci** can define other strong negations that do not have a classical character, as for instance $(\neg\neg{\sim}A)$ or $(\neg\neg\div A)$. Take a look at [42], our paper on semantics, in the section on **Ci**, to check this claim. $\Box$

Now, the reader may perhaps think that classically negated propositions in **bC** and **Ci** (especially given the reconstruction of classical inferences inside of these logics that such negations support by way of the above mentioned conservative translations) would be classical enough so as to be consistent propositions themselves, that is, that $\circ\div A$ would be a theorem, for instance, of **Ci**, for some classical negation $\div$. We will now show that this can hardly be the case:

FACT 3.50 Only consistent or inconsistent formulas can themselves be provably consistent in **Ci**. Thus, $(\circ A)$ is a theorem of **Ci** if, and only if, A is of the form $\circ B$, $\bullet B$, $\neg\circ B$ or $\neg\bullet B$, for some B.

Proof: On the one hand, we already know from FACT 3.33 and FACT 3.39 that formulas such as $\circ B$ or $\bullet B$, and their variations, are all provably consistent in **Ci**. To see that the converse is also true, consider the following three-valued matrices, such that 0 is the only non-distinguished value and $v(A \wedge B) = ½$ if $v(A) \neq 0$ and $v(B) \neq 0$, and $v(A \wedge B) = 0$, otherwise; $v(A \vee B) = ½$ if $v(A) \neq 0$ or $v(B) \neq 0$, and $v(A \vee B) = 0$, otherwise; $v(A \to B) = ½$ if $v(A) = 0$ or $v(B) \neq 0$, and $v(A \to B) = 0$, otherwise; $v(\neg A) = 1 - v(A)$; $v(\circ A) = 1$ if $v(A) \neq ½$, and $v(\circ A) = 0$, otherwise. $\Box$

As a consequence of the last result, in particular, formulas of the form $\circ{\sim}A$ and $\circ\div A$ will not be provable in **Ci**, and, from FACT 3.32, we conclude that **Ci** is *not* controllably explosive in contact with (at least some) classically negated propositions. As we shall see, on the other hand, there are many extensions of this logic that do have this property, at least for some particular A's (see FACT 3.66, or FACT 3.76). But what would have happened if we had indeed theorems such as tho ones ruled out above? Let us here allow ourselves some counterfactual reasoning, and ask ourselves about the possible validity of (IpE) in some specific paraconsistent logics, like the extensions of **Ci**, or of some of its fragments (given that we know, from THEOREM 3.35, that (IpE) still does not hold in **Ci**, anyway):

THEOREM 3.51 (IpE) cannot hold in any paraconsistent extension of **Ci** in which:
(i) $(\circ{\div}{\div}A)$ holds, for some given classical negation $\div$; *or*
(ii) $\neg(A\wedge\neg A)$ or $\neg(\neg A\wedge A)$ hold; *or*
(iii) $[(\neg A\vee\neg B) \vdash \neg(A\wedge B)]$ hold; *or*
(iv) $[\neg(A\wedge B) \vdash (\neg A\vee\neg B)]$ hold.
(IpE) cannot hold in any paraconsistent extension of **bC** in which:
(v) $[\neg(A\rightarrow B) \vdash (A\wedge\neg B)]$ hold.
(IpE) cannot hold in any adjunctive paraconsistent extension of C_{min} in which:
(vi) both $[(A\wedge B) \vdash \neg(\neg A\vee\neg B)]$ and $[\neg(\neg A\vee\neg B) \vdash (A\wedge B)]$ hold.
(IpE) cannot hold in any adjunctive paraconsistent logic in which:
(vii) both $\neg(A\wedge\neg A)$ and $[(A\wedge\neg A) \dashv\vdash \neg\neg(A\wedge\neg A)]$ hold.

Proof: For part (i), given that $\div$ is a classical negation we can then assume $[A \dashv\vdash {\div}{\div}A]$ to hold. Now, if (IpE) were valid one could conclude, in particular, that $[\circ A \dashv\vdash \circ{\div}{\div}A]$, and given that $(\circ{\div}{\div}A)$ is a theorem of this logic extending **Ci**, by hypothesis, one would infer $\circ A$ as a theorem, but this is (ext), exactly the axiom that is lacking to make **Ci** collapse into **eCPL**. This generalizes a similar argument to be found in [107], Theorem 9. To check part (ii), recall that, in **Ci**, $[\bullet A \dashv\vdash (A\wedge\neg A)]$, and (IpE) would then give $[\circ A \dashv\vdash \neg(A\wedge\neg A)]$, and we are again left with the theorem $\circ A$, as in part (i). For part (iii), recall that $(\neg A\vee\neg\neg A)$ is a theorem already of C_{min}, and the problem reduces then to part (ii). For parts (iv) and (v), we will just show that $[(\neg{\div}A) \vdash A]$ is obtained, and so we may conclude that controllable explosion occurs in contact with ${\div}A$. Given that $[\neg(A\wedge B) \vdash (\neg A\vee\neg B)]$ holds, consider the strong negation ${\sim}A \stackrel{\text{def}}{=} (\neg A\wedge\circ A)$, for which one would immediately obtain $[\neg{\sim}A \vdash (\neg\neg A\vee\neg\circ A)]$, and so, from (Min11), (ci1) and (Min8), we get $[\neg{\sim}A \vdash A]$. Given that $[\neg(A\rightarrow B) \vdash (A\wedge\neg B)]$ holds, pick up $\dot{\div}A \stackrel{\text{def}}{=} (A\rightarrow\bot)$, and, from (Min4), we have that $[\neg\dot{\div}A \vdash A]$. For part (vi), given once more that $(\neg A\vee\neg\neg A)$ is a theorem of C_{min}, (IpE) would give us $[\neg(\neg A\vee\neg\neg A) \dashv\vdash \neg(\neg B\vee\neg\neg B)]$, and the rules that we here assume give us $[(A\wedge\neg A) \dashv\vdash (B\wedge\neg B)]$, so, by adjunction, we conclude in particular that $[A, \neg A \vdash B]$. This is the main result in Béziau's [21]. Finally, for part (vii), (IpE) would give us $[\neg\neg(A\wedge\neg A) \dashv\vdash \neg\neg(B\wedge\neg B)]$, and so $[(A\wedge\neg A) \dashv\vdash (B\wedge\neg B)]$, and we are in the same situation as in (vi). This is a stronger version of the main result in Béziau's [22], where actually $[A \dashv\vdash \neg\neg A]$ was assumed, instead of $[(A\wedge\neg A) \dashv\vdash \neg\neg(A\wedge\neg A)]$. Of course, a similar version of this last result arises if one just uniformly substitutes $(A\wedge\neg A)$ for $(\neg A\wedge A)$ in its statement. Notice that the rules mentioned in parts (iii) to (vi) had already shown up as the items (x), (ix), (vii), (xii) and (xi) of THEOREM 3.18.□

So far we have some negative results about the validity of (IpE) in some possible paraconsistent extensions of **bC** or **Ci**, but are there paraconsistent extensions of these logics in which (IpE) *does* hold? In the search for an answer, one could start by testing the compatibility of the addition, to those logics, of at least one of the following rules of deduction, (RC): $[(A \vdash B) \Rightarrow (\neg B \vdash \neg A)]$ or (EC): $[(A \dashv\vdash B) \Rightarrow (\neg B \vdash \neg A)]$

(see the subsection **3.3**, where these were argued to be enough for the consistencyless fragment of our language), and also of at least one of the following:

$$\forall A\,\forall B\;[(A \vdash B) \Rightarrow (\circ A \vdash \circ B)]; \tag{RO}$$

$$\forall A\,\forall B\;[(A \dashv\vdash B) \Rightarrow (\circ A \vdash \circ B)]. \tag{EO}$$

We have already shown, in FACT 3.36, that (RC) cannot be added to **Ci** without collapsing into classical logic. We can now actually show more:

FACT 3.52 In extensions of **Ci**, the validity of (EC) also guarantees (EO).
Proof: From $[A \dashv\vdash B]$ we conclude, by (EC), that $[\neg A \dashv\vdash \neg B]$. From these two sentences, by positive logic, we conclude that $[(A\wedge\neg A) \dashv\vdash (B\wedge\neg B)]$, but from FACT 3.16(ii) and FACT 3.29(i) we know that $[\neg\circ C \dashv\vdash (C\wedge\neg C)]$, and so we have that $[\neg\circ A \dashv\vdash \neg\circ B]$. Finally, from FACT 3.34(iv), we have that $[\circ A \dashv\vdash \circ B]$. □

The problem of finding paraconsistent extensions of **Ci** in which (IpE) holds reduces then to the problem of finding out if (EC) can be added to this logic without losing the paraconsistent character. We suspect this can be done, but shall leave it as an open problem at this point. As to extensions of **bC**, on the other hand, we can already present a (very partial) result:

THEOREM 3.53 There are fragments of **eCPL** extending **bC** in which (IpE) holds.
Proof: We already know, from COROLLARY 3.22, that (IpE) does not hold for **bC** as it is. It suffices now to show that the addition of the rules (EC) and (EO) to **bC** may still originate a paraconsistent fragment of (extended) classical logic, once these rules are evidently enough to ensure (IpE). To such an end, one may simply make use of the following matrices by Urbas ([107], Theorem 8):

$\wedge$	**1**	**6/7**	**5/7**	**4/7**	**3/7**	**2/7**	**1/7**	**0**
1	1	6/7	5/7	4/7	3/7	2/7	1/7	0
6/7	6/7	6/7	3/7	2/7	3/7	2/7	0	0
5/7	5/7	3/7	5/7	1/7	3/7	0	1/7	0
4/7	4/7	2/7	1/7	4/7	0	2/7	1/7	0
3/7	3/7	3/7	3/7	0	3/7	0	0	0
2/7	2/7	2/7	0	2/7	0	2/7	0	0
1/7	1/7	0	1/7	1/7	0	0	1/7	0
0	0	0	0	0	0	0	0	0

$\vee$	**1**	**6/7**	**5/7**	**4/7**	**3/7**	**2/7**	**1/7**	**0**
1	1	1	1	1	1	1	1	1
6/7	1	6/7	1	1	6/7	6/7	1	6/7
5/7	1	1	5/7	1	5/7	1	5/7	5/7
4/7	1	1	1	4/7	1	4/7	4/7	4/7
3/7	1	6/7	5/7	1	3/7	6/7	5/7	3/7
2/7	1	6/7	1	4/7	6/7	2/7	4/7	2/7
1/7	1	1	5/7	4/7	5/7	4/7	1/7	1/7
0	1	6/7	5/7	4/7	3/7	2/7	1/7	0

$\rightarrow$	**1**	**6/7**	**5/7**	**4/7**	**3/7**	**2/7**	**1/7**	**0**
1	1	6/7	5/7	4/7	3/7	2/7	1/7	0
6/7	1	1	5/7	4/7	5/7	4/7	1/7	1/7
5/7	1	6/7	1	4/7	6/7	2/7	4/7	2/7
4/7	1	6/7	5/7	1	3/7	6/7	5/7	3/7
3/7	1	1	1	4/7	1	4/7	4/7	4/7
2/7	1	1	5/7	1	5/7	1	5/7	5/7
1/7	1	6/7	1	1	6/7	6/7	1	6/7
0	1	1	1	1	1	1	1	1

	$\neg$
1	0
6/7	5/7
5/7	2/7
4/7	3/7
3/7	4/7
2/7	5/7
1/7	1
0	1

where 1 is the only distinguished value, and add to these a matrix for $\circ$ that is constant and equal to 0. It is straightforward to check that the above matrices validate all axioms and rules of **bC** plus the two rules above, while formulas such as $(A\to(\neg A\to B))$ and $\neg(A\wedge\neg A)$ are still not validated by them.[17] □

3.8 Beyond Ci: The dC-systems. We have now come closer to the more orthodox approach to paraconsistent logics that the reader will find in the field, which *does* identify contradictoriness and inconsistency. All logics that we will be studying from here on, and, we argue, all logics of formal inconsistency presented in the literature so far, do not distinguish between these two notions. But this does *not* mean, the reader should be aware, that one can simply dispense with the new operators that have allowed us, so far, to talk about a formula being consistent or inconsistent, for, we remember from FACT 3.16, even though $[\bullet A \dashv\vdash_{\mathbf{Ci}} (A\wedge\neg A)]$ holds, $[\circ A \dashv\vdash_{\mathbf{Ci}} \neg(A\wedge\neg A)]$, for instance, does not hold (in fact, $[\nvdash_{\mathbf{Ci}} \neg(A\wedge\neg A)]$)! Suppose then that we construct now the logic **Cil** exactly by adding to **Ci** (that is, (Min1)–(Min11), (bc1), (ci), (MP), plus the definition of $\circ$ in terms of $\bullet$) the following 'missing' axiomatic rule:

(cl) $\neg(A\wedge\neg A) \vdash \circ A$. 'If $\neg(A\wedge\neg A)$ is the case, then A is consistent'

Much confusion has been raised around this particular formula, $\neg(A\wedge\neg A)$. Recall, from THEOREM 3.15(ii) and THEOREM 3.30, that it was not a theorem of our previous logics, **bC** or **Ci**. Now, of course, we can immediately conclude even more:

FACT 3.54 No paraconsistent extension of **Cil** can have $\neg(A\wedge\neg A)$ as a theorem.
Proof: If so, axiom (cl) would give us consistency, thus ruining paraconsistency. □

Nonetheless, some other paraconsistent extensions of **Ci**, such as **LFI1** (see its matrices in the subsection **2.4** or at FACT 3.29, and see its axiomatization in THEOREM 3.69), *do* have this as theorem (and, as a consequence of THEOREM 3.51(ii), they must lack a full replacement theorem).

The attribution of a privileged status to the formula $\neg(A\wedge\neg A)$, using it to express consistency inside some paraconsistent logics, stems from the early requisites put forward by da Costa on the construction of his famed calculi C_n:

- **dC[i]** in these calculi the principle of non-contradiction [sic], in the form $\neg(A\wedge\neg A)$, should not be a valid schema;
- **dC[ii]** from two contradictory formulas, A and $\neg A$, it would not in general be possible to deduce an arbitrary formula B;
- **dC[iii]** it should be simple to extend these calculi to corresponding predicate calculi (with or without equality);
- **dC[iv]** they should contain the most part of the schemas and rules of the classical propositional calculus which do not interfere with the first conditions.

While the requisite **dC[ii]** is nothing but the very definition of a paraconsistent logic (recall the subsection **2.2**), **dC[iii]** is simply a claim for extensions of these logics to higher-order calculi (that we will *not* explore here, reiterating instead the popular and still powerful argument of paraconsistentists about the fact that most, if not all, innovations of paraconsistent logic can already be met at the propositional level), and **dC[iv]** is indeed somewhat vague, having received much attention and many di-

[17] Notice, nevertheless, that all that is proved here is that there certainly exist fragments of classical logic extending **bC** for which (IpE) holds. But the matrices above do not fulfill, of course, our requisite for defining a paraconsistent logic (namely, disrespecting (PPS)), so that the question is still left open as to whether there are *paraconsistent* such extensions of **bC**! (With thanks to Dirk Batens for calling our attention to that.)

verse interpretations from several researchers (our own proposal on its interpretation will be found in the subsection **3.11**), the requisite **dC[i]** is in fact the one to blame for the confusion we were talking about. First of all, under our present perspective, to call the formula $\neg(A\wedge\neg A)$ 'principle of non-contradiction' is quite misleading, not only because this would bring us, as a side effect, to commit to very particular interpretations for the negation of a proposition, the conjunction of contradictory propositions, and the negation of this conjunction, but ascribe to us as well a very particular interpretation for the consistency connective (however, if —and only if— you are working in the context of some specific consistent logics, such as classical logic itself, or intuitionistic logic, we admit that this designation can indeed make sense). Evidently, FACT 3.54 is then just a consequence of such a contract.

Now, if, on the one hand, some authors have questioned the validity of $\neg(A\wedge\neg A)$ in the context of a paraconsistent logic,[18] on the other hand the construction of paraconsistent logics in which this formula does not hold has also been rather criticized since then, and for various reasons. Some of these criticisms unfold from or link to, by and large, still that same understandable and widespread confusion between the 'principle of non-contradiction' and the aims of paraconsistent logic, namely to avoid the 'principle of explosion' instead (see **dC[ii]**, and our subsections **2.1** and **2.2**) —but these more or less loose arguments can hardly be recast under our present formal definitions of those principles. A slightly more elaborate argumentation appears in Routley & Meyer's [100], where the authors are looking for some formalization of dialectical logic, and they claim to that effect not only that $\neg(A\wedge\neg A)$ is 'usually' a theorem of the 'entailment systems' that they have examined, but also that this does not conflict with other logical truths of those dialectical systems (in their words, this does not generate any 'intolerable tensions which destroy any prospect of a coherent logic'), even though these systems do have contradictory theorems (that are to be understood as 'synthetic a priori'), and validate adjunction. Moreover, they maintain that 'the orthodox Soviet position appears to retain $\neg(A\wedge\neg A)$ as a thesis', and they want to deal with it.[19] Now, none of the logics we study here are dialectical, in the sense of disrespecting the Principle of Non-Contradiction and actually proving contradictory formulas (recall the subsection **2.2**), and so the last critique above, in any case, simply falls idle.

[18] For instance, Béziau's [23], section 2.3, argues that, from a *philosophical* standpoint, it is hard to reconcile the validity of $\neg(A\wedge\neg A)$ and an intuitive interpretation for the negation symbol of a paraconsistent logic; as to the *technical* aspect of his criticism, it seems to consists basically of a consequence of THEOREM 3.51(vii) and the wish to obtain both adjunctiveness and the validity of (IpE).

[19] They also say some other things which seem a bit weird. First, that the 'non-orthodox' systems not containing $\neg(A\wedge\neg A)$ as a theorem are all *weaker* dialectical logics (as if the logics were all linearly ordered by strength!). Secondly, they insist on calling the formula $\neg(A\wedge\neg A)$ 'Aristotle's principle of non-contradiction', and after formally presenting their dialectical logics they argue that this formula is 'correct, both in syntactical and semantical formulations': *syntactically* correct because 'it is a theorem, hence valid, hence true' —despite the seemingly naive *petitio principii* brought therein; and *semantically* correct because '[one of its historical formulations] asserts that no statement is both true and false', and this feature, in the case of their logics, is supposed to be 'guaranteed by the bivalent features of the semantics' —now this is surely a mistake, for what guarantees this fact can only be the functional (rather than relational) character of their proposed interpretation, but in any case this last argument by these authors, even if they were kind enough to clear up the somewhat obscure relation of it with the first one, should hardly be accepted as a justification, given that any associated semantics provided to a consequence relation of a given logic is barely *circumstantial*, in a sense, and can often be recast in many apparently non-equivalent ways (if you're not happy with a particular semantics, you can always *look for another one*). A similar criticism of these points has been made before in Batens's [10], section 9.

But let us first explore some consequences of the new axiom (cl), before really questioning it any deeper, or looking for substitutes. The main and most far-reaching consequence is the following:

THEOREM 3.55 In **Cil** we can define the inconsistency operator as $\bullet A \stackrel{\text{def}}{=} (A \wedge \neg A)$ (from which the consistency operator will be defined as $\circ A \stackrel{\text{def}}{=} \neg(A \wedge \neg A)$).
Proof: It can immediately be seen, from the above definitions, that the axioms (bc1), (ci) and (cl) will still hold if one just substitutes all occurrences of the operators $\bullet$ and $\circ$ by their new definitions. □

COROLLARY 3.56 Given a theorem B of **Cil** we can substitute all occurrences of $\bullet$ and of $\circ$ in its subformulas according to the above definitions.
Proof: Recall from THEOREM 3.41 that all axioms of **Ci** can be written just with the use of $\bullet$, substituting $\circ$ for $\neg\bullet$, or just with the use of $\circ$, substituting $\bullet$ for $\neg\circ$. In the first case, where we have only '$\bullet$'s, the above theorem permits us to rewrite in **Cil** the proof of B using $(A \wedge \neg A)$ in the place of each formula $\bullet A$ that appears, and using $\neg(A \wedge \neg A)$ in the place of each formula $\neg\bullet A$ that occurs in the proof. In the second case, and for the same reason, we may rewrite the proof of B using $\neg(A \wedge \neg A)$ in the place of each formula $\circ A$ that appears, and using $(A \wedge \neg A)$ in the place of each formula $\neg\circ A$ that occurs in the proof (you may in this last part also wish to take FACT 3.40 and (Min11) once more into consideration). □

The above results are structurally similar to those of THEOREM 3.41 and its consequences, where a restricted form of replacement was obtained for the operators $\bullet$ and $\circ$, and we have seen that each one of them could be substituted in **Ci** by the negation of the other. But now we know more, we know that we can simply dispense with the operators $\bullet$ and $\circ$, substituting each formula $\bullet A$ and each formula $\neg\circ A$ for the formula $(A \wedge \neg A)$, each formula $\circ A$ and each formula $\neg\bullet A$ for the formula $\neg(A \wedge \neg A)$. This brings us to the definition of a particular subclass of the **C**-systems that we will call **dC**-*systems,* such as the **C**-systems in which $\bullet$ and $\circ$ can be defined in terms of the other connectives. **Cil** is the first example of a **dC**-system that we here consider; before presenting other examples let us point out some consequences of this last result for **Cil**. It is easy to see, for instance, that the restricted forms of contraposition presented in FACT 3.17 for **bC** and in FACT 3.34 for **Ci**, as well as the forms of *reductio* presented in FACT 3.14 for **bC** and in FACT 3.31 for **Ci**, and the forms of controllable explosion presented in this last fact, together with the fundamental fact relating controllable explosion and consistency in FACT 3.32, all have new versions in **Cil**, if we just change each ocurrence of $\circ$ and $\bullet$ for their definitions in THEOREM 3.55. We can also update THEOREM 3.11 with yet another way of reproducing classical inferences inside of **Cil** by the addition of the appropriate premises to its theories (namely the addition of a finite number of formulas of the form $\neg(A \wedge \neg A)$, the formula that in the present circumstances represents the consistency of A). Analogously to what we did in FACT 3.40, we can now prove that $[(A \wedge \neg A) \vdash_{\mathbf{Cil}} \neg\neg(A \wedge \neg A)]$, even though $[A \vdash_{\mathbf{Cil}} \neg\neg A]$ still does not hold.

Now, if FACT 3.50 has provided us with a very precise characterization of the consistent theorems in **Ci**, which turned out to be only consistent or inconsistent formulas themselves, namely the ones appearing in FACT 3.33 and FACT 3.39, we may now use those same results to conclude that formulas such as $\circ(A \wedge \neg A)$, and, by

FACT 3.38(i), also $\circ\neg(A\wedge\neg A)$, are theorems of **Cil**. These last theorems have raised yet some other protests in the literature. For instance, Sylvan (cf. [105]) claims that the fact that such a logic validates some 'unjustifiable' intuitionistically invalid theorems, together with the validity of $\circ(A\wedge\neg A)$ 'defeats certain paraconsistent objectives' (too bad that he did not proceed to clear up which objectives were these…). This echoes, in one way or another, to a common, and entirely well-founded, criticism, which has been raised by various authors, both to the fact that **C**-systems such as those we have been studying do maintain the whole of positive classical logic and to the fact that many of them (but not all!) are in fact **dC**-systems, and come up with rather particular definitions for the consistency operator. However, both these aspects can be easily varied and experimented. We have here, by a matter of simplicity, set up an investigation of **C**-systems based on classical propositional logic, but it is clear that other approaches may be tackled, by the investigation of **C**-systems based on relevance logic, or intuitionistic logic, as soon as some paradoxes of relevance, or paradoxes raising from some non-constructive assumption, are decided to be avoided. This is clearly not, however, a problem of *paraconsistency* as we have it, but a further (interesting) problem which can be added to it.

Some **dC**-systems based on intuitionistic logic have in fact been defined and studied, for instance, in Bunder's [29]. $\mathbf{B}_1$, the stronger logic of the main hierarchy of calculi proposed by the author of that paper, is obtainable simply by dropping the axioms (Min9), (Min10) and (Min11) out of **Cil**, while adding to the resulting logic the axiom:

(bun) $(A\to(\circ B\wedge(B\wedge\neg B)))\vdash\neg A$. 'If A implies a bottom particle, then $\neg A$ is the case'

It is more or less clear that the deletion of the above axioms from **Cil** will give the resulting logic a kind of intuitionistic behavior, and that the addition of (bun) cannot recover any of the classical properties which were lost. Despite of this, most, if not all, other claims that the author advances about this logic seem to be mistaken. He conjectures, for instance, that the strong negation defined by the antecedent of (bun), by setting $\dot{\sim}A\stackrel{\text{def}}{=}(A\to(\circ B\wedge(B\wedge\neg B)))$, for some formula B, *is not* a classical negation, and *continues not to be* a classical negation even if one adds back to $\mathbf{B}_1$ the axioms (Min10) and (Min11). This is wrong, for we know from FACT 3.45 that in this case the axiom (Min9) turns to be provable, and so we obtain a logic at least as strong as **Ci** (plus (bun), if this would make any difference), but than we remember from FACT 3.49 that $\dot{\sim}$ is *indeed* a classical negation in **Ci** (and even in **bC**, as we saw in THEOREM 3.48). The author then claims, and purports to prove, that his $\mathbf{B}_1$ is *not* a subsystem of da Costa's logic C_1, which, as we will see in the subsection **3.10**, is simply an extension of **Cil**. Once more he is wrong, and for the very same reason —FACT 3.49 shows us once more that (bun) is evidently provable in **Ci**, and so already **Ci** (and consequently C_1) extends $\mathbf{B}_1$ and all the other weaker calculi proposed by Bunder. From that point on, all of the remaining remarks made by this author on the comparison of his calculi with the ones proposed by da Costa falls apart. The only point remaining from those calculi, therefore, is that of constituting **dC**-systems based on intuitionistic, rather than classical, logic. But the author did not even try to study them any deeper, looking for instance for interpretations for these calculi!

In another paper, Bunder unwittingly produced an even bigger mistake. Starting from the reasonable idea of looking for other formulations for da Costa's version of *reductio* (that is, (RA0): $[\circ B,\ (A\to B),\ (A\to\neg B)\vdash\neg A]$, in the subsection **3.2**), the author simply proposes (in [31]) to change $\circ B$ for $\circ A$ in that formula, asserting that 'there seems to be no particular reason why, in (RA0), the B has a restriction, rather than the A'. In this case, however, we can use our THEOREM 3.13 again to see that this proposal would be equivalent to the addition of $[\circ B,\ A,\ \neg A \vdash\ C]$ to C_{min}, which is clearly absurd, for it would be trying to express the consistency of A by way of some foreign formula B! The author then claims that the 'paraconsistent' calculus D_1 (again, the 'strongest' one of a hierarchy D_n) that he obtains by adding this last formula as a new axiom to C_1 is 'strictly stronger than C_n', the calculi of da Costa's hierarchy, and purports to prove some facts about them. Once more these facts turn out to be mistaken, and this is easy to see if one remembers that $\circ\circ D$ is a theorem of **Ci** (see our FACT 3.33(i), or his Theorem 5), and so we are left with $[A,$ $\neg A \vdash\ C]$, for any A and C, and explosiveness is back. So, the author was actually right about his calculi being extensions of the calculi C_n, but only because they all *collapse* into classical logic, after all…[20]

As advanced above (and we shall confirm this below, in the subsection **3.10**), that the identification of consistency with the formula $\neg(A\wedge\neg A)$ was exactly what was done in da Costa's calculus C_1, which in fact just adds to **Cil** some more axioms to deal with the 'propagation of consistency' from simpler to more complex formulas. Now, many authors have criticized this identification —'there is nothing sacrosanct about the original definition of this schema as $\neg(A\wedge\neg A)$', says Urbas in [107]—, or else its consequences, as we have mentioned above (as the 'anomalies' described, for instance, in Sylvan's [105]). One of the most unexpected consequences of this identification, in fact, has already been pointed out in Urbas's [107], Theorem 4, and was hinted above in our subsection **3.2**:

THEOREM 3.57 In **Cil** the consistency of the formula A can be expressed by the formula $\neg(A\wedge\neg A)$, but *not* by the formula $\neg(\neg A\wedge A)$. In fact, one can even add $\neg(\neg A\wedge A)$ to **Cil**, but not $\neg(A\wedge\neg A)$, without this logic losing its paraconsistent character.

Proof: That consistency is so expressed in **Cil** and that $\neg(A\wedge\neg A)$ cannot be added to it are simply consequences, respectively, of COROLLARY 3.56 and FACT 3.54. But while it is easy to see that $[\neg(A\wedge\neg A) \vdash_{\mathbf{Cil}} \neg(\neg A\wedge A)]$, the converse of this does *not* hold, as we see from the matrices in THEOREM 3.21 (those three-valued matrices are in fact a much simpler way of checking the same result for which Urbas has used six-valued ones). Notice, in particular, that these matrices in fact also validate the formula $\neg(\neg A\wedge A)$. □

The above phenomenon is a bit tricky, and has actually fooled people working with the calculi C_n for perhaps too long a time (see, for instance, [76], note 6, ch.2, p.49, or else [42]). Let us then consider the following alternatives to the 'levo-'axiom (cl):

(cd) $\neg(\neg A\wedge A) \vdash \circ A$;
(cb) $(\neg(A\wedge\neg A)\vee\neg(\neg A\wedge A)) \vdash \circ A$.

[20] By the way, given the above considerations, one of Bunder's main results about these systems (besides the supposed proof about all the calculi D_n constituting different systems), objected to show that the calculi D_n do not satisfy (IpE) (namely the Theorem 10 that closes his [31]) evidently must fail. It is easy, in fact, to find counter-examples for the validity of the formula $[\neg(A\wedge\neg A),\ (A\to B),\ (A\to\neg B) \vdash$ $\neg A]$ in the matrices that he proposes (picking them up from Urbas's [107], who have used them correctly, in the case of the C_n systems): just choose $v(A)\in\{0,3\}$ and $v(B)=1$.

Evidently, the addition to **Ci** of the 'dextro-'axiom (cd), instead of the axiom (cl), would give us this logic **Cid** which has exactly the same qualities and defects as **Cil**, but which would singularize the formula $\neg(\neg A \wedge A)$ as much as the formula $\neg(A \wedge \neg A)$ has been previously singularized by **Cil**. The addition of (cb), instead, defining the logic **Cib**, would assure to both $\neg(A \wedge \neg A)$ and $\neg(\neg A \wedge A)$ the same status, and this would fix, for instance, the famed asymmetry in THEOREM 3.21(iii) (but not the ones in parts (i) and (ii)). Logics having (cb) instead of (cl) have already been studied (see [36] or [76]), but it should be noted that these still suffer from some anomalies related to the definition of consistency in terms of some operation over a conjunction of contradictory formulas. In fact, if the logics having (cb) as an axiom do identify the two formulas above, they do not necessarily identify these with some other formulas such as $\neg(A \wedge (A \wedge \neg A))$, or $\neg((A \wedge \neg A) \wedge A)$, for instance, even though all of the formulas $(A \wedge \neg A)$, $(\neg A \wedge A)$, $(A \wedge (A \wedge \neg A))$ and $((A \wedge \neg A) \wedge A)$ are equivalent on any **C**-system based on classical logic. All of this will have, of course, deep consequences when we go on to provide semantics to these logics, as the reader will see in [42]). Perhaps a good way of fixing all of this at once is by the addition of a new 'global' axiomatic rule to such **dC**-systems, such as:

(cg) $\quad (B \leftrightarrow (A \wedge \neg A)) \vdash (\neg B \leftrightarrow \neg(A \wedge \neg A))$,

or else the weaker deduction rule:

(RG) $\quad [B \dashv\vdash (A \wedge \neg A)] \Rightarrow [\neg B \dashv\vdash \neg(A \wedge \neg A)]$.

Logics having such rules are yet to be more deeply investigated. In one way or another, it is clear that the mere addition of such rules is not enough to remedy the whole of THEOREM 3.21 (but compare the following result to the proposal by Mortensen, in the subsection **3.12**). Indeed, similarly to what had happened in COROLLARY 3.22 and in THEOREM 3.35:

THEOREM 3.58 (IpE) does not hold for **Cib** plus (cg) or (RG).
Proof: Check for instance that THEOREM 3.21(iii) still holds, that is, that formulas such as $\neg(A \vee B)$ and $\neg(B \vee A)$ are still not equivalent, once more by way of the same matrices and distinguished values as in THEOREM 3.15(ii), but now changing only the value of $(1 \vee \frac{1}{2})$ from 1 to ½ (and leaving the value of $(\frac{1}{2} \vee 1)$ as it is, equal to 1). □

It is also noteworthy that da Costa in fact proposed not just one definition of consistency (the one above, from the calculus C_1), but considered instead the possibility of having weaker and weaker logics (see [49] or [50]), modifying the requirement for consistency in such a way as to produce an infinite number of logics at once (he acted more with an illustrative than with a practical purpose, but that manoeuvre has, in one way or another, produced some permanent impression). The idea is simple, namely that of having, for each C_n, $0 \leq n < \omega$, more and more premises to be fulfilled in order to guarantee consistency. In the case of $n=1$ we already know that $\circ A$ (da Costa denoted it A°) abbreviated the formula $\neg(A \wedge \neg A)$, for $1 < n < \omega$ it was taken to be $A^{(n)}$, where this abbreviation was recursively defined by first setting A^n, $0 \leq n < \omega$, as $A^0 \stackrel{\text{def}}{=} A$ and $A^{n+1} \stackrel{\text{def}}{=} (A^n)^{\circ}$, and then setting $A^{(n)}$, $1 \leq n < \omega$, as $A^{(1)} \stackrel{\text{def}}{=} A^1$ and $A^{(n+1)} \stackrel{\text{def}}{=} A^{(n)} \wedge A^{n+1}$. In other words, each of da Costa's **dC**-systems was defined by exactly the same axioms, changing only the definition of $\circ A$ in each case for $A^{(n)}$, for each given n.[21] It is clear in this way, if one really feels inclined to do it for some reason or another, that for each **dC**-system one can go on to multiply it into an infinite number of (in principle, distinct) **dC**-systems, applying the same strategy above. Of course, the same asymmetries already pointed out in the case of **Cil**, **Cid** and **Cib** (THEOREM 3.57), may still apply in each case in appropriate forms, and the theorems obtained from these systems must also be modified, in each case, according to the specific definition of consistency brought therein.

3.9 The opposite of the opposite. Having been introduced to the **dC**-systems, a particular class of **C**-systems that can dispense with the use of the operators $\circ$ and $\bullet$, we shall not, nevertheless, dedicate ourselves in what follows exclusively to the study of **dC**-systems. All the logics we will present from this point on are still bound to be **C**-systems extending **Ci**, but only by chance will they turn out to be **dC**-systems as well. What we will consider in this subsection is the addition of the following axiomatic rule for 'expansion' of negations, converse to (Min11): $(\neg\neg A \to A)$:

(ce) $\quad A \vdash \neg\neg A$.

Let **Cie** be the logic obtained by the addition of (ce) to **Ci** (recall, from THEOREM 3.30, that this addition is *not* redundant). In the subsections **3.1** and **3.2** we have learned about the role played by (Min11) in **bC** and the logics which extend it (recall THEOREM 3.13), and suggestions were made as to the reasons why da Costa has introduced (Min11) in his first paraconsistent calculi as a dual substitute to (ce), present in intuitionistic logic, as much as (Min10): $(A \vee \neg A)$ was intended to be the dual substitute to (PPS), the explosiveness (or *reductio*) that is lost by all paraconsistent calculi. Despite this, qualified forms of both (PPS) and (ce) are retained by **bC**, in the form of the rule (bc1): $[\circ A,\ A,\ \neg A \vdash\ B]$, and the rule $[\circ A,\ A \vdash\ \neg\neg A]$, this last one being a rule of **bC** that comes immediately from (bc1) and FACT 3.14(iii). Now, it happens that only (PPS), but not (ce), is a problem of paraconsistent logic, as we put it, and, as far as we know, (ce) was only avoided by da Costa in his first calculi (see [49] and [50]), in spite of his manifest intention, on his requisite **dC[iv]** (see the last subsection), to maintain 'most rules and schemas of classical logic not conflicting with the other requisites', because there seemed to be some apprehension about the addition of (ce) leading us back to classical logic, **CPL**, or perhaps making us just lose the paraconsistency character of our logics, after all. It is, however, very easy to see that this is not the case. Indeed:

THEOREM 3.59 (tPS): $(A \to (\neg A \to B))$ is not provable by **Cie**.

[21] One should observe, however, that the definition of the schema $A^{(n)}$ proposed in da Costa's foundational work, [49], in reality does *not* coincide with the definitions to be found in other studies in the literature (such as the well-known da Costa's [50], or da Costa & Alves's [53]), and those that we adopt here. Indeed, on page 16 of [49] the reader will find the following definition, setting $A^{(1)} \stackrel{\text{def}}{=} A^\circ$ and $A^{(n+1)} \stackrel{\text{def}}{=} A^{(n)} \wedge (A^{(n)})^\circ$. If one follows this last definition, one ought to conclude, for instance, that $A^{(3)}$ is to denote the formula $A^\circ \wedge A^{\circ\circ} \wedge (A^\circ \wedge A^{\circ\circ})^\circ$, while the definition we have presented above would give instead $A^\circ \wedge A^{\circ\circ} \wedge A^{\circ\circ\circ}$. It is easy to see, however, if one just makes use of any of the semantics and decision procedures that have been associated to the calculi C_n that these two formulas are *not* equivalent in each C_n (see [36] for the semantics of a slightly stronger version of these calculi —in the case of $n = 1$, for instance, axiom (cb) is used instead of (cl), and axiom (ce), which appears below, was also added; or else go to [76] and [74], for the original versions).

Proof: Use the matrices of **LFI1** again, as in FACT 3.29, or else the matrices of $\mathbf{P}^1$, as in THEOREM 3.30 —but in this last case you must change the matrix of negation, setting the value of $\neg$½ as ½, instead of 1. In fact, it is to be remarked that this modification on the matrices of $\mathbf{P}^1$ in fact originates a new and interesting maximal three-valued paraconsistent logic, $\mathbf{P}^2$. These three logics, **LFI1**, $\mathbf{P}^1$ and $\mathbf{P}^2$, will be studied in their own right in the subsection **3.11**, as members of a larger family of similar logics. □

Some of the immediate and main syntactical results obtained by the logic **Cie** are:

FACT 3.60 **Cie** proves the following:
(i) $\circ\neg A \vdash_{\mathbf{Cie}} \circ A$;
(ii) $\bullet A \vdash_{\mathbf{Cie}} \bullet\neg A$.
Proof: Just turn the FACT 3.38 upside-down. □

Now, some people felt unease by the presence of FACT 3.60(ii), understanding that this would mean a 'proliferation of inconsistencies' —given that any formula A proved to be inconsistent would, by way of that rule, generate infinitely many 'other' inconsistencies (the negation of A, the negation of the negation of A, and so on). Be that as it may, it is still clear that in **Cie** there are no *new* inconsistencies added by way of this procedure, in a sense, given that the converse of FACT 3.60(ii) is also valid here (it is the FACT 3.38(ii)), and so, in fact, $\bullet A$ and $\bullet\neg A$ are *equivalent* formulas! In **Cile**, the logic obtained by the addition of (ce) to **Cil**, instead of **Ci** (to see that this logic is also paraconsistent, use again the matrices of $\mathbf{P}^2$ in THEOREM 3.59) we evidently obtain a new version of the above result, and FACT 3.60(ii) converts itself into $[(A\wedge\neg A) \vdash_{\mathbf{Cile}} (\neg A\wedge\neg\neg A)]$, leading, so it seems, into a 'proliferation of contradictions'. Once again, this would perhaps not be said to be the case if the formulas at the right and the left hand side are again remembered to be equivalent.

Routley & Meyer, in [100], on their attempt to define a 'dialectical logic', *DL*, meeting the standards of 'Soviet logic' and to recover the 'orthodox Marxist view of negation' have pondered the possibility of criticism coming from some dialecticians to the effect that their 'negative logic is excessively classical', and considered the constitution of a 'weaker dialectical logic', *DM*, having only (Min11), but not (ce), as an axiom. But, even in the case of their *DL*, they have met inferences such as the ones above, acknowledging the possibility of generation of an infinite number of 'distinct' contradictions from any given one, and still defended that this would be all right — one just has to remember that it is still not the case that *any* contradiction is derivable, indeed, just a very specific set of contradictions, 'forming a chain', are derivable, but this, of course, 'does not result in total system disorganization'. But how could it be that these contradictions that they obtain in *DL* are all *distinct*, as they asserted? This is a bit tricky. Let A_0 and $\neg A_0$ be two theorems of *DL* (it's a *dialectical* logic, after all —see the subsection **2.2**), and let A_n abbreviate the formula $(A_{n-1}\wedge\neg A_{n-1})$. We have already learned in the last subsection about Routley & Meyer's argument for the validity of the schema $\neg(A\wedge\neg A)$, and from this we may conclude that each A_n will be a theorem, starting from the mere fact that both A_0 and $\neg A_0$ hold. But if, on the one hand, it is clear that $(A_n\rightarrow A_{n-1})$ will be a theorem of *DL*, on the other hand it is equally clear that $[A_n \dashv\vdash A_{n-1}]$ holds, as above, similarly also to what had occurred in the cases of **Cie** and **Ciel** (FACT 3.60(ii) and its variations). So, again, how could it be that 'all these contradictions are distinct', as asserted by these authors? The point is that the propositional bases of both *DL* and *DM* are relevance logics, and so it may occur that A_n and A_{n-1} are equivalent formulas, but it is still the case that $(A_{n-1} \to A_n)$ is not a theorem of *DL*. So, after all, we see that *this* is the sense of 'distinctness' employed by those authors, determined exclusively by the validity or not of a bi-implication, and *not* by the sets of consequences of the formulas under examination.

3.10 Consistency may be contagious! Supposing we can really trust the consistency of some formulas in our theories, what can we say about the more complex formulas that one can build using the last ones as components: will *these* be also consistent? From FACT 3.38(i): $[\circ A \vdash \circ\neg A]$ we know that already in **Ci** the consistency 'propagates' through negation, that is, the consistency of A is sufficient information for us to be sure about the consistency of $\neg A$. This is essentially a consequence of (Min11): $(\neg\neg A \to A)$ and the identification of inconsistency and contradiction guaranteed by the axiom (ci). Now, what do we know about the propagation of consistency through other connectives besides negation? Not much, so far.

The idea behind the construction of the original calculi C_n by da Costa (see [49] and [50]) was that of requiring each component to be consistent as a sufficient reason to count on the consistency of the more complex formula. Bluntly speaking, da Costa's C_1 was built by the addition to **Cil** (see the beginning of the subsection **3.8**) of the following axiomatic rules:

(ca1) $(\circ A \wedge \circ B) \vdash \circ(A \wedge B)$;
(ca2) $(\circ A \wedge \circ B) \vdash \circ(A \vee B)$;
(ca3) $(\circ A \wedge \circ B) \vdash \circ(A \to B)$.

Let's call **Cila** the logic obtained by the addition of (ca1)–(ca3) to **Cil**. The difference from **Cila** and the original formulation of C_1 is only one: that the connective $\circ$ in C_1 was not taken as primitive, but $\circ A$ was instead denoted as A^{o} and was taken more directly as an abbreviation of the formula $\neg(A \wedge \neg A)$ (recall the THEOREM 3.55). As for the other calculi in the hierarchy C_n, $1 \leq n < \omega$, they were built using the simple trick of letting $\circ A$ abbreviate more and more complex formulas (as we saw at the end of the subsection **3.8**).

As an immediate consequence of the above definitions, one can easily prove in **Cila** —and in each calculus C_n— the following 'translating' results (compare these with the less specific THEOREM 3.11 and with the generally applicable COROLLARY 3.47):

THEOREM 3.61 $[\Gamma \vdash_{\mathbf{CPL}} A] \Leftrightarrow [\circ(\Pi), \Gamma \vdash_{\mathbf{Cia}} A]$, where $\circ(\Pi) = \{\circ p : p$ is an atomic formula occurring as a subformula in $\Gamma \cup \{A\}\}$.[22]

Proof: Immediate, using (ca1)–(ca3). Note that the axiom (cl) plays no role here. □

COROLLARY 3.62 The following mapping conservatively translates **eCPL** inside of **Cia**:

(t2.1) $t_2(p) = \circ p$, if p is an atomic formula;
(t2.2) $t_2(A \# B) = t_2(A) \# t_1(B)$, if # is any binary connective;

[22] Might the reader observe that the first formulations of this result, on da Costa's [49], Theorem 9, page 16, and on da Costa's [50], Theorem 4, page 500, the general case in which an infinite number of atomic formulas occur in $\Gamma \cup \{A\}$ is not considered.

(t2.3) $t_2(\neg A) = \neg t_2(A)$;
(t2.4) $t_2(\circ A) = \circ t_2(A)$.

So, working with **Cila**, all we need to rely on in order to go on making 'classical inferences' is on the consistency of the atomic constituents of our formulas. As a particular consequence of that, one can now substitute each new axiomatic rule of **Cila** by an alternative version in terms of '$\bullet$'s instead of '$\circ$'s. Thus, the axiom (ca3), for instance, can be rewritten as $[\bullet(A \to B) \vdash (\bullet A \vee \bullet B)]$ (use FACT 3.34(i) and COROLLARY 3.62). And so on.

Proposing an infinite number of calculi, instead of one, as in the case of the C_n, only starts to make sense after we prove that we are not just repeating the same tune:

THEOREM 3.63 Each C_n deductively extends each C_{n+1}, for $1 \le n < \omega$.[23]
Proof: We will not here give it a try by usual 'syntactical means'. For sure, this will be much easier to check if one just considers the semantics associated with these calculi, for instance in [53] (*corrected* in [74]) and in [36] (or [76]). □

Evidently, all these calculi C_n extend also the calculus C_ω —they even extend C_{min}, the stronger logic on which we based **bC**, our first **LFI** (recall the subsection **3.1** for the definition of these logics). This C_ω, we argue, was indeed a very bad choice as a kind of 'limit' to the hierarchy C_n, $1 \le n < \omega$. Consider, for instance, the following result:

FACT 3.64 The only addition made by C_n (in fact, by **Cia**, for the axiom (cl) has no use in this result) to the rules provable by **bC** about the interdefinability of the binary connectives (see THEOREM 3.18) is the rule (ix): $[\neg(A \wedge B) \vdash_{\mathbf{Cia}} (\neg A \vee \neg B)]$, and its variants.
Proof: We just show that that rule holds already in **Cia**, and point the reader again to the semantical studies of the calculi C_n to check that the other formulas are still not provable in **Cila**. First of all, setting $\Gamma = \{\circ(A \wedge B), \neg(A \wedge B), A\}$ it can immediate be seen that $[\Gamma, B \vdash_{\mathbf{Cia}} \circ(A \wedge B)]$, $[\Gamma, B \vdash_{\mathbf{Cia}} (A \wedge B)]$ and $[\Gamma, B \vdash_{\mathbf{Cia}} \neg(A \wedge B)]$, so we apply FACT 3.14(ii) to obtain $[\Gamma \vdash_{\mathbf{Cia}} \neg B]$, and consequently $[\Gamma \vdash_{\mathbf{Cia}} (\neg A \vee \neg B)]$. But then, also $[\neg A \vdash_{\mathbf{Cia}} (\neg A \vee \neg B)]$, and so the proof by cases will give us $[\circ(A \wedge B), \neg(A \wedge B) \vdash_{\mathbf{Cia}} (\neg A \vee \neg B)]$. By (ca1) we then conclude that $[\circ A, \circ B, \neg(A \wedge B) \vdash_{\mathbf{Cia}} (\neg A \vee \neg B)]$, but we also have, from LEMMA 3.43(ii), that $[\vdash_{\mathbf{Cia}} (\neg A \vee \circ A)]$, and from that we obtain $[\circ B, \neg(A \wedge B) \vdash_{\mathbf{Cia}} (\neg A \vee \neg B)]$. By a similar reasoning, from $[\neg B \vdash_{\mathbf{Cia}} (\neg A \vee \neg B)]$, we finally arrive at our goal, $[\neg(A \wedge B) \vdash_{\mathbf{Cia}} (\neg A \vee \neg B)]$. □

COROLLARY 3.65 (IpE) cannot hold in the calculi C_n, or in any extension of them.
Proof: Just recall THEOREM 3.51(iv). □

The FACT 3.64 also suggests some further information about the plausibility of calling either C_ω or C_{min} 'limits' for the hierarchy C_n, $1 \le n < \omega$. For, as we have seen in THEOREM 3.18, these new forms of De Morgan rules that we now have in each C_n

[23] A supposedly general proof of this fact, dating still from the 'syntactical period', when no semantics had yet been presented to those calculi, appears for instance in da Costa's [49], pp.17–9, and once again in Alves's [2], pp.17–9, and is credited to Ayda Arruda. There is surely some mistake, however, in their attempt to prove the independence of each axiom $[A^{(n)}, A, \neg A \vdash B]$ with respect to the axioms of C_{n+1}, given that this very axiom assumes non-distinguished values in all matrices $\mathbf{T}_n$ thereby presented, if one only picks 1 as the value of A and picks for B any value in between 1 and $n+2$.

are *not* present even in C_{min}. Also, by a combination of FACT 3.40(i) and COROLLARY 3.56, we know that $((A\wedge\neg A)\to\neg\neg(A\wedge\neg A))$ is valid in **Cia**, and so in each C_n, while we also know, from the matrices of $\mathbf{P}^1$, as in THEOREM 3.15(ii), that this formula cannot be a theorem of neither C_{min} nor C_ω. Now, it is only compelling to think of a *deductive limit* for an infinite hierarchy of increasingly weaker calculi as the logic having as inferences exactly all sets of inferences common to the whole hierarchy![24] As we have shown in [39], it *is* possible to define such a logic, for each hierarchy of **dC**-systems as the one given above, by way of the useful tool of possible-translations semantics, obtaining, as a byproduct, some clear-cut and effective decision procedures, even though some other very interesting questions, such as how to finitely axiomatize this limit-calculi, or how to define a strong negation in them, if this is possible at all, were still left open (check also the paper [42]). Indeed, notice that when we go from each C_n to the following C_{n+1} we need in fact to add a further requirement in order to express the consistency of a formula A —while in C_n this was expressible by way of $A^{(n)}$, or, equivalently, by way of the set $\{A^1, A^2, \ldots, A^n\}$, in C_{n+1} that same set must be incremented by the formula A^{n+1}. So, ultimately, in C_{Lim}, the deductive limit of the hierarchy C_n, the consistency of A can evidently be expressed by an infinite number of formulas, and again we obtain a logic which is gently explosive, being thus an **LFI**. What we still do not know is if logics such as C_{Lim} can be alternatively characterized in such a way so as to also reveal themselves as *finitely* gently explosive, like all the other logics presented up to this point, based on the axiom (bc1). If this characterization is not possible, it is hard to see how a strong negation or a bottom particle could then be *defined* in such a logic,[25] so that this would make a

[24] This logic C_ω has puzzled people for too long as 'part' of the original hierarchy C_n. The existence of a logic as a real deductive limit to this hierarchy (see [39]) shows that it was clearly just a matter of coincidence that C_ω would appear as a kind of 'syntactical limit' to the original axiomatic formulation of the C_n, by the deletion of all the axioms and definitions involving the connectives $\circ$ and $\bullet$ ((bc1), (ci) and (cl)), and the 'deletion' also of (Min9) (actually, this last axiom was not in the original formulation of these calculi, and could not even be proved from the other axioms of C_ω —see THEOREM 3.3). To put the matter in clear terms, C_ω *can* of course be studied in its own right, as a very weak paraconsistent (non-**LFI**) logic based on positive intuitionistic logic, but it *should not* be seen as part of the hierarchy C_n, $n\geq 1$, for it has no more right to occupy that position than C_{min} or many other logics that could substitute it would have!

This coincidence had also some harmful effects on the philosophical appreciation of the logics produced by da Costa. As da Costa himself has put it in his original piece on these systems (cf. [49], p.21), 'roughly speaking, we could say that human reason seems to attain the peak of its power the more it approaches the danger of trivialization'. This statement has been inspiring people to naively defend stances according to which, for instance, 'the more a theory is useful to found mathematics, the more easily it results to trivialize it; and the more difficult it is to trivialize it, the less it is useful to found mathematics' (see [26], p.243). There are good and bad points about these somewhat hasty conclusions. First, as a general technical assertion about paraconsistent logics in general, da Costa's motto is certainly misleading, given the existence of maximal logics such as the three-valued *Pac* (subsection **2.4**), which is both as strong as a fragment of classical logic as it could be, and at the same time is not finitely trivializable at all. One could, then, restrict their attention to **LFI**s and repeat that motto in an environment in which it seems to make sense. In that case, of course, the second statement above would be affirming that no non-**LFI** could be useful to found mathematics —and *this* statement would be very likely to find its defensors (cf., for instance, Batens's attack [13] on Priest's [90]).

[25] Here, we really mean *defined* inside the logic, as a real *formula* of this logic. For instance, in Priest's [90] a logic containing no bottom particle is presented, but the author argues that such a propositional constant $\perp$ could be 'thought of informally as the conjunction of all formulas' (p.146), so that, for instance, a strong negation ~ would be obtainable from that in the usual way, by letting ~A be defined

case in which the Gentle Principle of Explosion does not coincide with the Supplementing one, or with *ex falso* (compare this with FACT 2.19, and the comments which follow that result).

Some other interesting theses of **Cila** are the following (see Urbas's [107]):

FACT 3.66 In **Cila** the schemas $\circ\bot$ and $\circ\dot{\div}\dot{\div}A$ are provable.
Proof: Recall first that $\dot{\div}A$ was the classical negation defined inside of **bC** (in THEOREM 3.48) as $(A\to\bot)$, and $\bot$ is a bottom particle that can be defined, for instance, as $(\circ B\wedge(B\wedge\neg B))$, for some B. Now, by FACT 3.33 and COROLLARY 3.56, we know that both $\circ\circ B$ and $\circ(B\wedge\neg B)$ are theorems of **Cil**, and then we conclude, by the axiom (ca1), that $\circ(\circ B\wedge(B\wedge\neg B))$, and so $\circ\bot$ is a theorem of **Cila**. Recall also from FACT 3.49 that $\dot{\div}A$ is equivalent, in **Ci**, to $\sim A$, and this last strong negation was defined as $(\neg A\wedge\circ A)$, and so we have in particular that $[(A\to\bot)\vdash_{\mathbf{Cila}}\circ A]$. So, from this last inference and from the fact that $\circ\bot$ is a theorem of **Cila**, as proved above, we use (ca3) and conclude that $[(A\to\bot)\vdash_{\mathbf{Cila}}\circ(A\to\bot)]$. As particular cases of the last two inferences, substituting A for $((A\to\bot)\to\bot)$ in the first case, and for $(A\to\bot)$ in the second case, we obtain, respectively, $[\dot{\div}\dot{\div}\dot{\div}A\vdash_{\mathbf{Cila}}\circ\dot{\div}\dot{\div}A]$ and $[\dot{\div}\dot{\div}A\vdash_{\mathbf{Cila}}\circ\dot{\div}\dot{\div}A]$, and the version of proof by cases obtained for $\dot{\div}$ leaves us at last with $[\vdash_{\mathbf{Cila}}\circ\dot{\div}\dot{\div}A]$. □

This last result gives us yet another reason for the failure of (IpE) in the C_n and in their extensions (COROLLARY 3.65) —now, it is THEOREM 3.51(i) that applies. As we shall see in subsection **3.12**, the failure of (IpE) makes it impossible for us to find a Lindenbaum-Tarski-like algebraization for these logics. In the case of the C_n the situation is actually worse: as Mortensen ([82]) has shown, no non-trivial congruence is definable for these logics, making these logics non-algebraizable even in a much more general sense, the one of Blok-Pigozzi (cf. THEOREM 3.83). There are, nevertheless, several extensions of the C_n in which non-trivial congruences *can* be defined, being thus much more receptive to algebraic treatments. We will be seeing many examples of these below.

Let us now investigate another way of propagating consistency, by liberalizing a little bit the conditions required by **Cila** (that is, C_1). Da Costa, Béziau & Bueno proposed, in [57], to substitute the above axioms, (ca1)–(ca3), by the following:

(co1) $(\circ A\vee\circ B)\vdash\ \circ(A\wedge B)$;
(co2) $(\circ A\vee\circ B)\vdash\ \circ(A\vee B)$;
(co3) $(\circ A\vee\circ B)\vdash\ \circ(A\to B)$.

We will call **Cilo** the logic obtained by the addition of (co1)–(co3) to **Cil**. It is very easy to see, using the positive axioms, that this logic, christened C_1^+ in [57], is a deductive extension of C_1. Requiring less assumptions in order to obtain consistency of a complex formula in terms of the consistency of its components, **Cilo** (or even **Cio**, already, without recourse to the axiom (cl)) gives us some interesting results such as:

THEOREM 3.67 $[\Gamma\vdash_{\mathbf{Cio}}\circ A]$ whenever $[\Gamma\vdash_{\mathbf{Cio}}\circ B]$, for some subformula B of A.
Proof: Immediate, using (co1)–(co3). □

as $(A\to\bot)$. But such an 'informal' bottom particle is simply no formula of our language! (And if it were, then we would have been done: all paraconsistent logics would turn out to be **LFI**s, in one way or another). This idea of Priest has already been criticized in Batens's [13].

FACT 3.68 **Cio** makes some new additions to FACT 3.64 and to the rules displayed in THEOREM 3.18 about the interdefinability of the binary connectives, namely the following provable schematic rules:

(vi) $\neg(A\wedge\neg B) \vdash_{\mathbf{Cio}} (A\to B)$;

(vii) $\neg(A\to B) \vdash_{\mathbf{Cio}} (A\wedge\neg B)$;

(xi) $\neg(\neg A\vee\neg B) \vdash_{\mathbf{Cio}} (A\wedge B)$.

Proof: Go to [57] or [76], or else the section on semantics for **Cibo** (and **Ciboe**) in [42], to check this. The actual syntactical proofs are, in any case, structurally similar to the one presented in FACT 3.64. □

Of course, given THEOREM 3.51(v), we know that FACT 3.68(vii) gives us yet another reason for the failure of (IpE) from this point on. But that we already knew, from the case of **Cila**, in FACT 3.64. What is new in this case is only that **Cilo** *can*, differently from **Cila**, define non-trivial congruences, making it possible to algebraize it *à la* Blok-Pigozzi (see FACT 3.81).

Once again, the axioms (co1)–(co3) have equivalent versions in terms of $\bullet$, instead of $\circ$, and it is an easy exercise to try to find them. The reader should remember that both **Cila** and **Cilo** have not only associated decreasing hierarchies, and can evidently define different calculi as their deductive limits, but they also can be structurally varied in terms of their inner definition of consistency, if we only change the axiom (cl) for axiom (cd), or (cb), or (cg), as we did in the subsection **3.8**, or if we add to them the axiom for expansion of negations, (ce), as in the subsection **3.9** (defining the logics **Cido**, **Cibo**, **Cito**, **Ciloe**, **Cidoe**, **Ciboe** and **Citoe**). In all these cases, we can show that the resulting logics are extended by the three-valued paraconsistent logic $\mathbf{P}^2$, as in the THEOREM 3.59.

There are, actually, an unlimited number of ways of propagating consistency.[26] Before proceeding to a general investigation of the 'extreme cases', in the next subsection, let us just briefly survey some propagation axioms which have already shown up in the literature so far, and some of their consequences. Consider, for instance, the following axioms, converse to (co1)–(co3):

(cr1) $\circ(A\wedge B) \vdash (\circ A\vee\circ B)$;

(cr2) $\circ(A\vee B) \vdash (\circ A\vee\circ B)$;

(cr3) $\circ(A\to B) \vdash (\circ A\vee\circ B)$.

Adding these to **Cibo** and to **Cio** (that is, **Cibo** minus the axiom (cb)) we build, respectively, the logics **Cibor** and **Cior** (and so on, *mutatis mutandis*, for **Cilo** and **Cido**). These give us yet more perspectives on the propagation (and back-propagation) of consistency, and some of the possible meanings which can be assigned to it). Suppose, on the other hand, that we, more simply, consider the following axioms in order to automatically guarantee the consistency of some complex propositions:

(cv1) $\vdash \circ(A\wedge B)$;

(cv2) $\vdash \circ(A\vee B)$;

[26] Working with monotonic logics, the addition of consistency-propagation to some logic always means a gain in deductive strength. However, in a non-monotonic environment in which consistency is pressuposed by default, given that propagating consistency in one direction can mean propagating inconsistency the other way, the addition of such axioms for propagation can either be innocuous or in some cases even have a weakening effect on the resulting system (see [79], and [15]).

(cv3) $\vdash \circ(A \to B)$;
(cw) $\vdash \circ(\neg A)$.

Let's add **v** to the name of a logic that contains the axioms (cv1)–(cv3), and add **w** to the name of a logic containing (cw). Let's also recall the axiom (ce): $[A \vdash \neg\neg A]$, from which we obtained the FACT 3.60 (backward propagation of consistency through negation). There are now several new possible combinations to be considered. Evidently, any logic having (cv1)–(cv3) proves not only (co1)–(co3) but also (ca1)–(ca3). So we have, for instance, the logic **Cibv**, and have at least two immediate ways of enrichening it with respect to the behavior of negation, obtaining the logics **Cibve** and **Cibvw**. As it happens, **Cibvw** axiomatizes the three-valued maximal paraconsistent logic $\mathbf{P}^1$ (matrices in THEOREM 3.15(ii)), that has the peculiarity of admitting inconsistency only at the atomic level, and **Cibve** axiomatizes $\mathbf{P}^2$ (matrices in THEOREM 3.59), a logic that admits inconsistency only at the level of atomic propositions, or of propositions of the form $(\neg^n p)$, where p is atomic and $\neg^n$ denotes n applications of negation. If, on the other hand, one considers the logic **Ciborw**, once more a three-valued paraconsistent logic pops up, namely the one given by the following matrices:

$\wedge$	**1**	**½**	**0**
1	1	1	0
½	1	½	0
0	0	0	0

$\vee$	**1**	**½**	**0**
1	1	1	1
½	1	½	1
0	1	1	0

$\to$	**1**	**½**	**0**
1	1	1	0
½	1	½	0
0	1	1	1

	$\neg$	$\circ$
1	0	1
½	1	0
0	1	1

where 1 and ½ are both distinguished. We shall here call this logic $\mathbf{P}^3$. All these three logics, $\mathbf{P}^1$, $\mathbf{P}^2$ and $\mathbf{P}^3$, are in fact **dC**-systems, and can ultimately dispense with the axiom (cb), proving it from the other axioms. If, on the other hand, we consider the logic **Ciore**, the result is a maximal three-valued logic again, **LFI2** (investigated in [44]) whose matrices differ from those of $\mathbf{P}^3$ only in the matrix of negation, assigning ½ instead of 1 as the value of $\neg(½)$.

All the above logics have some kind of 'non-structural' propagation of consistency, that is, a propagation that does not really depend on the particular connective in focus. Alternatively, one can propose other forms of propagation which do depend on the connectives being considered. Now, some reasonable symmetry conditions on inconsistency and its behavior with respect to the different connectives could suggest to us, for instance, the consideration of the following forms:

(cj1) $\bullet(A \wedge B) \dashv\vdash ((\bullet A \wedge B) \vee (\bullet B \wedge A))$;
(cj2) $\bullet(A \vee B) \dashv\vdash ((\bullet A \wedge \neg B) \vee (\bullet B \wedge \neg A))$;
(cj3) $\bullet(A \to B) \dashv\vdash (A \wedge \bullet B)$.

The logic **Cij**, built from the addition of (cj1)–(cj3) to **Ci**, can now be enriched with (ce) in order to give us **Cije**, an axiomatization for the above many times mentioned maximal three-valued paraconsistent logic **LFI1** (see its matrices in FACT 3.29, and consult [44] again). It is interesting enough to note that neither **LFI1** nor **LFI2** are **dC**-systems, that is, they *cannot* define the consistency operator by way of the other connectives. Let us now just summarize, give some references and mention some properties of the five above mentioned maximal paraconsistent three-valued logics, before we proceed to show, in the next subsection, that these are just the top of the iceberg:

THEOREM 3.69 The matrices of $\mathbf{P}^1$ (in THEOREM 3.15(ii)) are axiomatized by **Civw**; the matrices of $\mathbf{P}^2$ (in THEOREM 3.59) by **Cive**; the matrices of $\mathbf{P}^3$ (above) by **Ciorw**; the matrices of **LFI2** (above) by **Ciore**; the matrices of **LFI1** (in FACT 3.29) by **Cije**.

Proof: As a general reference for all these logics and all the other ones in the next section, consult [78]. As specific references for some of them, go to [103] and [84] for $\mathbf{P}^1$ (or [10], where it appeared under the name PI^v); notice that $\mathbf{P}^2$ has also appeared in [10], under the name PI^m, but was then redefined in [84] (where it actually was wrongly supposed to be characterizable using just one distinguished value, invalidating the soundness proof therein presented —note 13 of that paper shows that the author had even been informed about that) and later rediscovered in [76]; go to [44] for **LFI2** and **LFI1** (but the reader should bear in mind that this last logic is in fact equivalent to the logic called $\mathbf{J}_3$ in [60], and to the propositional fragment of the logic called **CLuNs** in [15], which is in fact identical to the logic Φ_v presented in [101], a logic that has been reappearing quite often in the literature). □

FACT 3.70 Of the rules displayed in THEOREM 3.18 on the interdefinability of the binary connectives, these are the instances validated by each of the five three-valued logics above:

(i) in $\mathbf{P}^1$, $\mathbf{P}^2$, $\mathbf{P}^3$ and **LFI2**: parts (i), (iv), (vi), (vii), (ix), (xi);

(ii) in **LFI1**: parts (i), (iii), (iv), (v), (vii), (viii), (ix), (x), (xi), (xii).

Also, formulas such as $\neg(A\wedge\neg A)$ and $\neg(\neg A\wedge A)$ may be easily seen to hold in **LFI1** and **LFI2**, and rules such as $(A\wedge\neg A) \vdash \neg\neg(A\wedge\neg A)$ hold in $\mathbf{P}^2$, **LFI1** and **LFI2**.

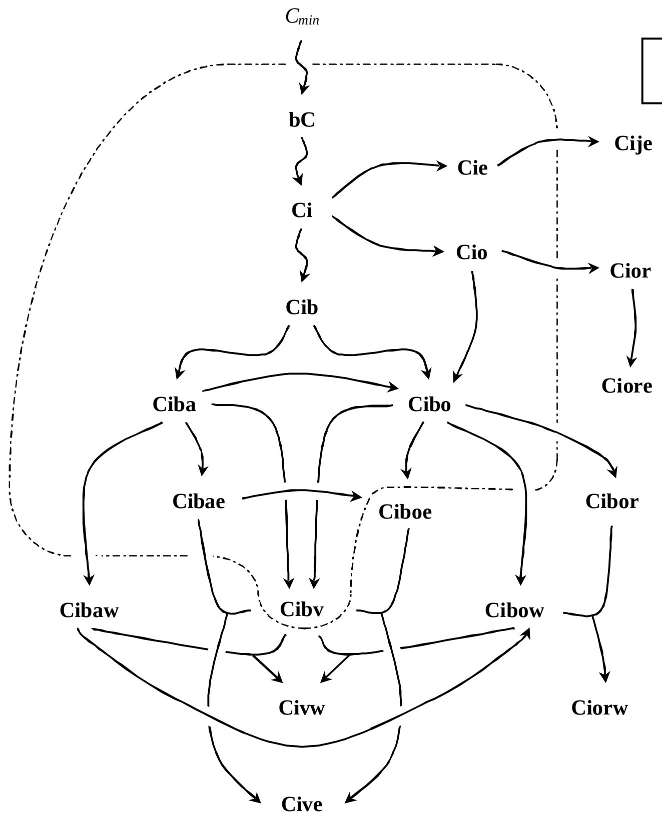


Figure 3.1

Proof: Just use the corresponding matrices to check this. Notice from FACT 3.68 that part (iv) was the only addition made by the first four logics above to the rules already validated by **Cilo**. □

This last result, of course, supplies us with still some further justifications for the failure of (IpE) in all these logics: parts (iii) and (vi) of THEOREM 3.51 applies to **LFI1**, parts (ii) and (vii) of 3.51 apply to both **LFI1** and **LFI2**, parts (iv) and (v) of 3.51 apply to all of the five logics; and finally we will see in FACT 3.76 that part (i) of 3.51 also applies to all of them.

We can, at this point, try our hand at sketching a very thin slice of the great number of **C**-systems introduced so far. Doing that, something like **Figure 3.1** might eventually be obtained. In that figure, an arrow leading from a logic **L**1 into a logic **L**2 says that **L**2 deductively extends **L**1. The logic C_{min}, at the upper end, is the only one that does not constitute a **C**-system; the logics at the lower ends are the three-valued ones appearing in THEOREM 3.69. The logics inside the dotted lines are some of those which we can prove to be *not* many-valued, by adapting the results in [76], pp.213–216 or, better, by checking [42]. The other logics not contemplated by these results, namely **Cior**, **Ciboe**, **Cibor**, **Cibaw**, and **Cibow**, are also conjectured to be not many-valued, but we must at this moment leave the proof of this fact in the hands of our clever readers. Do remember to have a look, however, at the elegant possible-translations semantics offered to **Ciboe** in [42] (also originating from [76], section 5.3).

3.11 Taking it literally: the Brazilian plan completed. The sagacious reader will have observed that all we have been doing so far, in this section on axiomatization of **C**-systems, was to basically try to explore at a very general level some of the possibilities for the formalization and understanding of the relationship between the concepts of consistency, inconsistency and contradictoriness. In particular, this research line makes it possible for us to reconsider and pursue, in an abstract perspective, a specific interpretation of da Costa's method and requisites on the construction of his first paraconsistent calculi (see **dC[i]–dC[iv]**, in the subsection **3.8**). Indeed, starting from the intuition that consistency should be expressible inside some classes of paraconsistent logics, and assuming furthermore that the consistency of a given formula would be enough to guarantee its explosive character (that is, assuming a Gentle Principle of Explosion, as formulated in the subsection **2.4**), we have arrived at the definition of an **LFI**, a Logic of Formal Inconsistency (see (D20), in the same subsection). To realize that (in a finitary way), we have above proposed the axiom (bc1): $[\circ A,\ A,\ \neg A \vdash\ B]$ for particular classes of **C**-systems based on classical logic. Even more than that, as we have remarked before, while **dC[ii]** simply establishes the non-explosive character of the paraconsistent negation, a general formulation of **dC[i]** is realized in a subclass of the **C**-systems, the ones in which the connectives '$\circ$' and '$\bullet$' happen to be definable from the remaining connectives, and to the members of this class we gave the name of **dC**-systems. Now, putting **dC[iii]**, the problem of providing higher-order versions of these logics, aside for a moment, we still need to provide an answer to **dC[iv]**, the requirement that 'most schemas and rules of classical logic' should hold in our logics. And that's the point we will ruminate in the present subsection.

Our proposed interpretation for **dC[iv]** will in fact be a very simple one, involving the following notion of 'maximality'. A logic **L**2 is said to be *maximal relative to* a logic **L**1 if: (i) both are written in the same language (so that they can be deductively compared); (ii) all theorems of **L**2 are provable by **L**1; (iii) given a theorem D of **L**1 which is not a theorem of **L**2, if D is added to **L**2 as a new schematic axiom, then all theorems of **L**1 turn to be provable. The idea, of course, is that any deductive extension of **L**2 contained in **L**1 and obtained by adding a new axiom to **L**2 would turn out to be identical to **L**1. We will call *maximal*, to simplify, any logic **L**2 which is maximal relative to some logic **L**1, previously introduced. Examples of maximal logics abound in the literature. It is widely known, for instance, that each Łukasiewicz's logic $Ł_m$ is maximal relative to **CPL**, the classical propositional logic, if and only if $(m-1)$ is a prime number. We also know that **CPL** is maximal relative to a 'trivial logic', in which all formulas are provable, but on the other hand it is also well-known that intuitionistic logic is *not* a maximal fragment of **CPL**, as the existence of an infinite number of *intermediate logics* promptly attests. As to the **C**-systems which have been introduced this far, only the five three-valued ones that were collected in the THEOREM 3.69 are maximal relative to **CPL**, or else relative to **eCPL**, the extended version of **CPL** introduced in the subsection **3.7** (so that, in particular, the calculus C_1, that we have presented as **Cila**, the strongest calculus introduced by da Costa on his first hierarchy of paraconsistent calculi, or the even stronger calculus C_1^+, that we presented as **Cilo**, proposed by da Costa and his collaborators much later, readily fail to be maximal, and to respect **dC[iv]**).

Let us explore then the idea that underlies the five three-valued maximal **C**-systems above. Suppose we are faced with this problem of finding models to contradictory, and yet non-trivial, theories. We might then intuitively start looking for non-trivial interpretations under which both some formula A and its negation $\neg A$ would be simultaneously validated. A very simple such interpretation would be found in the domain of the many-valued. Suppose we try to depart from classical logic as little as possible, so that the interpretation of our connectives will still be classical if we remain inside the classical domain, and suppose we just introduce a third value (½), besides true (1) and false (0), so that this third value will also be seen as a modality of *trueness*, that is, ½ will also be a distinguished value, together with 1, while 0 will be the only non-distinguished value. There are then two possible negations which are such that there is a model for both A and $\neg A$ being true, for some formula A (see the table to the right). One of these negations, the one that takes ½ into ½, is exactly the negation of **LFI1** and of **LFI2**, the other negation, taking ½ into 1, is exactly the negation of $\mathbf{P}^1$, $\mathbf{P}^2$ and $\mathbf{P}^3$. What about the other connectives? Let us again try to keep them as classical as possible (we want to keep on investigating **C**-systems *based on classical logic*), even at the level of the third value, that is, let us add to the requirement of coincidence of classical outputs for classical inputs the further higher-level 'classical' requirements to the effect that:

	$\neg$
1	0
½	½ or 1
0	1

(C∧) $v(A\wedge B)\in\{½, 1\} \Leftrightarrow v(A)\in\{½, 1\}$ and $v(B)\in\{½, 1\}$;

(C∨) $v(A\vee B)\in\{½, 1\} \Leftrightarrow v(A)\in\{½, 1\}$ or $v(B)\in\{½, 1\}$;

(C→) $v(A\to B)\in\{½, 1\} \Leftrightarrow v(A)\notin\{½, 1\}$ or $v(B)\in\{½, 1\}$.

This leaves us then with the following options:

$\wedge$	**1**	**½**	**0**
1	**1**	½ or 1	**0**
½	½ or 1	½ or 1	0
0	**0**	0	**0**

$\vee$	**1**	**½**	**0**
1	**1**	½ or 1	**1**
½	½ or 1	½ or 1	½ or 1
0	**1**	½ or 1	**0**

$\rightarrow$	**1**	**½**	**0**
1	**1**	½ or 1	**0**
½	½ or 1	½ or 1	0
0	**1**	½ or 1	**1**

Thus, we have, theoretically, 2^3 options of 'conjunctions', 2^5 options of 'disjunctions', 2^4 options of 'implications', and, as we saw above, 2^1 options of 'negations', making a total of $2^{13}(=8{,}192$, or 8K) possible 'logics' to play with. To remove the scare quotes of the previous passage we just have to show that these logics make some sense, and are worthy of being explored. To such an end, and to complete the definition of our 8K logics as **LFI**s, we will just also add to these logics the connectives for consistency and for inconsistency, implicitly assuming that the consistent models are the ones given by classical valuations, and only those (see matrices to the right).

	$\circ$	$\bullet$
1	1	0
½	0	1
0	1	0

Evidently, all these 8K logics will be fragments of **eCPL**, the Extended Classical Propositional Logic (recall the subsection **3.7**). It is also clear that the logic *Pac* (subsection **2.4**) is not one of these, for it cannot define the connectives $\circ$ and $\bullet$, though its conservative extension **LFI1** can (and it is one of the 8K).

Evidently, the five three-valued logics we discussed earlier are but special cases of the above outlined 8K logics, and we already know (from THEOREM 3.69 and **Figure 3.1**) that those five are axiomatizable by way of the addition of suitable axioms to the axiomatization of **Ci**, one axiom for each connective. In fact, as shown in [78], this idea can be extended to all the 8K logics above:

THEOREM 3.71 All the 8K three-valued sets of matrices above are axiomatizable as extensions of **Ci**.

Proof: In each case, one just has to add, for the negation, either the axiom $(A \rightarrow \neg\neg A)$ or the axiom $\circ\neg A$, depending respectively if the negation of ½ goes to ½ or to 1. And, for each other binary connective, $\#(\in \{\wedge, \vee, \rightarrow\})$, one just has to add either $\circ(A\#B)$ or else $(\bullet(A\#B) \leftrightarrow \sigma(A, B))$, where $\sigma(A, B)$ is a schema depending only on A and on B —these last axioms will evidently depend on the specific matrices of each #, and act in order to describe how inconsistency (or consistency) propagates back and forth for each binary connective. Full details on how to define these axioms may be found in [78]. □

Moreover, one can also prove that:

THEOREM 3.72 All the 8K three-valued logics above are distinct from each other, and they are all maximal relative to **eCPL**.

Proof: Again, we refer to [78] for the general proofs. The basic idea behind the proof of *distinctness* is the following: choosing any two of these 8K logics (without repetition), there will be some connective about which they differ, one of them giving 1 as an output for the same input(s) that the other one gives ½. But then the negations of such matrices will not be equivalent, and all we must do then is write down a formula which describes that situation in such a way that this formula will be a theorem of one of these logics, but a non-theorem of the other (again, see [78] on how to do it). For the *maximality* proofs, the reader might mind to be informed that for at least five of those logics (the ones referred to in THEOREM 3.69) the specific proofs were already presented elsewhere. It is also interesting to remark that the connective $\circ$ (or $\bullet$) plays a fundamental role in the general maximality proof exhibited in [78]. □

Now, how do these 8K logics compare with the other **C**-systems that have been studied this far? It is this simple: *every* logic investigated so far either coincides or is extended by some of the above 8K three-valued logics. So that now we have a very interesting class of (extended) solutions to the problem posed by da Costa's requirement **dC[iv]**! Furthermore, it is straightforward to check that all the above matrices do not only extend **Ci**, but also extend **Cia**, so that the original hunch by da Costa for the propagation axioms is a kind of minimal condition obeyed by every one of our 8K maximal three-valued logics. The only limitative point of the original proposal, under this approach, really rests in **dC[i]**, which is of course *not* verified by all those matrices, and in fact imposes a very restricted interpretation for the notion of consistency, limiting our sample space to only a *very* selective class of **dC**-systems, which is, however, larger than the reader might initially imagine (recall, in any case, that logics such as **LFI1** and **LFI2** are *not* **dC**-systems). Indeed:

FACT 3.73 All the 8,192 logics above are **C**-systems extending **Cia**. Of these, 7,680 are in fact **dC**-systems, being able to define $\circ$ and $\bullet$ in terms of the remaining connectives (and being maximal, thus, relative to **CPL**, and not only to **eCPL**). Of these, 4,096 are able to define $\circ A$ as $\neg(A\wedge\neg A)$, and so all of these do extend C_1 (that is, **Cila**). Of the 7,680 logics which are **dC**-systems, 1,680 extend **Cio**, the stronger alternative to **Cia**, and 980 of these are able to define $\circ A$ as $\neg(A\wedge\neg A)$, so that these 980 logics do extend C_1^+ (that is, **Cilo**).
Proof: This is just a combinatorial exercise on the above matrices, and we shall leave it for the reader to check. □

It might well be that not all of the above 8K three-valued maximal logics will be interesting as logics. Some of them, for instance, do not have symmetric matrices for the conjunction or for the disjunction (but notice that some such logics have had their use in results such as 3.21, 3.26, 3.35 and 3.57, or in 3.58), though any conjunction / disjunction is evidently equivalent to any other conjunction / disjunction (the negations of these conjunctions / disjunctions are what may differ). The fact that all the 8K three-valued logics do extend **Cia** (FACT 3.73) informs us, as a corollary to FACT 3.64, that:

FACT 3.74 (IpE) cannot hold in any of the 8K logics above.
Proof: Again, just recall THEOREM 3.51(iv). □

Now, if, in the next subsection, this failure of the replacement theorem will be seen to constitute a negative answer for the possibility of obtaining a Lindenbaum-Tarski-style algebraization for these logics (as already occurred for the calculi C_n and all of their extensions —see COROLLARY 3.65), the following result will help us to show in the following a positive answer for the possibility of obtaining a Blok-Pigozzi-like algebraization to each one of them (as already hinted for some extensions of C_n, such as C_1^+, see FACT 3.81 and FACT 3.82):

FACT 3.75 The following matrices of *classical negation* and *congruences* can be defined in each one of the above 8K logics:

	~
1	0
½	0
0	1

≡	**1**	½	**0**
1	1	0	0
½	0	½ or 1	0
0	0	0	1

Proof: To define the classical negation ~ one just has first to define $\perp$ either as $(B\wedge(\neg B\wedge\circ B))$ or as $(\circ B\wedge\neg\circ B)$, for some formula B, and then define $\sim A$ either as $(\neg A\wedge\circ A)$ or as $(A\to\perp)$. To define one of the above congruences one just has to set $(A\equiv B)$ as $((A\leftrightarrow B)\wedge(\circ A\leftrightarrow\circ B))$. If one wants to make sure that $v(A\equiv B)=1$ when both $v(A)=½$ and $v(B)=½$, this is also possible: just set some $(A\overset{2}{\equiv}B)$ as $\sim\sim(A\equiv B)$. □

In fact, it is not difficult to see that the above classical negation is indeed the *one and only* matrix of a strong negation that can be defined inside of these 8K three-valued logics (the paper [42] will also come back to this question). The reader will notice that this negation is indeed, in a sense, a 'highly' classical one. Indeed, it comes as a corollary, for instance, that:

FACT 3.76 The schema $\circ\sim A$ is provable in all of the above 8K three-valued logics.

This last result is more than what one needs to confirm, by way of THEOREM 3.51(i), the fact that (IpE) cannot hold in these logics (as in FACT 3.74).

A noteworthy expressibility result that can be proved for these 8K three-valued logics is the following:

FACT 3.77 (i) The matrices of $\mathbf{P}^1$ can be defined inside of any of the 8K three-valued logics above. (ii) All the matrices of all the 8K logics above can be defined inside of **LFI1**.

Proof: To check part (i), let $\wedge$, $\vee$, $\to$, $\neg$, $\circ$ and $\bullet$ be the connectives of any of the 8K logics above, and let ~ be the classical negation, defined inside this logic as in FACT 3.75. Then, the $\mathbf{P}^1$'s negation of a formula A can be defined as $\sim\sim\neg A$, the $\mathbf{P}^1$'s conjunction of some given formulas A and B, in this order, can be defined either as $\sim\sim(A\wedge B)$ or as $(\sim\sim A\wedge\sim\sim B)$, and the same we did for conjunction applies to both disjunction and implication, *mutatis mutandis*. The matrices for the connectives $\circ$ and $\bullet$ already coincide in all of these logics. Part (ii) is a particular consequence of the expressibility result that we have proven in [44], Theorem 3.6. In that result we showed, in fact, that the matrices definable in **LFI1** are all those, and exactly those, n-ary matrices that have classical (1 or 0) outputs for classical inputs (and that can have any output value if non-classical inputs are considered). Of course, all the above matrices, on these 8K three-valued logics, are, by definition, just 1-ary and 2-ary examples of such **LFI1**-definable *hyper-classical* matrices, as we have called them. □

COROLLARY 3.78 (i) The logic $\mathbf{P}^1$ can be conservatively translated inside any of the 8K three-valued logics above. (ii) Any of the 8K logics above can be conservatively translated inside of **LFI1**.

Are there other interpretations, besides *maximality*, of da Costa's requisite **dC[iv]** leading to yet some other classes of solutions to the problem of finding **C**-systems containing 'most rules and schemas of classical logic'? Are there non-many-valued (monotonic) solutions to that problem, or perhaps some other n-valued ones, for $n>3$? And, this is an important first step and probably an easier problem to solve, are there other interesting **C**-systems based on classical logic which are *not* extended by any of the above 8K three-valued logics? We must leave these questions open at this stage. It is interesting to notice, at any rate, that this problem has already been addressed here and there, in the literature. Besides [78], from which we drew the results in the subsection **3.12**, one could also recall, for instance, the adaptive programme for the confection of paraconsistent logics aiming to represent (non-monotonically) the dynamics of scientific reasoning and of argumentation (see [15]). Roughly speaking, the basic idea behind adaptive logics is that of working in between two boundary logics, classical logic often being one of them and a paraconsistent logic being the other one, so that consistency is pressuposed by default and we try to keep on reasoning (i.e. making inferences) inside of classical logic up to the point in which an 'abnormality' (an inconsistency?) pops out, a situation in which we had better descend to the level of the complementary paraconsistent logic, and go on reasoning over there. Indeed, the ancestral motivations of this programme (see [10]) seem to have been, as it is reasonable to conceive, yet another attempt to originate logics maintaining as much of classical logic as possible, so that paraconsistency will only be needed at limit cases.

As the reader will see in [42], when we go on to provide possible-translations semantics (as in [36], [39] or [76]) to some of the above non-three-valued logics, as for instance **Ci** or **Ciboe**, the intuition behind the construction of the previous three-valued paraconsistent logics can be pushed much farther, since it can be shown that some infinite-valued logics can also be *split* in terms of suitable combinations of clusters of three-valued logics.

3.12 Algebraic stuff. You may think, perhaps, that logic has 'too many formulas'. There is nothing unreasonable in supposing, however, that some of these formulas can in fact be identified, and indistinctly used in all contexts. If we consider classical logic, for instance, we will promptly see that there is no reason to distinguish between any two given theorems (or top particles) with respect to their relation to the other formulas of the classical language —even though they may very well still be understood as 'expressing' quite different facts, somehow conveying different bits of information. In the classical case, also, and for the very same reason, one does not really need to distinguish between any two given bottom particles (or two different pairs of contradictory formulas); even more than that, any two formulas A and B which turn out to be provably equivalent (that is, such that $[A \dashv\vdash B]$, or, what in many logics amounts to be just the same, to put it in terms of a bi-implication, such that $[\vdash (A \leftrightarrow B)]$) are, in a certain sense, indistinguishable, and can be indistinctly employed in the same contexts, to attend similar purposes. The action of putting the glasses through which some 'contingent' properties of formulas are hidden and only those features related to their general behavior in relation to other formulas are exposed is the task of *algebraization*. Whenever a given logic turns out to be algebraizable, so that the logical problems can be faithfully and conservatively translated into some given well behaved algebra, then it will be possible for us to use the powerful (universal) algebraic tools to tackle those problems, so that, in the next and final step, we will be able to translate the results back into logic.

Being the above remarks all too informal, we had better strive to put them in more precise terms. If the whole activity of mathematics and logic involves 'forgetting' some things (and calling attention to others), and identifying what could otherwise, at the first look, have seemed just different, their tools by excellence, in such respect, are the key notions of *equivalence*, *congruence*, *isomorphism*, and so on. Once the very definition of a logic, as we have proposed it at the start of section **2**, can immediately be seen as some sort of algebra having the set of formulas as its domain (indeed, in the structural and propositional case, it is exactly the free algebra generated by the primitive connectives of the languages —here understood as operators— over the set of atomic propositions), the quest for dividing these formulas into disjunct packages of equivalent and indiscernible ones can easily be accomplished if one is able to define a *congruence* relation over these formulas, that is, an *equivalence* (reflexive, symmetric and transitive) relation such that any two equivalent formulas (with respect to this relation) can be just justifiably and indistinctly used in all and the same contexts. So, if some given formula A appears as a component of some other formula G, then any formula B which is congruent to A should be able to do the same job, with no loss or increase in expressibility or generality. What we implicitly mean with this is that the new *quotient* algebra obtained by dividing the original algebra of formulas by this congruence relation should preserve the original 'operations', existing thus an obvious homomorphism from the original algebra into the quotient algebra. So, in dividing the formulas this way into classes of congruent ones, one can go on to work and dialogue with just the (arbitrary) representants of these classes, once they are supposed to behave exactly the same as any of their congruent colleagues with respect to any operation of (any isomorph of) the quotient algebra. Any two congruent formulas are 'the same up to a congruence', and can play exactly the same roles in some specific dramas.

It comes perhaps as no surprise the confirmation that the most easy and standard way of algebraizing a given logic is obtained by way of the relation of provable equivalence induced by its underlying consequence relation. Indeed, the so-called *Lindenbaum-Tarski algebraization* sets two formulas A and B as congruent if $A \dashv\vdash B$ —let's denote this fact by writing $A \approx B$. Such 'congruence' relation $\approx$ is evidently an equivalence relation, and to confirm that any two so congruent formulas 'work the same in all contexts', one has to check if they can be intersubstituted everywhere, that is, one has to prove a *replacement* theorem, to the effect that the *intersubstitutivity of provable equivalents*, (IpE), holds (recall its definition in the subsection **2.3**, and check [111]). Many logics have Lindenbaum-Tarski-like algebraizations, as it is the case for classical logic, intuitionistic logic, several normal modal logics, several many-valued logics, and so on. But not all algebraizable logics are algebraizable in the sense of Lindenbaum-Tarski, not being able for instance to prove replacement with respect to provable equivalence (or provable bi-implication). In the case of many non-normal modal logics, for example, what one needs is *strict* (that is, *necessary*) provable equivalence (that is, strict bi-implication). In the case of the paraconsistent logics studied here, frequent negative results on what concerns the validity of (IpE) —and so, on what concerns the possibility of obtaining an algebraization *à la* Lindenbaum-Tarski— have been met: In fact, *all* of the above **C**-systems have been shown at some point to lack (IpE) (recall the results 3.22, 3.35, 3.58, 3.65, and 3.74). Yet, the possibility of obtaining some positive results within some extensions of those **C**-systems was not ruled out (recall 3.53, but confront it with 3.51).

An immediate result about algebraizations that may come quite handy is the following one (cf. [25], Corollary 4.9):

FACT 3.79 Every deductive extension of an algebraizable logic is algebraizable.

A case study which was particularly well investigated is that of the logic **Cila** (the logic C_1 of da Costa's [49] —check the subsection **3.10**). Even though at least as early as in da Costa & Guillaume's [54] it had already been noticed that (IpE) does not hold for **Cila** (COROLLARY 3.65), so that no Lindenbaum-Tarski-like algebraization for this logic (or for any other of the weaker calculi C_n) can be available, several attempts have been made to find other kinds of algebraizations for this logic (check, for instance, da Costa's [48]). The intuitive idea underlying the search of other algebraizations, generalizing the idea of Lindenbaum-Tarski, has been quite often that of finding 'any' congruence on the set of formulas that could be used to produce a quotient algebra from the algebra of formulas of the logic. Furthermore, if such a congruence is no more necessarily supposed to be induced directly by way of the consequence relation associated to the logic, nor should this congruence be necessarily supposed to be expressible by way of a formula written in the very language of the logic (it may happen to be definable only metalinguistically —for instance, if you do need a metalinguistical 'and' to characterize it, but there is no adequate conjunction available to express it in the language of the logic), it is still reasonable to suppose as well that this congruence should put no distinguished and non-distinguished formulas inside the same class of equivalence (so, for instance, no class will simultaneously contain a theorem and a non-theorem), so that we will have no trouble in attributing a distinguished or a non-distinguished status to some class of equivalence (cf. [84]). The final blow to the search for congruences algebraizing the logic **Cila** was delivered by Mortensen's [82], where this author proved that:

THEOREM 3.80 No non-trivial quotient algebra is definable for **Cila**, or for any logic weaker than **Cila**.

It is never too late to remember that, for non-trivial logics, a *trivial quotient algebra* is an algebra defined by a congruence relation $\approx$ such that $A \approx B$ if, and only if, A and B are the same formula (so, all equivalence classes are singletons). Now, some authors have argued that the exclusive existence of trivial quotient relations for a given logic is a major 'defect' (cf. [84], section 3), while others do not think so (cf. [20]) —and this is the reason why we have used scare quotes in writing ''any' congruence', above. In any case, this last result can be easily remedied by extensions of **Cila**. Consider, for instance, the logic **Cilo** (the logic C_1^+ of da Costa, Béziau & Bueno's [57] —check again the subsection **3.10**). A non-trivial congruence can be defined within this logic by requiring, for any two given formulas, that they are not only provably equivalent, but are also both provably consistent. This can be put in terms of a single formula, by defining $A \approx B$ if $\vdash ((A \leftrightarrow B) \wedge (\circ A \wedge \circ B))$. One can then immediately prove that:

FACT 3.81 There is a non-trivial quotient algebra for **Cilo** (and already for **Cio**).
Proof: The above defined connective $\approx$ clearly sets up an equivalence relation. We have to show that it is in fact a congruence, so that given a schema $G(A)$ depending on A as a component formula (and possibly on some other formulas as well), we have to show that $G(A) \approx G(B)$ whenever $A \approx B$, where $G(B)$ is obtained by replacing each occurrence of A in $G(A)$ by B. Now, given this supposition that $A \approx B$, and recalling

from THEOREM 3.67 that the consistency of any component of a complex formula, in **Cio**, is enough to guarantee the consistency of the complex formula itself, we may infer that $\vdash \circ G(A)$ and $\vdash \circ G(B)$. To check that $\vdash (G(A) \leftrightarrow G(B))$ just do a straightforward induction on the complexity of G. In the trivial case in which no other connectives or formulas intervene, but A, there is really nothing to prove. The case of conjunction, disjunction and implication is also immediate, from positive logic. For negation, just recall, as a consequence of FACT 3.17, that contraposition holds for provably consistent formulas, so that from $A \dashv\vdash B$ and both $\vdash \circ A$ and $\vdash \circ B$ one can infer $\neg A \dashv\vdash \neg B$. This concludes the proof (a similar semantical argument can already be found in [57], Theorem 3.21), and it is obvious that this congruence is non-trivial —we know for example from FACT 3.66 that $\circ\bot$ is a theorem of **Cila**, and thus of **Cilo**, so that all bottom particles will of course belong to the same equivalence class determined by $\approx$ over **Cilo**. In all other respects, except for this last particular example, the above proof is clearly valid not only in **Cilo** but also in **Cio** (that is, **Cilo** without the axiom (cl) that transforms this last **C**-system, **Cio**, into a **dC**-system). □

Various other extensions of **Cila** having non-trivial quotient algebras have been proposed in the literature. In [84], for instance, Mortensen has proposed an infinite number of them, all situated of course somewhere in between **Cila** and classical logic. They were called $C_{n/(n+1)}$, for $n>0$ (C_0 is the name traditionally reserved for classical logic), and axiomatized by the addition to **Cila** of the following axioms, for each fixed $n>0$:

(M1n) $\neg^{n-1}A \vdash \neg^{n+1}A$, where $\neg^{n}$, as usual, denotes n iterations of $\neg$;

(M2n) $\bigwedge_{i=1}^{n}(\neg^{i-1}A \leftrightarrow \neg^{i-1}B) \vdash \bigwedge_{i=1}^{n}(\neg^{i}(A\#C) \leftrightarrow \neg^{i}(B\#C)) \wedge \bigwedge_{i=1}^{n}(\neg^{i}(C\#A) \leftrightarrow \neg^{i}(C\#B))$, where # is any binary connective, and $\bigwedge$ abbreviates, as usual, a long conjunction.

In the section 4 of [84] the connective $\approx$ defined by letting $A \approx B$ hold whenever $\vdash \bigwedge_{i=0}^{n}(\neg^{i}A \leftrightarrow \neg^{i}B)$ is shown to constitute a non-trivial congruence, for each $n>0$. The reason for non-triviality is that, in general, each $C_{n/(n+1)}$ can be understood as providing us with n+1 'negations': for any formula A of this logic we have that $\neg^{m-1}A$ is congruent to $\neg^{m+1}A$ if, and only if, $m \geq n$, so that there are n+2 distinct equivalent classes (represented by A, $\neg A$, …, $\neg^{n+1}A$) of the quotient algebra generated by $\approx$. Do any of these new **C**-systems coincide with any of the other above studied ones? Do they have any special interest in themselves (besides being equipped with a non-trivial congruence)?

How can one understand these more general algebras induced by more esoteric congruence relations, if they do not fit inside the 'classical' algebraization theory of Lindenbaum-Tarski? A neat and elegant solution to that can be found in the study of Blok & Pigozzi (cf. [25]), where a much more general theory of algebraization is developed, extending the work of other authors. Some terminology and definitions are needed to explain what is a *Blok-Pigozzi algebraization*. Fixing some logic $\mathbf{L} = \langle For, \Vdash \rangle$, an **L**-*algebra* is any structure homomorphic to **L** (being *For* a structured set of formulas constructed over some set of connectives, the corresponding **L**-algebra will of course contain, for each connective, an operator of the same arity 'interpreting' it). An **L**-*matrix model* of an **L**-algebra **Alg** is any pair <**Alg**, **D**>, where **D** is a proper subset of the universe of **Alg**, of the so-called *distinguished elements*. Formally, let an *interpretation* of a set of formulas *For* be an assignment of terms of **Alg** to each element of *For* (an assignment which is usually defined over some primitive elements and then extended to the whole set of formulas by way of the interpretation of the building structural operators). The *semantic consequence relation* $\vDash_{\mathbf{M}}$ associated to an **L**-matrix model **M** is then defined, as usual, by setting $\Gamma \vDash_{\mathbf{M}} A$ whenever A is assigned a distinguished element for every assignment of distinguished elements to all members of Γ. Matrices of finite many-valued logics are simple practical examples of *sound* and *complete* matrix models (that is, models such that $[(\Gamma \Vdash A) \Leftrightarrow (\Gamma \vDash_{\mathbf{M}} A)]$) that can be associated to some logics. In general, by a result of Wójcicki (see [111]) it is known that every structural logic can be characterized by sound and complete matrix models, in fact by κ-valued matrices, where κ has at most the cardinality of the set of formulas of the logic.

Any pair of terms φ and ψ of the **L**-algebra will be said to constitute an *equation*, to be designated by writing $(\varphi \doteq \psi)$. Such equations are always schematic, as any usual mathematical equation, and their non-operational components are said to be its *variables*; we may accordingly write $\varphi(C) \doteq \psi(C)$ to designate an equation having C as its single variable, and similarly for any number of variables. Now, what an interpretation does is exactly assigning values to these variables. One may then define an *equational consequence relation* induced by a class of **L**-algebras **KA**, to be denoted as $\vDash_{\mathbf{KA}}$, as follows: $[\Gamma \vDash_{\mathbf{KA}} (\varphi \doteq \psi)]$, where Γ is a set of equations, whenever the equation $(\varphi \doteq \psi)$ is a semantic consequence of Γ for every **L**-matrix model **M** of each **L**-algebra in **KA**, that is, when all those matrix models are such that $[\Gamma \vDash_{\mathbf{M}} (\varphi \doteq \psi)]$. The relation $\vDash_{\mathbf{KA}}$ is said to constitute an adequate *algebraic semantics* for a given logic **L** whenever there is a finite set of equations $\delta_i(C) \doteq \varepsilon_i(C)$, for $i<n$, such that: $(\Gamma \Vdash A) \Leftrightarrow [\{\delta_i(B) \doteq \varepsilon_i(B)$: for all $i<n$ and all $B \in \Gamma\} \vDash_{\mathbf{KA}} (\delta_i(A) \doteq \varepsilon_i(A))]$. In this case, the equations $\delta_i(C) \doteq \varepsilon_i(C)$, for $i<n$, are called *defining equations* of **L**, and we shall write simply $\delta \doteq \varepsilon$ as an abbreviation of them. Finally, an algebraic semantics for a logic **L**, induced by a class of **L**-algebras **KA**, is said to be *equivalent* (or *congruential*) if there can be defined in **L** a finite set of connectives with two variables $\approx_j$, for $j<m$, such that, for every equation $\varphi \doteq \psi$, we have that $[\{(\varphi \approx_j \psi)$: for all $j<m\}$ $\dashv\vDash_{\mathbf{KA}} \{\delta(\varphi \doteq \psi) \approx_j \varepsilon(\varphi \doteq \psi)$: for all $j<m\}]$. This set of connectives $\approx_j$, for $j<m$, will be abbreviated simply as $\approx$ and called a *system of equivalence* (or *congruence*) connectives for **L** and **KA**. Now, a logic **L** is said to be (Blok-Pigozzi-)*algebraizable* if it has an equivalent algebraic semantics. Another way of stating this definition (in terms of the consequence relation of **L**) is by requiring, to call a logic **L** algebraizable, to have in hand a set of equations $\delta \doteq \varepsilon$ and a set of formulas $\approx$ such that: (i) $\approx$ constitutes an equivalence relation; (ii) $(A_1 \approx B_1), \ldots, (A_n \approx B_n) \Vdash \sigma(A_1, \ldots, A_n) \approx \sigma(B_1, \ldots, B_n)$, for each n-ary connective σ; and (iii) $A \dashv\Vdash \delta(A) \approx \varepsilon(A)$. It should by now be completely clear how this generalizes the idea of (proving (IpE) and) producing a congruence over a set of formulas.

Not all logics are algebraizable (even in this broader sense of Blok-Pigozzi). For example, most modal logics, and the system **E** of entailment are not algebraizable, though they do have non-congruential algebraic semantics. As to the **C**-systems that we study in this paper, it has already been shown or mentioned some lines above (FACT 3.81 and below), that the logics **Cilo** and $C_{n/(n+1)}$, extensions of **Cila**, do have non-trivial congruences defined by finite sets of equations, being thus algebraizable in the sense of Blok-Pigozzi (though they are not algebraizable in the traditional sense of Lindenbaum-Tarski).

Also, as hinted in the last subsection, one can now prove that all the 8K three-valued maximal paraconsistent logics there presented are algebraizable (making use of and extending an argument by Lewin, Mikenberg & Schwarze, who have proved in [71] that the three-valued logic $\mathbf{P}^1$ is algebraizable):

FACT 3.82 All the 8K three-valued logics from the last subsection are algebraizable.
Proof: Just consider any of the two connectives $\equiv$ or $\overset{2}{\equiv}$ defined in the FACT 3.75, let $\delta(A)$ be defined as $((A\to A)\to A)$ and $\varepsilon(A)$ be defined as $(A\to A)$, and check that the conditions (i)–(iii) defining an algebraizable logic two paragraphs above do hold. □

It can also be shown, at this point, that some of our **C**-systems are not algebraizable. To such an intent, yet another characterization of algebraizable logics can come on handy. Let **L** be a logic and **M** be an **L**-matrix model. A *Leibniz operator* Λ is a mapping from each arbitrary subset S of **M** into the largest congruence $\approx$ of **M** compatible with S, where $\approx$ is *compatible* with S if whenever we have that $\varphi\in S$ and $\varphi\approx\psi$ we also have that $\psi\in S$. It can be proved that a logic **L** is algebraizable if, and only if: (iv) Λ is injective and (v) order-preserving on the collection CT(**L**) of all closed theories of **L**, (vi) Λ preserves unions of directed subsets of CT(**L**), where a subset of CT(**L**) is *directed* if there is a common upper limit to every finite collection of elements of CT(**L**). (At this point, we had better direct the reader to [25] for details and proofs.) In any case, one might observe that a consequence of these last observations is that, for every logic **L**, the Leibniz operator produces an isomorphism between the lattice of filters of each **L**-matrix model **M** and the lattice of congruences of **M**. So, if such an operator is not an isomorphism, for some **L**-matrix model **M**, then the logic **L** is not algebraizable. This was the idea used by Lewin, Mikenberg & Schwarze in [72] (and that we extend here) to refine THEOREM 3.80:

THEOREM 3.83 The logic **Cila** (that is, da Costa's C_1) is not algebraizable. The same holds even for the stronger logic **Cibaw** (see **Figure 3.1**, in the subsection **3.10**), or any weaker logics extended by **Cibaw**.
Proof: Consider the following set of truth-values, $\mathbf{V}=\{0, a, b, 1, u\}$, ordered as follows: $0\le a$, $0\le b$, $a\le 1$, $b\le 1$, $1\le u$, and where u and 1 are the distinguished elements. Consider now the following matrices defined over them:

$\wedge$	u	**1**	a	b	**0**
u	u	1	a	b	0
1	1	1	a	b	0
a	a	a	a	0	0
b	b	b	0	b	0
0	0	0	0	0	0

$\vee$	u	**1**	a	b	**0**
u	u	u	u	u	u
1	u	1	1	1	1
a	u	1	a	1	a
b	u	1	1	b	b
0	u	1	a	b	0

$\to$	u	**1**	a	b	**0**
u	u	u	a	b	0
1	u	1	a	b	0
a	u	1	1	b	b
b	u	1	a	1	a
0	u	1	1	1	1

	$\neg$	$\circ$
u	1	0
1	0	1
a	b	1
b	a	1
0	1	1

All axioms of **Cibaw** are validated by these matrices, as the reader can easily check. Now, it is also easy to check that there are no non-trivial congruences over **V**. Suppose for instance that $u \approx x$, for some $x \neq u$. In this case, as we know that $\neg\neg u \doteq 0$, and $\neg\neg x \doteq x$, then the condition (ii) above will give us $\neg\neg u \approx \neg\neg x$ from $u \approx x$, and so we conclude that $0 \approx x$, and thus $0 \approx u$. But, as we have observed before, there can be no congruence class containing both distinguished and non-distinguished values (in any case, this will violate condition (iii) above). We leave to the reader the easy exercise of showing, using the above connectives, that for any $x \approx y$, with $x \neq y$, one gets trapped at a similar predicament, namely, that of a distinguished value getting grouped with a non-distinguished one inside the same congruence class. Now, it is clear that $<A, \wedge, \vee>$ is a lattice, and that $\{0, a, 1, u\}$ and $\{0, b, 1, u\}$ are two filters over **V**. But there is just one congruence over **V** (which is of course the largest one compatible with both the filters just mentioned), and so the Leibniz operator cannot be an isomorphism. Once the logic **Cibaw** is, as a consequence, not algebraizable, FACT 3.79 informs us that none of its fragments can be algebraizable. □

Now, even non-algebraizable logics can happen to be amenable to sensible algebraic investigation. Indeed, a class of *weakly algebraizable logics* is characterized by the validity of conditions (iv) and (v) above, two of the three clauses of the characterization of Blok-Pigozzi algebraizability in terms of the Leibniz operator, and condition (v), alone, defines the class of *proto-algebraizable* logics. This last class includes all normal modal logics and most non-normal ones, but there are still some other logics which are not protoalgebraizable: an example is **IPC***, the implicationless fragment of intuitionistic logic, is neither algebraizable nor protoalgebraizable (cf. [25], chapter 5). Which of our non-algebraizable **C**-systems are protoalgebraizable, and which not (if any)? We shall leave this question open at this stage. It is interesting to notice, at any rate, that some sort of algebraic counterparts to some of these non-algebraizable **C**-systems have been proposed and studied, for instance, in Carnielli & de Alcantara's [37] and Seoane & de Alcantara's [102], where a variety of 'da Costa algebras' for the logic **Cila** has been introduced and studied, and a Stone-like representation theorem was proved, to the effect that every da Costa algebra is isomorphic to a 'paraconsistent algebra of sets'. It would be interesting now not only to extend that approach to other **C**-systems, but also to check how it fits inside this more general picture given by (Blok-Pigozzi-)algebraizable and protoalgebraizable logics.

An interesting application of the above mentioned algebraic tools is the following. Consider again, for example, the FACT 2.10(ii), where strong negations were shown to be 'definable' from bottom particles. Now, it is completely clear how this definition can be stated in practice if, for instance, a suitable implication is available inside of a compact logic (this is the case in all our examples, but needs not to be). This illustrates in fact how intuitionistic negation may be defined from a bottom particle and intuitionistic implication. But is that *definition*, in the general case, an *implicit* or an *explicit* one? For example, in positive classical logic (plus bottom and top) the theory containing both the formulas $((A \wedge B) \leftrightarrow \bot)$ and $((A \vee B) \leftrightarrow \top)$ implicitly defines the formula B (as the 'classical negation of A'). Are all implicit definitions also explicit ones? Or do we have, in some cases, to explicitly add some more structure to a logic to make explicit definitions expressible even when implicit ones are available? If, whenever a logic can implicitly define something, it can also explicitly define it, then the logic is said to have the *Beth definability property*, (BDP). Now, consider any class of **L**-algebras **KA**, for some logic **L**, and pick up a set **HA** of homomorphisms between any two of these **L**-algebras. A homomorphism $f\colon \mathbf{Alg}_1 \to \mathbf{Alg}_2$ in **HA** is said to be an *epi* if every pair of homomorphisms $g, h\colon \mathbf{Alg}_2 \to \mathbf{Alg}_3$ in **HA** is such that $g \circ f = h \circ f$ only if $g = h$. Evidently, all surjective homomorphisms are epis; if the converse also holds, that is, if all epis are surjective, we say that **KA** has the property (ES). Now, by a result of I. Németi (cf. [4]), an algebraizable logic has (BDP) if, and only if, its class of algebras has (ES). This is a very interesting result, and constitutes, in fact, just one example of how algebraic approaches can help us to solve real logical problems, in this case the problem of definability. Extensions of such results to wider classes of algebraic structures associated to (wider classes of) logics are clearly desirable.

4 FUTUROLOGY OF **C**-SYSTEMS

> When you encounter difficulties and contradictions, do not try to break them, but bend them with gentleness and time.
> —Saint Francis de Sales.

This is *not* the end. The next and natural small step for a paper, giant leap for paraconsistency, is providing reasonable interpretations for **C**-systems. This is the theme of our [42], where semantics for **C**-systems are presented and surveyed, ranging from the already traditional *bivaluations* to the more recently proposed *possible-translations semantics*, traversing on the way a few connections to many-valued semantics (a theme that already intromitted in our subsection **3.11**), and to modal semantics. A quite diverse approach to paraconsistent logics (in general) from the semantical point of view is also soon to be found in Priest's [91] (the remarkable possible-translations semantics, according to which some complex logics are to be understood in terms of *combinations* of simpler ones, will nevertheless not be found there —see instead [76], [36], [39], and, of course, [42]—, in addition, its section on many-valued logics is unfortunately too poor to give a reasonably good idea on the topic).

In this last section of the present paper we want to point out some interesting open problems and research directions connected to what we have herein presented. For example, in the section **2** we have extensively investigated the abstract foundations of paraconsistent logic, and the possibility and interest of defining the so-called *logical principles* at a purely logical level. There is still a lot of stirring open space to work here, and we will feel happy to have stimulated the reader to try their own hand at the relations between all the alternative formulations of (PPS), that is, all the different forms of explosion (or, if they prefer, the various forms of *reductio*, as in the subsection **3.2** —recall that the *reductio* and the Pseudo-Scotus are not always equivalent, for instance, if you think about intuitionistic logics). Think about it: are there any *interesting* logics, in our sense, disrespecting the Pseudo-Scotus, while respecting *ex falso* or the Supplementing Principle of Explosion, but still disrespecting the Gentle Principle of Explosion as well? In other words, are there interesting paraconsistent logics having either bottom particles or strong negations which do not constitute **LFI**s?

Recall that our approach contributed a novel notion of *consistency*. This is the picture again: There are consistent and inconsistent logics. The inconsistent ones may be either paraconsistent or trivial, but not both. The paraconsistent ones may be either dialectical or not. The consistent logics are explosive and non-trivial. The paraconsistent logics are non-explosive, and the dialectical paraconsistent ones are contradictory as well. The trivial logics (or trivial logic, if you fix some language) are explosive and contradictory (if the underlying logic has a negation symbol). Negationless logics are trivial if and only if inconsistent. Let us say that a theory *has models* only if these are non-trivial (they do not assign distinguished values to all formulas). So, the theories of a consistent logic have models if and only if they are non-contradictory. Paraconsistent logics may have models for some of its contradictory theories, and in the dialectical case all models of all theories are contradictory. Trivial theories (of trivial logics, or of any other logics) have no models. The consistency of each formula A of a logic **L** is defined exactly as what else one must say about A in order to make it explosive, that is, as what one should add to an A-contradictory theory in order to make it trivial. If the answer is 'nothing', then A is already consistent in **L** (whether the theories that derive this formula are contradictory or not). So, consistent logics are, quite naturally, those logics that have only consistent formulas. The above study sharply distinguishes the notions of non-contradictoriness and of consistency, and the model-theoretic impact of this should obviously be better appraised!

We now also have a precise definition of a large and fascinating class of paraconsistent logics, the *logics of formal inconsistency*, **LFI**s, and an important subclass of that, the **C**-*systems*. This is important to stress: according to our proposal it should be no more the case that the **C**-systems will be identified simply with the calculi C_n of da Costa, or with some other logics which just happen to be axiomatized in a more or less similar way. A general idea was put forward to be explored, namely that of being able to express *consistency* inside of our paraconsistent logics, and this helped collecting inside one big single class logics as diverse as the C_n and $\mathbf{P}^1$, or even $\mathbf{J}_3$ (now rephrased as **LFI1**), whose close kinship to the C_n seemed to have passed unsuspected until very recently (recall the subsections **2.4**, and end of **3.11**). This is, we may suggest, a fascinating challenge that we propose to our readers: To show that many other logics in the literature on paraconsistent logics can be characterized as **C**-systems, or, in general, as **LFI**s. This exercise has been explicitly put forward in the subsection **2.6**, but even previous to that, in the end of the subsection **2.4**, we have already hinted to the fact that other logics, such as Jaśkowski's **D2**, a discussive paraconsistent logic with motivations and technical features completely different from the ones that we study here, could be recast as an **LFI** (based on the modal logic $S5$) —more precisely, it can be recast as an **LFI** if only it is presented having some necessity operator, $\Box$, among its primitive or definable connectives (see note 11). Another recent example of that is the paraconsistent logic **Z** (are we perhaps running out of names?) proposed by Béziau in [24], in which a paraconsistent negation $\neg$ is defined from a primitive classical negation $\sim$ and a possibility operator $\Diamond$, by setting $\neg A \stackrel{\text{def}}{=} \Diamond{\sim}A$.[27] Again, it is easy to see that **Z** can also be seen as an **LFI** (in fact, a

[27] By the way, exactly the same logic was proposed by Batens in [16] under the name **A**, and appears on another of Béziau's paper, [23], section 2.8, under the appelation of 'Molière's logic'. Strangely enough, after longly attacking, in the section 2.5 of [23], those logics that he calls 'paraconsistent atomical logics', that is, those logics in which 'only atomic formulas have a paraconsistent behavior' (being thus controllably explosive with respect to every complex, or 'molecular' formula), a class of logics

C-system based on $S5$), in which the consistency of a formula A is expressed by the formula $(\Box A \vee {\sim}A)$. Which other paraconsistent logics constitute **C**-systems, or **LFI**s, and which *not*? Inverting the question, are there good reasons for one trying to *avoid* **LFI**s, that is, can the investigation of non-**LFI**s have good technical or philosophical justifications? And how would the **C**-systems based on intuitionistic logic (**I**-systems?) or on relevance logic (**R**-systems?) look like, and which interesting properties would they have? How would this improve our map and understanding of **C**-land? In general, how could one use the very idea of a **C**-system to build up some new interesting paraconsistent logics, what advantages would they bring and particular technical tools could they contribute to the general inquiry about **LFI**s? The point to insist here is on the remarkable *unification* of aims and techniques that **LFI**s can seemingly produce in the paraconsistency terrain!

Another related interesting route is the one of *upgrading* any given paraconsistent logic in order to turn it into an **LFI**. This is exactly what is done by the logic **LFI1** (or **CLuNs**, or $\mathbf{J}_3$) over the logics *Pac* and *LP* (see subsection **2.4**), for which the gain in expressive power should already be obvious to the reader. Now, consider, for instance, the three-valued *closed set logic* studied by Mortensen and collaborators in [85], whose matrices of conjunction and of disjunction coincide with those of **LFI1**, and whose matrix of negation coincides with that of $\mathbf{P}^1$, having, again, 0 as the only non-distinguished value. Now, it is easy to see that the addition of appropriate matrices of implication and of a consistency operator will turn the upgraded closed-set logic into one of our 8K three-valued maximal paraconsistent logics, discussed in the subsection **3.11** above. The motivation for such closed set logic is also to be found among some of the most striking features of the 'Brazilian approach' to paraconsistency, namely, the idea of studying paraconsistent logics which are in a sense *dual* to other *broadly intuitionistic* (also called *paracomplete*) logics. We have slightly touched on this issue at a few points above —see, for instance, subsections **3.1** and **3.2**— as this has been one of the preferred justifications used by da Costa, among other authors, for the constitution of many of his paraconsistent logics. Indeed, if classical logic is not rarely held by some authors as the 'logic of sets', particularly because of its Boolean algebraic counterpart, Heyting's Intuitionistic Logic is very naturally held, in a topological setting, as the 'logic of open sets'. The very same dualizing intuition that we have just mentioned can then lead one to study the 'logic of closed sets' as a very natural paraconsistent logic. This is done in [85], where this investigation is also lifted to the categorial space —again, if intuitionistic logic is very naturally thought of as the logic of a *topos*, the closed set logic can be thought of as the underlying logic of a categorial structure called *complemented topos*. The upgrade of the closed set logic into an **LFI** may thus set up some interesting new space for the study

that seems to comprise not many logics up to this moment (the logic $\mathbf{P}^1$ —recall THEOREM 3.15(ii) and THEOREM 3.69— being among them), Béziau presents the above mentioned logic **Z** as the logic enjoying 'the best paraconsistent negation' around. But even if one concedes an enlargement of this definition of atomical logics in order to comprise all paraconsistent logics which are only 'paraconsistent up to some level of complexity', this author would still have to deal with the *bourgeois* fact that the negation of his preferred logic **Z**, exactly as what occurred with $\mathbf{P}^1$'s negation, is such that $\{p, \neg p\}$ is not trivial, for atomic p, while $\{\neg p, \neg\neg p\}$ *is* trivial: why, in this largely analogous case, would that same phenomenon be 'philosophically justifiable', and not be reducible just to some more bits of 'formal nonsense', as he puts it? One interesting such a justification, we suggest, may be found in terms of the dualization obtained by logics such as Mortensen's closed set logic, also mentioned in the present section.

of topological and categorial interpretations of the notion of consistency. This proposed duality has also often been pushed, in the literature, in the contrary direction, namely, into the study of paracomplete logics which are dual to some given paraconsistent logics. Some samples of this can be found, for instance, in our papers [39] (where a logic called D_{min} is presented as dual to C_{min}, mentioned above in the subsection **3.1**), and [43] (where logics dual to slightly stronger versions of the calculi C_n are studied), as well as in Marcos's [78] (where 1K three-valued maximal paracomplete logics are presented, in addition to the 8K paraconsistent ones above mentioned). A more thorough study of the **LFU**s, the *Logics of Formal Undecidability* (or *Logics of Incomplete Information*), following the standards of the investigation set up by the present paper rests yet to be done.

We of course do not, and cannot, claim to have included and studied above *all* 'interesting' **C**-systems based on classical logic. We cannot but offer here a very partial medium-altitude mapping of the region, but we strongly encourage the reader to help us expand our horizons. So, if, guided mostly by technical reasons, we have started our study from the basic logic **bC** (subsection **3.2**), constructing all the remaining **C**-systems as extensions of **bC**, this should *not* be a impediment for the reader to study still weaker and *more basic* logics, such as **mbC**, the logic axiomatized by deleting (Min11): $(\neg\neg A \to A)$ from the axiomatization of **bC**, and their extensions, **mCi** (a logic studied under the name *Ci* in [17]) and so on. So, if **bC** was presented as a quite natural conservative extension of the logic C_{min} ([39]), **mbC** can similarly be presented as an extension of the logic *PI* ([10]). Just keep in mind that starting your study from **mbC**, instead of **bC**, and avoiding the axiom (Min11), you will be allowing for the existence of a few more in principle uninteresting partially explosive paraconsistent extensions of your logics (THEOREM 3.13), and you may also lose a series of other results, as for instance 3.14(iii), half of 3.17 and of 3.26, and also 3.20, 3.36, 3.41, 3.51(iii), 3.56, 3.57, 3.66, as well as some derived results and comments. Notice that we do not say that the loss of some of these results cannot be positive, but some symmetry certainly seems to be lost if the logic **mbC** happens to be extendable, for instance, in such a way as to validate the schematic rule $[(A\to B) \vdash (\neg B\to\neg A)]$, though it can in no way be extended so as to validate the similar rule $[(\neg A\to B) \vdash (\neg B\to A)]$.

What effects can the **LFI**s have on the study of some general mathematical questions, such as *incompleteness results* in Arithmetic? Indeed, recall that Gödel's incompleteness theorems are based on the identification of 'consistency' and 'non-contradictoriness' —what then if we start from our present more general notion of consistency (see (D19), subsection **2.4**)? And what if we integrate these logics with such modal logics as the *logic of provability*? (See [27], where consistency is also intended as a kind of dual notion to provability —if you cannot prove the negation of a formula, it is consistent with what you can prove—, and in which the necessary environment for the study of Gödel's theorems is provided.) What effects could that have (if at all) on the investigation of (set-theoretical) paradoxes? In fact, the analogy of the logics of formal inconsistency with the logics of provability is rather striking and worthy of being further explored; connections with other powerful internalizing-metatheoretical-notions logics, such as *hybrid logics*, and *labelled deductive systems* in general, are also to be expected.

We are *not* trying to escape from da Costa's requisite **dC[iii]** (subsection **3.8**), according to which extensions of our logics to higher-order logics ought to be available. But it still seems that most interesting problems related to *paraconsistency* appear already at the propositional level! Moreover, more or less automatic processes to *first-ordify* some given propositional paraconsistent logics can be devised, by the use of combination techniques such as *fibring*, if only we choose the right abstraction level to express our logics (see [34], where the logic C_1 is given a first-order version —coinciding with the one it had originally received, in [49] or [50], but richer in expressibility power— by the use of the notion of *non-truth-functional rooms*). One interesting thing about first-order paraconsistent logics is that they might allow for inconsistencies at the level of its objects, opening a new panorama for ontological investigations. Another interesting thing that we can conceive about first-order versions of paraconsistent logics in general, and especially of first-order **LFI**s, and that seems to have been completely neglected in the literature up to this point, is the investigation of consistent yet ω-*inconsistent* structures, or theories (also related to Gödel's theorems). Again, a point about that is scored by the logics of provability, but studious of paraconsistency should definitely have something to say about this.

Let us further mention some more palpable specific points to which we have already drawn attention here and there, and on which more research is still to be done. For instance, is the logic **bC** (subsections **3.2** to **3.4**) *controllably explosive*? Recall from FACT 3.32 that in the case of its extension **Ci**, the formulas causing controllable explosion coincide with the consistent theorems, and that, as it happens, **bC** does not have consistent theorems (THEOREM 3.10). For **bC**, however, only one side becomes immediate: consistent theorems cause controllable explosion… Another question: Does this logic **bC** have an intuitive adequate modal interpretation? And are there also extensions of **Ci** in which (IpE) holds (see the end of the subsection **3.7**)? What about investigating some other extensions of **bC** which do not extend **Ci** as well, such as **bCe**, obtained by the direct addition to **bC** of the axiom (ce): $A \vdash \neg\neg A$, studied in subsection **3.9**? Remember that we have extended **bC** to **Ci**, in the subsection **3.5**, arguing that all paraconsistent logics in the literature *do* identify inconsistencies and contradictions, and then all the other logics that we have studied after that in fact extended **Ci**. But the logic **bCe** also seems interesting in its own right, being able to accept some simple extensions that can express dual inconsistency, differently of what had occurred in the subsection **3.4** with some other extensions of **bC**, and paralleling a result obtainable for **Ci** (subsection **3.6**). **bCe** would also presumably constitute a step further in the direction of obtaining full (IpE), but in any case the general search for interesting **C**-systems extending **bC** and not extending **Ci** can already be funny enough. As to other ways of fixing the non-duality of the consistency and inconsistency operators in **bC**, alternative to extending this logic into **Ci**, suggestions have been made, for instance, for the addition to **bC** of schematic rules such as $\circ A, \neg\circ A \vdash B$ or else $\vdash \circ\circ A$ (perhaps having FACT 3.32 in mind, and trying to carry it forward into **bC**). None of these will work, however, as it is easy to see if one just considers again the matrices in THEOREM 3.16, noticing that they also validate the two last rules, while still not validating schemas such as (ci1) or (ci2). But there may quite well exist some other way out of this quagmire (perhaps the reader will find it).

Now, this is a quickie: can you find any (grammatical) conservative translation from **eCPL** (the extended classical propositional logic obtained by the addition to classical logic of the then innocuous consistency operator) into **bC** (recall the subsection **3.7**), as the one we had for **Ci** (COROLLARY 3.47)? And what happens when one considers, in the construction of **dC**-systems, the addition of more general rules such as (cg) or (RG) (see the subsection **3.8**), which implement more inclusive definitions for the consistency connective? Do we obtain interesting logics from that, fixing some asymmetries observed on the calculi C_n and its relatives? What effects does this move have on the semantical counterparts of these logics? Moving yet farther, we may ask about the logic C_{Lim}, the deductive limit to the hierarchy C_n (see subsection **3.10**), whether it can be proved to be *not* finitely gently explosive. For if it is not finitely gently explosive, given that it is gently explosive *lato senso*, then it cannot be *compact*. Or are we rather obligated to abandon *strong* completeness, in this case? Would there be other interesting **LFI**s in which consistency is not *finitely* expressible?

Recall the subsection **3.11**, where da Costa's requisite **dC[iv]** (subsection **3.8**) is taken very seriously, and we search for *maximal* paraconsistent fragments of classical logic, that is, logics having 'most rules and schemas of classical logic', and a class of 8K three-valued logics is presented as a solution to this. Now, are there other (full) solutions to this 'problem of da Costa'? And are there other interpretations, besides maximality, of da Costa's requisite **dC[iv]** leading to yet some other solutions to that problem? Are there non-many-valued (monotonic) solutions to that problem, or perhaps some other n-valued ones, for $n>3$? And, this is an important first step and probably an easier problem to solve: are there other interesting **C**-systems based on classical logic which are *not* extended by any of the above 8K three-valued logics? We must leave these questions open at this moment. It is interesting to notice, at any rate, that this problem has already been directly addressed here and there, in the literature. Besides [78], from which we drew the results in the subsection **3.11**, one could also recall, for instance, the *adaptive* programme for the confection of paraconsistent logics aiming to represent (non-monotonically) the dynamics of scientific reasoning and of argumentation (see [15]). Roughly speaking, the basic idea behind adaptive logics is that of working in between two boundary logics, often classical logic constituting one of them and a paraconsistent logic constituting the other one, so that consistency is pressupposed by default and we try to keep on reasoning inside of classical logic up to the point in which an 'abnormality' (an inconsistency?) pops out, a situation in which we had better descend to the level of the complementary paraconsistent logic, and go on reasoning over there. Indeed, the ancestral motivations of this programme seem to have been, as it is reasonable to conceive, yet another attempt to maintain the most of classical logic as possible (see [10]), so that paraconsistency is only needed at limit cases, while (most of) classical logic is, in principle, maintained in 'normal' situations. The difficulty here, as far as da Costa's requisite **dC[iv]** is concerned, seems to be *measuring* how much some different adaptive logics will respond to that same requirement of closeness to classical logic (if there is any measurable difference at all). As it is appealing to think of adaptive logics as situations in which two logics are combined in order to produce a third one, it seems also interesting to investigate if possible-translations semantics can, after all, be applied to such an environment as well, or at least stretch the analogies there as far as we can.

Some open questions can also be drawn from the subsection **3.12**. Notice, for instance, that all of Mortensen's axioms, (M1n) and (M2n), for every $n>0$, are validated by the matrices of the three-valued paraconsistent logic $\mathbf{P}^2$ (recall THEO-

REM 3.69), as that author himself have observed, and the question was left open, in [84], (and it remains still so, as far as we know) whether the logic $C_{1/2}$, the stronger of the logics $C_{n/(n+1)}$, would in fact coincide with the three-valued logic $\mathbf{P}^2$. Once $\mathbf{P}^2$ is known to be a maximal paraconsistent logic (cf. THEOREM 3.72), to show this coincidence would amount to showing that all axioms of $\mathbf{P}^2$ (and especially axioms (ca1)–(ca3), in the subsection **3.10**, specifying the consistent behavior of binary connectives) are provable from the axioms of $C_{1/2}$. Alternatively, using the fundamental FACT 3.32, one could try to show that $C_{1/2}$ is controllably explosive (or not, if what one wants is to disprove the conjectured coincidence) in contact with any formula involving binary connectives. In one way or another, it is quite interesting to note that the 8K maximal three-valued logics in subsection **3.11** show that there are several other logics different from $\mathbf{P}^1$ and from $\mathbf{P}^2$ that are next to the classical propositional logic 'in the same kind of way' (half of them extending C_1, as we point out in FACT 3.73), a problem that was left open in the closing paragraph of the above mentioned paper.

Now, what about extending our investigations on the *algebraizability* of **C**-systems (again, see the subsection **3.12**)? Can these algebras solve yet some other categories of logical problems? Again, notice that the problem of finding extensions of our **C**-systems which are algebraizable in the 'classical sense' was also left open (though the plausibility of the existence of such extensions was hinted) in the end of subsection **3.7**. To be precise, what was open was the existence of such extensions as fragments of some version of classical logic —the reader will have seen in the present section, however, that modal logics such as **Z** do extend **bC** and have no problem on what concerns (IpE) (given that $S5$ is algebraizable). And what to say of extending our general approach on section **2** to other 'kinds of logics' (that is, varying the *logic structure* that we defined there) so as to include other kinds of consequence relations, such as (multiple-conclusion) non-monotonic ones (see [5] and [12])? Under this new light shed by **C**-systems and **LFI**s, it is also interesting to see how one can also move on to improve our present (rather poor) proof-theoretical approach. Indeed, Hilbert-style systems, such as the ones we present here, often require too much ingenuity to be applied, leaving intuition or mechanization of proofs far behind. For some **dC**-systems we know that *sequent* systems have already been proposed (see for instance [97] and [17]), as well as *natural deduction* systems (see [58]), and *tableau* systems (see [38]). The *really* interesting cases, however, seem to be those of **C**-systems that are *not* **dC**-systems, so that the consistency connective is, in a sense, 'ineliminable'! A first step towards such a general treatment of **C**-systems in terms of tableaux has already been offered by us in [41], where the logics **bC**, **Ci** and **LFI1** were all endowed with sound and complete tableau formulations.

There is so much yet to be done!

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Errata to the paper 'A Taxonomy of **C**-systems', and more

last updated Feb 2005

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This note contains a collection of important corrections, problems, comments and clarifications to the paper:

> Walter A. Carnielli and João Marcos. A taxonomy of **C**-systems. In W. A. Carnielli, M. E. Coniglio, and I. M. L. D'Ottaviano, editors, *Paraconsistency: The Logical Way to the Inconsistent,* Proceedings of the II World Congress on Paraconsistency, held in Juquehy, BR, May 8–12, 2000, volume 228 of *Lecture Notes in Pure and Applied Mathematics*, pages 1–94. Marcel Dekker, 2002. Preprint available at:
> `http://www.cle.unicamp.br/e-prints/abstract_5.htm`.

It also contains a few solutions to problems that had been left open. Many of the suggested changes are implemented in the above paper's successor:

> Walter A. Carnielli, Marcelo E. Coniglio, and João Marcos. Logics of formal inconsistency. In D. Gabbay and F. Guenthner, editors, *Handbook of Philosophical Logic*, 2nd edition, volume 14. Kluwer Academic Publishers, 2005. Preprint available at:
> `http://www.cle.unicamp.br/e-prints/vol_5,n_1,2005.html`.

All contributions are welcome and will be credited to their authors!

. . . .

If you have not found *any* of the following slips in the paper,
maybe you have not read it carefully enough?

General: Deduction Metatheorem (DM) (M. E. Coniglio)
The Theorem 3.1 (p.48) is indeed correct for C_{min} as stated, for the mentioned reasons. Moreover, this obviously continues provable if new *axioms* are added to the logic. Nevertheless, if one extends this logic by adding new *rules*, then the (DM) often fails! Unfortunately, for lack of care in the presentation of our logics, we introduced them by adding new rules instead of the corresponding axioms... The problem is that we *do* want the (DM) to be valid in all our logics.

Consider a particular example. The logic **bC** is defined in Section 3.2 from C_{min} by adding the rule (bc1) $\circ A, A, \neg A \vdash B$ to the axioms and rules of the latter (p.50). Take now the 8-valued matrices from Theorem 3.53, and notice that they validate all axioms of C_{min} and its only rule, *modus ponens* (MP). Moreover, if one now defines a matrix for the consistency connective such that $v(\circ A) = \frac{4}{7}$ for every value of A, then also the rule (bc1) is validated by the matrices (its premises can never be simultaneously distinguished). But now notice $\circ A \to (A \to (\neg A \to B))$ is not validated by those matrices: Just take A and B as atomic sentences p and q such that $v(p) = \frac{1}{7}$ and $v(q) = \frac{6}{7}$. Then $v(\circ A \to (A \to (\neg A \to B))) = \frac{6}{7}$, while 1 is the only distinguished value of the matrices. So, in this formulation, **bC** would *not* respect the (DM). This was not what we intended, but it neatly illustrates what might happen when one thinks in terms of sequents but writes down Hilbertian axioms instead (in terms of sequents, the (DM) becomes just a rule for implication introduction). *Nostra culpa...*

To fix that flaw, all logics that we introduced by adding new inference rules should instead have been defined by adding the corresponding implicational axioms. Then, given that the (DM) will hold good, the initial rule will be readily derivable, by (MP). Therefore:

Page 50, Fact 3.8

Status unknown: We are not sure as yet if this is true. To be sure, one would have to check in detail whether the (DM) is still derivable from the axiom (Min1) and the rules (MP) and $(A \to B), (B \to C) \vdash (A \to C)$. To play safe, it is better to stick to the original axiom (Min2) all along, and use the last rule as derived.

At any rate, at least the following alternative formulation of the above mentioned Fact can be proven: ‘(Min2) can be substituted, in C_{min}, by the axiom $\vdash (A \to B) \to ((B \to C) \to (A \to C))$.’

Page 50, (bc1), line -19

$\circ A, A, \neg A \vdash B \leadsto \vdash \circ A \to (A \to (\neg A \to B))$

Page 51, (RA0), line 20

$\circ B, (A \to B), (A \to \neg B) \vdash \neg A \leadsto \vdash \circ B \to ((A \to B) \to ((A \to \neg B) \to \neg A))$

Page 51, (bc0), line -17

$\circ A, A, \neg A \vdash \neg B \leadsto \vdash \circ A \to (A \to (\neg A \to \neg B))$

Page 51, (RA1), line -15

$\circ B, (\neg A \to B), (\neg A \to \neg B) \vdash A \leadsto \vdash \circ B \to ((\neg A \to B) \to ((\neg A \to \neg B) \to A))$

Page 56, (bc2), line -14

$\neg\bullet A \vdash \circ A \leadsto \vdash \neg\bullet A \to \circ A$

Page 56, (bc3), line -11

$\neg\circ A \vdash \bullet A \leadsto \vdash \neg\circ A \to \bullet A$

Page 57, (bc4), line 4

$\bullet A \vdash \neg\circ A \leadsto \vdash \bullet A \to \neg\circ A$

Page 57, (bc5), line 5

$\circ A \vdash \neg\bullet A \leadsto \vdash \circ A \to \neg\bullet A$

Page 58, last paragraph before Subsection 3.5

Change all rules for the corresponding implicational axioms.

Page 58, (ci1), line -8
$\bullet A \vdash A \;\rightsquigarrow\; \vdash \bullet A \to A$
Page 58, (ci2), line -7
$\bullet A \vdash \neg A \;\rightsquigarrow\; \vdash \bullet A \to \neg A$
Page 58, (ci), line -4
$\bullet A \vdash (A \wedge \neg A) \;\rightsquigarrow\; \vdash \bullet A \to (A \wedge \neg A)$
Page 64, line -4
$\div A \vdash \neg A \;\rightsquigarrow\; \vdash \div A \to \neg A$
Page 69, (cl), line 16
$\neg(A \wedge \neg A) \vdash \circ A \;\rightsquigarrow\; \vdash \neg(A \wedge \neg A) \to \circ A$
Page 72, (bun), line 22
$(A \to (\circ B \wedge (B \wedge \neg B))) \vdash \neg A \;\rightsquigarrow\; \vdash (A \to (\circ B \wedge (B \wedge \neg B))) \to \neg A$
Page 73, (cd), line -8
$\neg(\neg A \wedge A) \vdash \circ A \;\rightsquigarrow\; \vdash \neg(\neg A \wedge A) \to \circ A$
Page 73, (cb), line -7
$(\neg(A \wedge \neg A) \vee \neg(\neg A \wedge A)) \vdash \circ A \;\rightsquigarrow\; \vdash (\neg(A \wedge \neg A) \vee \neg(\neg A \wedge A)) \to \circ A$
Page 73, lines -3 and -2
$\neg(A \wedge \neg A), (A \to B), (A \to \neg B) \vdash \neg A \;\rightsquigarrow\; \vdash \neg(A \wedge \neg A) \to ((A \to B) \to ((A \to \neg B) \to \neg A)))$
Page 75, (ce), line 15
$A \vdash \neg\neg A \;\rightsquigarrow\; \vdash A \to \neg\neg A$
Page 77, (ca1)–(ca3), lines 22–24
$(\circ A \wedge \circ B) \vdash \circ(A \wedge B) \;\rightsquigarrow\; \vdash (\circ A \wedge \circ B) \to \circ(A \wedge B)$
$(\circ A \wedge \circ B) \vdash \circ(A \vee B) \;\rightsquigarrow\; \vdash (\circ A \wedge \circ B) \to \circ(A \vee B)$
$(\circ A \wedge \circ B) \vdash \circ(A \to B) \;\rightsquigarrow\; \vdash (\circ A \wedge \circ B) \to \circ(A \to B)$
Page 78, line 7
$\bullet(A \to B) \vdash (\bullet A \vee \bullet B) \;\rightsquigarrow\; \vdash \bullet(A \to B) \to (\bullet A \vee \bullet B)$
Page 78, line -3
$A^{(n)}, A, \neg A \vdash B \;\rightsquigarrow\; \vdash A^{(n)} \to (A \to (\neg A \to B))$
Page 80, (co1)–(co3), lines -13 to -11
$(\circ A \vee \circ B) \vdash \circ(A \wedge B) \;\rightsquigarrow\; \vdash (\circ A \vee \circ B) \to \circ(A \wedge B)$
$(\circ A \vee \circ B) \vdash \circ(A \vee B) \;\rightsquigarrow\; \vdash (\circ A \vee \circ B) \to \circ(A \vee B)$
$(\circ A \vee \circ B) \vdash \circ(A \to B) \;\rightsquigarrow\; \vdash (\circ A \vee \circ B) \to \circ(A \to B)$
Page 81, (co1)–(co3), lines -16 to -14
$\circ(A \wedge B) \vdash (\circ A \vee \circ B) \;\rightsquigarrow\; \vdash \circ(A \wedge B) \to (\circ A \vee \circ B)$
$\circ(A \vee B) \vdash (\circ A \vee \circ B) \;\rightsquigarrow\; \vdash \circ(A \vee B) \to (\circ A \vee \circ B)$
$\circ(A \to B) \vdash (\circ A \vee \circ B) \;\rightsquigarrow\; \vdash \circ(A \to B) \to (\circ A \vee \circ B)$
Page 82, (cj1)–(cj3), lines -11 to -9
$\bullet(A \wedge B) \dashv\vdash (\bullet A \wedge B) \vee (\bullet B \wedge A) \;\rightsquigarrow\; \vdash \bullet(A \wedge B) \leftrightarrow (\bullet A \wedge B) \vee (\bullet B \wedge A)$
$\bullet(A \vee B) \dashv\vdash (\bullet A \wedge \neg B) \vee (\bullet B \wedge \neg A) \;\rightsquigarrow\; \vdash \bullet(A \wedge B) \leftrightarrow (\bullet A \wedge \neg B) \vee (\bullet B \wedge \neg A)$
$\bullet(A \to B) \dashv\vdash (A \wedge \bullet B) \;\rightsquigarrow\; \vdash \bullet(A \to B) \leftrightarrow (A \wedge \bullet B)$
Page 92, (M1n)and (M2n), lines -24 to -22
Change for the corresponding implicational forms.

The necessary changes at other places that might have not been listed above are all straightforward. Notice that in a few places, to prove or disprove (IpE), axioms are in general more than one needs, as rules might well do the job. At any rate, the weaker logics from above that contain only the rules instead of the corresponding implicational axioms might also be interesting or more appropriate in a few situations, and they deserve further study.

Pages 31–32, definitions of linguistic and deductive extensions

Notice that in general, in the literature, it is not required that such extensions be 'proper'. The adaptations in that case are straightforward.

As remarked in the paper, in the above comment, and also at some other remarks below, in most, if not all, cases that we talk about 'extensions' we are in fact assuming to be talking about logics that extend other logics by the addition of new axioms or of rules that do not invalidate the Deduction Metatheorem.

Page 35, definitions (Eq1) and (Eq2), and Fact 2.8 (M. E. Coniglio)

That (Eq1) defines an equivalence relation for formulas is an easy consequence of (Con1) and (Con3) (p.31). But (Eq2) does *not* in general define an equivalence relation for sets of formulas under exactly the same conditions. For that effect one needs to restrict the notion of a consequence relation, either by adding the property:

(Con4) $[(\forall B \in \Delta)\ \Gamma \Vdash B$ and $\Delta \Vdash A]$ implies $\Gamma \Vdash A$ (transitivity for sets)

or else by adding the property:

(Con5) $\Gamma \Vdash A$ implies $\Gamma^{\mathsf{fin}} \Vdash A$, for some finite $\Gamma^{\mathsf{fin}} \subseteq \Gamma$ (compacity)

First, it is obvious that adding either (Con4) or (Con5) to (Con1) and (Con3) will do the job. Second, it is equally easy to check that (Con4) derives (Con3) in the presence of (Con1) and (Con2). (Hint: Instantiate, in (Con4), Γ as $\Delta \cup \Gamma$, Δ as $\Gamma \cup \{\alpha\}$, and α as β.)

Finally, to check that (Con4) is *not* derivable from (Con1)–(Con3), consider, for instance, the logic $\mathbf{L}_{\mathbb{R}}$ having the set $\mathbb{R}$ of real numbers as its set of formulas and a consequence relation $\Vdash$ defined as follows:

$\Gamma \Vdash A$ iff $A \in \Gamma$, or $A = \frac{1}{n}$ for some $n \in \mathbb{N}$, $n \geq 1$, or
there is a sequence $(A_n)_{n\in\mathbb{N}}$ contained in Γ such that
$(A_n)_{n\in\mathbb{N}}$ converges to A.

It is easy to see that $\mathbf{L}_{\mathbb{R}}$ satisfies (Con1), (Con2) and (Con3). On the one hand, (Con4) is not valid in $\mathbf{L}_{\mathbb{R}}$. Indeed, take $\Gamma = \varnothing$, $\Delta = \{\frac{1}{n} : n \in \mathbb{N},\ n \geq 1\}$ and $\alpha = 0$. Then the antecedent of (Con4) is true: Every element of Δ is a thesis, and Δ contains the sequence $(\frac{1}{n})_{n\in\mathbb{N}}$ that converges to 0. On the other hand, the consequent of (Con6) is false: 0 is not a thesis in $\mathbf{L}_{\mathbb{R}}$.

The above example of $\mathbf{L}_{\mathbb{R}}$ was proposed in:

J.-Y. Béziau. *Research on Universal Logic: Excessivity, negation, sequents* (in French). PhD thesis, Université Denis Diderot (Paris 7), France, 1995.

If those properties are presupposed from the start there is no need to adjust the statement of Fact 2.8.

Page 38, Fact 2.11(i)

Every theory which is contradictory with respect to $\sim$ is explosive $\leadsto$ Every theory which is contradictory with respect to $\sim$ is trivial

Fact 2.11(ii)
A logic with a supplementing negation $\sim$ cannot be $\sim$-contradictory nor trivial, given part (a) of (D10), in the previous page.

Page 38, Fact 2.13(ii)
If **L** is finitely trivializable $\rightsquigarrow$ If **L** is non-trivial yet finitely trivializable

Page 39, second paragraph, **Page 43**, lines 17–19,
Page 45, line 8, **Page 46**, third paragraph,
Page 52, line 23, and a few other places,
Jaśkowski's logic **D2**
There is an awful lot of misunderstanding and confusion in the literature about the logic **D2**, one of the earliest samples of the paraconsistent vintage, introduced in:

> Stanisław Jaśkowski. A propositional calculus for inconsistent deductive systems (in Polish). *Studia Societatis Scientiarum Torunensis*, Sectio A, 5:57–77, 1948. Translated into English in *Studia Logica*, 24:143–157, 1967, and in *Logic and Logical Philosophy*, 7:35–56, 1999.
>
> Stanisław Jaśkowski. On the discussive conjunction in the propositional calculus for inconsistent deductive systems (in Polish). *Studia Societatis Scientiarum Torunensis*, Sectio A, 8:171–172, 1949. Translated into English in *Logic and Logical Philosophy*, 7:57–59, 1999.

Misled by decades of biased presentations of this logic, our paper commits basically the same mistakes in its presentation. However, it should be clear to anyone that reads the above papers, once and for all, that the logic presented in our paper is *not* Jaśkowski's **D2**. Let's call **J** the logic defined, as in the paper, by setting $\Gamma \Vdash_{\mathbf{J}} \alpha$ iff $\Diamond\Gamma \vDash_{S5} \Diamond\alpha$. This 'pre-discussive' logic **J** is indeed implicitly considered by Jaśkowski in his papers, but it does not represent the 'discussive' logic **D2**.
To define **D2**, Jaśkowski in fact uses the above '$\Diamond$-translation', but only after he preprocesses the classical connectives, in the following way. Let For denote the set of formulas of classical propositional logic, in a language containing the connectives $\neg$, $\wedge$, $\vee$, $\rightarrow$ and $\leftrightarrow$, and let For^{M} denote the set of formulas of a language containing also the unary modal connectives $\Diamond$ and $\Box$. Consider a mapping $j : For \rightarrow For^{\mathsf{M}}$ such that:

(i) $p^* = p$ for every atomic sentence p
(ii) $(\neg A)^* = \neg A^*$
(iii) $(A \wedge B)^* = A^* \wedge \Diamond B^*$
(iv) $(A \vee B)^* = A^* \vee B^*$
(v) $(A \rightarrow B)^* = \Diamond A^* \rightarrow B^*$
(vi) $(A \leftrightarrow B)^* = (\Diamond A^* \rightarrow B^*) \wedge (\Diamond B^* \rightarrow \Diamond A^*)$

Then, **D2** is the logic defined by setting $\Gamma \Vdash_{\mathbf{D2}} \alpha$ iff $\Diamond(\Gamma^*) \vDash_{S5} \Diamond(\alpha^*)$, where $\Gamma^* = \{\gamma^* : \gamma \in \Gamma\}$. It should be noticed that clause (iii) comes from the 1949 2-pages paper, that was only officially translated into English very recently; all the other clauses are indigenous to the 1948 paper. Without (iii), the resulting conjunction is left-adjunctive but not left-disadjunctive, as in the case of the logic **J** mentioned in our paper. Too much fuss has been made in the literature about the alleged 'non-adjunctive' character of **D2**. With the above definition, however, **D2** is perfectly adjunctive. Moreover, it *validates all axioms and rules of positive classical logic*, and yet $\neg$ *is non-explosive.*

Actually, in our paper we wrote several times that **D2** is an **LFI**, and we were indeed not wrong about that, even in the present updated formulation of the logic. Let's prove it. Consider the following set of abbreviations on For:

$\top \stackrel{\text{def}}{=} (A \lor \neg A)$, for any formula A (a top particle)

$\bot \stackrel{\text{def}}{=} \neg\top$ (a bottom particle)

$\blacksquare A \stackrel{\text{def}}{=} (\neg A \to \bot)$

$\blacklozenge A \stackrel{\text{def}}{=} \neg\blacksquare\neg A$

$\circ A \stackrel{\text{def}}{=} (\blacklozenge A \to \blacksquare A)$ (a consistency connective)

It is easy to check that $\circ$ has indeed the expected behavior of a consistency connective, namely: (a) $\circ p, p \nVdash_{\mathbf{D2}} q$; (b) $\circ p, \neg p \nVdash_{\mathbf{D2}} q$; (c) $\circ A, A, \neg A \Vdash_{\mathbf{D2}} B$. To check (a), just take an $S5$-model containing a sole world w in which p is true but q is false. To check (b), take again an $S5$-model containing a sole world w, but now let both p and q be false in it. To check (c), notice that it corresponds in the end to checking, in $S5$, the validity of the inference $(\Diamond A \to \Box A), \Diamond A, \Diamond\neg A \vDash_{S5} \Diamond B$. You might use your preferred $S5$-decision procedure to check that. As a consequence, one may now safely conclude that **D2** is a **dC**-*system based on classical logic.*

One final observation about **D2**. The fact that it is defined by way of a (double) translation into the modal logic $S5$ has led some people to believe and assert that **D2** is a 'modal paraconsistent logic'. It should be remarked, however, that **D2** fails one of the main characterizing properties of a normal modal logic: the *replacement property* (called, in our paper, (IpE), for 'intersubstitutivity of provable equivalents'). Indeed, while it is true, for instance, that $\neg(A \land \neg A) \dashv\Vdash_{\mathbf{D2}} (B \lor \neg B)$, the following inference fails: $\neg\neg(A \land \neg A) \Vdash_{\mathbf{D2}} \neg(B \lor \neg B)$. Indeed, for a counter-model to the latter inference, just take for A an atomic sentence p and consider a modal model with two worlds w and v such that w sees v, p is true in w but false in v. On that matter, check also the paper: (**Chapter 3.2** of the present thesis)

> João Marcos. Modality and paraconsistency. In M. Bilkova and L. Behounek, editors. *The Logica Yearbook 2004*, Proceedings of the XVIII International Symposium promoted by the Institute of Philosophy of the Academy of Sciences of the Czech Republic. Filosofia, Prague, 2005. Preprint available at:
> `http://wslc.math.ist.utl.pt/ftp/pub/MarcosJ/04-M-ModPar.pdf`.

Page 42, Fact 2.14

One needs to assume here that $\neg A$ does not always denote a bottom particle.

Page 44, Fact 2.15(ii)

Any explosive logic is partially explosive $\leadsto$ Any non-trivial explosive logic is partially explosive

Page 45, Definition (D27) of positive-preservation, and G. Priest &
Page 46, Definition (D28) of a **C**-system A. Avron &
M. E. Coniglio & J. Marcos & W. Carnielli

As it is, the definition of a **C**-system allows for some degenerate examples, such as that of a logic **L** that is a **C**-system based on whatever constitutes the 'negationless fragment' of **L** itself. This is not very informative. A better and more careful way of implementing the same intuition is as follows. Consider a set of connectives $\Sigma 1$ and let **L**1 be a consistent logic (that is, neither paraconsistent nor trivial) whose

formulas are written with the help of $\Sigma 1$. Let $\neg$ be some symbol for negation and let **L**2 be a logic whose formulas are written with the help of a set of connectives $\Sigma 2$ such that $\neg \in \Sigma 2 - \Sigma 1$. We say that **L**2 is a **C**-*system based on* **L**1 *with respect to* $\neg$ if:
(a) **L**2 is a conservative extension of **L**1, and $\neg$ is not definable in **L**1,
(b) **L**2 is an **LFI** (with respect to $\neg$), where $\Delta(A) = \{\circ A\}$ (recall (D15)).

Page 45, proof of Fact 2.19
Notice that one needs to really guarantee somehow that (1) '$\circ A$ is a not a top particle', (2) '$\{\circ A, A\}$ is not always trivial' and (3) '$\{\circ A, \neg A\}$ is not always trivial', for the proposed definition of $\circ A$ as $(A \to \bot) \vee (\neg A \to \bot)$. To wit, some properties of the symbol $\neg$ in the given paraconsistent logic must be known in advance. Indeed, if $A \leftrightarrow \neg A$ is provable, for instance, then both (2) and (3) fail.
Part (i) is in general unproblematic. Indeed, in positive classical logic, $\vdash ((A \wedge B) \to C) \leftrightarrow ((A \to C) \vee (B \to C))$, so, if $\circ A$, as above defined, is a top particle in a logic such as the one mentioned in the statement of the Fact, then $(A \wedge \neg A) \vdash \bot$, and the logic would not be paraconsistent.
For parts (ii) and (iii) it is enough to consider that the negation symbol $\neg$ has the two following 'negative properties':

(*verificatio*) $(\exists A)\ \neg A \nVdash A$ (*falsificatio*) $(\exists A)\ A \nVdash \neg A$

This justifies the 'in general' used in the proof of the Fact. Notice that these rules are the weakest forms of some basic characterizing negative rules for negation proposed in the paper: (**Chapter 4.1** of the present thesis)

João Marcos. On negation: Pure local rules. *Journal of Applied Logic*, 2005.
Preprint available at:
`http://www.cle.unicamp.br/e-prints/vol_4,n_4,2004.html`.

Page 45, definition (D27)
Extend the definition in the natural way for the case of logics containing more than one negation symbol.

Page 46, lines -18 and -17, bold paraconsistency
We announced that all of our **C**-systems would be boldly paraconsistent, but we did not prove that in the paper. Given the significance of this claim, it is only fair that we sketch here its proof.
Consider any of the 8K maximal paraconsistent 3-valued logics from Subsection 3.11, of which each of the other **C**-systems of the paper is a deductive fragment. Assume $\Gamma \nvDash \sigma(p_0, \ldots, p_n)$ for some appropriate choice of formulas. In particular, by (Con2), it follows that $\nvDash \sigma(p_0, \ldots, p_n)$. Now, consider a variable p not in $p_0, \ldots, p_n$. Let p be assigned the value $\frac{1}{2}$, and extend this assignment to the variables $p_0, \ldots, p_n$ so as to give the value 0 to $\sigma(p_0, \ldots, p_n)$. It is obvious that, in this situation, $p, \neg p \nvDash \sigma(p_0, \ldots, p_n)$.

Page 51, Theorem 3.12 M. E. Coniglio & J. Marcos
The 'real proof' is in fact not that naive. Given a derivation of a formula of the language of C_{min} in which (bc1) is used, there can always happen, in theory, that there is *another* derivation of the same formula that does not use (bc1) but that still makes use of the new connective $\circ$ of the extended language.

This result is not really worth the painful induction over the Hilbertian derivations. An alternative, and simpler, way of verifying the Theorem is by looking directly at the recursive semantics associated to both logics, and checking that the corresponding decision procedures for formulas *not* containing the consistency connective in the case of **bC** validates exactly the same formulas as the decision procedure of C_{min} does. For such procedures, check, further on, the paper mentioned in the comment to '**Page 66**, Fact 3.50'.

Page 57, Theorem 3.25
Wrong choice of matrices for the independence proofs, as it is (for instance, the axiom (bc1) is not validated by them). The easiest way of fixing this is by changing the matrix of negation, both in part (i) and in part (ii), for one such that $v(\neg A) = 0$ if $v(A) \in \{1, \frac{2}{3}\}$, and $v(\neg A) = 1 - v(A)$ otherwise.
It is also possible to prove the same theorem using 3-valued matrices, instead of 4-valued ones. Consider again the same matrices for $\wedge$, $\vee$, $\rightarrow$ and $\neg$ as in Theorem 3.23. For part (i), take $v(\circ A) = 1$ and $v(\bullet A) = \frac{1}{2}$ if $v(\circ A) \in \{1, 0\}$ and $v(\circ A) = 0$ and $v(\bullet A) = 1$ otherwise. For part (ii), take $v(\circ A) = \frac{1}{2}$ if $v(\circ A) \in \{1, 0\}$ and $v(\circ A) = 0$ otherwise, and take $v(\bullet A) = 1$ for every value of A.

Page 58–59, and **Page 62**, Theorem 3.41,
on the axiomatization of the logic **Ci**
The more we have tried to clarify and motivate the whole thing, the axiomatization(s) of **Ci** still remained somewhat hard to swallow. This is an important point, of course, as every other subsequent logic in the paper will extend this fundamental logic. A simple set of axioms for **Ci** is obtained if one just adds to **bC** the following new axioms:

(ci)	$\vdash \neg\circ A \rightarrow (A \wedge \neg A)$
(cis)	$\vdash \circ\circ A$
(inc)	$\vdash \bullet A \leftrightarrow \neg\circ A$

Remember to check also the paper mentioned in the comment to '**Page 66**, Fact 3.50', below.

Page 60, Fact 3.32 (M. E. Coniglio)
(or in any extension of this logic) $\leadsto$ (or in any axiomatic extension of this logic)

Page 61, Fact 3.36 (M. E. Coniglio)
The addition of (RC): [...] to **Ci** causes its collapse into classical logic $\leadsto$ The least extension of **Ci** that satisfies (RC): [...] and the Deduction Metatheorem collapses into classical logic

Page 61, Fact 3.37(i) (M. E. Coniglio & J. Marcos)
This part of the Fact is false. Indeed, to see that (bc2) is independent from the other axioms of **Ci**, consider again the 3-valued matrices for $\wedge$, $\vee$, $\rightarrow$ and $\neg$ as in Theorem 3.23, and define $v(\circ A) = 1$ and $v(\bullet A) = 0$ if $v(\circ A) \in \{1, 0\}$ and $v(\circ A) = 0$ and $v(\bullet A) = \frac{1}{2}$ otherwise. To see that (bc3) is independent from the other axioms of **Ci**, do again as above, but now define $v(\circ A) = \frac{1}{2}$ and $v(\bullet A) = 0$ if $v(\circ A) \in \{1, 0\}$ and $v(\circ A) = 0$ and $v(\bullet A) = 1$ otherwise.

Page 64, (Alt11), line 9
This has led to some confusion, and it must be clarified once and for all. If one is talking about a non-trivial extension of positive classical logic, (Alt10) and (Alt12) alone define all properties of classical negation. (Alt11) will be derivable from the other axioms and rules, and it is thus *not* necessary to talk about this last axiom at any point in the text, once it can be checked that one can count on the previous two axioms for negation. Classical logic is indeed axiomatizable by (MP) and axioms (Min1)–(Min9) of positive classical logic, plus (Alt10) and (Alt12). To be sure, this fact was already remarked in this paper, at p.50, lines 18–19.

Page 66, Fact 3.50 (M. E. Coniglio & J. Marcos)
The 'only if' part holds good, but the 'if' part is too restrictive as it is, and the alleged proof is wrong. In fact, as we know from Facts 3.32 and 3.33, p.60, all formulas preceded by a $\circ$ or a $\bullet$ 'behave classically' in **Ci**. And so does any complex boolean combination of such formulas. Accordingly, given a classical theorem $A(p_1, \ldots, p_n)$, where $p_1, \ldots, p_n$ are its atomic subformulas, then the following can be proven: $\vdash_{\mathbf{Ci}} \circ A(\circ p_1, \ldots, \circ p_n)$. Moreover, given any formula $A(C_1, \ldots, C_n)$, where $C_1, \ldots, C_n$ are all tops or bottoms of **Ci**, then $\circ A$ is also a top. There are, thus, many 'provably consistent' formulas of **Ci** left off by the statement of the above Fact.
All this is of course much easier to verify semantically. For that you might check, for instance, the paper: (**Chapter 2.2** of the present thesis)

João Marcos. Possible-translations semantics for some weak classically-based paraconsistent logics. Research report, CLC, Department of Mathematics, Instituto Superior Técnico, 1049-001 Lisbon, PT, 2004.
`http://wslc.math.ist.utl.pt/ftp/pub/MarcosJ/04-M-PTS4swcbPL.pdf`

Page 67, Theorem 3.51(i)
This is true in fact for any **LFI**, and not only for extensions of **Ci**.

Page 68, line 5 (M. E. Coniglio)
(RC) cannot be added to **Ci** $\rightsquigarrow$ (RC) cannot be added to **Ci**, together with the Deduction Metatheorem

Page 79, and **Page 95**, line -10, and **Page 101**, lines 8–12,
on the logic C_{Lim} and non-finitely gently explosive paraconsistent logics
The logic C_{Lim} is not compact, thus it is also not finitely gently explosive. Indeed, let p be an atomic sentence, and let $\Gamma^\kappa = \{p^n : 0 \leq n < \kappa \leq \omega\}$, where the formulas p^n are defined as at the end of p.74. Then, $\Gamma^\omega, \neg p \vdash B$, for every B, in every logic C_n, $1 \leq n < \omega$, thus $\Gamma^\omega, \neg p \vdash B$ is a sound inference in C_{Lim}. Suppose now that there is a finite subset $\Gamma_{\mathsf{fin}} \subseteq \Gamma$ such that $\Gamma_{\mathsf{fin}}, \neg p \vdash B$ holds good in C_{Lim}. Then, if A^m is the largest formula in Γ_{fin}, the derivation $\Gamma^m, \neg p \vdash B$ will also hold good, by monotonicity. But that same derivation does not hold good in C_{m+1}, and this logic extends C_{Lim}. Absurd.

Page 81, line 7
Cito $\rightsquigarrow$ **Cigo** (the 'g' is from axiom (cg), on p.74)
Citoe $\rightsquigarrow$ **Cigoe**

Page 88, Fact 3.75
The definitions of the congruence matrices have caused some confusion. To be sure, only one of them is always available for sure in the 8K logics: the one that makes $v(A \equiv B) = 1$ when $v(A) = \frac{1}{2} = v(B)$. The other matrix is only definable in *some* of the 8K logics.

Page 91, Fact 3.79
Every deductive extension $\rightsquigarrow$ Every non-linguistic deductive extension, that is, every deductive extension over a fixed language,

Page 93, last 4 lines (M. E. Coniglio & J. Marcos)
being thus algebraizable in the sense of Blok-Pigozzi (though [...]) $\rightsquigarrow$ being thus *equivalential* in the sense of Blok-Pigozzi (though [...]). That being known, to be BP-algebraizable they will only need to be shown, in addition, to be weakly algebraizable. For the argument, check the Theorem 3.16 of:

> Josep Maria Font, Ramon Jansana, and Don Pigozzi. A survey of abstract algebraic logic. *Studia Logica*, 74(1/2):13–97, 2003. Abstract algebraic logic, Part II (Barcelona, 1997).

The same theorem shows that *all* our present **LFI**s are at least protoalgebraizable, as all of them extend positive classical logic and contain thus an appropriate implication such that $\vdash A \to A$ and $A, A \to B \vdash B$.
(Notice that this partly settles a taxonomical question that appears on **Page 95**.)

Page 95, line 11, proof of Theorem 3.83 (M. E. Coniglio)
$\{0, a, 1, u\}$ and $\{0, b, 1, u\}$ are two filters $\rightsquigarrow$ $\{a, 1, u\}$ and $\{b, 1, u\}$ are two filters

Page 99, second paragraph
The logics **mbC** and **mCi** were mentioned in passing, but their axiomatizations were not clarified beyond any doubt. To obtain the logic **mbC**, indeed, all one needs to do is to 'delete axiom (Min11): $\neg\neg A \to A$' from the set of axioms of **bC**. Now, the axiomatization of **mCi** is trickier, because of the intended relation of classical opposition between the $\circ$ and the $\bullet$. The most obvious way of obtaining **mCi**, in a sense, seems to be through an infinite set of axioms, namely, by adding to **mbC** the following new axioms:

(ci) $\vdash \neg\circ A \to (A \wedge \neg A)$
$(\mathrm{cc})_n$ $\vdash \circ\neg^n\circ A$, where $\neg^0 A = A$, and $\neg^{n+1} A = \neg\neg^n A$, for every $n \in \mathbb{N}$
(inc) $\vdash \bullet A \leftrightarrow \neg\circ A$

Compare this to the set of axioms for **Ci** proposed above, in the comments to '**Page 58–59** etc'. Notice that, in **mCi**, the so-called 'Guillaume's Theses' from Fact 3.38, p.61, are no longer true, thus the need of (cis_n).
Check also, again, the paper mentioned in the comment to '**Page 66**, Fact 3.50'.

Page 108, item [111]
Theory of logical calculi $\rightsquigarrow$ *Theory of Logical Calculi*

Chapter Two

Possible-Translations Semantics for Logics of Formal Inconsistency

This chapter is composed of two contributions: **2.1** brings the more general paper 'Possible-translations semantics', henceforth PTSurvey; **2.2** brings the more specific paper 'Possible-translations semantics for some weak classically-based paraconsistent logics', henceforth WeakPTS. The next pages are written by way of an introduction. To fully understand and follow them, it might help that you have read the subsequent papers first. Or that you keep an eye here and another one there, like a dragon.

Resumo de PTSurvey

Este texto almeja dar uma visão panorâmica das semânticas de traduções possíveis, definidas, desenvolvidas e ilustradas como um formalismo muito abrangente para se obter ou representar semânticas para todo tipo de lógicas. Com tal ferramenta, uma ampla classe de lógicas complexas se revela muito naturalmente (de)componível em termos de alguma combinação adequada de lógicas mais simples. Vários exemplos serão mencionados, e alguns casos particulares de semânticas de traduções possíveis, dentre os quais se encontram as semânticas de sociedade e as semânticas não-determinísticas, serão referidos.

Resumo de WeakPTS

Esta nota fornece interpretações por meio de semânticas de traduções possíveis para um grupo de lógicas paraconsistentes fundamentais estendendo o fragmento positivo da lógica proposicional clássica. As lógicas PI, C_{min}, **mbC**, **bC**, **mCi** e **Ci**, entre outras, são todas inicialmente apresentadas por meio de semânticas bivalentes e sequentes, e são a seguir destrançadas por meio de semânticas de traduções possíveis —o conjunto de matrizes 3-valoradas das lógicas ingredientes é exibido, em cada caso, juntamente com o conjunto de funções de tradução admissíveis. Enunciados precisos e todos os detalhes não-óbvios das demonstrações são apresentados. Outros detalhes são deixados para o leitor.

Contents

> I hope that posterity will judge me kindly, not only as to the things which I have explained, but also as to those which I have intentionally omitted so as to leave to others the pleasure of discovery.
> —René Descartes, *La Géométrie*, 1637.

Theoretical and practical aspects of the Logics of Formal Inconsistency (**LFI**s) were both studied in detail in **Chapter 1.0**: A huge number of **LFI**s were presented there, most of them, though, in purely syntactical terms. Can one always provide adequate and informative semantics for those very same logics? The weakest samples among our previous **LFI**s are logics that are neither finite-valued nor do they have canonical modal semantics (once they fail replacement). It is not difficult to provide, however, bivalent semantics for those logics, mocking somehow their syntactical formulations. Such 2-valued non-truth-functional semantics are often not that much illuminating. The present chapter will show how those same logics can be alternatively interpreted in terms of another paradigm of formal semantics: the *possible-translations semantics* (PTS). The papers contained in the present chapter are helpful but somewhat sketchy: One is an extended abstract and another evolved from a research report aimed at helping interested readers find their way. This choice of presentation is hopefully condoned by the fact that PTS is only a subsidiary topic in the present thesis.

One size fits all

The paper PTSurvey (cf. [24]) starts by proposing a structure called 'possible-translations representation' (PTR) as an extremely general framework for specifying the notion of a consequence relation.[1] In principle, whatever non-degenerate definition of logic one might propose, it is always possible to come up with another thing one might want to call a 'logic' and that eludes that definition. Nonetheless, to a first approximation, any logic based on sets of formulas and on (single- or multiple-conclusion) consequence relations will have an adequate PTR, given the present comprehensive design of the latter concept. The basic idea is that of splitting a logic with the help of a collection of 'factors' (other logics) into which it can be 'translated', producing, by suitable combination of these translations, a 'conservative translation' that should provide an adequate (sound and complete) representation for the initial logic. In case the definition of the involved factors can somehow be alleged to involve semantic notions, then the corresponding PTR is said to constitute a 'possible-translations semantics' (PTS).

[1] For the case of single-premise 'simple' PTRs based on grammatical translations (that is, translations homomorphic over the algebra of formulas), my definition coincides with the definition of a *syntactical semantics* presented in [20]. My deepest thanks to Dov Gabbay for calling my attention to that.

The above described splitting process is by no means unusual. For an example from the history of linguistics, the paper mentions the Rosetta Stone and the quest for providing an adequate interpretation for the ancient Hieroglyphic writing. It was only by collecting bits and pieces of meaning from several translations of Hieroglyphic texts into other languages, with varying degrees of fidelity and authenticity, that a conclusive meaning could finally be /extracted from/attached to/ the ancient Egyptian religious texts.

Traduttore, traditore! The well-known Italian proverb that haunts skilled translators from all over punctuates the horny dilemma they are confronted with on an everyday basis: Should translation be as mechanized as possible, or should one let creativity come in? Notice that this question allegedly applies to translations both of technical and of literary texts. To invert the situation and put it in more charitable terms for the translators, can translation actually help *explicate* the meaning of the 'original' text? One could defend, for instance, that the true meaning of a poem is fully conveyed only by the set of all of its translations —and such a theory is beautifully confirmed by [21] or [27]. In a sense, nobody is a native speaker of the language of ideas: We are translating all the time to make ourselves understood, and to try understand the Other. Be that as it may, on what concerns machine translation the current situation remains at best dismaying. One might always recall for instance the story according to which a machine was being built in Shinar to translate English into Chinese, and vice-versa. To test its first prototype, a mighty hunter suggested the phrase "Out of sight, out of mind" to be fed into it. After translating it into Chinese, and then back into English, the final output produced was: "Blind idiot"... An alternative version of the story brings another Babylonian machine built to translate English into Russian, and vice-versa. To test it, the phrase "The flesh is weak, but the spirit is willing" was suggested and fed into it. It was then translated into Russian and back into English, using the latest technological advances on universal grammar, HPSG, GB, and context-sensitivity. The output produced was: "The meat is rotten, but the vodka is good". Yes, Babel is a reality. So far so good for automated translation.

Now, for some examples from the field of logic, the paper PTSurvey considers next some usual abstract definitions of the very notion a logical system. SCT (single-conclusion tarskian) and MCT (multiple-conclusion tarskian) consequence relations are characterized there both abstractly and semantically. In fact, there are traditional adequacy results that prove the equivalence of those two characterizations, in general: Every /SCT/MCT/ consequence relation characterized semantically also respects the abstract clauses defining an /SCT/MCT/ consequence relation; conversely, every logic respecting the appropriate abstract clauses can also be characterized semantically (check [33], but also [30] and [32]). The first result is easy and I leave it as an exercise. The canonical construction employed in the proof of the second result is shown in the paper to make use of a very specific PTS for each /SCT/MCT/ abstract logic. This PTS is simple and is based on a collection of many-valued factors; I also show (applying an idea from Suszko, in [31]) how they can all be reduced to factors that are at most 2-valued. So, to be sure, every /SCT/MCT/ logic is shown after all to have adequate PTS based on many-valued or on 2-valued factors.

Several degenerate examples of logics and of translations are also provided. Some more specific classes of semantics are mentioned as particular cases of PTS, but the demonstration of that claim is left for a future version of the paper. Keep your eyes open, if you're not a fool. And don't drink too much, or else you might miss the churrasco.

How much is that in 'real money'?

While the preceding paper was quite general and abstract, the next paper, WEAKPTS (cf. [25]) gets much more down to earth, and provides several examples of PTS as applied to some of the weakest among the **LFI**s from **Chapter 1.0** as well as to some other very weak paraconsistent logics deprived of a consistency connective. Nine paraconsistent logics, six of them **LFI**s, are here split with the help of PTS based on a common set of 3-valued matrices, varying only the set of admissible translations so as to suit the case of each logic. These logics are this time introduced directly in terms of the axioms governing their sets of admissible non-truth-functional bivaluations. Special attention should be paid to the logic **mCi**, suggested at the final section of [18] but here axiomatized for the first time. Its basic intuition is that formulas preceded with a consistency connective should 'behave classically'.

While the move from a logic to another, in this paper, can usually be made by adding or erasing a few axioms, the difference between my presentation of **mCi** and of its extension **Ci** is more remarkable: While the former uses an infinite number of axioms of a certain format, the latter uses only a finite number of them, for the other ones turn then to be derivable. The underlying idea is the following. All of our current Logics of Formal Inconsistency are based on classical logic and extend the weak non-gently explosive logic PI. Moreover, all of them, as we have seen in the previous chapter, can define a classical negation —this was shown there for the case of **bC**, but the same definition given in Theorem 3.48 of the TAXONOMY works equally well for **mbC**, as I point out in WEAKPTS. Now, if $\div$ denotes this classical negation and $\sim$ denotes here the primitive paraconsistent negation, an inconsistency connective $\bullet$ that behaves as dual to the primitive consistency connective $\circ$ can always be defined simply by setting $\bullet\alpha \stackrel{\text{def}}{=} \div\circ\alpha$. What the logic **mCi** does in extending the logic **mbC**, and what the logic **Ci** does in extending the logic **bC**, is exactly guaranteeing that this definition can alternatively be written as $\bullet\alpha \stackrel{\text{def}}{=} \sim\circ\alpha$. Once the logic **mCi** has a weaker control over the paraconsistent negation than the logic **Ci**, given that only the latter allows for $\sim\sim$-elimination, an infinite number of axioms came on handy in this paper in order to guarantee the fine interaction of $\sim$ with $\circ$.

In the WeakPTS, sequent-style formulations of all the above mentioned logics are offered from the start. Sequent systems for paraconsistent logics originated from the 'Brazilian school' approach are known at least since [28], and they received a new impulse as some of the most traditional **C**-systems and some variations on them were endowed with adequate sequent-style formulations in [3, 4, 5]. The connections between sequent systems and bivaluations are well-known (cf. [6]), and I do not go here into the trouble of proving the equivalence between these two forms of presentation for the above logics. My paper *does* show in some detail, however, how to prove the equivalence between the presentations of those logics in terms of bivaluations and in terms of the proposed PTS. To that effect, the use of a non-canonical measure of complexity of the formulas is helpful, as it was done in [9, 10].

A traditional key to proving that two semantics 'do the same job' consists in building a sort of bisimulation between them, showing that a model from one semantics can be simulated by a model from the other semantics, and vice-versa. On the one hand, bivaluational models are defined by attributing the value 1 ('true') or the value 0 ('false') to each formula of the language. On the other hand, we can understand a PTS-model as a pair consisting of a translation into a factor logic together with a model from that factor. As usual, the more models you have, the less inferences and theses your logic is likely to validate. Intuitively, to prove soundness you have thus to make sure that you do not have 'too many models', not to fail validating something that should be validated. To prove completeness you ought, conversely, to have a sufficient number of models, so as not to 'validate too many things'. Our 'convenience' result, following an idea from [13], shows that the set of bivaluational models, in each case, can simulate the corresponding set of PTS-models. Soundness is a corollary to that. The 'representability' result does the converse and completeness follows as a corollary. Now, this is the only really delicate point: Given a bivaluation, the choice of the simulating translation from among the admissible alternatives is not always obvious. I show in the paper how it can be done in each case, and I leave the rest of the easy but long inductive proofs on the reader's charge. There is no real novelty: In [23] I have illustrated such sort of proofs and their heuristics in painstaking detail.

Given that each particular formula of our paraconsistent logics will originate a finite number of translations into the corresponding factors, and given that those factors are 3-valued, it should be clear to everybody how the above mentioned PTS provide decision procedures for those 9 logics. Once the present paper did not illustrate the procedures in any detail, however, I will briefly do that in what follows, before closing this subsection, so as not to leave any doubt as to how they work. (You can safely jump the forthcoming ramblings if you have already fully understood the methods involved.)

As in our [9], let's consider here a metalinguistic equational logic in which the symbol ',' represents an '...and...', '|' represents an '...or...', '→' rep-

line	p	$\sim p$	$\sim\sim p$	$\sim\sim p \supset p$	$p \supset \sim\sim p$
$\boxed{1}$	0	$\cancel{0}$	$\ldots$	$\ldots$	$\ldots$
$\boxed{2}$		1	0	1	1
$\boxed{3}$			$\cancel{1}$	$\ldots$	$\ldots$
$\boxed{4}$	1	0	$\cancel{0}$	$\ldots$	$\ldots$
$\boxed{5}$			1	1	1
$\boxed{6}$		1	0	1	0
$\boxed{7}$			1	1	1

Figure 1: Illustration of quasi matrices.

resents an 'if... then...', and '$\leftrightarrow$' represents an '... if and only if...'. If one now takes the set of all bivaluation mappings $b : \mathcal{S}_{\mathbf{CPL}} \longrightarrow \{0, 1\}$ such that:

(b1.1) $\quad b(\alpha) = 1, b(\beta) = 1 \quad \leftrightarrow \quad b(\alpha \wedge \beta) = 1$

(b1.2) $\quad b(\alpha) = 1 \mid b(\beta) = 1 \quad \leftrightarrow \quad b(\alpha \vee \beta) = 1$

(b1.3) $\quad b(\alpha) = 0 \mid b(\beta) = 1 \quad \leftrightarrow \quad b(\alpha \supset \beta) = 1$

(b2**c**) $\quad b(\alpha) = 0 \quad \leftrightarrow \quad b(\sim\alpha) = 1$

then one obtains an adequate semantic characterization for classical propositional logic (**CPL**). It is easy to tinker with the above axioms on bivaluations, thus defining new logics instead of **CPL**. For instance, as we can see in the present paper, the logic C_{min} (a.k.a. PIf) is obtained if one just drops (b2) and puts the following bivaluational axioms in its place:

(b2) $\quad b(\sim\alpha) = 0 \quad \rightarrow \quad b(\alpha) = 1$

(b6) $\quad b(\sim\sim\alpha) = 1 \quad \rightarrow \quad b(\alpha) = 1$

While the bivaluations of **CPL** determine a well-known decision procedure by way of 2-valued matrices, this time another decision procedure can still be obtained for PIf by way of 2-valued 'quasi matrices'. In a sense, it all works pretty much as if we started writing every possible attribution of the truth-values 0 and 1 to the subformulas of a given formula, following its canonical complexity measure, but then we erased each attribution that disrespected the above bivaluational axioms.

In practice, suppose we would like to test the validity in PIf of the formulas $\sim\sim p \supset p$ and $p \supset \sim\sim p$. Then we would get something like in Figure 1. Lines 1 and 4 of Figure 1 are erased in consideration of the bivaluational axiom (b2), and line 3 is erased in consideration of (b6). We see that $\sim\sim p \supset p$ is a tautology of PIf given that all remaining lines of the quasi matrix satisfy this formula. On the other hand, $p \supset \sim\sim p$ is not satisfied by line 6.

Now, in case a further bivaluational axiom is added such as:

(b6$^{\mathbf{r}}$) $\quad b(\sim\sim\alpha) = 0 \quad \rightarrow \quad b(\alpha) = 0$

then the resulting semantics characterizes the logic $PIfe$, according to the present paper. Notice that now the line 6 of the quasi matrix from Figure 1 will be erased in consideration of (b6$^{\mathbf{r}}$), so that $p \supset \sim\sim p$ turns to be a tautology of $PIfe$.

Of course, to show that the above sketched decision procedures really work in the general case, one has to show that every possible bivaluation of PIf is thereby represented, and only those bivaluations are so represented, that is: (i) each bivaluation is simulated by a line of a quasi matrix (one does not erase more lines than needed); (ii) each line of a quasi matrix can be extended into a bivaluation that simulates it. I will here leave that proof as an exercise and move on instead to show how the decision procedure for the corresponding PTS works. This time the formulas of the logics in focus, in a sense, 'lose their individuality' and start to mean the same as the 'sum of all their translations.'

1	2	3	4	5	6	7
p	$\sim_1 p$	$\sim_2 p$	$\sim_1\sim_1 p$	$\sim_1\sim_2 p$	$\sim_2\sim_1 p$	$\sim_2\sim_2 p$
F	T	T	F	F	F	F
t	F	t	T	F	T	t
T	F	F	T	T	T	T

1	8	9	10	11
p	$\sim_1\sim_1 p \supset p$	$\sim_1\sim_2 p \supset p$	$\sim_2\sim_1 p \supset p$	$\sim_2\sim_2 p \supset p$
F	t	t	t	t
t	t	t	t	t
T	t	t	t	t

1	12	13	14	15
p	$p \supset \sim_1\sim_1 p$	$p \supset \sim_1\sim_2 p$	$p \supset \sim_2\sim_1 p$	$p \supset \sim_2\sim_2 p$
F	t	t	t	t
t	t	F	t	t
T	t	t	t	t

Figure 2: Illustration of a PTS-decision procedure.

Have a look at Figure 2. Notice that the validity of $\sim\sim p \supset p$ already in PIf is corroborated if you look at all lines of translations 8–11. However, the second line of the translation 13 in PIf shows a counter-model for the formula $p \supset \sim\sim p$. This counter-model is no longer allowed in case you turn your eyes to $PIfe$, as the set of possible translations for this logic does not include those translations that produce columns 5, 6, 9, 10, 13 and 14. As a byproduct of the 'bisimulative' proofs of the above mentioned convenience and representability results in the paper, the reader can see that there is a transformation taking each line and each translation (that is, a partial PTS-model) of a PTS-decision procedure into a corresponding line of a quasi matrix (that is, a partial bivaluational model), and, conversely, a (usually non-surjective) transformation taking each line of a quasi matrix into a line and a translation of a PTS-decision procedure. These transformations were also discussed in detail in section 2.3.3.7 of [23].

Não tem tradução

Say that a PTR/PTS has a *fixed vocabulary* in case all of its factors are identical —you might well consider a set of different translations from a source logic having the same logic as target. Each translation still provides you, in principle, with a different *scenario* for the evaluation of your original logic. Recall from PTSurvey (**Chapter 2.1**) that a semantics is called *unitary* in case its set of bivaluations or its set of translations is a singleton, and a semantics is called *large* in case it contains at least as many valuations or factors as formulas of the underlying language of the logic being interpreted. Moreover, following [19], call a translation *literal* in case it leaves atomic sentences unaltered, and call it *grammatical* in case it takes each connective of the source logic into a 'homonymous' connective of the target logic, that is, in case it is based on convenient homomorphisms between the underlying algebras of formulas.

From PTSurvey, we know that each valuation by itself determines a logic (based on a unitary semantics). So, classical logic, for instance, can alternatively be characterized by a simple PTS $\langle \mathsf{Log}, \mathsf{Tr} \rangle$ with a fixed vocabulary $\langle \mathcal{S}_k, \vDash_k \rangle$ such that: (a) every $\mathcal{S}_k = \mathsf{Alg}(\{\top, \bot\}, \sim, \wedge, \vee, \supset)$, where Alg is the algebra freely generated by the binary symbols $\sim, \wedge, \vee, \supset$ over the carrier $\{\top, \bot\}$, with all symbols interpreted as in classical logic, and $\vDash_k$ is defined accordingly; (b) $\mathsf{Tr} = \{t_j : \mathcal{S} \to \mathcal{S}_k\}_{j \in J}$ is the set of all mappings t_j such that:

$t_j(p) \in \{\top, \bot\}$, for p atomic,

$t_j(\sim\alpha) = \sim t_j(\alpha)$,

$t_j(\alpha \bowtie \beta) = t_j(\alpha) \bowtie t_j(\beta)$, for $\bowtie \in \{\wedge, \vee, \supset\}$.

Notice that atomic sentences are thereby translated into constants, or 0-ary connectives. It is easy to see that the above structure provides a fixed vocabulary and a set of non-literal grammatical translations that characterizes a large adequate PTS for **CPL**, alternative to the more usual set of bivaluations presented in the last subsection. Such PTS is not that terribly interesting, but it does provide a characterization for **CPL** in terms of a factor that contains no atomic sentences, so that the talk about 'propositions' in classical logic turns to be just a *façon de parler*, nothing deeper than that. The above structure also exemplifies the kind of construction that stems from the general adequacy results from the first paper of this chapter.

More interestingly, in WeakPTS (**Chapter 2.2**), a few large PTS with fixed vocabularies and based on a collection of 'informative' literal and grammatical translations were shown to adequately *split* a number of non-finitely-valued paraconsistent logics into 3-valued scenarios. Many more illustrations of that same phenomenon were exhibited in the last few years, as applied to much more complicated paraconsistent logics (cf. [23, 13]). But there is more. As it was shown in [23, 16], our PTS can also be used, for instance, to *splice* a new logic as the deductive limit of a sequence of other logics (for that effect we might let the vocabulary vary over the sequence and take the identity mappings as translations). Such a 'limit logic' might in fact happen to be quite strange (not compact, for instance) and difficult to characterize by other means than a PTS. As pointed out in the **Errata** to the previous chapter of this thesis, in the case of the original sequence of daCostian logics, C_n, $1 \leq n < \omega$, its deductive limit C_{Lim} provided us in fact with an example of an **LFI** that is not a **C**-system, given that consistency is not characterizable in this logic by a single unary connective, nor by any finite set of unary connectives.

If we recall that we are talking here about a certain way of *combining logics*, the immediate question as to which properties *transfer* from the factors into the logic defined by the PTR/PTS can be raised (cf. [29]). That is not easy to answer, though, if you consider the generality of our definitions (and they can be made more general, for instance, if you just change the underlying formalism or if you start considering logics as richer structures such as Pi-institutions instead of those simpler structures based on arbitrary sets of formulas and consequence relations of a certain sort). Given our current definition of a translation, at least soundness is sure to be guaranteed for the source logic of a PTR/PTS. Some other transference results can be investigated, in particular cases. For example, in the specific case of PTS based on finite-valued factors and recursive translations that produce a limited number of possible interpretations for each formula, the resulting structures were shown in the above subsection to preserve decidability. On the other hand, even for the case of identity translations, a non-fixed vocabulary of non-finitely-valued logics was able to produce, as we saw just above, a counter-example to the preservation of compacity. There is a whole area or research here still wide open for exploration.

But there *is* more (and this point is really worth emphasizing, in case one might still retain the wrong idea): Despite the circumstantial fact that PTS have been used most of the time up to now to provide adequate semantics for some rather recalcitrant logics, the scope of application of this tool is surely not limited to paraconsistent logics plus some not very informative examples such as the one of **CPL** at the beginning of the present subsection. In [16], for instance, the construction was dualized so as to apply to paracomplete logics as well. In [23, 22] similar characterizations were used for providing non-simple non-literal PTS for splitting many-valued logics of all sorts as combinations of 2-valued factors. Many other applications however can be imagined, for less exotic logics, and they *should* be investigated by competent researchers. In [20] the idea of using a sort of PTR for producing answers to 'general set-representation problems for algebras' is put forward. Indeed, if a logic has no usual algebraization in the sense of Blok-Pigozzi (cf. [7]), can we use an adequate PTR so as to split its algebraization problem in terms of the algebraization problems of simpler factors of it? Some initial investigations in that direction were reported in [8]. Will this alternative generalized style of algebraization help us solve interesting problems and prove some interesting new bridge theorems between logic and algebra? This line seems worth investigating. And similarly for proof-theoretic presentations: Will our PTRs help providing interesting new hypersequent-style or labelled tableaux for otherwise unruly logics? Some results in that direction were already reported in [2] for a particular class of PTS called 'non-deterministic semantics'. This line of investigation also definitively seems worth pursuing. As a matter of fact, the present version of WeakPTS does already hint at a procedure according to which any non-deterministic semantics could be recast in terms of a possible-translations semantics. In a recent paper, [1], Avron shows how our logics **bC** and **Ci** can be endowed with 3-valued non-deterministic semantics, and how the logic **Cila** (da Costa's C_1) cannot be given a finite-valued non-deterministic semantics, but only an infinite-valued one —with a 'locality property' that guarantees that it has an appropriate associated decision procedure. This clearly contrasts with the situation in possible-translations semantics, where 3-valued factors are known to be enough for characterizing **Cila** (cf. [23]). The relations between these two genres of semantics should still be studied with due care.

Brief history

The paper PTSurvey started as a research note (cf. [26]) on May 2003. Though the term 'possible-translations semantics' (PTS) had appeared as early as 1990 (cf. [12]), and my Master's Thesis was dedicated to the theme in 1999, none of the papers published on the topic so far had offered a clear-cut general definition of the term, and no real study had been made of the very scope of application of the PTS. My note aimed at filling those blanks, for the benefit of newcomers to the scene, whose first question would invariably be: But what *is* a PTS, after all?

On what concerns the general definition, I had been trying my hands on it since 1998, and I gave some talks on my evolving view of that topic at a few venues, on invitation: at the State University of Campinas (BR) in March 1999, at the occasion of a scientific visit of our ProBrAl German partners to our group; at the Rand Africaans University, in Johannesburg (ZA) in December 1999; at the Max Planck Institute in Saarbrücken (DE) in October 2000; at the University of Ghent (BE) in March 2001; at the University of Łódz (PL) in September 2002. On what concerns its scope of application, by the beginning of 2003 I was convinced that multiple-conclusion consequence relations and abstract logics should be used as the underlying framework for that study, for the sake of generality and symmetry. I had been working on multiple-conclusion consequence relations since 2002, and my interest on abstract logics had been on the increase since much longer —furthermore, I had just participated at the end of 2002 (while I was living in Brazil for a while, working under a CNPq doctoral grant) of the research and writing of the papers [10, 9], which expanded on an earlier draft of mine (cf. [22]) and relied strongly on that abstract sort of approach. As it was revealed in the paper [17] and as it had been explored in categorial terms by Carnielli & Coniglio in [14], our intuition was that the notion of a PTS could be seen as a way of combining logics —or, even better, a way of 'discombining' them. To describe that opposition we had coined in 1999 the terms 'splicing logics' and 'splitting logics'.

Portugal was by then probably the best place in the world for combining-logicians. After two short scientific visits to the Center for Logic and Computation (CLC) of the IST in January 2001 and January 2002, I was invited to come and work there as a student member of the CLC for a while, and that's what I did, from March 2003 on, under an FCT doctoral grant. A workshop on combination of logics (CombLog'04) was being organized in Lisbon for July 2004, thus I decided to submit an improved version of my PTS-note to it, and this contribution would in fact be accepted and published there as an extended abstract (cf. [24]) a few months later. The present version of the material, in this thesis, is a variation of that extended abstract, after the correction of a few inaccuracies and the addition of the proofs of all main claims.

The research report WeakPTS was written in Portugal around October 2003. We had all these new paraconsistent logics sprouting from **Chapter 1.0**, and we had the intuition that they could in general be endowed with adequate PTS with 3-valued factors, as it had been done earlier with some stronger samples among these logics, in [23, 13, 16]. We were also badly in need of characterizing beyond any doubt, syntactically and semantically, the weaker Logics of Formal of Inconsistency that we had created, such as **mbC**, **mCi**, **bC** and **Ci**. Both tasks were elegantly accomplished in the above mentioned report, whose results were heavily used in [15]. Those results were polished and corrected along the subsequent months until the present version of the research report, from November 2004. In helping me spot the mistakes I am much obliged to the unfailing interventions of Marcelo Coniglio.

The continuous support I received from Walter Carnielli and Carlos Caleiro, and the heated debates (plus the pingponging) I had with Arnon Avron at the Dagstuhl Castle (DE) in June 2003 and at the IST (PT) in March 2004 were also nothing less than essential to the design of the current version of this chapter.

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Possible-Translations Semantics

(Extended Abstract)

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Abstract

This text aims at providing a bird's eye view of possible-translations semantics ([10, 24]), defined, developed and illustrated as a very comprehensive formalism for obtaining or for representing semantics for all sorts of logics. With that tool, a wide class of complex logics will very naturally turn out to be (de)composable by way of some suitable combination of simpler logics. Several examples will be mentioned, and some related special cases of possible-translations semantics, among which are society semantics and non-deterministic semantics, will also be surveyed.

1 Logics, translations, possible-translations

Let a *logic* $\mathcal{L}$ be a structure of the form $\langle \mathcal{S}, \Vdash \rangle$, where $\mathcal{S}$ denotes its *language* (its set of *formulas*) and $\Vdash \subseteq \mathsf{Pow}(\mathcal{S}) \times \mathsf{Pow}(\mathcal{S})$ represents its associated *consequence relation* (cr), somehow defined so as to embed some formal model of reasoning. Call any subset of $\mathcal{S}$ a *theory*. As usual, capital Greek letters will denote theories, and lowercase Greek will denote formulas; a sequence such as $\Gamma, \alpha, \Gamma' \Vdash \Delta', \beta, \Delta$ should be read as asserting that $\Gamma \cup \{\alpha\} \cup \Gamma' \Vdash \Delta' \cup \{\beta\} \cup \Delta$.

Morphisms between any two of the above structures will be called *translations*. So, given any two logics, $\mathcal{L}_1 = \langle \mathcal{S}_1, \Vdash_1 \rangle$ and $\mathcal{L}_2 = \langle \mathcal{S}_2, \Vdash_2 \rangle$, a mapping $t : \mathcal{S}_1 \to \mathcal{S}_2$ will constitute a translation from $\mathcal{L}_1$ into $\mathcal{L}_2$ just in case the following holds:

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(T1) $\Gamma \Vdash_1 \Delta \Rightarrow t(\Gamma) \Vdash_2 t(\Delta)$

A translation is said to be *conservative* in case the converse of (T1) holds, i.e.:

(T2) $\Gamma \Vdash_1 \Delta \Leftarrow t(\Gamma) \Vdash_2 t(\Delta)$

Given a logic $\mathcal{L}=\langle\mathcal{S},\Vdash\rangle$, a *possible-translations representation* (ptr) over it is a structure of the form $\langle\mathsf{Log},\mathsf{Tr},\mathsf{Reg}\rangle$, where $\mathsf{Log}=\{\langle\mathcal{S}_j,\Vdash_j\rangle\}_{j\in J}$ is an indexed set of logics (also called *factors* or *ingredients* of this ptr), $\mathsf{Tr}=\{t_j:\mathcal{S}\to\mathcal{S}_j\}_{j\in J}$ is an indexed set of translations, and $\mathsf{Reg}\subseteq\mathsf{Pow}(\mathsf{Tr})$. To any such ptr one can immediately associate three levels of consequence relations: A *local* pt-cr, $\Vdash^j_{\mathsf{pt}}$, for each $t_j\in\mathsf{Tr}$, a *regional* pt-cr, $\Vdash^R_{\mathsf{pt}}$, for each $R\in\mathsf{Reg}$, and a *global* pt-cr, $\Vdash_{\mathsf{pt}}$. These relations will be defined by setting:

(L-pt) $\Gamma\Vdash^j_{\mathsf{pt}}\Delta$ iff $t_j(\Gamma)\Vdash_j t_j(\Delta)$

(R-pt) $\Gamma\Vdash^R_{\mathsf{pt}}\Delta$ iff $(\backepsilon t_j\in R)[\Gamma\Vdash^j_{\mathsf{pt}}\Delta]$,
where $\backepsilon$ is some (generalized) quantifier

(G-pt) $\Gamma\Vdash_{\mathsf{pt}}\Delta$ iff $(\forall R\in\mathsf{Reg})[\Gamma\Vdash^R_{\mathsf{pt}}\Delta]$

Obviously, (L-pt) is just a particular case of (R-pt). Taking $\mathsf{Reg}=\{\{t_j\}:t_j\in\mathsf{Tr}\}$ makes the regional pt-cr perfectly dispensable —we will call any ptr with that characteristic a *simple* ptr and write it more simply as $\langle\mathsf{Log},\mathsf{Tr}\rangle$. There are usually many ways of obtaining the same global pt-cr. Suppose for instance that '$\backepsilon=\forall$' in (R-pt). Then, $\Vdash_{\mathsf{pt}}$ will be exactly the same, for every Reg such that $\bigcup\mathsf{Reg}\supseteq\mathsf{Tr}$.

Given two logics $\mathcal{L}_1=\langle\mathcal{S}_1,\Vdash_1\rangle$ and $\mathcal{L}_2=\langle\mathcal{S}_2,\Vdash_2\rangle$, we will say that $\mathcal{L}_1$ is *sound* with respect to $\mathcal{L}_2$ in case $\Vdash_1\subseteq\Vdash_2$. Similarly, we will say that $\mathcal{L}_1$ is *complete* with respect to $\mathcal{L}_2$ in case $\Vdash_1\supseteq\Vdash_2$. Notice that translations can be endomorphisms. In particular, any logic is sound and complete with respect to itself, the identity endomorphism always constituting thus a trifling example of a ptr. A ptr over a logic $\mathcal{L}=\langle\mathcal{S},\Vdash\rangle$ is said to be *adequate* in case $\mathcal{L}$ is sound and complete with respect to $\langle\mathcal{S},\Vdash_{\mathsf{pt}}\rangle$. Thus, an adequate ptr can be seen as a way of combining a set of translations so as to obtain a very particular conservative translation. Finally, a *possible-translations semantics* (pts) is simply a possible-translations representation in which all factors are defined by 'semantic means' (in contrast to, say, 'abstract deductive' or 'proof-theoretical' means). This characterization certainly looks very vague, but I will show in more detail in the following subsections how the canonical semantic notions work and how they can be seen as special cases of simple pts, according to the above definitions.

One last methodological discrimination is sometimes useful. In case one starts with a logic $\mathcal{L}$ and then finds a set of factors for it in an adequate ptr, one will call the process *splitting logics*; in case one starts with the factors and then build a logic for which the corresponding ptr is adequate, the process will be called *splicing logics*. The immense majority of examples from the literature on *combining logics* is of a more synthetic character: More and more logics are spliced as time goes by. Here, on the contrary, it will be often natural to use ptr's in order to analyze some given logics, splitting them into simpler components in order to understand them. *Frango ut patefaciam.*

Digression 1.1 (*Categorial*) If one considers the category where logics are the objects and translations are the arrows, the diagrams we get for the ptr's all look like there were sunbeams irradiating from a common core. The logic that originates from the combination can be seen as the colimit of this diagram. In [11] the authors show how to generalize this construction for arbitrary diagrams. This should be compared to what is done in [29] in understanding *fibring* (a more general form of combination, check [23, 4]) as a categorial construction. A first advance in that direction, generalizing the basic construction of fibring, can be found in [16]. A different semantically-driven generalization of fibring, *cryptofibring*, is categorially investigated in [7]. □

Digression 1.2 (*Historical*) Possible-translations semantics were first introduced in [9], restricted to the use of finite-valued truth-functional factors. The embryo was then frozen for a period, and in between 1997 and 1998 it was publicized under the denomination 'non-deterministic semantics', in [12], and in several talks by Carnielli and a few by myself. Noticing that the non-deterministic element was but a particular accessory of the more general picture, from 1999 on the semantics retook its earlier denomination ([10, 24, 14, 15, 26]). □

1.1 What is a logic?

To be sure, this is a question that will *not* be answered in this section. Any number of answers to it can be found in the literature, if you dig hard enough. I will here instead recall how some among the most popular answers can be recast in the present framework.

Given a logic $\mathcal{L} = \langle \mathcal{S}, \Vdash \rangle$ as above, we will call it an MCT logic in case its cr is subject to the following restrictions:

(C1)	$(\Gamma, \varphi \Vdash \varphi, \Delta)$	(overlap)
(C2)	$(\Gamma \Vdash \varphi, \Delta)$ and $(\Gamma', \varphi \Vdash \Delta') \Rightarrow (\Gamma', \Gamma \Vdash \Delta, \Delta')$	(cut)
(C3)	$(\Gamma \Vdash \Delta) \Rightarrow (\Gamma', \Gamma \Vdash \Delta, \Delta')$	(dilution)

Call any clause of the form $\Gamma \Vdash \Delta$ an *inference*. Theories that appear at the left-hand side of the $\Vdash$ are also dubbed *countertheories*, or *premises* assumed by the inference; theories that appear at the right-hand side of the $\Vdash$ are also called *alternatives* sanctioned by the inference. An SCT consequence relation (cf. [31]) is a particular case of an MCT consequence relation, in which each inference has a single formula as alternative (no real 'alternative' in that case, is it?). Such alternative is often called *conclusion* of the inference. SCT logics are also called *single-conclusion*, in contrast to the more symmetrical (*multiple-premise*) *multiple-conclusion* MCT logics. It would be just as natural, of course, to consider here a SPMCT logic to be defined by the same restrictions above, but on a single-premise-multiple-conclusion environment. Very uncommon in practice, the SPMCT case works pretty much like the SCT case in most circumstances. Below I will only mention SPMCT logics explicitly, thus, when relevant.

Here are some degenerate examples of logics. Let a logic $\langle \mathcal{S}, \Vdash \rangle$ be called *overcomplete* in case its cr is characterized by one of the following universal properties:

(C0.0.0)	$(\Gamma \Vdash \Delta)$	(triviality)
(C0.0.1)	$(\Gamma, \alpha \Vdash \Delta)$	(nihilism)
(C0.1.0)	$(\Gamma \Vdash \beta, \Delta)$	(dadaism)
(C0.1.1)	$(\Gamma, \alpha \Vdash \beta, \Delta)$	(semitriviality)

Note, by the way, that THE trivial logic is characterized by the nonproper cr over the language $\mathcal{S}$. Clearly, SCT logics must identify trivial and dadaistic logics, and identify nihilistic and semitrivial logics. When we talk about THE dadaistic logic in a given language we will be referring to the logic having a non-trivial dadaistic cr. Similarly, THE nihilistic logic will refer to the logic having a non-trivial nihilistic cr, and THE semitrivial logic will denote the logic having a non-dadaistic non-nihilistic cr.

A formula β of a logic $\mathcal{L}$ is said to be a *thesis* of this logic in case $(\Gamma \Vdash \beta, \Delta)$, for any choice of Γ and Δ; an *antithesis* of this logic is any formula α such that $(\Gamma, \alpha \Vdash \Delta)$, for any choice of Γ and Δ. An arbitrary thesis is sometimes denoted by $\top$, and an arbitrary antithesis is sometimes denoted by $\bot$.

Theorem 1.1.1 (i) Every multiple-conclusion overcomplete logic is MCT. Every single-conclusion overcomplete logic is SCT.
(ii) The empty language defines a unique MCT / SCT logic.
(iii) Any arbitrary intersection of MCT / tarkian logics defined over some fixed language defines a MCT / SCT logic.

Proof:
(i): Just check that properties (C1)–(C3) of a MCT / SCT logic hold for each of the above four kinds of overcomplete logics.
(ii): Indeed, in the MCT case, $\mathsf{Pow}(\varnothing)\times\mathsf{Pow}(\varnothing) = \{\langle\varnothing,\varnothing\rangle\}$ and $\langle\varnothing,\langle\varnothing,\varnothing\rangle\rangle$ is obviously trivial. Similarly for the SCT case.
(iii): Given some language $\mathcal{S}$ and any indexed set of MCT / SCT logics $\{\langle \mathcal{S}, \Vdash_i\rangle\}_{i\in I}$, it is easy to see that $\langle \mathcal{S}, \bigcap_{i\in I}(\Vdash_i)\rangle$ is also a MCT / SCT logic. In particular, note that, in the MCT case, $\bigcap_{i\in I}(\Vdash_i) = \{\langle\varnothing,\varnothing\rangle\}$ iff $(\mathcal{S} = \varnothing)$, and then you're in case (ii); besides, note that the condition $I = \varnothing$ puts you directly in case (i). Similarly for the SCT case. $\square$

Theorem 1.1.2 Fix some MCT / SCT logic $\mathcal{L}$ over some non-empty language $\mathcal{S}$. Then:
(i) $\mathcal{L}$ is the trivial logic iff there is at least one formula in its language which is both a thesis and an antithesis of $\mathcal{L}$.
(ii) $\mathcal{L}$ is the nihilistic logic iff all of its formulas are antitheses of it.
(iii) $\mathcal{L}$ is the dadaistic logic iff all of its formulas are theses of it.
(iv) $\mathcal{L}$ is the semitrivial logic iff any formula implies any other (or the same) formula, but no antitheses nor theses are present in the language of this logic.

Proof: Immediate. □

Several other restrictions and extensions of the above notion of logic are studied in [25], from an abstract viewpoint. As in that paper, a logic here will be called *minimally decent* in case it is not overcomplete.

1.2 What is the canonical notion of entailment?

Let $\mathcal{V}$ denote an arbitrary set of *truth-values*, where $\mathcal{D}^{\mathcal{V}} \subseteq \mathcal{V}$ denotes its subset of *designated* values (the 'true truth-values'), and $\mathcal{U}^{\mathcal{V}} = \mathcal{V} \setminus \mathcal{D}^{\mathcal{V}}$ denotes its subset of *undesignated* values (the 'false truth-values'). Given a language $\mathcal{S}$, let a *valuation* over it be any mapping $\S^{\mathcal{V}} : \mathcal{S} \to \mathcal{V}$. Call any collection of valuations over $\mathcal{S}$ a (MCT) *semantics* sem over $\mathcal{S}$. This semantics will be called *κ-valued* if κ is the greatest cardinality of truth-values of the valuations in sem, that is, $\kappa = \sup_{\S^{\mathcal{V}} \in \mathsf{sem}}(|\mathcal{V}|)$. To any valuation $\S^{\mathcal{V}} \in \mathsf{sem}$ and any semantics sem one can associate *canonical* notions of *local entailment*, $\vDash^{\S}_{\mathsf{sem}}$ and *global entailment*, $\vDash_{\mathsf{sem}}$, by setting:

(L-ce) $\quad \Gamma \vDash^{\S^{\mathcal{V}}}_{\mathsf{sem}} \Delta$ iff $(\S^{\mathcal{V}}(\Gamma) \cap \mathcal{U}^{\mathcal{V}} \neq \varnothing$ or $\S^{\mathcal{V}}(\Delta) \cap \mathcal{D}^{\mathcal{V}} \neq \varnothing)$

(G-ce) $\quad \Gamma \vDash_{\mathsf{sem}} \Delta$ iff $(\forall \S^{\mathcal{V}} \in \mathsf{sem})[\Gamma \vDash^{\S^{\mathcal{V}}}_{\mathsf{sem}} \Delta]$

An *ordinary* MCT semantics is one in which a fixed cardinal of designated / undesignated values is set throughout all the valuations of the semantics. Obviously, any semantics can be made ordinary by just adding to each valuation a convenient number of truth-values that will not be used. Similarly to above, a SCT (*ordinary*) *κ-valued semantics* will be defined just like an MCT (ordinary) κ-valued semantics, only that all inferences will have exactly one formula at their right-hand sides.

Theorem 1.2.1 (i) Any MCT / SCT κ-valued semantics induces at least one MCT / SCT logic by way of one of its associated canonical entailment relations.
(ii) Consider any covering of the valuations of a given MCT / SCT semantics. Each layer of the covering can now be said to determine a new (universal) 'regional semantics', and the intersection of all the entailments associated to the latter gives you back the global entailment.

Proof: (i): It is easy to check that any $\vDash$ defined as in (L-ce) or in (G-ce) respects the properties (CR1)–(CR3). Note that this holds good irrespective of κ or of the number of valuations in sem.
(ii): Just recall Theorem 1.1.1(iii). $\square$

Given the above results, one sees that any semantic structure of the form $\langle\mathcal{S},\vDash\rangle$ defines an MCT and a SCT logic, and the logics corresponding to the global entailment relation can be obtained through the intersection of all local (or regional) entailment relations. As before, given a logic $\mathcal{L}=\langle\mathcal{S},\Vdash\rangle$ and a semantics sem over $\mathcal{S}$, one can now very naturally talk about $\mathcal{L}$ being *locally sound* with respect to some $\S\in\mathsf{sem}$ in case $\Vdash\subseteq\vDash^{\S}_{\mathsf{sem}}$, and being *globally sound* with respect to sem in case $\Vdash\subseteq\vDash_{\mathsf{sem}}$. Similarly for local and global completeness and adequacy. The statement of the following result parallels that of Theorem 1.1.2.

Theorem 1.2.2 Here is how you can obtain adequate ordinary semantics for each variety of overcomplete logic:
(i) For the trivial logic, consider the empty semantics (empty set of truth-values).
(ii) For the nihilistic logic, consider some semantics whose valuations make everything false.
(iii) For the dadaistic logic, consider some semantics whose valuations make everything true.
(iv) For the semitrivial logic, consider some semantics whose valuations either make everything true or make everything false.

Proof: Let $\mathcal{S}$ be an arbitrary fixed language, let $\mathcal{D}_n$ and $\mathcal{U}_n$ be pairwise disjoint arbitrary sets of truth-values, for each $1\leq n\leq 4$, such that $\mathcal{U}_2\neq\varnothing$, $\mathcal{D}_3\neq\varnothing$, $\mathcal{D}_4\neq\varnothing$ and $\mathcal{U}_4\neq\varnothing$. For each n, let $\mathsf{val}(\mathcal{D}_n)=\{\S:\S(\mathcal{S})\subseteq\mathcal{D}_n\}$ denote the sets of all valuations over $\mathcal{S}$ whose counterdomains range only over designated values, and let $\mathsf{val}(\mathcal{U}_n)=\{\S:\S(\mathcal{S})\subseteq\mathcal{U}_n\}$ do a similar thing for undesignated values. Consider now semantics such that $\mathsf{sem}_1\subseteq\mathsf{val}(\mathcal{D}_1)\cap\mathsf{val}(\mathcal{U}_1)=\varnothing$, $\mathsf{sem}_2\subseteq\mathsf{val}(\mathcal{U}_2)$, $\mathsf{sem}_3\subseteq\mathsf{val}(\mathcal{D}_3)$ and $\mathsf{sem}_4\subseteq\mathsf{val}(\mathcal{D}_4)\cup\mathsf{val}(\mathcal{U}_4)$. It is easy, then, to check that: (i) sem_1 is adequate for the trivial logic; (ii) sem_2 is adequate for the nihilistic logic; (iii) sem_3 is adequate for the dadaistic logic; (iv) sem_4 is adequate for the semitrivial logic. $\square$

1.3 What can be done with translations between logics?

The general definitions of translation and of conservative translation that you found at the beginning of the present section were studied in detail in [12, 19], and interesting specializations of these notions were proposed in [20]. Typical examples of everyday translations are given by the endomorphisms that define uniform substitutions in a logic whose language is formed by a free algebra (of formulas). One can here also easily check that:

Theorem 1.3.1 (i) A logic can always be conservatively translated into itself.
(ii) To check soundness or completeness of a given logic with respect to some MCT / SCT semantics amounts to checking the identity mapping from the language into itself to be a translation.

Proof: (i): Just consider the identity mapping $t : \varphi \mapsto \varphi$, for every $\varphi \in \mathcal{S}$.
(ii): Considering a logic $\mathcal{L}_a = \langle \mathcal{S}, \Vdash \rangle$ and a SCT semantic structure $\mathcal{L}_b = \langle \mathcal{S}, \vDash \rangle$, to show that $\mathcal{L}_a$ is sound with respect to $\mathcal{L}_b$ you have to show that the identity mapping, as in part (i), is a translation from $\mathcal{L}_a$ into $\mathcal{L}_b$. Similarly, to show that $\mathcal{L}_a$ is complete with respect to $\mathcal{L}_b$ your task is showing that the identity mapping is a translation from $\mathcal{L}_b$ into $\mathcal{L}_a$. $\square$

Here are some degenerate examples of translations:

Theorem 1.3.2 For arbitrary logics (not necessarily MCT nor SCT) over some fixed language $\mathcal{S}$:
(i) Any logic is translatable into the trivial logic.
(ii) Any single-conclusion logic is translatable into any logic having a thesis. Any single-premise logic is translatable into any logic having an antithesis.
(iii) The dadaistic logic is conservatively translatable into any logic having a thesis. The nihilistic logic is conservatively translatable into any logic having an antithesis. The semitrivial logic is conservatively translatable into any logic respecting (C1) and having no theses and no antitheses.
(iv) Given a logic with no (anti)theses at all, NO logic having a(n anti)thesis whatever is translatable into the former.
(v) Any logic having no theses nor antitheses is translatable into the semitrivial logic.

Proof: (i): Choose any $\alpha \in \mathcal{S}$, and set $t : \varphi \mapsto \alpha$, for every $\varphi \in \mathcal{S}$.
(ii): For the first part, set $t : \varphi \mapsto \top$, for every $\varphi \in \mathcal{S}$. For the second part, $t : \varphi \mapsto \bot$ will do the job.
(iii): Similar to (ii).
(iv): Let $\langle \mathcal{S}_1, \Vdash_1 \rangle$ be a logic with a thesis $\top$, and let $\langle \mathcal{S}_2, \Vdash_2 \rangle$ be a logic with no thesis. If there would be some translation $t : \mathcal{S}_1 \to \mathcal{S}_2$, then, in particular, $\Vdash_2 t(\top)$ would need to hold, but there is no formula in $\mathcal{L}_2$ with that property. Similarly for an antithesis.
(v): Exercise. $\square$

Problem 1.3.3 For more esoteric non-MCT logics, such as non-monotonic logics and other context-dependent applications it might seem more natural to work with a definition of translation that directly involves the inferences, instead of the formulas. In that case, a translation from $\langle \mathcal{S}_1, \Vdash_1 \rangle$ into $\langle \mathcal{S}_2, \Vdash_2 \rangle$ had better be defined, say, as a mapping $t : \mathsf{Pow}(\mathcal{S}_1) \to \mathsf{Pow}(\mathcal{S}_2)$ instead of $t : \mathcal{S}_1 \to \mathcal{S}_2$, as before. It might be better as well to think of a logic directly as a set of theories, instead of a set of formulas, endowed with a consequence relation. The properties of this sort of definitions are yet to be investigated in more detail. An advance in that direction was already made in [17], where the authors conceive SCT logics as two-sorted first-order structures (the sort of 'formulas' and the sort of 'theories'), and talk about 'transfers' as morphisms among those structures (of which translations between SCT logics, in the above sense, are but particular cases).

1.4 What are possible-translations semantics?

We have defined above the notion of a possible-translations representation (ptr) based on the combination of a collection of factors through local ($\Vdash^j_{\mathsf{pt}}$), regional ($\Vdash^R_{\mathsf{pt}}$) and global ($\Vdash_{\mathsf{pt}}$) consequence relations (cr). A possible-translations semantics (pts) was then characterized as a ptr based on factors defined by 'semantic means'. Moreover, the above sections have shown a conventional rendering of the received notion of 'semantics', slightly generalized in accordance with the principles of the theory of valuations (cf. [18]) and of abstract multiple-conclusion deductive systems (cf. [32, 30]).

There are several ways of combining logics. In a very pleasant paper, [3], Blackburn and de Rijke survey the reasons one might have for splicing logics, and propose a catalogue of the forms of combination based on the increasing level of involvement of the ingredient logics: They come up with nice pictures for 'refining structures', then 'classification structures', then 'totally fibred structures'. Another taxonomy is delineated at [8, 4, 28], where 'synchronization' and 'parameterization' appear as distinguished special cases of 'fibring'. How would the general picture for the combination through a possible-translations representation look like?

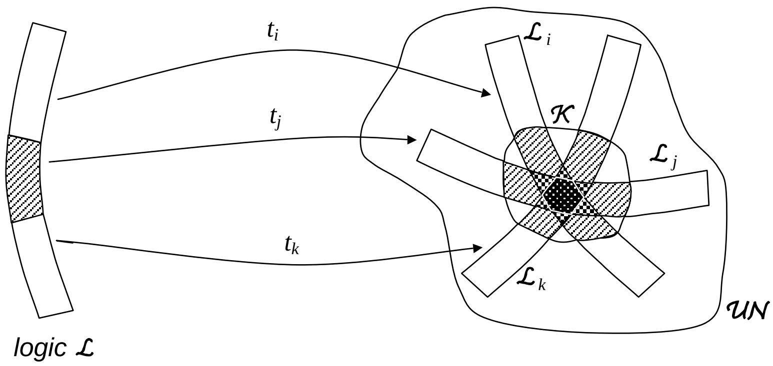

Figure 1: *The logical Rosetta Stone.*

An insightful analogy may be provided by concentrating on the situation in which a logic is split into its simpler components and comparing it to the deciphering of the 'Rosetta Stone' (cf. [15]). Carved in 196B.C. and found by Napoleon troops in July 1799 near the homonymous village (Rashid) located in the western delta of the Nile, the Rosetta Stone is a basalt slab containing three different inscriptions of a text written by a group of priests to honor the Egyptian pharaoh. Why is it important? Because it finally allowed scholars to decipher the Hierogliphic writing, a problem that had been open for several hundred years! After the work of Thomas Young, a British physicist, and Jean-François Champollion, a French Egyptologist, the code was finally broken, and a phonetic value was attached to hieroglyphs that had previously been thought to have a purely symbolic value. How was it done? The three scripts in the stone were the Hieroglyphic (used for important or religious documents), the Demotic (everyday Egyptian script) and the Greek (language of the rulers of Egypt at that time). With the aid of both Greek and Coptic (language of the Christian descendants of the ancient Egyptians), Champollion was able to decipher the Demotic writing, and from that he was able to trace back the meaning of the Hierogliphic signs. But how did they know that the three scripts represented the same text, to start with? Because the stone *said so*, at the very end of its Greek inscription! Another beautiful example of self-reference, therefore.

Based on the above story, Figure 1 gives a schematic illustration of what is going on when a ptr is designed. The Rosetta Stone is the 'logical universe' $\mathcal{UN}$ where all ingredient logics can be found, resembling perhaps an egg with the sunny side up. The long curved format of the logic represents the form of reasoning sanctioned by it. You can see that the morphisms (possible-translations) are intended to preserve that format. At a distinguished hachured region of each logic you may find its circumstantial theses and antitheses. Each translation should in particular take theses into theses, and antitheses into antitheses. The region where they can be found in $\mathcal{UN}$ is at its yolk $\mathcal{K}$. The appetizing part is the one in which the ingredients are cooked together so as to give us the corresponding possible-translations structure.

The next result shows some simple examples of ptr and pts:

Theorem 1.4.1 (i) Any logic has an adequate possible-translations representation.
(ii) Any (MCT / SCT) semantics can be seen as a possible-translations semantics with any positive number of factors.

Proof: (i): Just consider the identical mapping from the language into itself.
(ii): For any given semantic structure, you can define the natural 1-factor pts by way of the identical mapping, which does exactly the same job as the former semantics —though it does not really tell you more than you already knew. Now, assume you have a MCT / SCT semantics $\mathsf{sem} = \{\S_k\}_{k\in K}$. In case there is more than one valuation in sem, a second natural pts is obtainable in any case if you pick the single-valuation SCT semantics $\mathsf{sem}_k = \S_k$, for each $k \in K$, and consider as translations $|K|$ applications of the

identical mapping. Any pts that extends one of the above natural possible-translations semantics by the addition of redundant factors and translations leaves the resulting global pt-entailment untouched. □

One can count now on a more sophisticated interplay between local and global notions at hand: If an MCT / SCT semantics can be seen as a general way of gluing arbitrary collections of valuations, a possible-translations semantics can be seen as a more general way of gluing collections of any arbitrary kind of previously given semantics.

Call a semantics *unitary* in case it is defined by way of a single valuation, or a single factor; call it *large* in case the cardinality of the set of valuations or the set of factors is at least as big as the cardinality of the underlying language. Obviously, any unitary semantics is ordinary from its very inception; unitary semantics can be made large, and large semantics can always be made ordinary at request, by the addition of redundant valuations or truth-values. We already knew from Theorem 1.2.1(ii) than any MCT / SCT semantics can be reduced to the intersection of unitary MCT / SCT semantics; the last result above suggests now that any semantics can ultimately and quite naturally be converted into a large possible-translations semantics whose factors are all unitary semantics themselves.

Moreover:

Theorem 1.4.2 If you are talking about logics characterized by MCT / SCT entailments, or by simple possible-translations representations:
(i) Global soundness implies local soundness.
(ii) Local completeness implies global completeness.
In overcomplete logics:
(iii) Local soundness automatically transfers to global soundness.
(iv) Global completeness automatically transfers to local completeness.

Proof: Parts (i) and (ii): Just recall the definitions of (L-ce) and (G-ce) (subsection 1.2), (L-pt) and (G-pt) (section 1).
Parts (iii) and (iv): You need no more than 1 valuation to define an overcomplete logic, as we saw in Theorem 1.2.2. □

Note that, in non-overcomplete logics, there is no reason in general for global soundness to be expected to transfer to local soundness, or for local completeness to be expected to transfer to global completeness.

1.5 Which logics have adequate semantics?

Right now we have two things called MCT: The abstract consequence relations characterized by way of clauses (C1)–(C3) in subsection 1.1 and the semantics to which canonical entailment relations were associated in subsection 1.2. A similar thing can be said about abstract SCT consequence relations and SCT semantics. The attentive reader will certainly have noticed, though, that we have not as yet established a relation between the homonymous creatures! This subsection will correct this slip for the benefit of the interested.

Consider first the SCT case. Given a single-conclusion logic $\langle \mathcal{S}, \Vdash \rangle$ and a countertheory $\Pi \subseteq \mathcal{S}$, the *right-closure* of Π, denoted by $\Pi^{\mathbf{c}}$, is the set of all of its derived consequences, that is, the set $\{\pi : \Pi \Vdash \pi\}$.

Theorem 1.5.1 (i) In any SCT logic, $\Pi^{\mathbf{cc}} = \Pi^{\mathbf{c}}$, that is, $\Pi^{\mathbf{c}} \Vdash \pi \Leftrightarrow \Pi \Vdash \pi$. (ii) In any SCT logic $\langle \mathcal{S}, \Vdash \rangle$, given arbitrary $\Sigma \cup \Delta \cup \{\varphi\} \subseteq \mathcal{S}$, to check whether $\Sigma, \Delta \Vdash \varphi$ holds is equivalent to checking whether $(\forall \delta \in \Delta)\Sigma \Vdash \delta$ implies $\Sigma \Vdash \varphi$.

Proof: Immediate. □

Theorem 1.5.2 (*Lindenbaum-like*) Each SCT logic has at least as many (but no less than one) sound SCT unitary semantics as the number of its right-closed theories.

Proof: You have to take the truth-values from somewhere, and all that you have at this point is a logic $\langle \mathcal{S}, \Vdash \rangle$ with its underlying language $\mathcal{S}$ and its cr $\Vdash$. So, given any theory $\Delta \in \mathcal{S}$, take $\mathcal{V} = \mathcal{S}$ and $\mathcal{D} = \Delta^{\mathbf{c}}$ to be, respectively, the sets of truth-values and of designated values. Now, take the unitary semantics sem_Δ given by the identical mapping which takes each formula into itself. This defines a local / global entailment $\vDash_\Delta$ such that $\Gamma \vDash_\Delta \varphi$ iff ($\Gamma \not\subseteq \Delta^{\mathbf{c}}$ or $\varphi \in \Delta^{\mathbf{c}}$). Now, suppose you have (a) some $\Gamma \Vdash \varphi$ such that (b) $\Gamma \subseteq \Delta^{\mathbf{c}}$; all you need now is to show that (c) $\varphi \in \Delta^{\mathbf{c}}$. From ($b$) and (CR1), it follows that (d) $\Delta^{\mathbf{c}} \Vdash \gamma$, for every $\gamma \in \Gamma$. From (a) and (CR3) you have that (e) $\Delta^{\mathbf{c}}, \Gamma \Vdash \varphi$. From ($d$) and ($e$), by repeated applications of (CR2), you conclude that $\Delta^{\mathbf{c}} \Vdash \varphi$. But this finally implies (c), by definition of right-closure and Theorem 1.5.1(i). One defines, thus, a sound semantics corresponding to each right-closed theory of the underlying language. The collection of all such semantics is sometimes referred to as the LINDENBAUM BUNDLE.

Now, even if there are no non-empty theories, as in the case of the empty logic from Theorem 1.1.1(ii), you can count on a sound (and complete) unitary semantics, as in Theorem 1.2.2(i). □

Theorem 1.5.3 (*Wójcicki-like*) Any SCT logic has an adequate semantics.

Proof: Given a SCT logic $\langle \mathcal{S}, \Vdash \rangle$, define $\vDash_\Delta$, for each $\Delta \subseteq \mathcal{S}$, as in Theorem 1.5.2. Next, take the intersection of the Lindenbaum bundle, i.e., of all the unitary semantics thereby induced. Accordingly, define $\vDash = \bigcap_{\Delta \subseteq \mathcal{S}} (\vDash_\Delta)$. Now, such $\vDash$ is obviously sound for $\Vdash$. To check the converse, completeness, assume that $\Gamma \vDash \varphi$. Thus, $\Gamma \vDash_\Delta \varphi$, for every $\Delta \in \mathcal{S}$, and then it follows, by definition of $\vDash_\Delta$, that $(\forall \gamma \in \Gamma)\Delta^{\mathbf{c}} \Vdash \gamma$ implies $\Delta^{\mathbf{c}} \Vdash \varphi$. By part (i) of

Theorem 1.5.1, this amounts to the same as saying that $(\forall\gamma \in \Gamma)\Delta \Vdash \gamma$ implies $\Delta \Vdash \varphi$. But, by part (ii) of the same theorem, this is equivalent to writing $\Delta, \Gamma \Vdash \varphi$. In the particular case where $\Delta = \varnothing$ you will finally find what you want. □

Corollary 1.5.4 Every SCT logic $\langle \mathcal{S}, \Vdash \rangle$ has an adequate ordinary κ-valued semantics, with $\kappa \leq |\mathcal{S}|$. □

The previous result is very general, but a κ-valued semantics is more interesting in case its truth-values are well-behaved with respect to the underlying language, for instance, in case one can count on truth-functionality. The contrast between designated and undesignated values casts though a shadow of *bivalence*. Indeed:

Theorem 1.5.5 (*Suszko-like*) Every SCT logic has an adequate κ-valued SCT semantics, for $\kappa \leq 2$.

Proof: To make things easier, given a κ-valued SCT semantics, first you should make it ordinary. Next, for any κ-valuation § of the ordinary semantics $\mathsf{sem}(\kappa)$, and every consequence relation based on $\mathcal{V}_\kappa$ and $\mathcal{D}_\kappa$, define $\mathcal{V}_2 = \{T, F\}$ and $\mathcal{D}_2 = \{T\}$ and set the characteristic total function $b_\S : \mathcal{S} \to \mathcal{V}_2$ to be such that $b_\S(\varphi) = T$ iff $\S(\varphi) \in \mathcal{D}$. Now, collect all such bivaluations $b_\S$'s into a new semantics $\mathsf{sem}(2)$, and notice that $\Gamma \vDash_{\mathsf{sem}(2)} \varphi$ iff $\Gamma \vDash_{\mathsf{sem}(\kappa)} \varphi$. □

Everything can be easily dualized to the SPMCT case. Only that now, given a single-premise logic $\langle \mathcal{S}, \Vdash \rangle$ and a theory $\Pi \subseteq \mathcal{S}$, you had better work with the *left-closure* of Π, denoted by ${}^{\mathbf{c}}\Pi$, as the set of all of its deriving premises, that is, the set $\{\pi : \pi \Vdash \Pi\}$. The rest is straightforward to adapt.

I will now briefly show how the above constructions can be modified for the MCT case (cf. [30]). As usual, call $\langle \Sigma, \Pi \rangle$ a *partition* of the set $\Theta \subseteq \mathcal{S}$ in case $\Sigma \cup \Pi = \Theta$ and $\Sigma \cap \Pi = \varnothing$.

Theorem 1.5.6 (*Cut for sets*) Given a MCT logic $\langle \mathcal{S}, \Vdash \rangle$:
If $\Gamma, \Sigma \Vdash \Pi, \Delta$, for every partition $\langle \Sigma, \Pi \rangle$ of Θ then $\Gamma \Vdash \Delta$.

Proof: Exercise. Use (C2) (cut), and induction on the cardinality of the Θ. □

Theorem 1.5.7 (*L-theorem*) Each MCT logic has some sound MCT unitary semantics.

Proof: The overcomplete case is done. Otherwise, given a minimally decent logic $\langle \mathcal{S}, \Vdash \rangle$, call any partition $\langle \Sigma, \Pi \rangle$ of its language $\mathcal{S}$ *closed* in case $\Sigma \nVdash \Pi$. For every closed partition $\langle \Sigma, \Pi \rangle$ of $\mathcal{S}$, define the unitary semantics in which $\mathcal{V} = \mathcal{S}$, $\mathcal{D} = \Sigma$ and $\mathcal{U} = \Pi$. The local / global canonical entailment ${}_\Sigma\vDash_\Pi \subseteq \mathsf{Pow}(\mathcal{S}) \times \mathsf{Pow}(\mathcal{S})$ induced by that definition will be such that $\Gamma\,{}_\Sigma\vDash_\Pi\Delta$ iff

$\Gamma \cap \Pi \neq \varnothing$ or $\Delta \cap \Sigma \neq \varnothing$. Now, given an arbitrary inference $\Gamma \Vdash \Delta$, one can in particular conclude, by (C3) (dilution), that $\Gamma, \Sigma \Vdash \Pi, \Delta$. Supposing by absurd that both $\Gamma \cap \Pi = \varnothing$ and $\Delta \cap \Sigma = \varnothing$, one would be forced to conclude that $\Gamma \subseteq \Sigma$ and $\Delta \subseteq \Pi$, given that $\langle \Sigma, \Pi \rangle$ is a partition. From the above it follows that $\Sigma \Vdash \Pi$. This is impossible, for the partition $\langle \Sigma, \Pi \rangle$ is supposed to be closed. □

Theorem 1.5.8 (*W-theorem*) Any MCT logic has an adequate semantics.

Proof: Given an MCT logic $\langle \mathcal{S}, \Vdash \rangle$, define ${}_{\Sigma}{\vDash}_{\Pi}$, for each closed partition $\langle \Sigma, \Pi \rangle$ of $\mathcal{S}$, as in Theorem 1.5.7. Call cp the set of all such closed partitions. Take again the intersection of all the unitary semantics thereby induced, thus defining $\vDash\; = \bigcap_{\langle \Sigma, \Pi \rangle \in \mathsf{cp}} ({}_{\Sigma}{\vDash}_{\Pi})$. Soundness is easy to check. To check completeness, assume $\Gamma \nVdash \Delta$. Given the cut for sets (Theorem 1.5.6) we know that there will be some partition $\langle \Sigma, \Pi \rangle$ of $\mathcal{S}$ such that $\Gamma, \Sigma \nVdash \Pi, \Delta$. From (C3) (dilution), we know that such partition must be closed. Moreover, given (C1) (overlap), one must conclude that $\Gamma \subseteq \Sigma$ and $\Delta \subseteq \Pi$, and so $\Gamma {}_{\Sigma}{\nvDash}_{\Pi} \Delta$, thus $\Gamma \nvDash \Delta$. □

Corollary 1.5.9 Every MCT logic $\langle \mathcal{S}, \Vdash \rangle$ has an adequate ordinary κ-valued semantics, with $\kappa \leq |\mathcal{S}|$. □

Theorem 1.5.10 (*S-theorem*) Every MCT logic has an adequate κ-valued MCT semantics, for $\kappa \leq 2$. □

One can conclude from the above results that:

Theorem 1.5.11 (i) Every SCT / MCT logic has an adequate possible-translations semantics, in fact even a possible-translations semantics based on 2-valued factors (copies of classical logic).
(ii) The local and the global consequence relations associated to any simple possible-translations representation or possible-translations semantics based on SCT / MCT factors is SCT / MCT.

Proof: From Theorems 1.5.3 and 1.4.1. □

It is noteworthy that the above results for canonical semantics have pretty much the same flavor of a pts: Each unitary semantics can be seen as determining a translation, and the intersection of all of the appropriate unitary semantics in each case gives you the desired conservative translation.

2 Further illustrations

We have seen, in the previous section, that every MCT / SCT logic has an adequate MCT / SCT (2-valued) semantics. Moreover, any logic (MCT, SCT, or not) has an adequate possible-translations representation (ptr), and if it has an adequate semantics (MCT, SCT, or not) then it can be given an adequate possible-translations semantics (pts).

What about other less trivial examples of possible-translations semantics, not obtained by plain use of brute force, as above? Indeed, notice that the previous adequacy results were often either uninformative (when a logic was used to represent itself) or non-constructive (when a κ-valued semantics was posited but no recursive method was presented so as to define it). The situation can be improved in a some cases. In the case of sufficiently expressive finite-valued truth-functional logics, for instance, a constructive method can be designed for the specification of a recursive set of clauses that describe the 2-valued semantics announced by Theorem 1.5.5 (cf. [6, 5]).

Moreover, to get even more concrete, one can use a ptr to provide, say, a pts based on a couple of well-behaved and well-known finite-valued truth-functional factors for logics having NO adequate finite-valued truth-functional semantics, as done in [10, 24, 14, 26] for several paraconsistent and paracomplete logics. Also, deductive limits for infinite hierarchies of logics can very naturally be spliced, and decidability transferred from the factors to the product, as in [24, 14]. Moreover, truth-functional finite-valued logics can themselves be split in terms of 2-valued logics, that is, fragments of classical logic ([24, 27]), copies of classical logic can be combined into fragments of modal logics, and so on and so forth.

The final version of the paper will display a few representative such examples in detail.

3 Some other related semantic structures

The advantage of possible-translations semantics lies in its generality. It is no overstatement to assert that pretty much anything that one might want to call a semantics can be recast in the present framework. This leads us immediately to the main disadvantage of possible-translations semantics: its generality! Anything that is universally true can easily turn out to be also universally irrelevant. It is very important thus to characterize some interesting subclasses of possible-translations semantics, defined by stricter terms. Clauses restricting the set of translations or the factors involved are often helpful, often inevitable. With that in mind, *society semantics* ([13, 24, 21, 22]), *dyadic semantics* ([6, 5]), and (dynamic and static) *non-deterministic semantics* ([2, 1]) can all be precisely characterized as specialized forms of possible-translations semantics.

This will be done in detail in the final version of the paper.

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Possible-translations semantics for some weak classically-based paraconsistent logics

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Abstract

This note provides interpretation by way of possible-translations semantics for a group of fundamental paraconsistent logics extending the positive fragment of classical propositional logic. The logics PI, C_{min}, **mbC**, **bC**, **mCi** and **Ci**, among others, are all initially presented through their bivaluation semantics and sequent versions and then split by way of possible-translations semantics —the set of 3-valued matrices of the ingredient logics is put forward, together with the set of admissible translating mappings, in each case. Precise statements and all non-obvious details of proofs are supplied. Other details are left to the reader.

Key words: Possible-translations semantics, paraconsistent logics.

1 Languages, bivaluations, and sequents

Let $\mathcal{P} = \{p_1, p_2, \ldots, p_m, \ldots\}$ be a denumerable set of sentential letters, and consider the sets of formulas

$$\begin{aligned} \mathcal{S}_0 &:= p \mid (\varphi \wedge \psi) \mid (\varphi \vee \psi) \mid (\varphi \supset \psi), \\ \mathcal{S}_1 &:= \mathcal{S}_0 \mid {\sim}\varphi, \\ \mathcal{S}_2 &:= \mathcal{S}_1 \mid \circ\varphi, \\ \mathcal{S}_3 &:= \mathcal{S}_2 \mid \bullet\varphi, \end{aligned}$$

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where p ranges over $\mathcal{P}$, and $\wedge$ ('conjunction'), $\vee$ ('disjunction'), $\supset$ ('implication'), $\sim$ ('negation'), $\circ$ ('consistency'), $\bullet$ ('inconsistency') are connective symbols. As usual, the binary connective $\equiv$ ('bi-implication') is defined by considering $\varphi \equiv \psi$ as an abbreviation for $(\varphi \supset \psi) \wedge (\psi \supset \varphi)$. Outermost parentheses are omitted whenever there is no risk of confusion.

A mapping $b : \mathcal{S}_i \longrightarrow \{0, 1\}$ is called a bivaluation over $\mathcal{S}_i$. One can easily write some possible axioms governing the set of admissible bivaluations:

(b1.1) $b(\varphi \wedge \psi) = 1 \;\Rightarrow\; b(\varphi) = 1$ and $b(\psi) = 1$
(b1.1$^{\mathbf{r}}$) $b(\varphi \wedge \psi) = 0 \;\Rightarrow\; b(\varphi) = 0$ or $b(\psi) = 0$
(b1.2) $b(\varphi \vee \psi) = 1 \;\Rightarrow\; b(\varphi) = 1$ or $b(\psi) = 1$
(b1.2$^{\mathbf{r}}$) $b(\varphi \vee \psi) = 0 \;\Rightarrow\; b(\varphi) = 0$ and $b(\psi) = 0$
(b1.3) $b(\varphi \supset \psi) = 1 \;\Rightarrow\;$ if $b(\varphi) = 1$ then $b(\psi) = 1$
(b1.3$^{\mathbf{r}}$) $b(\varphi \supset \psi) = 0 \;\Rightarrow\; b(\varphi) = 1$ and $b(\psi) = 0$
(b2) $b({\sim}\varphi) = 0 \;\Rightarrow\; b(\varphi) = 1$
(b3) $b(\circ\varphi) = 1 \;\Rightarrow\; b(\varphi) = 0$ or $b({\sim}\varphi) = 0$
(b3$^{\mathbf{r}}$) $b(\circ\varphi) = 0 \;\Rightarrow\; b(\varphi) = 1$ and $b({\sim}\varphi) = 1$
(b4) $b({\sim}\circ\varphi) = 1 \;\Rightarrow\; b(\varphi) = 1$ and $b({\sim}\varphi) = 1$
(b5.n) $b(\circ{\sim}^n\circ\varphi) = 1$, given $n \in \mathbb{N}$
(b6) $b({\sim}{\sim}\varphi) = 1 \;\Rightarrow\; b(\varphi) = 1$
(b6$^{\mathbf{r}}$) $b({\sim}{\sim}\varphi) = 0 \;\Rightarrow\; b(\varphi) = 0$

where ${\sim}^0\varphi \stackrel{\text{def}}{=} \varphi$ and ${\sim}^{n+1}\varphi \stackrel{\text{def}}{=} {\sim}^n{\sim}\varphi$.

The converse of (b4) clearly follows from (b2) and (b3), and the latter two axioms are to be respected by most logics we will consider below. Moreover, the reader will surely have noticed the difference between (b4) and (b3$^{\mathbf{r}}$), the converse of (b3):

Fact 1.1 In the presence of (b2), axiom (b3$^{\mathbf{r}}$) can be derived from (b4). The axiom (b4) can be derived from (b3$^{\mathbf{r}}$) in the presence of (b3) and (b5.0).

All the above axioms are in 'dyadic form' (cf. [10]). In that case, there is a canonical method for transforming all of them into appropriate sequent rules, as devised in [9]. This results in the following:

(s1.1) $\varphi \wedge \psi \vdash \varphi$ and $\varphi \wedge \psi \vdash \psi$
(s1.1$^{\mathbf{r}}$) $\varphi, \psi \vdash \varphi \wedge \psi$
(s1.2) $\varphi \vee \psi \vdash \varphi, \psi$
(s1.2$^{\mathbf{r}}$) $\varphi \vdash \varphi \vee \psi$ and $\psi \vdash \varphi \vee \psi$
(s1.3) $\varphi \supset \psi, \varphi \vdash \psi$
(s1.3$^{\mathbf{r}}$) $\vdash \varphi, \varphi \supset \psi$ and $\psi \vdash \varphi \supset \psi$
(s2) $\vdash \varphi, {\sim}\varphi$
(s3) $\circ\varphi, \varphi, {\sim}\varphi \vdash$
(s3$^{\mathbf{r}}$) $\vdash \circ\varphi, \varphi$ and $\vdash \circ\varphi, {\sim}\varphi$
(s4) ${\sim}\circ\varphi \vdash \varphi$ and ${\sim}\circ\varphi \vdash {\sim}\varphi$
(s5.n) $\vdash \circ{\sim}^n\circ\varphi$, given $n \in \mathbb{N}$
(s6) ${\sim}{\sim}\varphi \vdash \varphi$
(s6$^{\mathbf{r}}$) $\varphi \vdash {\sim}{\sim}\varphi$

For the sake of legibility, the side contexts of the above rules were dropped. Any subset of those rules, together with reflexivity, weakening, cut, and the usual structural rules, determines a specific sequent system. We will write $\Gamma \dashv\vdash \Delta$ as an abbreviation for $\Gamma \vdash \Delta$ and $\Delta \vdash \Gamma$.

The following is a straightforward byproduct of the above:

Fact 1.2 Rule (s5.0) is derivable with the help of (s2), (s3) and (s4). Rules (s5.n), for $n \in \mathbb{N}$, are all derivable in the presence of (s3), (s4), (s5.0) and (s6).

2 Some fundamental paraconsistent logics

Let CL^+ denote the positive fragment of classical propositional logic, built over the set of formulas $\mathcal{S}_0$, axiomatized by way of the rules (s1.X) and interpreted through the set of all bivaluations respecting the axioms (b1.X).

The very weak paraconsistent logic PI (cf. [7]) is built over $\mathcal{S}_1$ simply by adding (s2) to the rules of CL^+ or (b2) to its bivaluational axioms. The full classical propositional logic, CL, could be obtained now from PI over $\mathcal{S}_1$ by adding

(b2$^\mathbf{r}$) $\quad b(\sim\varphi) = 1 \;\Rightarrow\; b(\varphi) = 0$

to the bivaluational axioms of PI, or, equivalently, by adding

(s2$^\mathbf{r}$) $\quad \varphi, \sim\varphi \vdash$

to PI's sequent rules. The bivaluational axioms (b2) and (b2$^\mathbf{r}$) are thus sufficient for interpreting classical negation in isolation from the other connectives, and the sequent rules (s2) and (s2$^\mathbf{r}$) can be seen as the pure characterizing rules of classical negation.

A fundamental logic of formal inconsistency (cf. [18]) called **mbC** is built next over $\mathcal{S}_2$ by adding (s3) to the rules of PI or, equivalently, by adding (b3) to its bivaluational axioms. A 0-ary connective $\bot$ ('bottom'), characterized semantically by setting $b(\bot) = 0$, can be defined in **mbC** if one takes $\bot \stackrel{\text{def}}{=} \circ\psi \wedge (\psi \wedge \sim\psi)$, for any formula ψ. As a byproduct:

Fact 2.1 A classical negation $\neg$ can be defined in **mbC** by setting $\neg\varphi \stackrel{\text{def}}{=} \varphi \supset \bot$.

The logic **mbC**, as presented above, had only a primitive consistency connective $\circ$ but no primitive connective for inconsistency. The latter can nonetheless be defined in **mbC** if one just sets $\bullet\varphi \stackrel{\text{def}}{=} \sim\circ\varphi$. This way one could in fact rebuild **mbC** over $\mathcal{S}_3$, if that be the case.

An important extension of **mbC** is the logic **mCi**, again built over $\mathcal{S}_2$, but now by adding (s4) and (s5.n), $n \in \mathbb{N}$, to the rules of **mbC**, or (b4) and (b5.n), $n \in \mathbb{N}$, to its bivaluational axioms. The fundamental characteristic of **mCi** is the classical behavior of its consistency connective $\circ$ with respect to the negation $\sim$:

Fact 2.2 In **mCi**:
(i) $b(\sim\circ\alpha) = b(\neg\circ\alpha)$,
(ii) $b(\sim^n\circ\alpha) = 1 \;\Leftrightarrow\; b(\sim^{n+1}\circ\alpha) = 0$.

As a particular consequence, the above mentioned inconsistency connective $\bullet$, in **mCi**, will be perfectly dual to the consistency connective $\circ$. Indeed:

Fact 2.3 In **mCi**, $\circ\alpha \dashv\vdash \sim\bullet\alpha$.

Let $\psi[p]$ denote a formula ψ having p as one of its atomic components, and let $\psi[p/\gamma]$ denote the formula obtained from ψ by uniformly substituting all occurrences of p by the formula γ. Given a pair of formulas α and β, we say that they are *logically indistinguishable* if for every formula $\varphi[p]$ we have that $\varphi[p/\alpha] \dashv\vdash \varphi[p/\beta]$. Algebraically, this will mean that α and β will have the 'same reference', and belong thus to the same congruence class. In terms of bivaluation semantics, this will mean that $b(\varphi[p/\alpha]) = b(\varphi[p/\beta])$, for any formula φ. By the very definition of $\bullet$ we know that the formulas $\bullet\alpha$ and $\sim\circ\alpha$ are logically indistinguishable. However, in spite of the equivalence between the formulas $\circ\alpha$ and $\sim\bullet\alpha$ mentioned in the last fact, such formulas are not logically indistinguishable inside the logics studied in the present paper. We will use our possible-translations tool to check this feature in Example 5.15, further on.

The logics PIf, **bC** and **Ci** extend, respectively, the logics PI, **mbC** and **mCi**, by the addition of the bivaluational axiom (b6) or, equivalently, of the sequent rule (s6). The logic PIf appears in ch.4 of [20] and then at [15] under the appellation C_{min}. Both **bC** and **Ci**, as well as an enormous number of their extensions, are studied in close detail at [18]. The logic **mCi** is suggested at the final section of the latter paper, but axiomatized here for the first time. This logic, together with **mbC**, constitute the most fundamental logics explored in [13]. Inaccuracies in the axiomatization (as introduced in [18]) and in the bivaluation semantics (as presented in [16, 17]) of the logic **Ci** are also fixed at [13].

On a similar vein, the logics $PIfe$, **bCe** and **Cie** can here be introduced as extensions of the previous logics obtained by the further addition of the bivaluational axiom ($\text{b6}^{\mathbf{r}}$) or, equivalently, of the sequent rule ($\text{s6}^{\mathbf{r}}$). In the light of the results from the preceding facts, it might seem natural that **mCi**, **Ci**, and **Cie** should from this point on be built instead directly over the extended set of formulas $\mathcal{S}_3$, where $\bullet$ could be introduced by a definition using $\sim$ and $\circ$, as above.

To summarize the 9 previously mentioned paraconsistent logics:

PI formulas: $\mathcal{S}_1$
sequent rules: (s1.X) and (s2)
axioms on bivaluations: (b1.X) and (b2)

mbC formulas: $\mathcal{S}_2$
sequent rules: as in PI, plus (s3)
axioms on bivaluations: as in PI, plus (b3)

mCi formulas: $\mathcal{S}_3$
sequent rules: as in **mbC**, plus (s4) and (s5.n), $n \in \mathbb{N}$
axioms on bivaluations: as in **mbC**, plus (b4) and (b5.n), $n \in \mathbb{N}$

PIf formulas: $\mathcal{S}_1$
sequent rules: as in PI, plus (s6)
axioms on bivaluations: as in PI, plus (b6)
(a.k.a. C_{min})

bC formulas: $\mathcal{S}_2$
sequent rules: as in **mbC**, plus (s6)
axioms on bivaluations: as in **mbC**, plus (b6)

Ci formulas: $\mathcal{S}_3$
sequent rules: as in **bC**, plus (s4) and (s5.0)
axioms on bivaluations: as in **bC**, plus (b4) and (b5.0)

$PIfe$ formulas: $\mathcal{S}_1$
sequent rules: as in PIf, plus (s6$^{\mathbf{r}}$)
axioms on bivaluations: as in PIf, plus (b6$^{\mathbf{r}}$)

bCe formulas: $\mathcal{S}_2$
sequent rules: as in **bC**, plus (s6$^{\mathbf{r}}$)
axioms on bivaluations: as in **bC**, plus (b6$^{\mathbf{r}}$)

Cie formulas: $\mathcal{S}_3$
sequent rules: as in **Ci**, plus (s6$^{\mathbf{r}}$)
axioms on bivaluations: as in **Ci**, plus (b6$^{\mathbf{r}}$)

The simplification in the rules and axioms of **Ci**, as compared to those of **mCi**, is sanctioned by the results in Fact 1.2.

For a quick scan, one can find in Figure 1 a schematic illustration displaying the relationships between the above logics. An arrow $\mathcal{L}1 \longrightarrow \mathcal{L}2$ indicates that the logic $\mathcal{L}1$ is (properly) extended by the logic $\mathcal{L}2$.

$$\begin{tikzcd}
\mathbf{mCi} \arrow[r] & \mathbf{Ci} \arrow[r] & \mathbf{Cie} \\
\mathbf{mbC} \arrow[u] \arrow[r] & \mathbf{bC} \arrow[u] \arrow[r] & \mathbf{bCe} \arrow[u] \\
PI \arrow[u] \arrow[r] & PIf \arrow[u] \arrow[r] & PIfe \arrow[u]
\end{tikzcd}$$

Figure 1: Some fundamental paraconsistent logics.

3 Bivalued entailment, modalities and matrices

Fixed any of the logics presented in the above section, let biv be its set of admissible bivaluations. Given $b \in \mathsf{biv}$, let $\Gamma \vDash_b \Delta$ hold good, for given sets of formulas Γ and Δ, iff $(\exists\gamma \in \Gamma)b(\gamma) = 0$ or $(\exists\delta \in \Delta)b(\delta) = 1$. The canonical *entailment* relation $\vDash_{\mathsf{biv}}$ is defined as usual: $\Gamma \vDash_{\mathsf{biv}} \Delta$ iff $\Gamma \vDash_b \Delta$ for every $b \in \mathsf{biv}$. Moreover, given a set of sequent rules seq, let $\vdash_{\mathsf{seq}}$ denote the *derivability* relation defined by its canonical notion of (multiple-conclusion) proof-from-premises. Entailment and derivability relations are examples of *consequence* relations. Given any consequence relation $\rhd$ associated to a logic $\mathcal{L}$, we will write $\Gamma \not\rhd \Delta$ to say that the inference $\Gamma \rhd \Delta$ fails according to $\mathcal{L}$, and we will write $\Gamma \lhd\rhd \Delta$ to say that both $\Gamma \rhd \Delta$ and $\Delta \rhd \Gamma$ hold good in $\mathcal{L}$.

Can the 9 above paraconsistent logics be given semantics that are more informative than their respective bivaluation semantics? Good question. It should be remarked for instance that those logics cannot be endowed with usual modal-like semantics. Indeed, all of them fail the *replacement property*, a property that is typical of normal modal systems:

Theorem 3.1 In any of the logics from Figure 1, $\dashv\vdash$ does not constitute a congruence relation over the set of formulas, that is, there are formulas α and β such that $\alpha \dashv\vdash \beta$, but ${\sim}\alpha \nvdash {\sim}\beta$.

Proof: Consider the 3-valued matrices of the logic **LFI1**, at Table 2, where F is the only undesignated truth-value.

$\wedge$	T	t	F
T	T	t	F
t	t	t	F
F	F	F	F

$\vee$	T	t	F
T	T	T	T
t	T	t	t
F	T	t	F

$\supset$	T	t	F
T	T	t	F
t	T	t	F
F	T	T	T

	$\sim$	$\circ$
T	F	T
t	t	F
F	T	T

Figure 2: Matrices of the logic **LFI1**.

It is easy to check that **LFI1** (properly) extends all the above paraconsistent logics —it constitutes in fact a maximally paraconsistent extension of those logics (cf. [20, 19]). Nevertheless, in **LFI1**, while tautologies such as $(p\vee{\sim}p)$ and $(q \vee {\sim}q)$ are equivalent, the formulas ${\sim}(p \vee {\sim}p)$ and ${\sim}(q \vee {\sim}q)$ are not equivalent: To see that, consider any 3-valued valuation such that the atomic sentence p receives the value t while q receives a different value. $\square$

Note 3.2 (A seeming paradox) The logic of formal inconsistency **mbC** (and any of its non-trivial paraconsistent extensions) can be seen both as a conservative extension and as a deductive fragment of classical logic, CL. Indeed, for the first assertion, recall the set of formulas $\mathcal{S}_0$ of positive classical logic (Section 1), and consider now the sets of formulas:

$$\mathcal{S}_4 := \mathcal{S}_0 \mid \neg\varphi,$$
$$\mathcal{S}_5 := \mathcal{S}_4 \mid {\sim}\varphi \mid \circ\varphi.$$

Interpret the connectives from $\mathcal{S}_4$ as in CL, using the bivaluational axioms (b1.X) and (b2.X) (where $\neg$ takes the place of $\sim$). Interpret the new connectives in $\mathcal{S}_5$ as in **mbC**, using the bivaluational axioms (b2) and (b3). It is clear that this last move provides just a new way of presenting **mbC**. Indeed, as we have seen in Fact 2.1, $\neg$ can be defined from the original presentation of **mbC**. Consider again the matrices of **LFI1**, from Table 2, a logic that deductively extends **mbC**. The classical negation $\neg$ in **LFI1**, defined as above, would be such that $v(\neg\varphi) = T$ if $v(\varphi) = F$, and $v(\neg\varphi) = F$ otherwise. It is easy to see, in that case, that the matrices of $\sim$ and $\circ$, the new connectives of $\mathcal{S}_5$ cannot be defined, in **LFI1**, from the matrices of the connectives in $\mathcal{S}_4$. If you recall now that CL is a maximal logic, then you have concluded the proof that **mbC** can be seen as a (proper) conservative extension of CL. For the second assertion, consider CL to be written in the language of $\mathcal{S}_5$. Recall that classical logic is presupposed consistent, and interpret the connective $\circ$ accordingly, by taking as axiom $b(\circ\varphi) = 1$. Based on the received idea that there is just 'one true classical negation', interpret both $\neg$ and $\sim$ using axioms (b2) and (b2$^{\mathbf{r}}$). In that case **mbC** is clearly characterized as a (proper) deductive fragment of CL. Notice that this is, however, a very peculiar fragment of CL —it is a fragment into which all classical reasoning can be internalized by way of a definitional translation.

Note 3.3 (More on internalizing stronger logics) Not only can **mbC** faithfully internalize classical logic, but it can also internalize the reasoning of other logics of formal inconsistency that are deductively stronger than itself. To see that, consider now the following sets of formulas:

$$\mathcal{S}_6 := \mathcal{S}_0 \mid \bot,$$
$$\mathcal{S}_7 := \mathcal{S}_6 \mid {\sim}\varphi,$$
$$\mathcal{S}_8 := \mathcal{S}_7 \mid \circ\varphi.$$

Interpret the 0-ary connective ('bottom') from $\mathcal{S}_6$ by taking as axiom $b(\bot) = 0$, and interpret the new connectives from $\mathcal{S}_7$ and $\mathcal{S}_8$ as in **mbC**. Again, this provides just another presentation for **mbC**, as we have seen in Section 1 that $\bot$ is definable in this logic. On the other hand, a new consistency connective strictly stronger than $\circ$ can be defined using the connectives from $\mathcal{S}_7$. Indeed, as in [18], consider a connective $\tilde{\circ}$ defined by setting $\tilde{\circ}\varphi \stackrel{\text{def}}{=} (\varphi \supset \bot) \vee ({\sim}\varphi \supset \bot)$ (or, equivalently, $\tilde{\circ}\varphi \stackrel{\text{def}}{=} \neg\varphi \vee \neg{\sim}\varphi$). This connective is naturally characterizable by axiom (b3) and its converse (b3$^{\mathbf{r}}$), while the original consistency connective of **mbC** was characterized by axiom (b3) alone. If you recall Fact 1.1 you will notice that the last definition determines a logic of formal inconsistency that lies right in between **mbC** and **mCi**. As a matter of fact, this approach provides one way of presenting the logic **CLuN**, the preferred logic of adaptive logicians (cf. [8]), often used as the lower limit logic of their inconsistency-adaptive systems. Though the first presentations of **CLuN** made this logic coincide with PI, it has been more recently presented as a conservative extension of PI obtained by adding a bottom connective to the language of the latter, as in $\mathcal{S}_7$ above. If one writes the whole thing in the language of $\mathcal{S}_8$, using the above defined consistency connective, **CLuN** is very naturally recast thus as a logic of formal inconsistency that lies in between **mbC** and **mCi**.

Problem 3.4 Is there a definitional translation of **mCi** into **mbC**? Can the logic **mbC** faithfully internalize in some way the reasoning of **mCi**?

Note 3.5 (Other logics extending mbC but not mCi)
Besides **CLuN**, there are many other interesting logics of formal inconsistency that extend **mbC** but do not go through **mCi**. There is even a large class of such logics that satisfies the full replacement property. I have shown in [21, 23], in fact, that any non-degenerate normal modal logic can be easily recast as a logic of formal inconsistency extending **CLuN** (and thus extending **mbC**), but not **mCi**.

Before the diversion provided by the above set of notes, we had seen in Theorem 3.1 that the 9 paraconsistent logics from the last section cannot be endowed with usual modal-like semantics. The reader might now be wondering whether those logics would still stand some chance of being *truth-functional*, should they turn out themselves to be characterizable by way of some convenient set of finite-valued matrices (just like their extension **LFI1**). But some negative results about that possibility can also be promptly checked as follows. The following theorem and its corollary correct a result suggested in [1]:

Theorem 3.6 No sequent of the form $\vdash {\sim}^i\varphi \equiv {\sim}^j\varphi$ is derivable, for non-negative $i \neq j$, in logics from the first two columns of Figure 1.

Proof: Consider a set of infinite-valued matrices that take the natural numbers $\mathbb{N}$ as truth-values, where 0 is the only undesignated truth-value. Define the matrices for the connectives as follows:

$$v(\varphi \wedge \psi) = \begin{cases} 1, & \text{if } v(\varphi) > 0 \text{ and } v(\psi) > 0 \\ 0, & \text{otherwise} \end{cases}$$

$$v(\varphi \vee \psi) = \begin{cases} 1, & \text{if } v(\varphi) > 0 \text{ or } v(\psi) > 0 \\ 0, & \text{otherwise} \end{cases}$$

$$v(\varphi \supset \psi) = \begin{cases} 0, & \text{if } v(\varphi) > 0 \text{ and } v(\psi) = 0 \\ 1, & \text{otherwise} \end{cases}$$

$$v(\sim\varphi) = \begin{cases} 1, & \text{if } v(\varphi) = 0 \\ v(\varphi) - 1, & \text{otherwise} \end{cases} \qquad v(\circ\varphi) = \begin{cases} 0, & \text{if } v(\varphi) > 1 \\ 1, & \text{otherwise} \end{cases}$$

It is easy to check that all the sequent rules from Section 1 are validated by the above matrices, with the sole exception of (s6$^{\mathbf{r}}$). At the same time, the above matrices can also easily be seen to invalidate all sequents of the form $\vdash \sim^i\varphi \equiv \sim^j\varphi$, for non-negative $i \neq j$. □

Corollary 3.7 (Uncharacterizability by finite matrices, *version* I) None of the logics from the first two columns of Figure 1 (i.e., the fragments of **Ci**) is finite-valued.

Proof: Would any of these logics be characterized by matrices with only m truth-values, then we would have, by the Pigeonhole Principle, some $i < j \leq (i + m^m)$ such that $v(\neg^i p) = v(\neg^j p)$, for all v. This would in turn validate some sequent of the form $\vdash \sim^i\varphi \equiv \sim^j\varphi$, for $i < j$. □

The following theorem and its corollary correct a result suggested in [11]:

Theorem 3.8 Let δ_{ij}, for $i, j \neq 0$, denote the formula $\circ p_i \wedge p_i \wedge \sim p_j$, and let δ^n denote the disjunctive formula $\bigvee_{1 \leq i < j \leq n}(\delta_{ij} \supset p_{n+1})$, for $n > 0$. No sequent of the form $\vdash \delta^n$ is derivable in the logics from the first two lines of Figure 1.

Proof: Take now the truth-values from the set $\mathbb{N} \cup \{\omega\}$, where ω is the only undesignated truth-value. Define the matrices for the connectives as follows:

$$v(\varphi \wedge \psi) = \mathsf{max}(v(\varphi), v(\psi)) \qquad v(\varphi \vee \psi) = \mathsf{min}(v(\varphi), v(\psi))$$

$$v(\varphi \supset \psi) = \begin{cases} \omega, & \text{if } v(\varphi) \in \mathbb{N} \text{ and } v(\psi) = \omega \\ v(\psi), & \text{if } v(\varphi) = \omega \text{ and } v(\psi) \in \mathbb{N} \\ 0, & \text{if } v(\varphi) = \omega = v(\psi) \\ \mathsf{max}(v(\varphi), v(\psi)), & \text{otherwise} \end{cases}$$

$$v(\sim\varphi) = \begin{cases} \omega, & \text{if } v(\varphi) = 0 \\ 0, & \text{if } v(\varphi) = \omega \\ v(\varphi), & \text{otherwise} \end{cases} \qquad v(\circ\varphi) = \begin{cases} 0, & \text{if } v(\varphi) \in \{0, \omega\} \\ \omega, & \text{otherwise} \end{cases}$$

It is easy to check that all the sequent rules from Section 1 are validated by the above matrices. At the same time, the above matrices can be seen to invalidate all sequents of the form $\vdash \delta^n$. Indeed, just consider a model such that $v(p_i) = i$, for $i \leq n$, and $v(p_{n+1}) = \omega$. □

Corollary 3.9 (Uncharacterizability by finite matrices, *version* II) None of the logics from the first two lines of Figure 1 (i.e., extensions of **mbC**) is finite-valued.

Proof: Notice, again using the Pigeonhole Principle, that the formula δ^n is validated by any set of m-valued matrices that is adequate for the logics extending **mbC** (use (s3) and (s1.2.2)) and such that $m < n$. $\square$

One logic from Figure 1, however, was not covered by the previous results. So, the following is here left open:

Problem 3.10 Find a proof that $PIfe$ is not characterizable by finite matrices.

4 Interpretations through possible translations

We will see in this section that all the previous paraconsistent logics can still be given adequate interpretations in terms of *combinations* of 3-valued logics, by way of specific possible-translations semantics (PTS). Consider the 3-valued matrices of $\mathcal{M}$, at Table 3), where F is the only undesignated truth-value.

$\wedge$	T	t	F
T	t	t	F
t	t	t	F
F	F	F	F

$\vee$	T	t	F
T	t	t	t
t	t	t	t
F	t	t	F

$\supset$	T	t	F
T	t	t	F
t	t	t	F
F	t	t	t

	$\sim_1$	$\sim_2$	$\sim_3$
T	F	F	F
t	F	t	t
F	T	t	T

	$\circ_1$	$\circ_2$	$\circ_3$
T	T	t	F
t	F	F	F
F	T	t	F

Figure 3: Matrices of $\mathcal{M}$.

Given a 3-valued assignment $a : \mathcal{P} \longrightarrow \{T, t, F\}$, let w be its unique homomorphic extension into the whole language of $\mathcal{M}$, and let $\Gamma \vDash_w \Delta$ hold good, for given sets of formulas Γ and Δ, iff $(\exists\gamma \in \Gamma)w(\gamma) = F$ or $(\exists\delta \in \Delta)w(\delta) \in \{T, t\}$. Then, the canonical (multiple-conclusion) entailment relation $\vDash_{\mathcal{M}}$ determined by the above 3-valued matrices is set by taking $\Gamma \vDash_{\mathcal{M}} \Delta$ iff $\Gamma \vDash_w \Delta$ for every interpretation $w \in \mathcal{M}$.

Consider next the following possible restrictions over the set of admissible translating mappings $* : \mathcal{S}_i \longrightarrow \mathcal{M}$:

(tr0) $p^* = p$, for $p \in \mathcal{P}$
(tr1) $(\varphi \bowtie \psi)^* = (\varphi^* \bowtie \psi^*)$, for $\bowtie \in \{\wedge, \vee, \supset\}$
(tr2.1) $(\sim\varphi)^* \in \{\sim_1\varphi^*, \sim_2\varphi^*\}$
(tr2.2) $(\sim\varphi)^* \in \{\sim_1\varphi^*, \sim_3\varphi^*\}$
(tr2.3) $(\sim^{n+1}\circ\varphi)^* = \sim_1(\sim^n\circ\varphi)^*$

(tr3.1) $(\circ\varphi)^* \in \{\circ_2\varphi^*, \circ_3\varphi^*, \circ_2(\sim\varphi)^*, \circ_3(\sim\varphi)^*\}$
(tr3.2) $(\circ\varphi)^* \in \{\circ_1\varphi^*, \circ_1(\sim\varphi)^*\}$
(tr3.3) if $(\sim\varphi)^* = \sim_1\varphi^*$ then $(\circ\varphi)^* = \circ_1(\sim\varphi)^*$
(tr4) if $(\sim\varphi)^* = \sim_3\varphi^*$ then $(\sim\sim\varphi)^* = \sim_3(\sim\varphi)^*$

One can now select appropriate sets of restrictions in order to split each of the paraconsistent logics from the last section by way of PTS:

Logic	Restrictions over the translating mappings
PI	(tr0), (tr1), (tr2.1)
mbC	(tr0), (tr1), (tr2.1), (tr3.1)
mCi	(tr0), (tr1), (tr2.1), (tr2.3), (tr3.2)
PIf	(tr0), (tr1), (tr2.2)
bC	(tr0), (tr1), (tr2.2), (tr3.1)
Ci	(tr0), (tr1), (tr2.2), (tr3.2), (tr3.3)
$PIfe$	(tr0), (tr1), (tr2.2), (tr4)
bCe	(tr0), (tr1), (tr2.2), (tr3.1), (tr4)
Cie	(tr0), (tr1), (tr2.2), (tr3.2), (tr3.3), (tr4)

Let Tr denote some set of translating mappings defined according to an appropriate subset of the previously mentioned restrictions. Define a pt-*model* as a pair $\langle w, *\rangle$, where $* \in \mathsf{Tr}$ and $w \in \mathcal{M}$, and let $\Gamma \Vdash_w^* \Delta$ hold good, for given sets of formulas Γ and Δ, iff $\Gamma^* \vDash_w \Delta^*$. A pt-*consequence relation* $\Vdash_{\mathsf{pt}}$ is then set by taking $\Gamma \Vdash_{\mathsf{pt}} \Delta$ iff $\Gamma \Vdash_w^* \Delta$ for every pt-model $\langle w, *\rangle$ allowed by Tr. Equivalently, in the cases presently under consideration, $\Gamma \Vdash_{\mathsf{pt}} \Delta$ also means, more simply, that $\Gamma^* \vDash_{\mathcal{M}} \Delta^*$, for every admissible translation $* \in \mathsf{Tr}$.

Note 4.1 (The development of PTS) A logic $\mathcal{L}$ is said to have a *possible-translations semantics* when it can be given an adequate interpretation in terms of pt-models, for some appropriate set of translating mappings. Each translation can then be seen as a sort of interpretation scenario for $\mathcal{L}$. This intuition is good enough for the purposes of the present paper, but the possible-translations tool is in fact more general than that. For a generous and clear formal definition of this sort of structures, check [22]. For other more specific and carefully explained examples, check [20, 15, 12]. The interested reader will notice that the PTS offered for **Ci** above is distinct from the one presented in [16]. Possible-translations semantics were first introduced in [11], restricted to the splitting of a logic into finite-valued truth-functional scenarios. The embryo was then frozen for a period, and in between 1997 and 1998 it was publicized under the denomination 'non-deterministic semantics', in [14], and in several talks by Carnielli and a few by myself. Noticing that the non-deterministic element was but a particular accessory of the more general picture, from 1999 on the semantics returned to its earlier denomination.

Note 4.2 (PTS and non-deterministic semantics) PTS are related to (but are more general than) the *non-deterministic semantics* (NDS) proposed by Avron & Lev (cf. [5]) in ways that are still to be more carefully explained. On what concerns the logics studied in the present paper, it should be noticed that [4] proposes a 2-valued NDS for PI, and [2] also offers an 3-valued NDS for PIf which is strikingly similar to the PTS presented for this logic above (and that comes from [20, 15]). More recently, [3] offers 3-valued NDS for the logics **mbC**, **bC**, **bCe**. Roughly speaking, one could say that *dynamic* NDS are based on clauses having the same format of (tr0)–(tr2.2), and *static* NDS additionally impose constraints having the format of (tr2.3) or (tr4) for each of the involved connectives. There is a mechanical way, thus, to move from a given NDS to an equivalent PTS. Further discussion of that issue shall be postponed to a future work.

We now have a number of quite diverse consequence relations associated to each of the above logics. Of course we want to keep this fauna under control —in the best of all possible worlds we want to be able to prove that all those consequence relations deliver just the same the result, for each given logic, that is, we want to prove that:

$$\vdash_{\mathsf{seq}} = \vDash_{\mathsf{biv}} = \Vdash_{\mathsf{pt}}$$

That is matter for the next, and final, section.

5 Adequacy of each of the newly proposed PTS

As mentioned in Section 1, the technology that solves the first part of our problem is well-known, and its outcome will here be taken for granted: $\vdash_{\mathsf{seq}}$ $= \vDash_{\mathsf{biv}}$.

Now, to check soundness of each of the paraconsistent logics in section 2 with respect to its specific PTS in section 4, one has two alternatives from the start. The first is to prove it directly from the axiomatizations in section 1 and the appropriate sets of translating mappings:

Theorem 5.1 (Soundness) $\vdash_{\mathsf{seq}} \subseteq \Vdash_{\mathsf{pt}}$.

Proof: Just translate each sequent axiom in all possible ways allowed by Tr and check that these translations are validated by $\mathcal{M}$. $\square$

The second alternative is to prove that each pt-model is bisimulated by some appropriate bivaluation:

Theorem 5.2 (Convenience)

$$(\forall w \in \mathcal{M})(\forall * \in \mathsf{Tr})(\exists b \in \mathsf{biv})\ \vDash_b \alpha \ \Leftrightarrow\ \Vdash^*_w \alpha.$$

Proof: Just set $b(\alpha) = 0$ iff $w(\alpha^*) = F$. Then check that the axioms in biv are all respected, in each case. $\square$

Corollary 5.3 (Soundness again) $\vDash_{\mathsf{biv}} \subseteq \Vdash_{\mathsf{pt}}$.

Now for completeness. Given that the evaluation of the consistency connective, $\circ$, in the way we have defined it, takes into account the evaluation of the negation connective, $\sim$, it will be helpful, when doing some of the next proofs by induction on the complexity of the formulas, to make use of the following non-canonical measure of complexity, **mc**:

(mc0) $\mathbf{mc}(p) = 0$, for $p \in \mathcal{P}$
(mc1) $\mathbf{mc}(\varphi \bowtie \psi) = \mathbf{mc}(\varphi) + \mathbf{mc}(\psi) + 1$, for $\bowtie \in \{\wedge, \vee, \supset\}$
(mc2) $\mathbf{mc}(\sim\varphi) = \mathbf{mc}(\varphi) + 1$
(mc3) $\mathbf{mc}(\circ\varphi) = \mathbf{mc}(\sim\varphi) + 1$

With such apparatus in hands, we can start looking for a proof that each particular bivaluation is bisimulated by some appropriate pt-model:

Theorem 5.4 (Representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr}) \;\; \Vdash_w^* \alpha \;\Leftrightarrow\; \vDash_b \alpha.$$

From what it would easily follow that:

Corollary 5.5 (Completeness) $\vDash_{\mathsf{biv}} \supseteq \Vdash_{\mathsf{pt}}$.

With respect to the above mentioned representability result, still to be proven, the safest strategy at this point seems to be that of checking it for each of our paraconsistent logics on its own turn, refining the statements and proofs to better suit each case. So, here we go:

Theorem 5.6 (PI-representability)

$$\begin{gathered}(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})\\ w(\alpha^*) = t \;\Leftrightarrow\; b(\alpha) = 1, \text{ and}\\ w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.\end{gathered}$$

Proof: To take care of w, set, for $p \in \mathcal{P}$:

(rw) $a(p) = F$ if $b(p) = 0$, and
$a(p) = t$ otherwise

and extend a into w homomorphically, according to the strictures of $\mathcal{M}$.

On what concerns $*$, set:

(rt0) $p^* = p$, for $p \in \mathcal{P}$
(rt1) $(\varphi \bowtie \psi)^* = (\varphi^* \bowtie \psi^*)$, for $\bowtie \in \{\wedge, \vee, \supset\}$
(rt2) $(\sim\varphi)^* = \sim_1\varphi^*$, if $b(\sim\varphi) = 0$
$(\sim\varphi)^* = \sim_2\varphi^*$, otherwise

The main statement above can now easily be checked by induction on the complexity measure **mc**. $\square$

Theorem 5.7 (mbC-representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})$$
$$w(\alpha^*) = T \;\Rightarrow\; b({\sim}\alpha) = 0, \text{ and}$$
$$w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.$$

Proof: To take care of w, set, for $p \in \mathcal{P}$:

(rw) $a(p) = F$ if $b(p) = 0$,
$a(p) = T$ if $b({\sim}p) = 0$, and
$a(p) = t$ otherwise

and extend a into w homomorphically, according to the strictures of $\mathcal{M}$.

On what concerns $*$, set:

(rt0) $p^* = p$, for $p \in \mathcal{P}$
(rt1) $(\varphi \bowtie \psi)^* = (\varphi^* \bowtie \psi^*)$, for $\bowtie \in \{\wedge, \vee, \supset\}$
(rt2) $({\sim}\varphi)^* = {\sim}_1\varphi^*$, if $b({\sim}\varphi) = 0$ or $b(\varphi) = 0 = b({\sim}{\sim}\varphi)$
$({\sim}\varphi)^* = {\sim}_2\varphi^*$, otherwise
(rt3) $(\circ\varphi)^* = \circ_3\varphi^*$, if $b(\circ\varphi) = 0$
$(\circ\varphi)^* = \circ_2({\sim}\varphi)^*$, if $b(\circ\varphi) = 1$ and $b({\sim}\varphi) = 0$
$(\circ\varphi)^* = \circ_2\varphi^*$, otherwise

Check now the result by induction on **mc**. Notice from (rt3) how the non-standard clause (mc3) of the previously defined non-canonical measure of complexity finally proves to be useful. □

Theorem 5.8 (mCi-representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})$$
$$w(\alpha^*) = T \;\Rightarrow\; b({\sim}\alpha) = 0, \text{ and}$$
$$w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.$$

Proof: Do as in parts (rt0)–(rt2) of Theorem 5.7, but now set:

(rt3) $(\circ\varphi)^* = \circ_1({\sim}\varphi)^*$, if $b({\sim}\varphi) = 0$
$(\circ\varphi)^* = \circ_1\varphi^*$, otherwise
(rt4) $({\sim}^{n+1}\circ\varphi)^* = {\sim}_1({\sim}^n\circ\varphi)^*$

Check the result by induction on **mc**. □

Theorem 5.9 (PIf-representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})$$
$$w(\alpha^*) = T \;\Rightarrow\; b({\sim}\alpha) = 0, \text{ and}$$
$$w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.$$

Proof: Do as in Theorem 5.6, except that in now setting:

(rt2) $({\sim}\varphi)^* = {\sim}_3\varphi^*$, if $b(\varphi) = 1 = b({\sim}\varphi)$
$({\sim}\varphi)^* = {\sim}_1\varphi^*$, otherwise

Check the result by induction on **mc**.

(A slightly different proof of this fact —check clause (rw)— can be found in the ch.4 of [20] and in [15] —bear in mind though that this logic PIf shows up there under the name C_{min}.) □

Theorem 5.10 (bC-representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})$$
$$w(\alpha^*) = T \;\Rightarrow\; b({\sim}\alpha) = 0, \text{ and}$$
$$w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.$$

Proof: Do as in Theorem 5.7, except that in now setting (rt2) as in Theorem 5.9. Check the result by induction on **mc**. □

Theorem 5.11 (Ci-representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})$$
$$w(\alpha^*) = T \;\Rightarrow\; b({\sim}\alpha) = 0, \text{ and}$$
$$w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.$$

Proof: Do as in Theorem 5.10, except that in now setting:

(rt3) $(\circ\varphi)^* = \circ_1({\sim}\varphi)^*$, if $b(\circ\varphi) = 1$
$(\circ\varphi)^* = \circ_1\varphi^*$, otherwise

Check the result by induction on **mc**.
(Notice that the PTS offered for **Ci** in the paper [16] uses different interpretations for the consistency connective and is based on a stricter set of restrictions over the set Tr. The present semantics seems, in a sense, to be more in accordance with the classical behavior of $\circ$ with respect to $\sim$.) □

Theorem 5.12 ($PIfe$-representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})$$
$$w(\alpha^*) = T \;\Rightarrow\; b({\sim}\alpha) = 0, \text{ and}$$
$$w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.$$

Proof: Do as in Theorem 5.9, except that in now setting the extra requirement:

(rt4) if $({\sim}\varphi)^* = {\sim}_3\varphi^*$ then $({\sim}{\sim}\varphi)^* = {\sim}_3({\sim}\varphi)^*$

Check the result by induction on **mc**.
(The practical difference in this proof with respect to the previous ones is that one will not only have a base case of induction for the atomic sentences and a complex case for each of the connectives, but one will also explicitly have to take into consideration the extra case of complex formulas preceded by at least two negation symbols.) □

Theorem 5.13 (bCe-representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})$$
$$w(\alpha^*) = T \;\Rightarrow\; b(\sim\alpha) = 0, \text{ and}$$
$$w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.$$

Proof: Do as in Theorem 5.10, except that in now setting (rt4) as in Theorem 5.12. Check the result by induction on **mc**. □

Theorem 5.14 (Cie-representability)

$$(\forall b \in \mathsf{biv})(\exists w \in \mathcal{M})(\exists * \in \mathsf{Tr})$$
$$w(\alpha^*) = T \;\Rightarrow\; b(\sim\alpha) = 0, \text{ and}$$
$$w(\alpha^*) = F \;\Leftrightarrow\; b(\alpha) = 0.$$

Proof: Do as in Theorem 5.8, except that in now setting (rt4) as in Theorem 5.12. Check the result by induction on **mc**. □

Example 5.15 We could now use the above defined PTS to check that, in **Cie** (thus, also in **Ci**, **bC**, **mCi**, **CLuN** or **mbC**), the formulas $\circ\alpha$ and $\sim\bullet\alpha$ are logically *distinguishable* even if equivalent, as announced in Section 2. Indeed, by the definition of $\bullet$, the formula $\sim\bullet\alpha$ is logically indistinguishable from the formula $\sim\sim\circ\alpha$. Yet, given a formula φ of the form $\sim p$ and a formula ψ of the form $\varphi[p/(p \wedge p)]$, it is easy to see that, in spite of the equivalence between $\varphi[p/\circ p]$ and $\varphi[p/\sim\sim\circ p]$ in logics as weak as **mCi**, formulas such as $\psi[p/\circ p]$ and $\psi[p/\sim\sim\circ p]$ are not equivalent in **Cie**. To check that, select some **Cie**-admissible translating mapping such that $(\circ p)^* = \circ_1\sim_1 p$, $(\sim(\circ p \wedge \circ p))^* = \sim_1(\circ p \wedge \circ p)^*$ and $(\sim(\sim\sim\circ p \wedge \sim\sim\circ p))^* = \sim_3(\sim\sim\circ p \wedge \sim\sim\circ p)^*$, and then select a 3-valued model $w \in \mathcal{M}$ for which $w(p) = t$.

Note 5.16 (Dualizing the above constructions) One might now start everything all over again, back from Section 1, and easily dualize all results for paracomplete counterparts of all the above paraconsistent logics. To such an effect, one only needs to explore the symmetry of the present multiple-conclusion environment, exchange each bivaluational axiom (si) and each sequent rule (si) for their converses (b$i^{\mathbf{r}}$) and (s$i^{\mathbf{r}}$), and exchange the consistency connective for a completeness, or determinedness, connective (as in [21]), and so on and so forth. The case of the dual of PIf was already explored in ch.4 of [20] and in [15], under the appellation D_{min}.

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Chapter Three
Modal Semantics for Logics of Formal Inconsistency

This chapter collects three papers: **3.1** brings 'Logics of essence and accident', henceforth Lea; **3.2** brings 'Modality and paraconsistency', henceforth ModPar; **3.3** brings 'Nearly every normal modal logic is paranormal', henceforth Paranormal.

Resumo de Lea

Dizemos que as coisas ocorrem acidentalmente quando elas de fa[c]to ocorrem, mas apenas por acaso. Na situação oposta, uma ocorrência essencial é inescapável, a sua inevitabilidade constituindo o sine qua non de sua própria concretização. Este artigo investigará lógicas modais numa linguagem desenhada para falar acerca de enunciados essenciais e acidentais. A completude de alguns dentre os sistemas mais fracos e mais fortes desta classe de lógicas é alcançada. Chama-se a atenção para o fraco poder expressivo da linguagem proposicional clássica enriquecida pelos operadores modais não-normais da essência e do acidente, e ilustra-se este fa[c]to tanto com relação à definibilidade dos operadores modais mais usuais quanto com relação à caracterizabilidade de classes de enquadramentos. Vários problemas interessantes e direções de investigação em aberto são sugeridos para investigação futura.

Resumo de ModPar

A lógica paraconsistente nasceu na vizinhança da lógica modal. Além do mais, como quaisquer outros lógicos não-clássicos, os paraconsistentistas frequentemente privaram com as modalidades. O primeiro sistema conhecido de lógica paraconsistente foi de fa[c]to definido como um fragmento de $S5$, no final dos anos 40. Mas um fragmento de um sistema modal não é necessariamente um sistema modal. Mostrarei aqui, com efeito, que a lógica $\mathbf{D2}$ de Jaśkowski não é uma lógica modal, no sentido contemporâneo usual do termo. Em contraste, mostrarei também, em seguida, que qualquer sistema modal não-degenerado é inerentemente paraconsistente.

Resumo de PARANORMAL

Uma lógica *extracompleta* é uma lógica que 'deixa de fazer a diferença': de acordo com uma tal lógica, todas as inferências valem independentemente da natureza dos enunciados envolvidos. Uma lógica *negação-inconsistente* é uma lógica que possui ao menos um modelo que tanto satisfaz um certo enunciado quanto a sua negação. Uma lógica *negação-incompleta* possui ao menos um modelo que não satisfaz um certo enunciado nem a sua negação. Lógicas *paraconsistentes* são negação-inconsistentes mas não-extracompletas; lógicas *paracompletas* são negação-incompletas mas não-extracompletas. Uma lógica *paranormal* é tão-somente uma lógica que é tanto paraconsistente quanto paracompleta.

Apesar de ser perfeitamente consistente e completa com relação à negação clássica, praticamente toda lógica modal normal, na sua linguagem e interpretação usuais, admite uma paranormalidade latente: ela é paracompleta com relação a uma negação definida como um operador de impossibilidade, e paraconsistente com relação a uma negação definida como não-necessidade. Com efeito, como aqui mostraremos, mesmo em linguagens desprovidas de uma negação clássica primitiva, as lógicas modais normais podem frequentemente ser caracterizadas alternativamente dire[c]tamente através de suas negações paranormais e operadores relacionados. Assim, ao invés de lógicas que falam sobre 'necessidade', 'possibilidade', e assim por diante, as lógicas modais podem ser vistas apenas como dispositivos forjados para o estudo da negação (modal). Este artigo mostra como e até que ponto tal caracterização alternativa das lógicas modais pode ser levada a bom efeito.

Contents

> It would be madness, and inconsistency, to suppose that things which have never yet been performed, can be performed without employing some hitherto untried means.
> —Francis Bacon, *Instauratio Magna*, 1620.

This chapter explores the links between Logics of Formal Inconsistency and usual modal logics. I will in the following introduce the herein contained papers, Lea (cf. [49]), ModPar (cf. [50]) and Paranormal (cf. [51]).

Some metaphysics

In **Chapter 1** a precise abstract characterization is provided for the Logics of Formal Inconsistency (**LFI**s), and with them a particular notion of consistency is formalized and put into use. A very inclusive class of logics in which this notion is internalized in terms of a single unary connective is then illustrated, mostly through Hilbertian axiomatizations, but also through some many-valued truth-functional interpretations. **Chapter 2.2** offers non-truth-functional interpretations to some of those logics (all of which are non-characterizable by finite matrices) and shows how those same logics can be interpreted as combinations of 3-valued scenarios by way of possible-translations semantics. Many people will find the previous semantic accounts in terms of many-valued or possible-translations semantics not very compelling, and this circumstance by itself would already justify the search for interpretations for **LFI**s in terms of more well-established semantics, such as possible-worlds semantics. The fact that many such logics fail the replacement property, as it has been noticed several times before, certainly does not help in our intention to characterize them from a modal viewpoint. Are there some **LFI**s that do satisfy replacement and, moreover, behave straightforwardly as ordinary modal logics? What kind of modal interpretations for negation could help us in the search for such **LFI**s? Can the consistency and the inconsistency connectives be given sensible modal interpretations at all?

As a methodological policy, it is often helpful to deal with one problem at a time. Let's try and find thus a solution for each part of our problem in separate before trying to find a solution to all problems. Of course, even if each part of the problem turns out to have a solution, it does not follow in general that there will be a solution for the whole problem at once. But in the present case, fortunately, each part of the solution has no reason to cancel the solution to any other part. What I would like to do in the following is to give a feeling of the heuristics involved in this kind of problem-solving activity —but you might as well have a look at [64], an authoritative guide to this territory. In case that might be found instructive, here are Pólya's four theoretical steps to problem-solving and how I dealt with them, in practice, in this particular case.

1. Understanding the problem. We want to find proper modal interpretations for **LFI**s. To do that, we will look for a way of modelling a paraconsistent negation and its consistency connective companion using the idiom of possible-worlds. Moreover, for the sake of simplicity, we will try to keep all other aspects of our logic as 'classical' as possible.

The idea of enriching paraconsistent logics with modal operators is anything but new. An early reference on this strategy is the paper [70], intended to investigate paraconsistent logics with alethic and deontic operators. More recently, **LFI**s with alethic and epistemic operators were investigated in [21]. In both approaches, the paraconsistent aspect was realized by forcing a possible-worlds semantics to be based on inconsistent worlds, and then adding modal operators to the language in the usual way. Our strategy here, though, will be in a sense a strategy of 'minimal deviation'. What we want to do is to keep the worlds entirely classical, considering instead a paraconsistent negation and a consistency connective as the sole primitive modal operators, that is, the only primitive operators whose interpretation would require looking at other (classical) worlds. In theory, that intent should not be too hard to fulfill, if we first think of paraconsistent negation as dual to intuitionistic negation and recall the general lines of the well-known modal interpretation of the latter in terms of $S4$, and if we look next for an operator that satisfies the conditions for behaving as a consistency operator is supposed to behave, from the very definition of **LFI**s. In the paper Lea, however, we want to deal with only one aspect of the problem, throwing away the paraconsistent negation and working directly with the consistency operator as added to an entirely classical language.

A possible difficulty that might arise from the above plot is the following. If consistency is to be understood as what might be lacking to a paraconsistent negation in order to make it explosive, what sense can be given to consistency as a connective of a logic when a paraconsistent negation is *not* present from the start? While in a consistent logic a sentence cannot be true together with its negation, in a paraconsistent logic that is not the case. So, while in a consistent logic one can conclude from the truth of a sentence the falsity of the negation of this sentence, in a paraconsistent logic it would be convenient that, in general, the negation of a sentence is at most *possibly false* when this sentence is true. On that modal understanding of paraconsistent negations, an inconsistency will turn out to be *a sort of accident*: A sentence that proves to be inconsistent is one that is true in the current state of affairs but false in at least one possible alternative state of affairs. On the one hand, from a semantic perspective this could be criticized as too weak an interpretation for the notion of inconsistency. Is that all that paraconsistency is about, dealing with *this* kind of inconsistencies? On the other hand, from a more pragmatic perspective it seems quite appropriate: Has inconsistency ever been anything else than an (unfortunate) accident? Further discussion of such interpretation of negation is to be found in a later step of the present process.

2. Devising a plan. In logic it is often easier to attack the whole problem at once instead of attacking just part of it. The fact is hardly surprising: If you use a richer language you should be able to say more things, and if your device has more components then it is more likely that you will find a way of conforming to it the tools you have. In order to fix a modal understanding for our new connectives of consistency and inconsistency, it is worth looking at what happens when the full language of **LFI**s is considered, as in **Chapter 3.3**. So, here is how we will do it: We will steal those new connectives from the latter paper and try to investigate them in **Chapter 3.1** as added to an entirely classical language. This way we can also try to make sense of those connectives for their own sake: Can we justify their use independently of the presence of a paraconsistent negation? Is the alternate reading of these connectives as connectives of essence and accident justified? Should this study be an appendix of the paper PARANORMAL or is it sufficiently independent to deserve rather a paper for itself?

Here are some more practical questions one may ask. Has this problem been studied before? Or maybe a similar problem? The well-known philosophical notion of *contingency* seems strikingly similar to the notion of *accident* that I present here. But they're distinct: A contingent truth is one that, from the present state of affairs, *could* be true but could *also* be false; an accidental truth, I recall, is one that *is* true in the present state of affairs as a matter of fact, *yet* could be false had things been otherwise. Another usual reading for 'contingent', in a classical framework, is the one that calls contingent any formula that is neither a tautology nor an antilogy. The latter reading could in fact be related to the former one, but we might as well simply ignore that issue here. The relevant questions for us at this point are: Were the notions of contingency and of non-contingency formally studied in modal logic? Were the technical problems involved in that study completely solved there, and if so can we use a similar approach to help in devising our own solutions to similar problems?

The earliest paper in which a modal logic is investigated in a language containing no primitive boxes nor diamonds but only the (non-normal) modal connectives of contingency and non-contingency as added to the classical language was Montgomery & Routley's [54]. The problem of axiomatizing the usual modal logics using this alternative language was formulated there, but only a few cases of classes of frames in which boxes and diamonds *did* turn to be definable from the new language were investigated. This paper was followed by a number of similar studies (cf. [56, 55, 57, 58]), most of them quite shallow from a technical viewpoint, none of them solving the original problem in full, for arbitrary classes of frames. From a number of possible philosophical uses for that language, not many were really explored until in [73] Routley used the language of contingency to formulate the "radical conventionalist thesis that all assertions of modalities are contingent". Almost two decades went by before a really important technical contribution was made. The interesting paper [22], by Cresswell, came and offered some conditions for the non-definability of boxes and diamonds in the language of non-contingency. The technique is standard, yet very useful. It is based on proving that the geometries of the canonical models of certain non-contingency logics do not allow for the definition of box or diamond (in their usual interpretations). This method can be partly adapted for the language of essence and accident. Cresswell's paper also provided an example of a logic of non-contingency that does not require reflexiveness from its adequate class of frames yet allows for the definition of the usual modal connectives. To find an analogous example is still an open problem for our logics of essence and accident, as explored in my paper.

Another decade had to wait before the initial problem of (non-)contingency would be finally extensively solved, by Humberstone (cf. [38]), in a very readable and instructive paper. It should be noticed, however, that Humberstone's solution, an axiomatization for the minimal logic of non-contingency with an infinite number of primitive rules, is not as simple and elegant as one might expect. At any rate, his paper also proves some interesting results on the definability of classes of frames from the poor language of non-contingency, and these can be partly adapted for the language of essence and accident. Finite axiomatizations for the minimal logic of non-contingency were found immediately after that, by Kuhn (cf. [43]) and Pizzi (cf. [63]). Pizzi's paper studies several non-equivalent formulations of the notion of contingency, and solves the problem for the received notion by way of the construction of a canonical model whose accessibility relation connects pairs of worlds to pairs of worlds. Kuhn's solution is the simplest one, despite its somewhat mysterious rationale. It does work well, however, and it can be adapted for the language of essence and accident.

All that said and done, the paper Lea was not intended to dwell on matters related to non-contingency, but it sought instead to explore a very precise modal definition of 'consistency' on its behavior as a connective for 'essence'. One thing that is not mentioned there, but will be noticed by any good reader, is that, even when the modal definition of paraconsistent negation $\neg$ is kept fixed as it is (namely, as the possibility of a classical negation), there are of course other modal definitions of consistency that will allow for the resulting logic to be characterized as an **LFI**. I explored only the definition that seemed more novel and more general. But in some cases even the modal connective $\triangle$ of 'non-contingency', as discussed above, will serve quite well in order to define a connective $\circ$ of 'consistency' (not by coincidence, the definition of consistency in **D2**, as exposed in the **Errata** to the **Chapter 1**, mocked the form of a non-contingency connective). As a matter of fact, in any class of reflexive frames we can check that both $(\triangle p \wedge p \wedge {\sim}p)$ and $(\circ p \wedge p \wedge {\sim}p)$ are explosive, while none of $(\triangle p \wedge p)$, $(\triangle p \wedge {\sim}p)$, $(\circ p \wedge p)$ and $(\circ p \wedge {\sim}p)$ are explosive. Moreover, as it was pointed out in the final section of the paper, in such classes of frames the formulas

$\triangle p$ and $(\circ p \wedge \circ{\sim}p)$ are equivalent —as predicted indeed by Walt Whitman in the quote that opens the paper.

3. Carrying out the plan. I can dissert about it, but you can also read the paper and check what was done, and how. Section 1 sets the stage, defining the language of essence and accident. Section 2 axiomatizes the minimal logic of essence and accident, to wit, the set of theorems and inferences validated by the above language with no restriction presumed over the set of frames. The strategy for the 'desessentialization' of a world, used in the construction of the canonical model, is adapted from Kuhn's strategy, mentioned above. Obviously, some changes are in order. Our Lemma 2.4 proves properties of the canonical model, and it is fundamental for the completeness result. This lemma clearly had to be adapted so as to conform to the properties of the present language, different from those of the language of non-contingency. Section 3 discusses the definability of the more usual modal connectives from the language of essence and accident. A certain definition is shown to work only for extensions of KT, and some of Cresswell's results about non-definability in the language of non-contingency are adapted to the present language. Not much more is proved there than the straightforward. Section 4 modifies the approach by Humberstone to the logics of non-contingency to show that the logics of essence and accident are even less expressive than the former, in a sense, in not being able to characterize many more usual classes of frames. Section 5 shows some connections between the earlier language of non-contingency and the new language of essence and accident and discusses some viable philosophical uses for the new notions.

The short paper Lea is packed with novel ideas and results. Some of them, however, have barely scratched the surface of the space of possibilities. This does not mean, though, that they are 'underdeveloped' in any respect. I believe that what was already done was much more than just a good start. That many destinies are left open for exploration is a sign that many roads were paved. They can only be improved for the traffic as the signals get installed, from now on. The connections of this paper to the subsequent one, Paranormal, are not overemphasized in the Lea for two main reasons. First, because it was unnecessary to do it, and even inadvisable for the the sake of relative independence of the two lines of investigation. Second, because the connections will be obvious anyway for the perspicacious reader. As it will be seen, the paper Paranormal could be understood, as a matter of fact, as a natural continuation of Lea in which the modal language of the latter is enriched with specific modal connectives for (non-classical) negation, and 'essence' is reinterpreted (abusively? naturally?) as 'consistency'.

4. Looking back. Because we're not angry young men (anymore), we'll now look back, but not in anger —we'll learn instead to accept the rituals of our society, as long as our society accepts us. Let us appreciate what was accomplished so far, to the extent that it relates to some interesting possible directions of continuation for this work, and then briefly discuss the reach and the significance of the present study.

Looking at the more recent literature on non-contingency, one should heed a few promising lines of investigation that have been trodden by Zolin. In [90], the author looks at the counterparts of some usual modal axioms in the language of non-contingency and finds the logics axiomatized with the help of such axioms to have first-order definable classes of frames. Yet he shows that such classes of frames do not coincide with the classes of frames that are characteristic of the related axioms in the more expressive language of boxes and diamonds. In particular, any class of frames definable in the non-contingency language can be shown to contain the class of functional frames. Moreover, on the path of completeness results, the canonical frame of the non-contingency version of any logic containing the seriality axiom is proven to be non-serial. Results parallel to these are still to be sought for the present language of essence and accident. Other papers by Zolin extend the use of non-contingency even further. A number of non-contingency logics receives in [92] (*apud* the reviewer) sequent-style versions that enjoy the Craig interpolation property but not cut-elimination. And in [91] (again, *apud* the reviewer), the meaning of non-contingency in the context of provability logics is investigated: Non-contingency is there interpreted very naturally as 'formal decidability', and completeness with respect to finite irreflexive transitive frames (as in Gödel-Löb's logic of provability) is attained. Similarly, in the language of /essence/consistency/ and /accident/inconsistency/, the meaning of 'formal consistency' and related connectives should still be studied in the same context of provability interpretations. In [48] I venture a first step in this direction: Assuming proofs to be defeasible, a paraconsistent negation is used to represent the notion of 'admissible falsehood' and a paracomplete negation is used to represent the notion of 'refutable truth'. Check it out.

Another interesting line of investigation seems to be the following. In the paper Lea, my approach to the modalities of essence and accident was indirect. I tried always to axiomatize the set of theorems and axioms determined by some class of frames derived from some known normal modal logic. But those new modalities are *not* normal. It would be only natural to investigate them instead using some semantics that is more suitable for non-normal modal logics, as the one based on 'minimal models' (cf. [19]). As pointed out in [31], the main property presupposed by these models about the logic in question is that it should be 'congruential', that is, that the replacement property should hold for it. And that much we can count on.

At last, let me make some considerations on the philosophical aspects of 'essence' and 'accident'. In Aristotle's Metaphysics (cf. [20]), the notion of essence is nothing short than fundamental. Roughly speaking, according to Aristotle, to characterize an entity one would have to characterize the sort of things that individuate its substance, that is, one would have to say what it is to be that entity, what are its essential features. In fact, the very term '*essentia*' has been coined by the Roman translators in order to avoid repeating the peculiar yet frequent Aristotelian expression '*to ti ên einai*' (τὸ τί ἦν εἶναι), literally 'the what-it-is-to-be'. In Aristotle's Logic (cf. [80]), the essence of an entity would typically be fixed by a definition, where by definition one does not mean a set of words explaining the meaning of a term that denotes that entity, but some sort of account (λόγος) which signifies the what-it-is-to-be for that entity. As I understand it, a modern realization of such an account might take us away from definite descriptions and closer to grammar and game-theoretical interpretations. But I had better drop the coin here, and move on.

Among all subsequent philosophers, Leibniz arguably had one of the richest modal idiolects. Necessity, contingency, essence, all the modal operators that we here discuss seem to appear in Leibniz's writings, and they frequently appear in fact in their attributive reading. In that reading, essential features are typically used as a justification for necessity ('what is necessary is so by its essence, since the opposite implies a contradiction' —V letter to Clarke, cf. [1]). This interpretation was applied, among other things, to update the Ontological Argument of St. Anselm: The proposition expressing God's existence would, in our present terminology, constitute an 'essential truth' —if God exists, It exists by way of necessity.

Jumping now to contemporary times, Wittgenstein in the *Tractatus* makes yet another use of the language of essence and accident (*Wesen* and *Zufall*, cf. [88]). Propositions are said to have essential and accidental features (verse 3.34): Essential features of a proposition are exactly those that are needed for it to express its sense, and what it has in common with other propositions sharing the same sense. Moreover, the world is constituted of atomic facts (verses 1 and 2), and those facts are essential combinations of things (verse 2.011). Logical facts are non-accidental (verse 2.012). Yet the facts of the world are wholly accidental, and the sense of the world lies outside the world (verse 6.41). In spite of the use of many modal terms and modal figures (to the point of having influenced Carnap on his approach to modal logic), Wittgenstein seems willing to shut the door to the employment of a fully modal language, in insisting that propositions are very specific kinds of truth-functions (verses 5 and 6) —though it has been modernly argued that the Tractarian semantics can be very naturally reconstructed in modal terms (cf. [44]). With a poor language and a bold intention, no wonder that the philosopher should conclude the book by asserting that there are many things about which he should stay silent (verse 7).

The reader will notice from the above historic examples that there is nothing like a standard use of the modal idiom in traditional discursive philosophy (I use 'discursive' here as opposed to a more formal, or 'technically informed', kind of philosophy). While an extensive philosophical literature was dedicated, and with a good reason, to the investigation of sentences with an essential content, for a long long time no good soul would make an effort so as to guarantee that there was a sufficiently precise and rich language adequate to the expression of quiddity. The paper Lea was aimed as a contribution to the logico-metaphysical legitimation of basic modal languages capable of expressing essence and accident in their assertoric uses (as discussed in the final section of Lea.

Substantial work should still be carried out in order to illustrate the advantages of the present use of the notions of essence and accident in philosophy. I find particularly promising their use in investigating Kripke's notion of 'rigid designation', if only to clarify by appeal to some clear-cut sort of essentialism how and in which conditions some identity propositions could be both necessary and *a posteriori* (cf. [42]), or to apply the same idea to more general propositions involving the characterization of proper names, general terms, or of natural kinds. Kripke's theory was an update on the anti-rationalist Humean theory about the existence of *a posteriori* truths, as recovered by the linguistic conventionalism of the logical positivism, and a reaction *both* to the collapse of the metaphysical and epistemological modalities promoted by Quine and Barcan-Marcus in their understanding of the received doctrine of essentialism *and* to the theories of resemblance and counterfactuality defended by David Lewis and other people that took the idiom of 'possible worlds' a bit too serious. Again, this is no place to detail the above proposals any further. At any rate, it is interesting to check how, based on Kripke's ideas, Murcho (cf. [60]) has defended a version of 'naturalized essentialism' according to which there would be essential properties of particulars that do not constitute neither logical nor conceptual necessities. Many questions though are left open by such an analysis: Would there be essentialist assertions known only *a posteriori*? Beyond essence, which would be the sufficient conditions for the individuation of existents? Would some form of 'accidentalism' be as important as essentialism for the description of universals? Can this in fact be related to the negative characterization of logics and logical constants that I propose in **Chapter 4.1**? Does that give us a hint about the possibility of providing negative characterizations for general terms or for natural kinds? Or would the 'anti-essentialist' posture be much more coherent, after all? A formal approach to these matters might help in settling a few answers, by allowing us to more easily evaluate the consequences of each philosophical stance.

Such issues shall here be left as matter for future work. The reader is invited to contribute.

Some esoterism

In spite of the impression left by the work of some algebraists, the research on algebraic logic is intended to make logics (and life) *simpler*. It will be somewhat disappointing, however, that some logics like da Costa's C_1 should turn out to be just *too* simple. However, instead of subscribing to the received wisdom according to which "logic can (and perhaps should) be viewed from an algebraic perspective" (cf. [37]), one could well oppose that reductionist strategy and use the very simplicity of some logics to argue for a non-algebraic study of them (cf. [4]).

1. Of algebraization. Let me expand on that. In mathematics, a *congruence* relation over a given structure is simply an equivalence relation over its domain that is /closed under/compatible with/ the operations of this structure. There are at least two obvious ways of quotienting a given structure so as to produce degenerate related algebraic structures, namely, through the roughest congruence relation, that puts every element of the domain in the same class, or through the finest congruence relation, the identity, that makes of every element a class of its own. Both ways are pretty fruitless: The former cannot discern an element from any other element, the latter understands that no two elements can ever be identified. An algebra that only admits of these two degenerate congruences is called *simple*. In the same spirit, a logical matrix for a non-overcomplete logic[1] is said to be simple (cf. [89], p.198) if the identity relation is the only congruence that it admits of, and a simple logic is a non-overcomplete logic that only admits of a single congruence relation, the identity (cf. [4]).

When we define a congruence over a given (tarskian) logic, we expect it to divide the set of formulas into classes whose elements are all *indiscernible* with respect to that congruence. The most straightforward style of algebraization for a logic is the so-called Lindenbaum-Tarski procedure, that makes use of the associated consequence relation: Whenever two formulas are interderivable they are put in the same class. If that procedure defines a congruence and associates a non-degenerate quotient algebra to the logic, the logic is said to be LT-algebraizable. In that case, the logic will also automatically respect the so-called *replacement property*, according to which the derivability of a formula φ from a theory Γ is preserved when subformulas of φ are replaced by equivalent formulas. For LT-algebraizable logics, equivalent formulas are thus indiscernible.

Now, there are other logics for which the replacement property holds good and there are also logics for which non-degenerate congruence relations can be associated, but that still fail to be LT-algebraizable (recall subsection 3.12 of the paper TAXONOMY, in **Chapter 1.0**). Nonetheless, adequate algebraic semantics can often still be associated to such logics, and several other styles of algebraization might apply to them, making them qualify for instance as 'protoalgebraizable', 'weakly algebraizable', 'equivalential', or 'Blok-Pigozzi-algebraizable' (cf. [11]). Simple logics, however, are hardly ever considered to be 'algebraizable' in any interesting sense of the word.

It had been known since long (cf. [27]) that the logic C_1 and its early companions fail the replacement property. Two decades had to pass though before Mortensen (cf. [59]) added to that result the observation that these

[1] Check the next chapter for a comprehensive definition of 'overcompleteness'.

logics are simple. So, in spite of early attempts by da Costa to provide adequate 'algebraic' counterparts for the logic C_1 (cf. [25]), following a recipe of Curry (cf. [23]), and some late attempts by other authors (cf. [18, 76]) to resurrect and update that approach, the logic C_1 was not to admit of any other congruence relation than the identity.

Contrary to anecdotal evidence, people do not algebraize just because that's what they do in life. From the point of view of the logician, the questions one should bear in mind are related to the applications of the algebraic tools, as guaranteed by the 'bridge theorems' that connect algebraic properties to (meta)logical properties. Any new algebraic tools that are proposed for the use in logic should substantiate their claim for deserving any attention from the community by providing material for bridges to be built. So, the really interesting question in the end is not so much whether algebraic counterparts can be associated to given logics, but what contribution the former structures can give to the latter once you have designed them.

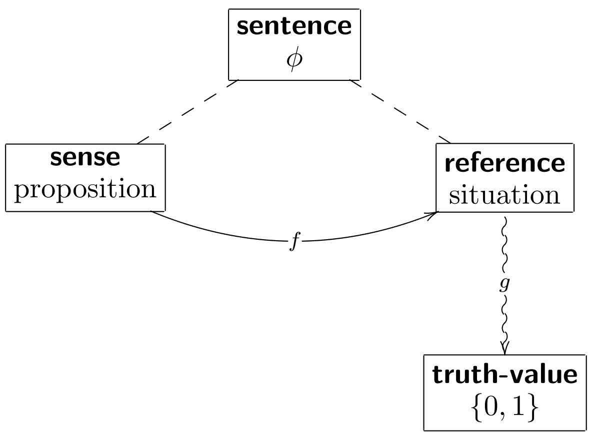
Some people think that logics that do not have a 'normal' algebraic counterpart are unworthy of consideration. Others have argued that 'normal' logics always have some built-in linguistic-algebraic element, say, in the way they build their set of formulas (cf. [46]). The Bourbakian architecture of mathematics (cf. [12]) proposed a structuralist *division of labor* according to which every mathematical structure would be characterized either as an algebraic structure, as an order structure, as a topological structure, or else as a 'multiple structure' that combine characteristics of more than one of the preceding 'mother-structures'. According to the modern Polish approach to 'logical calculi' (cf. [89]), logics are to be seen as hybrids including multiple elements of the Bourbakian mother-structures, yet it should be acknowledged that some profane authors have recently strived to eliminate the linguistic aspect of that approach (cf. [52]). An independent approach that has proposed to regard logics as a fourth class of mother-structures in their own right is that of *Universal Logic* (cf. [3]).[2] Free from the algebraizing impetus that would use algebra to justify investigations in logic and substantiate the very recognition of logical structures as real mathematical structures, universal logicians could still criticize a logic for failing the replacement property or for not being algebraizable in any usual sense, but they would not expel a logic from the realm of mathematics just because it turned out to be a simple logic. Quite to the contrary, as argued by Béziau in [4], such examples of logics that "cannot be reduced to algebra" could

[2]In defense of the great Poldavian mathematician, one should admit that the paper [12] had already left some space open for new mothers like Universal Logic to emerge. The old Nicholas, in spite of anathemazing the 'lifeless skeleton' of formal logic, praises the 'axiomatic method' and adverts that his 'rapid sketch' of the 'whole of the mathematical universe' is but *frozen*, as he writes that: "The structures are not immutable, neither in number nor in their essential contents. It is quite possible that the future development of mathematics may increase the number of fundamental structures, revealing the fruitfulness of new axioms, or of new combinations of axioms."

even be claimed to furnish some evidence for the thesis that the algebraic perspective cannot exhaust the wealth of logical investigations.

2. Some puzzles. With the same paper, [34], Frege helped at the same time to found analytic philosophy and to puzzle generations of logicians with his proposal of splitting the meaning of a sentence into its 'sense' (*Sinn*) and its 'reference' (*Bedeutung*). Modern first-order logic was also born with Frege (as well as with Peirce, and Schröder) in his *Begriffsschrift*, a few years later (1879), as "a formula language, modelled on that of arithmetic, of pure thought". This book was supposed by his author to be fundamentally different from the other more famous study of the 'Laws of Thought', Boole's landmark piece that preceded Frege's by more than 4 decades (1854). Using loan words from the Leibnizian vocabulary, Frege repeatedly stated (against Schröder) that his own approach was intended to provide for a *characteristica universalis*, a universal language to be applied first to mathematics and then to real world problems, while Boole's was a mere *calculus ratiocinator* intended for mechanically deducing all possible truths of (propositional) logic from the list of simple thoughts in a purely syntactical fashion (cf. [78]).

For the good or for the bad, modern tradition in Abstract Algebraic Logic has associated Frege's name to a particular class of logics for which a property somewhat stronger than replacement holds good, namely a sort of 'contextual replacement' according to which, for each theory Γ of a logic **L**, the class of formulas that are equivalent in the presence of Γ define a congruence relation over **L** (see [62, 32]). In [24] the authors explain this property by saying that a logic is Fregean "if interderivability is compositional". They claim that this idea is based on Roman Suszko's formal reading of Frege. I am not sure though that this is a good reading of Suszko. I explain. The issue here concerns the Fregean notion of 'sense'. While Czelakowski and Pigozzi might well be right, in [24], in saying that Frege "viewed this concept as extra-linguistic and did not attempt to incorporate it in his formal system", that is clearly not how Suszko himself chooses to formalize Frege. Schematically, this is how Suszko reconstructs the Fregean picture, in [81]:



According to the Suszkian Frege, a sentence's meaning can only be understood in terms of its sense and its reference, the latter being a function of the former. In addition, claimed Suszko, the picture would only be complete after the 2 classical truth-values were added as a function of the reference, for it is in them that resides the "genuine definition of logic". The reference of a sentence is maintained as an intermediary step in between the sentence and its truth-value, a step that one in general cannot get rid of, "unless one agrees that thought is about nothing, or, rather, stops talking with sentences". The 'algebraic truth-values' of many-valued logics were to play thus a referential role, while only two 'logical truth-values' would really exist. No further logical values were possible, for "obviously any multiplication of logical values is a mad idea" (cf. [82]). In his typical grandiloquent style, in this summary of a talk given in 1976 to the 22nd Conference on the History of Logic, Suszko complains that "after 50 years we still face an illogical paradise of many truths and falsehoods" (the 'truths' being the designated values of many-valued logic, and the 'falsehoods' the undesignated values). But he knows all too well who is to blame for that, as he adds: "Łukasiewicz is the chief perpetrator of a magnificent conceptual deceipt lasting out in mathematical logic to the present day". Suszko would enter the history of logic, as I showed in detail in the **Chapter 2.1**, for the idea that any semantics of a tarskian logic could in principle be reduced to a 2-valued semantics. So, the trade-off would in general involve the decision of retaining either truth-functionality or bivalence in our semantics, as we have discussed in our research note [17], the paper [14], and the forthcoming paper [15]. Finally, it should be remarked that, considering what has been said above about 'contextual replacement', for Suszko *congruent* formulas are simply formulas that have *synonymous senses* according to a given theory (cf. [81], supplement III) —and on that Suszko claims to be following Quine's notion of 'cognitive synonymy', proposed in order to explain the notion of 'analyticity' (cf. [71]).

A related puzzle left to us by Suszko should here be mentioned, namely, his own notion of a 'Fregean logic'. According to Suszko's [81], the main objective of his analysis is to get us rid of the 'intensional ghosts of modality' (a term he partly borrows from Herman Weyl's [87]). Suszko clearly dislikes traditional possible-worlds semantics. Apart from that, he drops only here and there some hints about what it means to be 'non-Fregean', as in: "The construction of [the] so-called many-valued logics by Jan Łukasiewicz was the effective abolition of the Fregean Axiom" (cf. [82]). It is unfortunate that Suszko does not appear to be willing to formally clear up beyond any doubt what he means by the 'Fregean Axiom'. For one thing —and recalling that Suszko regards truth-functionality and replacement as negotiable properties of a logic, while he takes structurality for granted at least since [46]— this 'Fregean axiom' was supposed by Suszko to be the main responsible for the format in which '2-valued extensional logic' came to be consacrated. "How was it possible that the humbug of many logical values persisted over the last fifty years?", he asks us in [82]. In this paper, and in [81], Suszko says, on the one hand, that the 'Fregean axiom' is 'equivalent' to a restriction in the possible referents of a sentence to the two elements from its set of possible truth-values and, on the other hand, Suszko also asserts in the latter paper that "because of the Fregean axiom, the replacement property of logically equivalent formulas holds in Fregean logic". These comments are certainly intriguing, given that 2-valuedness and replacement are completely independent properties of a logic! For a one-sided attempt to resolve this puzzle through partial replacement the reader is invited again to consult Czelakowski & Pigozzi's [24].

3. Of replacement. Where did we stop? I recollect and continue. As we have seen, the replacement property needs to be satisfied for a logic to be LT-algebraizable. For such a logic, equivalent formulas are indiscernible: If it has top particles, for instance, they will group into the same congruence class; similarly for bottom particles. From the point of view of the quotient algebra, any member of a congruence class behaves just like any other of its synonymous companions, and it can legitimately represent them for all operative purposes. This simplifies the initial task of working with the whole language of the logic. Not all logics are prone to such an algebraization procedure, however. Some logics, like C_1 (our **Cila**), cannot be further simplified. They are, so to speak, *anarchistic*: No formula has the right to represent any other formula. In case a non-degenerate congruence can be defined over a given logic, though, this logic gives hope for an algebraic treatment. Indeed, as we have seen in Section 3.12 of the paper TAXONOMY, in **Chapter 1**, there are extensions and alternatives to **Cila** that are not simple: The logic **Cilo**, for instance, is '(finitely) equivalential', and the 8K 3-valued maximal logics that extend **Cia** and were mentioned in that paper are all 'Blok-Pigozzi-algebraizable'. Some form or another of the replacement property always play a role in this process of defining classes of indiscernible formulas. Now, if replacement alone is not capable of guaranteeing that a logic falls into one of the main classes of algebraizability,[3] it does at least help a good deal in guaranteeing that the logic is not simple and not completely degenerate from an algebraic point of view. All that said and done, from this point on, in this section, I will be concentrating exclusively on logics satisfying the replacement property in its usual formulation.

According to Wójcicki ([89], chap. 3.2.0):

> We are free to create as many logical calculi as we wish, which certainly does not mean that the outcome of our activity will eventually turn out to be of any interest. Although there is no generally accepted

[3]The class of 'Fregean logics', the most demanding logico-algebraic class from the so-called 'Leibniz hierarchy', in [33], generalizing the Lindenbaum-Tarski procedure, requires not only replacement but also contextual replacement to be satisfied.

> definition of a 'good' or 'interesting' logic, we incline to consider certain properties of logical calculi as desirable whereas some others are not.

Among the 'desirable' properties of logical calculi the author includes *self-extensionality*, which is tantamount to the above mentioned replacement property in abstract logic. Moreover, in chapter 5 of the same book, one can find a proof that a structural tarskian logic is self-extensional if, and only if, it has an adequate class of 2-valued 'frame interpretations'. This is intended to establish a link between the replacement property and the logics having a usual modal-like semantics. The confidence about the existence of such a link is in fact shared by modal logicians and sympathizers (henceforth, *modalists*). Indeed, replacement is sometimes taken to constitute the characterizing property of 'classical operators' (check Segerberg's [75]) and, together with the duality between $\Box$ and $\Diamond$, it is taken to characterize as well what is known as the 'classical systems of modal logic' (check Chellas's [19]). In opposition to that, Béziau has argued, in [8], that to satisfactorily capture the notion of intensionality a logic must be non-self-extensional, and that people think that a logic must be self-extensional "rather because this is a nice technical and practical property than for any precise philosophical reason". Given that no precise philosophical reasons are offered by this author for us to take the contrary position either, and given that replacement is a property of every normal system of modal logic, I will be assuming here, together with the modalist tradition, that a logic that does not respect the replacement property is simply *not modal*, in the usual contemporary sense of the term.

Most paraconsistent logics and Logics of Formal Inconsistency presented in the previous chapters fail the replacement property[4] —a quite comprehensive result in that respect is Theorem 3.51 from the TAXONOMY, which shows that there are many paraconsistent logics which cannot even be extended so as to originate other paraconsistent logics that would satisfy replacement. Many authors have seen such a failure as a major technical and philosophical defect of the **C**-systems; some have thought that this was an intrinsic defect of paraconsistent logic in general. As we now know, the latter were wrong, while the former were… hmmm… misoriented. Richard Sylvan (née Routley), in [83], asserts for instance that the more traditional daCostian logics "appear to lack natural and elegant algebraic and semantical formulations, largely because they fail to guarantee intersubstitutivity of equivalents". Béziau says in [6] that "from the philosophical point of view there must be an intuition supporting the non-self-extensional behaviour of a negation",[5] and he repeats basically this same complaint in [7], insisting

[4] In the TAXONOMY this property was studied mostly from a syntactical perspective and was called (IpE), an acronym standing for 'intersubstitutivity of provable equivalents'.

[5] The author was mistaken, however, in that paper, in suggesting Sette's logic $\mathbf{P}^1$ as an example of a self-extensional logic.

that in the cases of logics like C_1, LP, and $\mathbf{J}_3$ "no philosophical justification for this failure has been presented". To that sort of analysis, da Costa and Otávio Bueno have retorted by comparing self-extensional logics with abelian groups (cf. [26]), saying that "from the perspective of pure logic, such a critique would be similar to that made by an algebraist who wishes that only commutative groups be studied".

Is the solution to the replacement quagmire to be found in the universe of modal logics? And how do we get there, finding convenient modal interpretations for all the connectives of our logics of formal inconsistency? Parts **3.2** and **3.3** of the present chapter will fully answer such questions.

The oldest paraconsistent logic ever, Jaśkowski's logic **D2** (cf. [39, 40]), was introduced as a certain fragment of the modal logic $S5$. Does it point the way out of our pickle? No, it doesn't. As I show in the paper ModPar (part **3.2** of the present chapter), **D2** and its close relatives are all Logics of Formal Inconsistency, as a matter of fact, but they all fail replacement and do *not* constitute thus examples of modal logics, in spite of the impression one might get from the related literature. This result is obtained, by the way, as a direct application of the Theorem 3.51(v) from the Taxonomy, our **Chapter 1.0**.

4. Of duality and modality. Paraconsistent logics have quite often been thought of, and with a good reason, as dual to intuitionistic-like logics, be they intermediate logics or, more generally, *paracomplete* logics (cf. [45]). Duality issues will in fact guide us from now on, and they will be very much explored for the rest of this thesis. But I am a newcomer on that scene. In the 30s and the 40s, several years before Jaśkowski's founding work on paraconsistency hit the press, one could find Karl Popper criticizing dialecticians for failing to take into account the Principle of Explosion that would render the theories trivial and uninformative in the presence of what he called 'embracing contradictions'. Later, though, after having engaged on a long dispute with Harold Jeffreys (recall note 10, in section 2.4 of the Taxonomy), and more or less at the same time in which da Costa was publishing his initial investigations in paraconsistency, Popper substantially updated, in [69], his first English-written paper ever, 'What is dialectic?' (cf. [65]), and added that he had in fact been thinking about a (paraconsistent) logic that would be dual to intuitionistic logic (cf. [66]), that he finally dismissed as too weak as to be useful (recall his argument about the failure of contraposition presented at section 3.3 of the Taxonomy). Popper was a distinguished critic of the logico-positivistic methodology and the verificationist approach to empirical sciences. To that he opposed a scheme according to which critical rationalism and falsificationism would take priority. The research in natural sciences would, accordingly, advance by the proposal of new theoretical conjectures and the attempt at refuting them through experimentation (cf. [67]). The strategy in that case —where some hypotheses and their negations could both temporarily be assumed to be unfalsified— would seem to appeal to a paraconsistent-like interpretation —where some propositions and their negations could both be assumed to be true. Not surprisingly, inasmuch as intuitionistic logic has been commonly given an interpretation as a verificationist logic of constructive truth, the falsificationist logic of constructive falsehood is expected to be paraconsistent.[6] The basic idea about the connections between falsificationism and paraconsistency has been explored by Popper's disciple, David Miller, in [53].[7] The constructive interpretation of the resulting logics can be found in Shramko's [77]. The obvious implications of the above ideas for Epistemology and for the Philosophy of Science make this an area of investigation that deserves a lot more attention.

Besides some previous scattered ideas and suggestions, *dual-intuitionistic* logic started to be developed only in the 80s, with the paper [36], by Nicholas Goodman, who called it 'anti-intuitionistic logic'. A Brouwer algebra, the dual to Heyting algebra, was the basic construct intended to represent the new logic, whose proof-theoretic presentation was based on single-premise-multiple-conclusioned inferences, dual to the multiple-premise-single-conclusioned inferences of intuitionistic logic in Gentzen's formulation of it. Several variations and extensions of that initial study of dual-intuitionistic logic were produced by Igor Urbas in [84]. Both intuitionistic logic and dual-intuitionistic logic have a constructive leaning, especially as reflected on the heredity condition of their Kripke semantics: While the former preserves truth towards the future, the latter preserves falsehood. Moreover, both logics satisfy the replacement property. On the other hand, the relational semantics of one of the earliest paraconsistent specimen, Nelson's logic (cf. [61]), takes both verification and falsification as primitive and equally important concepts, and takes both truth and falsehood as constructive notions, but the logic turns out to fail replacement. However, it should be pointed out that Nelson's logic admits of a non-degenerate congruence (for details, check chap. 6 of [86]) that from the point of view of abstract algebraic logic makes it qualify as a finitely equivalential logic (such congruence can be defined exactly like the non-trivial congruence defined for **Cilo** around the Fact 3.81 of the TAXONOMY). It would seem interesting to check whether a consistency connective could be naturally defined in this logic, to

[6] Curiously, in [13], a class of dual-intuitionistic logics called 'anti-constructive' are proposed with the following rationale: "This denomination can be understood taking into account that, as far as the intuitionistic philosophic program can be seen as committed to constructing truthood [sic], our anti-constructive logics can be seen as committed to eliminating falsehood". Such a purported 'anti-constructive falsehood elimination' remains at best unclear, however, as no interpretation is offered in the paper so as to justify it.

[7] Miller has even considered da Costa's C_1 as a possible 'logic of unfalsified hypotheses'. However, as he rightly recalls, the non-classical stance is not one that was favored by Popper himself, who insisted that "we should (in the empirical sciences) use the full or classical or two-valued logic" (cf. chap. 8 of [68]).

see if it also qualifies as a Logic of Formal Inconsistency. Another interesting line of investigation would seem to be the study of structures that are, in a sense, 'dual' to algebraic structures (but not necessarily in the sense of coalgebraic structures). For such structures, instead of privileging a certain notion of indiscernibility (or 'identity') given by congruence relations, a notion of 'apartness', 'discordance' or 'difference' would be expected to play a role. But I had better leave this point here as a somewhat vague suggestion, and move on.

A different modal-like interpretation of negation was contributed by the relevance logic community, starting with Routley & Routley's 'star operator' for the semantics of first-degree entailment (cf. [74]), greatly generalized later on by Mike Dunn (cf. [30]) in terms of a 'compatibility relation' that is added to Kripke frames. Roughly speaking, a negation sentence $\sim\alpha$ is true in a world x iff α is false in every world y compatible with x. The trick of course rests in defining the right conditions for compatibility in each case: In the case of Routley star, for instance, the compatibility relation is assumed to be symmetric, directed and convergent (cf. [72]). Yet another way of extending the notion of a modal semantics so as to apply it to larger classes of non-classical logics was proposed by Matthias Baaz in [2], as applied to da Costa's logic C_ω. In that paper, the structure of a Kripke model was enriched by a function T that associates to each world a set of negated sentences that, intuitively, are to be taken as true in that world independently of the truth-values of the subformulas. This results of course in a trick for making the underlying worlds non-classical without really touching them. Both the relevantist approach and the proposal by Baaz have the potential to enormously extend the scope of what can be called 'modal semantics'. Nonetheless, I will here be content with exploring the possibilities of the *usual* frame semantics —just a set of worlds and an accessibility relation connecting some of them.

A simpler way of defining a paraconsistent negation in the usual modal setting without committing oneself to the whole apparatus of dual-intuitionistic logic is by isolating and making use of its interpretation of negation —the dual interpretation of that of intuitionistic negation— independently of heredity conditions and of the particular semantics of implication. This of course entails the abandonment of usual 'constructive' interpretations of negation (cf. [86]), or at least the extension of the notion of 'proof' so as to take refutations and defeasible reasoning into consideration, as I propose in [48]. Let **not** denote a classical negation. Then, intuitionistic negation will be quite strong and demand for 'necessarily **not**', while paraconsistent negation will be more permissive and ask for 'possibly **not**'. Jean-Yves Béziau wrote a series of papers around the latter interpretation (cf. [5, 9, 10]), where this modal paraconsistent negation is explored inside the logic $S5$.[8]

[8] Béziau mentions a series of theorems and inferences validated or invalidated by this

In the 80s, however, such dual-intuitionistic negations had already been explored by Kosta Došen (for a survey, check [29]). Even earlier than that, this had actually been studied from a very general perspective, allowing for arbitrary conditions to be imposed over the accessibility relations. That study was done by Dimiter Vakarelov (cf. [85]), based on the (algebraic-related) thesis he had written on the theme in 1974.

Recall that in LEA, the first paper of this chapter, I will be showing how the language of classical logic can be enriched with a modal operator of consistency. If I now added to that language a modal paraconsistent negation, the resulting logics would obviously become **LFI**s. Moreover, one could then easily check that each normal modal logic can be recast in this new language, and vice-versa. But that would be too easy a solution, in fact, for we would have already started from a very rich language, including the whole set of classical connectives and a primitive classical negation. It would seem more interesting to check, instead, if the same solution could be attained if we started from the usual language of our **LFI**s, without a primitive classical negation, or even to check if the same could be done if we started from the language of positive classical logic plus the paraconsistent negation only. Section 4 of the paper MODPAR, the second paper in this chapter, hints at how both tasks can be successfully accomplished, and the paper PARANORMAL that closes this chapter shows in detail how they can be realized. The careful choice of initial language marks indeed the main difference between my present investigations and those of other authors. I base my study in the poor language of Béziau's logic **Z** (the paraconsistent version of $S5$), that is, from positive classical connectives plus connectives related to paraconsistency, and I show how to extend his proposal so as to cover all non-degenerate normal modal logics. In contrast, the papers by Došen start from the full language of intuitionistic logic and add to it extra non-classical negations, and the study by Vakarelov add those same non-classical negations either to the positive part of classical or to that of intuitionistic logic, but it also considers some further connectives to be always present, namely the 0-ary connectives denoting bottom and top particles.

Coda. Another attractive innovation from the paper PARANORMAL is the use of a multiple-premise-multiple-conclusion framework. That helps in easily characterizing an immediate notion of duality (reading inferences from left to right, or the other way around; changing 0's for 1's and vice-versa in any two-valued semantic characterization), that will be very important in the next, and final, chapter of the thesis. Paraconsistency can then be very easily understood as dual to paracompleteness (and such an idea was indeed

paraconsistent version of $S5$. In the paper MODPAR I point a mistake on his [10]'s list: The formulas $(\alpha \vee \beta) \to \neg(\neg\alpha \wedge \neg\beta)$, $(\alpha \vee \neg\beta) \to \neg(\neg\alpha \wedge \beta)$, and $(\neg\alpha \vee \beta) \to \neg(\alpha \wedge \neg\beta)$ are *not* theorems of $S5$, where $\neg$ denotes the modal paraconsistent negation. But there is also a related mistake is to be found in his [9]'s list: The formula $(\neg\alpha \vee \neg\beta) \to \neg(\alpha \wedge \beta)$ *is* a theorem of $S5$, contrary to what is affirmed there.

applied in Brunner & Carnielli's paper on 'anti-intuitionism', [13]). A natural question that might be entertained concerns the notions that are dual to consistency and inconsistency, notions that one might dub 'determinedness' and 'undeterminedness'. The related dual-**LFI**s, the class of logics that I dub **LFU**s, as proxies for *Logics of Formal Undeterminedness*, is then immediately characterizable. We gain thus a much better view of the world of *paranormality* —the world of both paraconsistency and paracompleteness.

A related contribution of the last paper of this chapter, applying the above mentioned notion of duality, is the proposal of a way of restoring and generalizing the classic-like 'square of oppositions', as a further step towards a more general 'theory of oppositions'. I will advance no more about this here, but recommend instead the reader to check the paper. The next chapter of the thesis will touch on this theme again.

A last important contribution of the PARANORMAL is the emphasis put on the so-called 'Derivability Adjustment Theorems' that show how **LFI**s and **LFU**s, in spite of constituting fragments of consistent and complete (or determined) logics, can recapture the full reasoning allowed by the latter. Thus, a gently explosive paraconsistent logic, for instance, fails the 'consistency presupposition' that is typical of many classic-like logics that extend it, but still such a paraconsistent logic can in principle recover the reasoning that depends on 'consistency assumptions', by directly adding such assumptions to the set of premises of a given inference that depends on them. This is, in a sense, *the* Fundamental Feature of **LFI**s, their most remarkable trait and essential virtue. And similarly for **LFU**s.

Brief history

I nurtured and cherished the idea of a modal approach to paraconsistent negations and the related perfect connectives of consistency and inconsistency for quite some time. While working on other more urgent issues I always kept that idea incubated on backlog and often suggested it as a research topic to colleagues. I started my own research on it by getting acquainted with the so-called 'Polish school of paraconsistency', which had allegedly produced logics with a modal flavor. I still remember being greatly surprised then to discover the amount of ambiguity and misunderstanding that exist in the literature concerning the so-called 'discussive logic(s)' generated by Jaśkowski's early approach to paraconsistent logic. Amazingly, I found most abuses and mistakes to be committed by an incredible sluggishness of the authors to simply go and read the original sources, the papers [39, 40]. I lectured about my own proposal of a modern reconstruction and generalization of discussive logic in July 2002 at the State University of São Paulo (BR) and in September 2002 at the Nicholas Copernicus University in Toruń (PL). As we know, and as the reader can recall from **Chapter 1** (and specially from its **Errata**), it turned out that Jaśkowski's logic **D2** can define a consistency connective but, despite the many claims to the contrary, this logic is certainly *not* modal in the usual sense of the word, as it fails replacement to start with. It was clear thus that it is not enough just to provide a modal interpretation to a non-modal paraconsistent logic by way of some handy trick. That kind of 'second-hand' interpretation is in fact not so hard to obtain, as it was shown in [28, 47], where translations were produced from usual 3-valued logics such as $\mathbf{P}^1$ and Ł$_3$ into usual modal logics such as T and $S5$. I discussed that interpretation in detail during a mini-workshop organized in Ghent (BE) in June 2001, together with Diderik Batens and Jean-Yves Béziau, on the multiple relations between paraconsistent and modal logics.

An early version of the paper Lea was put forward in mid-April 2004. It proposed modal interpretations for the consistency and the inconsistency connectives and investigated them independently of the presence of paraconsistent negations in the language. Its main results and some of their extensions had been tested in January 2004 on an audience from the XII Latin-American Symposium on Mathematical Logic (XII SLALM) at the University of San José (CR), and in March 2004 in a seminar of the Group for Pure and Applied Logic at the State University of Campinas (BR). Without a paraconsistent negation around, the consistency and inconsistency connectives appeared to have a compelling and reasonably original interpretation in terms of connectives for essential and accidental truth, and that philosophical interpretation was defended from the point of view of formal metaphysics in my contribution to the XI National Meeting on Philosophy (XI ANPOF Meeting), in October 2004 in Salvador (BR). The paper Lea was accepted for publication at the Bulletin of the Section of Logic of the University of Łódz (PL).

The two main forums I had for discussing and receiving lively feedback on my views related to Suszko's approach to abstract logic, structurality, algebraization, replacement and two-valued reduction of many-valued logics, as mentioned in the last section, were a talk given at the Seminaire de l'Institut de Logique et le Centre de Recherches Sémiologiques, on the occasion of a scientific visit to the Université de Neuchâtel (CH) in April 2003, and a contribution talk to the XII International Congress of Logic, Methodology and Philosophy of Science (XII LMPS), held in Oviedo (ES) in August 2003. My co-authors in [17] and [16] also had several occasions of presenting our related work on dyadic semantics, including two contributed talks to the III World Congress on Paraconsistency (WCP 3), held in Toulouse (FR) in July 2003.

The paper Paranormal was the natural complement to Lea, written in between the end of July and the beginning of September 2004, upgrading the language of Lea so as to obtain the full languages of **LFI**s and of their duals, the **LFU**s (Logics of Formal Undeterminedness). The main ideas of this paper were advanced in February 2004 in a seminar of the Center of Logic and Computation of the IST, in Lisbon (PT), and then presented as a contributed talk to the Logica 2004 Symposium, the XVIII international symposium promoted by the Institute of Philosophy of the Academy of Sciences of the Czech Republic, in Hejnice (CZ). A blend of ideas from this study and some ideas from the next chapter of the present thesis, together with a few further developments on the theme and their corresponding philosophical justifications, were presented under invitation at the International Workshop on Negation in Constructive Logic, promoted by the Professoriat for the Theory of Science and Logic of the Dresden University of Technology, held in Dresden (DE) in July 2004. Some related notes on modality, duality and natural language contained in the Paranormal had already been presented in a poster at the III World Congress on Paraconsistency (WCP 3), held in Toulouse (FR) in July 2003.

At last, the paper ModPar was planned initially as an abridged version of Paranormal. When I finally wrote the former in November 2004, however, I wanted it to bring in something new (namely, the issue about Jaśkowski's **D2**), and it ended up thus just by having a partial intersection with the latter paper, but not really coinciding with it. This text is soon to appear at the Logica Yearbook 2004, published in Czech Republic by ΦΙΛΟΣΟΦΙΑ.

For the many occasions I had of presenting this material, and for all the feedback I had on it, the people who commented, asked, doubted, wailed and gnashed their teeth, or just stared at me in utter wonder are too numerous to thank in but a few lines. I would only like to express here my special acknowledgement to Jairo da Silva, Marcelo Finger and Frank Sautter for their philosophical and technical appreciations of the herein contained papers during the preliminary oral examinations of the thesis, and to Jean-Yves Béziau for some important terminological clarifications. Of course, that does not mean that they would necessarily endorse my views on paraconsistency or on modal logic. The first two papers from this chapter were entirely written in Portugal under an FCT doctoral grant. The third paper was written under the same grant while I was back to Brazil for a longer stay.

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Logics of essence and accident

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> Logic and sermons never convince,
> The damp of the night drives deeper into my soul.
>
> (Only what proves itself to every man and woman is so,
> Only what nobody denies is so.)
>
> —Walt Whitman, *Leaves of Grass*, Song of Myself,
> sec.30 (1855–1881).

Abstract

We say that things happen accidentally when they do indeed happen, but only by chance. In the opposite situation, an essential happening is inescapable, its inevitability being the sine qua non for its very occurrence. This paper will investigate modal logics on a language tailored to talk about essential and accidental statements. Completeness of some among the weakest and the strongest such systems is attained. The weak expressibility of the classical propositional language enriched with the non-normal modal operators of essence and accident is highlighted and illustrated, both with respect to the definability of the more usual modal operators as well as with respect to the characterizability of classes of frames. Several interesting problems and directions are left open for exploration.

Keywords: philosophy of modal logic, non-normal modalities,
formal metaphysics, essence, accident

1 The what-it-is-to-be

A necessary proposition is one whose negation is impossible; a possible proposition is one that is true in some acceptable state-of-affairs. Necessity, $\Box$, and possibility, $\Diamond$, are the modal operators upon which the usual language of normal modal logics is built. We propose here, though, to study some interesting alternative modalities, namely the modalities of *essence* and *accident*. An accidental proposition is one that is the case, but could have been otherwise. An essential proposition is one that, whenever it enjoys a true status, it does it per force. We will write $\bullet\varphi$ to say that "φ is accidental", and $\circ\varphi$ to say that "φ is essential". In formal metaphysics there has often been some confusion between essence and necessity, and between accident and contingency. The present approach contributes to the demarcation of these notions. A quick comparison with the literature on non-contingency logics and some comments on alternative interpretations of the new connectives hereby presented will be postponed to section 5.

Let $\mathcal{P}$ be a denumerable set of sentential letters, and let the set of formulas of classical propositional logic, $\mathsf{S}_{\mathbf{CPL}}$, be inductively defined by:

$$\alpha ::= p \mid \top \mid \bot \mid {\sim}\varphi \mid (\varphi \wedge \psi) \mid (\varphi \vee \psi) \mid (\varphi \supset \psi) \mid (\varphi \equiv \psi),$$

where $p \in \mathcal{P}$, and φ and ψ are formulas. The set of formulas of the usual normal modal logics, $\mathsf{S}_{\mathbf{NML}}$, is defined by adding $\Box\varphi \mid \Diamond\varphi$ to the inductive clauses of $\mathsf{S}_{\mathbf{CPL}}$, and the set of formulas of the logics of essence and accident, $\mathsf{S}_{\mathbf{LEA}}$, is defined by adding instead $\circ\varphi \mid \bullet\varphi$ to the clauses of $\mathsf{S}_{\mathbf{CPL}}$.

A *modal frame* $\mathcal{F} = \langle W, R\rangle$ is a structure containing a set of worlds $W \neq \varnothing$ and an accessibility relation $R \subseteq W \times W$. A *modal model* based on that frame is a structure $\mathcal{M} = \langle \mathcal{F}, V\rangle$, where $V : \mathcal{P} \longrightarrow \mathsf{Pow}(W)$. The definition of *satisfaction* in a world $x \in W$ of a model $\mathcal{M}$ will be such that:

$\models^{\mathcal{M}}_x p$	iff	$x \in V(p)$
$\models^{\mathcal{M}}_x {\sim}\varphi$	iff	$\not\models^{\mathcal{M}}_x \varphi$
$\models^{\mathcal{M}}_x \varphi \vee \psi$	iff	$\models^{\mathcal{M}}_x \varphi$ or $\models^{\mathcal{M}}_x \psi$
	...	
$\models^{\mathcal{M}}_x \bullet\varphi$	iff	$\models^{\mathcal{M}}_x \varphi$ and $(\exists y \in W)(xRy \;\&\; \not\models^{\mathcal{M}}_y \varphi)$
$\models^{\mathcal{M}}_x \circ\varphi$	iff	$\not\models^{\mathcal{M}}_x \bullet\varphi$

The other classical operators are evaluated as expected. As usual, a formula φ will be said to be *valid* with respect to a class of frames $\mathbb{C}$, in symbols $\models^{\mathbb{C}} \varphi$, if $\models^{\mathcal{M}}_x \varphi$ holds good in every world x of every model $\mathcal{M}$ based on some frame in $\mathbb{C}$. We will write simply $\models$ for $\models^{\mathbb{C}}$ whenever the class of frames $\mathbb{C}$ can be read from the context. We say that a logic $\mathbf{L}$ given by some set of axioms Ax is *determined* by a class of frames $\mathbb{C}$ in case the provable formulas of the former coincide with the valid formulas of the latter.

Given a normal modal logic $\mathbf{L}$ determined by some class of frames $\mathbb{C}$, an *EA-logic* (of essence and accident) $(\mathbf{L})_{EA}$ is obtained by selecting all the formulas and the inferences in the language of **LEA** that are valid in $\mathbb{C}$. Notice that, in general, there is no reason why two logics $\mathbf{L}_1 \neq \mathbf{L}_2$ should imply $(\mathbf{L}_1)_{EA} \neq (\mathbf{L}_2)_{EA}$.

Recall that K, the minimal normal modal logic in the language of **NML**, determined by the class of all frames, can be axiomatized by:

All axioms and rules of **CPL**, plus

(0) $\vdash \varphi \supset \psi \quad\Rightarrow\quad \vdash \Box\varphi \supset \Box\psi$

(1) $\vdash (\Box\varphi \wedge \Box\psi) \supset \Box(\varphi \wedge \psi)$

(2) $\vdash \Box\top$

Sometimes it does not make much difference to work with $\mathsf{S}_{\mathbf{NML}}$ or with $\mathsf{S}_{\mathbf{LEA}}$, given that the modal connectives might turn out interdefinable. Indeed:

Proposition 1.1 Inside extensions of the modal logic K one can:

(i) take $\Box$ as primitive and define $\circ\varphi \stackrel{\text{def}}{=} \varphi \supset \Box\varphi$, $\bullet\varphi \stackrel{\text{def}}{=} \varphi \wedge \Diamond{\sim}\varphi$.

Inside extensions of KT, the modal logic axiomatized by $K + \vdash \Box\varphi \supset \varphi$ and determined by the class of all reflexive frames, one can:

(ii) take $\circ$ as primitive and define $\Box\varphi \stackrel{\text{def}}{=} \varphi \wedge \circ\varphi$.

2 The minimal logic of essence and accident

This section will prove that the axiomatization of $(K)_{EA}$, the minimal EA-logic of essence and accident (that is, the EA-logic determined by the class of all frames), can be given by the axioms Ax_K:

All axioms and rules of **CPL**, plus

(K0.1) $\vdash \varphi \equiv \psi \;\Rightarrow\; \vdash \circ\varphi \equiv \circ\psi$
(K0.2) $\vdash \varphi \;\Rightarrow\; \vdash \circ\varphi$
(K1.1) $\vdash (\circ\varphi \wedge \circ\psi) \supset \circ(\varphi \wedge \psi)$
(K1.2) $\vdash ((\varphi \wedge \circ\varphi) \vee (\psi \wedge \circ\psi)) \supset \circ(\varphi \vee \psi)$
(K1.3) $\vdash \bullet\varphi \supset \varphi$
(K1.4) $\vdash \bullet\varphi \equiv {\sim}\circ\varphi$

In particular, notice that:

Proposition 2.1 Here are some consequences of the above axiomatization:

(K2.0) Replacement holds irrestrictedly
(K2.1) $\vdash \circ\top$
(K2.2) $\vdash \varphi \supset (\circ(\varphi \supset \psi) \supset (\circ\varphi \supset \circ\psi))$
(K2.3) $\vdash \varphi \vee \circ\varphi$
(K2.4) $\vdash \circ\bot$

Proposition 2.2 Here are some alternatives to the previous axioms and rules:

(EAd) $\bullet\varphi \stackrel{\text{def}}{=} {\sim}\circ\varphi$ can be used instead of (K1.4)
(K2.3) instead of (K1.3)

We now check that the above proposal of axiomatization for $(K)_{EA}$ is indeed determined by the class of all frames. Soundness, $\vdash \varphi \;\Rightarrow\; \models \varphi$, can be easily checked directly, by verifying the validity of each of the axioms and the preservation of validity by each of the rules in Ax_K. It will be left as an exercise. Next, the standard technique for checking completeness is the construction of a canonical model $\mathcal{M}^* = (W^*, R^*, V^*)$, where:

W^* is the set of all maximally non-trivial sets of **LEA**-formulas
$x \in V^*(p)$ iff $p \in x$
$y \in R^*(x)$ iff $D(x) \subseteq y$

The only really difficult part here is the definition of $D : W \to \mathsf{Pow}(\mathsf{S})$, in order to settle the appropriate accessibility relation for this canonical model. The idea of using the 'desessentialization' of a world, $D(x) = \{\varphi : \circ\varphi \in x\}$, analogously to what is done in normal modal logics for formulas of the form $\Box\varphi$, does not work here, once the modality $\circ$ of essence itself is not normal. A clever solution adapted from [5] is to define $D(x) = \{\varphi : \circ\varphi \in x$, and $\circ\psi \in x$ for every ψ such that $\vdash \varphi \supset \psi\}$. A simpler solution that also works, adapted from [8], is to define $D(x) = \{\varphi :$ for every $\psi, \circ(\varphi \vee \psi) \in x\}$. The latter definition is the one we will adopt here. Using that one can then prove:

Lemma 2.3 (Lindenbaum) Every non-trivial set of **LEA**-formulas can be extended into a maximally non-trivial set of formulas.

Lemma 2.4 In the canonical model:

(P1) $\varphi \in D(x)$ and $\psi \in D(x) \;\Rightarrow\; (\varphi \wedge \psi) \in D(x)$
(P2) $\circ\varphi \in x \;\Leftrightarrow\; \varphi \notin x$ or $\varphi \in D(x)$
(P3) $D(x) \neq \varnothing$
(P4) $\varphi \in D(x)$ and $\vdash \varphi \supset \psi \;\Rightarrow\; \psi \in D(x)$
(P5) $D(x)$ is a closed set, that is, $D(x) \vdash \alpha \;\Rightarrow\; \alpha \in D(x)$
(P6) $\circ\varphi \notin x \;\Rightarrow\; \varphi \in x$ and $(\exists y \in W^*)(xR^*y$ and $\varphi \notin y)$

Proof For (P1), recall from **CPL** that $\vdash ((\varphi \vee \theta) \wedge (\psi \vee \theta)) \equiv ((\varphi \wedge \psi) \vee \theta)$. Thus, by rule (K0.1), we have $\vdash \circ((\varphi\vee\theta)\wedge(\psi\vee\theta)) \equiv \circ((\varphi\wedge\psi)\vee\theta)$. Call that theorem α. Now, from $\varphi \in D(x)$ and $\psi \in D(x)$ we can conclude that $\circ(\varphi \vee \theta) \in x$ and $\circ(\psi \vee \theta) \in x$, for an arbitrary θ. From axiom (K1.1), the theorem α and the maximality of x it then follows that $\circ((\varphi \wedge \psi) \vee \theta) \in x$.

For (P2), suppose first that both $\circ\varphi \in x$ and $\varphi \in x$. Then it follows, by **CPL**, the maximality of x, and axiom (K1.2), that $\circ(\varphi \vee \psi) \in x$, for an arbitrary ψ. For the converse, use axiom (K2.3), maximality, and the property (P1) for the particular case in which ψ is identical to φ.

For (P3), we may just check that any theorem $\top$ (such as, say, $\varphi \supset \varphi$) belongs to $D(x)$. Indeed, by rule (K0.2) we have that $\vdash \circ\top$, thus $\vdash (\top \wedge \circ\top)$. The result then follows from (K1.2) and the maximality of x.

For (P4), given $\varphi \in D(x)$ we know that $(\varphi \vee \pi) \in x$ for an arbitrary π, and in particular for $\pi = (\psi \vee \theta)$. But, from $\vdash \varphi \supset \psi$ we can conclude, using **CPL**, that $\vdash (\varphi \vee (\psi \vee \theta)) \equiv (\psi \vee \theta)$. The result now follows from (K0.1) and the maximality of x.

For (P5), given $D(x) \vdash \alpha$ we can conclude from property (P3), compacity and monotonicity that $\exists\theta_1, \dots, \theta_n \in D(x)$ such that $\theta_1, \dots, \theta_n \vdash \alpha$. But then, from property (P1) we have that $(\theta_1 \wedge \dots \wedge \theta_n) \in D(x)$, and from property (P4), using **CPL** and the maximality of x, we may conclude that $\alpha \in D(x)$.

At last, for (P6), assume $\circ\varphi \notin x$ and use first (K1.4), (K1.3) and the maximality of x to conclude that $\varphi \in x$. For the second part we have to show that such a world y exists, and as a prerequisite for the Lindenbaum Lemma we must be able to prove that $D(x) \cup \{\sim\varphi\}$ is non-trivial. To proceed by absurdity, suppose the contrary. Then, by **CPL** we will have that $D(x) \vdash \varphi$, and by property (P5) we conclude that $\varphi \in D(x)$. From property (P2) we have $\circ\varphi \in x$, contrary to what has been assumed at the start.

Theorem 2.5 (Canonical Model) $\models_x^{\mathcal{M}^*} \varphi \;\;\Leftrightarrow\;\; \varphi \in x$.

Proof This is checked by induction on the structure of φ. The cases of the classical connectives is straightforward. Now, consider the case $\varphi = \circ\psi$ (the case $\varphi = \bullet\psi$ is similar). Suppose first that $\circ\psi \in x$. Then, by property (P2) of the previous lemma we conclude that $\psi \notin x$ or $\psi \in D(x)$. By the definition of R^*, we conclude from $\psi \in D(x)$ that $(\forall y \in W^*)(xR^*y \Rightarrow \psi \in y)$. By the induction hypothesis, we have $\not\models_x^{\mathcal{M}^*} \psi$ or $(\forall y \in W^*)(xR^*y \Rightarrow\; \models_y^{\mathcal{M}^*} \psi)$, which means, by the definition of satisfaction (Section 1), that $\models_x^{\mathcal{M}^*} \circ\psi$. Conversely, suppose now that $\circ\psi \notin x$. By property (P6) we conclude that $\psi \in x$ and $(\exists y \in W^*)(xR^*y$ and $\psi \notin y)$. Again, the result follows from the induction hypotheses and the definition of satisfaction.

Corollary 2.6 (Completeness) $\Gamma \nvdash \varphi \;\Rightarrow\; \Gamma \not\models \varphi$.

3 Extensions of $(K)_{EA}$, and definability of $\Box$s and $\Diamond$s

In Proposition 1.1 we learned that $\circ$ and $\Box$ are interdefinable in extensions of KT. In general, let $\Rsh\colon \mathsf{S}_{\mathbf{LEA}} \to \mathsf{S}_{\mathbf{NML}}$ be such that $p^{\Rsh} = p$, $(\circ\varphi)^{\Rsh} = \varphi^{\Rsh} \supset \Box\varphi^{\Rsh}$, $(\bullet\varphi)^{\Rsh} = \varphi^{\Rsh} \wedge \Diamond{\sim}\varphi^{\Rsh}$, and $(\star(\varphi_1, \ldots, \varphi_n))^{\Rsh} = \star(\varphi_1^{\Rsh}, \ldots, \varphi_n^{\Rsh})$ for any other n-ary connective $\star$ common to both languages. We can say that $\Box$ is definable in terms of the language of $\circ$'s and $\bullet$'s of the logic $(\mathbf{L})_{EA}$ in case there is some schema $\circledcirc(p) \in \mathsf{S}_{\mathbf{LEA}}$ such that following is a thesis of $\mathbf{L}$ (i.e. is provable / valid in $\mathbf{L}$): $\Box\psi \equiv (\circledcirc(\psi))^{\Rsh}$. As a particular consequence of that, the following can now be proven:

Proposition 3.1 The definition $\Box\varphi \stackrel{\text{def}}{=} \varphi \wedge \circ\varphi$ is *only* possible in extensions of KT.

Proof To check that, one might just observe that in the minimal normal modal logic K the formula $\Box\psi \supset \psi$ can be inferred from $\Box\psi \supset (\psi \wedge (\psi \supset \Box\psi))$.

Recall that we have proved in the last section the completeness of $(K)_{EA}$, but the following still remains as an open problem:

Open 3.2 Provide a natural axiomatization for the logic $(KT)_{EA}$.

Given a frame (W, R), call a world $x \in W$ *autistic* (also known as *dead end*) in case there is no world accessible to it according to R, i.e. there is no $y \in W$ such that xRy. Call x *narcissistic* in case it can only access itself. Consider the axioms (V) $\vdash \Box\bot$ and (T_c) $\vdash \varphi \supset \Box\varphi$. The maximal normal modal logic $Ver = K + (V)$ is determined by the class of all autistic frames (i.e., frames whose worlds are all autistic), and the maximal normal modal logic $Triv = K + (T) + (T_c)$ is determined by the class of all narcissistic frames. Exactly midway in between Ver and $Triv$ lies the logic $TV = K + (T_c)$, determined by the class the class of frames whose worlds are all either autistic or narcissistic. It is easy to check that:

Proposition 3.3 (i) In $(Ver)_{EA}$, $\Box\varphi$ can be defined as $\top$. (ii) In $(Triv)_{EA}$, $\Box\varphi$ can be defined as φ. (iii) The logic $(TV)_{EA}$ can be axiomatized by $(K)_{EA} + \vdash \circ\varphi$.

Which other logics can be axiomatized and which logics can define $\Box$ in the language of **LEA**? A few related results, questions and conjectures will close this section.

Conjecture 3.4 $(K4)_{EA} = (K)_{EA} + \vdash \varphi \supset \circ\circ\varphi$, where $K4$ is the logic determined by the class of transitive frames.

Open 3.5 Find an example of a normal modal logic **L** distinct from TV and not extending the logic KT such that $\Box$ is definable in $(\mathbf{L})_{EA}$.

As in [3], the usual technique for non-definability results consists in showing that the geometry of the canonical model of $(\mathbf{L})_{EA}$ does not allow for the definition of $\Box$ in terms of the language of $\mathsf{S}_{\mathbf{LEA}}$.

Theorem 3.6 Let **L** be some normal modal logic. Then, $\Box$ is *not* definable in $(\mathbf{L})_{EA}$ if the canonical model of this logic contains at least one autistic world and one non-autistic world.

Proof Observe first that the formula $\Box\bot$ is satisfied by every autistic world, but it cannot be satisfied by any non-autistic world. On the other hand, we can check by induction on the construction of $\circledcirc(\bot)$ in the language of $\mathsf{S}_{\mathbf{LEA}}$ that such formula must have the same value in all worlds of the canonical model. Indeed, both the atomic case and the case of the classical connectives are straightforward. Moreover, if the values of the formulas $\theta_1, \ldots, \theta_n$ are the same in all worlds, so are the values of $\circ\theta_1, \ldots, \circ\theta_n$ (as they are all true). Thus, $\Box$ cannot in such circumstances be defined in terms of $\circledcirc$.

Notice that any logic that satisfies the conditions from the previous theorem is a fragment of Ver and also a fragment of KD, the modal logic axiomatized by $K+ \vdash \Diamond\top$ and determined by the class of all serial frames. That result was but a shy start. We are still left with the tough brain-teaser:

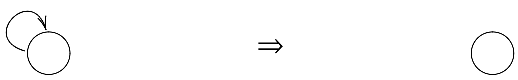
Open 3.7 Provide a full description of the class of all EA-logics in which $\Box$ is definable.

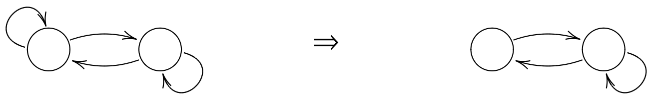
4 Characterizability of classes of frames

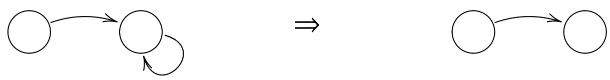
Another good test for the expressibility of a modal language consists in checking whether it can individualize many different classes of frames. A class $\mathbb{C}$ of frames will be said to be **LEA**-*characterized* in case there is some $\Gamma \subseteq \mathsf{S}_{\mathbf{LEA}}$ such that $\mathcal{F} \in \mathbb{C}$ iff $\models^{\mathcal{F}} \gamma$, for every $\gamma \in \Gamma$. Obviously, the class of *all* frames is **LEA**-characterizable (just take $\Gamma = \{\top\}$).

Say that a frame $\mathcal{F}^m = (W, R^m)$ is a *mirror reduction* of a frame $\mathcal{F} = (W, R)$ in case $\mathcal{F}^m$ is obtainable from $\mathcal{F}$ simply by erasing some or all reflexive arrows that appear in the latter, that is, in case $R \setminus \{(x, x) : x \in W\} \subseteq R^m \subseteq R$. Two frames are said to be *mirror-related* in case they are mirror reductions of some common frame.

Example 4.1 Here are some examples of mirror reduction:

(E1) 

(E2) 

(E3) 

One can now immediately prove the following Reduction Lemma:

Lemma 4.2

(RL1) If $\mathcal{F}^m = (W, R^m)$ is a mirror reduction of $\mathcal{F} = (W, R)$, then $\models^{\mathcal{F}^m} \varphi \Leftrightarrow \models^{\mathcal{F}} \varphi$.

(RL2) If two frames are mirror-related then they validate the same formulas.

Proof Part (RL1) can in fact be strengthened. Where $x \in W$, $\mathcal{M}^m$ is a model of $\mathcal{F}^m$ and $\mathcal{M}$ a model of $\mathcal{F}$, then an easy induction can prove that $\models_x^{\mathcal{M}^m} \varphi \Leftrightarrow \models_x^{\mathcal{M}} \varphi$. An interesting case is that of $\varphi = \circ\psi$ (or similarly, that of $\varphi = \bullet\psi$). First, note that $\models_x^{\mathcal{M}^m} \circ\psi$ iff $\not\models_x^{\mathcal{M}^m} \psi$ or $(\forall y \in W)(xR^m y \Rightarrow \models_y^{\mathcal{M}^m} \psi)$. Using the induction hypotheses, this reduces to $\not\models_x^{\mathcal{M}} \psi$ or $(\forall y \in W)(xR^m y \Rightarrow \models_y^{\mathcal{M}} \psi)$. In case $\models_x^{\mathcal{M}} \psi$ and xRx we obviously obtain $(\forall y \in W)(xRy \Rightarrow \models_y^{\mathcal{M}} \psi)$. The converse is straightforward.

Part (RL2) follows from (RL1).

As a consequence of the previous lemma, any **LEA**-characterizable class of frames must be closed under mirror-relatedness. In particular, note that:

Corollary 4.3 The following classes of frames are *not* **LEA**-characterizable:

(i) reflexive frames
(ii) serial frames
(iii) transitive frames
(iv) euclidean frames
(v) convergent frames

Proof Recall Example 4.1. The frame at the left-hand side of (E1) is both reflexive and serial, the frame at the l.h.s. of (E2) is transitive, and at the l.h.s. of (E3) we find a frame that is both euclidean and convergent. None of those properties is satisfied after mirror-reduction, as we can see at the right-hand sides of each example.

Compare the above with the more well-known situation of **NML**-characterizability (check for instance ch. 3 of [1]). The class of serial frames, for example, is **NML**-characterized by taking $\Gamma = \{\Diamond\top\}$.

Finally, here is a problem whose solution is highly non-trivial already in the analogous case of the language of **NML**:

Open 4.4 Provide a full description of the class of **LEA**-characterizable classes of frames.

5 On essence, and beyond

How much of our intuitions about essence and accident are captured by the new connectives $\circ$ and $\bullet$ studied above? And how do these notions differ from other usual modal notions such as those of *contingency* and *non-contingency*?

Suppose we extend the classical language by adding the unary connectives $\triangledown$ for contingency and $\vartriangle$ for non-contingency. The usual way of interpreting these notions is by extending the notion of satisfaction such that:

$$\models_x^{\mathcal{M}} \triangledown\varphi \quad \text{iff} \quad (\exists y \in W)(xRy \ \& \models_y^{\mathcal{M}} \varphi) \text{ and } (\exists z \in W)(xRz \ \& \not\models_z^{\mathcal{M}} \varphi)$$
$$\models_x^{\mathcal{M}} \vartriangle\varphi \quad \text{iff} \quad \not\models_x^{\mathcal{M}} \triangledown\varphi$$

The modal base for (non-)contingency was studied sporadically in the literature since the mid-60s (cf. [10]), for several classes of frames, and an axiomatization for the minimal logic of non-contingency was finally offered in [5], and immediately simplified in [8]. In the language of **NML** one could obviously define $\triangledown\varphi$ as $\Diamond\varphi \vee \Diamond{\sim}\varphi$ and $\vartriangle\varphi$ as $\Diamond\varphi \supset \Box\varphi$. One could now also easily consider the languages with both contingency and accidental statements and their duals, and then note for instance that $\models_K (\circ\varphi \wedge \circ{\sim}\varphi) \supset \vartriangle\varphi$ and $\models_{KT} \vartriangle\varphi \supset (\circ\varphi \wedge \circ{\sim}\varphi)$.

In the philosophy of modal logic, every modality has at least two central readings, a metaphysical reading that takes it as qualifying the truth of some statement, and an ontological reading that takes it as qualifying the properties of some object. Necessity, possibility, contingency and non-contingency were all used in the literature either in the metaphysical or in the ontological reading. Traditionally, the philosophical literature has often talked about essential and accidental properties of objects. A somewhat sophisticated way of internalizing that talk at the object-language level was devised by Kit Fine (cf. [4]), with the help of a sort of multimodal language in which there are operators intended to represent truth by reason of the nature of the involved objects, and a further binary predicate intended to represent ontological dependence. The present paper investigated instead a particular rendering of those notions in their naive metaphysical reading, simply by turning essence and accident into new propositional connectives.

Is the above reading solid from a philosophical standpoint? The question is not trivial to resolve. One has to concede that there is complete bedlam in the philosophical literature as potentially different kinds of modality often get conflated without much care. Sometimes one finds an identification between the notion of contingency and the notion of accident, sometimes necessity is opposed to contingency and the corresponding square of oppositions is turned into a triangle (maybe the Reverend has stolen a diamond, as in Stevenson's story?), sometimes the analytic $\times$ synthetic distinction is reformulated in terms of essential $\times$ accidental modes of judgement (somehow perverting Kant's proposal to understand essence as expressing an *a priori* synthetic truth). To be sure, the same terms can indeed receive several (hopefully related) uses in different areas of philosophy. But considerable prudence should be exercised so that the corresponding notions do not confound, and so that they do not get too circumscribed nor too stretched in their meanings.

The grammar of modalities in formal languages can often be mirrored in the grammar of adverbs in natural language (or was it the other way around?). Let's explore this analogy a bit. Adverbs are parts of speech comprised of words that modify a verb, an adjective, or another adverb. The first two cases are of interest here. In case the adverbs modify a verb, they derivatively modify a sentence of which this verb is the main verb. The *assertoric* status of the sentence is then subjected to the mood expressed by the adverb. In case they modify an adjective, they derivatively modify a noun. The *attributes* of the object to be denoted by that noun are then subjected to the revaluation set by the adverb. Most adverbs will allow for assertoric and attributive uses, at different circumstances, and a similar thing happens with modalities.

It appears that the notions of essence and accident have been more widely used attributively, at least in recent years. They have been often applied to predications, qualities, and properties. But in formal metaphysics one can also find those notions in their assertoric use. In [11], for instance, Gödel's modal reconstruction of the Ontological Argument is presented with an understanding of 'accidental truth' that is identical to the one that is adopted here. But, despite the relative infrequency of its employment in our times, the assertoric use of essence and accident is also not new. Indeed, in [6], a reasonably influential logic textbook from the XIX Century, John Neville Keynes (the father of John Maynard) already talked freely about essential and accidental propositions, as opposed to essential and accidental predications. Suspending for a while the final judgement about the soundness of the attributive use of such adverbs of essence and accident, this paper has tackled the investigation the technicalities involved in the choice of a modal language obtained simply by adding connectives for essence and for accident to the language of classical logic.

A few more technical objections could still be raised against the above modal renderings of essence and accident. One of them runs as follows. According to the present interpretation of 'essence', a formula is said to have an essentially true status in case it is simply false, and, indeed, in the Proposition 2.1, (K2.4) showed $\circ\bot$ to be a theorem of $(K)_{EA}$. What is that supposed to *mean*? Recall from the modal definition of satisfaction, in section 1, that a statement was defined to be 'accidentally true' in case it *is* true, but could have been false, had the world been different. An antilogical statement obviously cannot be accidentally true, thus it must be essentially so. A similar phenomenon happens in the logics of non-contingency, in which $\vartriangle\bot$ is always provable: a statement that is false in all worlds cannot be contingently true, thus it must be non-contingently so. If, notwithstanding the above explanation, the circumstance of an antilogical statement having an essential (that is, a non-accidental) status still upsets one's modal intuitions, a way of modifying the definition of essence in order to avoid this would be by exchanging the material conditional in the definition of $\circ\varphi$ as $\varphi \supset \Box\varphi$ for some stronger connective conveying the sense of strict implication (defining $\circ\varphi$ as $\varphi \dashv \Box\varphi$ or more simply $\Box(\varphi \supset \Box\varphi)$). A related intuitive objection points to the fact that in the present formalization the notion of essence is still too local: a statement could be essentially true in a world, but fail to be essentially true in another world that can access or be accessed from the former world. Again, one way of fixing that might be by way of the use of some sort of strict implication in the definition of essence, but a more direct solution might be just to make use of some heredity condition on the models, in order to guarantee that statements that are essentially true in a world have the same essential status in all other worlds that belong to its accessibility class. All such alternative formalizations of the notion of essence seem worth exploring.

Finally, for some more positive remarks on the present notion of essence and its possible uses, one might notice for instance that the received modal semantics of intuitionistic logic by way of a translation into the modal logic $S4$ already assumes (through the heredity condition) that all atomic sentences are essentially true in all worlds, so that any eventual truth is preserved into the future (monotonic proofs do not become false as more things get proven). The traditional ontological argument, as proposed by Anselm, discussed by Leibniz, or formalized by Gödel, also involves an appeal to propositions about essence: The sentence positing God's existence would be shown to express a non-accidental truth. Another immediate use for the present notion of essence is in formalizing Saul Kripke's notion of 'rigid designation', and understanding how some truths could be simultaneously necessary and *a posteriori* (cf. [7]): From a physicalist *a priori* true statement according to which "Water is essentially H_2O" (based on the presupposition that any chemical component of water is an essential component of it) and from an empirical verification of the statement that "Water is H_2O" it would arguably follow that "Water is necessarily H_2O" is an *a posteriori* truth. Yet another promising use of the notion of essence is in expressing the consistency of a formula in situations in which negation is non-explosive, allowing for paraconsistent phenomena to appear. With that idea in mind, any non-degenerate normal modal logic could be easily recast as a *logic of formal inconsistency* (cf. [2]), a paraconsistent logic that is rich enough as to be able internalize the very notion of consistency. From that point of view, an inconsistency is interpreted simply as an accident. This idea is explored in detail in another paper (cf. [9]).

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Modality and Paraconsistency

João Marcos*

Paraconsistent logic was born in the vicinity of modal logic. Moreover, as every other non-classical logicians, paraconsistentists have very often flirted with modalities. The first known system of paraconsistent logic was in fact defined as a fragment of $S5$, in the late 40s. But a fragment of a modal system is not necessarily a modal system. I will show here, indeed, that Jaśkowski's **D2** is not a modal logic, in the contemporary usual meaning of the term. By contrast, I will also show, subsequently, that any non-degenerate normal modal system is inherently paraconsistent.

1 What is a paraconsistent logic?

Classical logic is maculated by many irrelevancies. The enterprise of paraconsistency was designed so as to help cleansing a particular stain, by eschewing the so-called Principle of Explosion:

(PE) $\forall\alpha\forall\beta(\alpha, \neg\alpha \Vdash \beta)$.

According to (PE), contradictions are malicious creatures: Whenever they are present in a theory, anything goes, any statement is equally derivable.

In contemporary times, one of the most notorious insurgents against the Aristotelean doctrine that contradictions should be avoided for ontological, logical or psychological reasons was the Polish logician Jan Łukasiewicz (1910). But it was only a few so many years later that one of his disciples, Stanisław Jaśkowski (1948), would really inaugurate the study of non-trivial inconsistent formal systems.

Jaśkowski's proposal was that of a *discussive system*, 'a system which cannot be said to include theses that express opinions in agreement with one another'. To obtain such a system every statement was to be preceded

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by the reservation 'in accordance with the opinion of one of the participants in the discussion', or 'for a certain admissible meaning of the terms used'. These ideas were initially implemented with the help of the modal logic $S5$ into a sort of 'pre-discussive' system J, which was such that

$$\Gamma \Vdash_J \alpha \;\text{ iff }\; \Diamond\Gamma \vDash_{S5} \Diamond\alpha.$$

Obviously, J defines a paraconsistent logic. A very weak one, however. As it is easy to see, the consequence relation of J is essentially single-premised, as $\Gamma \Vdash_J \alpha$ iff $\gamma \Vdash_J \alpha$, for some $\gamma \in \Gamma$. There are in J no typically multiple-premised rules, thus, such as *modus ponens*. To fix that weakness, Jaśkowski was to propose a sort of preprocessing of the usual classical connectives, by recursively setting:

1. $p^* = p$, for every atomic variable p;
2. $(\neg\alpha)^* = \neg\alpha^*$;
3. $(\alpha \vee \beta)^* = \alpha^* \vee \beta^*$;
4. $(\alpha \wedge \beta)^* = \alpha^* \wedge \Diamond\beta^*$;
5. $(\alpha \supset \beta)^* = \Diamond\alpha^* \supset \beta^*$.

While clause 5, defining a 'discussive implication', belongs to Jaśkowski (1948), clause 4, defining a 'discussive conjunction', belongs to Jaśkowski (1949). The main 'discussive' logic **D2** was then put forward by setting

$$\Gamma \Vdash_{\mathbf{D2}} \alpha \;\text{ iff }\; \Gamma^* \Vdash_J \alpha^*.$$

It is straightforward to check that **D2** is a paraconsistent extension of the positive fragment of classical logic (that is, the logical constants $\vee$, $\wedge$ and $\supset$ in **D2** behave just like their classical homonyms). Notice that without clause 4 this observation about the positive fragment of classical logic would not be fully true, for the resulting logic would fail negation introduction, that is, it would fail $\alpha, \beta \Vdash (\alpha \wedge \beta)$, as it happens with J. There are indeed a few systems of paraconsistent logic that have this 'non-adjunctive' character. Any defense about this having been a feature desired and cherished by Jaśkowski seems to depend however on not having read his 1949 two pages paper (and that disgracefully applies to most discussivists from the literature).

The 'asymmetric' looks of clauses 4 and 5 have been criticized here and there. Based on the facts that the formulas $\Diamond(\Diamond\alpha \supset \beta)$ and $\Diamond(\Diamond\alpha \supset \Diamond\beta)$ are equivalent inside the modal logic $S4$ (a fragment of $S5$) and that the formulas $\Diamond(\alpha \wedge \Diamond\beta)$, $\Diamond(\Diamond\alpha \wedge \beta)$ and $\Diamond(\Diamond\alpha \wedge \Diamond\beta)$ are all equivalent inside $S5$, the following alternatives to the above clauses have been proposed:

4.1 $(\alpha \wedge \beta)^* = \Diamond\alpha^* \wedge \beta^*$;

4.2 $(\alpha \wedge \beta)^* = \Diamond\alpha^* \wedge \Diamond\beta^*$;

5.1 $(\alpha \supset \beta)^* = \Diamond\alpha^* \supset \Diamond\beta^*$.

Now, while it is true that any choice of translation would have the same effect for the positive fragment of **D2** (it would still coincide with the positive fragment of classical logic), the same is not true for the full logic, when the interaction of negation with the other connectives is considered. It is not true thus that different translation clauses 'would have just the same consequences', as claimed in Priest (2002, section 5.2). Different choices of discussive conjunction and discussive implication would in fact define logics distinct from **D2**. This phenomenon will be carefully illustrated in Section 3 of the present note.

Other usual classical connectives can be easily defined in **D2**, such as bi-implication: $(\alpha \equiv \beta) \stackrel{\text{def}}{=} (\alpha \supset \beta) \wedge (\beta \supset \alpha)$. Moreover, a classical negation $\sim$ can be defined in **D2** by setting $\sim\alpha \stackrel{\text{def}}{=} \alpha \supset \neg(\alpha \vee \neg\alpha)$ (hint: check that $\Diamond p$ and $\Diamond{\sim}p$ cannot be both true and cannot be both false in a given world of a model of $S5$). The logic **D2** can also define a consistency connective $\circ\alpha \stackrel{\text{def}}{=} (\sim\alpha) \vee (\sim\neg\alpha)$, in the sense of Carnielli and Marcos (2002), that is, a logical constant that says when explosion can be recovered, through the following Gentle Principle of Explosion:

(GPE) $\exists\alpha\exists\beta(\circ\alpha, \alpha \not\Vdash \beta$ and $\circ\alpha, \neg\alpha \not\Vdash \beta)$, while $\forall\alpha\forall\beta(\circ\alpha, \alpha, \neg\alpha \Vdash \beta)$.

The fact that **D2** enjoys (GPE) makes it qualify as an **LFI**, a Logic of Formal Inconsistency (more specifically, in this case, a **dC**-system based on classical logic). Consistent reasoning can often be recaptured from inside inconsistent logics, and the ability of doing just that is in fact a fundamental feature of **LFI**s. More precisely, if $\vDash_{\mathbf{CPL}}$ is the consequence relation of Classical Propositional Logic, the following *Derivability Adjustment Theorem* can be proved:

(DAT) $(\Gamma \vDash_{\mathbf{CPL}} \beta)$ iff $\exists\Sigma(\circ\Sigma, \Gamma \Vdash_{\mathbf{D2}} \beta)$.

The above result says that, even though **D2** fails the 'consistency presupposition' that is typical of classical logic, any classical inference can be recovered if a sufficient number of 'consistency assumptions' are added to the set of premises. We will see several examples of derivability adjustments in the next sections. Clearly, yet another way of recovering **CPL** from inside **D2** is by taking the new classical negation of **D2** into account. If $(\alpha)^{\neg,\sim}$ denotes a translation that changes any occurrence of the paraconsistent $\neg$ by its classical counterpart $\sim$, leaving the rest of the formula as it is, it is easy to check that $(\Gamma \vDash_{\mathbf{CPL}} \beta)$ iff $(\Gamma^{\neg,\sim} \Vdash_{\mathbf{D2}} \beta^{\neg,\sim})$. This direct translation is an alternative to the addition of further premises promoted by the derivability adjustments of the set $\circ\Sigma$, in the above (DAT).

It should be remarked that semantical features of a given logic are usually not inherited by its proper fragments. Thus, while classical logic is two-valued, other many-valued logics can only be given a two-valued semantics at the cost of their truth-functionality, and intuitionistic logic is not even a finitely-valued logic. Typically, in fact, non-classical fragments of **CPL** will have connectives that are not classically expressible —such as an intuitionistic or a paraconsistent negation. The realization that many-valued logics can all be embedded into certain modal logics, as in Demri (2000), do not make them any more 'modal' than they were before, and, as we will see in Section 3, the fact that **D2** is introduced through an embedment into the modal logic $S5$ does NOT make this system a 'modal logic', in the contemporary usual meaning of the term.

2 What is a modal logic?

Unfortunately, there is no generally agreed definition of the term 'modal logic'. Fortunately, however, this situation has not hindered the enormous advance of the studies in that area. Among the most solid achievements of those studies one should certainly count the modern development of Kripke-like semantics. There is nowadays a plethora of modal systems available. What do they have in common, if anything? I will assume here that the most fundamental feature of modal logics, common to both the usual models of normal modal logics and the minimal models of non-normal modal logics (see Chellas, 1980, chap. 7) consists in the validity of the so-called 'replacement property'. The validity of such a logical property coincides in fact with the abstract property that Wójcicki (1988, chap. 5) calls 'self-extensionality' and shows to be the characterizing feature of the logics that have 'an adequate frame semantics'. I will briefly explain in this section what this property means.

Let $\alpha \dashv\Vdash \beta$ abbreviate the combination of $\alpha \Vdash \beta$ with $\beta \Vdash \alpha$ —this is to say that α and β are *equivalent* formulas. In any logic with a classic-like bi-implication $\equiv$, as all the logics we will be mentioning in the present study, asserting $\alpha \dashv\Vdash \beta$ is the same as asserting $\Vdash \alpha \equiv \beta$. Let $\varphi(p)$ denote a formula in which the variable p occurs, and $\varphi(p/\delta)$ denote the formula obtained from φ by uniformly substituting all occurrences of p by the formula δ. Given a pair of formulas α and β, say that they are *indiscernible* if, for every formula $\varphi(p)$, one has that $\varphi(p/\alpha) \dashv\Vdash \varphi(p/\beta)$. In particular, indiscernible formulas are equivalent (to see that, take $\varphi(p)$ as p itself). An explicit definition, such as those we have been writing since the last section with the help of the extra-logical symbol '$\stackrel{\text{def}}{=}$' simply postulates that the formula at the left-hand side of that symbol should be treated as indiscernible from the formula at the right-hand side of that same symbol. Now, a logic enjoying the *replacement property* is simply a logic for which every pair of equivalent formulas is indiscernible, that is, a logic in which $\alpha \dashv\Vdash \beta$ implies $\varphi(p/\alpha) \dashv\Vdash \varphi(p/\beta)$, for any formula φ. It should be clear that this property allows us to replace any occurrence of a subformula by an equivalent expression, while derivability is preserved.

Modal logics, just as classical logic, enjoy the replacement property, and so they are such that $\alpha_1 \dashv\Vdash \alpha_2$ and $\beta_1 \dashv\Vdash \beta_2$ provide sufficient conditions for $(\alpha_1 \wedge \beta_1) \dashv\Vdash (\alpha_2 \wedge \beta_2)$ or $\Diamond\alpha_1 \dashv\Vdash \Diamond\alpha_2$. As it can be seen in Theorems 44, 78 and 124 of Carnielli, Coniglio, and Marcos (2005), there are many paraconsistent logics that FAIL the replacement property.

3 D2 is not a modal logic

The discussive logic **D2** has a very long and dramatic story (see Ciuciura, 1999). And it is not over yet. Besides non-adjunctiveness, another common obsession of discussivists concerns the alleged 'modal character' of **D2**. This section will exhibit a few properties of the logic **D2** and of some of its close relatives, and then show that none of these logics, nor their fragments, nor their paraconsistent extensions (if they exist), can enjoy the replacement property.

Inside a proper fragment of classical logic, the classical connectives can certainly not all be interdefined as usual. So, in **D2** the paraconsistent negation cannot interact with the other connectives such as classical negation does. Consider the following inferences:

(ID1) $(\neg\alpha \supset \beta) \Vdash (\alpha \vee \beta)$
(ID2) $(\alpha \vee \beta) \Vdash (\neg\alpha \supset \beta)$
(ID3) $\neg(\neg\alpha \supset \beta) \Vdash \neg(\alpha \vee \beta)$
(ID4) $\neg(\alpha \vee \beta) \Vdash \neg(\neg\alpha \supset \beta)$
(ID5) $(\alpha \supset \beta) \Vdash \neg(\alpha \wedge \neg\beta)$
(ID6) $\neg(\alpha \wedge \neg\beta) \Vdash (\alpha \supset \beta)$
(ID7) $\neg(\alpha \supset \beta) \Vdash (\alpha \wedge \neg\beta)$
(ID8) $(\alpha \wedge \neg\beta) \Vdash \neg(\alpha \supset \beta)$
(ID9) $\neg(\neg\alpha \wedge \neg\beta) \Vdash (\alpha \vee \beta)$
(ID10) $(\alpha \vee \beta) \Vdash \neg(\neg\alpha \wedge \neg\beta)$
(ID11) $\neg(\neg\alpha \vee \neg\beta) \Vdash (\alpha \wedge \beta)$
(ID12) $(\alpha \wedge \beta) \Vdash \neg(\neg\alpha \vee \neg\beta)$

It is easy to use the semantics of $S5$, based on reflexive, symmetric and transitive frames, to check that (ID1), (ID4), (ID9) and (ID11) are a consequence of reflexivity, while (ID7) and (ID8) are a consequence of symmetry and transitivity. Now, to prove the remaining inferences in **D2**, some derivability adjustments are in order (recall Section 1): To recover (ID2) and (ID6) one needs to add $\circ\alpha$ to the set of premises; to recover (ID5) and (ID10) one needs to add $\circ\beta$; in the case of (ID3) and (ID12), adding either $\circ\alpha$ or $\circ\beta$ will do.

One can now also readily show the difference between the various possible clauses for a preprocessing translation, as proposed in Section 1. Combining the three versions of the translation clause 4 and the two versions of the translation clause 5 there will be at most 5 distinct alternatives to the logic **D2**. And, in fact, there are. While all these logics agree in validating (ID1), (ID7), (ID9) and (ID11), none of them validates (ID2), (ID10) and (ID12). The logics based on the original clause 5 validate (ID4) but not (ID3), the other logics do exactly the contrary; the logics based on clause 5 also validate (ID8), while the others do not. The logics based on the original clause 4 fail (ID5) and (ID6), all the remaining logics validate (ID6). Finally, (ID5) is validated exactly by those logics that substitute clause 4.1 for clause 4. These 6 possible 'discussive logics' are thus all different, and each of them allows for its own derivability adjustments, in each case (exercise).

We will now check that all the 6 logics above fail the replacement property. Notice first that the definitions of the bi-implication, the classical negation and the consistency connective used in Section 1 work the same for any of the above logics. In particular, for the classical negation $\sim$, defined by setting $\sim\alpha \stackrel{\text{def}}{=} \alpha \supset \neg(\alpha \vee \neg\alpha)$, the theory $\{\alpha, \sim\alpha\}$ is explosive, and the formulas α and $\sim\sim\alpha$ are logically indiscernible. Now, by (ID7), an inference validated by all the above logics, we have that $\neg\sim\gamma \Vdash \gamma \wedge \neg\neg(\gamma \vee \neg\gamma)$. By conjunction elimination, a rule valid in the positive fragment of classical logic, $\neg\sim\gamma \Vdash \gamma$. But a classical negation is explosive, thus $\forall\gamma\forall\beta(\gamma, \sim\gamma \Vdash \beta)$. In that case we also have, by transitivity of deduction (the cut rule), that $\neg\sim\gamma, \sim\gamma \Vdash \beta$, for arbitrary formulas γ and β. In particular, we have $\neg\sim\sim\alpha, \sim\sim\alpha \Vdash \beta$, taking γ as $\sim\alpha$. Again, considering the properties of classical negation we have that $\alpha \dashVdash \sim\sim\alpha$. To proceed by absurdity, if the replacement property did hold good for any of the above logics one could then conclude that $\neg\alpha \dashVdash \neg\sim\sim\alpha$. From this and the cut rule one would finally derive $\neg\alpha, \alpha \Vdash \beta$, and the logic would not be paraconsistent, as we know it is.

4 Modal logics are paraconsistent

Can paraconsistent logics enjoy the replacement property at all? And can they have appropriate 'natural' modal semantics? How natural? The answer to those disquietudes is doubly positive, as we will see in this section. First: Yes, there are paraconsistent logics enjoying full replacement. Second: Yes, one does not need to adventure into strange new territories to find them. We had a modal paraconsistent negation around all the time, when we were dealing with usual normal modal logics —and there is an infinite number of the latter.

Béziau (2002, 2005) has been calling attention to that, recently: Just as much as intuitionistic negation has its standard modal interpretation in terms of a certain translation into $S4$ that interprets this negation by $\sim\Diamond$, a dual paraconsistent negation is obtained if one interprets it by using $\Diamond{\sim}$. The idea in reality is anything but new, and it has been deeply studied in between the mid-70s and the 80s (check specially Došen 1986 and Vakarelov 1989). This section will sketch the big picture —for many more details, an emphasis on duality, and proofs of all claims, check Marcos (2004).

Normal modal logics are extensions of the logic K: In their usual language, they admit the necessitation rule and propagate necessity through conjunctions. They also enjoy the replacement property, by their very design. The most obvious degenerate examples of normal modal logics are characterized by frames that are such that every world can access only itself or no other world. Now, it is not difficult to verify that, for any non-degenerate normal modal logic, a connective defined by setting $\neg\alpha \stackrel{\text{def}}{=} \Diamond{\sim}\alpha$ is a (sub-classical) paraconsistent negation, that is: (a) It only has positive properties that are also enjoyed by classical negation; (b) it has enough negative properties so that it qualifies as a 'minimally decent negation', in the sense of Marcos (2005b); (c) it is not explosive. Moreover, any such logic is in fact a Logic of Formal Inconsistency, and a **dC**-system (recall Section 1) where a consistency connective can be defined, for instance, by setting $\circ\alpha \stackrel{\text{def}}{=} \alpha \supset {\sim}\neg\alpha$.

Furthermore, not only is it possible to start from a (non-degenerate) normal modal logic and define operators that represent a paraconsistent negation and a consistency connective, it is also possible to do it the other way around. Indeed, consider as before the language of positive classical logic to be written over the connectives $\wedge$, $\vee$ and $\supset$, whose interpretation is the standard one over a Kripke-like modal structure, and add to that a negation $\neg$ to be interpreted by assuming, for worlds x and y of a model $\mathcal{M}$ with an accessibility relation R:

$$\models_x^{\mathcal{M}} \neg\alpha \ \text{ iff } \ (\exists y)(x\mathsf{R}y \ \text{ and } \ \not\models_y^{\mathcal{M}} \alpha).$$

In that case, a classical negation could be recovered simply by defining ${\sim}\alpha \stackrel{\text{def}}{=} \alpha \supset \neg(\alpha \supset \alpha)$. The other usual modal connectives would then be obtained by setting $\Diamond\alpha \stackrel{\text{def}}{=} \neg{\sim}\alpha$ and $\Box\alpha \stackrel{\text{def}}{=} {\sim}\neg\alpha$. Alternatively, the consistency connective could also be taken as primitive, by assuming:

$$\models_x^{\mathcal{M}} \circ\alpha \ \text{ iff } \ \models_x^{\mathcal{M}} \alpha \text{ implies } (\forall y)(\text{if } \ x\mathsf{R}y \ \text{ then } \ \models_y^{\mathcal{M}} \alpha).$$

In that case, a classical negation could alternatively be defined by setting ${\sim}\alpha \stackrel{\text{def}}{=} \alpha \to (\alpha \wedge (\neg\alpha \wedge \circ\alpha))$. The significance of this 'consistency connective' in a modal language deprived of a paraconsistent negation was put into proof in Marcos (2005a), where an interpretation was proposed for it as a connective expressing the notion of an 'essential truth' —as opposed to a merely 'accidental' one.

On what concerns some of the usual inferences (recall (ID1)–(ID12), from Section 3) that interrelate the distinct connectives of the positive classical logic by way of the new modal paraconsistent negation presented heretofore, it should be noted that none of them is validated if one considers the semantics of the minimal normal modal logic K. However, both (ID1) and (ID4) are validated if one considers the reflexivity condition that characterizes the logic KT, while both (ID9) and (ID11) are validated given the symmetry condition that characterizes KB. None of the other inferences is valid inside a non-degenerate modal logic.[1] To validate any of the latter, some derivability adjustments (recall Section 1) are needed, with the help of the above defined consistency connective. Indeed, it might be noticed that the logic K can recover (ID2) and (ID8) by the addition of $\circ\alpha$ as a new 'consistency assumption', and it can recover (ID7) by the addition of $\circ{\sim}\alpha$ as such an assumption. Moreover, the logic KT can recover (ID5) by the addition of $\circ\beta$, and it can recover both (ID10) and (ID12) by the addition of both $\circ\alpha$ and $\circ\beta$. Finally, (ID6) can be recovered in KB by the addition of KB, and (ID3) can be recovered in $S4$ by the addition of both $\circ\alpha$ and $\circ\beta$.

There are of course other studies in which paraconsistent negations are endowed with modal interpretations, such as those involving the so-called 'Routley star', in the context of relevance logics, where a ternary accessibility relation is used in giving truth conditions to some connectives (see Dunn, 1993). In comparison with those, the above straightforward interpretation of paraconsistent negation inside normal modal logics gains in simplicity what it loses in generality. In the present approach, at any rate, it has been shown that one could either start from the usual language of normal modal logics and define the paraconsistent-related connectives, or else start from the latter and then reintroduce the usual modal connectives. From that perspective, it should be clear to the reader that modal logics could alternatively be regarded as the study of certain modal-like inconsistency-tolerant systems. Instead of qualifying the truth of judgements in terms of belief or tense or duty or whatever other received adverbial expression, modal logic would have its role thus in the study of a more general 'theory of opposition', for the sake of those who believe that Aristotle is possibly not dead.

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[1] A mistake has thus remained in Béziau (2005), where (ID10) is said to be validated in $S5$.

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Nearly every normal modal logic is paranormal

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> The principal interest is philosophical: not to confine oneself to what is necessary for (current) practice, but to see what is possible by way of theoretical analysis.
> —Kreisel (1970).

An *overcomplete* logic is a logic that 'ceases to make the difference': According to such a logic, all inferences hold independently of the nature of the statements involved. A *negation-inconsistent* logic is a logic having at least one model that satisfies both some statement and its negation. A *negation-incomplete* logic has at least one model according to which neither some statement nor its negation are satisfied. *Paraconsistent* logics are negation-inconsistent yet non-overcomplete; *paracomplete* logics are negation-incomplete yet non-overcomplete. A *paranormal* logic is simply a logic that is both paraconsistent and paracomplete.

Despite being perfectly consistent and complete with respect to classical negation, nearly every normal modal logic, in its ordinary language and interpretation, admits to some latent paranormality: It is paracomplete with respect to a negation defined as an impossibility operator, and paraconsistent with respect to a negation defined as non-necessity. In fact, as it will be shown here, even in languages without a primitive classical negation, normal modal logics can often be alternatively characterized directly by way of their paranormal negations and related operators. So, instead of talking about 'necessity', 'possibility', and so on, modal logics could be seen just as devices tailored for the study of (modal) negation. This paper shows how and to what extent this alternative characterization of modal logics can be realized.

1 Affirmative and negative modalities

In the course of the last hundred years or so, traditional modal logic was extraordinarily reinvigorated, at the outset with the firsthand assistance of symbolic logic, then by the successful development of both its algebraic and relational semantics. Of all adverbs which have been formalized with the help of modal languages, the most popular turned out to be a certain '$\Box$-like' modality with a universal character and its '$\Diamond$-like' existential dual, irrespective of their circumstantial readings —alethic, deontic, doxastic, temporal, etc— on each particular application field. The gate to possible worlds (and to some bad science fiction) was opened by the tacit assumption that the usual classical connectives should be interpreted locally, while $\Box$ and $\Diamond$ were supposed to have a global scope.

To be perfectly fair, not all modal semantics conform to the above pattern. The traditional modal interpretation of intuitionistic and intermediate logics, for example, as well as the ternary relations of relevance logics, end up with a global interpretation of both the implication and the negation connectives, all other connectives being interpreted classically and locally. Other modal logics go farther, and are themselves built over non-empty sets of non-classical worlds, be they many-valued, incomplete or even inconsistent. On the other hand, several other linguistic modal bases have also been tried at a few occasions. To mention just a particularly meaningful one, I recall the contingency / non-contingency logics explored by several authors since Montgomery and Routley (1966), trading $\Box$ and $\Diamond$ for the non-normal modal connectives $\triangledown$ and $\vartriangle$, with which the former are interdefinable only in the case of sufficiently convoluted classes of frames.

Traditional literature on modal logic such as Hughes and Cresswell (1968) has it that a 'modality' is just an arbitrary finite sequence of $\Box$'s, $\Diamond$'s and $\sim$'s, where $\sim$ is a symbol for classical negation. Aristotle had a picture of a 'Square of Oppositions' (SOO) involving negation and quantification. An analogous picture (see Figure 1) for the basic case involving modalities can be found in Łukasiewicz (1953) —and probably even earlier.

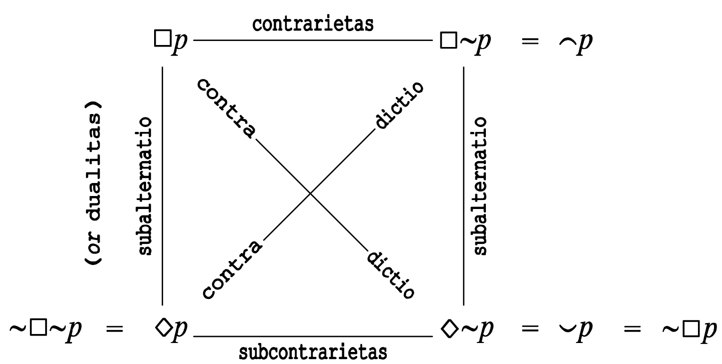

Figure 1: *Square of Modalities* (SOM)

The four modal corners from the above SOM were not really treated on an equal footing in the recent literature of modal logic. To be sure, that circumstance alone should not count against any of the modalities thereby contained, as no one still nowadays knows even what modal logic *is*, in general abstract terms. In a brilliant book originated from a frustrated attempt at such a definition, Segerberg (1982), p.128, the following comment can be found:

> Among the many possible operators that have never been proposed by anyone, there is one that should be mentioned here, the unary $\smile$, with $\smile\alpha$ bearing the intuitive reading 'it is not necessary that α' or 'α is non-necessary'. The concept of non-necessity does not appear to equal in intuitive significance that of impossibility, let alone those of necessity or possibility. But from a theoretical point of view, $\smile$ is on a par with $\frown$ as well as with $\Box$ and $\Diamond$. [the symbols for $\frown$ and $\smile$ are mine]

On that matter, according to Horn (1989), linguistic researches attest that, at least for pragmatic reasons, the bottom-right corners of both the SOO and the SOM seem not to have exact natural language equivalents in any of the world's living natural languages (but it should be noticed that this is no longer true if one considers artificially constructed languages such as *Lojban*, check Cowan (1997)). The noted asymmetry does not seem to have a convincing semantic explanation, and one can indeed find authors like Béziau in a series of papers culminating recently at Béziau (2004), preaching the study of the 'nameless corner of the square of oppositions and modalities' as an utterly intuitive enterprise. On what concerns the upper-right corner of the SOM, one should note that, alongside the classical connectives and a binary modality of strict implication, impossibility ($\frown$) was in fact the *only* primitive unary modality appearing in the cornerstone study that marked the contemporary revival of modal logic, the book of Lewis (1918).

In the philosophical literature (and only there!), modal logics are still often seen simply as the study of operators 'used to qualify the truth of a judgement' (check, for instance, Garson (2003)). Of course, such truth-qualifying operators can analogously be used to qualify falsehood, and if the left-hand side of the SOM can be seen as displaying operators that qualify *affirmation*, the right-hand side can similarly purport to display operators that qualify *negation*. But does that interpretation really make sense? Can $\frown$ and $\smile$ be seriously proposed as proxies for a negation operator? The answer is very often YES, but to understand that 'very often' it is useful to fix first some terminology.

1.1 Basic modal semantics

Consider the standard *language* (or *signature*) of classical propositional logic, with binary connectives for conjunction ($\wedge$), disjunction ($\vee$), implication ($\supset$), and a unary connective for negation ($\sim$). Let $\mathcal{S}_{\mathbf{CPL}}$ or $\mathcal{S}_{\wedge\vee\supset\sim}$ denote the set of *formulas* freely generated by a denumerable set of sentential variables, $\mathcal{P}$, over the above signature (the subscripts will be dropped when clear from the context). A *frame* here will be given by a non-empty set of *worlds*, $\mathcal{W}$, and a *model* over a given frame will be obtained by coupling it with a *(bi)valuation* $V : \mathcal{P} \times \mathcal{W} \to \{0, 1\}$. Valuations can be used to define a canonical notion of *satisfiability*, $\models_x^{\mathcal{M}} \subseteq \mathsf{Pow}(\mathcal{S}) \times \mathsf{Pow}(\mathcal{S})$, for each world x of a model $\mathcal{M}$, with the help of the following clauses that tell us how each connective should be understood:

$$
\begin{array}{lll}
\models_x^{\mathcal{M}} p & \text{iff} & V(p, x) = 1, \text{ for } p \in \mathcal{P} \\
\models_x^{\mathcal{M}} \alpha \wedge \beta & \text{iff} & \models_x^{\mathcal{M}} \alpha \text{ and } \models_x^{\mathcal{M}} \beta \\
\models_x^{\mathcal{M}} \alpha \vee \beta & \text{iff} & \models_x^{\mathcal{M}} \alpha \text{ or } \models_x^{\mathcal{M}} \beta \\
\models_x^{\mathcal{M}} \alpha \supset \beta & \text{iff} & \models_x^{\mathcal{M}} \alpha \text{ implies } \models_x^{\mathcal{M}} \beta \\
\models_x^{\mathcal{M}} {\sim}\alpha & \text{iff} & \not\models_x^{\mathcal{M}} \alpha
\end{array}
$$

To write $\not\models_x^{\mathcal{M}} \alpha$ is to say that $\models_x^{\mathcal{M}} \alpha$ does *not* hold. I will also denote that, alternatively, by writing $\alpha \models_x^{\mathcal{M}}$. In general, for a given world x of a model $\mathcal{M}$ of a given frame, I will assume that:

$$\Gamma \models_x^{\mathcal{M}} \Delta \text{ iff } (\exists \gamma \in \Gamma)\, \gamma \models_x^{\mathcal{M}} \text{ or } (\exists \delta \in \Delta) \models_x^{\mathcal{M}} \delta$$

The notion of a *valid inference* and the corresponding entailment (semantic global consequence relation) $\models_{\mathbf{CPL}} \subseteq \mathsf{Pow}(\mathcal{S}) \times \mathsf{Pow}(\mathcal{S})$ associated to classical propositional logic is fixed by setting $\Gamma \models_{\mathbf{CPL}} \Delta$ iff $\Gamma \models_x^{\mathcal{M}} \Delta$ for every world x of every model $\mathcal{M}$ of an arbitrary frame. Of course, in the case of **CPL**, the recourse to a set of worlds $\mathcal{W}$ does not help that much, as all the connectives of this logic are evaluated locally, that is, evaluated inside each (classical) world.

The expressive power of **CPL** is well-known: The logic has an adequate 2-valued functional semantics, and in fact every 2-valued n-ary truth-function can be written with the help of the above connectives. Some other particular connectives that are often used in the literature and that will be mentioned in the text below include the 0-ary connectives top ($\top$) and bottom ($\bot$), and the binary connectives for equivalence ($\equiv$) and coimplication ($\not\subset$, the 'dual' to implication in a precise sense to be specified in Subsection 2.1). Here is the intended interpretation of these connectives, together with some possible ways of defining them in terms of the connectives taken above as primitive or defined earlier on:

Definitions			*Characterizing properties*
$\alpha \not\subset \beta$	$\stackrel{\text{def}}{=}$	${\sim}\alpha \wedge \beta$	$\models_x^{\mathcal{M}} \alpha \not\subset \beta$ iff $\beta \supset \alpha \models_x^{\mathcal{M}}$
$\alpha \equiv \beta$	$\stackrel{\text{def}}{=}$	$(\alpha \supset \beta) \wedge {\sim}(\alpha \not\subset \beta)$	$\models_x^{\mathcal{M}} \alpha \equiv \beta$ iff $\models_x^{\mathcal{M}} \alpha \supset \beta$ and $\models_x^{\mathcal{M}} \beta \supset \alpha$
$\top$	$\stackrel{\text{def}}{=}$	$\alpha \supset \alpha$, for any α	$\models_x^{\mathcal{M}} \top$
$\bot$	$\stackrel{\text{def}}{=}$	$\alpha \not\subset \alpha$, for any α	$\bot \models_x^{\mathcal{M}}$

In the case of ordinary normal modal logics, I will consider again a frame based on non-empty set of classical worlds but now I will enrich it with an *accessibility* relation $\mathsf{R} \subseteq \mathcal{W} \times \mathcal{W}$ between the worlds, and read $x\mathsf{R}y$ as 'x sees y' or 'y is accessible to x'. A model based on such a frame, as before, will be assembled from a given valuation over the sentences and worlds, and a corresponding inductive definition of the interpretation for the whole set of formulas. This time the signature will contain two further unary connectives, box ($\Box$, often read as 'necessity') and diamond ($\Diamond$, often read as 'possibility'), and be denoted by $\mathcal{S}_{\mathbf{NML}}$ or $\mathcal{S}_{\wedge\vee\supset\sim\Box\Diamond}$. The interpretation of the new connectives is given by the following clauses (where $\Rightarrow$ substitutes 'implies' and & is used for 'and'):

$$
\begin{array}{ll}
\models^{\mathcal{M}}_{x} \Box\alpha & \text{iff } \ (\forall y \in \mathcal{W})(x\mathsf{R}y \Rightarrow \models^{\mathcal{M}}_{y} \alpha) \\
\models^{\mathcal{M}}_{x} \Diamond\alpha & \text{iff } \ (\exists y \in \mathcal{W})(x\mathsf{R}y \ \& \models^{\mathcal{M}}_{y} \alpha)
\end{array}
$$

All other definitions are similar to the classical case. Several different modal logics (that is, several different relations of global entailment) can be defined in the above signature, according to the restrictions set over the accessibility relations in each case. In fact, when talking about a logic, from here on, I will always make sure that its set of formulas and an associated consequence relation are clearly defined, be it in proof-theoretical, in semantical or in abstract terms. The minimal normal modal logic, K, where NO restrictions are made over R, can be axiomatized by adding to any complete set of axioms and rules for **CPL** any of the three following sets of further axioms and rules:

(1.1) $\vdash \Box(\alpha \supset \beta) \supset (\Box\alpha \supset \Box\beta)$
(1.2) $\vdash \alpha \ \Rightarrow \ \vdash \Box\alpha$

(2.1) $\vdash \alpha_0 \wedge \ldots \wedge \alpha_n \supset \alpha \ \Rightarrow \ \vdash \Box\alpha_0 \wedge \ldots \wedge \Box\alpha_n \supset \Box\alpha$,
where this rule reduces to (1.2) in case $n = 0$

(3.1) $\vdash \Box\top$
(3.2) $\vdash \Box\alpha \wedge \Box\beta \supset \Box(\alpha \wedge \beta)$
(3.3) $\vdash \alpha \supset \beta \ \Rightarrow \ \vdash \Box\alpha \supset \Box\beta$

(The axioms for $\Diamond$ are dual. For the purposes of this section, $\Diamond\alpha$ may be defined as $\sim\Box\sim\alpha$.)

The explicit definability of all 'admissible modal operators' from the basic modal language was investigated, for instance, in Wansing (1996), with respect to their associated 'proof-theoretic semantics'. Among the many new connectives that can now be defined in every **NML**, one could pinpoint contingency ($\triangledown$) and non-contingency ($\vartriangle$), definable for instance by setting $\triangledown\alpha \stackrel{\text{def}}{=} \Diamond\alpha \vee \Diamond{\sim}\alpha$ and $\vartriangle\alpha \stackrel{\text{def}}{=} \Diamond\alpha \supset \Box\alpha$, besides, of course, the two new modalities at the right-hand side of the SOM, $\frown$ and $\smile$, definable as in Figure 1.

1.2 Modal negations?

Some particular restrictions on the accessibility relation R will produce *degenerate* examples of modal logics. Call a world *autistic* in case there is no world accessible to it according to R, and call it *narcissistic* in case it can only see itself. The collection of all autistic frames (that is, frames whose worlds are all autistic) determines a logic called *Ver*, and can be axiomatized by the addition of the axiom $\vdash \Box\alpha$ to the axioms and rules of K. The collection of all narcissistic frames (that is, frames whose worlds are all narcissistic) determines a logic known as *Triv*, or *KT!* as in Chellas (1980), and can be axiomatized by the addition to the axioms and rules of K of the axiom $\vdash \Box\alpha \equiv \alpha$. It is easy to see that both *Ver* and *Triv* are but thin disguises for classical propositional logic: In the first, $\Box$ and $\Diamond$ are unary operators that produce tops, in the second, $\Box$ and $\Diamond$ behave like identity operators. The logic that I will call TV and that is situated exactly midway in between *Triv* and *Ver* is also important in the present story. It is determined by the class of all frames that are either narcissistic or autistic, and axiomatized by the addition to K of the axiom $\vdash \alpha \supset \Box\alpha$.

In what follows it will be helpful to use $\circledcirc^n$ as an abbreviation for n iterations of a given unary connective $\circledcirc$. I will be saying that a logic $\mathcal{L}_2$ is a (deductive) *fragment* of a logic $\mathcal{L}_1$ (and $\mathcal{L}_1$ is an *extension* of $\mathcal{L}_2$) if $\mathcal{L}_1$ can be written in a signature containing all the symbols from the signature of $\mathcal{L}_2$ and if, in that case, all valid inferences of $\mathcal{L}_2$ are also valid in $\mathcal{L}_1$.

Makinson (1971) proved that every normal modal logic is a fragment of either *Ver* or *Triv* (and possibly of both, that is, of TV). For instance, the modal logic KT, determined by the class of reflexive frames and axiomatized by the addition to K of the axiom $\vdash \Box\alpha \supset \alpha$, is only a fragment of *Triv* but not of *Ver*; on the other hand, the logic of provability GL, determined by the class of transitive and reversely well-founded frames and axiomatized by the addition to K of the axiom $\vdash \Box(\Box\alpha \supset \alpha) \supset \Box\alpha$, is only a fragment of *Ver* but not of *Triv*; finally, $K5$, determined by the class of euclidean frames and axiomatized by the addition to K of the axiom $\vdash \Diamond\alpha \supset \Box\Diamond\alpha$, is a fragment of TV. More importantly, every extension of K obtained by the sole addition of axioms of the form $\vdash \Diamond^i\Box^j\alpha \supset \Box^k\Diamond^l\alpha$, for $i, j, k, l \in \mathbb{N}$, complete with respect to a convenient combination of the so-called confluential (Church-Rosser) frames, *is* a fragment of *Triv* —and in fact, very few of the most widely known modal logics fail to be a fragment of *Triv*.

Can $\frown$ and $\smile$ be understood as 'negations' inside all of the above logics? For one thing, inside of *Ver* it seems already difficult to accept that reading: All formulas of the form $\frown\alpha$ and $\smile\alpha$ would be theorems of this logic... But what connectives are to count as 'negations', to start with? First of all, it must be cleared up that there is NO general —nor even partial— agreement in the literature on an answer to that. As we will see, this is not to say, however, that the very concept of negation is unruly!

Consider from this point on a (non-overcomplete)[1] logic $\mathcal{L}_1$ endowed with some symbol $\neg$ intended to denote 'negation'. Even if we consider no other circumstantial symbols from $\mathcal{L}_1$'s signature and its corresponding set of formulas $\mathcal{S}_1$, there is a number of *pure* positive meta-rules that might be considered to govern the behavior of negation with respect to $\Vdash_1$, the consequence relation associated to $\mathcal{L}_1$. For instance, the following two rules can fully characterize classical negation inside a non-overcomplete logic:

(Explosion)	$(\forall\Gamma,\Delta \subseteq \mathcal{S}_1)(\forall\alpha \in \mathcal{S}_1)\ \Gamma,\alpha,\neg\alpha \Vdash_1 \Delta$
(Implosion)	$(\forall\Gamma,\Delta \subseteq \mathcal{S}_1)(\forall\alpha \in \mathcal{S}_1)\ \Gamma \Vdash_1 \alpha,\neg\alpha,\Delta$

Any non-classical negation will have to fail one of the above rules, and possibly both. In that case, what are the 'stable' rules of negation, if any, i.e. the rules that *every* negation ought to obey? This is the very issue about which each author will have his preferred answer, and it seems that there is little hope for any sort of agreement to be expected to settle around that. However, there is some possibility of agreement, I submit, if one only turns the attention to a small set of pure *negative* rules, such as:

(*n-verificatio*)	$(\exists\Gamma,\Delta \subseteq \mathcal{S}_1)(\exists\alpha \in \mathcal{S}_1)\ \Gamma,\neg^{n+1}\alpha \nVdash_1 \neg^{n}\alpha,\Delta$
(*n-falsificatio*)	$(\exists\Gamma,\Delta \subseteq \mathcal{S}_1)(\exists\alpha \in \mathcal{S}_1)\ \Gamma,\neg^{n}\alpha \nVdash_1 \neg^{n+1}\alpha,\Delta$

In the present environment, the above rules have at least 3 immediate pleasant consequences for the behavior of $\neg^{n+1}$ over $\neg^{n}$: If $\neg^{n+1}$ is to obey those rules, it cannot produce only bottoms, it cannot produce only tops, and it cannot be an identity operator. Seems sensible enough: Is anyone prepared to accept or propose as a 'real negation' any symbol failing the above rules? On the one hand, those rules are sufficient to confirm already our intuition that the logic *Ver* should be ruled out as a system interpreting $\frown$ and $\smile$ as negation operators. What will we be able to say, however, about its fragments? On the other hand, the last rules are clearly respected by classical negation, and thus also by $\frown$ and $\smile$ inside the logic *Triv*. With that criterion in mind, from here on, I will assume, as in Marcos (2005d), that a *decent* negation should respect (*n-verificatio*) and (*n-falsificatio*), for all $n \in \mathbb{N}$.

Consider now a fragment $\mathcal{L}_2$ of $\mathcal{L}_1$, such that $\mathcal{L}_2$ is directly embeddable in $\mathcal{L}_1$ by way of an identity translation, that is, $\Vdash_2 \subseteq \Vdash_1$, where $\Vdash_2$ is the consequence relation associated to $\mathcal{L}_2$. In case the signature of $\mathcal{L}_2$ also contains $\neg$ then it is clear that $\neg$ will in $\mathcal{L}_2$ respect at most as many positive rules as it did in the case of $\mathcal{L}_1$, never more. One might say in that case that $\neg$ in $\mathcal{L}_2$ is *sub-*$\mathcal{L}_1$; if $\mathcal{L}_1$ is classical logic one might simply say that $\neg$ in $\mathcal{L}_2$ is *subclassical*. So, now one can at least ask the question: In which normal modal logics the operators $\frown$ and $\smile$ produce subclassical operators? It is not difficult to check for instance that *GL* is not one of such logics: As shown in Vakarelov (1989), the characterizing axiom of *GL* can be rewritten

[1]For a semantic account of that concept, check Section 2.

in terms of $\frown$ as $\vdash \frown(\alpha \wedge \frown\alpha) \supset \frown\alpha$, and this is not a valid formula in **CPL**. One can count though on the following straightforward answer to the above question:

The operators $\frown$ and $\smile$ constitute subclassical negations inside a given normal modal logic if and only if this logic is a fragment of *Triv*.

Indeed, we already know that $\frown$ and $\smile$ coincide with classical negation inside *Triv*. As a consequence, those symbols define decent subclassical negations, *a fortiori*, also in the fragments of *Triv*. On the other hand, a logic with a subclassical negation is by definition a fragment of classical logic, as long as both logics are written in the same language. But recall that *Triv is* classical logic in disguise, possibly with some extra boxes and diamonds coloring its inferences but behaving just like identity operators. This proves our case. (Alternatively, suppose that you erase the boxes and diamonds from any normal modal logic that is *not* a fragment of *Triv*. Then you clearly transform $\frown$ and $\smile$, taken to be defined as in Figure 1, into non-subclassical negations.) QED

Still and all, the reader should not imagine that all decent negations are subclassical. Post's cyclic many-valued negations, for instance, are counterexamples to that. This paper will concentrate, in one way or another, exclusively on the more usual subclassical negations.

The next sections will show which properties *are* enjoyed by $\frown$ and $\smile$, and to what classes of negations they belong to. It will also show how normal modal logics can be naturally reconstructed on other signatures based on $\frown$, $\smile$ and related connectives.

2 Varieties of paranormality

For the sake of the following discussion, let $\mathcal{L}$ be an arbitrary logic with an entailment relation $\models$ (recall Section 1.1) defined over a set of formulas $\mathcal{S}$ of a language that contains a negation symbol $\neg$ with a decent interpretation (that is, respecting rules *verificatio* and *falsificatio* from the last section). For all we know, such logic might turn out to have some queer models, such as:

(Dadaistic) $(\forall\alpha \in \mathcal{S})(\forall x \in \mathcal{W})\ \models^{\mathcal{M}}_{x} \alpha$

(Nihilistic) $(\forall\alpha \in \mathcal{S})(\forall x \in \mathcal{W})\ \alpha \models^{\mathcal{M}}_{x}$

(To simplify notation, I will from this section on drop the contexts Γ's and Δ's from the inferences.) From the above definitions, everything is true for a dadaistic model, and everything is false for a nihilistic model. Following Marcos (2005d), I will say that the logic $\mathcal{L}$ is *overcomplete* in case all of its models are either dadaistic or nihilistic. Thus, for a non-overcomplete logic, $(\exists\alpha,\beta\in\mathcal{S})\ \alpha\not\vDash\beta$. Now, even in the case of such a logic, it might still happen that negation has some funny models such as:

($\neg$-inconsistent)	$(\exists\alpha\in\mathcal{S})(\exists x\in\mathcal{W})\ \vDash_x^{\mathcal{M}}\alpha$ and $\vDash_x^{\mathcal{M}}\neg\alpha$
($\neg$-incomplete, or $\neg$-undetermined)	$(\exists\alpha\in\mathcal{S})(\exists x\in\mathcal{W})\ \alpha\vDash_x^{\mathcal{M}}$ and $\neg\alpha\vDash_x^{\mathcal{M}}$

So, a $\neg$-inconsistent model allows for some formula to be satisfied together with its negation, and a $\neg$-undetermined model allows instead for both formulas to be non-satisfied. Obviously, a dadaistic model is simply an extreme case of an inconsistent model, and a nihilistic model an extreme case of an undetermined model. In the present framework, and following Marcos (2005b), $\mathcal{L}$ will be called a *decent $\neg$-paraconsistent* logic if it allows for non-dadaistic $\neg$-inconsistent models, that is, if $(\exists\alpha,\beta\in\mathcal{S})\ \alpha,\neg\alpha\not\models\beta$. Dually, $\mathcal{L}$ will be called a *decent $\neg$-paracomplete* logic if it allows for non-nihilistic $\neg$-undetermined models, that is, if $(\exists\alpha,\beta\in\mathcal{S})\ \beta\not\models\alpha,\neg\alpha$. In particular, a paraconsistent logic will be non-explosive, and a paracomplete logic will be non-implosive (recall the definitions of those properties from Section 1.2). Following da Costa and Béziau (1997) and Béziau (1999), I will call $\mathcal{L}$ *paranormal* if it is both paraconsistent and paracomplete.

Paranormality comes in several brands. Explosion or implosion might be lost, but maybe it is possible to recover them, 'with gentleness and time'. Maybe there is something that we can *say* about a formula so as to guarantee that it behaves consistently / determinedly? Here is a way of realizing this intuition. Let $\overline{\underline{\bigcirc}}(p)$ be a (possibly empty) set of formulas on one single variable such that:

$$(\exists\alpha\in\mathcal{S})\ \ \overline{\underline{\bigcirc}}(\alpha),\alpha\not\vDash\ \text{ and }\ \overline{\underline{\bigcirc}}(\alpha),\neg\alpha\not\vDash,$$

and yet

$$(\forall\alpha\in\mathcal{S})\ \ \overline{\underline{\bigcirc}}(\alpha),\alpha,\neg\alpha\vDash$$

Following Carnielli and Marcos (2002), any logic containing such a schema of formulas is called *$\neg$-gently explosive*. A *logic of formal inconsistency* (**LFI**) is a paraconsistent yet gently explosive logic. In such a logic, $\overline{\underline{\bigcirc}}$ is said to express *$\neg$-consistency*.

Similarly, let $\overline{\underline{\star}}(p)$ be a (possibly empty) set of formulas on one single variable such that:

$$(\exists\alpha\in\mathcal{S})\ \not\vDash\alpha,\overline{\underline{\star}}(\alpha)\ \text{ and }\ \not\vDash\neg\alpha,\overline{\underline{\star}}(\alpha),$$

and yet

$$(\forall\alpha\in\mathcal{S})\ \vDash\neg\alpha,\alpha,\overline{\underline{\star}}(\alpha)$$

Any logic containing such a schema of formulas is called *$\neg$-gently implosive*. A *logic of formal undeterminedness* (**LFU**) is a paracomplete yet gently implosive logic. In such a logic, $\overline{\underline{\star}}$ is said to express *$\neg$-determinedness*, or *$\neg$-completeness*.

The following lines are very rough, but should suffice to inform the reader about what **LFI**s and **LFU**s are good for. As the reader might have suspected, $\neg$-consistency and $\neg$-determinedness in paranormal logics serve as sorts of 'normalizing connectives'. In fact, I will from here on call them 'perfect'. From the original meaning of the word, in Latin, we know that something is *perfect* when it is 'done to the end', when it is somehow 'complete', and 'nothing essential is lacking'. In case a logic has a negation lacking the 'consistency presupposition', if one adds to it the power to express consistency then one can somehow recover what had been lost: Consistency in this case is the sought perfection. To put it in a different and semi-formal way, consider a logic $\mathcal{L}_1$ in which explosion holds good for a decent negation $\neg$, that is, a logic that validates, in particular, $(\forall\alpha \in \mathcal{S}_1)\ \alpha, \neg\alpha \vDash_1$. Let $\mathcal{L}_2$ now be some other logic written in the same signature as $\mathcal{L}_1$ such that: (i) $\mathcal{L}_2$ is a proper fragment of $\mathcal{L}_1$ that validates many or most inferences of $\mathcal{L}_1$ that are compatible with the *failure* of explosion; (ii) $\mathcal{L}_2$ is *expressive* enough so as to be an **LFI**, thus, in particular, there will be in $\mathcal{L}_2$ a set of formulas $\overline{\underline{\bigcirc}}(p)$ such that $(\forall\alpha \in \mathcal{S}_2)\ \overline{\underline{\bigcirc}}(\alpha), \alpha, \neg\alpha \vDash_2$ holds good; (iii) $\mathcal{L}_1$ can in fact be *recovered* from $\mathcal{L}_2$ by the addition of $\overline{\underline{\bigcirc}}(p)$ as a new set of valid schemas / axioms. These constraints alone suggest that the reasoning of $\mathcal{L}_1$ might somehow be recovered from inside $\mathcal{L}_2$, if only a sufficient number of 'consistency assumptions' are added in each case. Thus, typically the following *derivability adjustment theorem* (**DAT**) can be proved:

$$(\forall\Gamma\forall\Delta\exists\Sigma)\ \ \Gamma \vDash_1 \Delta \ \text{ iff } \ \overline{\underline{\bigcirc}}(\Sigma), \Gamma \vDash_2 \Delta.$$

The essentials behind such sort of **DAT**s were highlighted in Batens (1989), but some very specific instances of **DAT**s could already be found in one of the forerunning formal studies on paraconsistent logic, da Costa (1963). It is no exaggeration to say that such theorems constitute the fundamental idea behind both the 'Brazilian approach' to paraconsistency (**C**-systems and **LFI**s) and the 'Belgian approach' (inconsistency-adaptive logics). As I see it, the main difference between the two approaches is in fact methodological (but also a bit ideological). As I argued in Marcos (2001), while retaining in a paraconsistent logic 'most rules and schemata of classical logic' was a desideratum laid down already in da Costa (1974), it was never really systematically pursued by the 'Brazilian school'. The approach favored by Batens (1989) and the 'Belgian school', in contrast, took the motto to the letter: Assuming consistency *by default*, maximality is pursued by way of allowing for non-monotonic reasoning to take place. Another remarkable peculiarity is that in an **LFI**, by its very design, the clauses in the above theorem can in fact be *internalized* at the object-level language, making its statement more convenient and language-independent. Sometimes, moreover, there are yet other ways of reproducing classical reasoning inside an **LFI** through a direct translation, without the addition of further premises.

For its importance, I will dub the ability of recovering consistent reasoning in one way or another the **Fundamental Feature of LFIs**.

Clearly, all that was said for consistency and **LFI**s in the previous paragraph can be easily dualized for determinedness and **LFU**s.

I will from here on consider only the simpler case in which $\overline{\underline{\bigcirc}}$ reduces to a single schema, thus to a consistency connective $\bigcirc$ whose contradictory opposite (its classical negation), will represent an inconsistency connective to be denoted by $\bullet$. A similar thing will be done for $\overline{\underline{☆}}$, in that I will be working from here on more simply with a unary determinedness connective ☆ and the accompanying undeterminedness connective ★.

2.1 Duality, at last

I have been mentioning *duality* all along, with a strong semantic intuition, but in a very loose way. Let me here make a short digression to explain precisely what that is supposed to mean.

Given an arbitrary connective $\circledcirc$, let its *dual* be denoted by $\circledcirc^d$. Given a set of formulas Γ, let Γ^d denote the result of substituting all connectives of Γ by their duals. Given a logic $\mathcal{L}_1$ with a consequence relation $\Vdash_1$ over a set of formulas $\mathcal{S}_1$, the dual logic $\mathcal{L}_2$ will be defined by setting $\mathcal{S}_2 = \mathcal{S}_1^d$ and $\Gamma^d \Vdash_2 \Delta^d$ iff $\Delta \Vdash_1 \Gamma$. So, semantically, all we have to do, somehow, is to read the original inferences from right to left, instead of reading them from left to right, and change the names of the logical constants whenever necessary (some connectives can of course be self-dual inside a given logic).

This little trick is just enough for conjunction to be characterizable as dual to disjunction (even more, each elimination rule for conjunction will be dual to a corresponding introduction rule for disjunction, and so on), implication as dual to coimplication (and this coincides in fact with the algebraic intuition about duality explored already in Rauszer (7374)), box as dual to diamond, explosive negation as dual to implosive negation, (para)consistency as dual to (para)completeness, **LFI**s as dual to **LFU**s, dadaism as dual to nihilism, and so on.

The place where duality will show up in the Square of Modalities (Figure 1) is in place of the relation of 'subalternation'. According to the traditional semantic intuition behind subalternation, the truth of each upper corner implies the truth of the corresponding bottom corner, but not the other way around. The application of this simple idea is not without problems: The subalternation in the SoO only works well once you grant existential import to the universal quantifier, the subalternation in the SoM works fine only if you are talking about normal modal logics extending *KD*, the 'deontic' system with the seriality presupposition (in which $\Diamond\top$ is provable). The above definitions of duality, however, suggest a full horizontal symmetry in the very same square, allowing for the mentioned provisos to be dispensed

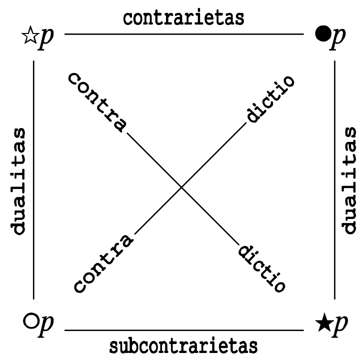

Figure 2: *Square Of Perfections* (SoP)

with. With that in mind, it does not really seem illuminating thus to think of diamond as subalternate to box (nor the other way around). That's why I proposed from the start the update of the (SoM) with the denomination '*dualitas*' in the place of '*subalternatio*'. Now, Figure 2 shows how the square would look like if rebuilt so as to apply to the perfect connectives introduced in Section 2. Notice that, according to the traditional semantic intuition of the square, $\star p$ and $\bullet p$ are 'contrary' (they cannot both be simultaneously true), $\circ p$ and $\bigstar p$ are 'subcontrary' (they cannot both be simultaneously false).

2.2 The route from modality to paranormality, and the easy way back

Where $\mathbb{K}$ is some class of frames and sig is some propositional signature, let $(\mathcal{L})_{\mathsf{sig}}$ denote the logic whose set of formulas is $\mathcal{S}_{\mathsf{sig}}$ and whose set of valid inferences is determined with the help of the canonical interpretation of the connectives in sig. With this abbreviation, every normal modal logic $\mathcal{L}$, in its usual language with set of formulas $\mathcal{S}_{\mathbf{NML}}$, will here be denoted as $(\mathcal{L})_{\wedge\vee\supset\sim\Box\Diamond}$.

We already know from the above that the usual language of normal modal logics is expressive enough so as to be able to define a decent paraconsistent negation $\smile$ and a decent paracomplete negation $\frown$. It is not difficult now to see how the corresponding perfect connectives for consistency and inconsistency ($\circ$ and $\bullet$), and for determinedness and undeterminedness ($\star$ and $\bigstar$) can also be produced. Of course, those connectives will only have their expected behavior under specific circumstances. Consider some normal modal logic $\mathcal{L}$ not extending TV (recall Section 1.2). Then, here is a possible set of definitions for the above connectives and the properties they should have in $\mathcal{L}$:

Definitions			*Properties enjoyed by them*
$\smile\alpha$	$\stackrel{\text{def}}{=}$	$\Diamond{\sim}\alpha$	$p, \smile p \not\models q$
$\bigcirc\alpha$	$\stackrel{\text{def}}{=}$	$\alpha \supset \Box\alpha$	$\bigcirc p, p \not\models$ and $\bigcirc p, \smile p \not\models$, and yet $\bigcirc p, p, \smile p \models$
$\bullet\alpha$	$\stackrel{\text{def}}{=}$	$\alpha \wedge \smile\alpha$	$\models \bullet\alpha$ iff $\bigcirc\alpha \models$, $\bullet\alpha \models \alpha$ and $\bullet\alpha \models \smile\alpha$
$\frown\alpha$	$\stackrel{\text{def}}{=}$	$\Box{\sim}\alpha$	$q \not\models \frown p, p$
$☆\alpha$	$\stackrel{\text{def}}{=}$	$\alpha \not\subset \Diamond\alpha$	$\not\models p, ☆p$ and $\not\models \frown p, ☆p$, and yet $\models \frown p, p, ☆p$
$\bigstar\alpha$	$\stackrel{\text{def}}{=}$	$\frown\alpha \vee \alpha$	$\bigstar\alpha \models$ iff $\models ☆\alpha$, $\alpha \models \bigstar\alpha$ and $\frown\alpha \models \bigstar\alpha$

Indeed, as a consequence of the above definitions:

A necessary and sufficient condition for $(\mathcal{L})_{\wedge\vee\supset\smile\bigcirc\bullet}$ to characterize a modal LFI, and for $(\mathcal{L})_{\wedge\vee\supset\frown☆\bigstar}$ to characterize a modal LFU is that $\mathcal{L}$ does not extend TV.

It is obvious that the condition is necessary. Indeed, if $\mathcal{L}$ is TV, $Triv$ or Ver, then it is not paranormal with respect to the new connectives above. Conversely, to show that this restriction provides a sufficient condition to verify the expected properties of the new connectives, consider first the case of $\smile$ and $\bigcirc$, and define a model $\mathcal{M}_1$ such that $\mathcal{W} = \{x, y\}$, $V(p, x) = 1$, $V(p, y) = 0$ and $V(q, x) = 1$, and any R such that $(x, y) \in \mathsf{R} \subseteq \mathcal{W} \times \mathcal{W}$. Such models are always possible in logics that do not extend TV, and all you have to do is to vary the accessibility relation according to the strictures of each class of frames. But then, $p, \smile p \not\models_x^{\mathcal{M}_1} q$. Next, consider any model $\mathcal{M}_2$ based on a frame such that $\mathcal{W} = \{x\}$, $V(p, x) = 1$. Then, $\bigcirc p, p \not\models_x^{\mathcal{M}_2}$, once $\Box$ is not an operator producing only bottoms —and we know that it is not, from rule (1.2) or axiom (3.1) (recall Section 1.1). Finally, consider a model $\mathcal{M}_3$ exactly like $\mathcal{M}_1$, except that now $V(p, x) = 0$. In this model $\bigcirc p, \smile p \not\models_x^{\mathcal{M}_3}$, for every logic distinct from Ver. It is clear, moreover, that $\bigcirc p, p, \smile p \models$ for any normal modal logic.
The case of $\frown$ and $☆$ is similar. QED

Now, what if we start from a paranormal language and try to define the usual connectives of normal modal logics? Can that be done at all? Again, the answer is very often YES, but, as we will see below, to understand that 'very often' one had better pay a lot of attention to the initial choice of the language.

Consider first the connectives $\smile$, $\bigcirc$, $\frown$ and $☆$ to be primitively defined by the clauses:

$$\models_x^{\mathcal{M}} \smile\alpha \quad \text{iff} \quad (\exists y \in \mathcal{W})(x\mathsf{R}y \;\&\; \alpha \models_y^{\mathcal{M}})$$

$$\models_x^{\mathcal{M}} \bigcirc\alpha \quad \text{iff} \quad \models_x^{\mathcal{M}} \alpha \text{ implies } (\forall y \in \mathcal{W})(x\mathsf{R}y \Rightarrow \models_y^{\mathcal{M}} \alpha)$$

$$\models_x^{\mathcal{M}} \frown\alpha \qquad \text{iff} \quad (\forall y \in \mathcal{W})(x\mathsf{R}y \Rightarrow \alpha \models_y^{\mathcal{M}})$$

$$\models_x^{\mathcal{M}} \star\alpha \qquad \text{iff} \quad \alpha \models_x^{\mathcal{M}} \text{ and } (\exists y \in \mathcal{W})(x\mathsf{R}y \;\&\; \models_y^{\mathcal{M}} \alpha)$$

Consider next an arbitrary normal modal logic $(\mathcal{L})_{\wedge\vee\supset\smile\bigcirc}$, where the non-classical connectives from the signature are interpreted as above. The question now is whether $(\mathcal{L})_{\wedge\vee\supset\sim\Box\Diamond}$ can be recovered from that. And the answer is that it can, if only the following definitions are set:

$$\bot \stackrel{\text{def}}{=} \alpha \wedge \smile\alpha \wedge \bigcirc\alpha, \text{ for any } \alpha \qquad\qquad \Box\alpha \stackrel{\text{def}}{=} \sim\smile\alpha$$

$$\sim\alpha \stackrel{\text{def}}{=} \alpha \supset \bot \qquad\qquad \Diamond\alpha \stackrel{\text{def}}{=} \smile\sim\alpha$$

Furthermore, to obtain an inconsistency connective one can obviously just set $\bullet\alpha \stackrel{\text{def}}{=} \sim\bigcirc\alpha$. It is not difficult to check, indeed, that even inside the minimal normal modal logic K the new connectives $\sim$, $\Box$ and $\Diamond$ behave exactly as they should. For instance, in K the following rules hold good: $(\alpha, \sim\alpha \models)$ and $(\models \sim\alpha, \alpha)$. As we know, those two rules fully characterize classical negation (recall Section 1.2). Therefore:

For every normal modal logic, $(\mathcal{L})_{\wedge\vee\supset\sim\Box\Diamond}$ and $(\mathcal{L})_{\wedge\vee\supset\smile\bigcirc\bullet}$ characterize the same logic under two different signatures.

Can the same be done if one starts from the language containing $\frown$ and $\star$ instead of $\smile$ and $\bigcirc$? The answer now is not as immediate as one might expect. Indeed, consider an arbitrary modal logic $(\mathcal{L})_{\wedge\vee\supset\frown\star}$, where the non-classical connectives are interpreted as above. How can a classical negation now be defined so as to work as expected for all classes of frames? It is easy to see that the above definitions will not do. An alternative is to set:

$$\sim\alpha \stackrel{\text{def}}{=} \alpha \supset \frown\alpha \qquad \Box\alpha \stackrel{\text{def}}{=} \frown\sim\alpha \qquad \Diamond\alpha \stackrel{\text{def}}{=} \sim\frown\alpha$$

In this case, however, in spite of $(\models \sim\alpha, \alpha)$ holding good for every normal modal logic, $(\alpha, \sim\alpha \models)$ holds good only for extensions of KT. Therefore, all one can guarantee in general is that:

For every extension of KT, $(\mathcal{L})_{\wedge\vee\supset\sim\Box\Diamond}$ and $(\mathcal{L})_{\wedge\vee\supset\frown\star\bigstar}$ characterize the same logic under two different signatures.

To recover full generality and symmetry in the second result, the easiest solution is to change implication for coimplication (putting *both* implication and coimplication in the signature is too easy a solution, as those two connectives alone already provide a functionally complete set of connectives for classical logic). So, using the coimplication alone one can set:

$$\top \stackrel{\text{def}}{=} \star\alpha \vee \frown\alpha \vee \alpha, \text{ for any } \alpha \qquad\qquad \sim\alpha \stackrel{\text{def}}{=} \alpha \not\subset \top$$

This new negation behaves classically already in K, and with its help one can define box and diamond, again, exactly as in the preceding set of definitions. Obviously, a connective for undeterminedness can be defined by setting $\bigstar\alpha \stackrel{\text{def}}{=} \sim\star\alpha$. The last paragraph shows that:

For every normal modal logic, $(\mathcal{L})_{\wedge\vee\not\subset\sim\Box\Diamond}$ and $(\mathcal{L})_{\wedge\vee\not\subset\frown\star\bigstar}$ characterize the same logic under two different signatures.

3 Imagine there are no sea battles...

I argued in Marcos (2005d) that the development of a really good theory about 'what negation is', in logic, presupposes the previous development of a modern and comprehensive formal version of the received *theory of oppositions*.[2] This was nothing short than a big issue in ancient Greek philosophy. Even nowadays, though, if one looks in retrospect, it is difficult to get a feeling that the deep philosophical advances made on this topic have received the formal counterpart they deserved. If we are to trust Plato on his account of the pre-Socratic philosophy, Heraclitus of Ephesus has seemingly spent his whole life thinking about opposition, and Parmenides spent his own thinking about how he could oppose Heraclitus on that. The dispute was allegedly also fed by their respective disciples, Cratylus and Zeno of Elea. It has often been argued that Aristotle's theory of opposition, and the Square of Oppositions that would be polished from it along the following centuries,[3] was born from an attempt to reconcile the opponents and make sense of the above dispute. A sympathizer of Heraclitus (whom he dubbed 'the Obscure') in some respects and a strong critic in many others, Aristotle seems also to have been the first (later, Apuleius, Boethius and Peter of Spain were also not entirely without fault) to pervert the initial idea of a theory of oppositions into a long and problematic theory of modal syllogisms.

In the last section we have seen how the language of normal modal logic could have been alternatively chosen as the language of paranormal negations and related operators. Maybe, had Aristotle not been the tutor of Alexander, there would never have been so much talk about sea battles, the contingency of the future and the necessity of the past. Had modal logic and kripke-like semantics been developed with the objective of understanding negation and exploring the viability of reasoning under inconsistent situations, and maybe the reader would have been surprised to learn only here and now that YES, the same modal ideas and tools could be used to talk about boxes and diamonds!

The negative modalities $\smile$ and $\frown$ have received some attention in the last decades as legitimate interpretations of negation. From this point on, let $\to$ and $-$ denote intuitionistic implication and negation. In Došen (1984) and subsequent papers, Kosta Došen showed how to axiomatize the logics $(\mathcal{L})_{\wedge\vee\to-\frown}$ and $(\mathcal{L})_{\wedge\vee\to-\smile}$, for $\mathcal{L} = K$ and for many extensions of K. Those logics were treated as bi-modal, with one accessibility relation (reflexive and transitive) used to interpret the intuitionistic connectives and another accessibility relation (that of $\mathcal{L}$) used to interpret $\frown$ and $\smile$. A similar approach had in fact been undertaken a decade earlier by Dimiter Vakarelov, and was

[2] In particular, as argued in Section 2.1, it could be advantageous in such a theory to talk about 'duality' instead of 'subalternation'.

[3] For the historical development of the SOO, check Parsons (2004).

published in Vakarelov (1989), where the logics $(\mathcal{L})_{\wedge\vee\to\frown\top\bot}$ and $(\mathcal{L})_{\wedge\vee\to\smile\top\bot}$ were axiomatized, for $\mathcal{L} = K$ and for many extensions of K, and also for signatures containing classical instead of intuitionistic implication.

An interesting problem that was left open was that of axiomatizing such logics in the language containing only the usual positive classical connectives of normal modal logics $(\wedge, \vee, \supset, \Box, \Diamond)$, extended only by the paranormal negations $\frown$ or $\smile$, without recourse to the perfect connectives $(\bigcirc, \bullet, \star, \bigstar)$, as above. Consider the paraconsistent case and the set of formulas $\mathcal{S}_{\wedge\vee\supset\smile}$. (Recall that the case where the related signature is extended by the addition of the connective $\bigcirc$ was fully solved above, where the logics obtained were shown to provide just different versions of the usual normal modal logics.) Suppose someone might object to the addition of the connective $\bigcirc$ as a 'natural connective' of our logics. This person then should take equal care so as not to add neither a bottom, $\bot$, nor a classical negation, $\sim$, to the original signature: On the one hand, we have already seen how $\sim$ and $\bot$ can be defined from $\bigcirc$; on the other hand, from a primitive $\sim$ one could easily define $\bot \stackrel{\text{def}}{=} \alpha \wedge {\sim}\alpha$, for an arbitrary α, and from a primitive $\bot$ one could define ${\sim}\alpha \stackrel{\text{def}}{=} \alpha \supset \bot$, and in both cases $\bigcirc$ could be recovered by setting $\bigcirc\alpha \stackrel{\text{def}}{=} (\alpha \supset \bot) \vee (\smile\alpha \supset \bot)$. Notice also that, whenever a classical negation $\sim$ is present, the consistency connective $\bigcirc$ will be sufficient so as to define the remaining perfect connectives from Figure 2: Just set $\bigstar\alpha \stackrel{\text{def}}{=} \bigcirc{\sim}\alpha$, $\star\alpha \stackrel{\text{def}}{=} {\sim}\bigcirc{\sim}\alpha$, and $\bullet\alpha \stackrel{\text{def}}{=} {\sim}\bigcirc\alpha$.

On what concerns the above problem, vividly denounced in Béziau (2002) for the case of $S5$, an axiomatization of $(\mathcal{L})_{\wedge\vee\supset\smile}$ was offered in Béziau (9798) only for that extreme case in which $\mathcal{L} = S5$. As Jean-Yves Béziau confessed, the extension of this result to the case of other normal modal logics proved non-obvious. I have recently found a thorough solution to the problem, but for limitations of space I can only display here the corresponding axioms. For the case of $\mathcal{L} = K$, an adequate axiomatization is given by adding to any complete set of axioms and rules for positive classical propositional logic the following further axioms and rules:

(I.1) $\vdash \alpha \supset \beta \;\Rightarrow\; \vdash \smile\beta \supset \smile\alpha$
(I.2) $\vdash \alpha \;\Rightarrow\; \vdash \smile\alpha \supset \beta$
(I.3) $\vdash \smile(\alpha \wedge \beta) \supset (\smile\alpha \vee \smile\beta)$

It is not difficult to extend this axiomatization so as to cover other logics. Indeed, for $\mathcal{L} = KT$ you just have to add $\vdash \alpha \vee \smile\alpha$ as a new axiom, for $\mathcal{L} = KB$ it suffices to add $\vdash \smile\smile\alpha \supset \alpha$ as a new axiom, for $\mathcal{L} = K5$ the axiom $\vdash \smile\alpha \supset (\smile\smile\alpha \supset \beta)$ will do. In fact, and here comes the GREAT SURPRISE, again it is possible to recover all normal modal logics from this simpler signature, if we now define classical negation by setting ${\sim}\gamma \stackrel{\text{def}}{=} \gamma \supset \smile(\alpha \supset \alpha)$, for an arbitrary formula α. So:

For every normal modal logic, $(\mathcal{L})_{\wedge\vee\supset\sim\Box\Diamond}$ and $(\mathcal{L})_{\wedge\vee\supset\smile}$ characterize the same logic under two different signatures.

The paracomplete case is a bit more complicated (recall the need we had for a coimplication in Section 2.2), as it can be proved that there is *no* definable classical negation in $(K)_{\wedge\vee\supset\frown}$, but only in $(KT)_{\wedge\vee\supset\frown}$. But there *is* a classical negation in $(K)_{\wedge\vee\not\subset\frown}$. The difficulties and details of the above mentioned solutions are to be found in Marcos (2005a).

It should be highlighted that one of the most remarkable features of all the above mentioned paranormal logics is the validity of the *replacement property* (a.k.a. *self-extensionality*). A very common and desirable property of logical systems, and a typical property of the usual systems of normal modal logic, replacement is known to fail in the great majority of well-known systems of paraconsistent logic, and that often translates into trouble for the study of their algebraic counterparts (check for instance the section 3.12 of the survey paper Carnielli and Marcos (2002)). The above modal paraconsistent logics, by their very nature, shun such difficulties.

One last comment. I have hinted above to the reticence that is sometimes to be found about the use of consistency connectives and **LFI**s, notwithstanding the possibility they inaugurate of internalizing nice properties such as the **DAT**s (recall Section 2). I have also mentioned the unavoidability of such connectives as soon as we are talking about positive classical propositional logic extended by some paraconsistent negation and by either a classical negation or a bottom. But the question might still remain as to whether that consistency connective makes any sense if there is no paraconsistent negation around. Let us assume the above modal interpretation of this consistency connective and of the related inconsistency connective to be taken as primitive, and let us conservatively extend classical propositional logic by the addition of such connectives. It is not difficult to see that the resulting language has little expressive power: No diamonds nor boxes can in general be defined, and the new connectives are not even 'normal' modal connectives in the sense of the former. In the language whose formulas are $\mathcal{S}_{\wedge\vee\supset\equiv\sim\bigcirc\bullet}$, however, one could read $\bullet\alpha$ as saying that 'α is the case, but could have been otherwise': It works as a kind of (local) connective for 'accidental truth'. Similarly, $\bigcirc$ could be read as expressing a (local) notion of 'essential truth'. In Marcos (2005c) I have axiomatized the minimal such logic of essence and accident, $(K)_{\wedge\vee\supset\equiv\sim\bigcirc\bullet}$, by extending positive classical propositional logic with the following axioms and rules:

(K0.1) $\vdash \varphi \equiv \psi \ \Rightarrow \ \vdash \bigcirc\varphi \equiv \bigcirc\psi$ (K0.2) $\vdash \varphi \ \Rightarrow \ \vdash \bigcirc\varphi$

(K1.1) $\vdash (\bigcirc\varphi \wedge \bigcirc\psi) \supset \bigcirc(\varphi \wedge \psi)$

(K1.2) $\vdash ((\varphi \wedge \bigcirc\varphi) \vee (\psi \wedge \bigcirc\psi)) \supset \bigcirc(\varphi \vee \psi)$

(K1.3) $\vdash \bullet\varphi \supset \varphi$ (K1.4) $\vdash \bullet\varphi \equiv \sim\bigcirc\varphi$

A similar interpretation could be proposed for the determinedness connective. One could read $\star\alpha$ as saying that 'α is not the case, but it could have been'. This suggests that $\star$ could work as a kind of (local) connective for 'counterfactual truth'. I will leave this here as a path that seems worth exploring. It is easy if you try.

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Chapter Four
An Abstract Perspective on Negation

This chapter is composed of two papers: Part **4.1** contains the 'On negation: Pure local rules', henceforth PURELOCAL; part **4.2** contains the 'Ineffable inconsistencies', henceforth INEFFABLE.

Resumo de PURELOCAL

Este é um estudo inicial sistemático das propriedades da negação do ponto de vista dos sistemas dedutivos abstra[c]tos. Ado[p]ta-se um arcabouço unificador de relações de consequência com conclusão múltipla de modo a nos permitir explorar a simetria na exposição e na comparação de um grande número de regras subclássicas contextuais positivas envolvendo esta constante lógica —dentre as quais, formas bem conhecidas de demonstração por casos, *consequentia mirabilis* e redução ao absurdo. Definições mais finas de paraconsistência e da paracompletude dual podem assim ser formuladas, permitindo a diferenciação das regras do pseudo-escoto e *ex contradictione*, e a apresentação de uma versão abrangente do Princípio da Não-Trivialidade. Uma proposta final é feita de tal sorte que —dada a falibilidade frequente das regras positivas puras envolvendo a negação— uma caracterização do que a maior parte das negações da literatura tem em comum deveria envolver, na realidade, um conjunto reduzido de regras negativas.

Resumo de INEFFABLE

Para cada lógica tarskiana consistente dada é possível encontrar outra lógica não-trivial que admite um modelo inconsistente e mesmo assim coincide com a lógica inicial dada do ponto de vista de suas relações de consequência com conclusão única associadas.
Um paradoxo? Esta breve nota lhe mostra como isso funciona.
Isto pode ser lido como uma descrição de uma expedição a regiões inexploradas da lógica abstra[c]ta, da teoria das valorações e da paraconsistência.

Contents

General and abstract ideas are the source of the greatest errors of mankind.
—Jean-Jacques Rousseau, *The Creed of a Savoyard Priest*, 1762.

This chapter enters the play almost as an appendix. It shows how I would have approached the main problems of this thesis, had I known then what I know now. Coming back to the initial theme of the first chapter of the thesis, the paper [11] does some investigation on General Abstract Logic (a.k.a. Universal Logic), but this time in the framework of multiple-premise-multiple-conclusion consequence relations. The choice of framework is claimed to make a significant difference, and many examples are brought forward to illustrate this claim. The paper [10] provides yet a further illustration of that, mixing abstraction and semantics. It questions the received notion of 'explosion', and entertains a definition of 'consistency' that does not depend on negation.

Here is how you should do it

One of the things that philosophy, mathematics and logic have in common is their concern for fine conceptual distinctions. Abstraction should be pursued, however, only to the point that it does not coalesce notions that had better stay apart. Many a time, the choice of framework can help either in disguising or otherwise in displaying a specific property of a formal system. Of course, there is no perfect framework —it all depends pretty much on your objectives and on your object of research at the time.

In the paper PureLocal I show some advantages of the choice of a multiple-conclusion framework for the study of logics and of logical constants (that is, logical connectives), in allowing us to draw some novel and insightful distinctions, to perform some upgrades on received theories, and to exploit all the themes of this thesis over a common background. The key ideas depend on making heavy use of the symmetry promptly provided by the new framework. Among the successes of the study, in the above mentioned directions, I could count the following: The inference rule known as *consequentia mirabilis* is shown to be misidentified by some authors with the rule of *reductio ad absurdum*; the rules of *pseudo-scotus* and of *ex contradictione sequitur quodlibet* are shown to be distinguishable; rules that are dual to *ex contradictione*, *consequentia mirabilis*, proof-by-cases, and *reductio ad absurdum* are all acknowledged in the paper; the *reductio* rules are shown not to derive all the other rules, as it has been claimed by other authors. As a matter of fact, the last section of the paper brings already a more extensive list of contributions.

I am certainly not the first to consider an abstract study of logics based on a multiple-conclusion framework. The roots of multiple-conclusion can indeed be traced as far back as to Gentzen's [4], Carnap's [1] and Kneale's [7]. It is somewhat unfortunate that the main source books that explore multiple-conclusion in obtaining results for Universal Logic, such as [19] and [25], are not known or accessible to a wider audience. The semantic aspect of such logics is quite simple. Under the canonical notion of entailment, not only truth must be preserved from the set of premises to the set of concluding alternatives, but falsehood must also be preserved from the alternatives to the assumed premises. The development of the syntactic aspects of such logics have been less of a consensus. It seems, though, that the situation has been changing, in recent years, with the advent of multiple-conclusion versions of natural deduction (cf. [24]) and of more geometrical outlooks on the development of proofs (cf. [3]).

To be sure, the multiple-conclusion framework of the paper PureLocal is not even a novelty in the context of the present thesis. Such a framework already had a role to play in **Chapters 2.1** and **3.3**. But this paper came earlier and went deeper into the subject. While it might have been possible to circumvent the more symmetricalist approach in the earlier chapters, that movement here would have done nothing but seriously cripple the paper. The paper, moreover, insists on the use of this framework for an abstract study of the properties of connectives.[1] I treat the latter much in the same way one treats 'natural kinds' in scientific discourse (cf. [13]), though the language I employ is not per force the language of 'essentialism'. To the contrary, my proposal is to characterize logics and their connectives, in general, not by the features they *have* in common, but by the features they 'lack in common'. Once the paper PureLocal formulates, examines and compares a number of abstract properties of logics, and a number of abstract properties of negation, one could quite naturally expect from it yet another answer to the BIG questions: 'What is a logic?' / 'What is negation?' Numerous papers and books have been written about that. What is novel in my proposal is the emphasis put on *negative properties*: In spite of the little chance of agreement that should be expected when people set their preferred set of positive properties about what such-and-such *is*, I claim that a more prolific and unifying approach would be one that looked for the properties that are *not* enjoyed by such-and-such, under its many possible guises. The best I can offer thus as a response to the BIG questions are a set of criteria for 'minimal decency'. There is no definitive canon to be found, but only a few guidelines for the logic-designer that wants to avoid degenerate examples of 'logics', and degenerate examples of 'negations'. Interestingly, the last criteria were also put into practice in **Chapter 3.3** in order to dodge a number of entities that we did not want to consider as candidates for '(modal paraconsistent) negations'.

On what concerns the above mentioned problems, the paper PureLocal also contains a substantive survey of the related literature. Several mistakes by other authors are localized and eliminated, when that is the case. (I might of course have left my own mistakes, as an uncalculated gift for attentive readers of the future.) As it has been remarked elsewhere, sym-

[1]Another approach to that study along similar lines can be found in Koslow's book, [8].

metry facilitates the work on duality. A byproduct of the paper is that many definitions that had been set on matters related to paraconsistency can be straightforwardly restated in terms of its dual paracompleteness. Some of the causes and the effects of a choice for 'paranormality' (recall the previous chapter) are also illustrated in this paper.

Here is how you should not do it

The paper INEFFABLE explores some of the issues raised by the preceding paper. More specifically, it stresses the semantic rationale behind what had been called 'Principle of Non-Overcompleteness', as a generalization of the 'Principle of Non-Triviality' proposed in **Chapter 1.0**, and it also explores the novel distinction that had been delineated between the rules of *pseudo-scotus* and of *ex contradictione sequitur quodlibet*, in order to show that degenerate examples of 'paraconsistency' (namely, those logics that disrespect only the former rule, but not the latter) are possible and should also be avoided thus through 'minimal decency'.

I assume paraconsistency to have been born when the first logical systems were developed with the professed intention of allowing for some inconsistencies at a local level while avoiding triviality at a global level (cf. [5, 12, 2]). Because allowing for inconsistencies meant that there could be a surplus of truths in the interpretation of a given logic, it was certainly natural that those who toiled over the design of the first paraconsistent logics became worried about there being too many truths around. They worried about that particular variety of 'overcompleteness'[2] in which every sentence of the logic turned out to be a thesis / a theorem / a tautology. They seem to be justified in their worry: If one proposes an approach to inconsistencies that lets some of them stay in our theories and perhaps even fructify, one will certainly not want to be so liberal as to let all inconsistencies become hopelessly indiscernible —a deranged state of affairs that I, in **Chapter 3.3** and in the papers composing the present chapter, call 'dadaism'.

Interestingly enough, the concern about dadaism (as a specific variety of overcompleteness) was already to be found in one of the papers on the notion of logical consequence proposed by Tarski himself.[3] To explain this point, I had better make a brief digression first. The early Polish tradition used to think about logic from a topological point of view, and to construe

[2]From the Polish 'przepełnienie'. 'Overcompleteness' was Jaśkowski's term according to the first English translation of his 1948 Polish paper. Nelson refers to this paper in 1959 (with the title in French, as it had in fact been published with a summary in French) and uses the term 'overcompleteness' eight years before Jaśkowski's paper was to receive its first published English translation. More or less at the same time, da Costa was starting to use the word 'triviality' in his papers published in Portuguese, even before he wrote his 1963 thesis, also in Portuguese. The second English translation of Jaśkowski's paper employs the term 'overfilling'.

[3]He certainly did not share, however, the motivations that moved the paraconsistentists in their concern. Recall indeed from section 1 of the TAXONOMY (**Chapter 1.0**) the passage about Tarski's hostility towards inconsistent theories.

logical consequence as a closure operator.[4] Later on, logic was turned into an animal of another breed, when the topological outlook on consequence remained but the set of formulas itself started to be presented as an algebra. Tarski presented most of his ideas on logical consequence in terms of closure operators, but it is not too difficult in general to translate the clauses governing the behavior of closure *operators* into similar clauses governing the behavior of (single-conclusion) consequence *relations*, a framework that became more common nowadays. The axioms proposed by Tarski for the notion of logical consequence varied a lot over the years. So, what I call 'tarskian logic' in some of my papers is in reality quite stipulative. On that matter I usually try to settle around the semantic notion of derivability presented by Tarski in [23]. For that notion a nice suitable adequacy theorem is available (recall **Chapter 2.1**), according to which a single-conclusion consequence relation is to be axiomatized in abstract terms by:

(CR1s)	$\Gamma, \varphi, \Delta \Vdash \varphi$	(overlap)
(CR2s)	if $\Delta \Vdash \varphi$, then $\Gamma, \Delta \Vdash \varphi$	(dilution)
(CR3s)	if $(\forall \delta \in \Delta)(\Gamma \Vdash \delta)$ and $\Delta \Vdash \varphi$, then $\Gamma \Vdash \varphi$	(cut for sets)

Before that, however, Tarski had already published a much more detailed account of 'some fundamental concepts of Metamathematics' (cf. [22]),[5] where to the above axioms he added the requirements that logics should be compact, and their underlying languages should be denumerable and should contain a bottom particle (that is, logics should respect, in particular, our 'Principle of *Ex Falso*', from **Chapter 1.0**). In [21], the first study towards the latter paper and Tarski's first published note on the theme of logical consequence, logic was running closer to topology, and to the above three axioms the requirement was added that closure operators should preserve arbitrary unions. Moreover, in that initial paper Tarski[6] also considered a number of specializations of the above defined structures, by the addition of further axioms. Among such possible axioms there is one that brings us back to the initial theme of this paragraph, namely:

(CR0s)	$(\exists \varphi) \nVdash \varphi$	(compatibility)

This compatibility condition —that seems to have passed unnoticed and to have been completely forgotten in later years— corresponds neatly, in the presence of dilution, to Jaśkowski-Nelson-da Costa's concerns about avoiding dadaism. It is easy to see that Tarski's compatibility corresponds, in the logics we here consider, to the already traditional 'Principle of Non-Triviality', and my present version of the 'Principle of Non-Overcompleteness'

[4] As it had been shown by Kuratowski, topologies can be seen as particular cases of closure operators, for which you require the empty set to have an empty closure and the union of closed sets to be identical to the closure of their union.

[5] First presented by Jan Łukasiewicz to the Warsaw Scientific Society on 27 March 1930.

[6] Or whoever wrote the paper for him. The paper is part of a 'comptes-rendus' where someone is supposed to write down a summary of the main contents of the lectures presented in a meeting of the Polish Mathematical Society. In the resulting text, Tarski is referred to in the third person.

introduces a generalization of the former principle, in abstract, to the point of regulating also three other examples of degenerate logics.

As it has been noted, the single-conclusion framework, under the above axioms, guarantees that truth is preserved forwards, from premises to conclusion. Full symmetry is only installed though with a multiple-conclusion framework, where falsehood is likewise preserved, but backwards. Where $\mathsf{Ptn}(\Sigma)$ denotes the set of all partitions of the set Σ, here are the 'tarskian' axioms in a multiple-conclusion fashion:

(CR1m)	$\Gamma, \varphi, \Delta \Vdash \Sigma, \varphi, \Pi$	(overlap)
(CR2m)	if $\Delta \Vdash \Sigma$, then $\Gamma, \Delta \Vdash \Sigma, \Pi$	(dilution)
(CR3m)	if $(\forall \langle \Sigma_1, \Sigma_2 \rangle \in \mathsf{Ptn}(\Sigma))(\Gamma, \Sigma_1 \Vdash \Sigma_2, \Delta)$, then $\Gamma \Vdash \Delta$	(cut for sets)

Sometimes one finds other conditions in the place of cut for sets, as for instance the following 'contextual' forms of cut for formulas:

(CR3cp)	if $\Gamma, \varphi \Vdash \psi$ and $\Gamma \Vdash \varphi$, then $\Gamma \Vdash \psi$
(CR3cc)	if $\varphi \Vdash \psi, \Delta$ and $\Vdash \varphi, \Delta$, then $\Vdash \psi, \Delta$
(CR3c)	if $\Gamma, \varphi \Vdash \Delta$ and $\Gamma \Vdash \varphi, \Delta$, then $\Gamma \Vdash \Delta$

It should be noted, however, that such alternative versions of cut can be shown to be strictly weaker than the initial formulation above, the 'cut for sets' (cf. chap. 2 of [19]). Again, the reason for the stronger choice of axioms for multiple-conclusion consequence relations, as an extension of the original approach by Tarski to consequence operators, is the availability of a nice suitable adequacy theorem (recall **Chapter 2.1**), maintaining something very much like the original semantic intuition.[7]

The multiple-conclusion framework has some further advantages. In the paper INEFFABLE I show that a single-conclusion framework cannot see the difference between a consistent logic and this 'same' logic when added of an extra dadaistic model. That a multiple-conclusion framework *can* see the difference should not really come as a great surprise. To explain that, let me first make yet another brief digression to recall a few concepts. Roughly speaking, the term *gap* is customarily used to mark a situation in which there is a 'paucity of truths'. For instance, in the semantics of paracomplete

[7] An authoritative referee has called my attention to the 'mistake' of calling 'tarskian' the class of logics whose consequence relation is multiple-conclusion and is axiomatized through clauses (CR1m)–(CR3m). He claimed that this is "what the literature calls 'Scott Consequence Relations'", and advised me to "see e.g. Gabbay's book on intuitionistic logic". Well, if there is a mistake involved in my decision, it is certainly not *my* mistake, and maybe not even of Gabbay's book (which book?). Dana Scott has indeed proposed the study of multiple-conclusion versions of the preceding tarskian axioms, initially formulated in terms single-conclusion consequence relations. Typically, [16, 15, 17] are the papers published by Scott that are cited by those who claim that 'multiple-conclusion logics are scottian'. I know that too well —I have made that confusion myself. Nonetheless, axiom (CR3m) is never to be found in those papers; at best one can find the weaker (CR3c) in its place. Scott's approach in these papers, in fact, always seems quite tentative, and it shows no hint of a deep underlying semantic motivation. Not surprisingly, nowhere has Scott an adequacy theorem to offer about the weaker notion of consequence relation he proposes. My own approach, thus, cannot be 'scottian'. It is based instead on the work of Shoesmith & Smiley (cf. [18, 19, 25]).

tarskian logics, the circumstance that a formula α and its negation $\sim\alpha$ both are given non-designated values can be reformulated by simply saying that α is 'neither true nor false' or that there is a truth-value gap in α. A similar account can be given about *gluts* and situations in which there is an 'excess of truths', as in the semantics of paraconsistent tarskian logics. Moreover, consequence relations are said to be *categorical* if distinct sets of valuations characterize distinct consequence relations. Now, in [14, 6] one is assured that, while single-conclusion tarskian consequence relations are in general *not* categorical, multiple-conclusion tarskian consequence relations for logics whose semantics contain either gaps or gluts *are* categorical.

Besides the typical inertia, and some ignorance, of scholars, it seems hard to find reasons, in fact, for single-conclusion consequence relations to remain so popular in the current literature on logic. Perhaps this can be explained by the everlasting influence of closure operators, or the appealing lopsidedness of the natural deduction formalism. Or maybe this is just because philosophers have accommodated around the notions of *theoremhood* and *truth*, to which study a single-conclusion framework seems tailored to fit. But that poses to me then yet another enigma. Why in the world do some metaphysicians seem to think nowadays that logic has anything to tell you about 'truth', in the first place? For all I know, they might be misled by the spell of language and the influence of old habits of thought. I do not understand why there is still such a persistent bias towards truth, anyway, when falsehood would in theory seem equally important. Why should truth be privileged over falsehood? Why to worry exclusively with dadaism when 'nihilism' —the situation in which every sentence of the logic turns out to be an antithesis / an antitheorem / an antilogy— should seem equally deranging? Nihilism, just like dadaism, makes everything indiscernible.

Other possible reasons that might have collaborated for the perpetuation of the single-conclusion framework in logic and of the related obsession about truth are: (a) that the most common logical systems enjoy compactness and some suitable form of deduction theorem (so that provable inferences are just as good as theorems); (b) that the most common systems of sequents are finitary and contain suitable conjunctions and disjunctions; (c) that the traditional study of constructiveness, or of (effective) provability, often emphasizes single-conclusion; (d) that some popular syntactical mechanisms correspond naturally to single-conclusion calculi. In contrast, multiple-conclusion can: (a) help implementing a natural notion of *duality*; (b) put *truth* and *falsehood* on equal footing, without requiring much from the underlying language; (c) provide a framework for empirical evidences to be collected directly about the *absence* of a given property; (d) internalize a primitive notion of *rejection*, alongside with *assertion*. On the latter point, special attention should be given to *refutative* systems, that dualize the Fregean primitive symbol of assertion as suggested by Brentano and studied by Łukasiewicz (for a natural deduction approach to the work of the latter, check for instance [20]).

The present chapter of the thesis illustrates on and on how the choice of underlying framework can make a difference for the purposes of Universal Logic, the abstract study of logical structures. As I have already mentioned, the paper Ineffable shows that a single-conclusion framework simply cannot detect some varieties of inconsistency that are allowed by an approach to Universal Logic based on the theory of valuations, even when overcompleteness is diligently shunned. Wariness on what concerns those latent possibilities is the least that should be expected if one aspires Universal Logic to be anything more than General Abstract Nonsense.

Brief history

An early version of the paper PureLocal was ready by June 2002, while I was working in Brazil under a CNPq doctoral grant. It was intended at the start to be presented at the Workshop on Paraconsistent Logic (WoPaLo), which I organized as part of the 14th Summer School in Logic, Language and Information (ESSLLI 2002), held in Trento (IT) in August 2002. However, the workshop was a big concert in need of an introduction, so I renounced to presenting the above paper and I wrote and presented there instead the overture [9]. A more thorough version of the above material was submitted nevertheless to the 'Proceedings of the WoPaLo' a few months later, and its ideas were criticized during a talk I gave at the Theory of Computation Seminar promoted by the Center for Logic and Computation of the IST (PT), in May 2003. The paper was corrected with extreme care while I was already working in Portugal under an FCT doctoral grant, and besides all the people acknowledged in the paper I should show my gratitude to the friend João Rasga for his continuous encouragement and his help with doing some important bibliographic research. The corrected final proofs of the paper, to be published soon by the Journal of Applied Logic, appeared on-line at Elsevier's ScienceDirect web-site in August 2004.

The ideas and the general critique contained in the paper Ineffable were first presented at the round-table 'Systems of Paraconsistent Logic', at the closure session of the III World Congress on Paraconsistency (WCP 3), held in Toulouse (FR) in July 2003.

A melange of ideas about multiple-conclusion and about the generalization of the Principle of Non-Trivialization propounded in both papers from this chapter constituted the kernel of a talk presented at the colloquium Logic, Ontology, Aesthetics: The Golden Age of Polish Philosophy, jointly promoted by the Université du Québec à Montréal, the Consulate of Poland, and the Concordia University (CA), in September 2004. I am very grateful for the reactions of the audience there, and specially for the historical clarifications made by Jan Woleński and Jean-Yves Béziau. Bog knows why, the organizers decided to prize my contribution as a 'best communication project'. I am confident that they did not regret their decision.

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On negation: Pure local rules

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Abstract

This is an initial systematic study of the properties of negation from the point of view of abstract deductive systems. A unifying framework of multiple-conclusion consequence relations is adopted so as to allow us to explore symmetry in exposing and matching a great number of positive contextual sub-classical rules involving this logical constant —among others, well-known forms of proof by cases, *consequentia mirabilis* and *reductio ad absurdum*. Finer definitions of paraconsistency and the dual paracompleteness can thus be formulated, allowing for *pseudo-scotus* and *ex contradictione* to be differentiated and for a comprehensive version of the Principle of Non-Triviality to be presented. A final proposal is made to the effect that —pure positive rules involving negation being often fallible— a characterization of what most negations in the literature have in common should rather involve, in fact, a reduced set of negative rules.

Keywords: negation, abstract deductive systems, multiple-conclusion logic, paraconsistency

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Proposal

'Contrariwise,' continued Tweedledee, 'if it was so, it might be; and if it were so, it would be; but as it isn't, it ain't. That's logic.'
—Lewis Carroll, *Through the Looking-Glass, and what Alice found there*, 1872.

This is an investigation of negation from the point of view of universal logic, the abstract study of mother-structures (in the sense of Bourbaki) endowed with consequence relations. In that, it has as important predecessors [10], [1], and related papers. The general framework adopted here for the study of pure rules for negation —those that do not involve other logical constants but negation— is that of multiple-conclusion consequence relations, as in [32]. Section 0 introduces the general framework and main related definitions and notations. Section 1 presents the most usual axioms regulating the behavior of multiple-conclusion consequence relations, such as overlap, (cautious) cut, (cautious) weakening, compactness and structurality, and shows how several distinct notions of overcompleteness can be defined. The latter notions can be used to catalogue four distinct varieties of triviality, and allow for an extension of da Costa's 'Principle of Tolerance,' (or rather 'Principle of Non-Triviality') in the last section. Although the present study is neither proof-theoretical nor semantical in nature, some hints are given on the import of several abstract schematic rules hereby presented from a semantic viewpoint, and reports are often given about the behavior of those rules in the context of some non-classical logics —such as relevance, modal and (sub)intuitionistic or intermediate logics— with which the reader might be familiar. Local, or contextual, rules can be studied in opposition to global rules —positive local schematic rules are meant to hold for any choice of contexts and formulas contained therein, positive global schematic rules are usually weaker rules meant to display relations among local rules. These kinds of rules are contrasted in papers such as [28], [20] and [12]; in [30] the author chooses to present global rules for the connectives as more 'legitimate,' here I acknowledge instead that local rules are fairly more common, and concentrate on them. The distinction between local and global rules is reminiscent of the traditional philosophical distinction between inference rules and deduction rules —an elegant modern abstract account of it can be found in ch. 3 of [14].

Section 2 presents a few blocks of local sub-classical rules for negation —among them, some rules that are positive (being universally respected in classical logic) and some rules that are negative (being classically valid for some choices of contexts and formulas but failing for others). The first bunch of rules comes in two dual sets: The first one regulates those properties of negation which are related to 'consistency assumptions' (the inexistence of non-dadaistic models for some formulas together with their negations), the second regulates 'completeness assumptions' (the satisfiability of either a formula or its negation in each non-nihilistic model). Consistency rules include *pseudo-scotus*, which underlies the Principle of Explosion, and *ex contradictione sequitur quodlibet*, and these two rules can be sharply distinguished in the present framework of multiple-conclusion consequence relations; completeness rules include excluded middle, proof by cases and *consequentia mirabilis*; some of those rules will partly span both categories, as for instance the completeness rule of *reductio ad absurdum*, which might interfere with *ex contradictione*. A second bunch of rules deals with other forms of manipulation of negation: Double negation introduction and elimination, contextual contraposition and contextual replacement are among those rules. The various interrelations between those sets of rules are carefully investigated here. The present study teams up and generalizes in part some other foundational studies on negation, such as [25], [9], [18], [22], [23] and [4]. Note that I will not insist here that a negation operator should have *any* of the above mentioned properties. Finally, the last bunch of rules comes again divided into two dual sets, which have the most distinguishing feature of being negative rules, dealing with some minimal properties that a reasonable negation should *not* have in order to reckon minimally interesting interpretations —I would be more reluctant to abandon one of these last negative properties than any of the preceding positive ones.

Paraconsistency, in particular, is equated to the failure of the Principle of Explosion, and this reflects in the failure of the most basic form of *pseudo-scotus*. Dual definitions are offered for paracomplete logics and their subclasses, and some ILLUSTRATIONS are given. Other fine definitions are easily introduced in this framework, as in section 3, so as to characterize a few interesting subclasses of paraconsistent logics. From the relations established among and inside the three blocks of rules mentioned above, the reader will immediately be able to trace, in particular, some causes and effects of paraconsistency from the point of view of universal logic. For an account of the effects of the above systematization for the praxis of the non-classical designer, section 3 also illustrates some of the necessary and sufficient conditions for paranormality —either paraconsistency or paracompleteness— in logic.

The first part of section 4 argues that, while individual classes of logics or classes of negations might well be characterized by positive rules, the *very notions* of logic and of negation, or at least the interesting realizations of those notions, are often best characterized *negatively*, by saying which properties they should not enjoy. Definitions of *minimally decent* classes of logics and classes of negations are then put forward. The section continues by surveying some of the most remarkable attempts to answer the bold question of ‘What is negation?’ ([22], [21], [23], [25] and [11]), calling attention to some of the merits of each approach and some of their flaws or deficiencies, while at the same time coherently situating them all in the framework set in the present paper for easier comparison.

The last section ends up by listing some of the main novelties and contributions of the present paper (you can go there and read them at any time), and hints at some generalizations and extensions of the basic notions hereby assumed and at directions in which this research should be furthered.

A warning: The intended generality in the exposition of the pure rules for negation, below, might make them hard to read, here and there. It is always easier though to start by looking at the basic cases of each family of rules. The reader should also try not to get psychologically deterred by the formulation of the FACTs relating those rules. Some might have the impression that I am trying to draw a map of the empire at a scale 1:1. That is surely *not the intention*. The goal is indeed to be precise about our roads and connections, but, curiously, the full details of the map itself are often not that important here —besides, the map is really easy to draw, once you get an idea of what's going on. Much of what follows is in fact part of many logicians' folklore, now updated into a uniform setting, which reveals relationships already known, and makes it easy to check some new unsuspected relationships... and to introduce some new concepts altogether. The idea, then, avoiding *disorder*, is that you get the spirit, and *don't* lose the feeling (let it out somehow).

0. Background

Logic, *n.* The art of thinking and reasoning in strict accordance with limitations and incapacities of the human misunderstanding. —Ambrose Bierce, *The Devil's Dictionary*, 1881–1906.

After a century of historical reinvention in the field of logic, it rests still rather uncontroversial to admit that there is no general agreement about what a logic or a logical constant *is*. Nonetheless, one might feel quite safe here, yet free, with the forthcoming non-dogmatic definitions.[1] Following a good deal of the recent literature, this short investigation will assume that logics are concerned with the formal study of (patterns of) reasoning, or argumentation, that is, they are concerned with deduction, with 'what follows from what.' Accordingly, let's take a *logic* $\mathcal{L}$ as a structure of the form $\langle \mathsf{S}_\mathcal{L}, \Vdash_\mathcal{L} \rangle$, where $\mathsf{S}_\mathcal{L}$ is a set of (well-formed) *formulas* and $\Vdash_\mathcal{L} \subseteq \wp(\mathsf{S}_\mathcal{L}) \times \wp(\mathsf{S}_\mathcal{L})$ is a (*multiple-conclusion*) *consequence relation*, or *entailment*, defined over sets of formulas (also called *theories*) of $\mathcal{L}$. Using occasionally decorated capital Greek letters as variables for theories, and doing a similar thing with

[1] By this 'non-dogmatic' I mean that the following definitions and formulations should be taken and investigated as what they are: *proposals*, rather than *prescriptions*. So, I will (try) not (to) be committing myself to any particular set of assumptions, but rather be interested in investigating the effects of each particular choice. A gentle bias towards the concerns of the paraconsistent scenario might though be noted —that is explained by this being the area of my major expertise and experience, and the area whose open questions originated this study.

lowercase Greek for formulas, then putting the consequence relation in infix format, I shall often write something as $\Gamma, \alpha, \Gamma' \Vdash_{\mathcal{L}} \Delta', \beta, \Delta$ to say that $\langle \Gamma \cup \{\alpha\} \cup \Gamma', \Delta' \cup \{\beta\} \cup \Delta \rangle$ falls into the relation $\Vdash_{\mathcal{L}}$. Such clauses will be called *inferences*, and their intended reading is that some formula or another among the *alternatives* in the right-hand side of $\Vdash_{\mathcal{L}}$ should follow from the whole set of *premises* in its left-hand side. The theories $\Gamma, \Gamma', \Delta', \Delta$ will be called *contexts* of the inference. A similar move is made by the *canonical model-theoretic account* of a consequence relation: At least one of the alternatives should be true when all the premises are true. Keeping in mind that each such inference should always be relativized to some previously given logic, I shall omit subindices whenever I see no risk of confusion among the plethora of diverse consequence relations and logics which will be allowed to appear below.

The following paragraphs are mostly notational and somewhat boring, so I guess the reader can thread them very quickly and return only when and if they feel the need of it. Note that expressions like '$\neg A$,' 'A / B' and 'A // B' will be used as abbreviations for the metalogical statements 'A is not the case,' 'if A then B' and 'A if and only if B,' and expressions like '$A \Rightarrow B$ {NN}' and '$A \Leftrightarrow B$ {NN}' will abbreviate the metalinguistic 'A implies B, in the presence of NN,' and 'A is equivalent to B, in the presence of NN.' Let $[A_b]_{b \leq \mathrm{C}}$ denote some *sequence* of the form '$A_{b_1}, \ldots, A_{b_z}$,' whose members are exactly the members of the family $\{A_b\}_{b \leq \mathrm{C}}$;[2] whenever the sequence is composed of inference clauses, commas will be read as metalinguistic conjunctions; whenever $\mathrm{C} = 0$, one is simply dealing with an empty sequence. Note that at the metalinguistic level we shall be freely using the mathematical reasoning from classical logic.

In order to add some structure to the set of formulas S, let $\circledcirc_i$ denote some *logical constant* of arity $\mathsf{ar}(i) \in \mathbb{N}$. S will be dubbed *schematic* (*with respect to* $\circledcirc_i$) in case $\circledcirc_i([\alpha_j]_{j \leq \mathsf{ar}(i)}) \in \mathsf{S}$ and $\{\beta_j\}_{j \leq \mathsf{ar}(i)} \subseteq \mathsf{S}$ imply $\circledcirc_i([\beta_j]_{j \leq \mathsf{ar}(i)}) \in \mathsf{S}$. This already embodies some notion of 'logical form.' To make it even stronger, S will be said to have an *algebraic character* in case it is the algebra freely generated over some set LC of logical constants with the help of a convenient set at of *atomic sentences*, thus implying, in particular, that $\{\beta_j\}_{j \leq \mathsf{ar}(i)} \subseteq \mathsf{S} \Rightarrow \circledcirc_i([\beta_j]_{j \leq \mathsf{ar}(i)}) \in \mathsf{S}$. An *endomorphism* in $\mathcal{L}$ is any mapping $* : \mathsf{S} \to \mathsf{S}$ that preserves the constants of $\mathcal{L}$, that is, such that $(\circledcirc_i([\alpha_j]_{j \leq \mathsf{ar}(i)}))^* = \circledcirc_i([\alpha_j^*]_{j \leq \mathsf{ar}(i)})$ for any $\circledcirc_i \in \mathsf{LC}$. Given a set S of formulas with algebraic character and a set of generators at, a *uniform substitution* —another commonly required ingredient of the notion of 'logical form'— is the unique endomorphic extension of a mapping $* : \mathsf{at} \to \mathsf{S}$ into the whole set of formulas. Given the aims of this study, I shall assume

[2] In general this family will be finite, or at most denumerably finite —ultimately, though, its cardinality will always be supposed here to be limited by the cardinality of the underlying set of formulas S.

below that a unary *negation* symbol $\sim$ will always be present as a logical constant in the underlying language of our logics, and S will be assumed to contain at least one formula of the form $\sim\varphi$. This assumption, together with the schematism of S which shall be postulated from here on, will allow us to quantify metalinguistically over formulas. As some further notational help, I will use the following symbols for iterated negations: $\sim^0\alpha := \alpha$ and $\sim^{\mathrm{n}+1}\alpha := \sim^{\mathrm{n}}\sim\alpha$—these will be used to inject a bit more of generality into the formulation of the rules in section 2.

Here, a(n *inference*) *rule* will be simply a relation involving one or more inferences. Given some rule A, I will sometimes be writing $(\forall\mathbf{form})A$ or $(\exists\mathbf{form})A$ in order to quantify in this way over the lowercase Greek elements that appear in A; similarly, I will be writing $(\forall\mathbf{cont})A$ or $(\exists\mathbf{cont})A$ in order to quantify accordingly over its elements in uppercase Greek. A formula φ will be said to *depend only on* its *component* formulas $[\varphi_i]_{i\leq\mathrm{I}}$ whenever φ can be written with the sole help of the mentioned component formulas and the logical constants of the language —this shall be denoted by $\varphi\langle[\varphi_i]_{i\leq\mathrm{I}}\rangle$. In a similar vein, to denote a theory Φ whose formulas depend only on the formulas $[\varphi_i]_{i\leq\mathrm{I}}$, one will write $\Phi\langle[\varphi_i]_{i\leq\mathrm{I}}\rangle$. Unless I say something to the contrary, when I state a rule below I shall be referring to the universal closure of this rule, that is, I shall be writing a *schematic rule*, a rule that holds for any choice of contexts and formulas explicitly displayed in it. In the same spirit, when I write by way of $\Gamma, [\alpha_i]_{i\leq\mathrm{I}} \nVdash [\beta_j]_{j\leq\mathrm{J}}, \Delta$ —or, what amounts to the same, $\neg(\Gamma, [\alpha_i]_{i\leq\mathrm{I}} \Vdash [\beta_j]_{j\leq\mathrm{J}}, \Delta)$— the metalogical denial of a rule, I shall mean that there is *some* choice of contexts Γ and Δ and of formulas $[\alpha_i]_{i\leq\mathrm{I}}, [\beta_j]_{j\leq\mathrm{J}}$ under which the rule $\Gamma, [\alpha_i]_{i\leq\mathrm{I}} \Vdash [\beta_j]_{j\leq\mathrm{J}}, \Delta$ does *not* hold. The notation $\Gamma, \alpha \dashv\Vdash \beta, \Delta$ shall abbreviate the metalogical conjunction of $\Gamma, \alpha \Vdash \beta, \Delta$ and $\Gamma, \beta \Vdash \alpha, \Delta$ —obviously, this is symmetric, and it results in the same to write $\Gamma, \beta \dashv\Vdash \alpha, \Delta$. To be sure, most statements below will have instances with the format $[A_b]_{b\leq\mathrm{C}} \,\#\, D$, where each element of $[A_b]_{b\leq\mathrm{C}}$ and each D represents an inference clause, and $\#$ represents some sort of 'implication': *Positive local* schematic rules such as (C1) and (C2) a few lines below will be constituted of universally quantified schemas, in the form $(\forall\mathsf{form})(\forall\mathsf{cont})([A_b]_{b\leq\mathrm{C}} \,\#\, D)$; *negative local* schematic rules such as $\neg$(C1) are opposed to positive rules, having thus the form $(\exists\mathsf{form})(\exists\mathsf{cont})\neg([A_b]_{b\leq\mathrm{C}} \,\#\, D)$; *global positive* schematic rules will have the form $(\forall\mathsf{form})([(\forall\mathsf{cont})A_b]_{b\leq\mathrm{C}} \,\#\, (\forall\mathsf{cont})D)$; *global negative* schematic rules will have the form $(\exists\mathsf{form})([(\exists\mathsf{cont})\neg A_b]_{b\leq\mathrm{C}} \,\#\, (\exists\mathsf{cont})\neg D)$. Note that each local, or contextual, rule of the above formats can immediately be given a global version, by suitably distributing some of the metalinguistic contextual quantifiers as expected.

1. Rules for abstract consequence relations

Ex falso nonnumquam sequitur verum, et tamen semper absurdum.
—Jakob Bernoulli, XVII century.

I now proceed to consider some rules which have often been proposed as general properties of 'any' consequence relation. Let's start by:

(C1) Overlap, or Reflexivity: $(\Gamma, \alpha, \Gamma' \Vdash \Delta', \alpha, \Delta)$
(C2) (Full) Cut: $(\Gamma \Vdash \alpha, \Delta$ and $\Gamma', \alpha \Vdash \Delta')$ / $(\Gamma', \Gamma \Vdash \Delta, \Delta')$

To facilitate reference in the following, call *simple* any logic whose consequence relation respects the two above properties (cf. [1]). Given that it is quite usual for a formula to be assumed to follow from itself, most known logics will indeed respect overlap, thus I will not explicitly consider here any weaker versions of this rule (but the reader should be aware of the existence of, for instance, some relevance logics failing the general version of overlap). The full formulation of cut above, however, is quite often more than one needs (or that one can count on) for most practical purposes, as the reader shall see in the following. Many a time, one of the following weaker formulations will suffice:

(C2.1.I) (I-)left cautious cut: $([\Gamma \Vdash \alpha_i, \Delta]_{i\leq \mathrm{I}}$ and $\Gamma, [\alpha_i]_{i\leq \mathrm{I}} \Vdash \Delta)$ / $(\Gamma \Vdash \Delta)$
(C2.2.J) (J-)right cautious cut: $(\Gamma \Vdash [\alpha_j]_{j\leq \mathrm{J}}, \Delta$ and $[\Gamma, \alpha_j \Vdash \Delta]_{j\leq \mathrm{J}})$ / $(\Gamma \Vdash \Delta)$

Obviously, (C2.1.1) and (C2.2.1) are identical rules; call them 1-*cautious cut*, and call 1-*simple* those logics respecting (C1) and (C2.k.1). In the FACTs I will mention below, I shall often be relying on overlap and 1-cautious cut, and sometimes I will use full cut. There are other interesting 'contextual versions' of cut which dwell in between its cautious versions and the full version, but I shall not study them here.

Other very common rules characterizing general consequence relations are:

(C3) Weakening, or Monotonicity: left weakening plus right weakening
(C3.1) Left weakening: $(\Gamma \Vdash \Delta)$ / $(\Gamma', \Gamma \Vdash \Delta)$
(C3.2) Right weakening: $(\Gamma \Vdash \Delta)$ / $(\Gamma \Vdash \Delta, \Delta')$

Useful information to bear in mind, to fill the gaps in the proofs of the assertions which will be found below, are the easily checkable derivations:

Fact 1.1 Consider the rules:
(r1) $(\Gamma, [\alpha_i]_{i\leq \mathrm{I}} \Vdash [\beta_j]_{j\leq \mathrm{J}}, \Delta)$
(r2) $[\Gamma \Vdash \alpha_i, \Delta]_{i\leq \mathrm{I}}$ / $(\Gamma \Vdash [\beta_j]_{j\leq \mathrm{J}}, \Delta)$
(r3) $[\Gamma, \beta_j \Vdash \Delta]_{j\leq \mathrm{J}}$ / $(\Gamma, [\alpha_i]_{i\leq \mathrm{I}} \Vdash \Delta)$
Then:

(i)	(r1) $\Rightarrow$ (r2), for $\mathrm{I} = 0$	{}
	(r1) $\Rightarrow$ (r2), for $\mathrm{J} = 0$	{(C2)}
	(r1) $\Rightarrow$ (r2), in all other cases	{(C2) and (C3)}

(ii)	(r1) $\Rightarrow$ (r3), for $J = 0$	$\{\}$
	(r1) $\Rightarrow$ (r3), for $I = 0$	$\{$(C2)$\}$
	(r1) $\Rightarrow$ (r3), in all other cases	$\{$(C2) and (C3)$\}$
(iii)	(r2) or (r3) $\Rightarrow$ (r1)	$\{$(C1)$\}$ ∎

Standard *tarskian* consequence relations (cf. [36]) are characterized by the validity of (C1), (C2) and (C3), but for *non-monotonic* logics this rule (C3) (and also (C2)) fails to obtain in full generality. Thus, the model-theoretic account related to non-monotonic logics should be expected to be an update of the standard one, so as to take contexts into account in evaluating the truth of formulas or the satisfiability of schematic rules. Some interesting milder versions of the weakening rule are the following:

(C3.1.K) (K-)left cautious weakening:
$([\Gamma \Vdash \alpha_k]_{k\in K}$ and $\Gamma \Vdash \Delta)$ / $(\Gamma, [\alpha_k]_{k\in K} \Vdash \Delta)$

(C3.2.L) (L-)right cautious weakening:
$([\alpha_l \Vdash \Delta]_{l\in L}$ and $\Gamma \Vdash \Delta)$ / $(\Gamma \Vdash [\alpha_k]_{l\in L}, \Delta)$

Now, many interesting non-monotonic logics —the so-called *plausible* ones (cf. [3]), of which adaptive logics (cf. [7]) under the 'minimal abnormality' strategy constitute a special case— will still respect (C1), (C2.1.I), (C2.2.J), (C3.2) and (C3.1.K). Other exotic consequence relations, such as the one induced by *inferentially many-valued* logics (cf. [26]), will only respect, in general, the properties (C2.1.I), (C2.2.J) and (C3). I will call a logic *cautious tarskian* in case it respects overlap, cautious cut and cautious weakening.

Note that, from this point on, I will often be using italic lowercase / uppercase letters as wildcards for a string of one / finitely-many arbitrary variables. Note also that 'finitely-many' does not exclude the empty string. Separating dots are not parsed. One can then easily check that:

Fact 1.2

(i)	(C2.k.0) and (C3.q.0)	$\{\}$
(ii)	(C2) $\Rightarrow$ (C2.x.a)	$\{\}$
(iii)	(C3.x) $\Rightarrow$ (C3.x.a)	$\{\}$
(iv)	(Cn.x.a+b) $\Rightarrow$ (Cn.x.a), for $n \in \{2,3\}$	$\{\}$
(v)	(C2.x.a) and (C2.x.b) $\Rightarrow$ (C2.x.a+b)	$\{$(C3.x)$\}$
(vi)	(C2.x.1) $\Rightarrow$ (C2)	$\{$(C3)$\}$ ∎

So, diverting from uninformative rules such as (i), we see that some forms of cut imply others (see (ii) and (iv)), and the same holds for weakening (see (iii) and (iv)). Cautious cut is in fact equivalent to full cut in the presence of weakening (see (v) and (vi)).

Some further important properties of general consequence relations are:

(C4) Compactness: left compactness plus right compactness

(C4.1) Left compactness: for any Γ and Δ such that $(\Gamma \Vdash \Delta)$ there is some finite $\Gamma' \subseteq \Gamma$ such that $(\Gamma' \Vdash \Delta)$

(C4.2) Right compactness: for any Γ and Δ such that $(\Gamma \Vdash \Delta)$ there is some finite $\Delta' \subseteq \Delta$ such that $(\Gamma \Vdash \Delta')$

(C5) Structurality: for any endomorphism $*$, $(\Gamma \Vdash \Delta)$ implies $(\Gamma^* \Vdash \Delta^*)$

Compactness is usually invoked, for instance, to guarantee the finitary character of proofs, and is often equivalent to the axiom of choice in model theory. Typical examples of consequence relations failing compactness are those of higher-order logics. Structurality is the rule that allows for uniform substitutions to preserve entailment. Still some other rules, such as those regulating left- and right-contractions, expansions and permutation will in the present framework come for free, given that I have chosen to express inferences using only *sets* —when the repetition of formulas or their order becomes important, as in the case of linear logics or in categorial grammar, it is convenient to upgrade the previous definitions so as to deal with multi-sets or ordered sets of contexts.

Not all the consequence relations which respect some or even all the above properties are decent and worth of being studied. A particularly striking way of being uninteresting and uninformative occurs when the nature of the formulas of the contexts involved in an inference does not really matter, but only the cardinality of the contexts is determinant of the validity of the inference involving them. Consider thus the following kind of property:

(C0.I.J) I.J-overcompleteness: $(\Gamma, [\alpha]_{i\leq \mathrm{I}} \Vdash [\beta]_{j\leq \mathrm{J}}, \Delta)$

0.0-overcompleteness says that whatever set of alternatives follows from whatever set of premises. This is clearly not a very attractive situation, as it ceases to draw a difference between inferences. Everything is permitted —one might call this 'Dostoyevski's God-is-dead situation'. But some other instances of overcompleteness may be worth looking at. If you fix a particular sequence of alternatives $[\beta_j]_{j\leq \mathrm{J}}$, you might call it an I.J-*alternative* if for some cardinal I and any contexts Γ and Δ one has that $(\Gamma, [\alpha]_{i\leq \mathrm{I}} \Vdash [\beta]_{j\leq \mathrm{J}}, \Delta)$ holds; call it simply a J-*alternative* if it is an I.J-alternative for any I. Similarly, if you fix a particular sequence of premises $[\alpha_i]_{i\leq \mathrm{I}}$, you might call it I.J-*trivializing* if $(\Gamma, [\alpha]_{i\leq \mathrm{I}} \Vdash [\beta]_{j\leq \mathrm{J}}, \Delta)$ holds for some cardinal J and any contexts Γ and Δ; call it simply I-*trivializing* if it is an I.J-alternative for any J. A particularly interesting case here is that of *finitely trivializing* theories, i.e. those theories which are I-trivializing for some finite I. Of course, if at least overlap holds then the whole set of formulas is both 1-trivializing and a 1-alternative theory. Note, for instance, that the difference between a 1.1-alternative and a 0.1-alternative is only very slight: It is the distinction, if it makes any sense to say that there is any, between a formula being a consequence of *anything* or of *whatever* (in Latin, *quocumque* versus *qualiscumque*). A similar observation can be made about 1.1- and 1.0-trivializing theories.[3] Any formula φ will be called a *top particle*, or simply a *thesis*,[4]

[3]But the distinction becomes ineffable once you start using single-conclusion instead of multiple-conclusion consequence relations (cf. [27]).

[4]The theses of a given logic are sometimes called its *logical truths*, in the manner of Quine. Some authors would prefer, though, to call logical truths the formulas which are

whenever it is a 0-alternative, and will be called a *bottom particle*, or an *antithesis*, whenever it is 0-trivializing.

Note that:

Fact 1.3 By definition:

(i) Any formula of a 0.1-overcomplete logic is a top particle;
(ii) any formula of a 1.0-overcomplete logic is a bottom particle;
(iii) any logic respecting weak cut and having a formula which is both a top and a bottom particle is 0.0-overcomplete;
(iv) any overcomplete logic is tarskian.

Moreover:

(v)	(C0.I.J) $\Leftrightarrow$ (C0.I+K.J+L), for $\mathrm{I}, \mathrm{J} > 0$	{}
(vi)	(C0.0.0) $\Rightarrow$ (C0.I.J)	{}
(vii)	(C0.0.1) $\Rightarrow$ (C0.0.0)	{bottom and (C2.k.j)}
(viii)	(C0.1.0) $\Rightarrow$ (C0.0.0)	{top and (C2.k.j)} ■

From the above we see that all varieties of overcompleteness reduce thus to one among 0.0-, 0.1-, 1.0- and 1.1-overcompleteness. From the point of view of the standard model-theoretic account, 0.1-overcomplete logics can be characterized by a unique model in which everything is true; similarly for 1.0-overcomplete logics and models in which everything is false. The empty set of valuations, with no truth-values, provide an adequate semantics for 0.0-overcomplete logics, and for 1.1-overcomplete logics you might combine two valuation mappings: One which makes all formulas true, and another one which makes them all false. From this point on, I will be calling a logic *dadaistic* in case it is 0.1-overcomplete, *nihilistic* in case it is 1.0-overcomplete, *trivial* in case it is 0.0-overcomplete, and *semitrivial* in case it is I.J-overcomplete for any $\mathrm{I}, \mathrm{J} > 0$.

As we have seen, the four above kinds of overcompleteness collapse into triviality in case weak cut is respected and there are bottoms and tops around. A cheaper way of producing that collapse is by assuming the following properties on consequence relations (extending the proposal in [22]):

(CG) Coherence: left coherence plus right coherence
(CG.1) Left coherence: $(\Gamma \Vdash \beta, \Delta) \;\Leftrightarrow\; (\forall\alpha)(\Gamma, \alpha \Vdash \beta, \Delta)$
(CG.2) Right coherence: $(\Gamma, \alpha \Vdash \Delta) \;\Leftrightarrow\; (\forall\beta)(\Gamma, \alpha \Vdash \beta, \Delta)$

Although the above properties are clearly admissible in most usual logics, they are also considerably esoteric, and we will not assume them at any point in this paper.

A warning: From this point on, unless otherwise stated, all the above sorts of overcompleteness shall explicitly be avoided.

proved under empty contexts (but not necessarily under all other contexts, what makes a difference if your logic is non-monotonic). This terminology is not at issue here —I shall rather, in general, just take *invariance* under contexts for granted and assume these definitional matters to be largely *conventional*, in the manner of Carnap.

2. Pure rules for negation

Sameness leaves us in peace, but it is contradiction that makes us productive.
—Johann Wolfgang Von Goethe, *Conversations with Eckermann*, March 28, 1827.

Let us now consider some general *pure* sub-classical properties of negation — in the sense that their statement does not involve other logical constants but negation— which often appear in the literature (some of them known since medieval or even ancient times). Be aware that, even though I will be in what follows presenting positive contextual (and, later on, negative contextual) schematic rules for negation and then studying their interrelations in the next FACTs by way of local or global schematic tautologies, lack of space will prevent me from analyzing in this paper the (usually weaker) global versions of the same contextual rules hereby presented, in spite of their possible interest.

For each choice of *levels* $\mathrm{m}, \mathrm{n} \in \mathbb{N}$, consider the rules:

(1.1.m)	$(\Gamma, {\sim}^{\mathrm{m}}\alpha, {\sim}^{\mathrm{m}+1}\alpha \Vdash \Delta)$ *pseudo-scotus*, or *explosion*	(2.1.n)	$(\Gamma \Vdash {\sim}^{\mathrm{n}+1}\beta, {\sim}^{\mathrm{n}}\beta, \Delta)$ *casus judicans*, or *implosion*, or *excluded middle*
(1.1.m.n)	$(\Gamma, {\sim}^{\mathrm{m}}\alpha, {\sim}^{\mathrm{m}+1}\alpha \Vdash {\sim}^{\mathrm{n}}\beta, \Delta)$ *ex contradictione sequitur quodlibet*	(2.1.n.m)	$(\Gamma, {\sim}^{\mathrm{m}}\alpha \Vdash {\sim}^{\mathrm{n}+1}\beta, {\sim}^{\mathrm{n}}\beta, \Delta)$ *quodlibet sequitur ad casos*

Rules of the form (1.1.m) postulate the existence of special kinds of 2-trivializing theories, those containing both a formula and its negation; rules (2.1.n) do the same for some similar 2-alternatives. From the simple schematic character of the rules, it is obvious that (1.1.m.n) follows from (1.1.m), and (2.1.n.m) follows from (2.1.n) —the latter are, in fact, *ex/ad nihil* forms of the former. The converses, however, are usually not that immediate, as one can conclude from FACT 1.3(vii) and (viii). One form of the rules in the family (1.X) or another have been in vogue since at least the XIV century, where they could indeed be found in the work of John of Cornwall (the 'Pseudo-Duns Scotus'), commenting on Aristotle's *Prior Analytics*. An emphasis on the validity of all forms of *casus judicans*, as regulating the so-called 'Principle of Excluded Middle' was strongly advocated already by stoics like Chrysippus, in which they would early be opposed, with equal strength, by Epicurus and, more modernly, by Brouwer. The validity of all forms of its dual rule, *pseudo-scotus*, regulates the so-called 'Principle of Explosion.' Accordingly, the rules in family (1.X) will be related to the metatheoretical notion of 'consistency,' and those in family (2.X) will be related to '(model-)completeness,' or 'determinedness.'

From the point of view of the standard model-theoretic account, (1.1.m) will make sure that no formula (of the form ${\sim}^{\mathrm{m}}\alpha$) can ever be true together with its negation; (1.1.m.0) will guarantee that any model for ${\sim}^{\mathrm{m}}\alpha$ and its negation will be dadaistic. A dual remark can be made about (2.1.n), (2.1.n.0), formulas being false together with their negations, and nihilistic models.

The attentive and well-informed reader will have already suspected that general *paraconsistency* has to do with the basic failure of explosion, that is, the failure of rule (1.1.0); dually, general *paracompleteness* has to do with the failure of (2.1.0). Thus, in particular, relevance logics provide examples of paraconsistent logics, and intuitionistic logic is an example of a paracomplete logic. In fact, duality intuitions will guide the statement of most negation rules above and below; sometimes rules from both sides of each dual pair will be well-known from the logico-mathematical praxis, in some other occasions only one of the sides will be really that common, like in the case of (1.1.m.n) —people rarely mention (2.1.n.m) at all. As a matter of fact, it seems that it is only because there is an old tendency to work under the asymmetrical multiple-premise-single-conclusion environments that people even care to look at (1.1.m.n), localizing the issue of (para)consistency over there instead of over (1.1.m). A more detailed discussion of that can be found in [27].

I proceed now to state some other rules which can easily be harvested in the literature:

(1.2.m.↓) $(\Gamma \Vdash {\sim}^{\mathrm{m}}\alpha, \Delta)$ / $(\Gamma, {\sim}^{\mathrm{m}+1}\alpha \Vdash \Delta)$

(1.2.m.↑) $(\Gamma \Vdash {\sim}^{\mathrm{m}+1}\alpha, \Delta)$ / $(\Gamma, {\sim}^{\mathrm{m}}\alpha \Vdash \Delta)$

dextro-levo symmetry of negation

(2.2.n.↓) $(\Gamma, {\sim}^{\mathrm{n}}\beta \Vdash \Delta)$ / $(\Gamma \Vdash {\sim}^{\mathrm{n}+1}\beta, \Delta)$

(2.2.n.↑) $(\Gamma, {\sim}^{\mathrm{n}+1}\beta \Vdash \Delta)$ / $(\Gamma \Vdash {\sim}^{\mathrm{n}}\beta, \Delta)$

levo-dextro symmetry of negation

(1.3.m.↓) $(\Gamma, {\sim}^{\mathrm{m}+1}\alpha \Vdash {\sim}^{\mathrm{m}}\alpha, \Delta)$ / $(\Gamma, {\sim}^{\mathrm{m}+1}\alpha \Vdash \Delta)$

(1.3.m.↑) $(\Gamma, {\sim}^{\mathrm{m}}\alpha \Vdash {\sim}^{\mathrm{m}+1}\alpha, \Delta)$ / $(\Gamma, {\sim}^{\mathrm{m}}\alpha \Vdash \Delta)$

causa mirabilis

(2.3.n.↓) $(\Gamma, {\sim}^{\mathrm{n}}\beta \Vdash {\sim}^{\mathrm{n}+1}\beta, \Delta)$ / $(\Gamma \Vdash {\sim}^{\mathrm{n}+1}\beta, \Delta)$

(2.3.n.↑) $(\Gamma, {\sim}^{\mathrm{n}+1}\beta \Vdash {\sim}^{\mathrm{n}}\beta, \Delta)$ / $(\Gamma \Vdash {\sim}^{\mathrm{n}}\beta, \Delta)$

consequentia mirabilis

According to [29], forms of *consequentia mirabilis* were first applied in modern mathematics by Cardano and Clavius, in the XVI century. A century later, Saccheri adopted them as some of his main tools for doing some early work on non-Euclidean geometry. At about the same period, Huygens, and to some extent also Tacquet, argued that one should refrain from merely 'formal' applications of *consequentia mirabilis* to mathematics, adopting instead the more 'intuitive' forms of *reductio ad absurdum* (cf. [8], and below). But then, results from FACT 2.3 will show that such a move is not without consequences: The latter rule is in general much stronger than the former.

Rules of symmetry, from families (1.2.X) and (2.2.X) (cf. [1]), are quite similar to their analogues in the families (1.3.X) and (2.3.X). They are sometimes used, for instance, in presenting the very definition of negation (cf. [18]) for logics intermediate between intuitionistic and classical logic.

Next, consider the rules:

(1.4.m) $(\Gamma \Vdash {\sim}^{\mathrm{m}}\alpha, \Delta$ and $\Gamma' \Vdash {\sim}^{\mathrm{m}+1}\alpha, \Delta')$ / $(\Gamma', \Gamma \Vdash \Delta, \Delta')$

right-redundancy

(2.4.n) $(\Gamma, {\sim}^{\mathrm{n}}\beta \Vdash \Delta$ and $\Gamma', {\sim}^{\mathrm{n}+1}\beta \Vdash \Delta')$ / $(\Gamma', \Gamma \Vdash \Delta, \Delta')$

left-redundancy, or *proof by cases*

Forms of proof by cases are some of the most ancient and probably the most common rendering of patterns of reasoning by excluded middle in mathematics and philosophy.

(1.5.m.↓.n) $(\Gamma, {\sim}^{n}\beta \Vdash {\sim}^{m}\alpha, \Delta$ and $\Gamma', {\sim}^{n+1}\beta \Vdash {\sim}^{m}\alpha, \Delta')$ / $(\Gamma', \Gamma, {\sim}^{m+1}\alpha \Vdash \Delta, \Delta')$

(1.5.m.↑.n) $(\Gamma, {\sim}^{n}\beta \Vdash {\sim}^{m+1}\alpha, \Delta$ and $\Gamma', {\sim}^{n+1}\beta \Vdash {\sim}^{m+1}\alpha, \Delta')$ / $(\Gamma', \Gamma, {\sim}^{m}\alpha \Vdash \Delta, \Delta')$

reductio ex evidentia

(2.5.n.↓.m) $(\Gamma, {\sim}^{n}\beta \Vdash {\sim}^{m}\alpha, \Delta$ and $\Gamma', {\sim}^{n}\beta \Vdash {\sim}^{m+1}\alpha, \Delta')$ / $(\Gamma', \Gamma \Vdash {\sim}^{n+1}\beta, \Delta, \Delta')$

(2.5.n.↑.m) $(\Gamma, {\sim}^{n+1}\beta \Vdash {\sim}^{m}\alpha, \Delta$ and $\Gamma', {\sim}^{n+1}\beta \Vdash {\sim}^{m+1}\alpha, \Delta')$ / $(\Gamma', \Gamma \Vdash {\sim}^{n}\beta, \Delta, \Delta')$

reductio ad absurdum

One or another form of *reductio ad absurdum* can be found integrating the standard suite of mathematical tools at least since Pythagoras's discovery / invention of irrational numbers —the reduction to absurdity is indeed the gist of methods of indirect proof and of proof by refutation. Zeno of Elea also excelled the use of this rule as applied to argumentation, foreshadowing a sort of dialectical approach to critical thinking which was to become very popular later on. But *reductio* is altogether dispensed by consequence relations such as that of intuitionistic logic (in accordance with results from FACT 2.3), in concert with its general demise of excluded middle.

Continuing, a second set of pure rules for negation which can also be handy and which are often insisted upon are the following —for each choice of levels $\mathrm{a}, \mathrm{b}, \mathrm{c}, \mathrm{d}, \mathrm{e} \in \mathbb{N}$:

(3.1.a.b.c.d) $(\Gamma, {\sim}^{a}\gamma \Vdash {\sim}^{b}\delta, \Delta)$ / $(\Gamma, {\sim}^{a+2c}\gamma \Vdash {\sim}^{b+2d}\delta, \Delta)$

(3.2.a.b.c.d) $(\Gamma, {\sim}^{a+2c}\gamma \Vdash {\sim}^{b}\delta, \Delta)$ / $(\Gamma, {\sim}^{a}\gamma \Vdash {\sim}^{b+2d}\delta, \Delta)$

(3.3.a.b.c.d) $(\Gamma, {\sim}^{a}\gamma \Vdash {\sim}^{b+2d}\delta, \Delta)$ / $(\Gamma, {\sim}^{a+2c}\gamma \Vdash {\sim}^{b}\delta, \Delta)$

(3.4.a.b.c.d) $(\Gamma, {\sim}^{a+2c}\gamma \Vdash {\sim}^{b+2d}\delta, \Delta)$ / $(\Gamma, {\sim}^{a}\gamma \Vdash {\sim}^{b}\delta, \Delta)$

double negation manipulation

(4.1.a.e) $(\Gamma, {\sim}^{a}\gamma \Vdash {\sim}^{a+2e}\gamma, \Delta)$
double negation introduction

(4.2.a.e) $(\Gamma, {\sim}^{a+2e}\gamma \Vdash {\sim}^{a}\gamma, \Delta)$
double negation elimination

(5.1.a.b.c.d) $(\Gamma, {\sim}^{a}\gamma \Vdash {\sim}^{b}\delta, \Delta)$ / $(\Gamma, {\sim}^{b+2d+1}\delta \Vdash {\sim}^{a+2c+1}\gamma, \Delta)$

(5.2.a.b.c.d) $(\Gamma, {\sim}^{a+2c+1}\gamma \Vdash {\sim}^{b}\delta, \Delta)$ / $(\Gamma, {\sim}^{b+2d+1}\delta \Vdash {\sim}^{a}\gamma, \Delta)$

(5.3.a.b.c.d) $(\Gamma, {\sim}^{a}\gamma \Vdash {\sim}^{b+2d+1}\delta, \Delta)$ / $(\Gamma, {\sim}^{b}\delta \Vdash {\sim}^{a+2c+1}\gamma, \Delta)$

(5.4.a.b.c.d) $(\Gamma, {\sim}^{a+2c+1}\gamma \Vdash {\sim}^{b+2d+1}\delta, \Delta)$ / $(\Gamma, {\sim}^{b}\delta \Vdash {\sim}^{a}\gamma, \Delta)$

contextual contraposition

(6.1.a.b.e) $(\Gamma, {\sim}^{a}\gamma \dashv\Vdash {\sim}^{b}\delta, \Delta)$ / $(\Gamma, {\sim}^{a+e}\gamma \dashv\Vdash {\sim}^{b+e}\delta, \Delta)$

(6.2.a.b.e) $(\Gamma, {\sim}^{a+e}\gamma \dashv\Vdash {\sim}^{b}\delta, \Delta)$ / $(\Gamma, {\sim}^{a}\gamma \dashv\Vdash {\sim}^{b+e}\delta, \Delta)$

(6.3.a.b.e) $(\Gamma, {\sim}^{a}\gamma \dashv\Vdash {\sim}^{b+e}\delta, \Delta)$ / $(\Gamma, {\sim}^{a+e}\gamma \dashv\Vdash {\sim}^{b}\delta, \Delta)$

(6.4.a.b.e) $(\Gamma, {\sim}^{a+e}\gamma \dashv\Vdash {\sim}^{b+e}\delta, \Delta)$ / $(\Gamma, {\sim}^{a}\gamma \dashv\Vdash {\sim}^{b}\delta, \Delta)$

contextual replacement (for negation)

The above rules regulate some fixed-point and involutive properties of negation. I should here insist that one ought not to confuse any of the above contextual rules with their (weaker) global versions. Note indeed, by way of an example, that basic forms of global contraposition, or even better, basic forms of global replacement will provide exactly what one needs for a negation to be amenable to a Lindenbaum-Tarski algebraization, and to have an adequate standard modal interpretation. But local forms of contextual contraposition and replacement will often fail for non-classical logics such as paraconsistent logics (see FACT 2.5 below), even though some of those logics will in fact be perfectly algebraizable (cf. [33] and the subsection 3.12 of [17]).

Let me now invite you to have a look at some of the aftereffects and interrelations among the rules introduced just above, to get a taste of how powerful they can be.[5]

Fact 2.1 Some relations that hold among the last set of rules for negation are:

(i)	$(t.u.\mathrm{a}.w.x.Y) \Rightarrow (t.u.\mathrm{a}+\mathrm{b}.w.x.Y)$, for $\mathrm{a}, \mathrm{b} \in \mathbb{N}$	$\{\}$
(ii)	$(t.u.v.\mathrm{a}.x.Y) \Rightarrow (t.u.v.\mathrm{a}+\mathrm{b}.x.Y)$, for $\mathrm{a}, \mathrm{b} \in \mathbb{N}$	$\{\}$
(iii)	$(4.u.\mathrm{a}.w) \Rightarrow (4.u.\mathrm{a}+\mathrm{b}.w)$, for $\mathrm{a}, \mathrm{b} \in \mathbb{N}$	$\{\}$
(iv)	$(3.x.\mathrm{a}.\mathrm{b}.0.0)$	$\{\}$
(v)	$(4.x.0.0) \Leftrightarrow (\mathrm{C1})$	$\{\}$
(vi)	$(6.x.\mathrm{a}.\mathrm{b}.0)$	$\{\}$
(vii)	$(3.x.\mathrm{a}.\mathrm{b}.\mathrm{c}.\mathrm{d}) \Rightarrow (3.x.\mathrm{a}.\mathrm{b}.t \times \mathrm{c}.t \times \mathrm{d})$, for $t > 0$	$\{\}$
(viii)	$(4.x.\mathrm{a}.\mathrm{e}) \Rightarrow (4.x.\mathrm{a}.t \times \mathrm{e})$, for $t > 0$	$\{(\mathrm{C2.k.1})\}$
(ix)	$(6.x.\mathrm{a}.\mathrm{b}.\mathrm{e}) \Rightarrow (6.x.\mathrm{a}.\mathrm{b}.t \times \mathrm{e})$, for $t > 0$	$\{\}$
(x)	$(x.1.\mathrm{a}.\mathrm{a}.\mathrm{f}+u.\mathrm{f}+v) \Rightarrow (4.y.\mathrm{a}+2\mathrm{f}+z.\mathrm{e})$, for $\langle x, u, v, y, z\rangle \in \{\langle 3,0,\mathrm{e},1,0\rangle, \langle 3,\mathrm{e},0,2,0\rangle, \langle 5,\mathrm{e},0,1,1\rangle, \langle 5,0,\mathrm{e},2,1\rangle\}$	$\{(\mathrm{C1})\}$
(xi)	$(x.2.\mathrm{a}.\mathrm{a}+2\mathrm{c}+y.\mathrm{c}.\mathrm{e}) \Rightarrow (4.z.\mathrm{a}.\mathrm{c}+\mathrm{e}+y)$, for $\langle x, y, z\rangle \in \{\langle 3,0,1\rangle, \langle 5,1,2\rangle\}$	$\{(\mathrm{C1})\}$
(xii)	$(x.3.\mathrm{a}+2\mathrm{c}+y.\mathrm{a}.\mathrm{e}.\mathrm{c}) \Rightarrow (4.z.\mathrm{a}.\mathrm{c}+\mathrm{e}+y)$, for $\langle x, y, z\rangle \in \{\langle 3,0,2\rangle, \langle 5,1,1\rangle\}$	$\{(\mathrm{C1})\}$
(xiii)	$(x.4.\mathrm{a}+2r.\mathrm{a}+2s.\mathrm{c}+t.\mathrm{c}+u) \Rightarrow (4.y.\mathrm{a}.\mathrm{e})$, for $\langle x, r, s, t, u, y\rangle \in \{\langle 3,0,\mathrm{e},\mathrm{e},0,1\rangle, \langle 3,\mathrm{e},0,0,\mathrm{e},2\rangle, \langle 5,\mathrm{e},0,0,\mathrm{e},1\rangle, \langle 5,0,\mathrm{e},\mathrm{e},0,2\rangle\}$	$\{(\mathrm{C1})\}$
(xiv)	$(x.4.0.0.\mathrm{f}+r.\mathrm{f}+s)$ and $(4.y.2\mathrm{f}+z.\mathrm{e}) \Rightarrow (\mathrm{C1})$, for $\langle x, r, s, y, z\rangle \in \{\langle 3,0,\mathrm{e},1,0\rangle, \langle 3,\mathrm{e},0,2,0\rangle, \langle 5,0,\mathrm{e},1,1\rangle, \langle 5,\mathrm{e},0,2,1\rangle\}$	$\{\}$
(xv)	$(v.x.\mathrm{a}.\mathrm{b}.\mathrm{e}.\mathrm{e})$ and $(v.y.\mathrm{b}.\mathrm{a}.\mathrm{e}.\mathrm{e}) \Rightarrow (6.z.\mathrm{a}.\mathrm{b}.2\mathrm{e}+w)$, for $\langle v, w\rangle \in \{\langle 3,0\rangle, \langle 5,1\rangle\}$ and $\langle x, y, z\rangle \in \{\langle 1,1,1\rangle, \langle 2,3,2\rangle, \langle 3,2,3\rangle, \langle 4,4,4\rangle\}$	$\{\}$
(xvi)	$(3.x.\mathrm{a}+2\mathrm{e}.\mathrm{b}.\mathrm{c}.\mathrm{d})$ and $(4.z.\mathrm{a}.\mathrm{c}+\mathrm{e}) \Rightarrow (3.y.\mathrm{a}.\mathrm{b}.\mathrm{e}.\mathrm{d})$, for $\langle x, y, z\rangle \in \{\langle 1,2,1\rangle, \langle 2,1,2\rangle, \langle 3,4,1\rangle, \langle 4,3,2\rangle\}$	$\{(\mathrm{C2})\}$, or $\{(\mathrm{C2.k.j})$ and $(\mathrm{C3.1})\}$, or $\{(4.3-z.\mathrm{a}.\mathrm{c}+\mathrm{e})$ and $(\mathrm{C2.k.j})$ and $(\mathrm{C3.1.p})\}$

[5]In the next facts, I do not claim of course to present 'all' the interesting results, and not even the 'best' possible results —in the sense of working always with the weakest premises and deriving the strongest conclusions by way of the feeblest set of assumptions, in the most general way. But I have advanced a great deal polishing the results in that direction, and the reader will see they are indeed not that bad.

(xvii) (3.x.a.b + 2f.c.d) and (4.z.b.d + f) $\Rightarrow$ (3.y.a.b.c.f), $\{$(C2)$\}$, or
for $\langle x, y, z\rangle \in \{\langle 1,3,2\rangle, \langle 2,4,2\rangle, \langle 3,1,1\rangle, \langle 4,2,1\rangle\}$ $\{$(C2.k.j) and (C3.2)$\}$, or
$\{$(4.3 − z.b.d + f) and (C2.k.j) and (C3.2.q)$\}$

(xviii) (3.x.a + 2e.b + 2f.c.d) and (4.w.a.c + e) and (4.z.b.d + f)
$\Rightarrow$ (3.y.a.b.e.f), for $\langle x, y, w, z\rangle \in \{\langle 1,4,1,2\rangle, \langle 4,1,2,1\rangle\}$ $\{$(C2)$\}$, or
$\{$(4.3 − w.a.c + e) and (4.3 − z.b.d + f) and (C2.k.j) and (C3.1.p) and (C3.2.q)$\}$

(xix) (4.x.a.c) and (4.y.b.d) $\Rightarrow$ (3.z.a.b.c.d),
for $\langle x, y, z\rangle \in \{\langle 2,1,1\rangle, \langle 1,1,2\rangle, \langle 2,2,3\rangle, \langle 1,2,4\rangle\}$ $\{$(C2)$\}$, or
$\{$(4.3 − w.a.c) and (4.3 − z.b.d) and (C2.k.j) and (C3.1.p) and (C3.2.q)$\}$

(xx) (5.1.a.b.c.d) and (5.1.b + 2d + 1.a + 2c + 1.f.e) $\Rightarrow$ $\{\}$
(3.1.a.b.c + e + 1.d + f + 1)

(xxi) (5.2.a + 2b + 1.b.e.d) and (5.3.b + 2d + 1.a.f.c) $\Rightarrow$ $\{\}$
(3.2.a.b.c + e + 1.d + f + 1)

(xxii) (5.3.a.b + 2d + 1.c.f) and (5.2.b.a + 2c + 1.d.e) $\Rightarrow$ $\{\}$
(3.3.a.b.c + e + 1.d + f + 1)

(xxiii) (5.4.a + 2c + 1.b + 2d + 1.e.f) and (5.4.b.a.d.c) $\Rightarrow$ $\{\}$
(3.4.a.b.c + e + 1.d + f + 1)

(xxiv) (5.x.a + 2e + 1.b.c.d) and (4.2.a.c + e + 1) $\Rightarrow$ (5.y.a.b.e.d), $\{$(C2)$\}$, or
for $\langle x, y, z\rangle \in \{\langle 1,2,2\rangle, \langle 2,1,1\rangle, \langle 3,4,2\rangle, \langle 4,3,1\rangle\}$ $\{$(C2.k.j) and (C3.z)$\}$, or
$\{$(4.1.a.c + e + 1) and (C2.k.j) and (C3.z.p)$\}$

(xxv) (5.x.a.b + 2f + 1.c.d) and (4.1.b.d + f + 1) $\Rightarrow$ (5.y.a.b.c.f), $\{$(C2)$\}$, or
for $\langle x, y, z\rangle \in \{\langle 1,3,1\rangle, \langle 3,1,2\rangle, \langle 2,4,1\rangle, \langle 4,2,2\rangle\}$ $\{$(C2.k.j) and (C3.z)$\}$, or
$\{$(4.2.b.d + f + 1) and (C2.k.j) and (C3.z.p)$\}$

(xxvi) (5.x.a + 2e + 1.b + 2f + 1.c.d) and $\{$(C2)$\}$, or
(4.1.b.d + f + 1) and (4.2.a.c + e + 1) and $\{$(C2.k.j) and (C3.z.p)$\}$
(4.2.b.d + f + 1) and (4.1.a.c + e + 1) $\Rightarrow$ (5.y.a.b.e.f),
for $x, y \in \{1, 4\}, x \neq y$

(xxvii) (6.x.a.b.e) $\Leftrightarrow$ (6.x.b.a.e), for $x \in \{1, 4\}$ $\{\}$

(xxviii) (6.2.a.b.e) $\Leftrightarrow$ (6.3.b.a.e) $\{\}$

(xxix) (6.2.a.a + e.e) $\Rightarrow$ (4.1.a.e) and (4.2.a.e) $\{$(C1)$\}$

(xxx) (6.x.a + e.b.e) and (4.1.a.e) and (4.2.a.e) $\Rightarrow$ (6.y.a.b.e),
for $x, y \in \{1, 2\}$, $x \neq y$ $\{$(C2.k.j) and (C3.1.p) and (C3.2.q)$\}$

(xxxi) (6.x.a.b + e.e) and (4.1.b.e) and (4.2.b.e) $\Rightarrow$ (6.y.a.b.e),
for $x, y \in \{2, 4\}$, $x \neq y$ $\{$(C2.k.j) and (C3.1.p) and (C3.2.q)$\}$

(xxxii) (6.x.a + e.b + e.e) and (4.1.m.e) and (4.2.m.e) $\Rightarrow$ (6.y.a.b.e),
for $x, y \in \{1, 4\}$, $x \neq y$, $\{$(C2.k.j) and (C3.1.p) and (C3.2.q)$\}$
and m = min(a, b) ∎

Assuming we are talking about 1-simple logics, that is, taking overlap and 1-cautious cut for granted, let me briefly comment on the above FACT: Note that, by schematism (remember last section), less complex rules —those dealing with fewer negations— usually imply more complex ones (see (i)–(iii)); less generous rules —those introducing or eliminating fewer negations— often imply more generous ones (see (vii)–(ix)), and in the most basic cases they sometimes do not tell you much (see (iv)–(vi)); each form of double negation introduction / elimination is implied by some appropriate form of double negation manipulation or contextual contraposition (see (x)–(xiii)) and a similar thing happens with respect to contextual replacement (see (xv)); moreover, some strong forms of the rules for double negation manipulation or contraposition can only hold together with the introduction / elimination rules for double negation in case the underlying consequence relation respects overlap (see (xiv)); contextual replacement alone can also force double negation introduction / elimination rules to hold (see (xxix) and (xxviii)); all forms of double negation manipulation can in fact be deduced from appropriate forms of double negation introduction / elimination (see (xix)); combinations of appropriate forms of contextual contraposition will also immediately yield some forms of double negation manipulation (see (xx)–(xxiii)); some forms of double negation manipulation will even imply others, given convenient rules for double negation introduction / elimination (see (xvi)–(xviii)), and a similar thing will happen with contextual contraposition (see (xxiv)–(xxvi)); some forms of contextual replacement will also imply others, either in general (see (xxvii) and (xxviii)) or in the presence of appropriate forms of double negation introduction / elimination (see (xxx)–(xxxii)).

It is now easy to conclude from the above that there are some rules which are somehow 'more fundamental' than others. For instance:

Illustration 2.2 Here are a few possible choices of rules from which all the other rules from families $(3.X)$, $(4.X)$, $(5.X)$ and $(6.X)$ follow, inside any cautious tarskian logic:

(1) (5.4.0.0.0.1) and (5.4.0.0.1.0)
(2) (5.1.1.1.0.0) and (5.2.0.1.0.0) and (5.3.1.0.0.0)

To check that, use the last FACT. In case (1), parts (i) and (ii) give you schematism, from which you can conclude (5.4.a.b.0.1) and (5.4.a.b.1.0), for any $\mathrm{a}, \mathrm{b} \in \mathbb{N}$. From that you have in particular that (5.4.2.0.0.1) and (5.4.0.2.1.0), thus (4.1.0.1) and (4.2.0.1) are inferred from (xiii). From (iii), (viii) and (v) you can derive all rules from family $(4.X)$. With the help of those rules and (xxvi) all the rules of the form $(5.1.Y)$ and $(5.4.Y)$ ensue, and using (xxiv) and (xxv) you can derive the rest of the family $(5.X)$. The remaining derivations are left to the reader.
In case (2), (4.2.0.1) follows from (5.2.0.1.0.0) by (xi) and (4.1.0.1) follows from (5.3.1.0.0.0) by (xii). From that, (5.1.1.1.0.0), and schematism, (5.4.a.b.c.d) follows, using (xxvi), and we're back to case (1). ■

Another interesting set of results concerning the above rules is presented in what follows.

Fact 2.3 Here are some other relations which can be checked to hold among the above rules for negation:

(i) $(1.X.\mathrm{a}.Y) \Rightarrow (1.X.\mathrm{a}+\mathrm{b}.Y)$, for $\mathrm{a}, \mathrm{b} \neq \downarrow, \uparrow$ $\{\}$

(ii) (1.x.a + b.Y) and (4.1.a.e) $\Rightarrow$ (1.x.a.Y), for e > 0 and $\langle x, y, z\rangle \in \{\langle 1,1,1\rangle, \langle 2,1,2\rangle, \langle 4,2,2\rangle, \langle 5,1,2\rangle\}$ {(C2)}, or {(C2.k.j) and (C3.y) and (C3.z)}, or {(4.2.a.e) and (C2.k.j) and (C3.y.p) and (C3.z.q)}

(iii) (1.x.Y.a + b) and (4.2.a.e) $\Rightarrow$ (1.x.Y.a), for e > 0 and $\langle x, y, z\rangle \in \{\langle 1,2,2\rangle, \langle 5,1,1\rangle\}$ {(C2)}, or {(C2.k.j) and (C3.y) and (C3.z)}, or {(4.1.a.e) and (C2.k.j) and (C3.y.p) and (C3.z.q)}

(iv) (1.3.a + b.$\downarrow$) and (4.1.a.e) $\Rightarrow$ (1.3.a.$\downarrow$), for e > 0 {(4.2.a + 1.e) and (C2)}, or {(4.2.a.e) and (C2.k.j) and (C3.1.p) and (C3.2.q)}

(v) (1.3.a + b.$\uparrow$) and (4.2.a.e) $\Rightarrow$ (1.3.a.$\uparrow$), for e > 0 {(4.1.a.e) and (C2)}, or {(4.1.a.e) and (C2.k.j) and (C3.1.p) and (C3.2.q)}

(vi) (1.1.0.1) and (5.4.0.0.0.0) $\Rightarrow$ (C1) {}

(vii) (1.1.m) $\Rightarrow$ (1.1.m.x) {}

(viii) (1.1.m.0) $\Rightarrow$ (1.1.m) {bottom and (C2.k.j)}

(ix) (1.1.m) $\Rightarrow$ (1.2.m.$\downarrow$) and (1.2.m.$\uparrow$) {(C2)} or {(C2.k.j)+(C3.1)}

(x) (1.2.m.$\downarrow$) or (1.2.m.$\uparrow$) $\Rightarrow$ (1.1.m) {(C1)}

(xi) (1.2.m.x) $\Rightarrow$ (1.3.m.x) {}

(xii) (1.1.m) $\Rightarrow$ (1.3.m.$\downarrow$) and (1.3.m.$\uparrow$) {(C2.k.j)}

(xiii) (1.3.m.$\downarrow$) or (1.3.m.$\uparrow$) $\Rightarrow$ (1.1.m) {(C1)}

(xiv) (1.1.m) $\Rightarrow$ (1.4.m) {(C2)}

(xv) (1.4.m) $\Rightarrow$ (1.x.m.Y), for $x \in \{1, 2, 3\}$ {(C1)}

(xvi) (1.3.m.$\uparrow$) and (2.1.m + 1.m) $\Rightarrow$ (4.1.m.1) {}

(xvii) (1.3.m + 1.$\downarrow$) and (2.1.m.m + 2) $\Rightarrow$ (4.2.m.1) {}

(xviii) (1.5.m.$\uparrow$.m + 1) $\Rightarrow$ (4.1.m.1) {(C1)}

(xix) (1.5.m + 1.$\downarrow$.m) $\Rightarrow$ (4.2.m.1) {(C1)}

(xx) (1.5.m + 1.x.n) and (4.1.m.1) $\Rightarrow$ (1.5.m.y.n), $\langle x, y, z\rangle \in \{\langle\downarrow,\uparrow,1\rangle, \langle\uparrow,\downarrow,2\rangle\}$ {(C2)}, or {(C2.k.j) and (C3.z)}, or {(4.2.m.1) and (C2.k.j) (C3.z.p)}

(xxi) (1.5.m.x.n) and (4.2.m.1) $\Rightarrow$ (1.5.m + 1.y.n), $\langle x, y, z\rangle \in \{\langle\downarrow,\uparrow,2\rangle, \langle\uparrow,\downarrow,1\rangle\}$ {(C2)}, or {(C2.k.j) and (C3.z)}, or {(4.1.m.1) and (C2.k.j) (C3.z.p)}

(xxii) (1.5.m.$\downarrow$.x) or (1.5.m.$\uparrow$.x) $\Rightarrow$ (1.1.m) {(C1)}

(xxiii) (1.5.m.$\uparrow$.n) $\Rightarrow$ (2.1.n.m) {(C1)}

(xxiv) (1.5.m.$\downarrow$.n) $\Rightarrow$ (2.1.n.m + 1) {(C1)}

(xxv) (1.2.m.x) and (2.4.n) $\Rightarrow$ (1.5.m.x.n) {}

(xxvi) (1.3.0.$\uparrow$) and (2.1.0.0) $\Rightarrow$ (C1) {}

(xxvii) (1.3.m.$\uparrow$) and (2.1.n.m) $\Rightarrow$ (1.5.m.$\uparrow$.n) {(C2)}

(xxviii) (1.3.m.$\downarrow$) and (2.1.n.m + 1) $\Rightarrow$ (1.5.m.$\downarrow$.n) {(C2)}

This much for the (1.X)-column. Dual results obtain for the (2.X)-column, if one only uniformly substitutes, in the formulation of the above items, each: (1.X) for (2.X), and vice-versa; (4.1.X) for (4.2.X), and vice-versa; tops for bottoms; (C3.1.X) for (C3.2.X), and vice-versa. ∎

To make things more concrete, if we assume we are talking about simple consequence relations then the non-obvious parts of the previous FACT boil down to something like this: Again, by schematism, less complex rules imply more complex ones (see (i)), but then, in the presence of appropriate forms of double negation introduction elimination, complex rules can on their turn be simplified (see (ii)–(v)); there are always equivalent forms of *pseudo-scotus*, dextro-levo symmetry of negation, *causa mirabilis* and right-redundancy (see (ix)–(xv)); *ex contradictione* is in reality weaker than *pseudo-scotus*[6] (see (vii) and (viii)); ↑-forms and ↓-forms of *reductio ex evidentia* can in fact imply each other if appropriate forms of double negation introduction or elimination are available (see (xx) and (xxi)); moreover, some double negation rules are implied by *reductio* (see (xviii) and (xix)); *reductio ex evidentia* also gives you *pseudo-scotus* (see (xxii)) and some forms of *quodlibet sequitur ad casos* (see (xxiii) and (xxiv)); in fact, you can only count on both 'full consistency' and 'semicompleteness' then you can get *reductio ex evidentia* back (see (xxv), (xxvii) and (xxviii)); no surprise, appropriate forms of *causa mirabilis* and *ad casos* can tell you something about double negation (see (xvi) and (xvii)). Note also, for more general classes of logics, that a consequence relation cannot fail overlap once it respects, for instance, either some basic forms of *causa mirabilis* and *ad casos*, or some forms of *ex contradictione* and contextual contraposition (see (xxvi) and (vi)). This much for the 'consistency' column ($1.X$); dual readings are readily available for the column of 'determinedness,' ($2.X$). Consequently, in case you have a (simple) paraconsistent or paracomplete logic you are bound to lose some forms of symmetry of negation, some of its miraculous and redundancy rules, and some forms of *reductio*.

In a single-conclusion framework, rules such as *pseudo-scotus*, symmetry, proof by cases and *reductio ex evidentia* are not expressible in the way they were here presented —so it will happen, for instance, that *pseudo-scotus* and *ex contradictione* will be indistinguishable. Observe that, if your (multiple-conclusion) consequence relation respects overlap, then the validity of *reductio ad absurdum* implies the validity of *ex contradictione*; differently from the single-conclusion case, though, *pseudo-scotus* can now still fail in such a situation. The attentive reader will have noticed that not everything is completely symmetrical, however, even in the multiple-conclusion framework. For instance:

Illustration 2.4 Inside simple logics:

(1) $(x.5.\mathrm{m}.\uparrow.\mathrm{n}) \Rightarrow (x.5.\mathrm{m}.\downarrow.\mathrm{n})$, for $\mathrm{n} \leq \mathrm{m}+1$

(2) $(x.5.\mathrm{m}.\downarrow.\mathrm{n}) \Rightarrow (x.5.\mathrm{m}+1.\uparrow.\mathrm{n})$, for $\mathrm{n} \leq \mathrm{m}$

To check those assertions, use parts (i) and (xviii)–(xxi) from the last FACT. Moreover, you can now easily check that all rules from families ($1.X$) and ($2.X$) become valid once both (1.5.0.↑.0) and (2.5.0.↑.0) are verified by a simple logic. Another option to generate a basis for all the other rules is to include a top particle together with (1.5.0.↑.0), or else to include a bottom particle together with (2.5.0.↑.0). Notice, at any rate, that one can easily think of a simple logic for which $(x.5.\mathrm{m}.\downarrow.\mathrm{n})$

[6]Recall for instance the semitrivial logic from the last section. That specific 1.1-overcomplete logic respects *ex contradictione* but not *pseudo-scotus*. A more general realization of that phenomenon as applied to non-overcomplete logics was explored in [27].

holds good, for all levels $\mathrm{m}, \mathrm{n} \in \mathbb{N}$, and where tops and bottoms are present, while $(x.5.0.{\uparrow}.0)$ is still not inferable —such is the case, for instance, of intuitionistic logic. ■

In [9], Béziau pointed out an interesting way of correcting the above asymmetry, which runs like this. Recall from section 0 that we have added and have been using symbols for iterated negations, defined in terms of a single negation, $\sim$, by setting $\sim^0\alpha := \alpha$ and $\sim^{\mathrm{n}+1}\alpha := \sim^{\mathrm{n}}\sim\alpha$, for $\mathrm{n} \in \mathbb{N}$. Now, take instead all such symbols $\sim^{\mathrm{n}}$ as primitive symbols, and consider the 'symmetric domain' given by the integers, requiring only the schematic axiom $\sim^{\mathrm{a}+\mathrm{b}}\alpha = \sim^{\mathrm{a}}\sim^{\mathrm{b}}\alpha$, for every $\mathrm{a}, \mathrm{b} \in \mathbb{Z}$, to be respected. Keeping the above rules exactly as they were presented, it is clear that all the FACTs that we proved (or else some slightly modified versions of them) keep provable with this new definition. But now the above pathology cannot obtain, and if $(x.5.\mathrm{m}.{\downarrow}.\mathrm{n})$ holds good, for a given simple logic and any given levels $\mathrm{m}, \mathrm{n} \in \mathbb{Z}$, then $(x.5.\mathrm{m}.{\uparrow}.\mathrm{n})$ will also hold good, as a consequence, for all $\mathrm{m}, \mathrm{n} \in \mathbb{Z}$. So far, so well. The author of [9], however, after using this symmetrization on the content of the above ILLUSTRATION to suggest that, in a symmetric domain, the differences between classical and intuitionistic negation will vanish, also proceeded to use particular cases of the derivations in FACT 2.3 in order to point some forms of the above rules from which all the other rules would be derived. More specifically, in the single-conclusion environment that he works in, he points out that the validity of *reductio ad absurdum* in a simple logic will be enough to allow for the derivation of all the other rules for negation. But, as we have seen above, in case we use a multiple-conclusion environment and there is no bottom present in the language of the logic, one might quite well have all forms of *reductio ad absurdum* holding good while *pseudo-scotus* still fails; in case there is no top in the logic, all forms of *reductio ex evidentia* might be available and still *casus judicans* might fail. (It does not really help to point out that canonical sequent-style presentations of intuitionistic logic are single-conclusion. Multiple-conclusioned presentations for that same logic have been known since long —check [35], for instance.) So, to be sure, contrarily to what Béziau asserts, here we see that *reductio ad absurdum* alone does *not* sanction the derivation of all the other rules for negation. One always has to be alert not to let a particular choice of framework fool oneself into deceivingly general conclusions.

Fact 2.5 Some further interesting relations among the two above sets of rules for negation are (let $\mathsf{opt} = \{\langle\downarrow,\downarrow,1\rangle, \langle\uparrow,\downarrow,2\rangle, \langle\downarrow,\uparrow,3\rangle, \langle\uparrow,\uparrow,4\rangle\}$):

(i) $(1.2.\mathrm{a}+r.x)$ and $(1.2.\mathrm{b}+s.y)$ and $(2.2.\mathrm{a}+t.x)$ and $(2.2.\mathrm{b}+u.x) \Rightarrow$ $(3.z.\mathrm{a.b.c.d})$, for $\langle x, y, z\rangle \in \mathsf{opt}$ and $\langle z, r, s, t, u\rangle \in \{\langle 1,1,0,0,1\rangle, \langle 2,0,0,1,1\rangle, \langle 3,1,1,0,0\rangle, \langle 4,0,1,1,0\rangle\}$ $\{\}$

(ii) $(w.2.\mathrm{a}.x)$ and $(3-w.2.\mathrm{a}+1.x) \Rightarrow (4.y.\mathrm{a.e})$, for $w \in \{1,2\}$ $\{(\mathrm{C1})\}$

(iii) (1.2.b.y) and (2.2.a.x) $\Rightarrow$ (5.z.a.b.0.0), for $\langle x, y, z\rangle \in \mathsf{opt}$ $\{\}$
(iv) (1.2.a + 1.x) and (1.2.b.y) and (2.2.a.x) and (2.2.b + 1.y) $\Rightarrow$ (5.z.a.b.c.d), for $\langle x, y, z\rangle \in \mathsf{opt}$ $\{\}$
(v) (1.2.a.x) and (1.2.b.y) and (2.2.a.x) and (2.2.b.y) $\Rightarrow$ (6.z.a.b.w), for $w > 0$ and $\langle x, y, z\rangle \in \mathsf{opt}$ $\{\}$
(vi) (1.2.a + 1.$\uparrow$) and (4.1.a.1) $\Rightarrow$ (1.1.a) $\{\}$
(vii) (1.2.a.$\downarrow$) and (4.2.a.1) $\Rightarrow$ (1.1.a + 1) $\{\}$
(viii) (1.2.a + 2e.x) and (4.1.a.e) $\Rightarrow$ (1.1.a) $\{$(C2.k.j)$\}$
(ix) (1.2.a.x) and (4.2.a.e) $\Rightarrow$ (1.1.a + 2e) $\{$(C2.k.j)$\}$
(x) (5.1.a.b.c.d) and (4.2.b.d) $\Rightarrow$ (1.1.b + 2d.a + 2c + 1) $\{\}$
(xi) (5.2.a.b.c.d) and (4.2.b.d) $\Rightarrow$ (1.1.b + 2d.a) $\{\}$
(xii) (5.3.a.b.c.d) and (4.1.b + 1.d) $\Rightarrow$ (1.1.b.a + 2c + 1) $\{\}$
(xiii) (5.4.a.b.c.d) and (4.1.b + 1.d) $\Rightarrow$ (1.1.b.a) $\{\}$
(xiv) (5.1.a.b.c.d) and (4.1.b + 1.d) $\Rightarrow$ (1.1.b.a + 2c + 1) $\{$(C1) and (C2.k.j)$\}$
(xv) (5.2.a.b.c.d) and (4.1.b + 1.d) $\Rightarrow$ (1.1.b.a) $\{$(C1) and (C2.k.j)$\}$
(xvi) (5.3.a.b.c.d) and (4.2.b.d) $\Rightarrow$ (1.1.b + 2d.a + 2c + 1) $\{$(C1) and (C2.k.j)$\}$
(xvii) (5.4.a.b.c.d) and (4.2.b.d) $\Rightarrow$ (1.1.b + 2d.a) $\{$(C1) and (C2.k.j)$\}$
(xviii) (C0.0.1) $\Rightarrow$ ($x.y.Z$), for $x \in \{2, 3, 4, 5, 6\}$ and $x.y \neq 2.4$ $\{\}$
(xix) (C0.0.1) $\Rightarrow$ ($x.y.Z$), for $x.y = 2.4$ $\{$(C2)$\}$
(xx) (C0.0.0) $\Rightarrow$ (Z) $\{\}$

Dual results hold if one uniformly substitutes, in the above items, each: (1.X) for (2.X), and vice-versa; (4.1.X) for (4.2.X), and vice-versa; (3.z.b.a.X) for (3.z.a.b.X); (5.z.b.a.d.c) for (5.z.a.b.c.d); (3.2.X) for (3.3.X), and vice-versa; (5.2.X) for (5.3.X), and vice-versa; (C0.1.0) for (C0.0.1). ■

So, at least as far as simple logics are concerned, one sees that appropriate forms of symmetry rules from the consistency and the completeness families together are enough to imply each rule from the second bunch of rules, that is, those rules involving double negation, contraposition or contextual replacement (see (i)–(v), and recall also FACT 2.1(xix)); furthermore, in the presence of appropriate forms of double negation introduction / elimination, one sees how symmetry rules imply *pseudo-scotus* and *casus judicans*, and how contextual contraposition rules imply *ex contradictione* and *ad casos* (see (vi)–(ix) and (x)–(xvii)). Finally, note that overcompleteness might give you the positive properties for free (see (xviii)–(xx)). As a particularly interesting base for deriving all the other rules, one might consider:

Illustration 2.6 Inside any logic respecting overlap (rule (C1)), all the rules from families (1.X)–(6.X) follow from the validity of basic rules such as (1.4.0) together with (2.4.0).

To check that all rules from families (1.X) and (2.X) follow from (1.4.0) and (2.4.0), recall parts (xv), (vii), (xxv) and (i) of FACT 2.3. For the remaining rules, use parts (ii)–(v) of FACT 2.5, together with parts (iv)–(vi) and (xx)–(xxiii) of FACT 2.1. ■

We might now reasonably ask ourselves: Have we not been too permissive? Is there anything *in common*, after all, among 'all negations'? I have prudently not said a word about that matter this far. More interesting for me was to note the consequences of each set of rules assumed to hold at each given moment. For instance, taking FACT 2.3 into consideration, if you are talking about a simple paraconsistent logic, then you should first allow for inconsistent models, thus you cannot expect any of the rules of the form $(1.x.0.Y)$ to be valid —except perhaps for *ex contradictione*, and this only in case there is no bottom particle present in your logic. Now, if *ex contradictione* is also not valid, as it is usually the case, then *reductio ad absurdum* must also fail. Moreover, taking FACT 2.1 into consideration, if your logic also lacks some form of double negation introduction / elimination, then not all forms of contextual contraposition will be interderivable, and not all forms of contextual replacement will be interderivable; in fact, some forms of contextual contraposition and of contextual replacement will be simply *prevented* from holding. Finally, taking FACT 2.5 into consideration, any double negation manipulation, contextual contraposition or contextual replacement rule that might be lacking will cause a failure of symmetry, and your simple logic might end up being either paraconsistent or paracomplete, in the presence of appropriate forms of double negation introduction / elimination; the failure of *pseudo-scotus* at given levels is incompatible with both symmetry and double negation rules at related levels; the failure of *ex contradictione* will condemn either some form of contextual contraposition or of double negation, and so forth. Dual results hold for paracomplete logics and undetermined models.

All that said and done, it might come as no surprise the acknowledgement that some of the few things which are common to all negations in the literature are not 'positive properties,' but 'negative' ones. In fact, it is not that they *have* something in common, but that they *lack* some things in unison. Consider the following set of negative rules, for each level $\mathrm{a} \in \mathbb{N}$:

(7.1.a)	$(\Gamma, {\sim}^{\mathrm{a}+1}\varphi \not\Vdash \Delta)$ nonbot	(8.1.a)	$(\Gamma \not\Vdash {\sim}^{\mathrm{a}+1}\varphi, \Delta)$ nontop
(7.2.a)	$(\Gamma, {\sim}^{\mathrm{a}+1}\varphi \not\Vdash {\sim}^{\mathrm{a}}\varphi, \Delta)$ *verificatio*	(8.2.a)	$(\Gamma, {\sim}^{\mathrm{a}}\varphi \not\Vdash {\sim}^{\mathrm{a}+1}\varphi, \Delta)$ *falsificatio*

Of course, I continue to consider above only sub-classical properties of negation: The negative rules stated above are rules which *can* hold in classical logic for some particular choice of contexts and of (negated) formulas, but that should *not*, I contend, hold in general for an object we intend to call 'negation.'[7]

[7] Note that I did *not* at any point require —and I will not require— that logics should have any theses / theorems / tautologies / top particles, as much as I also did not require at any point that logics should have any antitheses. Important logics such as Kleene's 3-valued logic have no theses at all. In particular, I surely did not require that logics should have *negated* theses, that is, theses of the form ${\sim}\alpha$. An example of paraconsistent logic extending positive classical logic by the addition of (2.1.0) and (4.2.0.1) and which can be proven to have no negated theses nor bottom particles is the logic studied under the name C_{min} in [16].

From a semantic point of view, (7.1.a) makes sure that our negation is not an operator which produces only bottom particles, and (8.1.a) poses a similar restriction on operators which produce only top particles —these could be held as some sort of very basic requirements for a decent version of this logical constant. Now, a decent negation operator should also embody some reasonable notion of 'opposition': Accordingly, (7.2.a) requires that the negation of some formula can be true while that formula itself is false, and (8.2.a) requires, dually, that some true sentence should have a false negation —thus, no extreme case will be allowed in which all models are dadaistic (that is, thoroughly inconsistent) or nihilistic (that is, thoroughly undetermined). In particular, any of those last two rules preclude identity as an interpretation of negation. This negative axiomatic outlook seems rare, but, I submit, is not really that controversial —in fact, I am unaware of *any* connective which has been seriously proposed intending to represent some sort of 'negation' and that does not respect all the above negative rules. Some interesting results involving the last set of rules follow:

Fact 2.7 Some further interesting relations among the three above sets of rules for negation are:

(i) $\neg$(7.1.a) $\Rightarrow$ (1.1.a) {}
(ii) $\neg$(7.1.a) $\Rightarrow$ (4.x.a + 1.e) {}
(iii) (7.x.a + b) $\Rightarrow$ (7.x.a) {}
(iv) (7.1.a) and (4.1.a + 1.e) $\Rightarrow$ (7.1.a + b), for e > 0 {(C2)}, or {(C2.k.j) and (C3.1)}, or {(4.2.a + 1.e) and (C2.k.j) and (C3.1.p)}
(v) (7.2.a) and (3.4.a + 1.a.e.e) $\Rightarrow$ (7.2.a + b), for e > 0 {}
(vi) (7.1.a) and (1.3.a.$\downarrow$) $\Rightarrow$ (7.2.a) {}
(vii) (7.2.a) $\Rightarrow$ (7.1.a) {}
(viii) (7.1.a + 1) and (1.2.a + 1.$\downarrow$) $\Rightarrow$ (8.1.a) {}
(ix) (7.1.a) and (1.2.a + 1.$\uparrow$) $\Rightarrow$ (8.1.a + 1) {}
(x) (7.2.a + 1) $\Rightarrow$ (8.1.a) {}
(xi) (7.1.a) and (1.3.a + 1.$\uparrow$) $\Rightarrow$ (8.2.a + 1) {}
(xii) (7.2.a) and (5.4.a.a + 1.e.e) $\Rightarrow$ (8.2.a + 2e + 1) {}
(xiii) (C0.0.1) $\Rightarrow$ $\neg$(8.x.y) {}
(xiv) (2.1.0) and $\neg$(7.x.0) $\Rightarrow$ (C0.0.1) {(C2.k.j)}

Dual results hold if one uniformly substitutes, in the above items, each: (1.X) for (2.X), and vice-versa; (7.X) for (8.X), and vice-versa; (4.1.X) for (4.2.X), and vice-versa; (x.4.a.a + 1.e.e) for (x.4.a + 1.a.e.e), and vice-versa; (C3.2.q) for (C3.1.p); (C0.1.0) for (C0.0.1). ∎

So we see that: If a logic disrespects nonbot then it cannot fail *pseudo-scotus* nor double negation elimination (see (i) and (ii)); this time more complex negative rules imply simpler ones by schematism (see (iii)), the converses being true in some special cases, in an appropriate logical environment, given some appropriate form of double negation introduction / elimination or some form of double negation manipulation (see (iv) and (v)); nonbot implies *verificatio* in the presence of *causa mirabilis*, while the converse is always true in virtue of schematism (see (vi), (vii) and (iii)); nonbot implies an appropriate form of nontop in the presence of an appropriate form of dextro-levo-symmetry (see (viii) and (ix)); *verificatio* always implies nontop in virtue of schematism (see (x)); *falsificatio* is implied by nonbot by way of an appropriate form of *causa mirabilis*, and is implied by *verificatio* in the presence of a conveniently strong form of contraposition (see (xi) and (xii)). This much if we put the family $(7.X)$ at the side of the premises; dual readings can be effected if we now put the family $(8.X)$ there. Notice also that, on the one hand, basic forms of overcompleteness imply the failure of the rules from the last two families (see (xiii)) and, on the other hand, a failure of any of the most basic forms of the last given rules occasions overcompleteness in the appropriate positive environment (see (xiv)) —or, to put it differently, non-overcompleteness together with determinedness might imply *verificatio*, together with consistency it might imply *falsificatio*.

One can conclude from this last fact that no paraconsistent logic can disrespect nonbot (and a similar restriction applies to logics without double negation introduction / elimination); on the other hand, if you fix a logic which respects weak cut, any explosive negation in it had better respect nontop, or else it can occasion overcompleteness. Moreover, if a logic respects *verificatio* then it automatically respects nonbot as well, and similarly for *falsificatio* and nontop; besides, in the presence of appropriate forms of levo-dextro-symmetry of negation, nonbot implies nontop. If a logic respects some of the above negative rules, then we are safeguarded against the most basic forms of overcompleteness. Non-overcomplete logics respecting weak cut and some of the above positive rules will also often respect some of the above negative rules, but a logic can respect all the given negative rules and yet respect none of the given positive rules (ok, I concede: This would be quite weak of a 'negation' —but check the next sections). Dual results can easily be checked for paracomplete logics.

Another pure negative rule which might occur to the reader at this point is the following:

(9.a) $\neg(\Gamma, \sim^{\mathrm{a}}\varphi \dashv\Vdash \sim^{\mathrm{a}+1}\varphi, \Delta)$
paradoxical inequivalence

Many set-theoretical paradoxes end up by sanctioning a paradoxical inference which fails some form of (9.a), rather than directly proving a pair of contradictory formulas. But the failure of (9.a) means the failure of both of the corresponding rules (7.2.a) and (8.2.a), and from that it follows, using the last FACT, that those failures leave us standing a very short step from some form of overcompleteness.

3. Causes and consequences for paranormal logics

It is contrary to common sense to entertain apprehensions or terrors upon account of any opinion whatsoever, or to imagine that we run any risk hereafter, by the freest use of our reason. Such a sentiment implies both an absurdity and an inconsistency.
—David Hume, *Dialogues Concerning Natural Religion*, 1779.

As I see it, a natural continuation of the last section should include an analysis of the consequences of the 'paraconsistent attitude,' that is, a brief list of properties enjoyed or avoided by logics for which the positive rule (1.1.0) fails, in the light of all previous FACTs. Calculating this is a purely mechanical task, so this section will only provide some ILLUSTRATIONs of such calculations, instead of trying the reader's patience with further lengthy enumeration of facile results.

To make things even more interesting, I will in fact start by quickly showing how the present environment can help in the specification of some interesting specializations of the notion of paraconsistency (see [17]). Recall that in paraconsistent logics the rule $(\Gamma, \alpha, {\sim}\alpha \Vdash \Delta)$ does not hold in general, that is, it is not valid for some choice of contexts Γ and Δ and some formula α. Of course, the rule *does* hold, for instance, in case either α or ${\sim}\alpha$ are bottom particles. Now, suppose there is some formula $\varphi\langle[\varphi_i]_{i\in\mathrm{I}}\rangle$ of a special format such that neither φ nor ${\sim}\varphi$ are bottom particles for all choices of components $[\varphi_i]_{i\in\mathrm{I}}$, but such that the rule $(\Gamma, \varphi, {\sim}\varphi \Vdash \Delta)$ always holds. In that case the logic will be said to be *controllably explosive* (*in contact with* φ). Explosive logics are those which are controllably explosive in contact with any formula φ to which the definition applies, and controllably explosive logics are always non-1.0-overcomplete, by definition. Paraconsistent logics cannot be explosive, but they *can* be controllably explosive, and they often are. Consider for instance the case of a logic in which (1.1.m) fails only for some $\mathrm{m} < \mathrm{a}$, where $\mathrm{m}, \mathrm{a} \in \mathbb{N}$, and suppose that (7.1.a) holds good —this logic will obviously be paraconsistent yet controllably explosive in contact with ${\sim}^{\mathrm{a}}\alpha$. An example of logic with that property is given by the 3-valued maximal paraconsistent logic P^1, studied in [31]. Dual definitions can easily be offered for paracompleteness and *controllable implosion*. Next, remember that the failure of the rule $(\Gamma, \alpha, {\sim}\alpha \Vdash \beta, \Delta)$ is equivalent to the failure of the rule $(\Gamma, \alpha, {\sim}\alpha \Vdash \Delta)$ in the presence of a bottom particle and (C2.k.j). Of course, $(\Gamma, \alpha, {\sim}\alpha \Vdash \beta, \Delta)$ *does* hold, for instance, in case β is a top particle. Suppose then that $\varphi\langle[\varphi_i]_{i\in\mathrm{I}}\rangle$ is a formula of a special format such that φ is not a top particle for all choices of components $[\varphi_i]_{i\in\mathrm{I}}$, but such that the rule $(\Gamma, \alpha, {\sim}\alpha \Vdash \varphi, \Delta)$ always holds. Logics with that property are called *partially explosive* (*with respect to* φ). Given a theory $\Phi\langle[\varphi_i]_{i\in\mathrm{I}}\rangle$ which happens not to make a J-alternative for every choice of its components, but such that $(\Gamma, \alpha, {\sim}\alpha \Vdash \Phi, \Delta)$ always holds, one may now naturally extend the previous definition so as to call the underlying logic *partially explosive with respect to* Φ. Explosive logics are partially explosive with respect to any formula φ or theory Φ to which the definition applies, and partially explosive logics are always non-0.1-overcomplete, by definition. Paraconsistent logics can be partially explosive with respect to some formulas, but not with respect to all sets of alternatives. Consider the case of a logic having a bottom and such that (1.1.0.n) fails only for some $\mathrm{n} < \mathrm{a}+1$, where $\mathrm{n}, \mathrm{a} \in \mathbb{N}$, and suppose that (8.1.a) holds good —this logic will obviously be paraconsistent yet partially explosive with respect to $\sim^{\mathrm{a}+1}\beta$. Kolmogorov-Johánsson's minimal intuitionistic logic gives an example of a partially explosive paraconsistent logic, since (1.1.0.0) fails in it while (1.1.0.n) holds good for every $\mathrm{n} > 0$. Finally, a logic is called *boldly paraconsistent* in case it is not partially explosive; obviously, boldly paraconsistent logics are, in particular, paraconsistent. Dual definitions can be offered for paracompleteness and both its partial and its bold varieties of implosion. Note that most paraconsistent logics are in practice designed, expected or even required to be boldly paraconsistent (see [34]). Relevance logics, in particular, are always boldly paraconsistent, in virtue of their *variable-sharing property:* Any inference $(\Gamma \Vdash \Delta)$ can only hold good in case Γ and Δ depend on some common atomic sentences. It is not true though that every boldly paraconsistent logic must have the variable-sharing property.

Say that a logic is foo *paranormal* in case it is either foo paraconsistent or foo paracomplete, where foo is one of the above varieties of paraconsistency / paracompleteness. Can we spell out some of the sufficient and some of the necessary conditions for foo paranormality? Surely. Note, for instance, that: From parts (xii) and (xxvi) of FACT 2.3, any logic respecting weak cut and the Principle of Excluded Middle but failing overlap will forcibly be paraconsistent; from parts (x)–(xiii) and (xx)–(xxiii) of FACT 2.1 and parts (x)–(xvii) of FACT 2.5 it follows that contextual contraposition and double negation rules are incompatible with each other, inside any 1-simple boldly paraconsistent logic; from part (i) and the qualification of part (ix) of FACT 2.3 we see that there is no reason to suppose, given a non-monotonic logic, that the failure of dextro-levo-symmetry should be held as a characterizing mark of paraconsistency. And, of course, similar things can always be said and done about the other paranormal class of logics, the paracomplete ones. In the way we have formulated, in the last section, the positive local rules for negation, from families $(1.X)$–$(6.X)$, it turns out that no rule alone has all the others as consequences, given some convenient set of properties of the underlying consequence relation, and, in the same spirit, there is no single rule whose failure causes the failure of all the other rules at once. But, in general, neither the validity nor the failure of a given rule, or set of rules, will be without consequences for some of the other rules. In particular, one could conclude from what has been seen in the above ILLUSTRATIONS and FACTs that all positive rules are inferable, for instance, from *pseudo-scotus*, (1.1.0), and *casus judicans*, (2.1.0), via overlap and cut.

Here are a few other selected causes and consequences of the paraconsistent stance:

Illustration 2.8 Let's look first for some possible *causes* for paraconsistency, that is, some (combinations of) conditions leading to the failure of (1.1.0). The following logics are paraconsistent:

(1) Simple logics respecting all rules from families (2.1.X) to (2.4.X) but failing any other rule from families (1.X) to (6.X).

(2) Logics respecting weak cut and some rule from family (7.X), while failing a rule at the same level from family (8.X) (e.g. respecting (7.1.a) and failing (8.2.a)).

(3) Non-nihilistic logics respecting weak cut and failing basic forms of the rules from family (8.X) (viz. (8.1.0) or (8.2.0)).

Here are some selected *consequences* of paraconsistency, that is, some conditions inferable from the failure of (1.1.0):

(4) If a logic respects overlap, then the basic forms of most rules from family (1.X), namely (1.2.0.x), (1.3.0.x), (1.4.0) and (1.5.0.x.y), will fail. Moreover, some basic forms of contextual contraposition, namely (5.2.0.0.z.0) and (5.4.0.0.z.0), will also automatically fail.

(5) The most basic form of nonbot (viz. (7.1.0)) will always be respected.

(6) The underlying logic will not be nihilistic.

If a logic respects the rules from family (8.X) and is *not* controllably explosive then:

(7) The logic is paraconsistent.

(8) All forms of nonbot are also respected.

Finally, here are a few consequences of *bold* paraconsistency:

(9) *Ex contradictione* will fail alongside with *pseudo-scotus* (and there is no need for a bottom to get that result).

(10) Several other basic forms of contextual contraposition, namely (5.1.0.0.z.0) and (5.3.0.0.z.0), will also fail inside logics respecting overlap. If the logic also respects weak cut, that is, if the logic is simple, then it will in general fail every rule of the form (5.x.y.0.z.0). ∎

As usual, the whole thing is easily dualizable for the paracomplete case.

4. Oh yes, why not?. . . (But then again, what is negation, after all?)

> There are only two means by which men can deal with one another: guns or logic. Force or persuasion. Those who know that they cannot win by means of logic, have always resorted to guns.
> —Ayn Rand, *Faith and Force: Destroyers of the Modern World*, 1960.

The results in the above sections have painfully illustrated the intricate links that tie the several positive contextual sub-classical rules for negation together. You might have noticed that, inside the appropriate logical environment, all positive rules were derivable, for instance, from (1.1.0) and (2.1.0), the most basic forms of *pseudo-scotus* and *casus judicans.* Alternatively, in a similar logical environment, some rules for contextual contraposition were also shown to be sufficient for deriving all the positive rules. Besides, if non-overcompleteness was also guaranteed, then you could also derive the negative rules from the above mentioned positive rules. The requisites for checking each link have also been made clear. You might have noticed, in particular, that full monotonicity had little use in the previous FACTs. Anyway, one of the basic lessons one should draw from the whole thing is that the failure of each positive rule carries forward to the failure of some, but not necessarily all, of the other positive rules.

But there is **more**. I now discuss another, perhaps even more basic lesson, that one should learn from the above. It is easy to run into 'triviality,' in an intuitive sense, if one does not explicitly try to regulate and avoid it. So, 0.0-overcomplete logics respect all the positive rules for negation, but at the same time respect none of the negative rules. Moreover, if an arbitrary logic does not respect (7.1.0) then it will automatically respect explosion, if only for silly reasons, and silliness will also guide you from the failure of (8.1.0) to the failure of implosion. Together with basic *casus judicans*, the failure of either (7.1.0) or (7.2.0) will lead you to a dadaistic logic, and together with basic *pseudo-scotus* the failure of either (8.1.0) or (8.2.0) will lead you to a nihilistic logic. What seems to be the safest thing to do about that? To be sure that you have *some* negative rules about logics and about negation around! This way you can at least avoid both the nonsensical situation of overcompleteness and the uncomfortable situation in which you have a sample of a logical constant —negation— which turns out to lack any real substance.[8]

This connects to the difficult trouble of defining what a logic or a logical constant *is* (or, in this case, what it *is not*). Well, one might complain that this discussion does not lead us anywhere, and that it is very likely that researchers will never reach anything like a general and final agreement about those notions (though they are very likely to keep on trying, perhaps by use of force or by appeal to some argument stemming from some unformalizable consideration about aesthetics or about the ultimate goal of science). Hey, but why should there be an agreement? This is not what we should be striving for! It seems to me that we should rather, as scientists and (meta-)logicians, be quite content in investigating, comparing and argumenting

[8]This approach is in fact an application of a certain metaphysical stance focused in some sort of *accidentalism:* The really 'essential' properties in certain characterizations might in some cases turn out to be the accidental ones —you enumerate the properties which your class of objects *should not* possess from among the ones which are actualizable, and then you have at least some necessary conditions for that class of objects to be 'meaningfully defined.' It is a bit like deciding what you will be when you grow up by listing all the things you do *not* want to be. There is of course no space for better defending this strategy here, from a more abstract point of view, so this had better be left for another occasion.

for and against each possible 'interesting' definition. Then, as the Western Canon says, "by their fruits ye shall know them." Irrespective of religious backgrounds, one might always aspire to find a bit more of impartiality and tolerance around...

Suppose you want to define a class of objects falling under the denomination **D**. If **D** has some common sense meaning(s) in ordinary language, that might give you a good start. You begin by abstracting from that meaning toward some specific direction, but it might happen that you do not want to give neither a purely normative nor a purely descriptive characterization of the **D**-objects. What should you do then? You might say, "Listen, I am only interested in **D**-objects in case they have the positive property bunda." The problem about positive properties is that there will often be some smart guy to come and say, "Now look how interesting is the class of **D**-objects which *do not* have bunda!" What is left of **D** in such a case? Some people say that you cannot negotiate all your positive properties (and our present commitment to negative properties is at least *consistent* with the idea that positive properties are important). For instance, you might define the class of non-monotonic logics as the class of logics given by consequence relations which do *not* have such-and-such property; but then, why should you still think that such consequence relations should still be said to define a *logic*? Fixed a given logic, it might be quite all right that you define a *paraconsistent* negation as the negation which lacks such-and-such property; but then, how can you really be sure in that case that you have a paraconsistent *negation* (cf. [13])? The problem about positive properties is that they can easily mutate from a happy finding into a heavy burden. And, depending on the way you write them down and insist on them, your preferred set of positive properties might easily make you oblivious of other interesting classes of objects which are very much related to your original intuitions about **D**, but remain excluded by your rigid dogmatic definition of it. On the other hand, having positive properties can be very convenient, for you to get a good glimpse of what rests ahead. It is just so easy to work with them.

So, suppose next that we all agree that 'decent' **D**-objects should *not* have the property favela. We might still have an argument as to whether **D**-objects should have bunda or not, as bunda and favela might be but slightly related properties, and turn out to be quite independent from each other. Now, the advantage of such a negative property is that it *does* give you a necessary condition for the objects to fall into an 'decent' compartment of the class **D**. To be sure, there might be trivial examples of **D** around, but now you are at least confident about having avoided some of them. Anyway, it seems hard to you and me to negotiate property favela. What is 'decent' though might not be 'decent enough'! So now we might go on to discuss whether 'decent' **D**-objects should not suffer from the property pipoca, in addition to (or instead of) their not having the property favela. Well, I do have my doubts as to whether we will be able to reach a complete and undisputable set of sufficient conditions for characterizing **D** —we might soon have a debate on the status of the next negative property that we consider: Is the denial of property pipoca 'really innegotiable'? Does it make sense to strive endlessly towards a really 'comprehensive' definition? Anyway, no matter the answer we will give to that, now we have at least agreed in avoiding favela, right?

Which positive properties are the *indisputable* ones, if any? I will not take a stand on that. I do not aim to convince you here of adopting any of the above positive properties about logics or logical constants. Just look at their consequences and make up your own mind about them, in the face of the particular application you might be targeting. Now, I do hope we will agree in avoiding inanity. In that case, take my hand and follow me to a cut-and-dried territory where we will look for '**mi**nimally **d**ecent' versions of our objects of discourse. Note that I will not maintain that what is not minimally decent does not fall under the scope of those definitions, but only that I will not *care* about what is not minimally decent, and I can only hope to convince you that you should also not care about that. Anyway, feel free to disagree and propose and study some other smaller or incomparable set of minimally decent properties, at any point!

I hope you did not get tired with the previous long abstract argumentative digression. Here is the meat. Given some set of formulas, I now proceed to define a **mid**-*consequence relation* as a binary relation over theories (subsets of the initial set of formulas) which is not I.J-overcomplete, for any finite I and J. We get rid thus of trivial, semitrivial, dadaistic and nihilistic logics, besides all other logics suffering from other kinds of finite overcompleteness. One might call this the *Principle of Non-Triviality*, (PNT): "Thou shalt not trivialize!" Newton da Costa has proposed some sort of such principle many decades ago (check [19] and [17]): "From the syntactical-semantical standpoint, every mathematical theory is admissible, unless it is trivial" (notice that he does not say what 'theory' or 'triviality' mean).[9] Interestingly, much more recently, people like Avron, with a completely different background and intentions, have been incorporating some instances of such a principle: In [2] and [4] this author requires consequence relations to be (simple and) non-0.0-overcomplete. People in the paraconsistent logic community working with single-conclusion consequence relations have accordingly interpreted (PNT) as requiring only that a logic should not be 0.1-overcomplete. They have thus explicitly tried to avoid both trivial and dadaistic logics, while they theoretically allowed for semitrivial and nihilistic logics to linger (a further discussion of this can be found in [27]). The above definition of a **mid**-consequence relation, however, clearly extends all the preceding definitions in a natural way —of course, in view of FACT 1.3,

[9] Da Costa dubbed this methodological principle the 'Principle of Tolerance in Mathematics,' by analogy to Carnap's homonymous principle in syntax (check p.52 of [15]).

if the logic has both a bottom and a top particles and respects weak cut, then the present requirement is identical to Avron's.[10] By the way, in view of the same FACT, it is only reasonable to define a **mid**-top as a top particle that is not also a bottom, and a **mid**-bottom as a bottom particle that is not also a top.

Now, for us here a **mid**-*negation* will be any unary operator satisfying the negative properties from families $(7.X)$ and $(8.X)$. Note that this requirement alone safeguards us against 0.0-, 0.1- and 1.0-overcompleteness. In view of the FACTs from the last section, on the one hand, even if a logic respects the above positive properties, nothing guarantees that it will respect the negative ones as well, and that it will escape overcompleteness. On the other hand, if some of the positive properties fail for a given logic, then this logic will often respect some negative properties as well, but not necessarily all of them. So, the safest thing to do seems to be just to strive for a **mid**-negation from the start.

By the bye, if our negative sub-classical properties alone are so weak, as one might complain, how is it that one can arrive from them to a full characterization of classical negation? One possibility is to guarantee, from a semantic perspective, that (7.2.0) and (8.2.0) come together with truth-functionality and two-valuedness. The margins of this paper are however too narrow to contain the truly marvelous demonstration of that proposition.

Ways of nay-saying. Before putting an end to this, let me now make a brief comparison among the present necessary properties of a (**mid**-)negation, and other characterizations which have been recently proposed in the literature (all the following proposals appeared in single-conclusion form, so here I will work with their straightforward reformulations into the multiple-conclusion environment).

In [22], Gabbay proposes a few increasingly complex 'definitions of negation,' based on a couple of necessary and sufficient sets of properties. The idea behind his most sophisticated definition was the following. Suppose you are working with structural tarskian logics. Let $\Theta = \{[\theta_k]_{k\leq \mathrm{K}}\}$ be a non-empty set of 'undesirable results' of 'unwanted sentences' of a logic $\mathcal{L}1 = \langle \mathsf{S}_{\mathcal{L}1}, \Vdash_{\mathcal{L}1}\rangle$, subject to the restriction that Θ should not be a K-trivializing set. Let $\mathcal{L}2 = \langle \mathsf{S}_{\mathcal{L}2}, \Vdash_{\mathcal{L}2}\rangle$ be called a *conservative extension* of $\mathcal{L}1$ if $\Gamma \Vdash_{\mathcal{L}2} \Delta \Leftrightarrow \Gamma \Vdash_{\mathcal{L}1} \Delta$, whenever $\Gamma \cup \Delta \subseteq \mathsf{S}_{\mathcal{L}1}$. Consider next a binary connective $\circledcirc$ such that:

[10] One should notice, though, that the present requirement on non-triviality, which sets all 0.0-, 0.1-, 1.0-, 1.1-overcomplete consequence relations into a class of their own, is exactly the same requirement to be found, later on, in Avron & Lev's [5]. The only methodological difference is that in the last paper the structures corresponding to such relations are somehow "excluded from our [theirs] definition of a *logic*"; in the present paper, instead, they are just said to constitute not 'minimally decent' such relations, but are allowed to stay as 'trivial' (that is, 'degenerate') examples of logics. Do notice also that the entailment relation usually associated to relevance logics, with its characterizing variable-sharing property, automatically respects the present formulation of (PNT).

(G1) $(\alpha \circledcirc \beta \Vdash \alpha)$ and $(\alpha \circledcirc \beta \Vdash \beta)$

(G2) $(\alpha, \beta \Vdash \alpha \circledcirc \beta)$

(G3) $(\gamma \circledcirc \top \dashv\Vdash \gamma)$ and $(\top \circledcirc \gamma \dashv\Vdash \gamma)$, for any top particle $\top$

(G4) $(\alpha \Vdash \beta)$ / $(\alpha \circledcirc \gamma \Vdash \beta \circledcirc \gamma)$ and $(\alpha \Vdash \beta)$ / $(\gamma \circledcirc \alpha \Vdash \gamma \circledcirc \beta)$

Notice that a connective having properties (G1)–(G4) will behave just like a classical conjunction. Now, a connective $\sim$ of $\mathcal{L}1$ is said to be a *negation* if, for some conservative extension $\mathcal{L}2$ of $\mathcal{L}1$ having a connective $\circledcirc$ with properties (G1), (G3) and (G4):

(GB) $(\gamma \Vdash_{\mathcal{L}1} {\sim}\alpha) \Leftrightarrow (\gamma \circledcirc \alpha \Vdash_{\mathcal{L}2} \theta)$, for some $\theta \in \Theta$

For an intuition about that sort of negation, you might understand (GB) as conveying the idea that γ and α are 'in conflict' in the presence of the undesirable sentence θ.

How can one capture the set of unwanted sentences, when it exists? Easy: Just consider the set of all negated 1-alternatives, that is, $\Theta = \{\theta : (\forall\gamma, \Gamma, \Delta)(\Gamma, \gamma \Vdash_{\mathcal{L}1} {\sim}\theta, \Delta)\}$. In case $\mathcal{L}1$ has some top particle, then Θ turns to be more simply the set of all formulas whose negations are theses of this logic. You might recall though from footnote 7 that this already goes much beyond our present general requirements on logics. Let me note in passing a few particular features of the above definition. Suppose that this connective $\circledcirc$ of $\mathcal{L}2$ also respects property (G2), that is, suppose that it behaves like a classical conjunction. Then, by overlap we have that ${\sim}\alpha \Vdash_{\mathcal{L}1} {\sim}\alpha$ and so, by (GB), ${\sim}\alpha \circledcirc \alpha \Vdash_{\mathcal{L}2} \theta$, for some $\theta \in \Theta$. From (G2) and cut, together with the fact that $\mathcal{L}2$ is a conservative extension of $\mathcal{L}1$, one can conclude that $\alpha, {\sim}\alpha \Vdash_{\mathcal{L}1} \theta$. Similarly, from (G1), (G2) and (GB) again, one also concludes that $\alpha \Vdash_{\mathcal{L}1} {\sim}{\sim}\alpha$. Moreover, in this case the underlying logic will be at least partially explosive: $\alpha, {\sim}\alpha \Vdash_{\mathcal{L}1} {\sim}\beta$, for every $\alpha, \beta \in \mathsf{S}_{\mathcal{L}1}$. Obviously, Θ should not contain a top particle, under pain of causing $\mathcal{L}1$ to fail (8.2.0), thus producing a negation that is not **mid**. For similar reasons, $\mathcal{L}1$ should not be 0.1-overcomplete, and we know that it is not 1.0-overcomplete from the very postulated existence of a non-trivializing set Θ. In case $\mathcal{L}1$ counts on some top particle $\top$, and the connective $\circledcirc$ of $\mathcal{L}2$ not only respects properties (G1)–(G4) but it is already expressible in $\mathcal{L}1$, then $\Vdash_{\mathcal{L}1} {\sim}(\alpha \circledcirc {\sim}\alpha)$ (use (G3) to check that). The interested reader will find in [24] an extension of the above definition of negation so as to cover also a class of non-monotonic logics.

In [23], the authors propose a 'simplified version' of (GB). Starting from full classical propositional logic, for each formula α they explicitly introduce the connective ${\sim}_\alpha$ for '*graded negation*,' together with another set of connectives for 'graded tolerance,' in order to axiomatize what they claim to be a conservative extension of classical logic. Next, they require graded negation to respect the following property:

(GH) $(\Gamma \Vdash {\sim}_\alpha\beta) \Leftrightarrow (\Gamma \Vdash \alpha)$ and $(\alpha \wedge \beta \Vdash)$

The idea, again, is that the inference of α from Γ is 'in contention with' β. As the authors claim that "it is becoming more widely acknowledged that we need to develop more sophisticated means for handling inconsistent information," one might be led to think that graded negations are non-explosive. This is surely not the case. Indeed, given overlap and any unary connective $\star$, it is easy to check that both $(\sim_\varphi \star\varphi \Vdash \varphi)$ and $(\varphi \land \star\varphi \Vdash)$ should hold good in their logic. The last inference seems quite puzzling, given that it holds for *any* definable unary connective $\star$ (thus also for identity, and for any negation originally intended to be non-explosive), and $\land$ is classical conjunction. Thus, we finally conclude, in particular, that $(\varphi \Vdash)$. This renders the present 'extension of classical logic' both non-conservative and nihilistic, thus non-paraconsistent —and so the paper seems not really to delivers what it promises. (To go back to single-conclusion consequence relations and write $(\alpha \land \beta \Vdash \gamma)$ instead of $(\alpha \land \beta \Vdash)$ at the right-hand side of (GH) does not help at all: The resulting logic will not be **mid**, being at least semitrivial.) The proposal is glaringly unsound.

Another fascinating investigation of negation was made by Lenzen, in [25]. One can find in that paper a list of 'necessary conditions for negation-operators,' namely (check PROPOSAL 42):

(L1) $(\Gamma, \alpha \nVdash \sim\alpha, \Delta)$

(L2) $(\Gamma, \alpha \Vdash \beta, \Delta) \Rightarrow (\Gamma', \sim\beta \Vdash \sim\alpha, \Delta')$

(L3) $(\Gamma \Vdash \alpha, \Delta) \Rightarrow (\Gamma' \Vdash \sim\sim\alpha, \Delta')$

(L4) If the logic has a top, then $\exists\alpha(\Gamma \Vdash \sim\alpha, \Delta)$

Now, (L1) is simply our own property (8.2.0). Even though the paper by Lenzen aims to give a special account of paraconsistent negations, it seems ungainly not to find in the above list of necessary properties for negation the dual of property (L1) in family (7.X). I cannot say much here about (L2) and (L3) —they are global properties, and I have postponed the discussion of such properties to a future paper. But again, it is a bit strange not to find other versions of (L2) —global contraposition— in the above list, and also not to find the dual verstion of (L3) there. At any rate, for the purposes of algebraization and modalization, (L2) is surely more than one needs (as keenly pointed out in [33]), given that the following version of global replacement is already enough:

(L2*) $(\Gamma, \alpha \dashv\Vdash \beta, \Delta) \Rightarrow (\Gamma', \sim\alpha \dashv\Vdash \sim\beta, \Delta')$

There are, though, an awful amount of interesting logics, algebraizable or not, with known modal interpretations or not, which are supposed to have a 'negation' that respects neither (L2) nor (L2*) (check [17] for many remarkable paraconsistent samples of such logics). As a final remark, in a multiple-conclusion consequence environment, it would of course seem only natural to add and study also the dual of (L4):

(L4$^\mathrm{d}$) If the logic has a bottom, then $\exists\alpha(\Gamma, \sim\alpha \Vdash \Delta)$

Let's leave it as a suggestion for further development.

Here is a last case study. In [11], Béziau aims to propose "a definition of negation not depending on explicit logical laws but on a conceptual idea." To that purpose, the author tries to formulate a semantical constraint which would be such that the following condition (BZ) is respected: Given a set of 'true' (designated) truth-values and a disjoint set of 'false' (undesignated) truth-values, it would always be possible to find models M1 and M2 such that φ and $\circledcirc\varphi$ would not be both true in M1 nor both false in M2, for some symbol '$\circledcirc$' aimed to model 'negation,' as opposed to 'affirmation' (check Fig. 1). Clearly, our rules (7.2.0) and (8.2.0), from the end of section 2, are just what one needs for the job, under a structural tarskian interpretation of semantics, but that's not the path trodden by the author. What he does in that paper, in fact, amounts to the following. Call any true value T and any false value F, and define the natural order among them, that is, set $F \preccurlyeq F$, $F \preccurlyeq T$, and $T \preccurlyeq T$. Next, call a unary operator $\circledcirc$ *positive* in case it is monotonic over $\preccurlyeq$, that is, in case $\S_1(\varphi) \preccurlyeq \S_2(\varphi)$ implies $\S_1(\circledcirc\varphi) \preccurlyeq \S_2(\circledcirc\varphi)$, for any choice of valuations $\S_1$ and $\S_2$. Finally, call $\circledcirc$ *negative* in case it is not positive. Béziau proposes that negative connectives have all the right to be called *negations*. Indeed, the identical operator ($\circledcirc^1_2$ in Fig. 1), for one, is surely not negative. But then, unfortunately, the last definition is not strong enough to get rid of the other forms of affirmation. Mind you, consider the operator $\circledcirc^2_2$ in Fig. 1, and consider valuations $\S_1$ and $\S_2$ and a formula φ such that $\S_1(\varphi) = F = \S_2(\varphi)$, but $\S_1(\circledcirc^2_2\varphi) = T$ while $\S_2(\circledcirc^2_2\varphi) = F$. Those valuations would characterize $\circledcirc^2_2$ as a negative operator, contrary to our expectations, and a similar example can be written with $\circledcirc^3_2$, this time taking $\S_1(\varphi) = T = \S_2(\varphi)$. In neither case can we say that condition (BZ) holds good. The proposal thus is not sound.

	$\circledcirc^3_2$
T	T
T	F
F	F

	$\circledcirc^2_2$
T	T
F	T
F	F

	$\circledcirc^1_2$
T	T
F	F

kinds of affirmation

. . . .

kinds of negation

	$\circledcirc^1_1$
T	F
F	T

	$\circledcirc^2_1$
T	F
F	F
F	T

	$\circledcirc^3_1$
T	F
T	T
F	T

	$\circledcirc^4_1$
T	F
T	T
F	F
F	T

Figure 1: **Affirmation $\times$ negation**

A full stop comes. I will make no further inquiries here into what negation *is* (or what it *is not*). I just wanted to convince you that the connective that is studied in this paper has some right to be *called* 'negation.' My feeling, though, is that a really good theory of 'what negation is' can only come as a byproduct of a more general and modern and comprehensive version of a *theory of oppositions*, as we learned from good ol' Aristotle. My interest here, however, was much more modest: This was rather a study about what negation *could be*, and what it *should not be*.

5. Directions

> 'Would you tell me, please, which way I ought to go from here?'
> 'That depends a good deal on where you want to get to,' said the Cat.
> 'I don't much care where. . . ' said Alice.
> 'Then it doesn't matter which way you go,' said the Cat.
> '. . . so long as I get *somewhere*,' Alice added as an explanation.
> 'Oh, you're sure to do that,' said the Cat, 'if you only walk long enough.'
> —Lewis Carroll, *Alice's Adventures in Wonderland*, 1865.

The present paper aimed at making several different contributions, suggestions, and some forceful yet not always claimed to be original remarks, among which:

-1- An elaborate illustration is given on the general use of *multiple-conclusion* consequence relations in the abstract study of deductive systems and logical connectives. Most studies in abstract (universal) logic, such as those by Béziau, have concentrated on single-conclusion consequence relations, and so have missed a lot of what you can get straightforwardly by considerations of *symmetry*. Other studies of multiple-conclusion consequence relations have usually not been made in a purely *abstract* setting, but more frequently in a *proof-theoretical* setting (as in the case of some excellent papers by Avron) or in a *semantical* setting (as in the case of the excellent book by Shoesmith & Smiley). The present paper should be read, then, as a call for integration.

-2- Many local sub-classical rules for consequence relations and for the negation connective are systematically studied here in multiple-conclusion format, and *negative* rules are given so much emphasis —or even more emphasis— as *positive* ones. In fact, *failing* those negative rules can be much more dangerous than failing the positive rules, as you can check at the end of section 2. Negative rules are argued to be, in a sense, more 'essential' than positive ones. An extensive justification for that argument is presented in the first part of section 4.

-3- Important general approaches to those same rules in the literature (Avron, Béziau, Curry, Gabbay, Hunter, Lenzen, Wansing, etc.) are surveyed, all along the paper. Corrections are made on some proposals and results by Béziau, and a proposal by Gabbay & Hunter is shown to apply only to overcomplete logics (though that flagrant limitation seems to have gone unnoticed up to this moment).

-4- A small yet comprehensive taxonomy of the most well-known classes of consequence relations is presented in section 1.

-5- The prerequisites for proving each fact interrelating rules for consequence relations and rules for negations are in each case clearly highlighted. This is quite useful for you to know at once whether you shall make use, say, of monotonicity (weakening) or of rules for double negation to prove each given relation.

-6- General rules that make consequence relations 'trivial' are presented, generalizing many other distinguished approaches from the literature.

-7- The multiple-conclusion environment allows us to present 'consistency' rules as *dual* to 'completeness' rules, in a clear and compelling way. As a consequence, rules that are duals to *ex contradictione*, *consequentia mirabilis*, proof-by-cases, and *reductio ad absurdum* are here introduced, apparently for the very first time.

-8- The same environment, again, allows one in fact to draw a sharp distinction between *pseudo-scotus* and *ex contradictione sequitur quodlibet*. This is certainly new, as new as the accompanying proposal to draw the very *definition* of paraconsistency as the failure of the former rules instead of the latter, in direct duality to the (most) characterizing feature of (intuitionistic-like) paracomplete systems: the failure of excluded middle.

-9- The definitions of paraconsistency and paracompleteness are precisely stated, and clearly shown not to bear any compulsory effect, for instance, on the invalidation of rules for double negation (and vice-versa). Some definitions of important subclasses of paraconsistent and paracomplete logics (partial, controllable, and bold) are also presented and exemplified, under a new generality and always having symmetry in mind.

-10- Studies of *consequentia mirabilis* (e.g. Pagli & Bellissima) have at times proposed to identify *mirabilis* with *reductio*. This is a historical and a technical abuse, clarified in the present paper.

-11- An illustrative list of sufficient and necessary conditions for (bold) (non-controllable) paraconsistency is presented, in section 3.

-12- Other proposals of characterizations of negation are offered and analyzed in section 4. Proposals by other authors are summarized and criticized. Incidentally, having already been mentioned by other authors, n-ary negations can also in this paper be seriously be taken into consideration (see below, in the present section), as they pretty smoothly fit the general framework.

The present study of negation was made quite general, this far, under the natural liberties and restrictions of the chosen framework and our decision to concentrate on pure local sub-classical rules for negation. The picky reader might observe, though, that even some seemingly innocuous assumptions that we made may turn out disputable, or at least limited from their very inception. Thus, I have assumed from the start, for example, that, in this paper, "a unary *negation* symbol $\sim$ will always be present as a logical constant in the underlying language of our logics." Now, why should negation be *unary*? One might think instead that it is much more natural to think of 'negation as conflict,' as in the second part of section 4. With that idea in mind, consider the following rules for a *binary* negation connective:

(A1.1) $(\Gamma \Vdash \sim(\alpha_1, \alpha_2), \Delta)$ / $(\Gamma, \alpha_1, \alpha_2 \Vdash \Delta)$

(A1.2) $(\Gamma \Vdash \alpha_1, \Delta)$ and $(\Gamma \Vdash \alpha_2, \Delta)$ / $(\Gamma, \sim(\alpha_1, \alpha_2) \Vdash \Delta)$

(A2.1) $(\Gamma, \alpha_1, \alpha_2 \Vdash \Delta)$ / $(\Gamma \Vdash \sim(\alpha_1, \alpha_2), \Delta)$

(A2.2) $(\Gamma, \sim(\alpha_1, \alpha_2) \Vdash \Delta)$ / $(\Gamma \Vdash \alpha_1, \Delta)$ and $(\Gamma \Vdash \alpha_2, \Delta)$

Clearly, a unary negation for a formula α can be defined from the above binary connective by considering $\sim(\alpha, \alpha)$. The rules of the preceding connective are analogous to the rules of NAND, also known as Sheffer stroke, or *alternative denial*. One could also look at the rules of its dual, *joint denial*, also known as NOR:

(J1.1) $(\Gamma \Vdash \sim(\alpha_1, \alpha_2), \Delta)$ / $(\Gamma, \alpha_1 \Vdash \Delta)$ and $(\Gamma, \alpha_2 \Vdash \Delta)$

(J1.2) $(\Gamma \Vdash \alpha_1, \alpha_2, \Delta)$ / $(\Gamma, \sim(\alpha_1, \alpha_2) \Vdash \Delta)$

(J2.1) $(\Gamma, \alpha_1 \Vdash \Delta)$ and $(\Gamma, \alpha_2 \Vdash \Delta)$ / $(\Gamma \Vdash \sim(\alpha_1, \alpha_2), \Delta)$

(J2.2) $(\Gamma, \sim(\alpha_1, \alpha_2) \Vdash \Delta)$ / $(\Gamma \Vdash \alpha_1, \alpha_2, \Delta)$

The above connectives obviously generalize our symmetry rules $(1.2.X)$ and $(2.2.X)$. Exercises for the reader: Check what should be done for generalizing the other positive and negative rules in accordance with the above binary connectives, and check what happens when other n-ary 'negations' are defined, including —don't be lazy— infinitary versions. (By the way, as you have the pencil in hand: I have checked the results in the above sections to exhaustion, but I would not be so surprised if some errors had slipped into the easy but general calculations. Have fun on the search for mistakes! I just hope the whole thing has worked well as an illustration of the idea behind the systematization.)

Finally, I must acknowledge that all of this was but an initial step into the realm of negation. I had better just add a last note of intentions. The reader should not assume that I am defending the pure negative rules from the families $(7.X)$ and $(8.X)$, which I used in the last section in the definition of 'minimally decent negations,' to be THE rules common to all negations. By no means. Not only do I want to leave, on the one hand, also those very rules open to debate, but on the other hand I also think that those rules are not even enough if you are serious about the notion of a decent negation. In fact, in most normal modal logics, operators such as the necessity operator *are* also expected to respect rules from families $(7.X)$ and $(8.X)$. But we surely do not want negation to be interpreted as necessity, or necessity to be read as a kind of negation! So, a 'minimally decent negation' is more likely to be the one that, besides being a **mid**-negation, also respects some *non-local negative* rules such as the following ones:

(G1.1.a)	$\neg[(\Vdash {\sim}^{a}\varphi) \Rightarrow (\Vdash {\sim}^{a+1}\varphi)]$	(G2.1.a)	$\neg[({\sim}^{a}\varphi \Vdash) \Rightarrow ({\sim}^{a+1}\varphi \Vdash)]$
(G1.2.a)	$\neg[(\Gamma \Vdash {\sim}^{a}\varphi, \Delta) \Rightarrow$ $({\sim}^{a+1}\Gamma \Vdash {\sim}^{a+1}\varphi, {\sim}^{a+1}\Delta)]$	(G2.2.a)	$\neg[(\Gamma, {\sim}^{a}\varphi \Vdash \Delta) \Rightarrow$ $({\sim}^{a+1}\Gamma, {\sim}^{a+1}\varphi \Vdash {\sim}^{a+1}\Delta)]$

where ${\sim}^{a}\Sigma$ denotes, as you might expect, $\{{\sim}^{a}\sigma : \sigma \in \Sigma\}$.

A follow-up to the present investigation should include statements of rules mixing negation and other more usual logical constants, such as conjunction, disjunction, implication and bi-implication, always from the point of view of universal logic, and maybe a survey of the effects of paraconsistency also in this terrain —it is well known for instance that some laws of implication might have dreadful consequences for paraconsistency, that rules such as disjunctive syllogism will often fail, that De Morgan laws will not always be convenient, that even *modus ponens* might in some situations be problematic, that adjunctive conjunctions might be dangerous, and so on. The present results will surely be decisive in the future investigation of the mixed rules. It would also be interesting and important, at some moment, to have a good look at global versions of most preceding contextual rules. This discussion also relates to the trouble of algebraization, which should be clarified in detail, and the whole thing will be easily dualizable from paraconsistent to paracomplete logics.

The next step should include the study of some recent contributions to the field: the consistency connective, and its dual completeness (or determinedness) connective, which can help internalizing the homonymic metatheoretical notions at the object language level, recovering through them the inference rules which might be lacking in columns $(1.X)$ and $(2.X)$. Such connectives also allow us to translate and talk about many (sub-)classical properties inside 'gentle' logics which do not enjoy them.

All that and we are still talking, in a sense, about sub-classical properties of negation. By way of closure, a few notes should also be added —without any intention of gauging the full ramifications of the subject in the literature— about some rules for negation which are 'really non-classical': This is the case of MacColl & McCall's connexive negation (depending on how you look at it), Post's cyclic negation, Humberstone's demi-negation, and so on and so forth. This much for the future.

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Ineffable inconsistencies

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Abstract

For any given consistent tarskian logic it is possible to find another non-trivial logic that allows for an inconsistent model yet completely coincides with the initial given logic from the point of view of their associated single-conclusion consequence relations.

A paradox? This short note shows you how to do it.

This can be read as the description of an expedition into unexplored regions of abstract logic, the theory of valuation and paraconsistency.

Keywords: abstract deductive systems, multiple-conclusion logic, theory of valuations, triviality, paraconsistency

1 Inconsistent classical logic

> Plus on voit ce monde, et plus on le voit plein de contradictions et d'inconséquences.
> —Voltaire, *Dictionnaire Philosophique*, XVIII century.

Take your preferred presentation of classical propositional logic. More concretely, take some denumerable set at of atomic sentences and some non-empty functionally complete set of logical constants C. As usual, the set S of classical formulas will be inductively built as the free algebra generated by C over at. Let $\mathcal{V}$ be a set of truth-values, $\mathcal{D} \subseteq \mathcal{V}$ a set of designated values (shades of truth) and $\mathcal{U} \subseteq \mathcal{V}$ a set of undesignated values (shades of falsehood), where $\mathcal{D} \cup \mathcal{U} = \mathcal{V}$ and $\mathcal{D} \cap \mathcal{U} = \varnothing$. Semantically, a classical state of the world will be simulated by an assignment $\mathsf{asg} : \mathsf{at} \to \mathcal{V}$, where both $\mathcal{D}$ and $\mathcal{U}$ are required to be non-empty —usually, they are taken to be singletons, symbolizing 'the true' and 'the false', if you like. Yes, if you have a boolean mind, you will probably be expecting each such assignment asg to be uniquely extendable into a valuation $\S : \mathsf{S} \to \mathcal{V}$, according to the truth-functional interpretation of each connective in C. Indeed, say you are talking about disjunction and negation, $\vee$ and $\sim$. In that case you are probably expecting their semantical interpretations to be induced by the set sem of all valuations $\S : \mathsf{S} \to \mathcal{V}$ such that:

$$\begin{array}{rcl} \S(\alpha \vee \beta) \in \mathcal{D} & \text{iff} & \S(\alpha) \in \mathcal{D} \text{ or } \S(\beta) \in \mathcal{D} \\ \S({\sim}\alpha) \in \mathcal{D} & \text{iff} & \S(\alpha) \in \mathcal{U} \end{array}$$

Because classical logic has a truth-functional semantics and because this semantics was formulated above in order to display the dependence of each complex classical formula on its immediate subformulas, and only on them, each of the §-clauses regulating the set sem counted with an 'iff' and had a very specific format, indicating the similarity between the algebra of classical formulas and the classical (boolean) algebra of truth-values.

The canonical single-conclusion tarskian consequence relation induced by sem, denoted by $\models^{\mathsf{s}}_{\mathsf{sem}} \subseteq \mathsf{Pow}(\mathsf{S}) \times \mathsf{S}$, is defined by:

$$\Gamma \models^{\mathsf{s}}_{\mathsf{sem}} \varphi \;\text{ iff }\; \S(\Gamma) \not\subseteq \mathcal{D} \text{ or } \S(\varphi) \notin \mathcal{U}, \;\text{ for every } \S \in \mathsf{sem},$$

where $\Gamma \cup \{\varphi\} \subseteq \mathsf{S}$.

Now, given any other set of formulas S and any set of truth-values $\mathcal{V}$, one can take sem' as any set of valuations $\S : \mathsf{S} \to \mathcal{V}$, and the definition of $\models^{\mathsf{s}}_{\mathsf{sem}'}$ will still make perfect sense, and it will define the consequence relation of *some* tarskian logic. With that idea in mind, valuation theorists (cf. [5, 4, 3]) come and ask you to simply forget about the structure of the set of truth-values and concentrate on the set of valuations itself, whichever way it might be introduced. And that is precisely what we shall be doing from now on.

Let $\S_d : \mathsf{S} \to \mathcal{V}$ be an arbitrary mapping such that $\S_d(\varphi) \in \mathcal{D}$, for any $\varphi \in \mathsf{S}$. A valuation like this plays the role of an inconsistent model, making everything 'true' at once. Suppose you now build a set sem^d by just adjoining $\S_d$ to the classical set of valuations sem. Is the new associated single-conclusion consequence relation, $\models^{\mathsf{s}}_{\mathsf{sem}^d}$, any different from the original consequence relation from classical logic? Surprising as it might seem, the answer is 'NO'. Indeed, suppose $\Gamma \models^{\mathsf{s}}_{\mathsf{sem}} \varphi$, for some formulas $\Gamma \cup \{\varphi\} \subseteq \mathsf{S}$. In that case, $\S(\varphi) \in \mathcal{D}$ whenever $\S(\Gamma) \subseteq \mathcal{D}$, for any $\S \in \mathsf{sem}$, by definition. This obviously still holds good for $\S_d$. Conversely, suppose $\Gamma \models^{\mathsf{s}}_{\mathsf{sem}^d} \varphi$. Then, $\S(\varphi) \in \mathcal{D}$ whenever $\S(\Gamma) \subseteq \mathcal{D}$, for any $\S \in \mathsf{sem}^d$. So, in particular, this holds good for every $\S \in \mathsf{sem}$.

The literature on paraconsistent logics is prolific on vague definitions of the very phenomenon of paraconsistency, at all levels. It is not without some disquietness that we find in the paraconsistent jungle definitions such as:

- "Paraconsistent logics are non-trivial logics which can accomodate contradictory theories."
- "Paraconsistent logics are non-explosive logics."
- "Paraconsistent logics are logics having some inconsistent models."

From a semantical perspective, all such definitions tend to say, when properly formalized, that the above second version of classical logic is paraconsistent. Yet it is characterized by the very same single-conclusion consequence relation of the first and more usual version of classical logic!

What's wrong, if anything?

2 The general recipe

> My desire and wish is that the things I start with should be so obvious that you wonder why I spend my time stating them. This is what I aim at because the point of philosophy is to start with something so simple as not to seem worth stating, and to end with something so paradoxical that no one will believe it.
> —Bertrand Russell, *The Philosophy of Logical Atomism*, 1918.

Again, take some set S of formulas built from the logical constants in some set C over the atomic sentences in at. As soon as we need below to talk about negation, we will simply suppose that there are schemas of the form $\sim\varphi$, where $\sim \in \mathsf{C}$, available for us. Next, take some set $\mathcal{D}$ of designated truth-values and some disjoint set $\mathcal{U}$ of undesignated truth-values. As usual, $\mathcal{V} = \mathcal{D} \cup \mathcal{U}$. Any set $\Gamma \subseteq \mathsf{S}$ will here be called a *theory*. In the last section we talked about single-conclusion consequence relations. Given some set sem of valuations $\S : \mathsf{S} \to \mathcal{V}$, you can also define the canonical *multiple-conclusion* consequence relation $\models^{\mathsf{m}}_{\mathsf{sem}} \subseteq \mathsf{Pow}(\mathsf{S}) \times \mathsf{Pow}(\mathsf{S})$ (cf. [7]) , by simply setting:

$$\Gamma \models^{\mathsf{m}}_{\mathsf{sem}} \Delta \text{ iff } \S(\Gamma) \not\subseteq \mathcal{D} \text{ or } \S(\Delta) \not\subseteq \mathcal{U}, \text{ for every } \S \in \mathsf{sem},$$

where $\Gamma \cup \Delta \subseteq \mathsf{S}$. Taking commas as unions and omitting curly braces, from an abstract viewpoint any *tarskian* consequence relation $\models$ defined as above will be characterized by the following universal axioms, where $\mathsf{Ptn}(\Sigma)$ denotes the set of all partitions of the set Σ:

(C1)	$(\Gamma, \varphi \models \varphi, \Delta)$	(overlap)
(C2)	$(\forall\langle\Sigma_1, \Sigma_2\rangle \in \mathsf{Ptn}(\Sigma))(\Gamma, \Sigma_1 \models \Sigma_2, \Delta)$ / $(\Gamma \models \Delta)$	(cut)
(C3)	$(\Gamma \models \Delta)$ / $(\Gamma', \Gamma \models \Delta, \Delta')$	(dilution)

Having said that, I will from now on suppose that every logic has an associated consequence relation, but not necessarily a canonical / tarskian one. Each consequence relation will embody some specific notion of inference, some directives about what-follows-from-what.

There are of course some dumb examples of tarskian logic that you will prefer to avoid, for the sake of 'minimal enlightenment'. Given S, $\mathcal{D}$ and $\mathcal{U}$, collect in $\mathsf{sem}(\mathcal{D}) = \{\S : \S(\mathsf{S}) \subseteq \mathcal{D}\}$ all the valuations that are 'biased towards truth', and collect in $\mathsf{sem}(\mathcal{U}) = \{\S : \S(\mathsf{S}) \subseteq \mathcal{U}\}$ all the valuations that are 'biased towards falsehood'. Any valuation $\S_d \in \mathsf{sem}(\mathcal{D})$ will from now on be said to constitute a *dadaistic model*, and any valuation $\S_n \in \mathsf{sem}(\mathcal{U})$ will be said to constitute a *nihilistic model*. Let des denote some non-empty subset of $\mathsf{sem}(\mathcal{D})$, and let und denote some non-empty subset of $\mathsf{sem}(\mathcal{U})$. Obviously, in a logic having a non-empty set of designated values and a consequence relation characterized by des, every formula is a tautology, a top particle; in a logic having a non-empty set of undesignated values and characterized by und, every formula is an antilogy, a bottom particle; in a logic having both designated and undesignated values and characterized by models which are either dadaistic or nihilistic, any given formula follows from any other given formula; in a logic with no models, any given theory follows from any other given theory. We will call any of the four above logics *overcomplete*. If you have not seen this before, the surprising bit is that, while the distinctions are clearly visible if you use a multiple-conclusion abstract framework, the four paths to overcompleteness lead to only two different logics in a single-conclusion abstract framework.

For a quick summary, here are the names we will give to each of the above four kinds of overcompleteness, and the way they are characterized:

(1) **dadaistic logic**	(2) **nihilistic logic**	(3) **semitrivial logic**	(4) **trivial logic**
Semantical conditions:			
$\mathcal{D}_1 \neq \varnothing$	$\mathcal{U}_2 \neq \varnothing$	$\mathcal{D}_3 \neq \varnothing$ and $\mathcal{U}_3 \neq \varnothing$	—
$\mathsf{sem}_1 = \mathsf{des}$	$\mathsf{sem}_2 = \mathsf{und}$	$\mathsf{sem}_3 = \mathsf{des} \bigcup \mathsf{und}$	$\mathsf{sem}_4 = \mathsf{des} \bigcap \mathsf{und}$
Single-conclusion abstract characterizations:			
$(\forall\beta\Gamma)$	$(\forall\alpha\beta\Gamma)$	$(\forall\alpha\beta\Gamma)$	$(\forall\beta\Gamma)$
$\Gamma \models^{\mathsf{s}}_1 \beta$	$\Gamma, \alpha \models^{\mathsf{s}}_2 \beta$	$\Gamma, \alpha \models^{\mathsf{s}}_3 \beta$	$\Gamma \models^{\mathsf{s}}_4 \beta$
Multiple-conclusion abstract characterizations:			
$(\forall\beta\Gamma\Delta)$	$(\forall\alpha\Gamma\Delta)$	$(\forall\alpha\beta\Gamma\Delta)$	$(\forall\Gamma\Delta)$
$\Gamma \models^{\mathsf{m}}_1 \beta, \Delta$	$\Gamma, \alpha \models^{\mathsf{m}}_2 \Delta$	$\Gamma, \alpha \models^{\mathsf{m}}_3 \beta, \Delta$	$\Gamma \models^{\mathsf{m}}_4 \Delta$

All four overcomplete logics are obviously tarskian (you can check, as an exercise, that they respect (C1), (C2) and (C3)). Moreover, if a logic is trivial then it is both dadaistic and nihilistic, and being either dadaistic or nihilistic a logic will also be semitrivial. As you should notice, $\models^{\mathsf{s}}_1 = \models^{\mathsf{s}}_4$, so the single-conclusion framework cannot *see* the difference between the situation in which all models satisfy all formulas and the situation in which the logic has no models. Even worse, $\models^{\mathsf{s}}_2 = \models^{\mathsf{s}}_3$, so single-conclusion consequence relations for which all formulas are always false are identical to consequence relations for which all formulas are either all false or all true. And perhaps we agree that truth-blindness is a serious blindness?

Single-conclusion truth-blindness and the upgraded multiple-conclusion consequence relation can help sorting out the paradox from the last section. Say that we have a *consistent logic* in case the logic is non-dadaistic but every theory is derivable from the set of all formulas, that is, $\mathsf{S} \models^{\mathsf{m}} \Delta$, for every Δ. If you can count on dilution, (C3), that is the same as saying that there is some β such that $\not\models^{\mathsf{m}} \beta$, and at the same time $\mathsf{S} \models^{\mathsf{m}}$, that is, $\mathsf{S} \models^{\mathsf{m}} \varnothing$. In that case, the addition of a dadaistic model to a consistent logic, as we did in the last section, clearly gives you inconsistency, given that it occasions $\mathsf{S} \not\models^{\mathsf{m}}$. But in the single-conclusion case, given (C1), both semantics will give you just the same: $\mathsf{S} \models^{\mathsf{s}} \varphi$, for every $\varphi \in \mathsf{S}$.

The situation gets particularly spiky when you think of a logic having a negation symbol. Say that we have a *∼-contradictory theory* $\Gamma \subseteq \mathsf{S}$ in case there is some formula $\varphi \in \mathsf{S}$ such that both $\Gamma \models \varphi$ and $\Gamma \models {\sim}\varphi$; say that we have a *∼-inconsistent model* $\S \in \mathsf{sem}$ in case there is some formula $\varphi \in \mathsf{S}$ such that both $\S(\varphi) \in \mathcal{D}$ and $\S({\sim}\varphi) \in \mathcal{D}$. Given (C1) and a logic with a negation symbol, contradictory theories are unavoidable. The same does not happen, though, with inconsistent models —the usual set of models for classical logic and for other consistent logics does indeed avoid such anomalous models. Consider the following classical universal rules:

(R1)	$(\Gamma, \alpha, {\sim}\alpha \models \Delta)$	(*pseudo-scotus*, or *explosion*)
(R2)	$(\Gamma, \alpha, {\sim}\alpha \models \beta, \Delta)$	(*ex contradictione sequitur quodlibet*)

Obviously, (R1) implies (R2). Now, while the failure of *pseudo-scotus* corresponds to the existence of some ∼-inconsistent model (such as the dadaistic one), the failure of *ex contradictione* corresponds, more specifically, to the existence of some non-dadaistic ∼-inconsistent model (which is much more interesting). Yet the two rules look exactly the same (as (R1) collapses into (R2)) inside a single-conclusion environment.

Say that we are talking about a *∼-consistent logic* in case this logic is non-dadaistic but does respect *pseudo-scotus*. As you should recall, in case the logic has no symbol for negation non-dadaism alone is a sufficient condition for consistency. Here then is the **paradox of ineffable inconsistencies**:

Let $\mathcal{L}$ be any fixed consistent tarskian logic.
Then it is always possible to find a non-semitrivial logic $\mathcal{IL}$ such that:

$$\Gamma \models^{\mathsf{m}}_{\mathcal{IL}} \beta, \Delta \text{ iff } \Gamma \models^{\mathsf{m}}_{\mathcal{L}} \beta, \Delta \text{ (and, in particular, } \Gamma \models^{\mathsf{s}}_{\mathcal{IL}} \beta \text{ iff } \Gamma \models^{\mathsf{s}}_{\mathcal{L}} \beta),$$

yet

$$\mathsf{S} \not\models^{\mathsf{m}}_{\mathcal{IL}} \text{ (while, by definition, } \mathsf{S} \models^{\mathsf{m}}_{\mathcal{L}} \Delta, \text{ for every } \Delta).$$

In case $\mathcal{L}$ has a symbol for negation and is ∼-consistent, then

$$\alpha, {\sim}\alpha \not\models^{\mathsf{m}}_{\mathcal{IL}} \text{ (while, by definition, } \Gamma, \alpha, {\sim}\alpha \models^{\mathsf{m}}_{\mathcal{L}} \Delta, \text{ for every } \Delta).$$

You already know the simple strategy to make the above trick work: Just add to $\mathsf{sem}_{\mathcal{L}}$ some dadaistic valuation. We will call the logic $\mathcal{IL}$ thus obtained the *inconsistent counterpart of* $\mathcal{L}$. In the case of classical logic, in the last section, its inconsistent counterpart was identical to the original version from a single-conclusion perspective. But now we know that while the inconsistent counterpart of classical logic still validates rules such as *ex contradictione*, it does **not** validate *pseudo-scotus* any longer. Note that the paradox does not subsist if you add a nihilistic valuation instead of a dadaistic valuation. In that case you would need a single-premise multiple-conclusion framework for it to make sense.

The only conundrum we are left with is the following. Logics such as $\mathcal{IL}$ are very naturally obtained from their consistent counterparts, and they happen to be neither overcomplete nor consistent. Are we willing to call them *paraconsistent*?

3 Paraconsistency is not enough

To make advice agreeable, try paradox or rhyme.
—Mason Cooley, *City Aphorisms*, 14th Selection, 1994.

Universal logicians (cf. [1]) believe that logic should be seen a mother-structure (in the sense of Bourbaki) based on some given set of formulas and a consequence relation defined over it. They do not require in general these formulas and relation to bring any further built-in structure (say, an algebraic structure over the set of formulas). But in practical cases, of course, it is often interesting for instance to fix some set of axioms or another over the consequence relation. I have indeed presented above the customary tarskian axioms (cf. [9]) and immediately after that I exhibited some trivial examples of tarskian logics: the overcomplete ones. Should we modify the given axioms in order to rule out those examples as illegitimate? One could surely do that, and it has indeed been done here and there in the literature, but I am not convinced that this is such a wise manoeuvre. First of all, the overcomplete logics fit very naturally both in the abstract and the semantical frameworks. Besides, I am only talking about 'overcomplete logics' once I had decided that they should be called 'logics', to start with. *Ad hoc* modifications of the definition of logic in order to avoid the above mentioned unpleasant examples do not seem to carry much persuasive power. Imagine the following conversation between two philosophers:

(∀belard) 'I bought an arm chair today.'
(∃loise) 'How nice.'
(∀belard) 'It has flatulence filter seat cushion.'
(∃loise) 'Good.'
(∀belard) 'It has a purple upholstered back.'
(∃loise) 'Hmmm. . . '
(∀belard) 'It has 42 slender chippendale legs.'
(∃loise) 'Wait a moment. I wouldn't call a 'chair' any object having more than 4 legs!'

Was ∀belard wrong in using the word 'chair' from the very start? Maybe ∃loise has a sound intuition, and this anomalous object will turn out to be impractical as a chair —its many legs are too difficult to clean, too heavy to carry, or something. Suppose the philosophers will some day agree about the essential properties of a chair, including its maximal number of legs. Will post-modernist designers still have a job? If they will, then what will be the next development to *trivialize* the notion of 'chair'?

Going back to logic, consider the *minimal* tarskian logic defined over some fixed set of formulas S. This logic is characterized by:

$$\Gamma \models^{\mathsf{m}} \Delta \;\text{ iff }\; \Gamma \cap \Delta \neq \varnothing,$$

where $\Gamma \cup \Delta \subseteq \mathsf{S}$. Clearly, this is the minimal logic respecting (C1), and it is easy to check that both (C2) and (C3) are also respected. ∃loise, again, finds this construction quite 'trivial' and dull. Should we then add a further restriction on the definition of logic so as to please her?

It does seem hopeless, and even counterproductive, to expect logicians to reach a final agreement about the answers to fundamental questions such as 'what is logic?', 'what is negation?' (or conjunction, or some other connective), 'what is paraconsistency?' and so on. This does not mean, however, that 'anything goes'. It often seems more realistic and reasonable to look for properties that we do *not* want to allow 'interesting' logics, negations, conjunctions, paraconsistent logics etc to have. This principle that combines a strong wish for both economy and significance had been made transparent as a sort of motto for paraconsistency since its infancy (cf. [2]): 'From the syntactical-semantical standpoint, every mathematical theory is admissible, unless it is trivial' (notice that da Costa does not clear up what 'theory' or 'trivial' mean). Investing on that idea, clarifying and updating it, the paper [6] shows one way of implementing this *negative* approach to general abstract nonsense. For the purposes of the present paper, it will be sufficient to require non-overcompleteness for the definition of a *minimally decent logic*. From an abstract viewpoint, that can be done by saying for instance that a minimally decent tarskian logic should also respect a further negative axiom, denying the very possibility of semitriviality. From a semantical viewpoint the thing gets a bit more complicated. It is not enough for valuation theorists to add the requirement that both the set of designated values and the set of undesignated values should be non-empty. One needs also to directly constraint the set of all valuations of an intended semantics —or else collections of dadaistic and nihilistic models might reappear. There is no need to go into details of that here. Other necessary conditions for minimal decency might of course still impose themselves at any future moment, according to the interest and experience of logic-designers.

Now, at least two lessons may be drawn from the paradox explored in the previous sections. The first lesson is about the usefulness of a multiple-conclusion environment when doing logic in general, and paraconsistent logic in particular (I recommend checking [6], where this framework was extensively used for the study of negation, its more usual positive properties and some negative properties that make it minimally decent). Obviously, as any other formalism, multiple-conclusion will also have its limitations, and the adequacy of its use will depend of the phenomenon that needs to be seized. On the positive side, however, there are several arguments pro multiple conclusion. Many of them are well-known, or quite obvious, and I will not try to survey them here (for the interested reader, it might be a good idea to check [7]). I will mention only one further particularly interesting advantage of that formalism, as connected to paraconsistency.

Even after the wide acknowledgment of the inferential character of logic, philosophy continues to suffer from a certain 'prejudice towards truth'. Arguably, because a compact tarskian logic sees no difference between inferences with a finite or an infinite set of premises, because the single-conclusion notation derived from the notion of a closure operator cannot mark the difference between constructive and non-constructive sets of theses, and because of persisting positivistic influences, the logico-philosophical community ended up accommodating with a lot of inertia around the notion of theoremhood, as opposed to the notion of inference from a set of premises. Even nowadays, the study of 'logics as sets of theorems' or 'logics as sets of truths' is very likely to have more practitioners than the more inferential-related approach. Besides, even proposals as interesting as those of Łukasiewicz in axiomatizing his modal many-valued logics using the notion of rejected propositions, alongside with accepted propositions, were soon to fall into almost complete disregard (among the few interesting exceptions is the paper [8]). But why should truth be privileged over falsehood? Why should acceptance be privileged over rejection?

The multiple-conclusion approach allows not only the inferential character of logic to be taken into proper account but its full symmetry also allows truth and falsehood to be put on equal footing. Playing with the right-left symmetry of the consequence relation turnstile one can very naturally talk for instance about the notion of *duality* of logics, of connectives, of rules. Given a consequence relation $\rhd$, its dual $\blacktriangleright$ is such that

$$(\Gamma \blacktriangleright \Delta) \text{ iff } (\Delta \rhd \Gamma).$$

Similarly, given any rule of a connective in the first consequence relation, $\rhd$, one can immediately look for the corresponding rule of the dual connective in the second consequence relation, $\blacktriangleright$, just reading the rule the other way around. This way an introduction rule for classical conjunction can be characterized as dual to an elimination rule for classical disjunction, implication can be characterized as dual to right residuation, negation as consistency (explosion) as dual to negation as completeness, or determinedness (excluded middle). Any definition involving paraconsistency can immediately be converted into a definition involving its dual, paracompleteness. In semantical terms, given a two-valued interpretation of a tarskian logic, its dual is obtained by uniformly substituting 'true' for 'false', and vice-versa.

It is about time for the 'single-conclusion bias' to be defeated once and for all. If not just for the hidden prejudice against multiple-conclusion, or plain sluggishness of many practitioners of logic, the only extra reason I see for no version of the above paradox to have been reported before (as far as I know) is because there seems not to have been much interest in exploring single-premise inferences (that idea has been taken forward, though, in papers such as [10]). Notice, at any rate, that at the single-premise-single-conclusion case the interpretation of the entailment sign, $\models$, confuses itself with the interpretation of the (classical) material conditional.

If you recall the definition of a consistent logic proposed in the last section, you will see that an inconsistent logic will either be overcomplete or it will disrespect *pseudo-scotus*. Once a *paraconsistent logic* is A LOGIC before anything else, it should be a minimally decent logic, hence it should be inconsistent but not overcomplete (notice that the failure of *pseudo-scotus* is perfectly compatible with dadaism). It is sad to recognize that, several decades after its initial developments, paraconsistent logic remains by and large a terrain wide open for adventurers and for intellectual impostures. The general inability demonstrated by the paraconsistent community so far in having constructive conversations attests to the great lack of coordination of the field. These last grumpy (yet justified) comments of mine might help explaining, at least partially, the serious lack of foundational papers which would help in finally defining some necessary conditions for minimal decency in paraconsistent logic. The second lesson of the present paper intends to be a contribution to that. Instead of proposing changes to the very definition of paraconsistency —say, to those hazy definitions recorded in Section 1— my sole suggestion here is that a *minimally decent paraconsistent logic* should, in a multiple-conclusion abstract environment, avoid not only *pseudo-scotus* but also *ex contradictione* —as it has generally been done in the single-conclusion environment, where the two rules are indistinguishable. Semantically, as observed in Section 2, this amounts to requiring the semantics of minimally decent (tarskian) paraconsistent logics not just to allow for $\sim$-inconsistent models, but, more specifically, to allow for non-dadaistic $\sim$-inconsistent models.

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Conclusão

> A língua deste gentio toda pela Costa he huma: carece de três letras — não se acha nella F, nem L, nem R, cousa digna de espanto, porque assi não têm Fé, nem Lei, nem Rei; e desta maneira vivem sem Justiça e desordenadamente.
> —Pero Magalhães Gandavo, *Tratado da Terra do Brasil*, 1570.

Como remate da presente monografia, apresento a seguir uma lista com algumas de suas principais contribuições, de acordo com minhas próprias preferências.

1. *Um estudo formal dos princípios lógicos.* Se você já ouviu falar das lógicas paraconsistentes, certamente terá ouvido falar de como estas lógicas supostamente derrotam ao menos um dentre os ditos 'Princípios' ou 'Leis' de *ex contradictione*, *ex falso*, *pseudo-escoto* e não-contradição, e possivelmente terá ouvido falar também de como estas lógicas respeitam ainda assim os princípios da não-trivialidade e da não-extracompletude. O arcabouço conceitual demarcado nos Capítulos **1.0** e **4.1** nos permite diferenciar entre si *todos* estes princípios, e outros princípios mais. *Ex contradictione* e *pseudo-escoto* estão relacionados a um certo princípio da explosão que não pode ser obedecido por lógicas paraconsistentes. Diversas variedades distintas de explosão (explosão suplementar, explosão parcial, explosão controlável e explosão gentil) são contudo compatíveis com a paraconsistência. À diferença da maior parte da literatura relacionada, a minha presente versão do Princípio da Não-Contradição também é compatível com a paraconsistência; não obstante, como seria de se esperar, o desrespeito a tal princípio em lógicas não-extracompletas requer um ambiente paraconsistente. Mostra-se que, em geral, a fórmula fetiche dos paraconsistentistas, $\neg(A \& \neg A)$, não possui qualquer relação com a paraconsistência.

2. *Definições de lógica paraconsistente.* Sem dúvida, estas dependerão de como você define 'lógica', como define 'negação', e como define 'paraconsistente'. Ao invés de fixar tais definições de uma vez por todas, proponho aqui uma nova abordagem *negativa* a tais definições (confira o Capítulo **4**, mas também os Capítulos **1** e **3.3**). Uma lógica é tão-somente uma estrutura muito geral dispondo de um conjunto de 'fórmulas' como domínio, sobre o qual está definida uma relação de 'consequência' conveniente, para indicar o que se pode inferir a partir de que. A principal propriedade de uma lógica *decente* consiste no desrespeito à extra-completude. Uma negação é em geral um símbolo unário que pretende incorporar alguma noção geral de 'oposição', e dentre as principais propriedades negativas de uma negação *decente* se encontram as regras que denomino *verificatio* e *falsificatio*, que irão garantir que a negação não seja um 'operador positivo' —intuitivamente, elas se certificarão de que a negação inverta alguns valores-de-verdade. Finalmente, uma lógica paraconsistente *decente* deve desrespeitar ambos *pseudo-escoto* e *ex contradictione*, de modo que estas lógicas disponham não apenas de um modelo inconsistente mas também de um modelo inconsistente não-dadaísta (confira o Capítulo **4.2**).

3. *Condições de coerência para os conectivos, e conectivos perfeitos.* As constantes lógicas frequentemente possuem seu significado definido por grupos de regras abstratas complementares, as quais mostram como tais constantes podem ser introduzidas ou eliminadas, do lado direito ou do lado esquerdo do símbolo de consequência. No Capítulo **1.0** mostro como a supressão de algumas destas regras sugere o acréscimo de regras negativas adicionais, de modo a manter a coerência e evitar exemplos estéreis —ou 'indecentes'— de conectivos. Mostro no Capítulo **3.3** como as regras que são perdidas por lógicas paraconsistentes e paracompletas podem frequentemente ser recuperadas pelo acréscimo de certos conectivos subsidiários —tais como os conectivos de consistência ou inconsistência— que completam o significado parcial, restabelecendo a perfeição perdida.

4. *Definição de* **LIF***s,* **C***-sistemas, e* **dC***-sistemas.* As Lógicas da Inconsistência Formal são introduzidas no primeiro capítulo e estudadas ao longo de toda a tese. Sua quase ubiquidade na seara da paraconsistência é repetidamente ilustrada: a maior parte das lógicas interessantes produzidas pela escola brasileira se encaixam na definição, todas as lógicas modais não-degeneradas podem ser reformuladas como **dC**-sistemas, a lógica discussiva **D2** de Jaśkowski e suas parentes próximas também constituem **dC**-sistemas. Um amplo levantamento da literatura relacionada é oferecido, e os problemas relacionados à algebrização de tais lógicas e à possível validade ou invalidade da regra de substitutividade são igualmente perscrutados. O 'plano brasileiro' é completado pela proposta a partir da qual lógicas maximais que respeitam todos os requisitos iniciais de da Costa podem ser obtidas. Exemplos de **C**-sistemas que não constituem **dC**-sistemas, de **LIF**s que não constituem **C**-sistemas, e de lógicas paraconsistentes que não constituem **LIF**s também são apresentados. Enfatiza-se fortemente o *Atributo Fundamental das* **LIF***s*, tal como refletido nos chamados 'Teoremas de Ajuste de Derivabilidade' ou nas traduções que permitem que o raciocínio consistente possa ser recapturado a partir dos ambientes inconsistentes das **LIF**s.

5. *Dualidade.* Um arcabouço conceitual de relações de consequência com premissas múltiplas e conclusões múltiplas à maneira de Gentzen é investigado nos Capítulos **2.1**, **3.3** e **4**. Tal arcabouço permite o estabelecimento de uma simetria plena entre as premissas e as conclusões, e cada inferência pode então ser dualizada simplesmente ao ser lida da direita para a esquerda ao invés de da esquerda para a direita, ou vice-versa. Em termos semânticos, esta manobra corresponde à substituição de verdade por falsidade e vice-versa em cada modelo dado. Lógicas paracompletas são assim caracterizadas como duais das lógicas paraconsistentes, e as duais das **LIF**s constituem as chamadas Lógicas da Indeterminação Formal, **LUF**s.

6. *Definição das estruturas de traduções possíveis.* No Capítulo **2.1**, certas definições bastante generosas de Representação por Traduções Possíveis e de Semântica de Traduções Possíveis são oferecidas pela primeiríssima vez, tanto em termos de lógicas com conclusão simples quanto em termos de lógicas com conclusões múltiplas. Mostra-se ali que a teoria de tais estruturas estende a teoria geral das matrizes e dos cálculos lógicos. No Capítulo **2.2** são apresentados diversos exemplos de semânticas de traduções possíveis aplicadas a algumas **LIF**s muito fracas que não são caracterizáveis por matrizes finitas nem por semânticas modais usuais.

7. **LIF***s modais.* Como já se acenou, defende-se aqui, nos Capítulos **3.2** e **3.3**, que as lógicas paraconsistentes possuem uma interseção significativa com as lógicas modais. Demonstra-se no Capítulo **3.2** que a lógica **D2** de Jaśkowski não constitui uma lógica modal usual, uma vez que **D2** falha a propriedade da substitutividade. Muitos exemplos naturais de **LIF**s satisfazendo plenamente a propriedade da substitutividade são apresentados, com a ajuda de interpretações modais para as negações paraconsistentes e os conectivos de consistência. Um estudo similar é realizado sobre as **LUF**s.

8. *Lógicas da Essência e do Acidente.* Estudados independentemente da presença de uma negação paraconsistente, os conectivos modais de consistência e de inconsistência podem ser lidos como conectivos que qualificam regimes de verdade essenciais e acidentais. Uma linguagem modal pobre é definida pelo acréscimo de tais conectivos à linguagem clássica, e uma lógica modal mínima da essência e do acidente é adequadamente axiomatizada no Capítulo **3.1**. Apresentam-se alguns resultados iniciais sobre a definibilidade da linguagem modal usual a partir da linguagem da essência e do acidente, bem como sobre a caracterizabilidade de classes de enquadramentos com o auxílio desta última linguagem.

9. *Várias confusões e equívocos de outros autores são apontados tão logo aparecem*, ao longo da tese inteira. Alguns dos defeitos são consertados.

10. *Filosofia Formal.* A tese como um todo constitui uma ilustração de como problemas filosóficos podem ser estudados com o auxílio de ferramentas lógicas convenientes, se ao menos concordarmos em fixar uma formalização conveniente para os termos em discussão. A filosofia pode assim reclamar também seu laboratório e seus instrumentos de medida.

No que tange à continuidade das presentes investigações, vários novos caminhos foram descerrados e diversos problemas foram deixados em aberto ao longo desta monografia. Auspiciosamente, a expectativa é de que eles venham a alimentar durante algum tempo o progresso das lógicas paraconsistentes. Ficam todos os colegas desde já convidados a contribuir.

Quaisquer opiniões e juízos de valor porventura expressos
nesta tese são, é lógico, de minha exclusiva responsabilidade.
Já a interpretação do texto, esta fica a cargo do leitor...

Fevereiro de 2005.

JOÃO MARCOS