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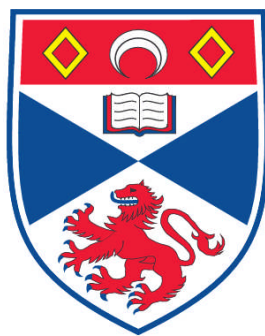
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The Structure of Logical Consequence: Proof-Theoretic Conceptions

by

Ole Thomassen Hjortland

A thesis submitted in partial fulfillment for the
degree of Doctor of Philosophy

in the
School of Philosophical, Anthropological and Film Studies

October 2009

Declaration of Authorship

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I was admitted as a research student in September 2006 and as a candidate for the degree of Doctor of Philosophy in September 2006; the higher study for which this is a record was carried out in the University of St Andrews between September 2006 and September 2009.

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So many and so various laws are giv'n;
So many laws argue so many sins

                                  – Milton, *Paradise Lost*

Abstract

The model-theoretic analysis of the concept of logical consequence has come under heavy criticism in the last couple of decades. The present work looks at an alternative approach to logical consequence where the notion of inference takes center stage. Formally, the model-theoretic framework is exchanged for a proof-theoretic framework. It is argued that contrary to the traditional view, proof-theoretic semantics is not revisionary, and should rather be seen as a formal semantics that can supplement model-theory. Specifically, there are formal resources to provide a proof-theoretic semantics for both intuitionistic and classical logic.

We develop a new perspective on proof-theoretic harmony for logical constants which incorporates elements from the substructural era of proof-theory. We show that there is a semantic lacuna in the traditional accounts of harmony. A new theory of how inference rules determine the semantic content of logical constants is developed. The theory weds proof-theoretic and model-theoretic semantics by showing how proof-theoretic rules can induce truth-conditional clauses in Boolean and many-valued settings. It is argued that such a new approach to how rules determine meaning will ultimately assist our understanding of the apriori nature of logic.

Acknowledgements

Philosophy is at its best when thought is extended into dialogue. The philosophical conversation denies an idea the suffocating intimacy of the author's intellectual harbour, and launches it into the open sea of scorn and scrutiny. In Arché and St Andrews I learned that the hardships of Neurath's boat are best faced in the company of friends and colleagues.

In the years leading up to my PhD in St Andrews, I benefited from the encouragement of a number of excellent teachers. To Thorleif Lyng I owe that first inspiration to pursue philosophy, and, above all, to perceive the critical nerve of the dissident as a virtue. Paul Johansen taught me about self-discipline and humility the *Kyokushin* way—by example. Every day is a struggle to follow their advice.

I served my first years in the philosophical trenches at the University of Bergen. Not lacking in enthusiasm, but short of direction, I found my calling in the solemn but authoritative logic lectures by Kåre Johnsen. Eivind Kolflaath drew the line between sound argument and African drumming in a place where straight lines are hard to come by. I am thankful to both of them for all their support, and for having left me with fond memories of the days in the department. Finally, thanks to Michaeł Walicki for pausing his logic lecture to point out that "it's not semantics—it's the real world".

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It's worth breaking the rules for my friends and family. It's worth breaking the rules with my friends and family. My parents, Trond Hjortland and Inger-Johanne Thomassen, enabled me to choose any life I wanted. I've had the best of all possible families. I can never thank them enough.

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Finally, thanks to Jesse Tomalty—you changed the premises, you changed the conclusion.

<div align="right">

Ole Thomassen Hjortland
St Andrews
September 2009

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For Famulus,

whose narrative is not so much diabolic transaction,

as diabolic translation

Part I

Proof-Theoretic Harmony

Chapter 1

Introduction

1.1   What is Logical Consequence?

Good advice was never Mephistopheles's forte, but every dog has its day. To the
student:

> Mein teurer Freund, ich rat' Euch drum
>
> Zuerst Collegium Logicum.
>
> Da wird der Geist Euch wohl dressiert,
>
> In spanische Stiefeln eingeschnürt,
>
> Daß er bedächtiger so fortan
>
> Hinschleiche die Gedenkenbahn,
>
> Und nich etwa, die Kreuz und Quer,
>
> Irrlechteliere hin und her.

Even if the disguised trickster eventually reneges, and suggests that the student
choose medicine over logic (for more worldly reasons), better advice is hard to come
by. Thus, unabashed by its origin, we embrace the suggestion. True, logic is *trivial*,
but only in the sense that it belonged to the academic *trivium*, the introductory

study of grammar, rhetoric, and logic. In fact, logic is at the heart of philosophy, and so more foundational than trivial. The choice of topic for the thesis reflects a belief in the traditional notion that logic, as a formal discipline, holds a privileged position in philosophy because of its *apriori* nature. To understand not only why logic is apriori, but how it can be both apriori *and* informative, philosophers of the past century have turned to a study of the concept of *logical consequence*, both in its *mathematical* guise and in its *epistemological* guise.

What is logical consequence? Roughly, it is the relation that holds between the premises and conclusion of a *valid argument*. Sometimes we say that an argument is valid just in case whenever the premises are true, the conclusion must be true. Alternatively, that it is impossible for the premises to be true and the conclusion false simultaneously.[1] Logic text books, both formal and informal, are riddled with formulations along such truth-preservational lines, all with the common element that the gloss on validity contains a *modal* element. It appears inevitable that such a modal element must be included, since, surely, logical consequence cannot be a matter of truth-preservation *simpliciter*. Just think of arguments which happen to have true premises and a true conclusion. Something more robust is required for the conclusion to *follow from* the premises. Logicians are in the market for a relation that *guarantees* the truth of the conclusion when the premises are true. That is, the conclusion *necessarily* follows from the premises.

But, modal talk is cheap. A *conceptual analysis* of logical consequence should attempt to explain the concept in terms of—not to say reduce to—more transparent concepts. In many logicians' view, a recourse to bare modal notions fails on this account. It is on the backdrop of such considerations that Alfred Tarski's ground-breaking work on logical consequence became part of the philosopher's logical kit. The achievement of Tarski (2003) was to combine a neat and economical analysis

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[1]We stay clear of questions about what precisely the relata of logical consequence are, e.g., propositions, sentences, utterances, etc.

of logical consequence with a powerful mathematical structure which is void of modal elements. Or so goes the tabloid version of the story.

1.2   The Bolzano-Tarski Tradition

In 1990, John Etchemendy wrote the following evaluation of the Tarski's seminal paper 'On the Concept of Following Logically':

> The highest compliment that can be paid the author of a piece of conceptual analysis comes not when his suggested definition survives whatever criticism may be leveled against it, or when the analysis is acclaimed unassailable. The highest compliment comes when the suggested definition is no longer seen as the result of conceptual analysis—when the need for analysis is forgotten, and the definition is treated as common knowledge. Tarski's account of the concepts of logical truth and logical consequence has earned him this compliment. (Etchemendy 1990, p. 1)

Was Etchemendy right in warning us against the hegemony of Tarski's work? Perhaps, although there were certainly critics from the very beginning. Relevant logicians had already argued that logical consequence took something more than mere truth-preservation: There must be a relation of *relevance* between premises and conclusion (formally, say, *variable-sharing*).[2] Intuitionistic logic had promulgated the alternative *Brouwer-Heyting-Kolmogorov* (BHK) interpretation.[3] Nevertheless, one thing that seems absolutely sure is that the two decades following Etchemendy's book have seen a widespread return to questions about the foundations of logical consequence. It is important to appreciate that the influence of Tarski's analysis of logical consequence has been enormous, and when compared

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[2]For the basics, see Priest (2008, ch. 10).
[3]See ibid., ch. 6. Also, footnote 26.

with the criticism produced by his closely related analysis of *truth* (Tarski 1933), the number of pre-Etchemendy dissidents appear embarrassingly low.[4]

What changed with Etchemendy? To understand we need to revisit the basics of Tarski's concept of logical consequence. Tarski deflated modal talk in the explanation of logical consequence by describing the relation in a set-theoretic framework. Formulae are *interpreted* by set-theoretic models, which tentatively represent a word-world relation, and truth-conditions are understood as *truth-in-a-model-conditions*.[5] Necessary truth-preservation is characterised as truth-preservation across interpretations. More precisely, an argument is valid just in case for every model $\mathcal{M}$, whenever the premises are true in $\mathcal{M}$, the conclusion is true in $\mathcal{M}$. The modal element is thus replaced with a quantification over set-theoretic structures. Of course, precisely how to detail the ontological significance of the set-theory, and therefore also the alleged word-world connection, is a matter of some contention. Nevertheless, the variety of applications and extensions Tarski's work has found, speaks for itself. It has grown to become not only part of mathematical canon with the development of model-theory, but also the engine behind much of formal semantics for natural language.[6]

Regardless of its succeess, however, philosophical scrutiny was called for when Etchemendy in his *The Concept of Logical Consequence* launched a two-pronged attack on the model-theoretic account. The first is an extensional objection, claiming that model-theoretic consequence is in danger of over-generating, i.e., holding arguments to be valid which we pre-theoretically take to be invalid. In a nutshell, Etchemendy argued that the size of the *domain of quantification*, i.e., the set of objects the quantifers range over, had an unintentional effect on the model-theoretic

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[4]It is noteworthy for what follows that Prawitz (1985) to an extent anticipated Etchemendy's criticism. For criticism of Tarski's theory of truth, see for instance Field (1972) and Putnam (1985).

[5]From which the famous slogan in Hodes (1984): "Truth in a model is a model of truth."

[6]The title Bolzano-Tarski hints at the fact that Tarski's work was anticipated in Bolzano (1837), albeit in an intensional framework. An even earlier, and less acknowledged, author who foreshadowed the tradition is the fourteenth century philosopher John Buridan (see especially his *Tractatus de consequentiis*, translated in Buridan 1985).

consequence relation. Since both quantifiers and—normally—identity are considered part of the *logical vocabulary*, i.e., the set of logical constants in the formal language, one can construct 'cardinality sentences' where there are no expressions whose interpretation is provided by the models. Logical constants are, in the terminology of Tarski (1986), *invariant* under interpretation. As a consequence, the only thing that can falsify a cardinality sentence, say, $\ulcorner \neg \exists x \exists y \; x \neq y \urcorner$, is the size of the quantification domain (in this case, when there more than one member of the domain). If, however, every model has a domain with some finite cardinality, there will be a first-order formula φ such that φ is a *logical truth*, i.e., is true in every model. Since logical truth is simply a limit case of logical consequence (with the empty set as the set of premises), it follows that the extension of model-theoretic consequences is afflicted as well.

Of course, in standard model-theory this problem is avoided by insisting that the *universe* $\mathcal{U}$ over which the quantification domains range is infinite. Hence, a model can have any non-empty quantification domain. But, Etchemendy continues, this is besides the point: Even if the universe happened to be finite, the cardinality sentences should still not count as logical truths. That they do is merely a symptom of the ontological inflexibility of the model-theoretic framework.[7] In particular, the *finitist* might want to subscribe to a background set-theory without an axiom of infinity, and thus without the resources to invalidate any finite cardinality sentence. It appears awkward to force someone of such a metaphysical leaning to accept that some cardinality sentences are logically true.

Even worse, Etchemendy observes that model-theoretic consequence is hostage to set-theory in a much wider sense. With second-order expressive power, we can for instance formulate the *Continuum Hypothesis* (CH) and other set-theoretic claims in pure logical syntax.[8] This is problematic. CH is famously independent

---

[7]This consideration bridges into the debate about *unrestricted quantification* in model-theory, see especially Rayo & Uzquiano (1999).

[8]See Shapiro (1991, p. 105) for details. In fact, one can now express that the domain is finite, or even of some given transfinite cardinality.

of the axioms of Zermelo-Frankel set-theory, but if Etchemendy is right, whether or not the hypothesis is true will make a difference for the concept of logical consequence. Maybe second-order logic is 'set-theory in sheep's clothing', but nonetheless it turns out that model-theoretic consequence is surprisingly sensitive to sophisticated set-theoretic considerations.

For Etchemendy, the extensional problem of over-generation is merely a symptom of a more general, conceptual problem. The problem, he suggests, is with the *reductive* character of model-theoretic consequence: The move from modal notions to truth-in-a-model and interpretations. At the heart of Etchemendy's criticism is the problem that model-theoretic consequence is 'epistemically impotent'. In Etchemendy's own words:

> The property of being logically valid cannot simply consist in membership in a class of truth preserving arguments, however that class may be specifed. For if membership in such a class were all there were to logical consequence, valid arguments would have none of the [modal] characteristics described above. They would, for example, be epistemically impotent when it comes to justifying a conclusion. Any uncertainty about the conclusion of an argument whose premises we know to be true would translate directly into uncertainty about whether the argument is valid. All we could ever conclude upon encountering an argument with true premises would be that either the conclusion is true or the argument is invalid. For if its conclusion turned out to be false, the associated class would have a non-truth-preserving instance, and so the argument would not be logically valid. Logical validity cannot guarantee the truth of a conclusion if validity itself depends on that self-same truth. (Etchemendy 2008, p. 266)

If Etchemendy is correct, model-theoretic consequence cannot carry the epistemological burden traditionally ascribed to logical consequence. Yes, it provides a

mathematical framework for necessary truth-preservation (over-generation issues notwithstanding), but it offers no insight into the role of logic as an avenue to apriori knowledge in philosophy.[9]

Tarski would surely have been puzzled about such an objection. It was never his intention to provide an analysis of the epistemological ramifications of logical consequence. In particular, one might imagine Tarski saying, appropriately, that such questions are best left to another part of mathematical logic: *proof-theory*. For, proof, unlike truth-preservation, is an inherently epistemic notion, one in which we might invest hopes of understanding the subtle connections between valid argument and apriori knowledge.

The question, though, is whether proof-theory, as a formal discipline, can offer anything in the way of a conceptual analysis of logical consequence. The answer, we maintain, is yes.

1.3 Proof-Theoretic Semantics

1.3.1 PTS

Proof-theoretic semantics (PTS) is a formal semantics for logic. Like its most prominent counterpart—model-theoretic semantics (MTS)—it aspires to give an account of logical constants, and, ultimately, of logical consequence. PTS can be roughly characterised by a short list of slogan-like features:

- *inferential*;

- *proof-conditional*;

---

[9]We will not go into any detail on the many responses to Etchemendy. The literature is massive, with both replies in the defense of model-theoretic consequence, and work extending Etchemendy's criticism. We here only mention some of the important contributions to the debate: McGee (1992), Read (1994), Priest (1995), Gómez-Torrente (1996), Sher (1996), Shapiro (1998b), and Blanchette (2000).

- *structural*;

- *procedural.*

Let us give some details to these properties. PTS is a formal semantics associated with *logical inferentialism* and *inferential role semantics* for logical constants. In short, the idea is that the meaning of a logical constant is fixed by the constant's behaviour in the inferential rules which govern its use.[10] Let us call this thesis INF. PTS is at heart an attempt at embedding INF in a precise proof-theoretic framework, along the lines of what model-theory does for truth-conditional semantics.[11]

Put contrastingly, we can say that PTS is *inferential* rather than *denotational.* Unlike MTS, where logical constants are associated with interpretations in set-theoretical structures (models), PTS associates logical constants with the role they play in proof-theoretic frameworks. Such frameworks are different types of *axiomatisation* of inference rules, standardly *natural deduction* or *sequent calculus*, but not exhausted by these.[12]

As opposed to the *truth-conditional* approach of MTS, the proof-theoretic frameworks of PTS are employed to formalise the *proof-conditions* of formulae containing the logical constants in question, i.e., the conditions under which a formula with a principal occurrence of a certain logical connective is provable. Just as MTS formalises truth-conditions in terms of *truth-in-a-model*-conditions (or even *designated-value-in-a-model*-conditions), PTS formalises proof-conditions differently depending on the specific proof-theoretic framework, e.g., as intro-rules

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[10]The problem of how we ought to understand 'fixing meaning' is discussed in some detail in Chapter 6. For some initial remarks on the relation to logical consequence, see Section 1.3.2.

[11]Standard references on proof-theoretic semantics include Sundholm (1986) (although a bit dated) and Kahle & Schroeder-Heister (2006). The latter is the introduction to a *Synthese* volume containing several informative pieces on the topic. In addition, an non-exhaustive bibliography can be found online on the Arché TWiki: http://arche-wiki.st-and.ac.uk/~ahwiki/bin/view/Arche/ProofTheoreticSemantics

[12]We will return to the importance of proof-theoretic generalisations of sequent calculus in later chapters (see especially Chapter 6).

or right-side sequent-rules, etc.. The motivating idea is still that the formalisms *approximate* the semantics of natural language expressions. Thus, giving an appropriate semantics for logical constants is a major part of PTS. Most of the recent literature deals with different formal issues in the proof-theoretic rendering of INF, and the present work will reflect this priority.

Although it should not be ruled out that PTS might offer resources to deal with non-logical expressions, for the purposes of what follows PTS is understood as a semantics for logical constants only. As a consequence, the inferential aspect of PTS should not be confused with more ambitious programmes, for example Brandom (1994, 2000, 2008).[13]

1.3.2 Provability and Derivability

The proof-theoretic framework is further supposed to equip PTS with the resources to analyse logical consequence as a *procedural* concept, not merely offer an extensional characterisation like MTS. In PTS, logical consequence is understood as something more epistemologically robust than mere truth-preservation (designated-value-preservation) over a class of models. It is rooted in a tradition of logic where the chief object of study is the stepwise reasoning from premises to conclusion. Such an approach involves taking seriously the idea of looking at *structural* properties of proofs as an integral part of the concept of logical consequence. It is a step towards giving consequence an inferential, and thus, hopefully, *epistemic*, content.

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[13]Similarly, a connection to the Wittgensteinean use-theoretic tradition is often cited:

> We can conceive the rules of inference [...] as giving the signs their meaning, because they are rules for the use of these signs. So that the rules of inference are involved in the determination of the meaning of the signs. In this sense rules of inference cannot be right or wrong. (Wittgenstein 1978, VII, 30)

The importance of this heritage, however, should not be exaggerated. No further exegetical connection will be provided in what follows.

One might object that this is confusing the question of what validity *is* (or what logical consequence *is*) with how we come to know that an argument is valid (or when a conclusion is a logical consequence of a set of premises). Perhaps this is a quibble over labels, but the proof-theoretic semanticist thinks that any rewarding conceptual analysis of logical consequence must adhere to the insight that at the heart of logic is the act of inferring, not the property of truth-preservation. Replying to this that logical consequence *just is* model-theoretic consequence (or necessary truth-preservation for that matter) reveals an impoverished understanding of the history of logic prior to the influential Bolzano-Tarski tradition. Similarly, the idea is not to conflate *provability* and consequence. Provability understood as derivability-in-**S** ($\vdash_S$) is a syntactic relation. Consider the following passage from Etchemendy (1990):

> It has long been acknowledged that the purely syntactic approach does not yield a general analysis of the ordinary notion of consequence, and in principle cannot. The reason for this is simple. It is obvious, for starters, that the intuitive notion of consequence cannot be captured by any *single* deductive system. For one thing, such a system will be tied to a specific set of rules and a specific language, while the ordinary notion is not so restricted. Thus, by "consequence" we clearly do not mean derivability in this or that deductive system. But neither do we mean derivability in some deductive system or other, for any sentence is derivable from any other in *some* such system. So at best we might mean by "consequence" derivability in some *sound* deductive system. But the notion of soundness brings us straight back to the intuitive notion of consequence [...] (ibid., pp. 2-3).

The right response for the proof-theoretic semanticist is to take onboard the first claim, but resist the idea that any proof-theoretic approach to consequence must

be *sound* with respect a model-theoretic consequence relation. Just like the model-theoretic consequence relations are somehow "sound" by internal standards (not with respect to some other formal relation), PTS ought to give an account of consequence whose success is not measured by comparison to some other formalism, but by general desiderata of the conceptual analysis. Furthermore, as opposed to what Etchemendy suggests, there is no reason to suspect that proof-theoretic conception of logical consequence must be *language dependent.* Just like MTS does not offer an account of consequence in terms of specific truth-conditions, PTS does not offer an account of consequence in terms of specific proof-conditions (i.e., a specific set of logical constants or a specific set of rules). Indeed, a prominent example of this is Dag Prawitz's work on PTS.[14] Ultimately, the aim is to think of PTS as providing semantics for a variety of logics, in the same spirit as MTS. For this reason it is imperative that PTS can shake off the revisionist association and become a formal semantics without an agenda of identifying the One True Logic. The theme of revision and PTS will be revisited both in Chapter 3 and Chapter 5.

1.3.3   Understanding and Analyticity

PTS in and of itself does not promise any epistemological story about logical consequence—it is merely a formal semantics. Nevertheless, some of the appeal of PTS is that it explicates a connection between the *semantics* of logical constants, our *understanding* of them (the knowledge of their meaning), and, finally, why we are *entitled* to draw certain inferences. Such a connection is, admittedly, based on the elusive notion of *analyticity*: In analogue to the idea that a proposition can be *true in virtue of its meaning*, there is the idea that an argument can be *valid* in terms of the meaning of premises and conclusion. 'Analytic validity', in Arthur Prior's phrase, might be associated with MTS as well as PTS, but it is in the latter that concept-aquisition—and thus understanding—is linked to inferential

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[14]See especially Prawitz (1985).

competence. In this sense, PTS serves as a framework for the justification of deduction, a project initiated in Dummett (1975).

The epistemological notion of analyticity, and a corresponding understanding of justification of deductive inference, has recently been espoused by Boghossian (1997, 2000, 2003). The view has received some favourable attention in Wright (2001, 2003), connecting it with acquisition of *warrant.* Less favourable is the now household criticism offered by Williamson (2003, 2006).[15] Williamson argues persuasively for the failure of upholding a so-called *assent-understanding* connection (where assent is a belief-like, but non-propositional, attitude towards an argument).

There can, in general, be no doubt that the jury on epistemological analyticity is still out. Unfortunately, there will be little in what follows to reassure the sceptic, be she of Quinean or Williamsonean persuasion. Instead, the main focus in the present work is on preconditions for successful *implicit definitions* of logical constants. More specifically, it is an investigation of the circumstances under which inference rules—serving as 'definiens'—can be said to *fix the meaning* of an involved logical constant. Any epistemological gain to be had from PTS must be preceded by a convincing story of how the meaning of logical constants, and thus concept-acquisition, is determined by an inferential practice. It is to this end that the debate about *proof-theoretic harmony* takes center stage.

Before we proceed to introduce this debate, we need to situate the ideas in its formal framework. Since it is less familiar to philosophers than model-theory, it is worth starting with a brief historical introduction to proof-theory as a formal discipline, uncorrupted by its semantic aspirations.

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[15]Both papers occur, in slightly revised form, as parts of Williamson (2007, chs. 4-5).

1.4   Proof-Theory: A Brief History

1.4.1   The Structural Era

At the same time as Tarski wrote his seminal papers (1933, 1936) initiating the
model-theoretic tradition in contemporary logic, Gerhard Gentzen—a student of
Hilbert's—started what was only later realised to be a revolution of proof-theory.

Gentzen (1934) and, independently, Stanisław Jaśkowski (1934), were the origina-
tors of so-called *structural proof-theory*.[16] These were early signs of proof-theory
breaking free from the *Frege-Hilbert style axiomatizations*. Sometimes just called
Hilbert style axiomatizations, these systems came into prominence with Hilbert
(1926) and Hilbert & Bernays (1934), but were anticipated much earlier in Frege's
Begriffsschrift (1879).[17] These systems are typically not *schematic*—the set of
rules includes (at least tacitly) a *substitution rule* that you use to derive the ver-
sions of the axioms needed. According to Church (1956, p. 158) the first schematic
Hilbert system is in von Neumann (1927).[18]

As opposed to the pre-structural proof-theory, Gentzen and Jaśkowski's natural
deduction provides a framework which goes some way towards capturing reasoning
in natural languages. Indeed, this was Gentzen's explicit concern:

> We wish to set up a formalism that reflects as accurately as possible
> the actual logical reasoning involved in mathematical proofs. [...] The
> calculus *NJ* [natural deduction for intuitionistic logic] lacks a certain
> formal elegance. This has to be put against the following advantages:
> A close affinity to actual reasoning, which had been our fundamental
> aim in setting up the calculus. The calculus lends itself in particular to

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[16]Jaśkowski's published his formalism first, but this was a linear style natural deduction, and
only for classical logic. The key insights from Gentzen's work was not yet present in Jaśkowski.

[17]See also Łukasiewicz & Tarski (1930) for a simplification of Frege's system.

[18]For a detailed history of Frege-Hilbert axiomatizations, see Church (1956, pp. 155-66).

the formalization of mathematical proofs. In most cases the derivations
for true formulae are *shorter* in our calculus than their counterparts in
the logistic calculi. This is so primarily because in logistic derivations
[i.e., Frege-Hilbert] one and the same formula usually occurs a number
of times (as part of other formulae), whereas this happens only very
rarely in the case of *NJ*-derivations. (Gentzen 1934, pp. 76-80)[19],[20]

Natural deduction does not operate on formulae alone, but allows (schematic) *reasoning under assumption*. Just like Tarski (2003) brings semantics from logical truths to logical consequence, the structural proof-theory is *rule-based* rather than *axiom-based*. This allows for more comprehensible derivations of simple consequences (e.g., in Hilbert's system proving $\ulcorner A \rightarrow A \urcorner$ requires five lines). More, in mimicking our inferential practice, natural deduction underwrites the epistemic import of proof-theory. Formally, the move underwrites the later development of *automated proof-search* (especially in sequent calculus), that is algorithmically producing proofs for arguments. In contrast, the Hilbert-systems are particularly susceptible to *proof-checking*, the process of deciding for a derivation-tree whether or not it is in fact a proof.

Early developments of natural deduction can be found in Curry (1950, 1977). Smullyan gave a linear (as opposed to tree-like) natural deduction along the lines of Jaśkowski. Popular variants can be found in Fitch (1952) (linear but with graphical device for subderivations) and Lemmon (1965) (linear and numerical). Through the later work of Dag Prawitz (especially 1965 and 1971), natural deduction became the locus of most developments in PTS. Intro- and elim-rules were plausible ways of spelling out proof-conditions for logical connectives, and

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[19]Compare Jaśkowski (1934): "The chief means employed in their [mathematicians] method is that of an arbitrary supposition. The problem raised by Mr. Łukasiewicz was to put those methods under the form of structural rules and to analyse their relation to the theory of deduction. The present paper contains the solution of that problem." (ibid., p. 232)

[20]On the background of the above, Troelstra & Schwichtenberg (2000) wonder if Gentzen had a normalization result for *NJ*, and thus the subformula property, even though he did not present it.

Prawitz's *normalisation theorem* (1965) offered a powerful analysis of the inferential interaction of the intro- and elim-rules.[21] In what follows, we will follow the Prawitz-style tree-like natural deduction.[22]

But Gentzen did not stop with natural deduction. Even though natural deduction was the *primus motor* for PTS, it was sequent calculus that proved the most valuable contribution to structural proof-theory. In Gentzen, sequent calculus was conceived as a *metacalculus* for natural-deduction, deriving sequents rather than formulae, i.e., structures of the form $\ulcorner \Gamma \Rightarrow \Delta \urcorner$ where Γ (the *antecedent*), Δ (the *succedent*) are finite subsets of well-formed formulae, WFF. In fact, Gentzen took Γ, Δ to be lists of formulae, and so a structural rule of *exchange* had to be added to the system to ensure commutativity of the comma. Today, sequent calculi are standardly presented with either sets or *multisets* instead, for which exchange is taken to be an *absorbed* or implicit rule.[23]

Gentzen himself proposed sequent calculus as natural deduction in a 'formalistic' style, meaning that is was—like its Hilbertian ancestors—a *local* calculus for which no assumptions (or assumption-discharges) apply *globally* (i.e., are not restricted to a single rule-application). Yet, it "takes over from [natural deduction] the division of the forms of inference into introductions and eliminations of the various logical symbols" (Gentzen 1934, p. 82). In other words, Gentzen wanted to provide a formalism that was local like Frege-Hilbert axiomatizations, but which was still based on *formation-rules* for logical constants.

Gentzen also volunteers a reading of a sequent $\ulcorner A_1,..., A_n \Rightarrow B_1,..., B_m \urcorner$ as the formulae $\ulcorner (A_1 \wedge,..., \wedge A_n) \rightarrow (B_1 \vee,..., \vee B_m) \urcorner$.[24] Although such a reading emphasizes the connection with Frege-Hilbert axiomatizations, it is unhelpful in

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[21]The normalisation result was anticipated in an unpublished proof by A. M. Turing. In particular, Turing proved normalisation for simply typed λ-calculus. See Troelstra & Schwichtenberg (2000, p. 224).

[22]The PTS interest in both cut elimination and normalization will be become evident later on.

[23]Multisets of formula are simply sets for which the number of occurrences of a formula matter, e.g., $\{A, B\} \neq \{A, A, B\}$.

[24]Recall that antecedent and succedents of sequents are assumed to be finite.

so far as one should not assume that the operators of the structural language (e.g., left- and right-side commas, sequent arrow) correspond to connectives in the object language. For one, such a correspondence massively depends on *structural rules*. Yet, to the extent that we want what Paoli (2002) calls a *truth-based reading* of sequents, where $\Gamma \Rightarrow \Delta$ is valid iff either at least one $A \in \Gamma$ is false or at least one $B \in \Delta$ is true, Gentzen's paraphrase might be desirable with '$\rightarrow$' taken as material implication.[25] But this is by no means the only available reading. For non-classical purposes, e.g. intuitionistic logic, we might prefer a *proof-based reading* where sequents are read according to a *BHK*-interpretation of logical connectives.[26] The advantage is that sequents are read *inferentially*, according to the idea that the calculus is a metacalculus, and thus does not operate over the same type of objects as a Frege-Hilbert system. Following this thought, we can think of sequents as first-order rules and sequent-rules as second-order rules.[27]

The *raison d'être* of Gentzen's sequent calculus is the *Hauptsatz*, or what is more commonly known as *cut elimination*. Gentzen proved for sequent systems for classical and intuitionistic logic (*LK* and *LJ* respectively) that any derivation can be transformed into a derivation with the same endsequent and with no applications of Cut.[28] (Note that there is a difference between mere *admissibility of Cut* (closure under Cut)—i.e., just the existence of a Cut-free derivation for any derivation in the system—and a proof involving an algorithm for constructing Cut-free derivations from any derivation.) As a corollary, we get the *Subformula Property*,

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[25]See Paoli (2002) for details. He also suggests an *informational reading* and a *Hobbesian reading*.

[26]

- p proves $A \wedge B$ iff p is a pair (p_0, p_1) and p_0 A, p_1 proves B;

- p proves $A \vee B$ iff p is either of the form $(0, p_1)$, and p_1 proves A, or of the form $(1, p_1)$ and p_1 proves B;

- p proves $A \rightarrow B$ iff p is a construction transforming any proof c of A into a proof $p(c)$ of B;

- $\bot$ is a proposition without proof.

[27]In Section 6.5 we return to the issue of interpretation of sequents.

[28]See Appendix A for different Cut rules. A more general Cut rule is mentioned earlier in Hertz (1929).

saying that in any *LK*- or *LJ*-derivation without Cut, all formulae occurring in the derivation are *subformulae* of the endesequent formulae.[29]

Among important pre-substructural developments of Gentzen's work on sequent calculus is Ketonen (1944) and its review by Bernays (1945) that helped make Ketonen's work known. He modified (∧L), (∨R), and (→L) in order to prove an inversion result for the *LK* system—roughly that if a conclusion-sequent of the rule is provable, then the premise-sequents are provable.[30] These are the rules now common in the **G3c** system (where structural rules are absorbed).[31] The inversion lemma was developed in Schütte (1950), where we also find a formulation of *one-sided systems* for classical logic (Appendix A.16). Further, see Kleene (1952b) for the systems that now correspond to the G1-systems, and Dragalin (1979) for the G3-systems.[32]

Cut elimination is considered the main result for sequent systems today, and the literature is enormous. For up to date presentations of the proofs for standard systems, see Troelstra & Schwichtenberg (2000) and Negri & von Plato (2001).

1.4.2   The Substructural Era

It has been said that sequent calculus is 90% of proof theory-today, and this is perhaps no exaggeration. The reason is the substructural revolution in proof-theory in the last couple of decades. Arguably, Gentzen was not the first to formulate inference rules which were *structural* in the sense that they do not involve any logical constants. *Weakening*[33] and Cut[34] can already be found in Hertz

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[29]This property of course depends on the assumption that the involved sequent rules behave as formation rules. See also Section 2.3.3.

[30]See Troelstra & Schwichtenberg (2000) for the inversion lemma.

[31]See Appendix A.14.

[32]The systems are listed in full in Appendix A.

[33]This is a translation of Gentzen's 'Verdünnung' due to Curry (1950). Kleene (1952a) gives the name 'Thinning'. Note also that there is an affinity with Tarski's *monotonicity condition* on consequence operators. See Tarski (1930). Finally, Hertz calls it 'unmittelbarer Schluss'.

[34]Cut is sometimes (perhaps a bit misleadingly) called *transitivity*. Again, this is reminiscent of Tarski (1930).

(1929). Further, Gentzen introduces *Contraction* and *Exchange*[35]. (As historical curiosa we note the abbreviations of Weakening, Contaction, and Exchange as, respectively, **K**, **W**, and **C** via the *Curry-Howard formulae-as-types interpretation*. In other words, take care not to represent weakening as **W**!)

With the explicit formulation of structural rules comes the possibility of studying so-called *substructural* logics, i.e., systems where one or more structural rule is given up (or modified). Such logics have a rich and strong tradition dating back to before Gentzen, but only with his systems could they be studied as substructural systems (rather than, say, Frege-Hilbert systems of particular sorts). In particular, intuitionistic logic, whose substructural representation involves a restriction of right-side Weakening, was formalised (in part) by Kolmogorov (1925), and later by Glivenko (1929), and Heyting (1930). Similarly, a fragment of the relevant logic **R** was anticipated by Orlov (1928). In a contemporary setting a family of relevant logics can be described by adding and subtracting structural properties (see Read 1988, p. 60 for an overview), but most standardly relevant logics are described substructurally as systems without Weakening.[36]

By giving up not only Weakening but Contraction we get to a type of system known as *Linear logic*. The name originates from Girard (1987), but similar systems had been considered earlier (a famous example is Kripke 1959). The particular contribution made by Girard's work was to introduce S4-like modalities (the *exponentials* !, ?) to recapture some of the lost structural rules in a controlled environment. Specifically, with the exponentials, both intuitionistic and classical logic can be embedded in (translated into) linear logic. More generally, applications to computer science, and the ensuing booming literature, have made Girard's contribution a milestone of the substructural revolution.

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[35]Curry has 'permutation'.

[36]Needless to say, the standard volume for relevant logic is Anderson & Belnap (1975). Note that the relevant logic **R** does not have a standard sequent calculus system, but there is a formalisation in *Display logic* (see Belnap 1982).

The definitive volumes on substructural logics are Restall (2000) and Paoli (2002). A comprehensive history of substructural logics can be found in Došen & Schroeder-Heister (1993).

1.5   Thesis Outline

The thesis is divided into two Parts: *Proof-Theoretic Harmony* and *The Semantic Role of Proof-Conditions*. The first part introduces the traditional approach to PTS, and part of the philosophical motivation for pursuing the programme (especially Dummett's philosophy, Section 2.3.1). Chapter 2 discusses the outbreak of PTS, the initial reactions to Prior's `tonk`, and the relationship between harmony as an informal concept on one side, and the related proof-theoretic notions like conservativeness (Section 2.3.2) and normalisation (Section 2.3.3) on the other. We debate and discard a number of early diagnoses of `tonk`.

Chapter 3 is a criticism of the long-standing connection between PTS and revisionism, specifically the thought that PTS motivates abandoning classical logic in favour of intuitionistic logic. We evaluate different approaches to PTS for classical logic—for example multiple-conclusion (Section 4.5) and bilateralism (Section 3.2.3)—with respect to different measures identified by the traditional PTS (as discussed in Chapter 2). Section 3.3, in anticipation of later chapters, defends a multiple-conclusion approach to PTS for classical logic against a number of objections in the literature.

Chapter 4 introduces a recent promising analysis of proof-theoretic harmony: Generalised Elimination Harmony (GE-harmony). After looking at how the investigation so far motivates a *local* approach to harmony (Section 4.2), we assess the progress made by some early versions of GE-harmony (Section 4.3). A modified account of GE-harmony is proposed in Section 4.4, with special emphasis on the

connection between the GE-template and normalisation (Section 4.4.4). Revisiting classical negation and revision, we end Chapter 4 by situating GE-harmony in a multiple-conclusion framework (Section 4.5.1), and discuss the consequences of moving proof-theoretic harmony into the substructural era. In particular, we discuss the relationship between additive and multiplicative rules and GE-harmony in Section 4.5.2.

In Chapter 5 we take a wider perspective on the connection between meaning-constitutive rules and revision of logic. In Section 5.2.1 we outline a Quinean meaning-change argument about the revision of logic, and its impact on INF. We proceed in Section 5.3 to present a theory of revision which weds INF with non-verbal dispute in about logic: minimalism about logical constants. Section 5.3.3 to 5.3.6 we detail the proof-theoretic impact on PTS. We conclude with a criticism which vindicates the Quinean charge in Section 5.4.

We take into account the difficulties identified with traditional INF, when we in Chapter 6 offer a new theory of meaning-determination. We outline a connection between MTS (truth-conditions) and PTS (proof-conditions) in Section 6.2. The proposed framework is extended to cover a range of matrix logics in Section 6.3, and in Section 6.4 we follow up by discussing how the proof-theoretic framework of PTS can be generalised to include the extended theory. In conclusion, Section 6.5 offers an interpretation of the interplay between MTS and PTS in terms of the semantics-pragmatics interface. Finally, Chapter 7 gives a summary of the new theory's impact on PTS. We also provide some thoughts on future research topics extending the present work.

Chapter 2

Harmony, Conservativeness, and Normalisation

2.1 Historical Preamble

2.1.1 Gentzen's Remark

PTS as a programme was initiated by a frivolous but brilliant remark in Gentzen (1934). Introducing his natural deduction, Gentzen says:

> To every logical symbol &, $\vee$, $\forall$, $\exists$, $\supset$, $\neg$, belongs precisely one inference figure which 'introduces' the symbol—as the terminal symbol of a formula—and one which 'eliminates' it. The fact that the inference figures &-E and $\vee$-I each have two forms constitutes a trivial, purely external deviation and is of no interest. *The introductions represent, as it were, the 'definitions' of the symbols concerned, and the eliminations are no more, in the final analysis, than the consequences of these definitions.* This fact may be expressed as follows: In eliminating a symbol, we may use the formula with whose terminal symbol we

are dealing only 'in the sense afforded it by the introduction of that symbol'. (ibid., p. 80, emphasis added)

In Gentzen (1936):

> Yet we must still keep in mind the following: the structure of a mathematical proof does not in general consist merely of a passing from *valid* proposition to other *valid* propositions via inference. It happens, rather, that a proposition is often *assumed* as valid and further propositions are *deduced* from it whose validity therefore depends on the validity of this assumption. [...] In order to describe *completely* the *meaning* of any *proposition* occurring in a proof we must therefore state, in each case, upon which of the assumptions that may have been made, the proposition in question *depends*. (ibid., p. 150)

Gentzen offers no further details about the semantic observation. However, one might say that his formalisms speak for themselves. The insistence on having separate rules for different logical constants, and giving a calculus with the Subformula Property (see Def. 2.8), were the first steps towards a proper proof-theoretic analysis of meaning.

2.1.2 Early Work

If Gentzen's work was the birth of structural proof-theory, his semantic remarks marked the inception of PTS. His insight is carried forward by a multitude of authors, but, interestingly, they are not as proof-theoretically sophisticated as Gentzen's original work. For example, Carnap—who had at least read Gentzen (1936)—endorses something like INF in his *Logische Syntax der Sprache* (1934), but the idea is not proof-theoretic in Gentzen's sense. From the *Foreword*:

Up to now, in constructing a language, the procedure has usually been, first to assign a meaning to the fundamental mathematico-logical symbols, and then to consider what sentences and inferences are seen to be logically correct in accordance with this meaning. Since the assignment of the meaning is expressed in words, and is, in consequence, inexact, no conclusion arrived at in this way can very well be otherwise than inexact and ambiguous. The connection will only become clear when approached from the opposite direction: *let any postulates and any rules of inference be chosen arbitrarily; then this choice, whatever it may be, will determine what meaning is to be assigned to the fundamental logical symbols.* (ibid., p. XV, emphasis added)

The crucial difference from Gentzen is that Carnap applies his so-called *Principle of Tolerance*, that "we have in every respect complete liberty with regard to the forms of language; that both the forms of construction for sentences and the rules of transformation [...] may be chosen quite arbitrarily". There is nothing in Gentzen that indicates that he believes that *arbitrary* inference rules can serve a definitional purpose. On the contrary, Gentzen's writing on the topic strongly indicates that he wanted the paring of intro- and elim-rules to be disciplined by some functional constraint. We return to this in Section 4.2.2.

Similarly, other authors in the inferentialist tradition spend little or no time discussing what sorts of adequacy conditions must be put on inference rules for them to be taken as meaning-conferring. The most famous, perhaps, is a series of programmatic papers by Popper (1946, 1947, 1948, 1949). Popper aims to reconceive the foundations of logic by offering 'inferential definitions' which lead to a new justification of deduction. He suggests that the class of 'formative signs'—logical constants—can be demarcated by inferential definitions. Validity, in turn, is defined by reference to these formative signs.

Popper's theory met with massive criticism in contemporary reviews, e.g., by Curry, Beth, and Kleene.[1] Popper later abandons the project and declares that drawing a sharp line between logical and non-logical expressions is most likely impossible.[2] Nevertheless, Popper's time was perhaps not spent in vain. Together with Kneale's 'The Province of Logic' (1956), his work was the main inspiration for A. N. Prior's famous polemic about `tonk` (see below). Kneale, who proposes a multiple-conclusion version of Gentzen's natural deduction, says that "rules which we can treat as definitions [...] determine the sense of the signs completely by fixing their roles in argument" (ibid. p. 257). Kneale and Popper have in common that they explicitly draw on Gentzen's proof-theory, but the tendency is the same as that of Carnap's work: There is no systematic discussion of whether or not some conditions must be satisfied by the rules in order for them to determine meaning.

2.2 Two Traditions After `Tonk`

2.2.1 The Year `Tonk` Broke

For Prior, this is carelessness that spells disaster. Referring to the above work by Popper and Kneale, he offers the ironic gem 'The runabout inference-ticket', which Nuel Belnap (1962) rightly compared to another rhetoric masterpiece, Lewis Carroll's 'What the tortoise said to Achilles' (1895). Surely, Prior's paper, even if written half a century after Carroll's, has had a similar astonishing effect on the philosophy of logic. Not bad for two pages.

Prior's concern is with what he calls *analytic validity*:

It is sometimes alleged that there are inferences whose validity arises solely from the meanings of certain expressions occurring in them. The

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[1]An extensive list of reviews and comments can be found in Schroeder-Heister (1984b).

[2]Schroeder-Heister (1984b, p. 80) reports that Popper's main reason for giving up his work on logic was Tarski's disinterest in the project. Schroeder-Heister's paper is actually a systematic reworking of Popper's project, albeit without some of the philosophical pretensions.

precise technicalities employed are not important, but let us say that
such inferences, if any such there be, are analytically valid. (ibid., p.
38)

As an example he takes $\wedge$-I and $\wedge$-E, two rules which are remarkably transparent.
He continues by mimicking Popper and Kneale:

Anyone who has learnt to perform these inferences knows the meaning
of 'and', for there is simply nothing more *to* knowing the meaning of
'and' than being able to perform these inferences. (ibid.)[3]

But if this is the case, Prior suggests, then why is it not equally legitimate to
enrich our language with a new logical constant—`tonk`—whose meaning is given
by the following inference rules?

Example 2.1. `Tonk`, natural deduction:

$$\frac{A}{A\,\texttt{tonk}\,B}\;(\texttt{tonk}-I) \qquad \frac{A\,\texttt{tonk}\,B}{B}\;(\texttt{tonk}-E)$$

Alternatively, in sequent calculus, where $R\texttt{tonk}$ corresponds to $I\texttt{tonk}$, $L\texttt{tonk}$ to
$E\texttt{tonk}$:

Example 2.2. `Tonk`, sequent calculus:

$$\frac{\Gamma, B \Rightarrow \Delta}{\Gamma, A\,\texttt{tonk}\,B \Rightarrow \Delta}\;L\texttt{tonk} \quad \frac{\Gamma \Rightarrow A, \Delta}{\Gamma \Rightarrow A\,\texttt{tonk}\,B, \Delta}\;R\texttt{tonk}$$

Prior's 'answer': There is no difference between the cases. In a language with
tonk one can legitimately infer B from A, for any A, B, since both `tonk`-rules
are 'analytically valid'. Accordingly, doubting the validity of `tonk`-rules would be

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[3]Note the epistemological vein of the quote.

as nonsensical as doubting $\wedge$-I and $\wedge$-E. If the rules are somehow definitional, doubting their validity is only a failure of nerve. Prior does nothing to dispel the apparently paradoxical upshot. He simply ends with sarcastic praise of the newly discovered connective, "which promises to banish *falsche Spitzfindigkeit* from Logic for ever" (ibid., p. 39). The reader is left to appreciate the irony.

The connective `tonk` deserves a closer look in anticipation of what is to follow. In Prior's original formulation, `tonk` is a hybrid between disjunction and conjunction, with one $\vee$-I rule and one $\wedge$-E rule. Of course, adding the other $\vee$-I and $\wedge$-E will still yield an undesirable outcome, but, importantly, it is not the very same connective (even if it gives the same consequence relation in certain contexts). For one, `tonk` enhanced with the addition rules—call it `tonk`$^+$—behaves differently with respect to proof-theoretic harmony and categoricity (see Chapter 4 and Chapter 6). It remains the case, however, that in a system with `tonk`-rules, any formula is derivable from any non-empty set of premises, given that the system is *transitive*, i.e., for every A, B, C in the language, if $A \vdash B$ and $B \vdash C$, then $A \vdash C$. The result is a *trivial* consequence relation: For every pair $<\Gamma, A>$ where Γ is a non-empty set of formulae and A is a formula, $\Gamma \vdash A$.[4]

It is sometimes said that tonk leads to inconsistency, but this is a claim that needs qualification. In a system consisting only of the `tonk`-rules there will be no theorems, since none of the rules discharge assumptions, and there are no axioms. Thus, in particular, the system is not *Post inconsistent* (or *absolutely inconsistent*), i.e., there is a formula ϕ such that $\nvdash \phi$. In fact, any formula satisfies this condition.

Of course, Prior's moral is not diminished by this observation. It is bad enough that the consequence relation is trivial in the above sense. And anyway, in the environment of most negations we get Post-inconsistency as well. For example,

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[4]Shoesmith & Smiley (1978, pp. 248-249) calls this type of calculus—where $A \vdash B$ for every A, B—a *singular calculus*. Calling the tonk-induced $\vdash$ trivial might be a bit misleading since it does require a non-empty premise set.

adding an intuitionistic negation governed by EFQ and RAA, the following derivation suffices to prove any formula:

$$\cfrac{\cfrac{\cfrac{\cfrac{[A]^1}{Atonk\bot}}{\cfrac{\bot}{\neg A}}\ (RAA)_1 \qquad \cfrac{\cfrac{[\neg A]^2}{\neg Atonk\bot}}{\cfrac{\bot}{\neg\neg A}}\ (RAA)_2}{\cfrac{\bot}{\ }}\ (\neg E)}{A}\ (EFQ)$$

Classical negation will make the derivation even more immediate with CRA (or DNE). Similarly, we cannot prove $\ulcorner \Rightarrow A \urcorner$ for any A in a sequent calculus with only tonk rules. (It is quite easy to see that we cannot derive any sequent with an empty antecedent side.) Adding $L\bot$ and $R/L\neg$, however, yields:

$$\cfrac{\cfrac{\cfrac{\cfrac{\bot \Rightarrow}{\Rightarrow \neg\bot}}{\Rightarrow \neg\bot tonk\bot} \qquad \cfrac{\bot \Rightarrow}{\neg\bot tonk\bot \Rightarrow}}{\Rightarrow}}{\ }\ (Cut)$$

Note that this is again an intuitionistically valid derivation; there is never more than one member on the succedent side of the sequents. Alternatively, here is a classical derivation which uses both negation rules plus contraction, but not $L\bot$. Note that this derivation uses the tonk$^+$ rules.

$$\cfrac{\cfrac{\cfrac{\cfrac{\cfrac{A \Rightarrow A}{\Rightarrow \neg A, A}}{\Rightarrow \neg A, Atonk\neg A}}{\Rightarrow Atonk\neg A, Atonk\neg A}}{\Rightarrow Atonk\neg A} \qquad \cfrac{\cfrac{\cfrac{\cfrac{A \Rightarrow A}{\neg A, A \Rightarrow}}{\neg A, Atonk\neg A \Rightarrow}}{Atonk\neg A, Atonk\neg A \Rightarrow}}{Atonk\neg A \Rightarrow}}{\cfrac{\Rightarrow}{\Rightarrow A}}\ (Cut)$$

Needless to say, given Post-inconsistency the systems will always be *simply inconsistent*, i.e., there is a ϕ in the language such that $\vdash \phi$ and $\vdash \neg\phi$.

2.2.2   Diagnosis of `Tonk`: Part I

Why should the proof-theoretic semanticist fret about `tonk`? No one would contemplate introducing `tonk` in an evironment that trivializes the consequence relation. Everyone agrees that `tonk` is defective in standard systems. Adding a `tonk`ish connective is, in Belnap's words "like adding to cricket a player whose role was so specified as to make it impossible to distinguish winning from losing" (Belnap 1962).

We need to investigate what the moral of Prior's `tonk` is. Prior himself offers little in the way of a closing statement on the issue, but, fortunately, a series of authors have taken up the challenge left by `tonk`. One early diagnosis is offered by Stevenson (1961). Stevenson is not averse to Popper and Kneale's claim that the meaning of logical constants is given by their inferential role (i.e. INF), but he thinks that they have too hastily concluded that "we can *completely* justify an inference by appealing to the meaning of a logical connective as stated in permissive rules" (ibid., p 125). Rather, justification of an inference involves two steps: *validation* and *vindication*. The former is given by the inference rules for the connective in question—thus `tonk` is valid—but the latter, vindication, requires a *soundness proof*, i.e., a demonstration that the meaning-conferring rules preserve truth-in-a-model. `Tonk`-*I* and `tonk`-*E* clearly do not, hence there is no vindication.[5]

Wagner (1981) has a proposal along similar lines, but with a different conclusion. According to Wagner, Prior's worry was not merely that `tonk` fails on account of truth-preservation, but more fundamentally, that it is semantically defective.[6] Against this, Wagner suggests that `tonk` is not *meaningless*. The thesis INF is correct in so far as the inferential role of the logical connectives determines their *sense*, but determining sense might come short of determining a *denotation*.

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[5]Actually, the 'clearly not' here is a bit hasty: `tonk` might be sound given a non-Boolean semantics. We return to this in Chapter 6.

[6]Precisely what Prior thought about this, however, is a vexed issue.

> Once we distinguish between sense and denotation for connectives,
> just as we do for names and predicates, Prior's argument breaks down.
> Although it is unclear what, in general, senses are, let us suppose that
> specifying the logical role we take a connective to have might (at least
> partway) specify its sense. [...] Turning to denotation, we can now
> say that a connective c denotes a truth function f if and only if f
> conforms in the obvious way to the sense of c. [...] If, on the other
> hand, no truth function reflects our use of c, c lacks a denotation.
> We are unaccustomed to speaking of denotationless connectives, which
> may not exist in natural language, but there is nothing paradoxical
> about them (ibid., p. 293).

There is a sense in which both Stevenson and Wagner break with the spirit of INF
(not that they were concerned with upholding it). On their view the legitimacy
of a connective is inevitably tied up with its truth-conditional semantics, and a
motivation for going with inferential roles in the first place is typically that the
truth-conditional approach is unhelpful.[7] With Stevenson in particular, it becomes
unclear what sort of work INF is doing. Why not take it that the 'validation' is a
complete justification of inference rules?

There is some more mileage in Wagner's approach. After all, Wagner thinks that
the rules (even the `tonk`-rules) are *sense-determining*, and so the inferential role is
still wearing the trousers. Further, the denotation of a connective λ must *conform*
to the sense of λ. So, it is ultimately the inferential role that determines the
denotation of a logical connective. In fact, the Fregean notion of sense is precisely
the ingredient of an expression's meaning that determines its semantic value, and
also what one grasps when one understands the expression. This may not be
palatable for the inferentialist who worries about truth-conditional *reification* of
meaning, but it still appears perfectly consistent with INF as formulated above.

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[7]Although the exact complaints against truth-conditional semantics vary from author to author. See for example Dummett (1991, ch. 3, ch. 14), Prawitz (1977), and Read (1988, ch. 9).

Nonetheless, one might think that even if Wagner's approach is consistent with INF, it fails on independent grounds. First, to the extent that the theory involves a sense vs denotation distinction, it introduces some familiar worries with a Fregean framework. Wagner is quick to dismiss this by saying that "its essence should survive whatever revisions of Frege might be necessary", but no discussion of this is provided.[8] Second, it is all too evident that by 'denotation', Wagner has in mind Boolean truth-functions. But in line with the consideration that PTS ought to be independent of any particular logical system, the notion of denotation ought to be as general as the corresponding notion of sense given by proof-theoretic rules. The obvious amendment is to consider appropriate denotations for (first-order) quantifiers and (normal) modalities; but any full-blooded theory also ought to include non-Boolean truth-functions. Third, even if we can get the scope of possible denotations right, we are still in the dark about what it is for a denotation to conform to the sense. Or, in other words, how do sense-determining inference rules in turn determine denotations?

For now we leave these problems behind, but we return to them in detail in Chapter 6. There we will argue for a resurrection of Wagner's line, albeit with a more precise understanding of the relationship between proof-conditions and truth-conditions. In what follows, however, we will mostly focus on the strictly proof-theoretic approach to `tonk`. Is there any hope of applying proof-conditional constraints to rule out `tonk` and other rogue connectives?

2.2.3   Diagnosis of `Tonk`: Part II

Arguably, the most influential reply to Prior's challenge is due to Belnap (1962). Like Wagner, Belnap takes Prior's main conclusion to be that "there is no meaningful proposition expressed by A-*tonk*-B" (ibid., p. 131), and, like Wagner, he does

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[8]Another Fregean take on PTS can be found in Hodes (1984).

not agree. Belnap thinks the moral drawn from `tonk` must be modified: The problem is not with INF *per se*, but with the notion that *any* set of inference rules can consistently be introduced in to a logic, and thus serve as a meaning-determining practice. Such a naïve inferentialism severely underestimates structural assumptions that are already in place when the rules for a connective are introduced.

> It seems to me that the key to a solution lies in observing that even on the synthetic view, we are not defining our connectives *ab initio*, but rather in terms of an *an antecedently given context of deducibility*, concerning which we have some definite notions. By that I mean that before arriving at the problem of characterizing connectives, we have already made some assumptions about the nature of deducibility.

For `tonk`, one obvious structural assumption about the antecedent context of deducibility is *transitivity*, without which the problem does not arise.[9] Upon accepting that such assumptions are in place prior to the introduction of a logical connective, it also becomes evident that careless use of 'defining' inference rules will lead to inconsistency. In a word, intro- and elim-rules must be tempered by the antecedent context of deducibility. The trick is to identify the precise conditions under which a logical connective can be introduced into the language (and its rules into the logic), without upsetting the antecedent structural assumptions about deducibility.[10]

Put differently, and closer to Belnap's own terminology, the idea is to find the *existence conditions* for a logical constant. If a connective λ fails on this account, no proposition is expressed by $\ulcorner A \ \lambda \ B \urcorner$.[11]

What is Belnap's proposal? Any logical constant is introduced on the backdrop of an antecedent context of deducibility, i.e., in the framework of a consequence

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[9]This is explicitly recognisable in the sequent calculus derivation as application of the cut rule.

[10]This is similar to the approach favoured in Hacking (1979).

[11]Alternatively, one might think that such rules would still determine meaning, but that the connective in question would not be a *logical* connective.

relation with certain properties. For now, let us say that a consequence relation is simply a relation between a set of well-formed formulae and a well-formed-formula. Clearly, most such relations will not be interesting consequence relations since so far, any relation is allowed. When Belnap talks about an antecedent context of deducibility he has in mind *ordinary consequence relations*, i.e., consequence relations for which reflexivity, weakening, contraction, and transitivity hold.[12]

With that assumption, Belnap introduces the following definitions. Let $WFF_{\mathcal{L}}$, $WFF_{\mathcal{L}^+}$ be the set of well-formed formulae of languages $\mathcal{L}$ and $\mathcal{L}^+$ respectively. $L = \langle \vdash, WWF_{\mathcal{L}} \rangle$ and $L^+ = \langle \vdash^+, WFF_{\mathcal{L}^+} \rangle$ are logical systems where $\vdash$, $\vdash^+$ are consequence relations.

Definition 2.1. The logical system L^+ is an *extension* of L iff

1. $\mathcal{L} \subseteq \mathcal{L}^+$;

2. $WFF_{\mathcal{L}} \subseteq WFF_{\mathcal{L}^+}$, and;

3. $\vdash \; \subseteq \; \vdash^+$.

Definition 2.2 (Conservative extension)**.** The logical system L^+ is a *conservative extension* of L iff

1. L^+ is an *extension* of L, and;

2. if $\Gamma \subseteq WFF_{\mathcal{L}}$ and $A \in WFF_{\mathcal{L}}$, then whenever $\Gamma \vdash_{L^+} A$, $\Gamma \vdash_L A$.[13]

Note that it follows from this that any conservative extension of a Post-consistent logical system will itself be Post-consistent. Of course, if the original logical system is Post-*inconsistent*, then any extension is a conservative extension. An example: As the above proof shows, in natural deduction, intuitionistic negation is non-conservative over the system with `tonk` as its only connective. It is important to

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[12]The terminology is from Avron (1994). Belnap includes exchange, but we leave this out as it is mostly uninteresting for present purposes.

[13]According to Belnap, the notion of conservative extensions first appears in work by Emil Post.

appreciate that conservativeness is a property that is highly framework-relative: E.g., natural deduction and sequent calculus does not always yield the same result. (One example—*Peirce's Law*—will be discussed shortly.) Further, as the example of `tonk` has taught us, the presence of structural rules—e.g., transitivity—is also relevant for the result.

Example 2.3 (Tonk). Let $\vdash$ be a (standard) natural deduction deducibility relation such that there is a pair $<A,B>$ for which $A \nvdash B$. Then adding `tonk` to the language and `tonk`-I, `tonk`-E to the logic, produces a non-conservative extension of $\vdash$.

This, Belnap thinks, explains why INF works for conjunction (with $\wedge$-I, and $\wedge$-E) but not for `tonk`. Non-conservativeness tells us that `tonk`-rules cannot be taken to determine the meaning of a connective in the context of an ordinary consequence relation. However, there is a certain ambiguity in Belnap's original proposal. It appears that he takes the conservativeness test to be limited to adding logical constants to a logical system with no logical constants already in the language. In other words, the test is one of conservativeness *with respect to structural properties of the deducibility relation* (i.e., an ordinary consequence relation), not with respect to the antecedent language. As non-conservativeness might arise from the proof-theoretic interplay between two connectives (where both are conservative over the antecedent deducibility relation), these two formulations of the test are non-equivalent. In fact, as it turns out, it is the latter formulation that has prevalence in the PTS literature.

Further, Belnap suggests that even if we are in a position to demonstrate the existence of a connective (via conservativeness), it does not follow that the connective is *unique*.[14] That is, we want to make sure that

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[14]Conservativeness and uniqueness are treated in some detail in Došen & Schroeder-Heister (1985) (see also Smiley 1962). But the definite account is in Humberstone (2008). Greg Restall (2007) is a discussion of Belnap's two criteria and the context of deducibility.

> [...] there cannot be two connectives [plonk, plink] which share the
> characterization given to plonk but which otherwise sometimes play
> different roles. Formally put, uniqueness means that if exactly the
> same properties are ascribed to some other connective, say *plink*, then
> A-*plink*-B will play exactly the same role in inference as A-*plonk*-B
> both as premiss and as conclusion. (Belnap 1962, p. 133)

Let λ and λ' be two logical connectives governed by identical inference rules, except with λ and λ' respectively. We can then ask whether or not $F(\lambda)$ is interderivable with $F(\lambda')$, where $F(\lambda')$ is the result of of substituting an occurrence of λ' for every occurrence of λ in $F(\lambda)$.

The criterion of uniqueness will not feature prominently in what follows, however. It is conservativeness that has achieved the most attention in the literature on PTS, and, consequently, conservativeness will serve as a starting point for an analysis of *proof-theoretic harmony*. More than Belnap himself, it is Dummett who has brought conservativeness to philosophical prominence in the inferentialist camp. Before we explore more sophisticated approaches to `tonk`, we pause to understand some of the broader motivation for adopting constraints such as conservativeness.

2.3   Dummett on Harmony

2.3.1   Philosophical Backdrop

Belnap does little towards motivating conservativeness as *the* constraint on legitimate meaning-conferring rule-sets. True, the conservativeness test does rule out `tonk`, and a range of `tonk`ish connectives, but even if we grant that conservativeness is necessary—which it might not be—why think that it is *sufficient*? In other words, are there illegitimate connectives that are not weeded out by the conservativeness test.

Before we assess the formal prowess of the conservativeness test, let us introduce a more detailed account of the relationship between conservativeness and meaning provided by Dummett. His *The Logical Basis of Metaphysics* (1991) is indisputably the most systematic attempt at deploying proof-theoretic resources to give a theory of meaning for logical constants. In fact, Dummett casts his net much wider. His aim is to characterise conditions for successful linguistic practice in natural language *tout court*. It just happens that on Dummett's view, the logical constants lend themselves to a rigid analysis because of their systematic nature.[15] In particular, through their participation in deductive argument, logical constants are "connected at start and finish with the ordinary assertoric use of language" (ibid., p. 193). With the hope that the semantically significant part of our linguistic practice for logical constants is exhausted by the inferential practice, Dummett proposes to study the constraints on successful practice in a proof-theoretic setting.

Why think that properties of inference rules are a good guide to the semantics of logical constants? Dummett offers the following meaning-theoretic backdrop: In general, linguistic practice is conditioned by two broad classes of principles:

- *Verificational principles*;

- *Pragmatic principles.*

The first class of principles control when we are *entitled* to make an assertion, and under what conditions we are required to acknowledge it. The second class of principles govern what sort of *commitments* an assertion engenders through its consequences.[16] Earlier, in Dummett (1973a), he says:

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[15]That is not to say that there is no inferential noise in natural language connectives like 'and', 'or', 'not', etc. Any attempt to formalise inferential behaviour must accept some major concessions in comprehensiveness. For a discussion of the pragmatics of logical connectives, see Edgington (2006).

[16]Brandom (1994, 2000) has developed the idea of entitlement and commitment as core semantic notions, set in the framework of a Sellarsian *game of giving and asking for reasons*. Brandom suggests that we ought to understand the notion of assertion derivatively from such a game. See also Brandom (1983).

> Learning to use a statement of a given form involves, then, learning two things: the conditions under which one is justified in making the statement; and what constitutes acceptance of it, i.e. the consequences of accepting it. (ibid., p. 453)

Dummett rightly observes that, in general, identifying such principles in our linguistic practice is a tall order. However, he adds that, fortunately, logical constants are governed by principles that may plausibly be spelled out in a natural deduction framework. More precisely, verificational principles for a logical constant λ are given by the set of λ-intro rules; the pragmatic principles by the set of λ-elim rules (see in particular Dummett 1991, p. 216). If this is correct, the natural extension of the thought is to mimic the constraints on verificational and pragmatic principles with proof-theoretic constraints on natural deduction rules. For this purpose Dummett adopts conservativeness.

It is in the context of verificational and pragmatic principles that the term 'harmony' is first coined by Dummett.[17] He stresses the possibility of a malfunctioning linguistic practice, that is, a practice where these principles are somehow at odds with each other. Inconsistency, Dummett suggests, is the 'grossest' type of such malfunction, but there are a number of ways in which the practice might semantically misfire. In other words, a linguistic practice might subtly fail to deliver a semantic content to an expression. It is in the presence of such malfunction that it is legitimate to *criticise* and, potentially, *revise* an established linguistic practice (and, thus, an inferential practice).[18] This can happen for instance in cases where a discourse turns out to be *paradoxical* (e.g., for a naïve truth predicate), but also

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[17]As far as we know, the term makes its first appearance in Dummett (1973a, p. 454): "Such change [in the linguistic practice] is motivated by the desire to attain or preserve a harmony between the two aspects of an expression's meaning."

[18]The issue of revision of logic will be treated in some detail in Chapter 5.

less visibly if there is failure of *harmony* at the level of the principles governing the practice.[19]

> The possibility of failure arises primarily because of the multiplicity of principles governing our linguistic practice. For the language to function as intended, these principles must be in harmony with one another; but the mere fact that certain principles are observed in no way guarantees that the necessary harmony will obtain. (ibid., p. 210)

Thus, harmony is a relation between verificational and pragmatic principles. For logical constants, in particular, it is a relation between a set of intro-rules and a set of elim-rules. Interestingly, even if harmony is a hopelessly many-faceted relation for semantic principles in general, Dummett thinks it can be given an elucidating formal characterisation when we restrict our attention to deductive arguments and the accompanying inferential practice.[20]

> [A]lthough we have no right to assume it a priori, we may at least hope that, in their case [logical constants], the matter can be treated entirely in terms of logical laws. (ibid., p. 215)

An informal idea of harmony is now taking shape. Dummett is suggesting that intro-rules and elim-rules must somehow be balanced off against each other. It is undesirable—and, according to Dummett, semantically undermining—to have rules for a logical constant λ such that the consequences that can be drawn with the elim-rules outstrip the grounds for introducing the formulae with the intro-rules; or, *vice versa*, elim-rules that will not allow you to wholly recapture those grounds.

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[19]Dummett contrasts his view—which is comparable to Belnap's view about tonk—with the late Wittgensteinean approach where a linguistic practice is immune to revision, even in the face of inconsistency (see ibid. p. 209).

[20]One of the few non-logical cases Dummett discusses is the slur 'Boche', a pejorative term for Germans. See Dummett (1973a, p. 454). This case also receives some interesting treatment in Williamson (2003).

In the first case, the intro-rules are too weak (or alternatively, the elim-rules too strong); in the second, the intro-rules too strong (alternatively, the elim-rule too weak). Either way, the equilibrium is upset—the verificational and pragmatic principles are in tension.

Obviously, `tonk` is a good candidate for a connective that is *disharmonious* in this informal sense. Indeed, `tonk` appears to allow consequences "not warranted by our methods of arriving at the premises" (ibid., p. 217). More, given the assumptions discussed above, `tonk` is the limit case where the disharmony is so overwhelming as to trivialise the practice. Yet, it should also be clear from the discussion that it is perfectly possible to have a disharmonious logical constant that does not trivialise. First, the elim-rule might be just *slightly* stronger than what is licensed by premises in the intro-rules.[21] Second, the elim-rules might be too weak, and thus obviously not trivialising. Although Dummett says that this latter disharmony "will not produce so deleterious an effect" (ibid.), the two ways in which the equilibrium can be upset will be taken equally seriously in what follows. It is not evident why one would think that rules which 'undergenerate' conclusions are any less harmful than rules that 'overgenerate'.

2.3.2   Harmony-As-Conservativeness

As we have seen, Dummett proposes to interpret intro- and elim-rules as verificational and pragmatic principles respectively, and, furthermore, conservativeness as harmony for logical constants.

> The best hope for a more precise characterisation of the notion of
> harmony lies in an adaption of the logicians' concept of a conservative
> extension. (ibid., p. 218)

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[21]The talk of deductive strength applied here should be taken in a loose sense. To say, *simpliciter*, that a rule is (deductively) stronger than another can be made precise, but any grading of the strength will vary radically with the comparison class. In a word, 'slightly stronger' must be taken *cum grano salis*.

But why is it that conservativeness is an appropriate formal rendering of the informal harmony requirement? Dummett offers an argument:[22] Let $\mathcal{L}$ be a language that contains the logical connective λ (possibly with other connectives), and the corresponding logical system L contains its inference rules. Assume that the grounds for asserting λ statements are disharmonious with respect to its consequences, i.e., the $E\lambda$ rules outstrip the grounds given by the $I\lambda$ rules. In the presence of λ, there will be (given some minimal assumptions abouts its rules) arguments that are valid which were not valid in the λ-free language. The question is if all of these are arguments involving λ. Dummett reasons as follows: Since the λ-rules are disharmonious, there must be some consequence of a λ-statement that is not a consequence of the grounds for asserting the λ-statement. Such a consequence cannot itself be a λ-statement, Dummett concludes, "for the drawing of *such* conclusions must count as part of our conventions governing the justification of assertions involving [λ]" (ibid.).

In a word, this is the idea we will call *harmony-as-conservativeness*: "there is harmony between the two aspects of the use of any given expression if the language as a whole is, in this adapted sense, a conservative extension of what remains of the language when that expression is subtracted from it" (ibid., p. 219).

Dummett goes on to label conservativeness-as-harmony *total harmony* (or *harmony in context*) indicating that this global notion of harmony is "in a high degree relative to the context, that is, the base theory to which the addition is being made" (ibid., p. 251). This is not unproblematic: Assume that the context of deducibility already allows $\Gamma \vdash A$ for each Γ, A. Introducing `tonk`-rules in this context is perfectly conservative. Worse, non-conservativeness is elusive even in more interesting cases. A famous example is Peirce's Law $\ulcorner((A \to B) \to A) \to A)\urcorner$ which is a classical tautology but intuitionistically invalid.[23] For Dummett it is

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[22]For details, see ibid., p. 218.

[23]Alternatively, *Peirce's Rule*:

$$\frac{(A \to B) \to A}{A} \ (PL)$$

of meaning-theoretic significance that classical negation cannot be conservatively added to a standard natural deduction proof-system without negation. In fact, in the standard setting the positive fragment of full classical logic is not the classical fragment but the intuitionistic fragment.

> The addition of negation [to the positive fragment], subject to the classical rules, does not produce a conservative extension; rather, it enables us to derive the whole wide range of classical laws that do not involve negation but are intuitionistically invalid. (ibid., p. 291)

However, this is a presentation-dependent fact: There are sequent calculus proof-systems where the extension is perfectly conservative. Consider **G1c**, the sequent calculus for classical logic with explicit structural rules. In the positive fragment of this system we can construct the following derivation of Peirce's Law:

$$\frac{\dfrac{\dfrac{\dfrac{A \Rightarrow A}{A \Rightarrow B, A} \; (K)}{\Rightarrow A \to B, A} \; (R\to) \qquad A \Rightarrow A}{\dfrac{(A \to B) \to A \Rightarrow A, A}{(A \to B) \to A \Rightarrow A} \; (W)} \; (L\to)}{\Rightarrow ((A \to B) \to A) \to A} \; (R\to)$$

One might ask why it is that this derivation is possible when **G1c** shares its operational rules and structural rules with the intuitionistic counterpart **G1i**. The short answer is that it makes a difference whether these rules are embedded in a multiple-succedent calculus or a single-succedent calculus.[24] The classical derivation hinges on the application of right-side contraction and weakening, steps which would not be allowed in the intuitionistic system. There, the succedent multiset must be either a singleton or the emptyset. (See Appendix A.11)

Similarly, if classical negation is added to the $\{\to, \vee\}$-fragment, we can prove $\ulcorner (((A \to (B \vee C)) \to ((A \to B) \vee (A \to C)) \urcorner$ as a theorem.

[24] It is important to realise, however, that also intuitionstic logic has a multiple succedent sequent calculus, m-**G3ip**. The trick is to restrict the operational rules for $\to$ in the classical calculus **G3cp**. See Troelstra & Schwichtenberg (2000, p. 82-3) for details.

Once this observation has been made, Read (2000) offers us the 'translation' back into natural deduction. For why not think that multiple-conclusion can do the same job in a natural deduction framework as it does in a sequent calculus framework? In fact, such a system was formulated in Boričić (1985).[25] **NC** is a normalisable natural deduction system for classical logic. As requested, Peirce's Law is provable in the positive fragment of **NC** (see Appendix A.9 for the details on the system):

$$\dfrac{\dfrac{\dfrac{[A]^{(1)}}{A, B}\ (K)}{\dfrac{A, A \to B}{}\ (I\to)^1 \qquad [(A \to B) \to A]^{(2)}}{\dfrac{\dfrac{A, A}{A}\ (W)}{((A \to B) \to A) \to A}\ (I\to)^2}\ (E\to)}$$

Indeed, in **NC** there is no argument in the negation-free fragment that can only be proved with the help of the negation-rules. Read correctly concludes that Dummett's argument from non-conservativeness is in a certain respect an artifact of the formal presentation. Of course, the revisionist intuitionist might alter strategy and claim that multiple-conclusion systems are bad for some reason or other. In fact, Dummett independently argues that this is the case. We return to the question of the legitimacy of multiple-conclusion in Section 3.3.

Another couple of well-known observations also tell against harmony-as-conservativeness. First, Dummett himself admits that conservativeness is a lopsided test: It only identifies one type of disharmony, namely where the elim-rules are too strong (or the intro-rules too weak). The reverse case, where the intro-rule is too strong, does not necessarily lead to non-conservativeness. To see this, consider `tonk`'s near relative `tunk`:[26]

$$\dfrac{A \quad B}{A\,\texttt{tunk}\,B}\ (I\texttt{tunk}) \qquad \dfrac{A\,\texttt{tunk}\,B \quad \overset{[A]^u}{\vdots}\ \dot{C} \quad \overset{[B]^u}{\vdots}\ \dot{C}}{C}\ (E\texttt{tunk})(u)$$

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[25]Boričić's system is based on earlier work in von Kutschera (1962).

[26]I first became aware of the connective `tunk` in conversations with Crispin Wright.

The pair of rule-sets is disharmonious (it is the dual case of `tonk`), but do not yield non-conservative extensions. (The formal reason for this will become clear below in the discussion about conversion steps.) Again, conservativeness is not a sufficient test for harmony in our informal sense.[27]

Second, Read (2000) observes that Dummett's argument for conservativeness only shows that it is *sufficient* for harmony, not that it is necessary. What we would really like is a formal analysis of harmony that is both necessary and sufficient. In other words, we need the additional question of whether harmony entails conservativeness: Dag Prawitz (1994) has suggested that the answer is 'no' by providing a counterexample involving incompleteness and a truth predicate. Here are the rules for the naïve truth predicate (where A itself may involve occurrences of the truth-predicate):

$$\frac{A}{Tr(\ulcorner A \urcorner)} \; (ITr) \qquad \frac{Tr(\ulcorner A \urcorner)}{A} \; (ETr)$$

Adding these rules to Peano arithmetic (**PA**), and allowing instances of it in the induction axiom schema, yields a non-conservative extension since the Gödel sentence becomes provable.[28] Since it appears unworkable to deny the harmony of the inference rules for the naïve truth predicate, Prawitz concludes that harmony does not entail conservativeness.[29]

Between the above considerations it appears unlikely that harmony-as-conservativeness is an adequate analysis of the constraint we want. Nevertheless, we will see in what follows that traces of this first analysis can be found in improved formalisations of harmony. To see this connection we need to look at the second notion of harmony proposed by Dummett.

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[27]Dummett is aware of this and turns to this issue under the heading 'stability' later in the *The Logical Basis of Metaphysics* (see ch. 13). Dummett never gives a formal criterion for stability, but most of the later writings on harmony can be seen as an attempt to spell out this requirement in precise proof-theoretic terms.

[28]For details, see Shapiro (1998a) and Ketland (1999).

[29]However, this much cited 'counterexample' has come under recent criticism in Steinberger (2009, ch. 2.7). He argues that the culprit is not the truth-predicate alone, but the truth-predicate together with the term-forming operator for quotation, $\ulcorner \urcorner$.

2.3.3   Harmony-As-Normalisation

Dummett constrasts total harmony with *intrinsic harmony*, a harmony constraint which, supposedly, more directly deals with the relationship between intro- and elim-rules. The requirement is inspired by Prawitz's *normalisation theorem*, or what Dummett calls the 'levelling of local peaks':

> The analogue, within the restricted domain of logic, for an arbitrary logical constant **c**, is that it should not be possible, by first applying one of the introduction rules for **c** and then immediately drawing a consequence from the conclusion of that introduction rule by means of an elimination rule of which it is the major premiss, to derive from the premisses of the introduction rule a consequence that we could not otherwise have drawn. Let us call any part of a deductive inference where, for some logical constant **c**, a **c**-introduction rule is followed immediately by a **c**-elimination rule a 'local peak for **c**'. Then it is a requirement, for harmony to obtain between the introduction rules and elimination rules for **c**, that any local peak for **c** be capable of being levelled, that is, that there be a deductive path from the premisses of the introduction rule to the conclusion of the elimination rule without invoking the rules governing the constant **c**. (Dummett 1991, p. 247-48)[30]

In order to get a sufficient grasp of what Dummett is after we need to chase down the origin of the idea of intrinsic harmony. Dummett himself explicitly refers to Prawitz (1965), and the formulation of the normal form of proofs.[31] Prawitz's brilliant doctoral thesis contained a key result in the development of structural

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[30]An aside: It appears that Dummett strictly speaking requires *the existence of normal form* which is a weaker property than normalisation, i.e., a procedure for producing a normal form of any derivation.

[31]More in particular, Dummett seems to work on the backdrop of Prawitz (1973, 1974).

proof-theory. The so-called normalisation proof was an analogue to Gentzen's *Hauptsatz*, the proof of cut-elimination for sequent calculus.

Prawitz's idea was to formalise an *inversion principle* for natural deduction systems, and apply the local principle for rules to prove a global result: Normalisation. In his wording:

> Observe that an elimination rule is, in a sense, the inverse of the corresponding introduction rule: by an application of an elimination rule one essentially only restores what had already been established if the major premiss of the application was inferred by an application of an introduction rule. This relationship between the introduction rules and the elimination rules is roughly expressed by the following principle, which I shall call the *inversion principle*:

> *Let α be an application of an elimination rule that has B as consequence. Then, deductions that satisfy the sufficient condition [...] for deriving the major premiss of α, when combined with deductions of the minor premisses of α (if any), already "contain" a deduction of B; the deduction of B is thus obtainable directly from the given deductions without the addition of α.* (Prawitz (1965, p. 33)

The term 'inversion principle' (*Inversionprinzip*) was coined in Lorenzen (1950, 1955). Lorenzen was presumably the first to formulate the idea that a natural constraint on inference rules for a logical constant λ is that whenever some formula C follows from the grounds for introducing a λ-formula, then C ought to follow from the λ-formula directly.[32] This idea anticipates two different later developments: First, there is a sense in which it indicates that we can avoid pointless detours produced by first introducing a λ-formula only to immediately eliminate

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[32]For details on Lorenzen's inversion principle and his *Konsequenzlogik*, see Moriconi & Tesconi (2008).

it. Second, it hints at a method for *inducing* elim-rules from intro-rules. We turn to the former idea first, and Prawitz's explications of it. Later on we will spend some time on formulating the latter idea (see Chapter 4).

What is normalisation and how is it related to the inversion principle? We see now a connection with Dummett's 'levelling of local peaks'. When a λ intro-rule is followed immediately by a λ elim-rule for which the intro-conclusion is the major premise of the elim-rule, this formula constitutes a *local maximum of complexity*. Sometimes we say that the formula is a *detour formula*: If the rules are harmonious we should be able to go directly from the grounds of the intro-rule to the conclusion of the elim-rule without bothering with the intermediate intro/elim applications. This suggests that there is a related derivation which is a proof of the same argument by moving directly from the grounds to the (intermediate) conclusion. We will see, however, that it is a highly non-trivial issue how to apply these reductions in a manner that guarantees that no new maxima is generated.

Definition 2.3 (Maximum formula)**.** A formula occurring in a derivation Π that is both the consequence of an application of a λ intro-rule and the major premise of an application of a λ elim-rule is said to be a *maximum formula* in Π.

Built into this definition is typically an assumption that intro-rules for a logical constant λ will have a λ-formula as conclusion, and, correspondingly, the major premise of a λ elim rule is also a λ-formula. This is why we think of the meeting point between the two rule-applications as an increase in *formula complexity*.[33]

In preparation for later discussions of normalisation, we need to add a further complication. In the presence of rules like $E\vee$ and $E\exists$, where the form of the conclusion might be completely independent of the major premise, consecutive applications of an elim-rule might yield a *segment* of the same formula in the derivation.

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[33]The complexity of a formula A is defined as the number of logical connectives in A (except $\bot$).

Example 2.4. The following derivation involves a segment of the formula $(A \vee B) \vee C$:

$$
\cfrac{A \vee (B \vee C) \quad \cfrac{\cfrac{[A]^1}{A \vee B}}{(A \vee B) \vee C}}{\cfrac{[B \vee C]^2 \quad \cfrac{\cfrac{[B]^3}{A \vee B}}{(A \vee B) \vee C} \quad \cfrac{[C]^4}{(A \vee B) \vee C}}{(A \vee B) \vee C}\ {}_{(3,4)}}{(A \vee B) \vee C}\ {}_{(1,2)}
$$

We will call rules of this type *del-rules* (disjunction-elimination like rules). They are worth noticing not only because they complicate the standard treatment of normalisation in intuitionistic logic, but because this type of rule will come to prominence in what is to follow (see Chapter 4). We then give a definition of a segment and maximum segment, for which the above maximum formula is the special case where the length of the segment is 1.

Definition 2.4 (Segment). A *segment* is a sequence A_1, ..., A_n of consecutive occurrences of a formula A in a derivation Π such that:

(i) A_1 is not the consequence of application of a del-rule;

(ii) For each $i < n$, A_i is a minor premise of an application of a del-rule; and

(iii) A_n is not the minor premise of an application of a del-rule.

Definition 2.5 (Maximum Segment). A segment A_1, ..., A_n is a *maximum segment* if A_1 is a consequence of an intro-rule application (or *EFQ*) and A_n is the major premise of an elim-rule application.[34]

Clearly, if $A_1 = A_n$, then A_1 is a maximum formula. Whatever the length of a maximum segment, it must start and end with an intro- and elim-rule for the same connective (with the exception of *EFQ* and possibly other special rules). Below

---

[34]Note that we can talk about the *complexity of a segment* by taking the degree of the segment's formula.

we will see that Prawitz's normalisation proof uses a subinduction on the length of segments in a derivation.

We are then in a position to systematically evaluate standard natural deduction rules with respect to the deletion of maximum segments. First, Prawitz identifies *conversions* (or reductions) for logical constants in **Ni** (natural deduction for propositional intuitionistic logic, see Appendix A.1):

Definition 2.6 (Detour Conversions). Conversions for intro- and elim-rules in **Nip**. Let $i \in \{1,2\}$:[35]

Conjunction $\wedge$:

$$\frac{\dfrac{\Pi_1 \quad \Pi_2}{A_1 \quad A_2}}{\dfrac{A_1 \wedge A_2}{A_i}} \quad \rightsquigarrow \quad \begin{matrix}\Pi_i \\ A_i\end{matrix}$$

Disjunction $\vee$:

$$\frac{\dfrac{\Pi}{A_i}}{A_1 \vee A_2} \quad \begin{matrix}[A_1]^u \\ \Pi_1 \\ C\end{matrix} \quad \begin{matrix}[A_2]^v \\ \Pi_2 \\ C\end{matrix}}{C} \ {}_{(u,v)} \quad \rightsquigarrow \quad \begin{matrix}\Pi \\ [A_i] \\ \Pi_i \\ C\end{matrix}$$

Implication $\rightarrow$:

$$\frac{\dfrac{\begin{matrix}[A]^u \\ \Pi \\ B\end{matrix}}{A \rightarrow B} \ {}_{(u)} \quad \begin{matrix}\Pi_1 \\ A\end{matrix}}{B} \quad \rightsquigarrow \quad \begin{matrix}\Pi_1 \\ [A] \\ \Pi \\ B\end{matrix}$$

---

[35]Gentzen (1934, pp. 80-81) gave an example of a conversioin step (for implication), but did not give any systematic account nor connect it with his *Hauptsatz*:

> An example may clarify what is meant [by introduction rules as 'definitions']: We were able to introduce the formula $\mathfrak{U} \supset \mathfrak{B}$ when there existed a derivation of $\mathfrak{B}$ from the assumption formula $\mathfrak{U}$. If we then wished to use that formula by eliminating the $\supset$-symbol (we could, of course, also use it to form longer formulae, e.g., $(\mathfrak{U} \supset \mathfrak{B}) \vee \mathfrak{C}$, $\vee$-*I*), we could do this precisely by inferring $\mathfrak{B}$ directly, once $\mathfrak{U}$ has been proved, for what $\mathfrak{U} \supset \mathfrak{B}$ attests is just the existence of a derivation of $\mathfrak{B}$ from $\mathfrak{U}$. Note that in saying this we need not go into the 'informal sense' of the $\supset$-symbol.

Negation $\neg$:

$$
\begin{array}{ccc}
& [A]^u & \\
& \Pi_2 & \Pi_1 \\
\Pi_1 & \dfrac{\bot}{\neg A}\ (u) & [A] \\
\dfrac{A \qquad \neg A}{\bot} & & \Pi_2 \\
& \rightsquigarrow & \bot
\end{array}
$$

In Prawitz's formulation of **Nip** there are no primitive rules for negation (nor in **Ncp**). Rather, negation is defined as is standard with $\bot$ and $\rightarrow$. Consequently, there is no use for the last conversion step. Instead, Prawitz's proof proceeds by the trivial observation that whenever a maximum segment is formed by starting with *EFQ* and applying an elim-rule on the consequence, we can use *EFQ* to go directly to the end-formula of the derivation. E.g.,

$$
\dfrac{\dfrac{\bot}{A \land B}}{A} \qquad \rightsquigarrow \qquad \dfrac{\bot}{A}
$$

Interestingly, since there is no $I\bot$ rule, the corresponding $E\bot$ rule is a limit case where the grounds for introducing the formula is something like the emptyset. Put differently, Prawitz's $I\bot$ is a kind of *null-rule*.[36] We return to the status of the somewhat special connective $\bot$ in Section 3.2.3. In Section 4.5 we discuss $\bot$ and harmony more generally.

With the above conversion steps, we can say with Dummett's terminology that the rules in question all satisfy *intrinsic harmony*, i.e., any local peak can be leveled.[37] With respect to proof-theoretic harmony, we can observe that the conversions display the fact that anything derivable with the elim-rules can be reached directly from the grounds of the intro-rules. The result is derivations in what is known as *normal form*:

---

[36]See Prawitz (1978, p. 35) for details.

[37]There is an interesting exegetical issue here. Steinberger (2009) has argued, *pace* Read (1994), that Dummett never intended intrinsic harmony to be about normalisation, but only about *local reductions*. Other transformations, which might be needed for normalisation, were not considered part of the harmony notion.

Definition 2.7 (Normal Form)**.** We say that a derivation Π is in *normal form* if it contains no maximum segments.

In other words, what Dummett would need is a general result indicating the existence of normal form derivations for every derivation in the system in question (or at least appropriate conversion steps for the inference rules, see footnote 37). Incidentally, there is such a result for intuitionistic logic; even better, there is a result giving a *procedure* for transforming any derivation into normal form. This is Prawitz's normalisation theorem for intuitionistic logic.

Notice, however, that the result requires that we deal with some further complications. First, note that there might be *redundant applications* of del-rules, i.e., where one of the minor premises involve no discharge. Such cases can trivially be removed by simplifying to a derivation where we merely use the subderivation to the intermediate conclusion.

Second, with del-rules around, we also need to treat segments of length > 1. This is done by *permutation conversions* which shift the elim-rule application on A_n of the maximum segment (see Def. 2.5 again) upwards over minor premises of the del-rule. For example, in the case of $\vee$ we get:

$$
\cfrac{\cfrac{\begin{array}{ccc} \Pi & \Pi_1 & \Pi_2 \\ A \vee B & C & C \end{array}}{C}\quad \Pi'}{D} \quad \rightsquigarrow \quad \cfrac{\begin{array}{ccc} & \cfrac{\Pi_1}{C}\ \Pi' & \cfrac{\Pi_2}{C}\ \Pi' \\ \cfrac{\Pi}{A \vee B} & \cfrac{}{D} & \cfrac{}{D} \end{array}}{D}
$$

With this modification out of the way, we get the main result. For Dummett's purposes this means that intuitionistic logic satisfies the conditions of both total and intrinsic harmony.

Theorem 2.1 (**Nip** Normalisation (Prawitz))**.** *An **Nip** derivation Π from Γ to A reduces in some number of conversion steps to a derivation Π' in normal form from Γ to A.*

The proof proceeds by main induction on the complexity of a segment (i.e., the segment formula), which Prawitz calls the *degree d* of a segment (i.e., the number of connectives except $\perp$ in the occurring formula), and a subinduction on the length l of all segments (sometimes called the *rank*). Let the induction value of a derivation Π in **Nip** from Γ to A be $v = <d, l>$, where d is the highest degree of a maximum segment in the derivation. Then, Prawitz shows by transformation that there is a derivation Π' with induction value less than v. The basic idea is that the induction metrics (together with right order of dealing with maximum segments) ensure that even if the length of the derivation is growing as a result of the conversions, the process will terminate since either the degree or the length is reduced.

Importantly, normalisation is a result that provides us with a range of neat insights. First, there is the potential for a refutation method based on showing the non-existence of a normal proof. If an **Nip** argument $<\Gamma, A>$ cannot have a derivation in normal form, then by Thm. (2.1) it is not derivable in the system. Second, by the form of normal form derivations we aquire a valuable corollary, the so-called Subformula Property. Let us first give a definition:

Definition 2.8 (Subformula)**.** Subformulae of a formula A are defined inductively as follows:

1. A is subformula of A;

2. if $B \wedge C$, $B \vee C$, $B \rightarrow C$ is a subformula of A, the so are B, C;

Thm. (2.1) lets us prove as a corollary that this property applies to the system **Nip**.

Corollary 2.2 (Subformula Property)*.* Every formula occurring in a normal form **Nip** derivation of A from Γ is a subformula of A or of some $B \in \Gamma$.

Moreover, from Cor. (2.2) we get a result that might have some importance for PTS more generally: the Separation Property:

Corollary 2.3 (Separation Property). If Π is a normal form **Nip** derivation, then if $I\lambda$ or $E\lambda$ is applied in Π, then λ occurs in A or some $B \in \Gamma$.

Why is this result of interest for the inferentialist? First, note that Separation Property entails total harmony, i.e. conservativeness. Assume for proof-system **S** that it has the Separation Property, and let $\mathbf{S}^{-\lambda}$ be the result of restricting this system to the subsystem where the language is the λ-free fragment and the rules for λ are omitted. **S** is a conservative extension over $\mathbf{S}^{-\lambda}$, for assume that there was an argument $<\Gamma, A>$ in the λ-free fragment such that $\Gamma \nvdash_{\mathbf{S}^{-\lambda}} A$ but $\Gamma \vdash_{\mathbf{S}} A$. Then the non-$\lambda$-rules are not sufficient to prove a non-λ-argument $<\Gamma, A>$. Thus, in $\Gamma \vdash_{\mathbf{S}} A$, the λ-rules must contribute, contradicting the assumption that **S** has the Separation Property.

There is, in other words, an immediate sense in which Dummett can link intrinsic harmony (conversion steps) with total harmony (conservativeness). Nonetheless, the result does not generalise: There are, as we shall see in Chapter 3, logics which normalise but for which the Separation Property does not hold. Second, even if we ignore its relation to other formal properties, there might be an argument to the effect that the inferentialist ought to require the Separation Property. An example of someone who endorses this view is Neil Tennant (1997). He claims that the property ought to constrain any proper logical constant.[38]

> The basic rules that determine logical competence must specify the unique contribution that each operator can make to the meanings of complex sentences in which it occurs, and, derivatively, to the validity of arguments in which such sentences occur. [...] It follows from separability that one would be able to master various fragments of the language in isolation, or one at a time. It should not matter in what order one learns (aquire the grasp of) the logical operators.[39]

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[38]See Tennant (1997, pp. 313-22).

[39]Note that Tennant thinks the Separability Property is conceptually prior to harmony, contrary to the manner in which it is introduced here.

It is unclear whether this motivation needs to be accepted by inferentialists in general. Just holding (INF) does not appear to commit one to the idea that "it should not matter in what order one learns the logical operators". Rather, there is an underlying thought here, advocated by both Dummett and Tennant. It is the idea that only *molecularism*—the overarching claim that sentences have *determinate* content independent of the language as a whole—can account for *compositionality*; in particular, *semantic holism* cannot.[40] If the inferentialist is so inclined (or so convinced), then there might be a more robust motivation for Separability.[41] Yet, the connection between the formal properties and the meaning-theoretic desiderata is an intangible one. For compositionality, and its connection to molecularism, is a many-faceted affair. We leave this issue behind with a simple warning that unless a position like molecularism is defensible, an inferentialism that takes finite sets of inference rules for a logical connective to *determine* its meaning isolated from the context of the proof-system, remains chimerical.

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[40]See e.g. Tennant (1997, p. 46-7).
[41]See Dummett (1991, ch. 10) and Tennant (1987, chs. 5-7).

Chapter 3

Harmony in Classical Logic

3.1 Classical Negation and Revisionism

So far we have outlined the Dummett-Prawitz approach to an inferentialist criterion for inference rules. Their investigation is clearly geared towards intuitionstic logic and the thought that with proof-theoretic constraints informed by meaning-theoretic considerations, a problem with classical logic will become apparent. We will call that attitude *revisionism*. It needs to be made absolutely clear that revisionist tendencies, be it against classical logic or anything else, are an issue separate from PTS and logical inferentialism. Yet, given the prominence of revisionists in the PTS tradition, it is worth going through the arguments to convince ourselves that even if the revisionism must be abandoned, there is something to be said for PTS in general. Let us now turn to the details of their argument. How does classical logic fail to pass the inferentialists' bar?

First of all, it is crucial that there is a certain fluidity involved in what precisely classical logic *is*. There is no deep problem here, it is simply that the expression 'classical logic' picks out a number of things, e.g., a class of theorems, a model-theoretic relation, a provability relation, classical *semantics*. No danger is involved except when properties you are interested in might apply to, say, one classical

proof-system but not another. The revisionist's burden is to convince us that whatever formal reasons there are for thinking that classical logic is bad, are not just *presentation-dependent* properties, but something that gets at—so to say— the heart of classical logic. Alternatively, their strategy could be to defend one presentation as the correct one. Different proof-systems are good for different purposes, and different formalisms have their advocates and detractors. Typically, such debates are coloured by claims about which formalism best captures ordinary reasoning, a line of argument that I find both obscure and obscuring.[1] To the best of my knowledge, no serious data has been put forward to support one framework or another. However, we will revisit this debate briefly in Section 4.5, then with reference to the issue of multiple-conclusion. We focus on natural deduction in this chapter because of the trends in the inferentialist literature.

We saw above in Section 2.3.2 that standard natural deduction for classical logic lacks *total harmony*. **Ncp** fails on this account because classical negation (or rather $E\perp_C$, with classical negation as defined) is non-conservative over the system restricted to the implicational rules, $I\rightarrow$ and $E\rightarrow$, i.e., **Ncp** is a non-conservative extension over the positive intuitionistic fragment, $\mathbf{Nip^+}$. On the other hand, intuitionistic negation (in the shape of $E\perp_I$) yields a conservative extension for the same system.

Similarly, Gentzen's negation rules $I\neg$ and $E\neg$, together with $E\perp_I$ also produce a conservative extension over minimal logic **Nmp**.[2] These negation rules, however, only give intuionistic logic. In order to get classical logic, Gentzen suggests adding *LEM* (law of excluded middle) or *DNE* (double negation elimination), both of which will results in non-conservativeness. Prawitz's system takes **Nip** and replaces $E\perp_I$ with $E\perp_C$ (often just called *classical reductio* and contrasted with the intuitionistically valid *reductio ad absurdum* rule), which also yields non-conservativeness.[3]

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[1]Revisit, for example, Gentzen's original motivation for developing natural deduction.

[2]See Appendix A.3.

[3]Both of these facts can be proved with fairly straightforward derivations.

All these results are connected with the fact that introducing classical negation in some guise or other typically impacts provability for the implicational fragment of the calculus. Some remarks on this are in order. First, note that when we investigate Gentzen's negation rules, $I\neg$ and $E\neg$, it becomes apparent that if $\ulcorner \neg A \urcorner$ is defined as $\ulcorner A \to \bot \urcorner$ these rules are instances of the $\to$-rules already in the system:

$$\frac{\genfrac{}{}{0pt}{}{[A]}{\genfrac{}{}{0pt}{}{\vdots}{\bot}}}{A \to \bot} \qquad \frac{A \to \bot \quad A}{\bot}$$

Now contrast the classical reductio rule, $E\bot_C$. When we read negation as defined we get:

$$\frac{\genfrac{}{}{0pt}{}{[A \to \bot]}{\genfrac{}{}{0pt}{}{\vdots}{\bot}}}{A}$$

The resulting inference is clearly not an instance of a valid implicational inference in the sense that $\bot$ could be substituted for any formula. In other words, adding $E\bot_C$ to **Nip** is tantamount to helping oneself to another inference rule for '$\to$'.[4]

Second, the above observation can be further entrenched by noticing that the $\to$-fragment of **Nip** can be extended to the $\to$-fragment of **Ncp** by adding another implicational rule, *Peirce's Rule*, corresponding to *Peirce's Law above* (see Section 2.2.3):

$$\frac{\genfrac{}{}{0pt}{}{[A \to B]^u}{\genfrac{}{}{0pt}{}{\vdots}{A}}}{A} \; (PR)(u)$$

---

[4]Whether or not this is an elim-rule is an interesting issue. We return to the question below in Section 3.2.1.

This rule is suggested in Curry (1950). Of course, this is trivially a conservative extension since the language remains the same. It does not vindicate classical negation for Dummett and Prawitz, but it does give us a natural deduction system characterising the *classical* implicational fragment.[5]

What we have seen so far is that there is a range of standard natural deduction presentations of classical logic that all fail with respect to Dummett's total harmony. Further, since the Separation Property entails conservativeness (see Section 2.3.3), all of these systems have 'inseparable' connectives (in particular, '¬' and '→'). This analysis, however, includes no mention of intrinsic harmony. For many revisionist inferentialists, the deeper analysis of the problem with classical negation involves the details of conversion steps for classical negation.[6]

We saw in Def. (2.6) that the $I\neg$-$E\neg$ formulation of intuitionistic negation can be given a conversion step as follows:

$$
\begin{array}{ccc}
 & \begin{array}{c}[A]^u \\ \Pi_2 \\ \bot \end{array} & \\
\begin{array}{c}\Pi_1 \\ A\end{array} & \dfrac{}{\neg A}\ {}_{(u)} & \\
\multicolumn{2}{c}{\bot} &
\end{array}
\quad\rightsquigarrow\quad
\begin{array}{c}
\Pi_1 \\ [A] \\ \Pi_2 \\ \bot
\end{array}
$$

Consequently, both Prawitz's system **Nip** (without negation rules) and Gentzen's **Nip**$^\neg$ normalise. But, importantly, this result does not carry over to classical logic in any straightforward way.

Take for instance the classical system **Ncp**$^\neg$ with *DNE* in addition to $I\neg$ and $E\neg$:

$$
\dfrac{\dfrac{\begin{array}{c}[\neg A]^u \\ \vdots \\ \bot\end{array}}{\neg\neg A}\ {}_{(u)}}{A}
$$

---

[5]Note that classical reductio in axiom form, $\ulcorner((p \to \bot) \to p) \to p\urcorner$, is an instance of Peirce's Law.

[6]E.g., Prawitz (1977).

The derivation cannot be converted into a derivation without the maximum formula like its intuitionistic counterpart. Dummett, in his discussion of negation, considers the similar system $\mathbf{Ncp}^D$ with $I\neg^D$, $E\neg^D$, and DNE (where no nullary connective $\bot$ is needed):

$$
\begin{array}{ccc}
\begin{array}{c}
[A]^u \\
\vdots \\
\dfrac{\neg A}{\neg A}\ (I\neg)(u)
\end{array}
&
\dfrac{A \quad \neg A}{B}\ (E\neg)
&
\dfrac{\neg\neg A}{A}\ (DNE)
\end{array}
$$

Dummett is quick to point out that there can be no leveling of local peaks for the same reasons as in $\mathbf{Ncp}^{\neg}$:

$$
\begin{array}{c}
[\neg A]^u \\
\vdots \\
\dfrac{\neg\neg A}{\dfrac{\neg\neg A}{A}}\ (u)
\end{array}
$$

Correspondingly, $\mathbf{Ncp}^{LEM}$ yields the same problem, but parred with $E\vee$:

$$
\dfrac{\dfrac{\Pi}{A \vee \neg A} \quad \begin{array}{c}[A]^u \\ \Pi_1 \\ C\end{array} \quad \begin{array}{c}[\neg A]^v \\ \Pi_2 \\ C\end{array}}{C}\ (u,v)
$$

Prawitz observes that for the system $\mathbf{Ncp}$ (and $\mathbf{Nip}$ for that matter) the $E\bot_C$-rule does not fit the intro- or elim-rule mold of the other connectives. He entertains the thought of replacing the rule with:

$$
\begin{array}{cc}
\begin{array}{c}
[A]^u \quad [A]^v \\
\vdots \qquad \vdots \\
\dfrac{B \qquad \neg B}{\neg A}\ (Dil)
\end{array}
&
\dfrac{\neg\neg A}{A}\ (DNE)
\end{array}
$$

and thus allowing negation as primitive. Prawitz quickly concludes that this will not remedy the situation. First, he dislikes the fact that the intro-rule is *improper*, that is, it has the principal connective occurring in one of the subderivations.[7]

---

[7]In Dummett's terminology this is called *non-sheereness* (1991, p. 257).

Second, it does not satisfy Prawitz's inversion principle since there are derivations where an occurrence of a maximum formula cannot be reduced. His example is *LEM*:[8]

$$
\dfrac{\dfrac{\dfrac{[A]^1}{A \vee \neg A} \quad [\neg(A \vee \neg A)]^2}{\dfrac{\neg A}{A \vee \neg A}} \;{}_{(1)} \qquad [\neg(A \vee \neg A)]^2}{\dfrac{\neg\neg(A \vee \neg A)}{A \vee \neg A}} \;{}_{(2)}
$$

Here the formula $\neg\neg(A \vee \neg A)$ occurs as both the major premise of an elim-rule and as the conclusion of an intro-rule, and there is no way in which we can preserve the provability of *LEM* while removing the maximum formula.

Does this mean that normalisation fails for classical logic? No, Prawitz (1965) proved the result for the $\vee,\exists$-free fragment of **Nc** (or, more generally, del-rule free classical logic). Since these logical constants are definable in the remaining language ($\neg$, $\wedge$, $\rightarrow$, $\forall$), the result can still be said to hold of classical logic. The trick is to deal with classical negation as a special case. Prawitz first showed that anything provable the $\vee,\exists$-free fragment of **Nc** is provable using only atomic formulae as consequences in $E\perp_C$. The proof works by transforming arbitrary proofs using $E\perp_C$ into proofs where the application of the same rule involves a formula of lower complexity.[9] Actually, given the absence of $\vee$ and $\exists$, the normalisation result for classical logic is more straightforward than the intuitionistic result: Normal form can be defined over maximum formula without bothering with segments and permutation conversions.

Theorem 3.1 (**Ncp** Normalisation). *An **Ncp** derivation (in the $\vee,\exists$-free language) Π from Γ to A reduces in some number of conversion steps to a derivation Π' in normal form from Γ to A.*[10]

---

[8]See Prawitz (1965, p. 35)

[9]See ibid., p. 39-40.

[10]Actually, there is an improved normalisation result due to Stålmarck (1991). By adding extra reduction rules covering the cases of $\vee$ and $\exists$, normalisation is proved for the full language.

Unsurprisingly, however, the corollaries are not the same as for Thm. (2.1).[11] $E\perp_C$ must be taken as an exception, as its negated discharge formula might not be a subformula of any premise, nor of the conclusion. This happens for example in a derivation of *LEM*, where the formula $\neg\neg(A \vee \neg A)$ is not a subformula of the conclusion.

Corollary 3.2 (Subformula Property **Ncp**). Every formula occurring in a normal form **Ncp** derivation (in the $\vee,\exists$-free language) of A from Γ is a subformula of A or some $B \in \Gamma$, except for assumptions discharged by applications of $E\perp_C$ and occurrences of $\perp$ immediately below such assumptions.[12]

Furthermore, as we already had reason to anticipate, adding $E\perp_C$ also affects the Separation Property. In particular, since the extension of the intuitionistic implicational fragment is non-conservative, there are normal derivations of A from Γ where we need to apply rules for connectives not occurring in either A or Γ. Peirce's Law is an example:[13]

$$
\cfrac{[A \to \perp]^2 \quad \cfrac{[(A \to B) \to A]^3 \quad \cfrac{\cfrac{\cfrac{[A \to \perp]^2 \quad [A]^1}{\perp}(E\to)}{B}(E\perp_C)}{A \to B}(I\to)(1)}{A}(E\to)}{\cfrac{\cfrac{\perp}{A}(E\perp_C)(2)}{((A \to B) \to A) \to A}(I\to)(3)}
$$

This is a normal derivation of Peirce's Law. There are applications of intro-rules immediately followed by applications of the corresponding elim-rule, but not where the conclusion of the former features as the major premise of the latter. The conclusion is in the $\to$-fragment, and yet there is no way to do without the applications of $E\perp_C$.[14] Consequently, the Separation Property fails (exactly as expected given

---

[11]One corollary of Thm. (3.1) that we do get is the consistency of classical logic. See Prawitz (1965, p. 44). Of course, this extends to intuitionistic logic since it is deductively weaker. The consistency corollary is inspired by Gentzen's consistency proof using cut elimination.

[12]From this it follows that the qualification will be restricted to negations of subformula.

[13]Recall that in Prawitz's system negation is defined, $\neg A = A \to \perp$. We treat the rules here accordingly.

[14]The derivation also involves vacuous and multiple discharge.

the non-conservativeness). The upshot is important: The normalisation theorem is a global property of a proof system that is to some extent independent of conversion steps for the logical connectives. It is these conversion steps that guarantee the Separation Property in the intuitionistic case, so the move from normalisation to Separation does not hold in general. One might then suspect that like conservativeness, normalisation is not the ideal test for the inferentialist.[15]

As it becomes apparent that several of the PTS desiderata that have been associated with harmony (or with meaning-determining inference rules in general) are independent of each other, the inferentialist must pronounce on precisely which of the aforementioned properties must feature in her semantic theory. This is crucial if the inferentialist revisionist is to have a convincing case against any logic, say, classical logic, as falling short of proof-theoretic demands. We return in Chapter 4 to the analysis of proof-theoretic harmony. In what follows we will evaluate different classicist responses with respect to the proof-theoretic demands explored so far. Two main insights are reached: First, PTS is not intrinsically revisionistic; in particular, classical logic is perfectly compatible with any reasonable proof-theoretic demand. Second, negotiating the proof-theoretic framework required for different logics to fulfill the demands will inform the development of an exact harmony criterion.

---

[15]Peter Milne (1994) raises an interesting point regarding the conversion steps for intuitionistic logic. What guarantee do we have that there are no further rules that can be added to intuitionstic logic which displays the same sort of conversion, but which still does not yield classical logic? In other words, is there an *intermediate logic* which is harmonious in the sense of *harmony-as-normalisation*?

> In order to prove that no extension was possible, one would, I suppose, have to go through every possible rule-extension of intuitionist logic that yields either classical logic or one of the uncountable infinity of intermediate propositional logics. The suggestion that completeness has been proved proof-theoretically may therefore be premature. (ibid., p. 56)

3.2   Classical Logic Revamped

We saw in Section 2.3.2 that conservativeness is a highly framework-sensitive prop-
erty. What about normalisation—is it equally variant under different presentation
of classical logic? There has been a whole battery of replies to the arguments
that Dummett and Prawitz advances against classical logic in the last couple of
decades. So far the emphasis has been on the proof-theoretic differences between
intuitionistic and classical logic, a difference that Dummett finds pregnant with
revisionist significance. Concluding his investigation, he says:

> This more detailed look at classical negation confirms [...] that it is not
> amenable to any proof-theoretic justification procedure based on laws
> that may reasonably be regarded as self-justifying. (Dummett 1991,
> p. 299)[16]

Classical logic fares poorly with respect to his various constraints, while "intuition-
istic logic", on the other hand,

> appears capable of being justified proof-theoretically by any of the
> procedures we have discussed; and this means that the meanings of
> the intuitionistic logical constants can be explained in a very direct
> way, without any apparatus of semantic theory, in terms of the use
> made of them in [inferential] practice. Dummett (1991, p. 299)[17]

We now turn to different classicist replies, and attempts at defusing the objections
raised by Dummett and Prawitz. The question is whether the proof-theoretic
observations these objections are based on are deep facts about the difference
between intuitionistic and classical logic, or just artifacts of the presentations.

---

[16]Prawitz (1977, p. 34) reaches the same conclusion.

[17]"That is not, of course, to say that the classical negation-operator cannot be intelligibly
explained; it is only to say that it cannot be explained by simply enunciating the laws of classical
logic." (ibid.)

3.2.1 $E\perp_C$ Revisisted

Peter Milne's discussion of harmony and inference rules (Milne 1994) contains several interesting suggestions about how PTS can be extended to classical logic. On the issue of conversions for classical negation he suggests an unorthodox perspective on $E\perp_C$ in the system $\mathbf{Ncp}^T$ (see Appendix A.5). Instead of considering it as an elimination rule, think of it as an *introduction rule* for classical negation.[18] This is surprising, of course, since the other standard intro-rules have in common that they literally *introduce* their respective connectives as the principal connective in the conclusion. Certainly, this appears to be part of the motivation for Dummett's analogy between assertion-conditions and intro-rules. Of course, Milne is the first to admit that this is counterintuitive, but nonetheless it merits a closer look.

With the reconception of $E\perp_C$ we get the following conversion:

$$
\dfrac{\dfrac{\dfrac{[\neg A]^u}{\Pi_0}}{\dfrac{\bot}{A}\,(i)} \quad \dfrac{\Pi_1}{\neg A}}{\bot} \quad \rightsquigarrow \quad \dfrac{\dfrac{\dfrac{\Pi_1}{\neg A}}{\Pi_0}}{\bot}
$$

Clearly the conversion behaves a bit differently from the ones displayed in Def. 2.6. Importantly, the formula which is the conclusion of the intro-rule and the major premise of the elim-rule is not a maximum of complexity in the standard sense defined by the formula degree.[19] In fact, this formula does not necessarily have a principal occurrence of a negation (A might have any form). Rather, the negated formula, the formula of the highest complexity, is discharged in the subderivation.

The question, then, is whether we have any reason for being suspicious of conversions of this type. More precisely, what is the significance of allowing conversions

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[18]Slater (2008) makes the same claim but with a broader motivation about the three-fold nature of classical connectives. In essence, however, his view has the same limitations as Milne's (e.g., failure of the Subformula Property).

[19]Note that on Milne's view A is actually the major premise of the elim-rule.

like this for the normalisation theorem? In fact, there are some noteworthy differences from the treatment of normal form in Section 2.3.3. First, because of the re-labelling of rules, the extension of 'maximum formula' is different, and, consequently, what we count as a normal form derivation will be different. As an example, the following derivation of *LEM* has no maxima:

$$
\cfrac{[\neg(A \vee \neg A)]^3 \qquad \cfrac{\cfrac{[\neg(A \vee \neg A)]^3 \qquad \cfrac{\cfrac{[\neg\neg A]^2 \quad [\neg A]^1}{\cfrac{\bot}{A}\ (1)}}{A \vee \neg A}}{\cfrac{\bot}{\neg A}\ (2)}}{A \vee \neg A}}{\cfrac{\bot}{A \vee \neg A}\ (3)}
$$

We get a 'normal form' which is non-standard: Conversions delete successive applications of intro-rules and elim-rules (of the same connective), but what is being deleted is not necessarily the formula of the highest *degree*. Perhaps the purist would not want to call this sort of procedure *normalisation* at all, but we leave that issue aside. The property undeniably has a strong affinity to normalisation. The important question, rather, is whether or not the above negation conversion will reduce the appropriate induction values for any given non-normal derivation. Of course, as opposed to Prawitz's original proof strategy we cannot simply rely on the degree of the maximum formula A, since in the converted derivation $\neg A$ might itself be a maximum formula. This is the case depending on the structure of the subderivations Π_0 and Π_1 which are transfered from the original derivation to the converted one. Specifically, when Π_1 ends with an application of classical reductio ($E\bot_C$ as the intro-rule) and Π_0 starts with an application of $E\neg$ (where $\neg A$ is the major premise, and so $\neg\neg A$ the minor premise).

We might immediately suspect that the whole proof itself will be reduced in length after a conversion, but this need not be the case. In proof-systems that are *linear* or *affine*, i.e., that do not allow multiple discharge of assumptions, we know that

the length of the converted derivation will be reduced.[20]   As the derivation is always finite, the process will terminate at some point. However, neither **Nip** nor **Ncp** have this property. They both include multiple discharge, a policy that might engender explosion of derivation length. Schematically, we can have a conversion of the following type:

$$
\begin{array}{c}
\begin{array}{ccc}
[\neg A]^1_1 & \ldots & [\neg A]^1_n \\
\Pi_0 & & \\
\dfrac{\bot}{A} \ (1) & & \Pi_1 \\
& & \neg A
\end{array} \\
\hline
\bot
\end{array}
\quad \leadsto \quad
\begin{array}{c}
\begin{array}{ccc}
\Pi_1 & & \Pi_1 \\
\neg A_1 & \ldots & \neg A_n \\
& \Pi_0 & \\
\end{array} \\
\bot
\end{array}
$$

The subscripts on $\neg A$ simply indicate the number of formula occurrences. Depending on the length of Π_1, the converted derivation might be longer than the original. Since we have no guarantee that there are no new maximum formulae in the converted derivation (and the degree of the maximum formula is higher than the one deleted in the conversion, i.e., A), it is difficult to see what headway we have made.

It is clear, at least, that the suggested reduction procedure does give some strange results. For instance, take the following, rather perverse, derivation:

$$
\begin{array}{c}
\dfrac{\dfrac{\neg A \to A \quad [\neg A]^1}{A} \quad [\neg A]^1}{\dfrac{\bot}{A} \ (1)} \quad \dfrac{[\neg\neg A]^2 \quad \neg A}{\dfrac{\bot}{\neg A} \ (2)} \\
\hline
\bot
\end{array}
$$

The derivation is obviously a detour: The three topmost lines in the left-most branch would have sufficed. More, it is a non-normal derivation, by Milne's light, since in the left-most branch the bottom A is the major premise of an $E\neg$ application and the conclusion of an $I\neg$ application.[21] Applying the $\neg$ conversion, we get:

---

[20]See Restall (2009a, p. 29-31) for some discussion.
[21]Note that the intro-application discharges doubly.

$$\cfrac{\neg A \to A \qquad \cfrac{\cfrac{[\neg\neg A]^1 \quad \neg A}{\bot}}{\neg A}\ (1)}{\cfrac{A \qquad\qquad\qquad \cfrac{\cfrac{[\neg\neg A]^2 \quad \neg A}{\bot}}{\neg A}\ (2)}{\bot}}$$

This is not yet the derivation we would expect for the argument, but, in fact, there are no maximum formula to convert. The bottom $\neg A$ in the right-most branch is a conclusion of an $I\neg$ application, but, importantly, it is the *minor premise* of the $E\neg$ application.[22] However, puzzling results prove nothing. It still remains to be seen if any derivation can indeed be put in normal form, and, furthermore, what sort of motivation one could have for the normal form resulting from these derivations.

Milne (1994) certainly does not suggest that the above conversion will lead to normalisation, and one might think that the issue is of little consequence to the inferentialist. The real question is whether the conversion is still in the spirit of Prawitz's inversion principle (see Section 2.3.3): Whatever we can conclude with the elim-rule is already 'contained' in the grounds for introducing the major premise. Given the conversion step, this does seem to be the case independent of considerations about normalisation. Nevertheless, from the inferentialist's viewpoint one might be more alarmed by the failure of the Subformula Property. That the property does fail is evident from the above derivation of *LEM*. Without the Subformula Property, and thus the Separation Property, the result might be of less interest for the inferentialist: It is not clear whether the classical logician can avoid the charges from the revisionist intuitionist.

Normalisation aside, the more interesting discussion comes with Milne's responses to meaning-theoretic worries he anticipates with the suggested introduction rule. One of the objections he considers was originally stated by Prawitz. Let us assume that $E\bot_C$ is an intro-rule, and, furthermore, that like Prawitz we consider intro-rules to be meaning-conferring. One might then worry that we have now

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[22]A similar thing can happen in **Nip**, but in that case A and $\neg\neg A$ are not equivalent, so the result appears less surprising.

introduced a meaning-conferring rule for introducing A which rests on the assumption of a more complex formula, $\neg A$. But, presumably, the meaning of $\neg A$ will involve reference to A. In particular, if $A = p$ where p is atomic, Prawitz warns us that the assertion conditions for the atomic fragment of the language will be affected by negation.[23] Even if no further detail is offered, it is clear that Prawitz's objection is that this situation flies in the face of the compositionality of language.

Milne offers a series of replies to Prawitz. First, there is the option of denying that intro-rules have some sort of semantic primacy in PTS. One might instead, *pace* Prawitz, take elim-rules to be meaning-conferring, and thus let intro-rules be justified via their corresponding elim-rules. Of course, this will require an appropriate formulation of harmony, but that is an open issue as far as Milne is concerned.

Second, Milne entertains the idea that syntactic complexity does not correspond to meaning-theoretic complexity. How could this be made precise? The suggestion is that it is a mistake to lopsidedly focus on truth-conditions—*falsity-conditions* are equally important to the classical semanticist. Inspired by Read (1988) (who in turn is inspired by Wittgenstein's *Tractatus*), Milne presents a system where atomic formulae can be *signed* by an operator in the structural language (i.e., not in the object language), c. We read cA as 'the contrary of A', where it is ambiguous whether or not A is negated.[24] More precisely, if $A = \neg B$, then cA stands for B (or, equivalently, $\neg\neg B$); if $A = B$, then cA stands for $\neg B$. We can then formulate the two-fold rules:

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[23]"But this explanation of the meaning of atomic sentences seems incredible. Firstly, it breaks with the molecular and inductive character of intuitionistic (and platonistic) theories of meaning since the canonical proofs of an atomic sentence A now depend on the canonical proofs of $\neg A$, which again depend on the canonical proof of A. Secondly, the break appears at a point where the intuitionistic theory seems especially solid: even holistically inclined theories tend to make an exception for atomic sentences [...] since it is usually felt that their meaning is independent of the meaning of logical compound formulas." Prawitz (1977, p. 36)

[24]Compare Rumfitt's view of harmony in classical logic in Section 3.2.3.

$$\begin{array}{c} [A]^u \\ \vdots \\ \dfrac{\bot}{cA} \ _{(u)} \end{array} \qquad \dfrac{A \quad cA}{\bot}$$

The crucial difference is that the c-reductio rule is ambiguous between intuition-istic reductio, $I\neg$, and classical reductio, $E\bot_C$. It simply tells you that you can toggle from negated to unnegated or from unnegated to negated as long as you can derive a contradiction from the assumption. Milne's observation is that the c-rules are fine with respect to Prawitz's complexity objection. Here we withhold any further discussion of this until we reach systems with assertion and denial as primitives below (see Section 3.2.3). The main issue, we will see, is how to understand rules which operate on structural operators rather than object language connectives. Milne himself moots the worry that the operator c is 'parasitic' on negation, but deems it unproblematic.

Finally, Milne argues that even if there are considerations against treating $E\bot_C$ as an intro-rule, there is a more general problem with the approach favoured by Prawitz. In fact, there is reason to think that we cannot have an intro-rule which gives the assertion conditions for negation without recourse to negation (or something related) in the premises or assumptions. If this is the case, Prawitz objection against $E\bot_C$ as an intro-rule is perhaps undermotivated.

Milne's point rests on a result about when a formula can be *essentially negative*:

Definition 3.1 (Essentially Negative Formulae)**.** We define inductively when a formula is essentially negative as follows:

- $\bot$ is essentially negative;

- no atomic formula is essentially negative;

- $A \wedge B$ is essentially negative iff A or B is;

- $A \vee B$ is essentially negative iff A and B are;

- $A \to B$ is essentially negative iff B is.

Under the above definition, Milne can then prove the following result:

Theorem 3.3. *If Π is a derivation in **Ncp** from Γ to A, whenever all premises $B \in \Gamma$ and all discharged assumptions of Π are not essentially negative (except at most one disjunct of the major premise in each application of E$\vee$), then A is not essentially negative.*[25]

As a corollary, we get the result for **Nip** as well. The theorem also works for all the other standard natural deduction systems for classical logic that we consider here. But whatever the details of the system, the fact stands that $E\bot_C$, as an intro-rule, is not alone in being a negation rule that appears to work by allowing essentially negative formulae in the subderivations. Indeed, all negation intro-rules we have seen so far share this property! Of course, in the case of $I\neg$, this manifests itself in the fact that the subderivation is a proof of $\bot$ from A. It is thus unclear what the power of Prawitz's objection is. That it somehow rests on the account of $\bot$ appears to be inevitable. In Prawitz's systems **Nip**, **Ncp**, the nullary connective $\bot$ is primitive, and negation is defined in terms of it. But so far we lack any plausible account of how we understand $\bot$ in the absence of negation.[26]

Summed up, though, one might question what the advantage of Milne's view is. Even if there is a conversion step for classical negation, it is unclear what the importance for the inferentialist is if the conversion does not deliver any interesting results about the behaviour of the logical connectives. It might look like we are merely covering up the old defects: The classical system still yields non-conservativeness with respect to negation; it gives a qualified Subformula Property (as in Cor. 3.2); and the Separation Property fails. If this is the best the classicist can do, there might still be scope for the revisionist to stand her ground.

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[25]The proof is by induction on the length of proof. Details can be found in ibid., p. 61-2.
[26]See Section 3.2.3 for more discussion of the nature of $\bot$.

3.2.2   Multiple-Conclusion Natural Deduction

We return to the system **NC**, and another proposal for how to circumvent Dummett and Prawitz's attack on classical logic. We saw in Section 2.3.2 that Read argues that classical logic could regain total harmony (conservativeness) by going multiple-conclusion in natural deduction. In a sense, this is adapting the sequent calculus profile of classical logic to a natural deduction environment.[27] Since the positive fragment is sufficient to prove Peirce's Law in sequent calculus, the hope was that it would be the case in multiple-conclusion natural deduction as well. And, indeed, Peirce's Law can be proved in the positive fragment of **NC**.

We might continue by asking about *intrinsic harmony* in the context of **NC**. In particular, we might return to the question of the legitimacy of classical negation in a setting where multiple-conclusion is allowed. It turns out that we can get much more out of this analogy than total harmony. Recall that we were able to prove Peirce's Law in the positive fragment of **NC** because of the following multiple-conclusion rules (together with structural rules):

Example 3.1. Multiple conclusion implication:

$$\frac{\begin{array}{c}[A]\\\vdots\\\Gamma,B\end{array}}{\Gamma,A\rightarrow B}\;I\rightarrow \qquad \frac{\Gamma,A\quad \Delta,A\rightarrow B}{\Gamma,\Delta,B}\;E\rightarrow$$

Applying a similar modification of the negation-rules $I\neg$ and $E\neg$ we get the following result:[28]

Example 3.2. Multiple conclusion negation

$$\frac{\begin{array}{c}[A]^u\\\vdots\\\Delta,\bot\end{array}}{\Delta,\neg A}\;(I\neg_{NC})(u) \qquad \frac{\Gamma,A\quad \Delta,\neg A}{\Gamma,\Delta}\;(E\neg_{NC})$$

---

[27]Compare natural deduction *in sequent calculus style*. See Dummett (1977).
[28]Similar modifications apply to the other rules. See Appendix A.9.

What about normalisation? Even though Boričić (1985) reports that normalisation holds for the system **NC**, we are not given a direct proof of the theorem. Neither he nor Read provides information about how conversion steps are supposed to work with multiple-conclusion rules; rather, they rely on an indirect demonstration through a translation between **NC** and sequent calculus, and an accompanying preservation result between normalisation and cut elimination due to Zucker (1974).

The main challenge with multiple-conclusion rules is that there are, strictly speaking, two sorts of commas around: *Conjunctive commas*—corresponding to sequent-style left-side commas—and *disjunctive commas*—corresponding to right-side commas. In a sequent, such commas have a *location* (right or left of the sequent arrow) to distinguish between them, whereas in natural deduction, this is no longer the case. Of course, in **Nip**, the problem never arises since the the calculus is single-conclusion (and commas are always unambiguously conjunctive).[29] But, by acknowledging that PTS for classical logic is best dealt with in **NC**, the equivocation is introduced.

So far we have mostly surpressed conjunctive commas (and open assumptions in the rules), but the issue will resurface in Section 4.5.1.[30] Until then, we ignore the complication and give rules where commas are always disjunctive. We then get a conversion step for negation which is reminiscent of the one for intuitionistic negation, albeit it involves the added complexity of multiple-conclusions:

$$
\cfrac{\Pi_2}{\Gamma, A} \quad \cfrac{\cfrac{[A]^u}{\cfrac{\Pi_1}{\Delta, \bot}}}{\Delta, \neg A} \; {\scriptstyle (u)} \atop \cfrac{}{\Gamma, \Delta, \bot} \qquad \rightsquigarrow \qquad \begin{array}{c} \Pi_2 \\ \Gamma, A \\ \Pi_1 \\ \Gamma, \Delta, \bot \end{array}
$$

Note that in the converted derivation Π_2 ends in a disjunctive $\ulcorner \Gamma, A \urcorner$, so Π_1 in turn will preserve Γ as a disjunctive context. This is a way of saying that either

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[29]That is, unless the system is in sequent style (see footnote 27).

[30]See also Section 4.2.2 where we discuss an example of connectives that are distinguished by the presence of conjunctive contexts in the inference rules (specifically, in quantum logic).

some member of Γ obtains, or A obtains. If the latter, either $\bot$ or some member of Δ obtains (according to Π_1). The result is the same end-line as in the original non-normal derivation.[31]

What is more, we can use the new rules to prove *LEM* without the use of any of the old characteristically classical rules:

$$\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{[A]^1}{A \vee \neg A}}{A \vee \neg A, \bot}}{A \vee \neg A, \neg A} \; (1)}{A \vee \neg A, A \vee \neg A}}{A \vee \neg A}$$

Alternatively, we can have $\bot$ as primitive and prove *LEM* without rules for negation:

$$\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{[A]^1}{A, \bot} \; (K)}{A \to \bot, A} \; (I\to)(1)}{A \vee (A \to \bot), A} \; (I\vee)}{A \vee (A \to \bot), A \vee (A \to \bot)} \; (I\vee)}{A \vee (A \to \bot)} \; W$$

Read makes the pertinent remark that under these circumstances, i.e., with negation defined, it is the $\to$-rules that are doing the work (together with weakening). As a consequence, both intuitionistic and classical negation can be defined with $\bot$, $\to$, but the $\to$ rules will distinguish between the systems. In fact, the above derivation does not even apply the $\bot$-rule! Nevertheless, the rule is required for the derivation of *DNE*.[32]

It is worth mentioning that **NC** upstages other normalisable natural deduction systems for classical logic because it also gives neater corollaries. In particular, we get a Subformula Property unnegotiated by $E\bot_C$, and thus also the Separation

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[31]Multiple conclusion in natural deduction will receive more detailed attention later in Section 4.5.1.

[32]See Read (2000, p. 149)

Property. This is not unexpected given the close relation between **NC** and the sequent calculus **G1c**, where both properties also hold as corollaries of the cut elimination theorem.[33] Furthermore, unlike Prawitz's result, the normalisation theorem for **NC** is not restricted to the $\vee$, $\exists$-free fragment of the language.

Again, these modifications of classical logic render it unclear at best whether Dummett and Prawitz's objections still have any bite. In **NC**, classical logic has both total and intrinsic harmony. Even more, with full Subformula and Separation Properties, the situation is indistinguishable from that of intuitionistic logic. If the revisionist still wants to maintain that there is a reason to reject classical logic, there appear to be only two strategies available. Either new proof-theoretic constraints must be forthcoming that will reinstate the relevant difference between the logics, or—and this is the option preferred by Dummett—some argument must be given why multiple-conclusion systems are not in good standing. We will have ample opportunity to explore the debate about the second approach below (Section 3.3).

3.2.3 Assertion and Denial: Bilateralism

A closely related—and equally interesting—strategy for the classicist is what we will refer to as *bilateralism* (as opposed to *unilateralism*). We have already seen it anticipated in Milne (1994) above where it was suggested that falsity-conditions be treated on par with truth-conditions. But rather than consider such a *semantic* division of labour, we will consider a *pragmatic* division of labour, between *assertion-conditions* and *denial-conditions*. Bilateralism is the idea that a proper account of (classical) logical constants, and negation in particular, requires that denial is not analysed as assertion of a negation, but as a primitive alongside assertion. Obviously, this bifurcation will affect inference rules to the extent that

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[33]See Troelstra & Schwichtenberg (2000, ch. 4.2).

the rules not only characterise conditions for asserting a λ-sentence, but also conditions for denying a λ-sentence.

A number of important papers on the logic of denial were written in the 1990s (around the time Bill Clinton was president), all roughly with the same starting point: Questioning Frege's argument that we ought to equate denial with the assertion of negation (Price 1990, Smiley 1996, Rumfitt 1997).[34] Frege's argument has found proponents in Geach (1965, pp. 454-55) and Dummett (1973a, pp. 316-17). Frege worried that introducing denial as a primitive type of judgement along side assertion would lead to unnecessary proliferation of inference rules. In Geach's phrase, why bother with a 'futile complication' if these patterns can already be covered by assertion of negation?

We will not pause here to go through Frege's argument or the many replies in detail. It suffices to say that there are a number of interesting systems that require denial and the assertion of negation to be distinct, e.g., supervaluationism, dialetheism (see Restall 2005 for details). Moreover, as we will see later in Chapter 6, denial as a non-embeddable illocutionary sign in the structural language can make a significant difference for results about the relationship between truth-conditional semantics and proof-theory.

We leave this issue for now, and concentrate on another virtue of denial more pertinent for the present topic: It allows for a presentation of classical logic with interesting proof-theoretic properties. Rumfitt (2000) has argued at length that bilateralism will help the classicist to deflect the revisionist arguments. More precisely, the suggestion is to enrich the structural language of the proof-system (not the object language—importantly, denial is not treated as a logical constant) with *signs*, $+ -$, indicating whether or not the formula is asserted or denied. For some language $\mathcal{L}$, we let WFF^+ be the set of well-formed *signed* formulae over

---

[34]See also Price (1983). Tracing the pedigree of the logic of denial even further back, two seminal contributions are axiomatic rejection in Carnap (1942) and inference rules for rejection in Łukasiewicz (1963). Łukasiewicz introduced a rejection stroke, $\dashv$, corresponding to Frege's assertion stroke, $\vdash$.

a set of well-formed formulae WFF. As suggested by Rumfitt, we can read the signed formulae $+A$, $-A$ informally as 'A? Yes' and 'A? No' respectively. Of course, unlike $\neg$, the denial sign, $-$, is not embeddable (nor is the assertion sign).

With WFF^+ in mind we can provide inference rules involving denial explicitly. And, as it happens, we can give an interesting presentation of classical logic. For instance, the $\vee$-rules can now be formulated directly as the duals of the $\wedge$-rules:

$$\frac{-(A \vee B)}{-A} \; (-E\vee_i) \quad \frac{-(A \vee B)}{-B} \; (-E\vee_{ii}) \quad \frac{-A \quad -B}{-(A \vee B)} \; (-I\vee)$$

Similarly, we can give rules governing the interplay between negation and denial:

$$\frac{-A}{+(\neg A)} \; (+I\neg) \qquad \frac{+(\neg A)}{-A} \; (+E\neg)$$

These rules belong to the system $\mathbf{Ncp^{+-}}$ (see Appendix A.8 for the full system). It is worth noting that this formulation is obviously not yet sufficient for the derivation of LEM since there is no discharge of assumptions (the $\wedge$-rules are standard, simply adding assertion signs).[35] In order to capture classical negation, Rumfitt introduces a couple of non-operational rules, which he calls *co-ordination principles*. These principles co-ordinate assertion and denial, and so do not explicitly introduce or eliminate any logical constants. In this respect, it is appropriate to compare the co-ordination principles with structural rules (e.g., weakening).

$$\begin{array}{c} [\alpha]^u \\ \vdots \\ \dfrac{\bot}{\alpha^*} \end{array} (RED^*)(u) \qquad \frac{\alpha \quad \alpha^*}{\bot} \; (LNC^*)$$

In these rules $\alpha \in WFF^+$, and α^* is the signed formula that results from α by reversing its sign (e.g., if $\alpha = +A$, then $\alpha^* = -A$). Note that these rules are the same as the ones we saw for falsity-conditions in Milne's system above. On the face

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[35]Gibbard (2002, p. 297) remarks that the resulting system is a non-classical *constructive logic with strong negation*.

of it, the rules remind us of the intuitionistic negation-rules $I\neg$ and $E\neg$, but, as was already pointed out, the co-ordination rules are two-faced. Crucially, the RED^*-rule stands proxy for both 'intuitionistic' and 'classical reductio'.[36] For, even if the rules are not strictly speaking negation-rules, they do mimic their negation counterparts when in presence of the above negation rules $+I\neg$ and $+E\neg$. What is more, given these rules, LEM is derivable, and so are DNE, $E\perp_C$, and the two natural but superfluous negation rules:

$$\frac{+A}{-(\neg A)}\ {}_{(-I\neg)} \qquad \frac{-(\neg A)}{+A}\ {}_{(-E\neg)}$$

Rumfitt's system $\mathbf{Ncp}^{+-}$ is adapted from Smiley (1996), but in the latter system there is only one co-ordination principle:[37]

$$\frac{\overset{[\alpha]^u}{\vdots}\quad \overset{[\alpha]^v}{\vdots}}{\underset{\beta \qquad \beta*}{\alpha^*}}\ {}_{(Dil^*)(u,v)}$$

However, Rumfitt's co-ordination principles give Smiley's rule as a derived rule. Unsurprising really, taking into account that this is the denial-rule corresponding to intuitionistic and classical dilemma.

Another important remark about Rumfitt's system is that $\perp$ is not taken to be a nullary propositional constant, in fact it does not have a *determinate propositional content*. Rather, following Tennant (1999), $\perp$ marks a 'logical dead end', it is a *punctuation mark* in a derivation. Consequently, $\perp$ can be said to belong to the structural language like $+$ and $-$. Accordingly, $\perp$ is not embeddable (thus, Rumfitt points out, defining $\neg A$ as $A \rightarrow \perp$ is ruled out). This squares both with the observation that Rumfitt's co-ordination principles do not trade in logical constants, and, more importantly, it underwrites the categoricity result for the logic $\mathbf{Ncp}^{+-}$ (we return to this issue in Chapter 6).[38]

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[36]In fact, Rumfitt himself say that this rule is "the hallmark of classicism, not of bilateralism" (Rumfitt 2008a, p. 1062).

[37]Rumfitt calls this *Smileian Reductio*.

[38]See Wansing (1999) for a reply to Tennant.

Finally, it is clear that the understanding of the validity of signed inference rules requires a machinery over and above truth-in-a-model-preservation. Informally, the idea is to assign *correctness* and *incorrectness* to signed formulae (in the sense that a judgement is correct or incorrect), where $+A$ is correct iff $v(A) = 1$, $-A$ is correct iff $v(A) = 0$. Hence, the task is to show that the inference rules of $\mathbf{Ncp}^{+-}$ are *correctness-preserving*. Even if Smiley and Rumfitt only gives an informal gloss on correctness, following Humberstone (2000), we can formalise the notions as *correctness-valuations* which are induced by truth-valuations, and proceed by defining a corresponding consequence relation in terms of the correctness-valuations.[39]

Returning to the question at hand, let us see why the bilateral approach improves on the issue of giving PTS for classical logic. Rumfitt thinks that his system displays "a kind of harmony" (Rumfitt (2000, p. 806), but it is unclear precisely which notion of harmony he has in mind.[40] Note in particular that Rumfitt spends no time discussing the complications that arise for the informal gloss on harmony: When we talk of grounds under which we can assert a λ-statement and consequences that can be drawn from a λ-statement, there is the issue of which judgement mode the λ-statement has, and, correspondingly, which judgement mode grounds and consequences have.[41] For instance, the $\rightarrow$-rules favoured by Rumfitt involve an interplay between assertion and denial, a difficulty that is absent from Dummett's original characterisation of harmony. Obviously, if such rules are allowed to be meaning-determining, then harmony needs rethinking.

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[39]Needless to say, the details here work on the assumption that the underlying truth-functional semantics is boolean. We return to the issue of generalisations in Chapter 6.

[40]Puzzlingly, Dummett concedes that the system is in harmony without discussing the details. See Dummett (2002, p. 290).

[41]Incidentally, this puts some pressure on the idea, most explicitly propounded in Brandom (2000, pp. 62-63), that "[w]hat corresponds to an introduction rule for a propositional content is the set of sufficient conditions for asserting it, and what corresponds to an elimination rule is the set of necessary consequences of asserting it, that is, what follows from doing so". In the bilateralist framework, an introduction rule might be sufficient conditions for *denying* the propositional content. A quasi-contrapositive reading of this tells us that an introduction rule can provide the necessary conditions for asserting a propositional content (e.g., Rumfitt's denial rules for disjunction). Regardless, see the criticism of Brandom's characterisation in Read (2008b).

More concretely, the new complication translates into a challenge for conversion steps and normalisation. In Rumfitt's $\mathbf{Ncp}^{+-}$ the primitive $\lor$-rules can be given a simple reduction step mirroring the $\land$-rules. The reason is that the intro- and elim-rules for disjunction are denial-rules, while the conjunction-rules are the usual assertion rules. This gives us a reduction of the following form where the maximum formula can be removed:

$$\frac{\dfrac{\begin{array}{cc}\Pi_1 & \Pi_2\\ -A_1 & -A_2\end{array}}{-(A_1 \lor A_2)}}{-A_i} \quad \rightsquigarrow \quad \frac{\Pi_i}{-A_i}$$

The $+$-rules for $\lor$ and the $-$-rules for $\land$ work as we should expect. What about negation? The negation rules are now both immediate, i.e., they involve no discharge of assumptions. In fact, they are merely flip-flop rules, tantamount to the negation rules in classical sequent calculus (e.g., $\mathbf{G1c}$). Here is a conversion:

$$\frac{\dfrac{\dfrac{\Pi}{-A}}{+(\neg A)}}{-A} \quad \rightsquigarrow \quad \frac{\Pi}{-A}$$

Yet, the particular choices of rules leave $\to$ in a peculiar situation. In $\mathbf{Ncp}^{+-}$ the primitive rules are the following:

$$\frac{+(A \to B) \quad +A}{+B}\ {}_{(+E\to)} \quad \frac{-(A \to B)}{+A}\ {}_{(-E\to_i)} \quad \frac{-(A \to B)}{-B}\ {}_{(-E\to_{ii})}$$

Immediately we see that all of the primitive rules are elim-rules, one $+$-rule and two $-$-rules. In order to have a conversion step for $\to$ we need the derived intro-rules (these follow together with co-ordination principles). True, the problem is not a deep one—there is a normalisable system to be had for classical bilateralism. But, the particular rules of the system as they are formulated here is not ideal. Rumfitt does not discuss harmony as normalisation for his system, but it seems clear that this is one of the desirable properties he has in mind.

Regardless of normalisation, Rumfitt does have more concrete things to say in favour of the system which connects with Dummett's harmony notions. The first, and most straightforward, claim is that the calculus has the Separation Property.[42] One can, for example, prove Peirce's Law without recourse to negation-rules. Yet, the celebration of this result is tempered by the observation that such derivations are of course only possible as a result of the co-ordination principles. Given their close affinity to standard classical negation rules, it's no surprise that derivations with non-conservatively applied negation-rules (e.g., $E\perp_C$) can now be exchanged into denial-currency instead.[43]

Rumfitt also argues that in the bilateralist system it is intuitionistic logic, not classical logic, that receives an awkward formalisation. Humberstone (2000) points out that by restricting RED^* to the co-ordination principle

$$\frac{\begin{array}{c}[+A]^u \\ \vdots \\ \perp\end{array}}{-A}$$

we get a bilateralist intuitionistic logic.[44] The idea is pretty transparent: The restricted principle only allows inferring from assertion to denial, reminiscent of how intuitionstic reductio only let us go from unnegated to negated. Thus, LEM is not derivable, nor is the derived rule $-E\neg$. Rumfitt suggests that the latter fact renders the system 'anomalous' since the intuitionist, unlike the classicist, fails to provide the consequences that can be drawn from denying a negated formula.

Nevertheless, no argument is provided to convince us that there are not alternative formulations on which the intuitionist can remedy this problem. Granted, such a negation will not have the flip-flop property of its classical counterpart, but then

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[42]The proof is outlined by Rumfitt, but the details can be found in Bendall (1978). Bendall's system is different from Rumfitt's, but from the fact that the former system has the Separation Property it follows without much work that the latter system also has it.

[43]For a criticism of this exchange, see Murzi & Hjortland (2009).

[44]Note that we do not have all the negation rules as primitive. If they are, DNE is provable without co-ordination principles.

this is an asymmetry the intuitionist has learned to live with. Rather than surrendering the bilateral environment completely to the classicist, the intuitionist will charge the classicist with loading the dice by introducing co-ordination principles that obfuscate the non-constructive bias. In fact, Dummett (2002) offers a reply to Rumfitt's criticism of the intuitionist unilateralist which is at heart questioning the bilateralist's assumption about the interplay between assertion and denial. "Rumfitt fails, however, to explain his own conception of the correctness of a denial, and hence his own interpretation of negation" (ibid., p. 293). If the co-ordination principles are illegitimate (by the intuitionist's lights), it is little help that the negation rules are symmetric. Irrespective of negation-rules, the intuitionist might want to part ways with the classicist already at the level of correctness of assertion and denial.[45]

Ferreira (2008) reports a problem with Rumfitt's RED^*-rule which is grist for Dummett's (and the intuitionist's) mill. Rumfitt takes care to show that LNC^* can be restricted to atomic formulae, and the full rule is then admissible given the operational and structural rules (this can be shown by an easy induction).[46] The importance of this, according to Rumfitt, is that LNC^* is a "precondition for the connectives to possess coherent bilateral sense" (Rumfitt 2008a, p. 1060). Without it we have no guarantee that the two judgements, assertion and denial, are contradictory. Put differently, the principle is a key component in understanding the speech act engine behind Rumfitt's bilateralism.

Yet, interestingly, Ferreira shows that a corresponding proof of the atomic version of RED^* is not forthcoming. In fact, with RED^* restricted to atomic signed formulae, $-(A \land \neg A)$ is not derivable in Rumfitt's system (even if A itself is atomic). Rumfitt replies that since RED^* is not intrinsic to the bilateral sense of connectives—but rather the principle essentially separating classical from intuitionistic logic (see footnote 36)—the case is not symmetric to that of LNC^*. He

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[45]See Rumfitt (2002) for a reply to Dummett.
[46]See Rumfitt (2000, p. 815-16).

does not dispute Ferreira's claim, but simply adds that there is no need for the result in question.[47]

There is scope here for the opponent to gain some foothold. What license do we have to think that whereas LNC^* is a component of the bilateral sense of the connectives, RED^* is not? Both are equally structural (i.e., include no logical connectives explicitly) and both correspond to the usual negation rules. Just like LNC^*, RED^* has an impact on the derivability of certain principles in the calculus. True, it makes sense to say that LNC^* governs the conditions under which our speech acts are in conflict, but, similarly, RED^* governs how we rationally treat situations which involve contradictory speech acts. The difference between the classicist and the intuitionist resides in the disagreement about this issue as well as that of negation. Why think that denial is common ground between the parties any more than negation is? In fact, given what we know about intuitionist discussion of the appropriate attitude to LEM, we do have reason to expect that also pragmatic attitudes ought to take on distinct logical behaviour across the two logics.

For all this, it is plausible that Rumfitt has managed to provide further reason to think that classical logic is in no worse standing than intutionistic logic with regard to proof-theory and PTS in particular.[48] After all, it is possible to construct a bilateral framework where classical logic has both total and intrinsic harmony.[49] On the further matter of saddling the intuitionist with a problem of her own, the proposal appears less successful. Bilateralism might simply be an environment where both logics can flourish—all the better for PTS.

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[47]Gibbard (2002) expresses doubts about Rumfitt's attempt to justify DNE through the negation-rules and the fact that LNC^* can be restricted to atomic formulae. In particular, he offers alternative rules for negation that allows LNC^* to be restricted, but which do not lead to DNE. See also Rumfitt (2002).

[48]For a less favourable account of Rumfitt's project, see Gabbay (n.d.). Gabbay cleverly devises a rule inspired by Stephen Read's 'bullet' (see Section 4.3.3) in order to show that bilateralism has problems with normalisation.

[49]With the small caveat about normalisation mentioned above.

3.2.4 Other Approaches to Classical Logic

Another interesting axiomatisation of classical logic is due to Gabbay (n.d.). Gabbay's aim is a normalisable natural deduction system for classical logic. He is of course aware of the above mentioned result in Stålmarck (1991), but he thinks there is an improvement to be made by not having the controversial rule $E\perp_C$ in the system. Recall that this rule is, in effect, a troublesome exception in the normalisation proofs for classical logic: Its classification as either a negation intro-rule or elim-rule appears spurious. Instead, Gabbay suggests the following rules for classical negation:

$$
\begin{array}{cc}
\begin{array}{c}
[A]^u \\
\vdots \\
\dfrac{\perp}{\neg A} \; (I\neg)(u)
\end{array}
&
\begin{array}{c}
[\neg B]^u \\
\vdots \\
\dfrac{\neg A \qquad A}{B} \; (CE\neg)(u)
\end{array}
\end{array}
$$

The proof of normalisation is indirect in the sense that Gabbay gives the result for a related system with Sheffer Stroke, $|$.[50] The rules for Sheffer Stroke, inspired by Read (1999), are as follows:

$$
\begin{array}{ccc}
\begin{array}{c}
[A]^u, [B]^u \\
\vdots \\
\dfrac{\perp}{A|B} \; (ESS)(u)
\end{array}
&
\dfrac{A|B \quad A \quad B}{C} \; (ISS_I)
&
\begin{array}{c}
[C|C]^u \quad [C|C]^u \\
\vdots \qquad \vdots \\
\dfrac{A|B \qquad A \qquad B}{C} \; (ISS_C)(u)
\end{array}
\end{array}
$$

For intuitionistic logic, we take ESS and ISS_I together with EFQ; for classical logic we simply replace ISS_I with ISS_C. Notice that Gabbay's preferred negation rules are special cases of the Sheffer Stroke rules where A = B. Consequently, a proof of normalisation for the system with Sheffer Stroke yields normalisation for the negation rules as well.

Gabbay is concerned with normalisation and not the Subformula Property (or Separation for that matter), so the fact that the issue of non-conservativeness

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[50]The connective is most known for being an *adequate* connective for classical logic, i.e., with it as the only truth-functinonal connective, all others can be defined.

still remains does not detract from the system in his view. However, given this fact, it is unclear what the net gain from the original normalisation proofs is. Especially considering that Gabbay's result only holds for the $\neg$, $\wedge$, $\forall$-fragment of the language.

Yet another modification of the natural deduction rules can be found in Milne (2002). As with the multiple conclusion approach discussed in Section 3.2.2, Milne is inspired by the, relatively speaking, well-behaved classical sequent rules. By reading off natural deduction rules from **G1c** he comes up with a set of non-standard natural deduction rules. The key idea is that the multiple-conclusion succedents in classical sequent calculus instruct us that classical natural deduction ought to have intro-rules where connectives are 'introduced' embedded in a disjunction. The disjunctions are simply a manner in which multiple-conclusion can be mimicked in a genuinely single-conclusion environment. As an aside, note that this speaks to the issue of including two different structural commas (left- and right-) in natural deduction, since one comma—the disjunctive one—is handled in the object language.

Milne is trading on the fact that there is a natural correspondence between intro- and elim-rules in the intuitionistic natural deduction system **Nip**$^\neg$ and the right- and left-rules in intuitionistic sequent calculus **G1i**. However, for **G1c** where the succedents are not restricted to singletons or the emptyset, it is less clear what the corresponding natural deduction rules are. (Needless to say, if we accept multiple-conclusion in natural deduction—à la Read—the problem does not arise.) By treating multiple-conclusion cases as disjunctive, Milne makes available some essentially classical intro-rules for $\neg$ and $\rightarrow$. It turns out that the other rules can be treated in the same manner as for intuitionistic logic, giving the usual suspects from **Nip**$^\neg$. The classical culprits that the intuitionist cannot have in multiple succedent version are $R\neg$ and $R\rightarrow$. Rewriting these ideas in natural deduction under the disjunctive reading, Milne gets the following rules:

$$
\begin{array}{cc}
\begin{array}{c}
[A]^u \\
\vdots \\
\dot{D} \\
\hline \neg A \vee D
\end{array} \; (I\neg^\vee)(u)
&
\begin{array}{c}
[A]^u \\
\vdots \\
B\{\vee D\} \\
\hline (A \to B)\{\vee D\}
\end{array} \; (I\to^\vee)(u)
\end{array}
$$

The brackets in $I\to^\vee$ indicate that a special case of this rule is where there are no disjuncts; the result is simply the standard $\to$ intro-rule. With the first rule we get an obvious derivation of LEM; with the second we get the classical law (A $\to$ B) $\vee$ A. Thus, both of these rules are inherently classical. Adding them to the fragment of **Nip**$^\neg$ with $I\wedge$, $E\wedge$, $I\vee$, $E\to$, and EFQ gives us classical logic. Let us call this system **Ncp**$^\vee$.

What is the advantage according to Milne? Again it is an issue about how to give a harmonious account of classical logic. Milne proceeds to show that there are conversion steps for his new rules, although in a somewhat surprising form. For negation, the challenge is to show that there is a reduction for $I\neg^\vee$ together with EFQ.[51]

$$
\begin{array}{c}
\begin{array}{c}
[A]^1 \\
\vdots \Pi_1 \\
\dot{B} \\
\hline \neg A \vee B
\end{array} \;(1) \qquad
\begin{array}{c}
A \quad [\neg A]^2 \\
\hline C_1 \\
\vdots \Pi_2 \\
\dot{D}
\end{array} \quad
\begin{array}{c}
A \quad [\neg A]^2 \\
\hline C_n
\end{array} \;
\begin{array}{c}
[B]^2 \\
\vdots \Pi_3 \\
D
\end{array} \\
\hline
\hspace{4cm} D \hspace{3cm} (E\vee)(2)
\end{array}
\quad \rightsquigarrow \quad
\begin{array}{c}
A \\
\hline \Pi_1 \\
\hline B \\
\hline \Pi_3 \\
\hline D
\end{array}
$$

It goes without saying that the reduced derivation is non-standard in the sense that it involves not only the intro- and elim-rules in question, but an application of $E\vee$. This is needed since the connective under investigation—the negation—is not the principal operator in the formula introduced by its intro-rule, $I\neg^\vee$. The leveling of a local peak is then applied not to a negation-formula, but to a disjunction. Regardless, the result is a procedure for removing joint occurrences of $I\neg^\vee$ and EFQ.[52] Unsurprisingly, this fact feeds into the further corollaries. In particular, the Separation Property does not hold in general, but only in a modified version:

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[51]See ibid., p. 518-519 for the related conversion step for $\to$. The case similarly involves $E\vee$.
[52]Because of the necessary presence of a del-rule, simplification conversions are of course required as well.

Corollary 3.4 (Separation Property*). If Π is a normal form $\mathbf{Ncp}^\vee$ derivation, then if $I\lambda$ or $E\lambda$ is applied in Π, then λ occurs in A or some $B \in \Gamma$, *or*, λ is a connective with a principal occurrence in the rules of some λ^* such that λ^* occurs in A or some $B \in \Gamma$.

In general, the problem with the system is that the conversion step, as the system in general, is hostage to the presence of disjunction in the (object) language. Other conversions work exclusively on a vocabulary involving the connective the rules are associated with. In contrast, this reduction involves rules for all the connectives being 'introduced' by the intro-rule. It just happens that there are two of them. Given the strong affinity to multiple-conclusion natural deduction, it is highly unclear what the advantage of $\mathbf{Ncp}^\vee$ is. After all, as opposed to $\mathbf{NC}$, it does not yield the Subformula Property or the Separation Property, and one would be hard pressed to argue that much is gained in terms of how natural the derivations are.[53]

Finally, there is a more peripheral look at harmony in classical logic offered by Weir (1986). Rather than dancing to the intuitionist's flute, Weir wants the classicist to provide alternative proof-theoretic constraints that can be motivated independently from the intuitionist's constraints. If this can be done, and it can be shown

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[53]More recently, Milne has presented unpublished work at the *1st Foundations of Logical Consequence Workshop*, Arché, University of St Andrews. He offers a classical natural deduction system with the three non-standard rules:

$$\frac{\begin{array}{cc} [A]^u & [\neg A]^v \\ \vdots & \vdots \\ C & C \end{array}}{C} \qquad \frac{\begin{array}{cc} [A]^u & [A \to B]^v \\ \vdots & \vdots \\ C & C \end{array}}{C} \qquad \frac{B}{A \to B}$$

The right-most rule is just a special case of conditional proof. The other two rules are motivated by the tautologies *LEM* and $A \vee (A \to B)$ respectively. Together with *modus ponens*, *EFQ*, and standard conjunction and disjunction rules we get a classical system with both the Subformula and Separation Property. Here is a derivation of Peirce's Law:

$$\frac{\dfrac{\dfrac{[(A \to B) \to A]^1 \quad [A \to B]^3}{A} \quad [A]^4}{A} \; (3,4)}{\dfrac{((A \to B) \to A) \to A \quad [((A \to B) \to A) \to A]^2}{((A \to B) \to A) \to A} \; (1,2)}$$

that the classicist performs better on the alternative account, the score is apparently equalised.

> If we can find principles in terms of which classical logic comes out on top, and which are at least as natural as those under which it fares badly, then the proof-theoretic arguments balance out. (ibid., p. 462)

Weir takes his cue from Prawitz's inversion principle (see Section 2.3.3), but points out that Prawitz's formal formulation of the principle is not a proper functional *inverse*. Taking the idea more literally, Weir wants intro- and elim-rules for λ such that an application of $I\lambda$ followed by an application of $E\lambda$ leaves us with the very situation we started with. Although this does work for Prawitz's $\wedge$-conversions, it only holds for the special case of $\vee$-conversion where $C = A_i$. Similarly, $\wedge$ also works in the other direction: Starting with an application of $E\wedge$ followed by $I\wedge$ brings us back to the starting point:[54]

$$
\begin{array}{cc}
\Pi & \Pi \\
\vdots & \vdots \\
A \wedge B & A \wedge B \\
\cline{1-1}\cline{2-2}
A & B \\
\end{array}
$$
$$
A \wedge B
$$

Weir continues by introducing a graphical recipe for a generalised Inversion principle, i.e., where both directions (intro-elim and elim-intro) loops back to the starting point.[55] It is more or less unproblematic to show that this idea can be extended to $\rightarrow$ and $\neg$ (in the guise of $I\neg$ and $E\neg$), but so far that only gives Weir intuitionistic logic without $\vee$.

The key step is dealing with disjunction in an exclusively classical manner for which the new Inversion principle holds. This is achieved by giving the following rules:

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[54]On the assumption that we can help ourselves to two occurrences of $A \wedge B$.
[55]See ibid., p. 466-67 for details.

$$\begin{array}{c} [\neg B]^u \\ \vdots \\ \dfrac{A}{A \vee B} \; (I\vee_1^W)(u) \end{array} \qquad \begin{array}{c} [\neg A]^u \\ \vdots \\ \dfrac{B}{A \vee B} \; (I\vee_2^W)(u) \end{array}$$

$$\dfrac{A \vee B \quad \neg A}{B} \; (E\vee_1^W)) \qquad \dfrac{A \vee B \quad \neg B}{A} \; (E\vee_2^W))$$

Weir can then provide the sought-after inversion:

$$\begin{array}{c} [A]^1 \\ \vdots \\ \dfrac{B}{A \vee B} \quad {}^{(1)} \quad \neg B \\ \hline A \end{array} \qquad \dfrac{A \vee B \quad [\neg A]^1}{\dfrac{B}{A \vee B} \; {}^{(1)}}$$

Finally, Weir argues that, as opposed to the Dummett-Prawitz argumentation, the new Inversion principle fails for intuitiontionistic logic, regardless of presentational details. (Interestingly, in this case it is the disjunction rules that cause the trouble.) Weir's conclusion is that his constraint demonstrates that, contrary to what Dummett and Prawitz argued, it is not classical logic that is too strong, but intuitionistic logic that is too weak. Too weak, that is, to allow for proper inversion.

One recurring bullet that Weir offers to bite is that his presentation of classical logic does not yield a Subformula Property. As with Prawitz's **Ncp** the classicist has a partial result, but this might still provide the intuitionist with some ammunition. Nonetheless, Weir's Inversion principle does go some way towards leveling the playing field. An arising stalemate must probably be solved in a background discussion of how to meaning-theoretically motivate the different principles.

3.3 Multiple-Conclusion Defended

Let us take stock and evaluate the dialectical situation between the intuitionist revisionist and the classicist. Is there any revisionary mileage in PTS and the different proof-theoretic constraints inspired by harmony? We have seen a number

of axiomatisations of classical logic which bypass different difficulties presented by the Dummett-Prawitz camp. Obviously, harmony-as-normalisation does not constitute any real challenge to the classicist: There is a number of classical systems that are normalisable. More contentious is the issue of legitimate conversion steps and the relation to the Subformula and Separation properties. It is also possible that normalisation—where the notion of maximum formula is altered—can be had without these properties, so an option for the revisionist is to require this higher standard from the classical opponent. However, even with the bar raised accordingly, the classicist has the resources to persist with a PTS account of her logic.

With respect to well-behaved conversion steps and the Separation Property the above discussion indicates that the approaches that fare the best are (a) *multiple-conclusion natural deduction*, **NC**, and (b) *classical bilateralism*, $\mathbf{Ncp}^{+-}$. This is no coincidence, for as we shall see, there is an intimate relationship between these two frameworks. Put more succinctly, the structural resources of multiple-conclusion and bilateralism leave the two frameworks with the same expressive power.[56] In fact, for the above systems, it is straightforward to define a faithful translation between the multiple-conclusion framework, call it SET-SET, and the signed single-conclusion framework, call it SET-FRML$^+$. For example, if A_1, ..., $A_n \vdash_{NC} B_1$, ..., B_m, then $+A_1$, ..., $+A_n$, $-B_1$, ..., $-B_{m-1} \vdash_{Nc^+} +B_m$. Conversely, if $+A_1$, ..., $+A_n$, $-B_1$, ..., $-B_m \vdash_{Nc^+} \pm C$, then

$$\Gamma \vdash_{NC} \Delta = \begin{cases} A_1, ..., A_n \vdash_{NC} B_1, ..., B_m, C, if + C; \\ A_1, ..., A_n, C \vdash_{NC} B_1, ..., B_m, if - C. \end{cases}$$

The real question is whether there are significant philosophical arguments that can distinguish between the two approaches. There are two separate issues here: First, the burden is on the intuitionist revisionist to convince us that they are in fact

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[56]The formal details of this 'expressive power' will be made more precise in Chapter 6, see especially 6.2.5.

both defective as vindications of classical logic. Second, the classical inferentialist could embrace both, or argue that one approach is superior. Rumfitt, himself a classicist of the bilateralist leaning, appears to go for the second alternative:

> The close formal relationship between bilateral calculi and their multiple-conclusion cousins, however, should not blind us to what is, for present purposes, a crucial philosophical difference. (Rumfitt 2000, p. 810)

What is this difference? In order to make headway we need to look at the arguments against multiple-conclusion in the literature. Incidentally, the classically-minded Rumfitt shares his dislike for multiple-conclusion with revisionists such as Dummett and Tennant. Let us have a look at the different objections against the SET-SET framework raised in the literature. First out, Dummett:

> Sequents with two or more sentences in the succedent, by contrast, have no straightforwardly intelligble meaning, explicable without recourse to any logical constant. Asserting **A** and asserting **B** is tantamount to asserting ⌜**A** and **B**⌝; so, although the sentences in the antecedent of a sequent are in a sense conjunctively connected, we can understand the significance of a sequent with more than one sentence in the antecedent without having to know the meaning of 'and'. But, in a succedent comprising more than one sentence, the sentences are connected disjunctively; and it is not possible to grasp the sense of such a connection otherwise than by learning the meaning of the constant 'or'. (Dummett 1991, p. 187)

This is where Dummett's reliance on an analysis of denial as an assertion of negation takes center stage. Of course, Dummett is correct in claiming that conjunction and disjunction are asymmetric with respect to assertion. However, this does not show that conjunctive commas are conceptually kosher prior to their

object language counterparts, whereas disjunctive commas are not. Unlike the antecedent-side commas, the disjunctive (right-hand) commas ought not to be treated assertorically. Rather, we think of the rational (normative) constrain imposed by a multiple-conclusion consequence relation as saying that one cannot *assert all premises* and *deny all conclusions* simultaneously. With such a reading there is not necessarily a genuine disjunctive element: $\langle \Gamma, \Delta \rangle$ is invalid iff we assert A_1, and we assert A_2, etc. for each $A_i \in \Gamma$, and we *deny* B_1, and *deny* B_2, etc. for each $B_i \in \Delta$. Interestingly, we can dualise and say, equivalently, that the argument $\langle \Gamma, \Delta \rangle$ is valid iff we deny A_1, or we deny A_2, etc., OR, we assert B_1 or assert B_2, etc. Thus, Dummett's insight is turned up side down: We now have a reading of multiple-conclusion sequents that does not presuppose conjunction but disjunction. Summed up, to the extent that it is a worry at all that there are meta-level conceptions of connectives, Dummett seems to have put too much emphasis on the role played by the interaction between assertion and conjunction.

This counter to Dummett can be made more precise using a framwork offered in a series of papers by Restall (2005, 2008a, 2008b, 2009b). Restall says:

> We can think of the rules for the connectives as giving instructions on how to treat assertions and denials—at least with regard to whether or not these assertions and denials are out of bounds or not. (Restall 2009b, p. 5)

On Restall's approach we consider structures deceivingly similar to arguments, pairs of collections of statements, written [X : Y]. Call such a structure a *state*. We say further that for a state [X : Y], X is the set of asserted statements and Y is the set of denied statements. A state [X : Y] such that $X \cap Y \neq \emptyset$ is an *incoherent* state (i.e., states where the very same statement is both asserted and denied). For an incoherent state [X : Y], we write $X \vdash Y$, indicating precisely the *pragmatic reading* of logical consequence above.

One can then contemplate adding natural constraints on coherence. Restall suggests, for example, that any state [A : A] is incoherent (reflexivity); if [X : Y] is coherent, and X$'$ ⊆ X and Y$'$ ⊆ Y, then [X" : Y"] is also coherent (weakening); and, if [X : Y] is coherent, then so is either [X, A : Y] or [X : Y, A] (transitivity). Of course, one might have conceptions of assertion and denial (and associated conceptions of coherence) which drops any or all of these constraints. The difference, as Restall remarks, amounts roughly to the difference between substructural and non-substructural sequent calculus.[57] That observation is further bolstered by the option of introducing pragmatic constraints on logical connectives. One might for instance say that if [X : Y, A ∧ B] is coherent, then either [X : Y, A] is coherent or [X : Y, B] is coherent, and so on. As with the structural constraints above, we recognise sequent rules by taking the contraposition of the pragmatic constraints.[58]

Importantly, such a reading of sequent calculus in terms of assertion and denial is different in some respects from the Smiley-Rumfitt style signed calculus discussed in Section 3.2.3. Although signs and the state-interpretation can be translated into each other, the former is intended as a superstructure on traditional semantics, while the latter is an analysis of multiple-conclusion consequence. The deep point here is one of foundational semantics: Restall's strategy offers a way in which inferentialism can be combined with truth-conditional semantics by way of pragmatic roles. Smiley and Rumfitt, on the other hand, have no such objective. For them, the traditional truth-conditional semantics is still the protagonist. Later, in Chapter 6, we will return to the connection between proof-theory and truth-conditions in a systematic fashion. In that connection, Section 6.5 will discuss the potential of Restall's strategy in more detail. Before that, however, we need to attend to some more general worries about multiple-conclusion, typically raised by those whole-heartedly opposed to the classical line.

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[57]Analogously, Restall points out that the framework builds in contraction by working with sets rather than subsets. Again, this, as in sequent calculus, affects the logic of the structural commas of the states.

[58]In Restall (2008b) the framework is applied for an analysis of paradoxes.

For a sample of what the single-conclusion aficionado has to say against multiple-conclusion more generally, we turn to an argument of Tennant's:

> [T]he classical logician has to treat of sequents of the form $X : Y$ where the succedent Y may in general contain more than one sentence. In general, this smuggles in non-constructivity through the back door. For provable sequents are supposed to represent acceptable arguments. In normal practice, arguments take one from premises to a single conclusion. There is no acceptable interpretation of the 'validity' of a sequent $X : Q_1, ..., Q_n$ in terms of preservation of warrant to assert when X contains only sentences involving no disjunctions. If one is told that $X : Q_1, ..., Q_n$ is 'valid' in the extended sense for a multiple-conclusion arguments just in case $X : Q_1 \vee ... \vee Q_n$ is valid in the usual sense for single-conclusion arguments, the intuitionist can demand to know precisely which disjunct Q_i, then, proves to be derivable from X. (Tennant 1997, p. 320)

The latter point—that multiple-conclusion is unacceptable for the intuitionist—has been dealt with in a convincing manner in Steinberger (2008). First, multiple conclusion is in no way intrinsically classical: There are well-known intuitionistic multiple-succedent calculi. Two systems, **m-G1ip** and **m-G3ip**, are obtained by the standard classical sequent calculi, **G1cp** and **G3cp** respectively, by restricting the rules for $\rightarrow$. E.g., in the former case (with explicit structural rules) we give the following rule:

$$\frac{\Gamma, A \Rightarrow B}{\Gamma \Rightarrow A \rightarrow B, \Delta} \ {\scriptstyle (R\rightarrow)}$$

Contrast the original rule which preserves the right-side context (see Appendix A.10).[59] All other rules remain multiple-succedent in the ordinary sense.

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[59]See Troelstra & Schwichtenberg (2000, p. 67, pp. 82-3) for details on the systems.

But, as Steinberger goes on to point out, even if there were no multiple-conclusion alternative open for the intuitionist, the argument is faulty for more fundamental reasons. The intuitionist revisionist and the classicist were at a stalemate: Both alleged that theirs was the One True Logic, but neither had the resources to convince the interlocutor. The genius of Dummett and Prawitz's novel take on the debate was to relocate the debate: PTS is the correct meaning-theoretic approach, and since intuitionistic logic, but not classical logic, abides by its constraints, the latter contains unwarranted principles. If the argument had been correct—which it is not—the intuitionist could have helped herself to the Disjunction Property with a clean conscience. But, Tennant's argument is premature: In a discussion of whether or not classical logic does in fact abide by the PTS constraints, it is a blatantly illicit move to argue from one of the properties which the revisionary argument was precisely supposed to establish.

There is another charge against multiple-conclusion in the quotation by Tennant, one which is potentially harder to adjudicate."In normal practice", he says, "arguments take one from premisses to a single conclusion". A first remark, albeit perhaps a bit ad hominem: It is strange to have an advocate of intuitionist revisionism recourse to an argument from 'normal practice'. After all, their case is founded on the tenet that such a practice might be dysfunctional (see Section 2.3.1), and, thus, susceptible for revision. Why then should the classicist be hostage to the arbitrary nature of 'normal practice'?

Nevertheless, the argument from our ordinary inferential practice is dubious even notwithstanding the above point.[60] Making good sense the claim is not easy, but presumably the objection is based on an observation to the effect that arguments in normal practice may contain lists of premisses (without conjunction), but not lists of conclusions (without disjunctions). Normally, when we reach a conclusion which is disjunctive we express this by using disjunction explicitly. The thought, I take it, is

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[60]Restall (2005, p. 11-12) makes the case that there are indeed multiple-conclusion arguments in ordinary reasoning.

that for premises and conjunction the same is not the case. Without any empirical data, however, such an argument appears spurious. In fact, it seems perfectly legitimate to list conclusions disjunctively without explicit use of disjunction. As with premises, the context might determine how we should interpret the list. The fact that we do not normally proceed argumentatively in such a manner is of no consequence.

Surely, upon investigating normal inferential practice we will find, for instance, that most arguments are also *single-premise*, either because the premises are asserted as one statement, or because some premises are tacit (in *enthymematic form*). Whatever the usual form of arguments might be, it does not standardly constrain the mathematics of the logician. In fact, along these line, a neat suggestion by Kosta Došen (1989, p. 365) is helpful: Just as true premises sometimes are enthymematic, perhaps *false conclusions* are also enthymematic.[61] Only upon failing to provide a single, conclusive conclusion is there any need for multiple-conclusion.

Finally, against multiple-conclusion, Rumfitt (2000, pp. 795-96) has raised an alternative objection. Citing Kneale as one of the culprits, Rumfitt says that "not only is it doubtful whether people actually give such arguments, it is also doubtful whether we can attain any intelligible conception of them". For Rumfitt a SET-SET framework gives a collection of arguments whose constituent statements are not *used* but *mentioned.* However, since our inferences are structures where statements—both premises and conclusions—are used, the formalism should mirror this fact.

But why think that multiple-conclusion arguments merely mention the involved statements? Rumfitt takes multiple-conclusion to be the confused result of a *metalogical* remark about logical consequence, namely, that "if certain propositions

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[61]Došen is another of the PTS proponents of sequent calculus. Two other interesting sources for multiple-conclusion systems are Kneale (1956)—see Section 2.1.2—and, of course, Shoesmith & Smiley (1978).

are true, then certain other propositions cannot all be false" (ibid.). He insists, however, that proper inferences must be expressible in the form $A_1, \ldots \ A_n \vdash B$. However, it is unclear why single-conclusion structures are not also just 'met-alogical remarks', and, regardless, the assertion/denial interpretation discussed above shows that we are by no means forced to read SET-SET arguments truth-conditionally.[62]

3.4 Conclusion

We have argued that classical logic has a place in PTS. However, offering this place comes with a price. The traditional boundaries of inferentialist proof-theory is shifted towards unfamiliar grounds. If one accepts that the Separation Property is an important tenet of PTS, the onus is on the classicist to provide formalism in which it can be combined with classical negation. Both Read and Rumfitt succeed in doing this, but not without introducing proof-theoretic complications. Alternatively, the classical logician sympathetic to PTS might simply resist the claim that Separation plays a crucial role in successful meaning-fixing. The danger is, however, that such a classicist is waxing holistic: One cannot isolate the meaning of logical constants in PTS; only the system in full provides the meaning.

Multiple-conclusion is the better option. It leaves classical logic without a remainder, and even if reproached by most intuitionist revisionist, there is no knock-down argument against including multiple-conclusion in PTS. In fact, in anticipation of Chapter 4 and Chapter 6, multiple-conclusion greatly enhances the chances of developing PTS into a proper formal semantics, ridden of an unnecessary revisionist bias.

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[62]Rumfitt is himself a proponent of bilateralism, but fails to appreciate the connection with multiple-conclusion. See also Rumfitt (2008b).

Chapter 4

Generalised Elimination Harmony

4.1 Introduction

Independently of the revisionist debate between the intuitionist and the classicist, there are other aspects of harmony that remain. Recall that the original motivation for harmony and its formal counterparts was Prior's tonk argument, and the consensus that PTS cannot allow for unconstrained introduction of inference rules. If we are to allow for defect rules, and, in extension of that, revision of defect rules, there has to be some restrictions on which inference rules are deemed acceptable.

Dummett, Prawitz and others argued that when the demarcation between permissible and impermissible rules has been drawn, the classical logicians will find themselves outside the scope of PTS. Against this, we saw in Chapter 2 that PTS as a revisionary strategy for the intuitionist is hampered by some flexibility in the classicist's axiomatisations. By enhancing the expressive power of the structural language in the proof-theory (by multiple-conclusion and primitive denial, respectively), the proof-theoretic gap between classical and intuitionistic logic is closed. That is not to say that there are not other resources the classicists employ, but at least these two are both live options.

But, even if the intuitionist project is at an impasse, it is a separate issue whether PTS constraints have a broader semantic and revisionary significance. In the beginning of Chapter 2 we learned how proof-theoretic constraints were aimed at ruling out tonk and related connectives. The general problem of an embarrassment of riches for PTS—of which tonk is a symptom—does not stand or fall with the more specific revisionary ideology promoted by the intuitionist camp. In fact, one might argue, irrespective of which logical flag one is sailing under, tonk and its ilk are a problem.

Thus, even if the foregoing proof-theoretic constraints cannot motivate a divide between intuitionistic and classical logic, they might still hold the key to divorcing PTS from Prior's objection. We know from Belnap that conservativeness is sufficient (given some background assumptions about the context of deducibility) to avert disaster. Yet, it was equally clear that harmony-as-conservativeness comes short of what we expect from our formalisation of harmony. Specifically, conservativeness does nothing to prevent elim-rules that are too weak. In the larger picture, then, conservativeness is a lopsided constraint—it only ensures that the consequences drawn from a λ statement do not outstrip the grounds for asserting it. More, since conservativeness is not necessary for harmony, one might suspect that a better proof-theoretic analysis of the concept is still forthcoming.

Is there any improvement to be had in turning to intrinsic harmony, i.e., 'the levelling of local peaks'? We have already seen a host of (primarily classical) natural deduction systems which are normalisable although they do not in general support conservativeness for every order of introducing their connectives (e.g., Peirce's Law again). As discussed above, the central issue is that the normalisation theorem is a global property of a proof system. There might be permutational or simplificational moves available to institute normalisation even if there are intro- and elim-rules that do not display the sort of neat conversions that Prawitz identified. Classical logic is a case in point.

Another example is that of the modal logic **S4**. Prawitz (1965, ch. VI) shows that **S4** is normalisable with the following rules adopted from Curry (1950):

$$\frac{\genfrac{}{}{0pt}{}{\Gamma}{\genfrac{}{}{0pt}{}{\vdots}{A}}}{\Box A} \; (I\Box_{S4}) \qquad \frac{\Box A}{A} \; (E\Box_{S4})$$

where for $I\Box_{S4}$ each $B \in \Gamma$ is *modal* (i.e., $B = \Box C$ or $B = \bot$). Read (2008b) made the point that whereas normalisation does hold for these systems, one is hard pressed to say that the modal rules are in harmony. Actually, the details of Prawitz's proof reveals that adopting an alternative—and restricted—definition of an **S4**-*derivation* (i.e., what is to count as a derivation in the proof-system—the rules remain the same) is critical for the success of the result. The rules themselves, on the other hand, appear to be disharmonious in so far as $I\Box_{S4}$ is restricted by the side-condition. In fact, **S5** shares the same elim-rule as **S4** but the corresponding intro-rule $I\Box_{S5}$ only requires that the members of Γ be modal or co-modal (i.e., $\neg\bot$ or $\neg\Box$).[1]

The pertinent question is whether normalisation has any direct impact on the legitimacy of `tonk`. In general, the answer is unfortunately no. Even if `tonk` in standard environments (say, if added to classical logic) wreaks havoc in the form of irremovable maximum formulae, there is nothing preventing us from having surprising definitions of derivation that block the unholy coupling of `tonk`-intro and -elim. In particular, non-transitive systems where some derivations cannot be chained together might provide a normalisable habitat for `tonk`. In Section 6.4.4 we return to the details of this observation.

However, in the presence of an ordinary consequence relation, we might still profit from Dummett's idea of intrinsic harmony and Prawitz's inversion principle. The challenge is to give a local formulation of the harmony that encapsulates the spirit of the earlier proposals. In the process, we might find that there are systematic

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[1]See the discussion of functionality of harmony in Section 4.2.2.

connections with normalisation, but that normalisation entails that such a new harmony constraint will not be one of them.

4.2 Harmony Localised

4.2.1 Harmony-as-Reduction

In the diagnosis of where harmony-as-conservativeness and -normalisation go wrong, we have already hinted at a more appropriate take on harmony. The common problem with the two approaches explored so far—total and intrinsic harmony—is that they only constrain inference rules to the extent that they contribute to *global* properties of the proof-system, i.e., conservativeness and normalisation. But, inference rules which appear blatantly to disregard our pre-formal conception of harmony might facilitate normalisation and other global properties as long as they are introduced into an appropriate proof-theoretic framework. This is one of the lessons learned by the above exploration of PTS for classical logic.

In order to keep the inference rules behind a normalisable system in check, we must impose a *local* constraint which tracks the relation between grounds for and consequences of its logical constant. Read suggests that "[w]hat is needed is a rethink of the notion of intrinsic harmony, to discern the true relationship between the introduction- and elimination-rules" (Read 2000, p. 129). Rather than concerning ourselves with normalisation directly, we should revisit Prawitz's inversion principle and conversion steps. Motivated by the fact that these were constraints that operated locally on the inference rules, the hope is that a formal notion of harmony can be teased out. To separate it from its predecessors we call the new approach *harmony-as-reduction*.

4.2.2   Harmony and Functionality

To set the scene for harmony-as-reduction, let us revist Gentzen's brief remarks on PTS. Following the famous passage on introductions represening the 'definitions' of logical connectives (Gentzen 1934, p. 80), he continues by saying:

> By making these ideas more precise it should be possible to display the
> E-inferences as *unique functions* of their corresponding I-inferences, on
> the basis of certain requirements. (ibid., emphasis added)

There is an important idea here. Proof-theoretic harmony, in whatever formal guise we prefer, ought to be a *function* yielding elim-rules as output for intro-rules. In other words, harmony is not simply a matter of a test telling us whether a pair of rule sets $<I, E>$ obeys some formal constraint, there ought to be a set of elim-rules as functional value set of intro-rules as functional argument. Even more, it ought to give us a *unique* set of elim-rules for each well-formed input. This is non-trivial. There are examples of normalisable natural deduction systems where different intro-rules yield the same elim-rule. More formally, we have a *non-injective function f*: There might be two inputs x, y such that $x \neq y$ but $f(x) = f(y)$.

Recall, for example, the rules $I\square_{S4}$ and $I\square_{S5}$ above (Section 4.1), and the fact that they are both paired with the same elim-rule. Similarly, the modal system **K** can be given by requiring that if A depends on Γ, then the conclusion $\square A$ of $I\square_K$ depends on $\square\Gamma$, i.e., $\{\square B \mid B \in \Gamma\}$:[2]

$$\frac{\Gamma \Rightarrow A}{\square\Gamma \Rightarrow \square A} \ (I\square_K)$$

Worse, if the system has $\diamondsuit$ as primitive the ensuing inference rules are not even functional. The two standard **S4** rules are due to Fitch (1952):

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[2] The rule is in sequent style in order to explicate the difference. See Read (2008b) for further details.

$$\frac{A}{\Diamond A} \ (I\Diamond) \qquad \begin{array}{c} \quad [A]^u \\ \quad \vdots \\ \Diamond A \quad C \\ \hline C \end{array} (E\Diamond)(u)$$

where for $E\Diamond$, every assumption on which the minor premise C depends on (except A itself) is modal, and C is co-modal. Again, as with the $\Box$-rules, by loosening the restriction, this time on the elim-rule, we get an **S5** system. This time, however, the result is even more puzzling: For someone (like Gentzen or Read) who takes intro-rules to be conferring meaning on connectives, and the elim-rules to be mere 'corollaries', it is presumably unacceptable that the same set of intro-rules could harmoniously be paired with two distinct sets of elim-rules. Of course, if your preference is for *elim-rules* as semantically prior, the problems have been dualised—the $\Box$ rules fail on account of functionality, the $\Diamond$-rules on account of injectivity. Regardless, the situation is uncomfortable.[3]

Dummett (1991, pp. 285-87) discusses another example. If we compare the standard disjunction elimination rule with its counterpart in the quantum logic **Nqp**, $E\veebar$, we see that they are only separated by the presence of contexts (Θ, Δ) in the subderivations:[4]

Example 4.1. Quantum logic, **Nqp**:

$$\begin{array}{c} \Gamma \quad [A]^u \quad [B]^v \\ \vdots \quad \vdots \quad \vdots \\ A \veebar B \quad C \quad C \\ \hline C \end{array} (E\veebar) \qquad \begin{array}{c} \Gamma \quad \Theta, [A]^u \quad \Delta, [B]^v \\ \vdots \quad \vdots \quad \vdots \\ A \vee B \quad C \quad C \\ \hline C \end{array} (E\vee)$$

The difference, although merely structural, underpins the nonderivability of the law of distributivity (for $\wedge$ over $\veebar$) in **Nqp**. Again, it is a situation where the elim-rules are different, but the intro-rules for $\vee$ and $\veebar$ are the same. This is the case despite the fact that $E\veebar$ equally well supports the conversion step with

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[3]Read suggests a solution for modal operators that we revisit in Section 5.3.6.
[4]Proof-theory for quantum logic is dealt with in, e.g., Nishimura (1980), Cutland & Gibbins (1982), and Faggian & Sambin (1998).

$I\vee$. Dummett suggests that the pairing of $E\veebar$ with $I\vee$ is *unstable* since a system with the connectives $\{\vee, \veebar\ \wedge\}$ will collapse as we can derive $A \vee B$ from $A \veebar B$. Consequently, $\{\vee, \veebar\ \wedge\}$ is a non-conservative extension of $\{\vee, \veebar\}$ since the law of distributivity, for $\veebar$, becomes provable.

Finally, an interesting example that is not usually mentioned is $\bot$ in Prawitz's **Nip** and **Ncp**. Recall that classical and intuitionistic logic here have different elim-rules (EFQ and $E\bot_C$ respectively), yet the same 'introduction rule'. Granted, this is a truth with modification since $\bot$ apparently does not have an intro-rule, but Prawitz prefers saying that the nullary connective has a limit intro-rule, namely the null-rule. Prawitz's response is a bit hard to grasp. Unlike for example Tennant, he insists that $\bot$ does have determinate propositional content, so, as with other connectives, he needs a semantic story. Since Prawitz has all the meaning-theoretic eggs in the intro-rule basket, he prefers the somewhat artificial reply that there is a default null-rule which is harmony with the respective elim-rules for $\bot$. However, unless there are two structurally distinct null-rules, it appears that both elim-rules (the classical and the intuitionistic) are harmonious with respect to the same rule.[5]

In general, then, expecting the elim-rule output to be a unique function appears to put pressure on a number of rule-sets. We might identify two different strategies handling the issue of functionality in light of this: First, the hard-liners might propose that we simply decree that non-functional operators are non-logical. Thus, modal operators, and perhaps a series of more or less exotic connectives will not meet the criterion of logical constanthood. I have little sympathy for this approach. The overarching principle for formulating PTS should—as earlier stated—be to include a wide variety of logics. The fact that modal operators are tricky to treat proof-theoretically (while usually easy to treat model-theoretically) is hardly an excuse.

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[5]See also Section 2.3.3.

Second, one might simply feel obliged to drop functionality as a property of harmony, irrespective of what the other details of the relation turn out to be. This comes with a cost, however. Harmony, in the informal sense described by Dummett, appears to presuppose functionality. Assuming that two distinct elim-rules yield different classes of consequences, it appears inevitable that at least one of them will either be too weak or too strong. For, if harmony is a matter of the elim-rule providing exactly the consequences which are already warranted by the grounds for the intro-rule, one of the rules must be under- or overshooting. Similarly, if one prefers a semantics where the elim-rules are privileged, the same moral ought to apply to intro-rules.

Either way, we are left with the awkward situation that the harmony constraint does not determine which inference is the correct one. A initial thought might be to suppose that whenever harmony provides more than one rule set as output, we always go with the deductively stronger set. But this is a strategy that should give us considerable pause: Increased deductive strength often (but not always) comes with a corresponding weakening of *discriminatory strength*, i.e., the ability to to make logical distinctions.[6] Think, for example, of the difference between modal logics, where stronger systems tend to collapse strings of modalities that are deductively distinct in weaker systems. Model-theoretically, we observe that the weaker systems have more models, and thus more potential counter-models. Who is to say that the practice of refuting, as opposed to that of proving, is not to be preferred when we adopt one inferential practice over another?

Evidently, none of the harmony constraints investigated so far involves a functionality constraint in this sense considered here. Yet, there is the feeling that since it is directly inspired by Prawitz's inversion principle, harmony-as-reduction is anticipating precisely this idea. Even if Prawitz simply displays the pairs and observes that they share a feature, namely loyalty to the inversion principle, the

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[6]For details for and complications with the notion of discriminatory power, see the excellent paper Humberstone (2005).

suggestion is to investigate the principle further to uncover the method by which we can produce the correct elim-rules on the basis of intro-rules.

4.3 GE-Harmony

4.3.1 General Elimination Rules

In a series of articles, Read has developed a harmony-as-reduction approach which promises to deliver a series of improvements on the traditional accounts:[7]

- injective (one-to-one) functionality;

- induction of elim-rules from intro-rules;

- a more accurate analysis of harmony vs normalisation;

- a novel diagnosis of tonkitis.

The resulting notion, called *generalised elimination harmony* (GE-harmony) by Francez & Dyckhoff (2009), is based on some observations about natural deduction rules first made by Schroeder-Heister (1984a).[8] The paper provides schemata for intro- and elim-rules for any n-ary operator which functions as *standard templates*. Its novelty lies in the power to let sets of intro-rules *induce* corresponding sets of elim-rules.[9] GE-harmony is not only a better analysis of Gentzen's original remarks, it is probably the most sophisticated means of understanding the challenge posed by `tonk`.

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[7]Read (2000, 2008b, 2008a).

[8]The extension is originally from Schroeder-Heister (1981). See also Avron (1990) for a connection to sequent calculus.

[9]Schroeder-Heister also introduces the idea of assuming and discharging *rules* (or subderivations) as hypotheses. I am indebted to Allen Hazen for pointing out that this generalisation is anticipated in Fitch (1966). We return to this issue below.

Before we look at the approach more schematically, it is worth revisiting the inversion principle from Section 2.3.3. We are told that if an application of an elim-rule $E\lambda$ has a consequence C, then the intro-rules, $I\lambda$, combined with the subderivations of minor premises in $E\lambda$ (if any), already yield a derivation of C without the application of $E\lambda$. Inspecting the laundry list of connectives, we see that the moral expressed by Prawitz's inversion principle is obscured by the fact that elim-rules come in different forms. Whereas conjunction offers the genuine inversion that Weir requested (see Section 3.2.4), disjunction has an elim-rule that not only involves subderivations but which ultimately derives an arbitrary formula (which, of course, need not be a subformula of the major premise). If we take the difference seriously, we will notice that what $E\vee$ says is that anything which can be derived from $A \vee B$ can be derived directly from the grounds for introducing $A \vee B$, i.e., from A and B independently.

Thinking of this as a recipe we can give a corresponding rule for $\wedge$:[10]

Example 4.2. GE$\wedge$:

$$\frac{A \wedge B \quad \overset{\displaystyle [A,B]^u}{\underset{\displaystyle C}{\vdots}}}{C} \ (GE\wedge)(u)$$

We then get a corresponding conversion step reminiscent of the $\vee$ case—the *generalised elimination rule* for $\wedge$:

$$\frac{\dfrac{\Pi_1}{A_1} \quad \dfrac{\Pi_2}{A_2}}{A_1 \wedge A_2} \quad \dfrac{[A_1, A_2]^1}{\dfrac{\Pi_3}{C}}}{C} \ (1) \quad \rightsquigarrow \quad \frac{\dfrac{\Pi_1}{A_1} \quad \dfrac{\Pi_2}{A_2}}{\dfrac{\Pi_3}{C}}$$

In the GE form, the $\wedge$-rules follow the style of the standard $\vee$-rules. The reduction is simply an explication of the point that anything which can be derived

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[10]This generalisation also owes to the development of linear logic where we distinguish between *multiplicative* and *additive* conjunction (see Girard 1987). We leave the details of this discussion to Section 4.5.2 below.

from the introduced formula can be derived from its grounds directly. In Read's favoured terminology, we can say that the intro-rules not only expound the *sufficient* grounds for asserting a statement, they also implicitly characterise the *necessary* consequences of asserting the very same statement. If not, there would be no sense in which the elim-rules—which patently concern the necessary conditions— could be induced from the corresponding intro-rules (but see also footnote 41, Chapter 3).

What is the relationship between the GE-rule and the standard $\wedge$ rules? If we let $C = A$ we see that the subderivation in $GE\wedge$ becomes a trivial derivation from $\ulcorner A,\ B \urcorner$ to A. Assuming something like reflexivity and weakening, i.e., that for any A, B:[11]

$$
\begin{array}{c}
A, B \\
\vdots \\
A
\end{array}
$$

we can disregard the subderivation, and the resulting rule is a standard $E\wedge$ rule for A; similarly for B. Conversely, we can derive the generalised rule from the standard rules by observing the following:

$$
\begin{array}{c}
\dfrac{A \wedge B}{A} \quad \dfrac{A \wedge B}{B} \\
\underbrace{\qquad\qquad} \\
\vdots \\
C
\end{array}
$$

Here we need two applications of the standard $E\wedge$, followed by the subderivation of C from $(A,\ B)$.[12] The result is simply a permutation of the GE-rule.

We might then venture to give a general schema inspired by these examples. First, let us look at some schemata occurring in Francez & Dyckhoff (2009). The

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[11]Compare the axiom $\ulcorner \Gamma,\ A \Rightarrow A,\ \Delta \urcorner$ in the **G3**-style sequent calculus. In natural deduction, however, the structural rules are absorbed as *discharge policies*. This is a topic that will loom large in what follows (see especially Section 5.3.5).

[12]Again, the derivation tacitly assumes that something like *contraction* is possible (more precisely, vacuous discharge).

schemata are slightly modified from their originals: We ignore complications arising from quantifiers, and we use notation which is different in inessential ways. Keep in mind, however, that in Section 4.4 we will discuss some grievances with the particular formulation. We will then proceed to develop an improved version of the GE-template which differs from both Francez & Dyckhoff (2009) and Read (2008a).

The schematic intro-rule for a logical constant λ occuring as principal connective in a formula φ is as follows:

$$\frac{\overset{\displaystyle [\Sigma_i]^{j_1,\dots,j_{m_i}}}{\overset{\displaystyle \hat{\Pi}_i}{\Delta_i}}}{\varphi} \,\, (\delta I^{j_1,\dots,jm})_i$$

Let us explain the notation: The subscript i (on Σ) indicates that that this is the ith intro-rule for the operator λ in φ, where $i \in \{1, \dots, n\}$. $[\Sigma_i]^{j_1,\dots,j_{m_i}}$ are (possibly empty) sets of assumptions discharged by δI, and Δ_i are sets of formulae (possibly empty).

As an example, take $I\wedge$ (Example 4.2): There is only one intro-rule (so $i = 1$), and Σ is empty as the rule infers $A \wedge B$ as φ immediately from $\Delta = \{A, B\}$. More interestingly, $I\rightarrow$ has $\Sigma = \{A\}$, and discharges the formula after deriving $\Delta = B$ and concluding $\varphi = A \rightarrow B$. (A brief warning: As we will see when we later develop GE-harmony for multiple conclusion, the current notation adopted from Francez & Dyckhoff (2009), where Δ as the premises of the intro-rule, is not convenient.)

The suggestion, then, is to (harmoniously) induce the following GE-rule:

$$\frac{\begin{matrix} & & & & [\Delta_1]^1 & & [\Delta_n]^n \\ & & & & \Pi'_1 & & \Pi'_n \\ \dfrac{\Pi}{\varphi} & \dfrac{\Pi_1}{\Sigma_1} & \dots & \dfrac{\Pi_n}{\Sigma_n} & \psi & \dots & \psi \end{matrix}}{\psi} \,\, (\delta GE)(1,\dots,n)$$

Call this the GE-*induced* rule. Notation: ψ is an arbitrary formula. φ is the major premise, Σ_i are (sets of) minor premises corresponding to the assumptions of intro-rules, Δ_i are (sets of) assumptions for subderivations corresponding to the grounds in intro-rules. Each of the assumption sets Δ are discharged upon concluding ψ.[13] Examples: For our GE-rule for $\wedge$ it is evident that there is only one set of grounds, $\Delta_1 = \{A, B\}$ (but no minor premises Σ). Upon deriving C from the set, one discharges the assumption set and concludes C from the major premise $\varphi = A \wedge B$.

What about $GE{\rightarrow}$? Given the description of $I{\rightarrow}$ above we can plot out the induced GE-rule by transferring the content of the intro-rule to the GE-schema. The resulting rule is as follows:

Example 4.3. GE$\rightarrow$:

$$
\dfrac{A \rightarrow B \quad A \quad \overset{\displaystyle [B]^u \atop \displaystyle \vdots}{C}}{C} \; (GE{\rightarrow})(u)
$$

This rule appears to have been first formulated for natural deduction in Dyckhoff (1987). It is later used for developing an intuitionistic relevant logic in Tennant (1992, 2002), and also for translations between natural deduction and sequent calculus in von Plato (2001).[14] As before, the GE-rule is equivalent to the standard $E{\rightarrow}$, i.e., *modus ponendo ponens*, given some structural assumptions. To see that it is a special case of the GE-rule, simply let $C = B$. For the other direction we have the permutation:

$$
\dfrac{A \rightarrow B \quad A}{B} \atop {\vdots \atop C}
$$

---

[13] Note that the subscripts tracking the intro-rules and the superscripts tracking the discharges are identical. There is no confusion: We are simply using the same ordering on the subderivations corresponding to intro-rules (left-to-right) as we are on the different discharges.

[14] A similar typed rule also appeared earlier in Martin-Löf (1984, p. 44). Note that Tennant 2002 calls GE-rules for *parallelised* rules (as opposed to serial rules).

However, this is not the rule suggested by Schroeder-Heister, nor is it the rule adopted by Read in his account of GE-harmony (which differ from Francez & Dyckhoff 2009). Rather, in the case of the $GE{\rightarrow}$ rule—and presumably all intro-rules involving subderivations—they employ the idea of the *higher-order rule*, i.e., an inference rule where subderivations themselves can be assumed and discharged. With higher-order rules, one can schematise GE-rules in a more straightforward manner, albeit with the cost of having variables that range over both formulae and subderivations. Read offers the following template where intro-rules are understood as giving grounds Σ_i which are ambiguous between direct grounds (as in $I\wedge$) and subderivations involving discharge (as in $I{\rightarrow}$):

$$\cfrac{\varphi \quad \overset{\textstyle [\Sigma_1]}{\overset{\vdots}{\psi}} \quad \cdots \quad \overset{\textstyle [\Sigma_n]}{\overset{\vdots}{\psi}}}{\psi}$$

The critical difference that leads to an output which deviates from the former template is that Σ_i might stand proxy for a subderivation, written $\chi \Rightarrow \xi$. As a consequence, the schema returns the following GE-rule for $\rightarrow$:[15]

Example 4.4. Higher-order rule for $\rightarrow$:

$$\cfrac{A \rightarrow B \quad \overset{\textstyle [A \Rightarrow B]^u}{\overset{\vdots}{C}}}{C} \ (GE{\rightarrow}')(u)$$

We call this type of rules *Schroeder-Heister-style* rules to distinguish them from the *Dyckhoff-style* rules. Strictly speaking, the rule is different from the first rule we gave for $\rightarrow$. $GE{\rightarrow}$ says that whenever we can derive C from the assumption B, we can derive the conclusion C from the major premise $A \rightarrow B$ together with a minor premise A, and subsequently discharge the assumption B. In contrast, its

---

[15]Although the idea of assuming and discharging rules might be philosophically problematic, it has some advantages. Apart from the role it plays in the harmony debate, there is also some evidence that certain connectives can only be axiomatised with such a device. See Hazen (1996) for the example of $((A \rightarrow B) \rightarrow B)$ in intuitionistic logic.

higher-order counterpart says that whenever we can derive C on the assumption that B can be derived from the assumption of A, we can derive the conclusion C from the major premise $A \to B$, and subsequently discharge the assumption $A \Rightarrow B$. Yet, even if the rules are not the same—there certainly may be structural environments in which they come apart—they are closely related. Especially, note that $GE\to'$ does not discharge A, it discharges the derivation from A to B. Of course, if it did, the rule would result in a simplified rule much stronger than *modus ponendo ponens*.

It hardly needs mentioning that giving all-encompassing schemata for intro- and -elim rules is a tall order. Nonetheless, there is a lot to be said for trying. First, it might assist not only our understanding of harmony, but also the understanding of reduction and normalisation (more on this to follow in Section 4.3.3). Second, it is a critical part of giving *adequacy* (or completeness) results for intuitionistic logic in PTS. Early work was done by both by Prawitz (1978), Zucker & Tragesser (1978), and Schroeder-Heister (1984a). They all rely on developing templates for intro- and elim-rules, and consequently face, either explicitly or implicitly, the difficult question of what is to count as an intro- and elim-rule respectively. Typically, rules like those presented by Milne (see footnote 53) do no fit the mold if interpreted as intro-rules. This should not be taken as an argument against such deviant rules, however, but rather as an incentive to improve on the templates. Although, we do not consider rules of Milne's specific form, we will attempt to achieve some level of generality in what follows. But first we turn to one of the advantages with the first formulation of GE-harmony.

4.3.2   Tonk **GE-harmonised**

What happens with tonk in the context of GE-harmony? Clearly, if it is to be any help for the development of PTS, tonk cannot pass the GE-harmony test. Fortunately, seeing that tonk-intro rules GE-induce elim-rules quite different from

its standard elim-rules is an easy exercise. As before, we input the rule in the intro-schema and proceed to transfer it to the GE-template. The result is the following rule pair:

Example 4.5. `Tonk`:

$$
\cfrac{A}{A\,\texttt{tonk}\,B}
\qquad
\cfrac{A\,\texttt{tonk}\,B \qquad \begin{array}{c}[A]^u\\ \vdots\\ C\end{array}}{C}\ {\scriptstyle(u)}
$$

Not only is this not the standard `tonk`-elim rule, it does not generate a singular calculus (i.e., it is not the case that $A \vdash B$ for each A, B). We can readily see that the simplified form of the rule, where $C = A$, gives a rule that obviously allows for an harmless conversion step with the intro-rule. Moreover, the disjunctive nature of `tonk`-intro is revealed by adding the corresponding rule with B as only premise (i.e., `tonk`$^+$). As expected GE-harmony then induces the standard rule disjunction rule $E\vee$.

Dually, GE-harmony also deals with disharmony caused by weak elim-rules (strong intro-rules) where the grounds for asserting the statement cannot be recaptured by the consequences that we can draw from it. Recall the dysfunctional connective `tunk` from Section 2.3.2:

$$
\cfrac{A \quad B}{A\,\texttt{tunk}\,B}\ {\scriptstyle(I\texttt{tunk})}
\qquad
\cfrac{A\,\texttt{tunk}\,B \qquad \begin{array}{c}[A]^v\\ \vdots\\ C\end{array} \qquad \begin{array}{c}[B]^u\\ \vdots\\ C\end{array}}{C}\ {\scriptstyle(E\texttt{tunk})(u,v)}
$$

Given the injective functionality of GE-harmony, and the fact that `tunk`-intro is simply the standard $I\wedge$, we know that the output will be $GE\wedge$. Thus, both of these examples are excluded by GE-harmony. A promising start, but even if `tonk` is the litmus test for harmony, it is not conclusive evidence that GE-harmony is the right constraint. We want to know how GE-harmony relates to other interesting properties, what happens with more controversial connectives (e.g., negation), and

how it handles exotic rules. Before we move on this in later sections, however, let us first revisit normalisation and take a first look at the connection with GE-harmony.

4.3.3   Normalisation and GE-Harmony

Given that GE-harmony is explicitly informed by Prawitz's inversion principle, we should expect there to be a connection with normalisation. After all, we have seen that well-behaved conversion steps (together with auxiliary simplifications and permutations) lead to the normalisation theorem in a range of cases. Is GE-harmony a guide to normalisation?

Let us first observe how the two schema for intro- and elim-rules interact. Letting φ be the maximum formula in an application of the intro-rule followed by an application of the elim-rule, the Francez & Dyckhoff template gives the following reduction:[16]

$$
\cfrac{\cfrac{[\Sigma_i]^{j_1,\ldots,j_{m_i}}}{\hat{\Pi}_i} \quad \cfrac{}{} \quad \cfrac{}{} \quad [\Delta_1]^1 \quad [\Delta_n]^n}{\cfrac{\Delta_i}{\varphi}_{(\delta I^{j_1,\ldots,j_m})_i} \quad \cfrac{\Pi_1}{\Sigma_1} \quad \ldots \quad \cfrac{\Pi_n}{\Sigma_n} \quad \cfrac{\Pi_1'}{\psi} \quad \ldots \quad \cfrac{\Pi_n'}{\psi}}{\psi}_{(\delta GE)(1,\ldots,n)}
$$

which can be reduced to

$$
\begin{array}{c}
\Pi_i \\
\Sigma_i \\
\hat{\Pi}_i \\
\Delta_i \\
\Pi_i' \\
\psi
\end{array}
$$

However, the schematic reduction glosses over crucial details. Given an instantiation of the schema, there is no guarantee that the resulting reduction will enable

---

[16]Importantly, Francez & Dyckhoff (2009) are not trying to establish a connection with normalisation. Rather, they are interested in the connection with what they call *Local Intrinsic Harmony* (see ibid., p. 7-8). Local Intrinsic Harmony consists of *local soundeness* and *local completeness*, two notions adopted from Pfennig & Davies (2001). The upshot of their investigation is that GE-harmony entails Local Intrinsic Harmony.

an inductive step in a proof of normalisation. Whether or not this is the case depends, *inter alia*, on the distribution of connectives over the schema's paramaters. Recall, for example, the discussion of $E\bot_C$ in Section 3.2.1. We return to the question about harmonious rules for classical negation in Section 4.5.1, but for now we will look at another instructive example.

Read (2000) gives an illuminating example that shows that normalisation is not entailed by GE-harmony. In fact, the example shows more!

Example 4.6. Bullet:

$$\begin{array}{cc}
\begin{array}{c}
[\bullet]^u \\
\vdots \\
\dfrac{\bot}{\bullet} \ (I\bullet)(u)
\end{array}
&
\begin{array}{c}
 \\
\dfrac{\bullet \quad \bullet \quad \begin{array}{c}[\bot]^u \\ \vdots \\ C\end{array}}{C} \ (E\bullet)(u)
\end{array}
\end{array}$$

It is straightforward to check that the connective $\bullet$, called *bullet*, is GE-harmonious.[17] Yet, it displays a behaviour that led Read to call it a 'proof-theoretic Liar'. It turns out that, in some respect, bullet is worse than `tonk`:[18] In the presence of *EFQ* (and contraction) it proves $\Gamma \vdash A$, for any $<\Gamma, A>$ (including $\Gamma = \emptyset$). First, we see that by letting $C = \bot$ we get the simplified derived rule (where $\bot \Rightarrow \bot$ is considered trivial by reflexivity):[19]

$$\dfrac{\bullet \quad \bullet}{\bot} \ (SE\bullet)$$

Assuming further that contraction allows us to simplify this to a rule with one instance of $\bullet$, we can give the following derivation:[20]

---

[17]The connective was originally called 'blob' but I prefer 'bullet' following the LaTeX code for the symbol. In retrospect, perhaps it ought to have been called 'the Scottish Constant'.

[18]Compare `super-tonk` in Section 4.4.3.

[19]By the standard move we can permute the subderivation of C from $\bot$ to show that we can retrieve the initial rule from the simplified one.

[20]This involves some cheating. For the more detailed look at the bullet-derivation of inconsistency, see Section 4.4.4.

$$\frac{\dfrac{[\bullet]^1}{\bot}\;(SE\bullet)}{\dfrac{\bot}{\bullet}\;(I\bullet)(1)}\;(SE\bullet)$$
$$\frac{\bot}{C}\;(EFQ)$$

Allowing for negation in the language, and the rules $I\neg$ and $E\neg$, we can also derive both A and $\neg A$ for any formula A.

This is alarming. GE-harmony does not entail consistency, and, as a consequence, since normalisation entails that $\bot$ is nonderivable (see Prawitz 1965, p. 44), GE-harmony cannot entail normalisation either.[21] Obviously, neither the Subformula nor the Separation Property will hold, and $\bullet$ yields a non-conservative extension of any consistent system. In fact, Read reaches the same conclusion by observing that the conversion step for $\bullet$ cannot contribute to the inductive proof of normalisation:

$$
\frac{
\dfrac{\dfrac{[\bullet]^1}{\Pi_1}}{\dfrac{\bot}{\bullet}\;(1)}\quad \dfrac{\Pi_2}{\bullet}\quad \dfrac{\dfrac{[\bot]^2}{\Pi_3}}{C}
}{C}\;(2)
\quad\rightsquigarrow\quad
\dfrac{\dfrac{\dfrac{\dfrac{\Pi_2}{\bullet}}{\Pi_1}}{\bot}}{\dfrac{\Pi_3}{C}}
$$

Roughly put, the problem is that a copy of $\bullet$ is transfered from the original derivation to the converted derivation, and there is no guarantee that it does not form a maximum formula (again, this will depend on the structure of Π_1 and Π_1).[22] We return to the details of this conversion in a more general setting later (see Section 4.4.4 for details). For now, we are content to notice that there is noticeable gap between GE-harmony and other conceptions of harmony discussed in Chapter 2. Since revision due to failure of GE-harmony is separate from conservativeness, normalisation, and the Separation Property, it is worthwhile to return the revision of classical negation discussed in Chapter 3.

---

[21] Given some plausible assumptions about the logic for $\bot$.

[22] Compare the case of $E\bot_C$ as intro-rule where $\neg A$ appears in the converted derivation, but where the 'maximum formula' is A. See Section 3.2.1.

Yet, the most puzzling result is that if GE-harmony is the correct formalisation of harmony, then harmony does not even entail consistency. Remember that Dummett called inconsistency 'the grossest form of malfunction' an inferential practice could have. Harmony, then, appears to fail as an all-purpose protection against semantic dysfunctionality. On closer inspection, however, the result might be less surprising. Belnap's and Dummett's original discussion of harmony and conservativeness ran two thoughts together: First, that the inferentialist ought not to allow rules that lead to inconsistent (alternatively, that trivialise); second, that the intro- and elim-rules must somehow have corresponding deductive strength (or, specifically, must obey the inversion principle). The confusion has led Dummett astray—the right notion of proof-theoretic harmony is neither total nor intrinsic harmony.

Nevertheless, that does not mean that Dummett was not right in claiming that inconsistency is a gross malfunction, and that it should be ruled out. It just turns out that the task of ruling out inconsistency is independent from the task of 'balancing' intro- and elim-rules. Read's achievement is to have separated the two issues clearly with a precise formalisation of proof-theoretic harmony. Our task is to elaborate on this observation by investigating the conditions under which GE-harmony also entails consistency.

4.4   Modification of GE-harmony

4.4.1   Double discharge

With the exception of bullet, we have so far only looked at GE-harmony with respect to familiar, so-called 'laundry list' connectives. However, we need to see whether the GE-template can also handle more intricate inference-rules, and, ultimately, more exotic proof-theoretic frameworks. In the present section we look at some inference rules that will require us to revisit the GE-template introduce in

Section 4.3. Later, in Section 4.5, we look at whether GE-harmony will also make sense in a multiple-conclusion setting.

In discussion, Roy Dyckhoff has pointed out that there are in fact fairly innocuous looking rules that present a challenge for GE-harmony. The upshot is that we must give up the idea of having a *single* elim-rule induced from a set of intro-rules. Let us consider two rules which are not particularly exotic, but which are more complicated than the intro-rules we have looked at so far. The two following rules, one negation intro-rule and one biconditional intro-rule, are different in that they involve two premises which are both subderivations.

Example 4.7. Double discharge rules:

$$
\frac{\overset{[A]}{\vdots} \quad \overset{[B]}{\vdots}}{A \leftrightarrow B} \; (I{\leftrightarrow}) \qquad
\frac{\overset{[A]}{\vdots} \quad \overset{[A]}{\vdots}}{\neg A} \; (Dil)
$$

There is *prima facie* nothing suspect about rules of this form. We have seen how rules like *Dil* can contribute to $\perp$-free presentations of intuitionistic and (in the case of *CDil* or *CRA*$^{\neg}$) classical logic. Of course, one might object that there are other rules which can do the same job, say, using $\wedge$ and $\rightarrow$ to define $\leftrightarrow$. But that is beside the point. In case we want primitive rules for a connective, PTS better be able to handle them, and, specifically, the question of harmony still applies.

Following the procedure we applied in Section 4.3, we try to apply the GE-template to $I{\leftrightarrow}$. However, the instantiation is no longer straightforward. For example, if we take our cue from the GE-rule for $\rightarrow$ we end up with a rule that appears misguided:

$$
\frac{A \leftrightarrow B \quad A \quad B \quad \overset{[A, B]}{\overset{\vdots}{C}}}{C} \; ?
$$

Letting $C = B$ or A now makes little sense as the premises involve both A and B themselves. The rule is a puzzling result for $\leftrightarrow$. It merely states that if you have a derivation of A and a derivation of B, then anything that follows from A and B, follows from A and B (together with $A \leftrightarrow B$). The major premise plays no part in the inference. Thus, it is difficult to imagine that the suggested rule can play a role in capturing the standard meaning of $\leftrightarrow$.[23]

Dyckhoff suggests that the solution is to accept that GE-harmony must allow for more than one GE-rule. Rather than inducing *the* GE-rule from a set of intro-rules, the schema ought to induce a set of GE-rules. A re-structuring of the GE-template is required: We want the above rules to deliver a set of GE-counterparts. As an example, the GE-schema should output the following rule pairs for the $\leftrightarrow$ rule:

Example 4.8. Bifurcated GE-rules for $\leftrightarrow$:

$$
\begin{array}{cc}
& [B] \\
& \vdots \\
\cfrac{A \leftrightarrow B \quad A \quad C}{C}
&
\quad
\cfrac{A \leftrightarrow B \quad B \quad \overset{\displaystyle [A]}{\underset{\vdots}{C}}}{C}
\end{array}
$$

Showing that these rules are equivalent with the standard elim-rules is routine. The single GE-rule for $\leftrightarrow$ has now been replaced by two rules which display the expected, bidirectional, behaviour of the connective.

A further example might help to shed light on the ambiguity in the original GE-template. Instead of providing a single intro-rule with double discharge, we now compare a connective with two intro-rules which both discharge an assumption. We do not have such a connective in the ordinary laundry list, but we can easily define a tertiary connective which takes the appropriate rules. Let us introduce a new tertiary connective $\twoheadrightarrow ABC$ with the following rules. It can be shown that $\twoheadrightarrow ABC$ is equivalent to $(A \to C) \vee (B \to C)$.

---

[23]The rules are not equivalent to the the pair of standard elim-rules:

$$
\cfrac{A \leftrightarrow B \quad A}{B} \; {}_{(E\leftrightarrow_i)} \qquad \cfrac{A \leftrightarrow B \quad B}{A} \; {}_{(E\leftrightarrow_{ii})}
$$

Example 4.9. Two elim-rules with discharge:

$$
\begin{array}{cc}
\begin{array}{c}
[A]^u \\
\vdots \\
\dot{C} \\
\hline
\twoheadrightarrow ABC
\end{array} \ {}^{(u)}
&
\begin{array}{c}
[B]^u \\
\vdots \\
\dot{C} \\
\hline
\twoheadrightarrow ABC
\end{array} \ {}^{(u)}
\end{array}
$$

We can now imagine a pattern of distinct structures for intro-rules, depending on the number of rules and the number of premises in each of them. Furthermore, it will make a difference whether a premise is a subderivation or not. The task at hand is to redevelop the GE-template to deal not only with the specific examples used here, but any set of intro-rules that follow this general pattern. We postpone any detailed treatment of Example 4.9 until we have introduced a modification of the GE-template.

4.4.2   Revised GE-template

In order to accommodate rules of this form, Read (2008a) has offered an amendment of his GE template. The key difference is that the GE-template is a way to harmoniously induce a *collection* of GE-rules. We here elaborate on the formal upshot by, first, re-configuring Read's template to Dyckhoff-style; and, second, testing a range of non-standard cases in order to conclude with some general facts about the output. We will then move on to discuss two unexplored topics: Conversion steps and normalisation for the new GE-templates, and, finally, a generalisation to multiple-conclusion natural deduction.

Like before, let δ be the formula with a principal occurrence of the connective in question. Let Π_i be grounds for introducing δ, where $0 \leq i \leq m$. Then for each Π_i we write the intro-rules as follows:

$$
\frac{\pi_{i_1} \quad \ldots \quad \pi_{i_{n_i}}}{\delta}
$$

Correspondingly, there is a collection of GE-rules for δ, written:

$$\delta \quad \overset{[\pi_{1_{j_1}}]^u}{\underset{\gamma}{\vdots}} \quad \dots \quad \overset{[\pi_{m_{j_m}}]^v}{\underset{\gamma}{\vdots}}$$
$$\frac{}{\gamma}$$

Informally, every GE-rule is such that for each intro-rule i there is a derivation of an arbitrary formula, γ, from π_{i_j} for some j. As an example, take the standard $I\vee$ rules where $m = 2$, $n_1 = 1$, $n_2 = 1$ (indicating that there are two intro-rules, with one premise each). There is then only one GE-induced rule: The standard $E\vee$. Observe that for each intro-rule i, it requires a derivation of an arbitrary formula from one of the subderivations π_{i_j}. As there is only one to chose in each intro-rule, the output is a single GE-rule.

On the other hand, as opposed to the GE-rule in Example 4.2, $\wedge$ now bifurcates into two separate GE-rules:

Example 4.10. Bifurcated GE-rules for $\wedge$:

$$\frac{A \wedge B \quad \overset{[A]^u}{\underset{C}{\vdots}}}{C} \qquad \frac{A \wedge B \quad \overset{[B]^u}{\underset{C}{\vdots}}}{C}$$

Naturally, the two new GE-rules are equivalent to the simplified $E\wedge$ rules. However, there is a key difference between these rules and Example 4.2: Whereas the latter required both weakening and reflexivity for the equivalence, the former simply needs reflexivity. Weakening is still required, though, in order to show that the different GE-rules are equivalent.

The fact that the new Read-style GE-template does not differentiate between subderivations π that are discharging and those that are simply premises (e.g., π in $I\vee$ and $I\rightarrow$) gives us Schroeder-Heister style GE-rules where subderivations are themselves discharged. (See Example 4.4.) Returning to Example 4.7, this gives us the rules of the following sort:

Example 4.11. Higher-order GE-rules for $\leftrightarrow$:

$$
\dfrac{A \leftrightarrow B \qquad \overset{[A \Rightarrow B]^u}{\underset{C}{\vdots}}}{C} \ {\scriptstyle (u)}
\qquad
\dfrac{A \leftrightarrow B \qquad \overset{[B \Rightarrow A]^u}{\underset{C}{\vdots}}}{C} \ {\scriptstyle (u)}
$$

We can subsequently re-write these rules in the equivalent Dyckhoff-style, giving us the GE-rules seen above in Example 4.8.

So we see that $m = 1$, $n_1 = 2$ yields 2 GE-rules (as is the case for $\wedge$ and $\leftrightarrow$). What if $m = 2$, $n_1 = 2$ and $n_2 = 2$? To produce rule-sets with the required form is routine: Take a 4-ary connective $\sqcup ABCD$ with the following rules (it is equivalent to $(A \wedge B) \vee (C \wedge D)$):

Example 4.12. Two intro-rules (no discharge):

$$
\dfrac{A \quad B}{\sqcup ABCD} \qquad \dfrac{C \quad D}{\sqcup ABCD}
$$

According to the GE schema, we get:

$$
\dfrac{\sqcup ABCD \qquad \overset{[A]^u}{\underset{E}{\vdots}} \quad \overset{[C]^v}{\underset{E}{\vdots}}}{E}
\qquad
\dfrac{\sqcup ABCD \qquad \overset{[B]^u}{\underset{E}{\vdots}} \quad \overset{[D]^v}{\underset{E}{\vdots}}}{E}
$$

The two rules both satisfy the criterion of including a subderivation from each of the intro-rules, but they do not exhaust the permutations. In fact, the rules are incomplete as witnessed by the non-derivability of $B \vee C$ (or $A \vee D$). This is fixed by supplying the complementary rules:

$$
\dfrac{\sqcup ABCD \qquad \overset{[B]^u}{\underset{E}{\vdots}} \quad \overset{[C]^v}{\underset{E}{\vdots}}}{E}
\qquad
\dfrac{\sqcup ABCD \qquad \overset{[A]^u}{\underset{E}{\vdots}} \quad \overset{[D]^v}{\underset{E}{\vdots}}}{E}
$$

In other words, when $m = 2$, $n_1 = 2$ and $n_2 = 2$, we get an output of 4 distinct GE rules.

Let us return to Example 4.7 again, but this time the negation rule *Dil*. Applying the modified GE template, we get a bifurcation into an odd-looking rule-pair:

Example 4.13. Bifurcated GE-rules for $\neg$:

$$
\begin{array}{cc}
\begin{array}{c}
\quad\quad [B]^u \\
\quad\quad \vdots \\
\dfrac{\neg A \quad A \quad C}{C} \; (u)
\end{array}
&
\begin{array}{c}
\quad\quad [\neg B]^u \\
\quad\quad \vdots \\
\dfrac{\neg A \quad A \quad C}{C} \; (u)
\end{array}
\end{array}
$$

For the left-most rule: Since both B and C here are arbitrary (in particular, B does not occur in the major premise), a special case is the instance where $B = C$, giving us back a standard *EFQ*-like rule. Conversely, the subderivation of C from B can be permuted down by letting the conclusion of the standard *EFQ* be B. Strangely, then, we find that the leftmost rule is alone equivalent to the simplified rule. No more natural is the GE-rule induced by the Dyckhoff-Francez template:

$$
\begin{array}{c}
\quad\quad [B, \neg B]^u \\
\quad\quad \vdots \\
\dfrac{\neg A \quad A \quad C}{C} \; (u)
\end{array}
$$

Crudely put, the rule says that any formula follows from a formula and its negation, if it follows from some (possibly the same) formula and its negation. The puzzling structure of the rule is due to the fact that the negation occurs both in the conclusion of the intro-rule, and in one of the subderivations.[24]

---

[24]Another slightly puzzling case is a version of reductio used by Dummett. By the GE-template we do not get the the standard *EFQ* rule:

$$
\begin{array}{cc}
\begin{array}{c}
[A]^u \\
\vdots \\
\dfrac{\neg A}{\neg A} \; (u)
\end{array}
&
\begin{array}{c}
\quad\quad [\neg A]^u \\
\quad\quad \vdots \\
\dfrac{\neg A \quad A \quad C}{C} \; (u)
\end{array}
\end{array}
$$

There is no simplification that gives the fully general *EFQ* since in the trivial reflexive derivation C will have to be a negated formula.

Next let us return to the connective $\twoheadrightarrow$ in Example 4.9 where there are two intro-rules, both of which discharge assumptions. We get the following, somewhat intricate, GE-rule:

$$
\cfrac{\twoheadrightarrow ABC \quad A \quad \overset{\displaystyle [C]^u}{\overset{\vdots}{D}} \quad B \quad \overset{\displaystyle [C]^v}{\overset{\vdots}{D}}}{D} \ {\scriptstyle (u,v)}
$$

For each intro-rule $I\twoheadrightarrow$, the GE-rule divides a subderivation (the only one) into two minor premises: The assumptions, A and B, and the conclusions, C in both cases, where the latter is itself the assumption of a new subderivation. We have here put the rule in Dyckhoff style without higher-order derivations. The rule is equivalent to the obvious simplified rule:

$$
\cfrac{\twoheadrightarrow ABC \quad A \quad B}{C}
$$

We can show that the GE-rule can be recaptured with the simplified rule by permuting the subderivation:

$$
\cfrac{\twoheadrightarrow ABC \quad A \quad B}{\underset{\displaystyle D}{\overset{\displaystyle C}{\vdots}}}
$$

For the other direction, let $D = C$:

$$
\cfrac{\twoheadrightarrow ABC \quad A \quad \overset{\displaystyle [C]^u}{\overset{\vdots}{C}} \quad B \quad \overset{\displaystyle [C]^v}{\overset{\vdots}{C}}}{C} \ {\scriptstyle (u,v)}
$$

Further, recall Weir's non-standard $\vee$-intro rules from Section 3.2.4. Let us investigate what the ramifications of GE-harmony are.

Example 4.14. Weir's disjunction rules:

$$\dfrac{\begin{array}{c}[\neg B]^u \\ \vdots \\ A\end{array}}{A \vee B}\ (I\vee_1^W)(u) \qquad \dfrac{\begin{array}{c}[\neg A]^u \\ \vdots \\ B\end{array}}{A \vee B}\ (I\vee_2^W)(u)$$

The result are two GE-rules of a form which turns out to be equivalent to simplified rules which are just the standard disjunctive syllogisms:

$$\dfrac{A \vee B \quad \neg A \quad \begin{array}{c}[B]^u \\ \vdots \\ C\end{array}}{C}\ (GE\vee_1^W)(u) \qquad \dfrac{A \vee B \quad \neg B \quad \begin{array}{c}[A]^u \\ \vdots \\ C\end{array}}{C}\ (GE\vee_2^W)(u)$$

We can now begin to see the contour of the systematic GE-output. In particular, assuming that for any logical connective λ its intro-rules $I\lambda_1$, ..., $I\lambda_n$ have the same number of premises, n_i, we have that the number of GE-rules is $\#\text{GE} = n_i{}^m$.

$m \setminus n_i$	1	2	3	4	5
1	1	2	3	4	...
2	1	4	9	12	
3	1	8	27	64	
4	$\vdots$				

The information we can gather from this is limited, however. Intro-rules like those for $\wedge$ and those of Example 4.7 (where there is discharge involved) are not distinguished: They both have a single intro-rule ($m = 1$) with two premises ($n_1 = 2$), and so give two GE-rules as output. Yet they are obviously of different form. In the Schroeder-Heister style, only the intro-rules with discharge leads to higher-order rules; in Dyckhoff-style this means a splitting into two minor premises—the assumption from the intro-rule is the first minor premise, and the second is a subderivation from the formula derived form the assumption in the intro-rule to an arbitrary formula.

Needless to say, the GE-induced rules quickly become aritifical—but then keeping them natural is not the aim either. Rather, it is a systematic way of producing elim-rules that accord with the spirit of Prawitz's inversion principle (see Section 2.3.3). The GE template is meant to make precise the inversion idea that anything that follows from a λ-formula, must follow directly from its grounds.

As we have seen in many of the above examples, there are frequently simplified rule-sets that are equivalent with the GE-induced rule sets. Yet, there is a crucial difference which will be highlighted in what follows: The equivalence results standardly operates on some structural background assumptions. It is only with these in place, e.g., weakening, contraction, that the GE output can be equated with the simplified rules. So far we have been fairly relaxed in the structural bookkeeping, but we will shortly, in Section 4.5, become more fastidious about this dimension of the inference rules.

4.4.3   Super-Tonk

Before that, however, let us make a brief digression to look at an interesting contribution to the tonk-literature made by Wansing (2006). Wansing's paper is a response to a logic called tonk-logic, introduced by Cook (2005). Put concisely, Cook shows that there is a non-transitive, finitely many-valued semantics that validates the tonk-rules without trivialising the logic.[25] In other words, as Belnap anticipated, even tonk has its home. Wansing, however, wants to capitalise on Cook's insight by developing a framework for evaluation which frameworks a connective is trivialising with respect to. In particular, are there connectives which are trivialising with respect to all non-trivial frameworks? Even if this is not the case for tonk, it is the case for super-tonk.

Following Wansing we ignore the truth-functional aspect of tonk-logic for the time being, and concentrate on a logic L a as pair $\langle \mathcal{L}, \vdash \rangle$, $\mathcal{L}$ is a formal language and

---

[25]For the details about tonk-logic, see Section 6.4.4.

⊢ a provability relation.[26] We say that a connective λ is *trivialising* with respect to a class of logics Λ iff λ can be added to any logic in Λ, and for every such $L \in \Lambda$ the resulting logic is trivial, i.e., either $\vdash = \emptyset$ or $\forall A \forall B \ A \vdash B$.

Wansing defines a number of non-trivial classes of logics with different restrictions. For each one he considers a connective that trivialises. Here we will only look at the case of `super-tonk`:

Definition 4.1. Non-trivial logics:

$\mathcal{G} := \{L \mid \vdash \neq \emptyset \text{ and } \exists A \exists B \ A \nvdash B\};$

Definition 4.2. A connective is *non-trivially trivialising* iff it is trivialising with respect to $\mathcal{G}$.

Wansing then defines a nullary connective, `super-tonk`, which out-performs its ancestor `tonk` in precisely this respect:

Example 4.15. `Super-tonk` is non-trivially trivialising:[27]

$$\begin{array}{cc}
\begin{array}{c}
[C]^u \\
\vdots \\
\dfrac{D \quad A}{\texttt{super-tonk}} \ (I\texttt{super-tonk})(u)
\end{array}
&
\begin{array}{c}
A \\
\vdots \\
\dfrac{\texttt{super-tonk}}{B} \ (E\texttt{super-tonk})
\end{array}
\end{array}$$

Wansing concludes, whereas there is "a cure for tonkitis, no non-trivial notion of consequence is a cure for super-tonkitis." (ibid., p. 659)

Clearly, if `tonk` was bad for reasons having to do with PTS, it is hard not to say the same about `super-tonk`. So, even if the latter is a sort of limit case, i.e., non-trivially trivalising, we want its diagnosis to involve disharmony. Let us check, then, what happens when we apply the GE-template to I`super-tonk`. The rules has two premises, and so produces two GE-rules:

$$\begin{array}{cc}
\begin{array}{c}
[D]^u \\
\vdots \\
\dfrac{\texttt{super-tonk} \quad C \quad E}{E} \ (GE\texttt{super-tonk}_1)(u)
\end{array}
&
\begin{array}{c}
[A]^u \\
\vdots \\
\dfrac{\texttt{super-tonk} \quad E}{E} \ (GE\texttt{super-tonk}_2)(u)
\end{array}
\end{array}$$

---

[26]The provability relation ⊢ might be SET-FRML or SET-SET.

[27]Note that in Wansing's E`super-tonk` rule A is not discharged.

Notice that because of the asymmetry of the premises in the intro-rule, the two GE-induced elim-rules are also asymmetric. If we replace the original Esuper-tonk rule with the GE-rules we end up with a set of rules which is not non-trivially trivialising. So even if super-tonk is worse than tonk—with respect to some measures—its malfunction can still be identified as disharmony.

4.4.4   Normalisation, GE, and mitigability

What about normalisation for the improved GE-template? Is there a systematic sense in which maximum formulae can be removed in conversion steps? For the list of conversions in Def. 2.6 we had examples where the conversions are schematic, i.e., in the cases where there is more than one intro-rule (elim-rule) the reduction might involve one or the other intro-rule (elim-rule) paired with an elim-rule (intro-rule). More generally, then, what is our guarantee that such a procedure will be applicable in general with GE-harmony?

Although we have already seen that GE-harmony does not entail normalisation (e.g., because of bullet), there is still a method for producing conversion steps for such proofs. It is just that sometimes they fail to remove the maximum formula, or that they produce a formula of higher degree in the converted derivation. For the ith intro-rule for a connective δ, any of the corresponding δ GE-rules will allow a derivation path that provides a conversion. Compare the standard case $I\vee$ and $E\vee$ where, depending on which intro-rule is being applied, there is always a conversion available using one or the other of the subderivation from the one elim-rule:

$$
\begin{array}{ccc}
\begin{array}{ccc}
\Pi_0 & [A]^u & [B]^v \\
A & \Pi_1 & \Pi_2 \\
\hline
A \vee B & C & C \\
\end{array} & &
\begin{array}{ccc}
\Pi_0 & [A]^u & [B]^v \\
B & \Pi_1 & \Pi_2 \\
\hline
A \vee B & C & C \\
\end{array}
\end{array}
$$
$$
\qquad\qquad C \qquad\qquad\qquad\qquad C
$$

In the presence of multiple GE-rules, however, one might worry that there are derivations where a δ intro-rule comes immediately before a δ elim-rule without

there being the right subderivation present in the GE-rule to provide a path appropriate for a conversion step. Fortunately, the worry is unfounded. By developing the details of Read's GE-template we see that whichever pair of intro- and elim-rules one picks, there is a reduction readily available. Take Example 4.12: The principle is just a generalisation of the ∨-conversion. An application of any $I\sqcup$ followed by an application of any $E\sqcup$ will always yield a conversion since there is exactly one intro-rule premise that corresponds to the assumption of one or the other elim-rule subderivation. E.g., with the second intro-rule and the first elim-rule:

$$
\cfrac{\cfrac{\Pi_0 \quad \Pi_1}{\cfrac{C \quad D}{\sqcup ABCD}} \quad \cfrac{[A]^u \quad [C]^v}{\cfrac{\Pi_2 \quad \Pi_3}{E \quad E}}}{E} \qquad \rightsquigarrow \qquad \cfrac{\Pi_0}{\cfrac{C}{\cfrac{\Pi_3}{E}}}
$$

Put more abstractly, we get fusions of the following sort (see Read 2008a):

$$
\cfrac{\cfrac{\vdots \quad \cdots \quad \vdots}{\pi_{i_1} \quad \cdots \quad \pi_{i_{n_i}}}}{\delta} \quad \cfrac{\cfrac{[\pi_{1_{j_1}}]}{\vdots} \quad \cdots \quad \cfrac{[\pi_{m_{j_m}}]}{\vdots}}{\dot\gamma \quad \cdots \quad \dot\gamma}}{\gamma}
$$

To elaborate more on the structure we move beyond Read's formalisation by Dyckhoff-ifying the template, i.e., directly avoid the higher-order element by setting the assumptions as separate minor premises. We propose the following template, which is clearly different from the original Francez-Dyckhoff template (see Section 4.3.1) in implementing the idea of multiple GE-rules. Yet it incorporates the Dyckhoff-style rules, and, as such, yields a different result from Read's GE-template.

$$
\cfrac{\cfrac{[\Sigma_{i_1}]^{u_1}}{\vdots} \quad \cfrac{[\Sigma_{i_{n_i}}]^{u_{n_i}}}{\vdots}}{\cfrac{\sigma_{i_1} \quad \cdots \quad \sigma_{i_{n_i}}}{\delta}} \, (u_1,\dots,u_{n_i}) \qquad \cfrac{\delta \quad \cfrac{[\sigma_{1_j}]^{u_1}}{\dot\gamma} \quad \cdots \quad \cfrac{[\sigma_{m_{j_m}}]^{u_{jm}}}{\dot\gamma} \quad \Sigma_{1_j} \quad \cdots \quad \Sigma_{m_{jm}}}{\gamma} \, (u_1,\dots,u_{jm})
$$

As before, δ is the introduced formula with the principle occurrence of the connective λ in question. σ_{i_j} are formulae, and $\Sigma_{i_{j_i}}$ are sets of formulae (any such Σ might of course be empty if the premise in the intro-rule is not a subderivation). The first subscript indicates the position in the ordering of intro-rules, the second the position in the ordering of premises of the intro-rule.

We can then indicate the general method for conversion grafting the two foregoing templates:

$$
\cfrac{\cfrac{[\Sigma_{i_1}]}{\sigma_{i_1}} \quad \cdots \quad \cfrac{[\Sigma_{i_{n_i}}]}{\sigma_{i_{n_i}}}}{\delta} \quad \cfrac{[\sigma_{1_j}]^u}{\Pi_{1_j}^*} \quad \cdots \quad \cfrac{[\sigma_{m_{jm}}]^v}{\Pi_{m_{jm}}^*} \quad \cfrac{\Pi_{1_j}'}{\Sigma_{1_j}} \quad \cdots \quad \cfrac{\Pi_{m_{jm}}'}{\Sigma_{m_{jm}}} \quad \rightsquigarrow \quad \cfrac{\Pi_{i_j}'}{\Sigma_{i_j}} \\ \cfrac{\Pi_{i_j}}{\gamma}
$$

This calls for some explanation. For any δ intro-rule i, there will be exactly one subderivation in any δ elim-rule whose assumption is the same as one of the premises in the intro-rule, namely σ_{i_j} (where the jth premise from each intro-rule is the assumption in a subderivation of the arbitrary formula γ). By finding the corresponding subderivations (indexed in the same way, with additional superscripts to differentiate types of subderivations in the template), we locate the normal form derivation which removes the detour through the maximum formula δ. Notice that due to the construction of GE-rules, there is always a unique conversion step for each intro- and elim-pair.

One more example (4.9). We omit the cases where the GE-elim rules have been simplified in the obvious ways.

$$
\cfrac{\cfrac{[A]^u}{\Pi_0} \quad \Pi_1 \quad \Pi_2 \quad \cfrac{[C]^v}{\Pi_3} \quad \cfrac{[C]^w}{\Pi_3}}{D} \quad \rightsquigarrow \quad \cfrac{\cfrac{\Pi_1}{A} \cfrac{}{\Pi_0} C}{\cfrac{\Pi_3}{D}}
$$

An interesting observation about the connection between GE-harmony and the global property of normalisation is that with GE-ification *every* elim-rule is a

del-rule (see Section 2.3.3). Consequently, normalisation for GE-harmonious systems require massive permutations of derivation-segments, but, importantly, adds nothing that Prawitz's framework cannot accommodate.

One caveat: Recall that Prawitz's proof of normalisation also involves so-called permutation conversions for $\vee$ and $\exists$, the del-rules of **Ni**. Here is the permutation for $\vee$:

$$
\cfrac{\cfrac{\begin{array}{ccc} \Pi & \Pi_1 & \Pi_2 \\ A \vee B & C & C \end{array}}{C}}{D}\ \Pi'
\quad\rightsquigarrow\quad
\cfrac{\begin{array}{ccc} & \cfrac{\Pi_1\ \ \Pi'}{C} & \cfrac{\Pi_2\ \ \Pi'}{C} \\ \Pi & \cfrac{}{D} & \cfrac{}{D} \\ A \vee B & & \end{array}}{D}
$$

In the original derivation, the final occurrence of C in the segment is the conclusion of the $E\vee$ applications, whereas in the converted derivation this is replaced by two segments where the final occurrence of C still *depends* on the open assumptions of A or B in the minor premises of the $E\vee$ application. Prawitz remarks that this is of no consequence for **Ni** since there are no restrictions on dependencies for major premises of elim-rules (there are only restrictions on $I\forall$ and the minor premise of $E\exists$).[28] Unfortunately, without discussing particular inference rules, and a particular system, one has to account for the possibility that some elim-rules do have such restrictions on the major premise. That might involve a further complication for a proof of normalisation.

One task remains. We need to investigate in some more detail why the entailment from GE-harmony to normalisation fails. This is important not only because normalisation in and of itself is a desirable property (and sometimes associated with harmony), but because the theorem tends to deliver consistency as a corollary. Prawitz (1965, pp. 41-4) used normalisation to prove consistency for classical logic (in the $\vee$-free fragment), and the result extends with some tweaking to full intuitionistic logic, **Nip**, and full classical logic, **Ncp**.[29] In general, these results

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[28]See Prawitz (1965, p. 51)

[29]Although, of course, these results followed *indirectly* from Prawitz's original corollary. See Troelstra & Schwichtenberg (2000, pp.184-89) for details.

rely on a structural observation about normal derivations: Glossing over some details, the point is that normal form divide derivations into an E-part (of elim-rules) and an I-part (of intro-rules) such that every branch has a *minimum formula* dividing the two parts. Let us try to make this precise by introducing a further result due to Prawitz. First some definitions:

Definition 4.3 (Thread)**.** A sequence A_1, ..., A_n of formula occurrences in a derivation Π is a *thread* in Π if:

   (i) A_1 is a top-formula in Π;

   (ii) A_i is immediately above A_{i+1} for each $i \leq n$ in Π;

   (iii) A_n is the end-formula of Π.

Definition 4.4 (Path)**.** A sequence A_1, ..., A_n of formula occurrences in a derivation Π is a *path* in Π if:

   (i) A_1 is a top-formula in Π that is not discharged by a del-rule;

   (ii) For each $i \leq n$, A_i is not the minor premise of an elim-rule, and either (1) A_i is not the major premise of a del-rule, and A_{i+1} is the formula occurrence immediately below A_i, or (2) A_i is the major premise of a del-rule, A_{i+1} is an assumption discharged by that very del-rule;

   (iii) A_n is either a minor premise of an elim-rule, or the end-formula of Π, or a major premise of a del-rule such that that rule-application does not discharge any assumptions.

If a path in Π is also a thread, we say that it is a *main path* in Π.

Theorem 4.1 (Prawitz (1965))**.** *Let Π be a normal form derivation, let β be a path in Π, and let $\sigma_1,...,\sigma_n$ be the sequence of segments in β (recall that a segment might just be a formula). There is a* minimum *segment σ_i which separates two parts of β, the* I-*part and the* E-*part, such that:*

(i) *For each $j < i$ (i.e. in the E-part), σ_j is a major premise of an elim-rule and the formula in σ_{j+1} is a subformula of the formula in σ_j;*

(ii) *If $i \neq n$, σ_i is a premise of an intro-rule;*

(iii) *For each $i < j < n$, σ_j is a premise of an intro-rule, and the formula in σ_j is a subformula of the formula in σ_{j+1}.*

With this result we can give a general theorem which basically follows Prawitz's observation about the connection between normalisation and consistency. However, what we want is not a result for any particular system, but rather a general proof for a range of systems. In particular, we will assume that any system **S** under consideration consists of intro- and elim-rules which follow the templates in this section. Of course, as we have seen natural deduction systems might have rules which do not fit these standards, but we set that aside. In particular, we will ignore $\perp$ and, contrary to Prawitz, give a proof of Post-consistency.

Theorem 4.2 (Consistency). *If a system $\boldsymbol{S}$ is normalisable, then $\boldsymbol{S}$ is Post-consistent.*

Proof. For reductio, assume that an atomic formula p is provable in **S**. Let Π be a normal form proof of p, and let β be a main path in Π. By condition (iii) in Thm. 4.1 there cannot be any segment σ_j such that $i < j < n$. For, if there were, and each intro-rule introduces a formula of higher formula complexity than 0, then, contrary to our assumption, p could not be the end-formula of β. Thus, p must be the formula of the minimum segment. But, by condition (i), for each σ_j, where $j < i$, the formula of σ_j is the major premise of an elim-rule. Hence, β starts with an undischarged assumption, and so Π has an undischarged assumption. $\square$

Nonetheless, such a result is of little help to PTS if GE-harmony is the right account of harmony. Since GE-harmony does not entail normalisation, a system can be inconsistent event though all the rules are harmonious (which is indeed

the case with a system with only the $\bullet$-rules and EFQ). The challenge is to understand the gap between harmony and normalisation. More precisely, under which conditions can we have GE-harmony without conversions that will facilitate normalisation?

Let us return to the key Example 4.6—bullet. Read observed that bullet is GE-harmonious but fails on several other accounts: conservativeness, normalisation, consistency. The reason, in Read's analysis, is that bullet already at the intro-rule level is dysfunctional. Thus, ultimately, even a well-matched elim-rule cannot save the day. This is not to say, of course, that $I\bullet$ is inconsistent on its own. It is merely that the intro-rule is strong enough to somehow implicitly carry with it inconsistency, a latent property that is triggered by the presence of a harmonious elim-counterpart.[30] Not surprising, perhaps, since $I\bullet$ has a paradoxical ring to it: If one can derive absurdity from an assumption of bullet, one can conclude bullet. Granted, some assumption about the nature of $\bot$ is required for the two rules to be jointly inconsistent (see Section 4.3.3), but that is hardly helpful.[31]

More than just being non-sheer, the bullet-conversion produces an occurrence of the very same connective. It is worth looking at the details of what goes wrong when we attempt to apply the conversion to a derivation of C from $\bullet$. First, here is the conversion step again:

$$
\cfrac{\cfrac{\cfrac{[\bullet]^1}{\Pi_1}}{\cfrac{\bot}{\bullet}}\;{\scriptstyle(1)} \quad \cfrac{\Pi_2}{\bullet} \quad \cfrac{\cfrac{[\bot]^2}{\Pi_3}}{C}}{C}\;{\scriptstyle(2)} \quad\rightsquigarrow\quad \cfrac{\cfrac{\cfrac{\cfrac{\cfrac{\Pi_2}{\bullet}}{\Pi_1}}{\bot}}{\Pi_3}}{C}
$$

---

[30]Read's contention is that intro-rules give both necessary and sufficient conditions for asserting a λ-statement, so, crucially, adding another intro-rule to $\bullet$ would be game-changing. In particular, GE-harmony will then reformat the elimination output.

[31]In discussion, Graham Priest has taken this as a small victory for the paraconsistent approach. After all, without an explosive $\bot$ (EFQ), bullet will not trivialise the system. However, the victory only stands if GE-harmony is the *only* constraint the inferentialist ought to apply to inference rules. I take it that an important moral of Read's bullet is that we need a further constraint.

Spelling out the derivation of triviality using the two rule (and *EFQ*) we get the following derivation:

Example 4.16. Inconsistency from bullet:

$$
\cfrac{
\cfrac{[\bullet]^1 \quad [\bullet]^1 \quad \cfrac{\cfrac{[\bot]^4}{\bot}\,(EFQ)}{}}{\cfrac{\bot}{\bullet}\,(I\bullet)(1)}\,(E\bullet)(4)
\qquad
\cfrac{[\bullet]^2 \quad [\bullet]^2 \quad \cfrac{\cfrac{[\bot]^5}{\bot}\,(EFQ)}{}}{\cfrac{\bot}{\bullet}\,(I\bullet)(2)}\,(E\bullet)(5)
\qquad
\cfrac{[\bot]^3}{C}\,(EFQ)
}{C}\,(E\bullet)(3)
$$

A reduction of this derivation 'levels the local peak'—the rightmost $\bullet$ in the last $E\bullet$ application—but it re-produces two copies of bullet, and the conversion gives them the same derivation as the original maximum formula. We have only moved the bump in the carpet. In particular, let Π_2 (from the above conversion) be the leftmost derivation of $\bot$; Π_3 be the *EFQ* step giving C from $\bot$; and, finally, Π_1 be the rightmost derivation of $\bot$ from $\bullet$. The resulting derivation requires two copies of Π_1 (one for each $\bullet$), but as the subsequent step is an $E\bullet$ application, there are yet again two maximum formulae in the derivation.

One might then consider bullet to be an example where the mere *stipulation* of a new connective (i.e., through the intro-rule as implicit definition) has semantically misfired.[32] The suggested assertoric practice for bullet, together with the inferential practice suggested by a harmony constraint, leads to inconsistency. Any attempt at holding on to harmony (and, in particular GE-harmony) as a guiding principle for revising our inferential practice, has the onus of locating the problem with bullet elsewhere. Why is it that bullet, but not the usual run of the mill connective, cannot be disciplined by a harmony constraint alone?

An immediate response is the observation that bullet's intro-rule deviates from the norm by allowing a copy of itself in a premise. In the terminology of Dummett (1991, p. 257), we say that an λ intro-rule is *sheer* whenever λ does not occur in any premise or assumption. Thus, bullet is non-sheer.[33] Recall that it is due

---

[32] A further discussion of what is required for successful stipulation is outside of the scope of the present work. Although see Hale & Wright (2000) for an interesting discussion.

[33] Nor is bullet *pure* since $\bot$ occurs in the rules as well.

to the non-sheerness that removal of the maximum formula fails in the attempted conversion step for bullet. One might then be tempted to add a constraint of sheerness to GE-harmony in order to rule out bullet and its relatives. Such a strategy might also be a route to consistency by putting a more exigent constraint on inference rules: GE-harmony *plus* sheerness.

Unfortunately, however, there is a battery of apparently innocuous rules that are non-sheer. One of them is the Dilemma rule we looked at in Example 4.7. Applying the first of the GE-rules we get a conversion of the form:

$$
\cfrac{\cfrac{\begin{array}{cc} [A]^u & [A]^v \\ \Pi_0 & \Pi_1 \\ B & \neg B \end{array}}{\neg A} \quad \begin{array}{c} [B]^w \\ \Pi_2 \quad \Pi_3 \\ A \quad C \end{array}}{C} \quad \leadsto \quad \begin{array}{c} \Pi_2 \\ A \\ \Pi_0 \\ B \\ \Pi_3 \\ C \end{array}
$$

With the second:[34]

$$
\cfrac{\cfrac{\begin{array}{cc} [A]^u & [A]^v \\ \Pi_0 & \Pi_1 \\ B & \neg B \end{array}}{\neg A} \quad \begin{array}{c} [\neg B]^w \\ \Pi_2 \quad \Pi_3 \\ A \quad C \end{array}}{C} \quad \leadsto \quad \begin{array}{c} \Pi_2 \\ A \\ \Pi_1 \\ \neg B \\ \Pi_3 \\ C \end{array}
$$

In both of these conversions we delete the maximum formula $\neg A$, which, importantly does not occur in any of the converted derivations. The intro-rule is non-sheer as negation occurs in $\neg B$ in the second premise. But, importantly, it is not the non-sheerness that upsets the two suggested derivations. For, as it happens, the first derivation is as bad the second even though it does not produce a negation in the output derivation. Rather, it is the fact that in both cases, B might be of higher degree than A that causes the problem. Depending on the structure of Π_0 (Π_1) and Π_3, B ($\neg B$) might itself be a maximum formula, and, worse, since Π_3 in the original derivation might have discharged multiple occurrences of B, the resulting derivation might have more rule-applications than the first.

---

[34]Recall that we have already observed that the first GE-rule is sufficient on its own. Indirectly, then, normalisation could always rewrite to use this rule rather than the second.

Such an observation does not amount to a refutation of normalisation, but it does mean that the standard Prawitz procedure for proving the result is blocked. Some induction value other than the degree of the maximum formula (and length of segment) is needed as a metric for normalisation. Two conclusions can be drawn from this: First, it tells us that a number of familiar intro-rules (e.g., Dilemma rule) will resist normalisation if when paired with their GE-harmonious set of elim-rules. Second, sheerness plus GE-harmony is not sufficient for normalisation, and, thus, not sufficient for consistency.

Instead, what marks the difference between, on the one hand, the 'problem' intro-rules, and, on the other hand, standard intro-rules as seen in Def. 2.6, is that the latter rules only include *strict subformulae* of the conclusion in their premises, i.e., any formula is a subformula of the conclusion (see Def. 2.8) but not identical to the conclusion. In particular, this also holds for the negation rules for intuitionistic logic, $I\neg$ and $E\neg$. In contrast, both $I\bullet$ and *Dil* fail on this account. And, more interestingly, so does classical reductio, where $\neg A$ occurs as an assumption for a rule with A as conclusion.[35] In other words, the problems encountered by using $E\perp_C$ as an intro-rule is a special case of the more general phenomenon: The problem is not that a negation, specifically, occurs in the premises, but that a formula that is not a strict subformula of the conclusion occurs in the premises.

Let us call intro-rules where all formulae in the premises are strict subformula of the conclusion *mitigable*. The contention is that if all intro-rules for a connective λ are mitigable, and all elim-rules for λ are GE-induced, then there is a conversion

---

[35]Compare also the conversions for `super-tonk`:

$$
\cfrac{\cfrac{\begin{array}{cc}[C]^1\\ \Pi_0 \quad \Pi_1\\ D \quad\quad A\end{array}}{\texttt{super-tonk}}\,{\scriptstyle(1)} \quad \cfrac{\begin{array}{cc}\quad [D]^2\\ \Pi_2 \quad \Pi_3\\ C \quad\quad E\end{array}}{}}{E}\,{\scriptstyle(2)} \quad\rightsquigarrow\quad \begin{array}{c}\Pi_2\\ C\\ \Pi_0\\ D\\ \Pi_3\\ E\end{array}
$$

$$
\cfrac{\cfrac{\begin{array}{cc}[C]^1\\ \Pi_0 \quad \Pi_1\\ D \quad\quad A\end{array}}{\texttt{super-tonk}}\,{\scriptstyle(1)} \quad \begin{array}{c}[A]^2\\ \Pi_2\\ E\end{array}}{E}\,{\scriptstyle(2)} \quad\rightsquigarrow\quad \begin{array}{c}\Pi_1\\ A\\ \Pi_2\\ E\end{array}
$$

step of the right form. Why is this a plausible conjecture? At least we know by the structure of the GE-template, that every GE-rule will have the conclusion of the intro-rule as major premise, and any minor premise will only contain strict subformulae of the conclusion (except the arbitrary conclusion of subderivations). Thus, any new maximum formula produced in the conversion will be of lower degree than the deleted maximum formula. Together with permutation conversions this suggests that there will be a reduction in the induction value, i.e., either in the highest degree of a maximum formula or in the sum of lengths of segments.

Albeit only a gesture, the above takes on some distance towards appreciating a connection between GE-harmony and normalisation. But even if such a connection can be made more rigid, the problem still remains that all we can hope for is a proof that GE-harmony and mitigability is sufficient for normalisation (and, in turn, for consistency). A moral for Prawitz's result for classical logic is precisely that treatment of special cases (e.g., classical reductio $E\perp_C$) might give normalisation even for systems where these properties do not hold. In conclusion, we can only opine that GE-harmony ought to be accompanied by a constraint informed by the precise conditions under which GE-harmony yields normalisation results. The merit of such a proposal is that it promises to close the gap between GE-harmony and consistency. Whether such a condition can be formulated accurately, however, is an open problem.

4.5 Multiple-Conclusion and GE

4.5.1 Classicality and Multiple-Conclusion

Let us pause and take stock: We have seen so far that GE-harmony can deal with a range of cases in a convincing fashion. Of course, nothing in what is said so far provides any form of guarantee that there are not rule-sets for which the GE-harmony analysis breaks down. Even if the account gives functionality and

normalisation in standard cases, one might worry that the applicability is limited. The problem is that given the *open-endedness* of the notion of an inference rule, there is radical flexibility in what sort of formalism might come under consideration.[36] The GE inferentialist cannot be held accountable to any arbitrary development or generalisation of proof-theoretic formalism.

However, from the fact that an absolute generalisation of GE-harmony is out of the picture, it does not follow that the range of cases cannot be criticised. In what follows, we will focus on one particular dimension of proof-theory which has been under-appreciated in the harmony debate: This is the *substructural dimension*. We will investigate the lacuna left by traditional PTS (including GE-harmony), and discuss the problems which arises from bringing proof-theoretic harmony, and GE-harmony in particular, in to the substructural era.

Rather than starting immediately from a substructural perspective, we will first return to GE-harmony, and one particular application of some importance: Classical negation. The idea is that the application to classical logic will display an important lesson about the limits of the GE-approach. More precisely, it will underline the difficulty involved in identifying logical connectives isolated from their structural context.[37] So let us return to GE-harmony and see how it affects the revisionary debate about classical negation. In particular, is classical negation GE-harmonious? In fact, Read's explicit aim with GE-harmony is, among other things, to provide a harmonious natural deduction for classical logic. We quote at length:

> What I intend to do is to explain how the sense of the connectives of
> classical logic can be captured by inference rules in such a way that
> harmony and autonomy are guaranteed; and in particular, to exhibit a
> reformulation of **NK** in which all the negation-free theses of classical

---

[36]An example is natural deduction systems for many-valued logics. See Baaz et al. (1993).

[37]Recall Belnap's insistence on the importance of the 'antecedently given context of deducibility' in Section 2.2.3.

logic are provable without the use of the rules for negation. The rules
will be in harmony, in that all proofs in ¬, &, ∨, →, ∀ and ∃, com-
plete for the theses of classical logic, will be normalizable. Every proof
can be put in normal form by eliminating maximal formulae, formulae
introduced by introduction-rules and acting as major premise of the
corresponding elimination-rules. The result is a natural deduction sys-
tem for full classical logic satisfying the criteria we have noted above
for a logic to be autonomous. (Read 2000, p. 144)[38]

The passage is a bit confused. Read states that "[t]he rules will be in harmony,
in that all proofs [...] will be normalizable". It is certainly true that **NC** is
normalisable (which we reported in Section 3.2.2), but Read's preferred notion
of harmony is GE-harmony, and, as we have seen in Section 4.3.3, GE-harmony
does not entail normalisability. The real issue for our purposes, then, is whether
the **NC** rules are GE-harmonious. It is a puzzling omission in Read's original
discussion that his preferred GE-template was not turned on classical negation,
the critical case.

Why think that GE-harmony must be revisited for classical negation in particu-
lar? Recall that Read's **NC** negation rules are multiple-conclusion (see Example
3.2). As it stands, neither Read's GE-template, nor the Dyckhoff-style template
developed here, incorporates this aspect. Although Read does not provide a mod-
ified version of his preferred schema, we might hope to modify it according to
the multiple-conclusion framework. This, however, is a non-trivial extension. It
introduces some unexpected problems which need to be addressed. But before
we can shed light on the difficulties that arise, let us modify the Dyckhoff-style
GE-template proposed above. The critical change is situating the formulae in
disjunctive contexts.

Consider the following templates:

---

[38]See Section 3.2.2 for more on Read's take on classical logic and harmony.

$$
\begin{array}{c}
\begin{array}{ccc}
[\Gamma_{i_1}] & & [\Gamma_{i_{n_i}}] \\
\vdots & & \vdots \\
\Sigma_{i_1}, \sigma_{i_1} & \dots & \Sigma_{i_{n_i}}, \sigma_{i_{n_i}} \\
\hline
\multicolumn{3}{c}{\delta, \Sigma_{i_1}, ..., \Sigma_{i_{n_i}}}
\end{array}
\qquad
\begin{array}{ccccccc}
 & [\sigma_{1_j}] & & [\sigma_{m_{j_m}}] & & & \\
 & \vdots & & \vdots & & & \\
\delta, \Delta & \Theta & \dots & \Theta & \Gamma_{1_j}, \Delta_{1_j} & \dots & \Gamma_{m_{j_m}}, \Delta_{m_{j_m}} \\
\hline
\multicolumn{7}{c}{\Theta, \Delta, \Delta_{1_j}, ..., \Delta_{m_{j_m}}}
\end{array}
\end{array}
$$

where, as before, $1 \geq i \geq m$, of m δ intro-rules; for each intro-rule i, there is a subderivation of Θ from σ_{i_j} for some j; and, for each intro-rule i, there is a minor premise Γ_{i_j} for the j corresponding to the σ_{i_j} (with a multiple-conclusion context Δ_{i_j}).

Compare the rules of the system **NC** in Appendix A.9. The intro-rules follow the structure indicated by our intro-template, whereas the elim-rules are not generalised. In fact, even $E\vee$ is not a GE-rule in **NC**. However, we will see that, as with the single-conclusion frameworks, the standard rules are equivalent with their corresponding GE -rules. Let us take $\vee$ as an example. From the following two intro-rules we get a single GE-rule (rightmost):

Example 4.17. Disjunction:

$$
\cfrac{\Delta, A}{\Delta, A \vee B} \qquad \cfrac{\Delta, B}{\Delta, A \vee B} \qquad
\cfrac{\Delta, A \vee B \quad \begin{matrix}[A] \\ \vdots \\ \Theta\end{matrix} \quad \begin{matrix}[B] \\ \vdots \\ \Theta\end{matrix}}{\Delta, \Theta}
$$

We can show that the GE-rule is equivalent to the standard **NC** elim-rule. First, from the GE-rule, we recapture the simplified rule by the usual instantiation:

$$
\cfrac{\Delta, A \vee B \quad \begin{matrix}[A] \\ \vdots \\ A, B\end{matrix} \quad \begin{matrix}[B] \\ \vdots \\ A, B\end{matrix}}{\Delta, A, B}
$$

As opposed to earlier proofs of equivalence, the subderivations now rely on an explicit application of weakening in **NC**. Conversely, we permute the subderivation as before, to get the more standard looking multiple-conclusion rule for disjunction:

$$\frac{\Delta, A \vee B}{\Delta, A, B}$$
$$\vdots$$
$$\Delta, \Theta$$

What about classical negation? The GE-rule follows the same pattern:

Example 4.18. GE for classical negation:

$$\frac{\Delta, \neg A \quad \Delta', A \quad \begin{array}{c} [\bot]^u \\ \vdots \\ \Theta \end{array}}{\Delta, \Delta', \Theta} \; {\scriptstyle (u)}$$

By similar considerations as above, this rule is equivalent to the simplified **NC** rule. Thus, apparently, the GE-template provides the result that we would want for classical negation. In terms of the revisionist agenda, this means that at least harmony, understood as GE-harmony, provides no motivation for giving up on classical logic. So much the worse for the intuitionist revisionist. For PTS, however, this is unmitigated victory as the semantics can widen its field of application.

So where does the complication enter the picture? The answer is that we are now being irresponsible with contexts, as foreshadowed in Section 3.2.2. The multiple-conclusion GE-template glosses over some crucial distinctions: The added contexts Σ_{i_j}, Δ_{i_j} and Θ are, of course, disjunctive, while the Γ_{i_j} are conjunctive contexts. Hence, writing $\ulcorner \Gamma_{i_j}, \Delta_{i_j} \urcorner$ is misleading as it obfuscates the fact that we are dealing with different commas. The structural comma in $\ulcorner \Gamma_{i_j}, \Delta_{i_j} \urcorner$ says that Γ_{i_j} *or* Δ_{i_j}, but whereas latter has its members arranged disjunctively, the former is arranged conjunctively. This reveals that the template now involves a much more delicate structure. As an example, let $\Gamma_{i_j} = \{A, B\}$ and $\Delta_{i_j} = \{C, D\}$. Obviously, writing $\{A, B, C, D\}$ will be extremely poor bookkeeping. In informal paraphrase, what this ought to say is: either (*A and B*) *or* (*C* or *D*).

Only because of the happenstance that Boričić and Read presented **NC** without conjunctive contexts did we fail to appreciate this distinction. Explicating the

conjunctive contexts as well, we see that the problem occurs independently of the GE-template. In **Ncp**, the difference is as follows:

$$
\frac{\genfrac{}{}{0pt}{}{[A]^u}{\genfrac{}{}{0pt}{}{\vdots}{B}}}{A \to B} \ (u) \qquad \frac{\genfrac{}{}{0pt}{}{\Gamma, [A]^u}{\genfrac{}{}{0pt}{}{\vdots}{B}}}{A \to B} \ (u)
$$

Thus, in **NC**, the corresponding difference gives the following rule-variations:

$$
\frac{\genfrac{}{}{0pt}{}{[A]^u}{\genfrac{}{}{0pt}{}{\vdots}{\Delta, B}}}{\Delta, A \to B} \ (u) \qquad \frac{\genfrac{}{}{0pt}{}{\Gamma, [A]^u}{\genfrac{}{}{0pt}{}{\vdots}{\Delta, B}}}{\Delta, A \to B} \ (u)
$$

Just like the two first rules might yield different calculi (compare quantum disjunction from Example 4.1), the two multiple-conclusion rules might also give different results. The difference is that in the notation of the lower right-most rule, the structural comma is *ambiguous*. Looking back the related sequent calculus **G1cp**, it is really no surprise that **NC** lands us with this problem. For, the classical sequents disambiguate commas by their *location* in the sequent, i.e., right-handed vs left-handed.[39] Transferring intuitionistic logic to natural deduction is another matter, however, as its standard sequent system is single- (or empty-) succedent. Thus, modulo some considerations about the empty succedent and $\perp$, there is no second comma to disambiguate in the natural deduction framework.

Not so for classical logic. A quick fix is to offer different syntax for the two structural operators. For instance, using ',' and ';' to differentiate between conjunctive and disjunctive contexts. We do not intend to indulge in any details here—it suffices to say that syntactically enhancing the structural language will in this case involve complications for the notion of derivation, and, in turn, for the corresponding notion of normal form and normalisation. The point we are after, rather, is of a more general nature: Upon realising that harmony in general, and GE-harmony

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[39]See Chapter 6 and especially Section 6.4.5 for more on the notion of a *location*.

in particular, is insensitive to the presence of structural distinctions, it is natural to ask whether this might impact the inferentialist promise of a functional harmony constraint. After all, the structural language—which will depend on the proof-theoretic framework—is part of the input in the GE-template.

4.5.2   Additive and Multiplicative Connectives

Up until now, we have not considered in any detail the possibility that presence/absence of contexts might distinguish logical constants. In Section 4.2.2, Example 4.1, we saw that disjunction in the quantum logic **Nqp** behaves like classical disjunction in all aspects except the conjunctive contexts. Developing natural deduction in a multiple-conclusion framework naturally leads to the related question of whether disjunctive contexts might yield a proliferation of connectives, and, furthermore, if such a proliferation will have an influence on GE-harmony. By an analogy to sequent calculus, we will see that this is in fact the case, and, furthermore, that it emphasises a problem with GE-harmony as it stands.

To demonstrate how the contexts become important, let us return to the **NC**-rules again. Recall, for example, the rules for $\to$ that allowed us to formalise a multiple-conclusion version of the $\to$-fragment of classical logic (e.g., Peirce's Law is provable):

Example 4.19. Implication in **NC**:

$$
\frac{\begin{array}{c} [A]^u \\ \vdots \\ \Gamma, B \end{array}}{\Gamma, A \to B}\ (I{\to}^{NC})(u) \qquad \frac{\Delta, A \to B \quad \Gamma, A}{\Gamma, \Delta, B}\ (E{\to}^{NC})
$$

By introducing multiple-conclusion rules for $\to$ we must take heed of the difference between *additive* (context-sharing) and *multiplicative* (non-context-sharing) rules.[40] This difference is already present in sequent calculus, and has come into

---

[40]Sometimes also called *extensional* and *intensional* rules (especially in connection with relevant logics). Hitherto this issue has been hidden by the fact that (conjunctive) contexts in the premises have been supressed.

prominence with the development of Linear logic.[41] Corresponding to the same $R\rightarrow$ rule in **G1cp** (and similarly for **G1ip**) there are two $L\rightarrow$-rules, the multiplicative and additive, respectively:

Example 4.20. Implication in **G1cp**:

$$\frac{\Gamma \Rightarrow A, \Delta \quad \Gamma', B \Rightarrow \Delta'}{\Gamma, \Gamma', A \rightarrow B \Rightarrow \Delta, \Delta'} \qquad \frac{\Gamma \Rightarrow A, \Delta \quad \Gamma, B \Rightarrow \Delta}{\Gamma, A \rightarrow B \Rightarrow \Delta}$$

The two rules are equivalent in the presence of weakening and contraction (thus, in particular, in **G1cp**), but characterise distinct logical constants in linear logic where neither of these rules hold. Interestingly, although Read follows Boričić (1985) in giving only the multiplicative rules for $\rightarrow$, we will see that, as in sequent calculus, **NC** has a corresponding additive rule which is equivalent (since, again, weakening and contraction hold in **NC**). In a sense, this simply reinforces the assumption that for classical logic there is a correspondence between sequent calculus and multiple-conclusion natural deduction. The following additive $E\rightarrow^{NC}$ rule is equivalent to its multiplicative counterpart:

$$\frac{A \rightarrow B, \Delta \quad A, \Delta}{B, \Delta} \ {\scriptstyle (E\rightarrow_a^{NC})}$$

Obviously, the additive rule is an instance of the multiplicative rule where $\Gamma = \Delta$, and where contraction is applied after the conclusion. For the other direction, we assume the premises of the multiplicative rule and derive its conclusion using weakening and the additive rule:

$$\frac{\dfrac{A \rightarrow B, \Gamma}{A \rightarrow B, \Gamma, \Delta} \ {\scriptstyle (K)} \quad \dfrac{A, \Delta}{A, \Gamma, \Delta} \ {\scriptstyle (K)}}{B, \Gamma, \Delta}$$

Given that there are two standard elim-rules—the additive and the multiplicative— we ought to expect two generalised rules as well.

---

[41]See Girard (1987).

$$\cfrac{A \to B, \Delta \quad A, \Delta' \quad \genfrac{}{}{0pt}{}{[B]^u}{\genfrac{}{}{0pt}{}{\vdots}{\Delta''}}}{\Delta, \Delta', \Delta''} \ (GE{\to}_m^{NC})(u) \qquad \cfrac{A \to B, \Delta \quad A, \Delta \quad \genfrac{}{}{0pt}{}{[B]^u}{\genfrac{}{}{0pt}{}{\vdots}{\Delta'}}}{\Delta, \Delta'} \ (GE{\to}_a^{NC})(u)$$

We use subscripts a and m to indicate additivity and multiplicativity, respectively. By a similar argument as above, we can show that $GE{\to}_m^{NC}$ and $GE{\to}_a^{NC}$ are equivalent in the presence of weakening and contraction. For the direction multiplicative to additive we simply note that it is an instance where $\Delta = \Delta' = \Delta''$ followed by contraction. For the other direction we need the following derivation:

$$\cfrac{\cfrac{A \to B, \Delta}{A \to B, \Delta, \Delta'} \ (K) \quad \cfrac{A, \Delta'}{A, \Delta, \Delta'} \ (K) \quad \genfrac{}{}{0pt}{}{[B]^1}{\genfrac{}{}{0pt}{}{\vdots}{\Delta''}}}{\Delta, \Delta', \Delta''} \ (GE{\to}_a^{NC})(1)$$

The double lines indicate that there might be multiple applications of weakening. We can then see that the two rules are equivalent, and we can turn to the special case of the standard rule, $E{\to}^{NC}$. It is sufficient to show that $E{\to}_a^{NC}$ is equivalent to $GE{\to}_a^{NC}$. From GE to E, we have:

$$\cfrac{A \to B, \Delta \quad A, \Delta \quad \genfrac{}{}{0pt}{}{[B]^1}{\genfrac{}{}{0pt}{}{\vdots}{B}}}{\cfrac{\Delta}{B, \Delta}} \ (1)$$

In the other direction, we permute the subderivation down as before:

$$\genfrac{}{}{0pt}{}{\cfrac{A \to B, \Delta \quad A, \Delta}{B, \Delta}}{\genfrac{}{}{0pt}{}{\vdots}{\Delta, \Delta'}}$$

Crucially, we here need to make sure that the context Δ is carried on in the subderivation from B to Δ' (from the GE-rule). We can think of it in the following disjunctive way: If B holds, then we get Δ' by assumption, and the conclusion by

weakening; alternatively, if Δ holds, we get Δ trivially, and the conclusion, again, by weakening. Either way, we have a proof of the conclusion.

Analogously, negation is subject to a similar bifurcation.[42] Recall the multiple-conclusion rules offered by Boričić and Read:

$$\dfrac{\begin{array}{c}[A]^u\\[-2pt]\vdots\\[-2pt]\dot{\Delta}\end{array}}{\Delta, \neg A}\ (I\neg^{NC})(u) \qquad \dfrac{\Gamma, A \quad \Delta, \neg A}{\Gamma, \Delta}\ (E\neg_m^{NC})$$

Again, the elim-rules can be exchanged for an equivalent additive rule given weakening and contraction. The proofs of *LEM* and *DNE* remain unaltered by the switch.

$$\dfrac{\Delta, A \quad \Delta, \neg A}{\Delta}\ (E\neg_a^{NC})$$

Generalising, we aquire two different GE-rules:

$$\dfrac{\neg A, \Delta \quad A, \Delta' \quad \begin{array}{c}[\bot]^u\\[-2pt]\vdots\\[-2pt]\Delta''\end{array}}{\Delta, \Delta', \Delta''}\ (GE\neg_m^{NC}) \qquad \dfrac{\neg A, \Delta \quad A, \Delta \quad \begin{array}{c}[\bot]^u\\[-2pt]\vdots\\[-2pt]\Delta'\end{array}}{\Delta, \Delta'}\ (GE\neg_a^{NC})$$

The proof of their equivalence, and their equivalence with the standard rules $E\neg_{m/a}^{NC}$, is similar to the implicational case.

Now contrast the $\to$ and $\neg$ cases with $\wedge$ in **NC**:

Example 4.21. Conjunction in **NC**:

---

[42]The case of $E\bot^{NC}$ is a bit puzzling:

$$\dfrac{\bot, \Delta}{A, \Delta}\ (E\bot^{NC})$$

This is simply a multiple-conclusion version of *EFQ*. Presumably, like its single-conclusion counterpart, the rule has the null-rule as its corresponding intro-rule. But which rule is then GE-harmoniously induced by the null-rule?

$$\frac{\Gamma, A \quad \Delta, B}{\Gamma, \Delta, A \wedge B} \ (I\wedge_m^{NC})$$

The $I\wedge_m^{NC}$ rule has an additive brother, while the elim-side, on the other hand, is neutral in the standard form, i.e., neither rule has two premises so there is no question of sharing vs non-sharing.

$$\frac{A \wedge B, \Delta}{A, \Delta} \ (E\wedge_1^{NC}) \qquad \frac{A \wedge B, \Delta}{B, \Delta} \ (E\wedge_2^{NC})$$

In other words, $\wedge$ opposite of the cases we have seen so far where the intro-rule is neutral whereas the elim-rules are bifurcated into additive and multiplicative (which again are equivalent given weakening and contraction).[43] Here are the two multiple-conclusion GE-rules for conjunction:

$$\frac{A \wedge B, \Delta \quad \overset{[A]^u}{\overset{\vdots}{\Delta'}}}{\Delta, \Delta'} \ (E\wedge_{1,a}^{NC}) \qquad \frac{A \wedge B, \Delta \quad \overset{[B]^u}{\overset{\vdots}{\Delta'}}}{\Delta, \Delta'} \ (E\wedge_{2,a}^{NC})$$

For the disjunction in Example 4.17, the situation is dual. The two intro-rules have only one version, while the elim-rule—in its GE version—has an additive and a multiplicative version.

4.5.3   The Substructural Challenge

How do we expect GE-harmony to operate on the additive/multiplicative distinction? If, as predicted by the logical behaviour (and claimed by substructural enthusiasts), additive and multiplicative rules do in fact underwrite two distinct logical constants, then it is critical that harmony does not run them together. A preliminary suggestion is that GE-harmony ought to induce additive elim-rules from additive intro-rules, and correspondingly for multiplicative rules.

---

[43]Compare the sequent calculus **G1cp** again.

Why think that this is the case? After all, in a situation where both intro- and elim-rules have additive and multiplicative versions, another option is to cross-match them. This might not be desirable since versions of structural rules (in absorbed form) might become derivable (see Troelstra & Schwichtenberg 2000, pp. 292-93), but that does nothing towards ruling the ensuing connectives out as semantically ill-formed. The real issue here is with the one-to-one functionality of GE-harmony. If stipulations of intro-rules can be either additive or multiplicative, and this decision impacts the semantics of the logical constant (according to PTS), then preserving the content stipulated from the set of intro-rules to the GE-induced elim-rules must involve preserving this structural aspect.

Return to the above examples: In the case of $\wedge$ one might consider introducing a constraint on the contexts that forces $I\wedge^{NC}$ to be the two $I\wedge_a^{NC}$ rules (and similarly for multiplicative input). Yet, GE-harmony does not distinguish between the output of the two additive intro-rules and the two multiplicative intro-rules. No matter which rule one picks, the set of GE-rules is the same. In other words, *uniqueness* fails if one concerns oneself with the substructural dimension.[44] Correspondingly, for $\rightarrow$ and $\neg$ we get a situation which is more dramatic: *Functionality* fails since one and the same set of intro-rule has two GE-rule sets, neither of which

---

[44]Interestingly, sometimes an additive/multiplicative divide is applied to the $L\wedge$ rule as well:

$$\frac{\Gamma, A_i \Rightarrow \Delta}{\Gamma, A_0 \wedge A_1 \Rightarrow \Delta} \qquad \frac{\Gamma, A_0, A_1 \Rightarrow \Delta}{\Gamma, A_0 \wedge A_1 \Rightarrow \Delta}$$

The key difference is that the leftmost $L\wedge$ rule introduces a measure of weakening by only preserving one of the conjuncts in the bottom-up procedure. In the rightmost rule, however, there is no getting rid of a superfluous conjunct except in the presence of an explicit weakening rule, or where weakening is built into the axioms. (See Troelstra & Schwichtenberg (2000, p. 77) for details on sequent calculus with absorbed structural rules). Surprisingly, the analogous difference is captured by the distinction between the two GE-templates we have seen. In the early version there is only one $GE\wedge$-rule whereas the improved template has two (ignoring multiple-conclusion for the time being):

$$\frac{A \wedge B \qquad \overset{\displaystyle [A,B]}{\overset{\displaystyle \vdots}{C}}}{C} \qquad \frac{A_0 \wedge A_1 \qquad \overset{\displaystyle [A_i]}{\overset{\displaystyle \vdots}{C}}}{C}$$

is uniquely induced by the template. There is, in other words, no telling from the set of intro-rules whether the GE-rule ought to be additive or multiplicative.[45]

Why is this a problem for PTS? Here is one possible answer: If, as discussed above, harmony requires functionality, then GE-harmony, in its current form, does not suffice for harmony. Two distinct rule-sets can be GE-harmonious with the same set of intro-rules. Of course, giving up on functionality is an option, but it abandons at least part of the motivation for and advantage with GE-harmony as a proof-theoretic account of the informal concept of harmony. As we discussed in Section 4.2.2, Dummett's idea of harmony does seem to presuppose the functionality of the constraint. Consider again the inversion principle: Although it does not explicitly say anything about the contexts of conclusions or premises, one can conceive of a closely related principle that adds the provisos about the contexts.

In fact, Dummett already tacitly admits that much in his diagnosis of the quantum logic rule in Example 4.1.

$$
\cfrac{A \overset{\vdots}{\underset{\vee}{\Gamma}} B \quad \overset{[A]^u}{\underset{C}{\vdots}} \quad \overset{[B]^v}{\underset{C}{\vdots}}}{C} \; (E\underset{\vee}{\vee}) \qquad\qquad \cfrac{A \overset{\vdots}{\underset{\vee}{\Gamma}} B \quad \overset{\Theta,[A]^u}{\underset{C}{\vdots}} \quad \overset{\Delta,[B]^v}{\underset{C}{\vdots}}}{C} \; (E\vee)
$$

The rule requires that the assumptions in the subderivation be without contexts, but the grounds on which the introduction of the disjunction expression rests (i.e., the standard $I\vee$ rules) do not themselves include such a restriction. This is important not only because it shows how the structural consideration creeps into the traditional notions of harmony advocated by Dummett and Prawitz, but, also, because it shows that the structural ambiguity discussed above is not a problem only in a multiple-conclusion framework—it cuts across the single- and multiple-conclusion divide since it concerns both conjunctive and disjunctive contexts.

---

[45]It is worth noting from the general structure of GE-rules, that all rules with discharge in the intro-rules will yield GE-rules for which the additive/multiplicative divide apply.

4.6   Conclusion

For the inferentialist there are a number of strategies for handling the challenge posed by substructural considerations. The traditional inferentialist might be unimpressed by the argument simply because they fail to take seriously the possibility of a framework in which the additive and multiplicative rules are not equivalent, i.e., where the structural rules of weakening and contraction do not hold. Someone unmoved by the potential in substructural logics might contemplate the option of simply excluding frameworks of this sort (they are certainly not part of the traditional inferentialists' repertoire), and accept that the output may be distinct, but equivalent, rules. As with most philosophical strategies based on logical inertia, however, this is unlikely to succeed. With the growing number of substructural logics applied in philosophical debates (see also Section 5.4), question of their semantics and justification will inevitably be part of the philosophy of logic. Instead, such an approach will severely limit the applicability of PTS: Proof-theoretically well-defined logical constants, which are distinct for structural reasons alone, will not be allowed a proof-theoretic semantics.

A second approach is to take the bull by the horns. The GE-template must be reworked to be sensitive to the structural distinctions in so far as they are part of the logic of logical constants. This is an invitation that will not be taken up in the present work. Rather, our concern in the next chapter is a third reply, a kind of strategy with some following in the literature. In a word, structural rules are without *semantic import*. An example is Read (2008a): Classical and relevant implication are distinguished logically by the structural rule weakening, but have shared content since the operational rules are the same.

Variations and extensions of such a view will be dealt with in Chapter 5. Finally, in Chapter 6, we revisit the semantic role played by structural rules and structural properties in the context of a more robust story about how inference rules determine semantic content.

Part II

The Semantic Role of

Proof-Conditions

Chapter 5

Revision and Inferentialism

5.1 Introduction

Are there *unrevisable* beliefs? Are there beliefs that we, rationally speaking, ought never to revise, or even beliefs we *cannot* revise? Quine made himself famous by answering both 'yes' and 'no', albeit not at the same time. In a word, the reason was his changing view on the role of *logical* beliefs in our doxastic make-up.[1] The Quine of *Two Dogmas* (1951), the radical revisionist, says:

> [N]o statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? (ibid., p. 43)

And furthermore:

> Truth values have to be redistributed over some of our statements. Reëvaluation of some statements entails reëvaluation of others, because

---

[1]What is a *logical* belief? For the purposes of what follows we take a logical belief to a belief about *validity-attribution*, i.e., a belief about the validity/invalidity of an argument.

of their logical interconnections—the logical laws being in turn simply
certain further statements of the system, certain further elements of
the field. (ibid.)[2]

But with Quine ([1960](#)) the position appears to be redeveloped, and his view on
logic takes on an anti-revisionist slant. Logic is allowed a privileged position
in the web of belief through its pivotal role in *radical translation*. Here Quine
considers what he takes to be a 'popular extravaganza', the view that there are
true contradictions together with failure of explosion:

> My view of the dialogue is that neither party knows what he is talking
> about. They think that they are talking about negation, '$\sim$', 'not'; but
> surely the notion ceased to be recognisable as negation when they took
> to regarding some conjunctions of the form '$p.\sim p$' as true, and stopped
> regarding such sentences as implying all others. Here, evidently, is the
> deviant logician's predicament: when he tries to deny the doctrine he
> only changes the subject. (Quine [1986](#), p. 81)

The above tension in Quine's philosophy has been pointed out by several com-
mentators, e.g., Haack ([1974](#)) and Dummett ([1978a](#)). However, this received view
is criticised in Priest ([2006a](#)). Even if Quine is frequently portrayed as giving an
argument against revisionism for logical beliefs, he himself did not intend a shift
in position. In fact, Quine proceeds to remind the reader that despite the 'change
of subject'

> I am concerned to urge the empirical nature of logic and mathematics
> no more than the unempirical character of theoretical physics; it is
> rather their kinship that I am urging, and a doctrine of gradualism. [...]

---

[2]Not everyone found Quine's attempt at wedding belief holism with the revisability of logic
convincing. See for instance Wright ([1986](#), p.191-94) for a criticism.

> Logic is in principle no less open to revision than quantum mechanics
>
> or the theory of relativity. (Quine 1986, p. 100)[3]

Although perhaps not taken as Quine originally intended, his famous 'change of logic, change of subject' quip has had a lasting influence in the philosophy of logic. Unlike its originator, however, other philosophers have not only adopted the point without the framework of radical translation and stimulus-meaning, they consider the argument a polemic against the mere possibility of revising logical beliefs.[4] The real issue, we suggest, is the paraphrase 'change of logic, change of *meaning*', even if, as one ought to expect, Quine carefully avoids this wording (see e.g., Putnam 1976). With a gross oversimplification, let us say that the line of argument is roughly as follows: If revision of a logical belief involves changing the meaning of logical expressions—say, 'not'—then we are not, in Quine's sense, reëvaluating our belief system by merely redistributing truth values over a set of propositions. Upon changing the logic, we are considering different propositions, and thus entertaining different beliefs. This, it is suggested, makes a mockery out of revision of logical beliefs.[5]

Yet, debates in which the class of logical beliefs is in question are by no means unfamiliar to philosophers. The difference between sharp and vague discourses, and between decidable and undecidable discourses are examples where philosophers have routinely offered theories that involve a change of the accompanying logic. The same is true about moving from a mathematical framework for classical mechanics to one for quantum mechanics. Even more, in the theory of semantic paradoxes many of the most persistent solutions involve a theoretic framework based on non-classical logics. Contemporary diagnostics of the Liar and its many brethren is a breeding pit for non-classical approaches to logic.

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[3]Gradualism is a view also defended later in Shapiro (1991, p. 97).

[4]In Quine (1960), logical constants are analysed in terms of stimulus-meaning: "The semantic criterion of negation is that it turns any short sentence to which one will assent into a sentence from which one will dissent, and vice versa".

[5]Haack (1974, p. 8) gave an early statement of such an argument.

Two influential approaches to semantic paradoxes are represented by Priest (2006b) and Field (2008); both endorsing non-classical logics, *paraconsistent* and *paracomplete*, respectively. Both address the 'change of logic, change of meaning' argument; and both want to dispel it in order to promote their solution. Before we go on to look in more detail at the argument from meaning-change, it is worthwhile to include some instructive observations from the aforementioned. First Field:

> The question [of meaning change] is *clear* only to the extent that we know how to divide up such firmly held principles into those that are "meaning constitutive" or "analytic" and those which aren't, and this is notoriously difficult. (Field 2008, p. 17)[6]

Field takes his cue from Quine and Putnam when he doubts that such a distinction can be maintained. In what follows we will investigate a recent strategy to delineate the meaning-constitutive from the non-meaning-constitutive, a strategy that has been employed to make sense of revision of logical beliefs. We conclude that the strategy is unsuccessful. Fortunately, the revisionist has other resources. We take a second moral from Priest, who joins Field in the choir of skeptics. Unwilling to accept that a paraconsistent approach is *merely* a change of meaning, he says:

> Someone who rejects classical logic, say a paraconsistent logician, need not deny that the (classical) meaning of '$\sim$' is sufficient to guarantee the validity of inference $p.\sim p \vdash q$ in classical logic (the pure abstract logic); what they will certainly deny is that this is the meaning of negation, as it occurs in vernacular reasoning [...] According to them, the semantics of their pure logic is the correct semantics for vernacular negation. Seen in this way, a dispute between rival logics is, then, exactly a dispute over meaning. (Priest 2006a, p. 171)

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[6]This, presumably, is why the paraphrase with 'meaning' is anathema to Quine. Compare also Putnam (1976): "we simply do not possess a notion of 'change of meaning' refined enough to handle this issue" (p. 190).

True, there are debates about logical revision which are motivated by the desire to accurately model ordinary reasoning, say, the many uses of 'not' in natural language. Nevertheless, it is equally clear that this cannot be a general guide to understanding revisionary debates in the philosophy of logic. As was accentuated by our discussion of Dummett in Section 2.3.1, revision of logic can be motivated by non-empirical considerations having to do with, say, meaning-theoretical principles. And Dummett is certainly not alone in this tradition: For semantic paradoxes, logicians are not ordinarily in the game of taking their cue from ordinary speakers—the problem is exactly that they get it wrong. Although some are willing to accept inconsistent practices, and a theory of truth embedded in such a practice, most would prefer to be less Wittgensteinean about logical theorising. The upshot is that we are in need of a plausible story about when we are entitled to revise our logical principles, and, subsequently, how it affects the semantics of logical constants.

5.2   Revisionism and Semantics

5.2.1   PTS Again

Before we can return to the issue of revisionism, let us first try to sharpen some of the semantic underpinnings of the meaning-change argument. In order to give some content to Field's remark above, let us revisit some of the fundamental ideas from foregoing chapters: What is the connection between INF, i.e., that the meaning of a logical constant is fixed by the constant's behaviour in the inferential rules which govern its use, and revision of logic? Even more specifically, what is the connection between revision and INF as captured by PTS?

In order to shed light on the significance of proof-theoretic harmony, and, ultimately, the limitations of proof-theoretic harmony, we will investigate the interplay between meaning-constitutive rules and revision of logical beliefs in a PTS

setting. As before, we will understand talk of inference rules as talk of formal rules in a proof system. The result is that we carry out the discussion of logical beliefs—and revision of them—in the context of formal systems. Some authors, noticeably Resnik (2004), have made much out of the distinction between revision of formal systems and revision of logical beliefs (about reasoning in our vernacular).[7] Without hesitation we admit that there is a significant gap, but, firstly, the present debate is not merely about "revising" our formalism—whatever that might mean—it is about studying ordinary reasoning through a mathematical idealisation, i.e., it is formalism together with an application; and, secondly, many of the fields for which revision of logical belief is most pertinent deal to a large extent with formal logic as a model of ordinary reasoning, e.g., the semantic paradoxes, vagueness.

Of course, discussing revision in a formal setting means that outcome is to some extent hostage to the limits of the formalism—even if we stay neutral about choice of *logic*. Because of this the choice of formal framework is far from innocuous.[8] For the most part, we stay neutral about whether natural deduction or sequent calculus (or another form of axiomatization) is preferable. Yet, as we shall see, which choice is made has affected the debate about what meaning-constitutive rules are.

5.2.2   Gentzenianism and Hilbertianism

With PTS we can now revisit Field's remarks about the distinction between meaning-constitutive and non-meaning-constitutive principles. There are at least two important issues that need to be elaborated before the connection to revisionism becomes clear. The first is a question about the structure of meaning-constitution: Which inferential rules ought we consider meaning-constitutive? The

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[7]In general, another prominent critic of formal methods applied to ordinary reasoning is Harman (1986).

[8]Recall, for example, the difference in non-conservativeness between natural deduction and sequent calculus (Section 2.3.2).

second is about the nature of meaning-constitution: In what sense can a set of inferential rules fix the meaning of an expression, and—given the fuzziness of the label 'meaning'—what *exactly* is being fixed in the process? We start by addressing the former question. In Chapter 6 we return to the second question.

In the chapters so far we have investigated the Gentzenian tradition in some details, focusing especially on the notion of proof-theoretic harmony. As we have seen, a key feature of what we call Gentzenian PTS is that specific subsets of the primitive inference rules of a system are considered meaning-constitutive. If we assume a standard natural deduction framework, for example, Gentzen's original proposal was to think of the intro-rules as an (implicit) 'definition'. There is, however, no immediate reason why one ought not think of the *elim-rules* as meaning-constitutive instead (e.g., Schroeder-Heister 1985), or even a mixed approach (e.g., Milne 1994 and Rumfitt 2000). If you think, as has been proposed for instance by Peacocke (1987), that the *obviousness* of a basic inferential rule is a symptom of its meaning-constitutive power, you might conclude that *modus ponens* is meaning-constitutive for '→' whilst for '∨' it is the intro-rules.[9] But, a rule appearing to be obvious seems to be too much of an unstable phenomenological criterion to have real semantic import.

Alternatively, one might suggest, with Dummett, that a choice of which subset of rules is semantically primitive has to do with *assertion-conditions*, *commitments*, and *entitlements* (see Section 2.3.1). If one subscribes to such a thought, and, additionally, that intro-rules encode the semantically central aspect of such notions, then there is a *pro tanto* reason for ascribing priority to them. Irrespective, we have seen, in Section 3.2.3, that such a division of inference rules is tendentious: There is no reason for *assertion* to dominate the theory, rather than, say, denial. In fact, in Chapter 6, we will take this thought further and explore the idea of introducing other speech acts as well.

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[9]Formally, this might be because *hypothetical* rules, i.e., using discharge of assumptions, are considered more involved than *categorical* rules.

Whatever choice one makes with respect to subsets of inference rules, however, the key aspect of Gentzenianism might be considered independent. The idea, following Dummett, is that an inferential practice might be dysfunctional—and so susceptible for revision—because of internal conflict between basic principles. At the core of Gentzenianism is the idea that implementing proof-theoretic harmony as a functional constraint is a systematic fashion in which logic can (and should) be revised. GE-harmony, as explored in Section 4.3, focuses on intro-rules, but this perspective is contingent: There is no reason why one should not devise functional constraints which operate in the opposite direction.

In contrast to the Gentzenian tradition there is a less discerning approach which refuses to divide the inferential rules into meaning-constitutive and non-meaning-constitutive. Such a tradition—clearly part of the Quinean heritage to which Field's above remark tacitly subscribes—is typically labelled *holism*. But to avoid confusion with other positions also termed 'holism' we prefer calling this *Hilbertianism* (as opposed to Gentzenianism). The main claim, that the system of rules contributes *tout court* towards the meaning of the involved logical expressions, is reminiscent of the pre-Programmatic Hilbert's *Foundations of Geometry* where the axioms of the system are both foundational principles and (implicit) definitions.[10] Some further content is lent to the label by the fact that Gentzen wrote his proof-theoretic work as a student of Hilbert and the formalistic school.[11]

The obvious advantage of the Hilbertian approach is that the problem of motivating a distinction between meaning- and non-meaning-constituting disappears—and with it the technical problems posed by harmony and other Gentzenian tricks. The Hilbertian is happy to admit that the entire axiomatic system (and think now of axioms as degenerate cases of inferential rules) fixes the content of the involved logical constants. A bit anachronistically, then, we can include under the label

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[10]For Hilbert's own outline of the idea, see his famous correspondence with Frege in Frege (1980).

[11]There is an analogy here to the philosophy of mathematics debate about neo-Fregeanism. In Ebert & Shapiro (Forthcoming) the term 'neo-Hilbertian' was coined.

'Hilbertianism' content-fixing by other formalisms like natural deduction and sequent calculus. The idea, however, remains the same: No proper part of the axiomatisation is semantically responsible. Dummett remarks critically:

> The meanings of all the expressions of the language are, on this view,
> determined by our linguistic practice *as a whole*. If we change any part
> of the practice, we may change the meanings of indefinitely many—in
> the limiting case, of all—the expressions in the language. (Dummett
> 1991, p. 228)

For example, the Hilbertian might take a more lenient view on non-conservative extensions of the system: True, introducing a new operator, λ, might lead to new consequences in the original λ-free language, but this just reflects the plasticity of the semantic content when the system at large is updated. This could be the attitude towards, say, the non-conservativeness of classical negation with respect to the implicational fragment of intuitionstic logic. Of course, such a concession is impossible for Dummett and his followers as it blunts their chief argument against the classicist. Treating the semantics of logical constants in isolation (or at least not *en masse*) is considered pivotal by the Gentzenian.

Why the insistence on segregation? Because with Hilbertianism comes the threat of a re-conjuration of the anti-revisionism that the Gentzenian sought to dispel. Dummett made the case that the holist comes in a broadly Wittgensteinian shape.[12] There is no sense in which the practice can semantically malfunction; it bestows meaning on the expressions blindly. In other words, the Hilbertian is willing to admit that an inconsistent practice is bad for independent reasons, but that it is does not impinge on the semantics.

That is not to say that the Hilbertian cannot combine semantic holism with consistency as a requirement on proper meaning-forming. What the Hilbertian balks

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[12]Recall the mention of inconsistency and disharmony in Section 2.3.1. See Dummett (1991, pp. 209-10).

at is the possibility of local constraints on fragments of the language (such as GE-harmony). One manner in which to phrase the situation, then, is to say that the Hilbertian, in virtue of only accepting semantic malfunction in the whole system, has less possibility of locating—and compartmentalising—the semantic effects of revision. Perhaps the Hilbertian is willing to live with that since since the upshot of the position seems to be that revision of logic cannot be motivated by semantic malfunction.

5.3   Minimalism for Logical Constants

5.3.1   The Semantic Core

Let us return to the old impasse. The desideratum was to provide some sensible framework for revision of logic, one in which the discovery of paradoxes, say, could legitimately lead us to give up one logic for another. The Gentzenian and the Hilbertian, as outlined above, offer two approaches where revisionism is combined with INF. Common between them, however, is the acknowledgement that revision of logic involves meaning-change.

For the revisionist who set out to defuse the meaning-change argument this solution is not altogether happy. Short of full surrender to the Quinean point, they wanted change of logic with stable semantic content across the revision. To achieve this, one ought to expect that the challenge of characterising meaning-constitutive rules must be faced head-on. What are the prospects for this more ambitious revisionism?

One approach which has received attention lately finds inspiration in some oblique remarks made by Putnam (1957):

> It will be argued that the words 'true' and 'false' have a certain 'core'
> meaning which is *independent* of *tertium non datur*, and which is ca-
> pable of precise delineation. (ibid., p. 166)

Putnam wanted to safeguard the truth predicate used in quantum logic discourse
from a meaning-change argument launched by classicist skeptics. These critics
had proposed that the alleged logical revision involved in the quantum framework
was a 'mere change of meaning', in particular, the proponents of quantum logic
meant something different by 'truth' than their classical ex-comrades.[13] His re-
sponse was that we need not accept that something like the *principle of bivalence*
is meaning-constitutive for 'truth'. Rather, Putnam focused on the many com-
monalities between the classical and the many-valued predicate, and suggested
that these might be sufficient for sameness of meaning.

Later, in Putnam (1976), the meaning-change argument for logical constants is
raised. "[I]t might be suggested", he says, "that we identify the logical connectives
by the logical principles they satisfy" (ibid., p. 188). Equally unimpressed with
this as a 'meaning-change' argument against quantum logic, Putnam counters by
proposing that there is nothing forcing us to include *distributivity* (i.e. $\ulcorner(p \wedge (q \vee r)) \leftrightarrow ((p \wedge q) \vee (p \wedge r))\urcorner$) as a meaning-constitutive principle.

> Only if it can be made out that [distributivity] is 'part of the meaning'
> of 'or' and/or 'and' (which? and how does one decide?) can it be
> maintained that quantum mechanics involves a 'change in the meaning'
> of one or both of these connectives. (ibid., p. 190)

Perhaps feeling the weight of Quinean anti-analytic considerations, Putnam ends
on a cautionary note, rejecting the idea that our notion of 'change of meaning' is

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[13]It goes without saying that this debate about logic was the natural counterpart of similar
debates in the philosophy of science. Consider, for example, the Kuhnean line in which cer-
tain terms (e.g., mass) takes on a different meaning after a shift paradigm (say, Newtonian to
Einsteinean), and the inter-paradigmatic communication is riddled with *incommensurability*.

refined enough to draw such a distinction. Yet, others, less inhibited by Quine's warning, have taken Putnam's suggestion at face value, and propose to tackle the problem of refining our notion of 'change of meaning'. Here from Restall (2002):

> If any set of rules is sufficient to pick out a single meaning for the connective, take that set of rules and accept those as meaning determining. The other rules are important when it comes to giving an account of a kind of logical consequence, but they are not used to determine meaning. (ibid., p. 11)

Entertaining this thought for the time being, we see that the problem is the same as for the Gentzenian. How do we draw a line between meaning-constitutive and non-meaning-constitutive principles in a non-arbitrary way? We call the claim that there is a 'semantic core', consisting in a privileged set of principles governing a logical constant, *minimalism for logical constants*.

5.3.2   Revision and the Context of Deducibility

Let us stick with Putnam's example for a second. Distributivity is a trademark difference between classical logic and quantum logic. The quantum logician subscribing to minimalism for the logical constants wants to abandon the principle, yet does not admit that it affects the meaning of logical constants. Axiomatic systems notwithstanding, the principle is standardly a theorem derived from a set of primitive rules. The question, then, becomes which rules are modified, and how, from classical logic to quantum logic.

As we have seen previously in Example 4.1, the difference in primitive rules in **Nqp** comes with the rules for *disjunction*. A minor modification of the ∨-elim rule blocks the derivation of distributivity:

$$\frac{\Gamma \quad\quad [A]^u \quad [B]^v}{A \overset{\vee}{} B \quad \overset{.}{C} \quad \overset{.}{C}}{C} \ {\scriptstyle (E\vee)} \qquad\qquad \frac{\Gamma \quad\quad \Theta,[A]^u \quad \Delta,[B]^v}{A \vee B \quad \overset{.}{C} \quad \overset{.}{C}}{C} \ {\scriptstyle (E\vee)}$$

or represented in sequent-style:

$$\frac{\Gamma \vdash A \vee B \quad \Delta, A \vdash C \quad \Theta, B \vdash C}{\Gamma, \Delta, \Theta \vdash C} \ {\scriptstyle (\vee E^*)} \qquad\qquad \frac{\Gamma \vdash A \vee B \quad A \vdash C \quad B \vdash C}{\Gamma, \vdash C} \ {\scriptstyle (\vee E^*Q)}$$

The above rules differ only with respect to the contexts, Δ, Θ, allowed in the sub-derivations, and we saw in Section 4.2.2 how both rules have the same intro-rules, the standard $I\vee$ rules. In other words, the quantum logic disjunction is a special case of the classical counterpart (i.e., where the contexts are empty). Inspired by such an example, and with Putnam's minimalism in mind, one might suggest that only change of contextual parameters in a rule does not engender a change of meaning.

Another example—due to Greg Restall—will perhaps further entrench the idea. In the dispute between the classicist and the intuitionist it is notoriously difficult to pinpoint precisely where the difference ought to be located. As witnessed by the numerous different axiomatisations of the logics, it is alarmingly presentation-dependent which logical constant produces the difference (or, rather, which rules produce the difference). For instance, in Prawitz style natural deduction, **Ncp**, the only difference is $\bot E_I$ and $\bot E_C$, where the former is a special case of the latter. Alternatively, one might think to blame disjunction, in the classical system **Ncp**^{LEM}, or locate the difference in implication and include Peirce's law in rule form (see Appendix A).

The minimalist might say: Despair not. Fortunately, there is a type of formalism that avoids the problem and puts the difference in line with the quantum case. For, as we saw in Section 2.3.2, sequent calculi for classical and intuitionistic logic

(in the **G1** type system with explicit structural rules) differ only with respect to succedent contexts. In particular, this is the only difference in the ¬-rules:

Example 5.1. Negation:

$$\frac{\Gamma \Rightarrow A, \Delta}{\Gamma, \neg A \Rightarrow \Delta} \; L\neg_{CL} \qquad \frac{\Gamma, A \Rightarrow \Delta}{\Gamma \Rightarrow \neg A, \Delta} \; R\neg_{CL}$$

$$\frac{\Gamma \Rightarrow A}{\Gamma, \neg A \Rightarrow} \; L\neg_{IL} \qquad \frac{\Gamma, A \Rightarrow}{\Gamma \Rightarrow \neg A} \; R\neg_{IL}$$

This observation is also made in Haack (1974, p. 10). She continues in a minimalist vein: "Since this restriction involves no essential reference to any connectives, it is hard to see how it could be explicable as arising from divergence of meaning of connectives". Again we have a situation where the only difference is in *auxiliary parts* of the rules. However, the weakening of the rules is sufficient to block any classical derivation which is not intuitionistically valid. In particular, the law of exluded middle (LEM) and double negation elimination (DNE) fail. Incidentally, by restricting the *antecedent* contexts instead, we get *dual intuitionistic logic*, **G1dip**, a paraconsistent system where explosion and double negation introduction fail:[14]

Example 5.2. Dual-intuitionistic negation:

$$\frac{\Rightarrow A, \Delta}{\neg A \Rightarrow \Delta} \; L\neg_{CL} \qquad \frac{A \Rightarrow \Delta}{\Rightarrow \neg A, \Delta} \; R\neg_{CL}$$

Restall (2002) makes a similar observation about the difference between classical and *relevant* negation. Here $L\neg_{CL}$ and $R\neg_{CL}$ suffice for both logics, but whereas classical logic allows the structural rule K, weakening, relevant logics typically do not:

$$\frac{\Gamma \Rightarrow \Delta}{\Gamma, A \Rightarrow \Delta} \; KL \qquad \frac{\Gamma \Rightarrow \Delta}{\Gamma \Rightarrow A, \Delta} \; KR$$

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[14]See e.g. Goodman (1981) and Urbas (1996).

The minimalist for logical constants has the following conjecture: The negation rules involve the same *concept of negation*, but allow different contextual constraints, that is, different *environments* of derivability, or, in the terminology we have preferred above, different *proof-theoretic frameworks*. Clearly, this is in line with Belnap's observation that logical constants must be tested for conservativeness and uniqueness on the backdrop of an *antecedent context of deducibility* (see Section 2.2.3).[15] The result is systems where the logic of negation differs, although the meaning-constitutive rules are shared between them. As a consequence, negation has *shared semantic content* across the logics, although the theories of negation are different.

In general, we want a situation where one parameter, namely facts about the proof-theoretic framework (e.g., multiple- vs single-succedent), does not influence the content of the logical constants. Let us say that these facts about the proof-theoretic framework are *structural properties*. As anticipated by Belnap's remark, this open-ended category includes properties such as transitivity, monotonicity, reflexivity, multiple- vs single-conclusion, etc.

5.3.3 Operational Meaning vs Global Meaning

With the above observations comes the promise of a principled way to separate the meaning-constitutive from the non-meaning-constitutive. In fact, some authors have already attempted to articulate more precisely what the distinction hinted at consists in. The most developed minimalist position is due to Paoli (2003, 2005). Paoli follows Wansing (2000) in giving a sequent calculus framework for PTS.[16] The motivating idea is to profit from the divide between *operational* and *structural* rules in sequent calculus, e.g. **G1cp**, a distinction that—as we shall

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[15]See also Restall (2007).

[16]Although Wansing's view of what counts as meaning-constitutive appears to differ from Paoli's: "The basic idea of proof-theoretic semantics is that under certain conditions the schematic rules for introducing an n-ary connective f into premises and conclusions, *together with a set of structural assumptions*, specify the meaning of f." (ibid., p. 9, emphasis added)

see in Section 5.3.5—is mostly tacit in natural deduction. Roughly speaking, the difference is between rules that govern derivation steps involving principal occurrences of a particular logical constant, and rules that govern derivations in general. Put syntactically, operational rules *look inside* formulae while structural rules only discern the structure of a sequent.[17] Examples we have already seen are weakening (K), contraction (W), cut, exchange, but others are possible too, depending on the type of sequent we are working with.

A first shot at the thesis: Structural rules—in virtue of their generality—are not meaning-constitutive for any logical constant. Paoli first applies the distinction to tease out two notions of meaning in PTS. For a logical constant λ, and a sequent calculus **S**:

Definition 5.1. Two types of meaning in **S**:

- *Operational meaning* of λ: Fully specified by the operational rules for λ in **S** ($L\lambda$ and $R\lambda$);

- *Global meaning* of λ: Specified by the class of **S**-provable sequents containing λ.

Let us look at a concrete example. Recall the distinction between additive and multiplicative connectives from Section 4.5.2. Here are the additive rules for $\wedge$ and $\vee$:

$$\frac{\Gamma, A_i \Rightarrow \Delta}{\Gamma, A_0 \wedge A_1 \Rightarrow \Delta} \ (L\wedge) \qquad \frac{\Gamma \Rightarrow A_0, \Delta \quad \Gamma \Rightarrow A_1, \Delta}{\Gamma \Rightarrow A_0 \wedge A_1, \Delta} \ (R\wedge)$$

$$\frac{\Gamma, A \Rightarrow \Delta \quad \Gamma, B \Rightarrow \Delta}{\Gamma, A \vee B \Rightarrow \Delta} \ (L\vee) \qquad \frac{\Gamma \Rightarrow A_i, \Delta}{\Gamma \Rightarrow A_0 \vee A_1, \Delta} \ (R\vee)$$

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[17]Compare Gentzen (1934, p. 82): "[B]ut we have the advantage of being able to reserve them [structural rules] special places within our system, since they no longer refer to logical symbols, but merely to the structure of the sequents".

In addition to the classical negation rules already presented in Example 5.1, the sequent calculus **G1cp** for classical logic shares the above operational rules with the *subexponential linear logic* **LL** (without multiplicative connectives).[18] However, unlike **G1cp**, the linear logic **LL** does not have the structural rules weakening or contraction. In terms of operational meaning in Def. 5.1, that entails that there is shared semantic content for the logical constants, e.g., $\neg$, even though the **G1cp**-class and the **LL**-class of derivable sequents are distinct, i.e., the global meaning differs.

Thus, obviously, operational and global meaning often come apart: There are endless substructural systems which display the same relationship as that between **G1cp** and **LL**. The adherent of PTS is left with a choice: Should the semantic content of λ be specified globally or operationally? Paoli, who is himself concerned with Quine's meaning-change argument, suggests that the minimalist ought to adopt operational meaning. What is the advantage?

> This allows to counter Quine's meaning variance charge: since the operational meaning of negation remains unaltered, there is no such "change of subject" as Quine adumbrates. Genuine disagreement arises whenever **L** and **L**$'$ ascribe different properties to what we can plausibly identify as the *same* constant, given the invariance of its operational meanings across logics. (Paoli 2005, p. 557)

The minimalist for logical constants denies that there is a shift of meaning in cases where the operational meaning remains invariable. Granted, there are logics, and debates about logical revision, that outstrip change of the global meaning alone, but at least the minimalist can salvage a number of important revisionary debates. With shared content guaranteed, the minimalist avoids the threat of rendering, say, the dispute between the classicist and the intuitionist a 'mere verbal disagreement'.

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[18]For details, see Paoli (2002, ch. 2).

On the other hand, the Hilbertian (or the holist) is free to adopt global meaning as the correct notion of semantic content—with the result that *any* change in the class of derivable sequents (theorems) might yield a change in meaning. However, there is a lot of flexibility in the global notion of meaning: Is the class of sequents containing the logical constant λ the class containing *only* λ; or, perhaps, containing only λ, and, further, only one occurrence of λ? Constraints such as, e.g., purity and sheerness, have counterparts in sequent calculus, and the Hilbertian must take care to develop a precise formulation of global meaning.[19]

5.3.4 Structural Properties

For now we leave global meaning behind and focus on operational meaning. It is crucial to observe that minimalism in its current 'operational' form does not do all that we set out to do. For instance, the difference between the classical and quantum disjunction in Section 5.3.2 seems to amount to a bifurcation of operational rules, and thus of operational meaning. But, the objective was for these two logics to have the same disjunction. Similarly, classical, intuitionist and dual-intuitionist negation do not differ with respect to derivable sequents because of structural rules; it is the contexts in the operational rules that do the trick. Thus, in the framework of Def 5.1 there is still meaning-variance galore. Just as we suggested strategies for how the Hilbertian can make precise the notion of global meaning, the minimalist must find a more fine-grained formulation of operational meaning. In practice, this amounts to dissecting the notion of operational rule, to find the constituents that must remain invariant.[20]

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[19]Paoli (2003, p. 537) mentions two different policies for the Hilbertian: Molecularist, which disallows the presence of other logical constants; and, contextual, which makes no such further demand.

[20]In fact, Paoli does seem to think that restrictions on contexts belong to the global meaning: "We disregard for the moment the problem of the possible restrictions on contexts, which however pertains to the structural aspect of the logic, rather than to the operational one." (Paoli 2003, p. 539).

Thus, evidently, we must refine the definition so that global meaning not only incorporates structural *rules* (like in the example of **G1cp** vs **LL**) but also, more generally, structural *properties* (like restrictions on contexts). Borrowing a notion from Sambin et al. (2000), what we are after is something like *visibility* for operational rules. They make the following observation about their system, *basic logic* **B**:

> One of the main discoveries of basic logic is that the meaning of a connective is determined also by contexts in its rules, which can bring in latent information on the behaviour of the connective, possibly in combination with other connectives.[21]

In order to get a handle on the semantic contribution made by contexts, they propose the following restriction: Neither the active formula(e) in the sequent-premises nor the principal formula in the conclusion are flanked by contexts (i.e., they are *visible*), and all passive contexts—on the opposite side from active and principal formulae—are free. Compare rules from **G1cp** and **B** respectively:[22]

Example 5.3. Visibility in $L\rightarrow$:

$$\frac{\Gamma \Rightarrow A, \Delta \quad \Gamma, B \Rightarrow \Delta}{\Gamma, A \rightarrow B \Rightarrow \Delta} \ (L\rightarrow) \qquad \frac{\Rightarrow A \quad B \Rightarrow \Delta}{A \rightarrow B \Rightarrow \Delta} \ (L\rightarrow)_B$$

Now consider something more radical: Ignore contexts on the passive side of the sequents. The result is not supposed to be an actual sequent calculus rule, but rather a fragment of the rule with no information about contexts. Thus, the minimalist might hold that operational meaning is unaltered by any difference in contexts, be they on the active or passive side of premise-sequents or conclusion-sequent. In this way, also differences such as those in Examples 5.1 and 5.2 (context-restrictions in negation rules) can be considered semantically insignificant.

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[21]The last remark is interesting. Note, for example, that even in a logic without explicit weakening rules, the structural rule can be partially or fully recaptured by the contexts in operational rules. See Troelstra & Schwichtenberg (2000, ch. 3.5). See also footnote 44, Chapter 4.

[22]For details on the system **B**, see Sambin et al. (2000, p. 993).

5.3.5   Natural Deduction

Re-conceptualising Paoli's framework more broadly, in terms of structural assumptions, also provides the means to revisit natural deduction rules. Although there are exceptions, like sequent-style natural deduction and **NC** (see Section 3.2.2), standard natural deduction does not have explicit structural rules. Rather, like the **G3** type sequent calculi, natural deduction systems have *absorbed* structural rules. That is, structural properties are implicitly included in the operational rules; in this case, the intro- and elim-rules.

We can then envisage a parallel notion of operational meaning for natural deduction. The question is how we can locate the structural contribution which corresponds to structural rules in sequent calculus. There is some hope for doing this. Translations between sequent calculus and natural deductions trade on the similarities between the structural rules weakening and contraction and so-called *discharge policies*.[23] Consider again conditional proof, $\rightarrow I$:

$$\frac{\begin{array}{c}[A]^u\\ \vdots\\ B\end{array}}{A \rightarrow B}\ {\scriptstyle(\rightarrow I)(u)}$$

In the execution of this rule there is a global property impinging on the derivation. Since the rule is hypothetical it involves the discharge of an assumption. It is global because the discharge can occur at any point higher up in the derivation tree. Compare sequent calculus where discharge is integrated locally in sequent-rules. It is this global character of natural deduction that Girard referred to as "quite a serious defect" (Girard 1995, p. 15).

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[23]See Zucker (1974), Pottinger (1977), Negri & von Plato (2001, ch. 8), and von Plato (2001). Incidentally, the translation by von Plato depends on developing natural deduction with general elimination rules.

That aside, it is interesting for our purposes that the policies governing the discharge of assumptions is a multi-faceted thing. Classical logic, **Ncp**, for example, allows both *multiple discharge* and *vacuous discharge*. Let us give some examples:

Example 5.4. Discharge policies:

$$\frac{\dfrac{[A]^1}{B \to A}}{A \to (B \to A)} \, (1) \qquad \frac{\dfrac{\dfrac{[A \to (A \to B)]^2 \quad [A]^1}{A \to B} \quad [A]^1}{\dfrac{B}{A \to B} \, (1)}}{(A \to (A \to B)) \to (A \to B)} \, (2)$$

When we appreciate that discharge policies are not tied to any particular intro- or elim-rules—they are global restrictions on handling assumptions—we can see that the policies intuitively correspond to structural rules: Multiple discharge to contraction; vacuous discharge to weakening. And, as expected, in the case of →, disallowing vacuous discharge yields a *relevant implication*; disallowing both vacuous and multiple discharge yields a *linear implication*.

Can we talk about a distinction between global and operational meaning in natural deduction? Yes, it appears pertinent to categorise discharge policies as structural properties. Also, non-conservativeness appears to leave precisely the type of gap that can motivate a distinction between global and operational meaning: For instance, as we saw in Section 2.2.3, the presence of classical negation alters the class of provable →-formulae. The minimalist is free to deny that classical negation impinges on the meaning of implication. Rather, the operational meaning is invariant even though the class of theorems is not.

An interesting case of non-conservativeness comes from adding the rules for classical negation to the rules of intuitionistic negation. As was shown by Harris (1982) intuitionistic negation collapses into classical negation in standard proof-systems, i.e., $\neg_C A \dashv\vdash \neg_I A$. Some authors (e.g., Hand 1993) have used this as an objection against the the intuitionist's negation: There is asymmetry since the meaning of the intuitionistic negation is obliterated by the presence of its classical colleague,

but not vice versa. For the minimalist, such an argument appears too hasty. The *meaning* of intuitionistic negation does not collapse in to the *meaning* of classical negation, even if the class of theorems become the same.

5.3.6   Other Formalisms

Read, another inferentialist with minimalist leanings, has argued for shared content for a range of normal modalities. As a response to the fact that the standard natural deduction rules for normal modal operators are disharmonious (i.e., disharmonious by GE-harmony lights), Read (2008b) prefers *labelled* rules, for which harmonious formulations can be found.[24] In particular, labelled natural deduction allows us to formulate intro- and elim-rules for '$\Box$' that stay the same across a range of normal modal logics (**K**, **T**, **S4**, **S5**). Instead, the deductive difference between the system is due to structural rules for a relation, $<$, governing the indices (labels) of the system. Here are the intro- and elim-rules for $\Box$.[25] Read also observes that these rules are GE-harmonious.[26]

Example 5.5. Modal rules in labelled natural deduction:

$$\cfrac{\begin{array}{c}[i<j]^u\\ \vdots\\ A_j\end{array}}{\Box A_i}\,{}_{(I\Box)(u)} \qquad \cfrac{\Box A_i \quad i<j}{A_j}\,{}_{(E\Box)}$$

Put informally, on the assumption that a world i can see a world j (possibly $i = j$), if it follows that A holds in j, we can infer that $\Box A$ holds in i and discharge the assumption. For the elim-rule, if it is case that $\Box A$ holds in i and i can see j, then A holds in j.

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[24]See Section 4.1 and 4.2.2 for a prequel about natural deduction rules for modal operators.

[25]Read also gives analogous rules for $\Diamond$.

[26]To see this, notice that $E\Box$ is a simplification of:

$$\cfrac{\Box A_i \quad i<j \quad \begin{array}{c}[A_j]^u\\ \vdots\\ B_k\end{array}}{B_k}\,{}_{(GE\Box)(u)}$$

We can then impose structural rules on the relation $<$ to differentiate between different modal logics. For example, **S4** takes reflexivity and transitivity:

$$
\begin{array}{cc}
\begin{array}{c}
[i < j]^u \\
\vdots \\
A_j \\
\hline
A_j
\end{array} \; (T)(u)
&
\begin{array}{c}
 \quad \quad \quad [i < k]^u \\
 \quad \quad \quad \vdots \\
i < j \quad j < k \quad \quad A_l \\
\hline
A_l
\end{array} \; (4)(u)
\end{array}
$$

Of course, in order for such rules to be structural in the intended sense, $<$ must belong to the structural language of the proof-theory, and not to the object language. Read, calling them *auxiliary symbols*, volunteers the following opinion:

> The rules by which the logics differ are generic rules, rules governing the auxiliary symbols—the labels and the relation '$<$'—while the operational rules, the specific rules for the operations, remain constant. What this means is that there is throughout these logics, these theories of modality, the same sense of necessity and possibility. What is different is the logic that they satisfy, not the meaning of '$\Box$' and '$\Diamond$'.

In fact, he goes on to compare the situation to that of the relevantist and the classicist, thus tacitly subscribing to the broader minimalist thesis discussed above:

> We do not want the intuitionist, classicist and relevantist to disagree about the meaning of '$\rightarrow$'—that way lies Carnapian tolerance, with a different logic appropriate for each different meaning. Rather, we want them to agree on what they disagree about, that is, to disagree about the same thing, to attribute different logics to the one connective '$\rightarrow$', with a univocal meaning.

What about more exotic proof theoretic frameworks? Is there any hope of sorting them under a minimalist divide too? Once we have agreed that what goes on in the contexts stays in the contexts (semantically speaking), further generalizations

can be treated as structural assumptions. One prominent example that has led to proof-theoretic advances is *hypersequents*.[27] Hypersequents are finite multisets of ordinary sequents: $\Gamma_1 \Rightarrow \Delta_1 \mid ... \mid \Gamma_n \Rightarrow \Delta_n$. We read the bar, '|', as a metadisjunction over sequents. Let G be a metavariable for a sequent. Then the following are examples of negation rules:

Example 5.6. Hypersequent rules for negation:

$$\frac{G|\Gamma \Rightarrow A, \Delta}{G|\neg A, \Gamma \Rightarrow \Delta} \; (\neg L) \qquad \frac{G|A, \Gamma \Rightarrow \Delta}{G|\Gamma \Rightarrow \neg A, \Delta} \; (\neg R)$$

For our purposes we observe that the core part of the rule remains unchanged. In other words, the minimalist can insist that even in the presence of sequents as contexts, the operational meaning is stable.[28] Good news for the minimalist since hypersequents can be used to formalise a variety of systems for which we have no ordinary sequent calculus.

5.4 Consequence and Meaning-Change

What did the minimalist accomplish? The notion of operational meaning is introduced to give a stable 'core' meaning for logical constants over a range of systems. If successful, the minimalist can circumvent the meaning-change argument and reestablish notions of shared content and genuine (non-verbal) disagreement in logic. Of course, the minimalist need not deny that sometimes revision of logic involves change of meaning, but she has the resources to keep stable meaning for many prominent debates about revision.

Nonetheless, the minimalist about logical constants still faces some unpleasant difficulties. First, it is difficult to motivate any sharp demarcation between what

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[27]Hypersequents were introduced by Pottinger (1983) and Avron (1987). See Paoli (2002, ch. 4.1.2) for a brief introduction.

[28]A similar conclusion can be reached for another generalisation of sequent calculus: n-sided sequents. We wait with the introduction of these to a more opportune moment in Chapter 6.

count as structural properties and what count as operational properties. Just like the development of proof-theory itself, the identification and separation of structural properties is an open-ended process. It is fundamentally hostage to what sort of proof-system one is considering: Sometimes what we believe to be a core feature of a logical constant is extracted and formulated as a structural property encompassing the entire system. Think, for example, of Gentzen's structural rule exchange. If the minimalist were willing to genuinely regard this rule as structural, and thus as semantically insignificant, the so-called core of the logical constants will become very insubstantial indeed.

Even if one can find a formally non-arbitrary distinction between the structural and the operational, it remains to be shown that such a divide is also philosophically well-motivated. Inferentialism takes the meaning of a logical constant to be fixed by the inference rules that govern its use. Undeniably, structural rules (and structural properties) also govern the inferential use of our logical constants, so there is a meaning-theoretical pressure to explain why there should be a semantic division of labour. Of course, such a charge rests on the assumption that PTS ought to be largely informed by ordinary reasoning practice, an assumption that I find doubtful. Nevertheless, it is worth flagging that there is no consensus on the semantic importance of structural rules. In fact, some people, like Došen (1989), Girard (1995) and Sambin et al. (2000), have taken the opposite view, maintaining that the meaning of logical constants is largely due to the contribution of the structural rules:

> In fact, a close inspection shows that the actual meaning of the words "and", "imply", "or", is wholly in the structural group and it is not too excessive to say that a logic is essentially a set of structural rules! (Girard 1995)

Such an attitude is underscored by the fact that structural properties have a growing role in the philosophical debates about logic and revision of logic. Certain properties of consequence, like reflexivity and transitivity, have sometimes been taken for granted, if not even constitutive of consequence relations.[29] Nowadays, however, philosophers are becoming less hesitant about revising structural properties in order to solve puzzle cases. A well-known example is the relationship between Curry's paradox and contraction. But other examples exist: Transitivity has been rejected both for purposes of relevance (Tennant 1997) and for the logic of vagueness (Zardini 2008).

There is a natural follow-up question: If we accept the minimalist thesis, and consider, say, the structural rule weakening as having no semantic significance for logical constants, what exactly is the debate between the proponent and the opponent of K *about*? There is shared content for logical constants, so the disagreement is not, for example, about the meaning of 'and'. One possible line is to consider there to be a fact of the matter, and ontologically grounding such a debate on *logical facts*. But for the metaphysically squeamish this is simply opening the floodgates: The non-factualists about logic (e.g., Resnik 1996, 1999, 2004) cannot accept that the content of a revisionary debate involves realist commitments. What is more, for the Dummettian anti-realist, the project of reducing ontological questions about realism and anti-realism comes to a grinding halt.

Is there an alternative? The most convincing strategy, I suggest, is to concede that structural properties do have semantic import, but with the important reservation that they do no have semantic import for logical constants. Instead, we take our cue from the close relationship between structural properties in proof-theory, and properties of consequence relations (or consequence operators). We have mentioned that there is, for example, a suggestive connection between monotonicity and weakening, between transitivity and cut. If we take such properties to be properties of logical consequence, it seems plausible that the properties are tied

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[29]See e.g. Hacking (1979).

up with the content of a validity predicate, *Val*, ranging over arguments. The proposition expressed by 'Val($\ulcorner \langle \Gamma, A \rangle \urcorner$)' is, in other words, sensitive to which properties one ascribes to logical consequence.

That would help us understand what an argument about structural properties is *about*, but it would reidentify the dispute as merely verbal. That is, two parties disagreeing over structural properties amounts to subscribing to different validity predicates. A problem becomes immediately apparent: If the original task was to circumvent Quine's meaning-change argument, we have now reintroduced the worry. Presumably, beliefs about validity or consequence or what follows from what is as part of our web of belief as beliefs involving only logical constants.

5.5   Conclusion

Field is certainly right in saying that the argument from meaning-change depends on an account of what it is for a set of inference rules to be meaning-constitutive. Nonetheless, that is not an invitation to give up on the very idea of inference rules being meaning-constitutive, any more than it is an invitation to give up on the distinction between a mere verbal dispute and a genuine dispute in logic. We have concluded that extracting structural properties and considering them without semantic significance is a poor strategy. We have nothing more than a promissory note to the effect that a stable and non-ad hoc line between the structural and the operational can be drawn. And either way, the idea that structural properties underwrites a sort of *logical fact* that is independent of the semantics of logical constants is obscure. In the end, structural properties contribute to the semantics, be it at the level of logical constants or at a metalevel for notions such as validity.

The challenge for PTS is to investigate the semantic role played by proof-conditions, even if these proof-conditions are given by structural properties. In the next chapter we turn to a new theory for how inference rules determine meaning. It will lead

us to reconsider the way in which the meaning of logical constants is framework-sensitive.

Chapter 6

PTS and Determining Meaning

6.1 Introduction

In Section 1.3.1 we introduced the informal idea of logical inferentialism (INF) as the claim that logical constants have their meaning fixed by the inferential rules that govern their use. Hitherto we have focused on how to make this claim precise in PTS: First, what sorts of constraints ought to be put on meaning-determining inference rules (in particular, proof-theoretic harmony); second, what is the relationship between INF and revision of logic.

However, we have spent little time dwelling on precisely what is meant by 'fixing meaning' or 'determining meaning' in INF. Traditionally, little detail has been provided in the literature, although it is frequently claimed that inference rules are *meaning-conferring* (e.g., Dummett 1991, p. 289 and Milne 1994, p. 50), *meaning-constitutive* (e.g., Tennant 1997, p. 234), *sense-conferring* (ibid., p. 229), that the meaning of the connectives can be *read off* of the rules (Dummett 1991, p. 205) or even that the rules should be *equated* with the meaning. Presumably, these different glosses on what determining meaning comes to are not equivalent.

Philosophers like Dummett, Prawitz, and Tennant typically have a dislike for *reification* of meaning. Sanctioning entities—*meaning-makers* if you want—which underwrite the semantic value of an expression is held to be metaphysically dubious. The result is a hesitation in taking seriously the question: What *is* meaning? The hardliners might want to answer with a shrug: Meaning *is* the use.[1] Or, more radically, reject the question as confused. Either way, many philosophers—myself included—will be left mystified. Most will agree that meaning *supervenes* on use, but they still want an answer to the question. What is it, if anything, that is being determined by the use?

Anyone subscribing to INF, who is sympathetic to the thought that meaning cannot simply be equated with use, ought to have a story of *how* meaning is determined by inference rules (or by use in general for that matter), and what, precisely, is being determined. We need to give content to notions such as 'meaning-conferring' or 'meaning-constitutive'. In what follows we will investigate one avenue for treating these issues, broadly speaking an account of the *semantic role of proof-conditions*.

Failing to uphold tradition, we will couple PTS with a blatant reification of meaning in the form of *truth-conditions*. The guiding idea is that inference rules carve out the *semantic content* of logical constants. Inferentially speaking, it is a matter of reading off truth-conditions from an inferential practice. But this is not much better than a metaphor. To make the idea precise in terms of PTS, we will investigate the relationship between truth-values and valuations on one side and proof-theoretic inference rules on the other. It will become apparent that there is a sense in which the rules *induce* sets of valuations, which in turn specify truth-conditions for logical constants.[2]

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[1] The old slogan is riddled with problems. Which use qualifies? Both past and present use, possible use, unsystematic use?

[2] A complication here is that in the formal exercise, *truth-condition* is, as we discussed earlier, more precisely put as *truth-in-a-model-condition* or even *designated-value-in-a-model-condition*. We will not discuss here to what extent such generalised truth-conditional formalism can be ascribed semantic significance. (See Section 6.4.3 for more on designated values and many-valued logics.)

But if meanings are truth-conditions (or perhaps truth-conditions plus something else), then what is the role of proof-conditions, and why is it that we are still abiding the spirit of the inferentialist programme? Even if we concede that meanings are truth-conditions, and inference rules merely determine this meaning, the role played by the inferential practice might be significant. For, although we may not want to call the rules meaning-constitutive, in the sense of determining *semantic value*, we can follow Hodes ([2004]) in calling the rules *sense-constitutive*. If the inference rules not only determine semantic value, but also allow us to *grasp the sense* of a logical constant, their role is intrinsic for *concept-aquisition* and *-possession*.[3]

Exploring this line of thought is an attempt at partly answering Dummett's worries about *manifestation* and *aquisition* while not setting semantics adrift from truth-conditions.[4] It locates questions about the epistemology of meaning (understanding) in the realm of PTS, without downplaying the role of a truth-conditional semantics. We are then required to take on a question of another sort of semantic shortcoming, one not covered by the notion of proof-theoretic harmony.

Recall Section [2.2.2] where we introduced some early attempts at diagnosing the problem with Prior's connective `tonk`. Philosophers like Stevenson and Wagner suggested that the problem was best looked upon as a truth-conditional failure on the part of `tonk`. Since there is no *truth-function* underpinning the connective, it is semantically dysfunctional. Yet, neither of them managed to make this claim sufficiently precise or sufficiently general. A diagnosis of `tonk`, as we discussed in the chapters on proof-theoretic harmony, ought to tell us something general about when inference rules fail to confer meaning. We will return to this analysis of `tonk` later in the chapter, and investigate under what circumstances the connective fails for reasons having to do with truth-functionality.

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[3]Recall the epistemological pretensions of PTS mentioned in Chapter [1]. Note, however, that Dummett ([1978b], ch. 1, see especially *Postscript*) is not hostile to the notion of truth-conditions, but that it is understood *intuitionistically*.

[4]See especially Dummett ([1973b]).

There is, however, a more principal task that must be undertaken. We will see that whether or not inference rules succeed in determining truth-conditions relies heavily on both the proof-theoretic framework and the model-theoretic details of the semantic values. In fact, under standard circumstances there is a threat of *underdetermination*: The inference rules only partially carve out the semantic content of the logical constant in question. In general, when do *proof-conditions* yield (appropriate) truth-conditions? The challenge, which derives from a technical observation in Carnap (1943), has been called the *categoricity problem.*[5]

6.2 Logics, Valuations, and Absoluteness

6.2.1 Preliminaries

To outline the technical problem we will follow the terminology of Dunn & Hardegree (2001) and Hardegree (2005). Let us first introduce the basic formal framework of valuations, valuation spaces, arguments and logics. We will then go on to discuss the determination relation between these two levels.

Definition 6.1 (Valuations)**.** Let *WFF* be the well-formed formulae over a language $\mathcal{L}$, and let $\mathbf{B} = \{1, 0\}$ be the Boolean truth-values. A *valuation* is a map $v\colon WFF \to \mathbf{B}$. Call the set of all valuations the *universe of valuations* $\mathcal{U}$. Then a *valuation space* is a $V \subseteq \mathcal{U}$.

One valuation space is normally taken to be privileged for classical logic (**CPL**), the set of *admissible valuations*, V_{CPL}. The valuation space V_{CPL} is constructed by taking a set of *assignments* from the atomic *WFF*s to truth-values, $v_0\colon WFF_0 \to \mathbf{B}$, and inducing the valuations $v \in V_{CPL}$ by the *truth-conditional clauses* for the connectives. E.g.,

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[5]That this problem was connected with (INF) was first suggested by Rumfitt (1997) and later by Raatikainen (2008).

($\neg$) $v(\neg A) = 1$ iff $v(A) = 0$

There is nothing in Def. 6.1, however, that precludes valuations that are *inadmissible*—i.e., that yield a truth value distribution that contradicts the clauses—from being a member of a valuation space. For example, since the truth-conditional clause for conjunction in **CPL** is $v(A \wedge B) = 1$ iff $v(A) = 1$ and $v(B) = 1$, any valuation such that $v(A \wedge B) = 1$ but, say, $v(A) = 0$, is inadmissible. Nevertheless, this is a valuation; it is just the sort of valuation that rules out the standard truth-conditional clause for conjunction. Put differently, it is a valuation that the (classical) inferentialist wants to rule out by proof-theoretic means.

Correspondingly, we introduce a general notion of a logic over a formal language.

Definition 6.2 (SET-FRML Logics)**.** Given a language $\mathcal{L}$ and a set of *WFF*s over $\mathcal{L}$, an *argument* is a pair $\langle \Gamma, A \rangle$ such that $\Gamma \subseteq$ *WFF* and $A \in$ *WFF*. A SET-FRML *logic* is any collection L of arguments (on WFF). If $\langle \Gamma, A \rangle \in L$ we say that $\langle \Gamma, A \rangle$ is *L-valid*, and we write $\Gamma \Vdash_L A$.[6]

Throughout we use the notation SET-FRML for single conclusion (but multiple-premise) logics and SET-SET for multiple conclusion logics. Notice in particular that the latter allows an *empty* conclusion-set, whereas the former requires any argument to have a conclusion (and only one conclusion). Of course, both frameworks might have empty premise-set. Similarly, FRML-SET is single-premise but multiple-conclusion. Other generalisations follow a similar pattern. It is also crucial that even if these frameworks anticipate proof-theoretic formalisms like sequent calculus, there is so far no constraint saying that the sets must be finite.[7]

Further, the notion of a *logic* used here is minimal in the sense that no further requirements are put on the properties of the $\Vdash$-relation, e.g., *reflexivity*, *transitivity*, or *substitution* (all of which have been proposed as essential features of

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[6]Note that this definition corresponds to the weak notion of a *consequence relation* introduced in Section 2.2.3.

[7]We return to this issue below in Section 6.4.1.

a consequence relation, see e.g. Avron 1994.) We here distinguish (i) L-validity ($\Vdash_L$) from (ii) the model-theoretic consequence relation ($\vDash$), and (iii) provability relations relative to a system S ($\vdash_S$). In other words, a logic in the sense of Def. 6.2 is not a proof-theoretic entity. The connection between $\Vdash$ and different $\vdash$ is a further question we will explore below.

We then need a couple of standard definitions about the relations between a valuation and a formula (or an argument).

Definition 6.3. For a valuation v, $\Gamma \subseteq WFF$, $A \in WFF$:

- *v verifies* A iff $v(A) = 1$ (Γ iff $v(B) = 1$ for each $B \in \Gamma$);

- *v falsifies* A iff $v(A) = 0$;

- *v refutes* an argument $\langle \Gamma, A \rangle$ iff v verifies Γ but falsifies A;

- *v confirms* $\langle \Gamma, A \rangle$ iff v does not refute $\langle \Gamma, A \rangle$.

6.2.2 The Go-Between

With the basics in place we can focus on the central issue: Inducing a logic from a valution and inducing a valuation from a logic. Obviously, by taking a valuation-space we can induce a logic (in the above sense) by taking all the model-theoretically valid arguments as L-valid. Put differently, if there is a valuation v that refutes an argument $\langle \Gamma, A \rangle$, then the argument is not L-valid.

Definition 6.4 (V-validity)**.** For a valuation space $V \subseteq \mathcal{U}$:

- an argument is *V-valid* iff there is no $v \in V$ that refutes it;

- $\mathbb{L}(V) =_{df} \{\langle \Gamma, A \rangle : \langle \Gamma, A \rangle$ is V-valid$\}$

Similarly, we take a logic L and we induce a valuation-space by excluding valuations that refute any of the arguments in L.

Definition 6.5 (L-consistency). Let L be a logic, v a valuation:

- v is *L-consistent* iff v confirms every argument in L;

- $\mathbb{V}(L) =_{df} \{v : v \text{ is } L\text{-consistent}\}$.

The two new functions $\mathbb{V}$ and $\mathbb{L}$ allow us to study the so-called categoricity problem in an abstract way. Specifically, there is now a precise way in which arguments (and thus, as we shall see, inference rules) carve out semantic content by informing us of which valuations are permissible.[8] For **CPL**, for example, the desideratum is a class of arguments that makes it the case that for any valuation v in the valuation space, $v(\neg A) = 1$ iff $v(A) = 0$. If the logic fails to rule out any valuation that does not deliver this result, the upshot is that inferences fail to determine the expected meaning for the negation.

More generally, the idea is to investigate the interplay between the two functions $\mathbb{L}$ and $\mathbb{V}$, given different valuation spaces and different logics. As opposed to the framework investigated in Hardegree (2005), however, we need look beyond classical logic and Boolean valuations, and, similarly, beyond SET-FRML and SET-SET frameworks for logics. But, before we start looking at variations it is worth pointing out some general facts about the functions. An abstract look reveals some useful properties.

6.2.3   Galois connections

A core idea in Hardegree (2005) is the idea of using Galois connections to study the relationship between valution spaces and logics. We will briefly look at the definition and show why it is that it fits our two functions.

Definition 6.6 (Galois connection). Let $\langle S, \leq \rangle$, $\langle T, \leq \rangle$ be posets (i.e., reflexive, transitive, anti-symmetric). Then, we say that a pair of functions $\langle f_1, f_2 \rangle$ such

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[8]We avoid the term 'admissible' since it has a standard use as explained above.

that $f_1 \colon S \to T$ and $f_2 \colon T \to S$ is a *Galois connection* if it satisfies the following conditions:

(1) if $x \leq y$ then $f_1(x) \geq f_1(y)$

    if $x \leq y$ then $f_2(x) \geq f_2(y)$

(2) $x \leq f_1(f_2(x))$

    $x \leq f_2(f_1(x))$

Note that if $\langle f_1, f_2 \rangle$ is a Galois connection, then the maps $S \to f_2(f_1(S))$ and $T \to f_1(f_2(T))$ are *closure operators*.[9]

Lemma 6.1. *The pair* $\langle V \to \mathbb{L}(V), L \to \mathbb{V}(L) \rangle$ *is a Galois connection:*

(1) If $\mathrm{L}_1 \subseteq \mathrm{L}_2$, *then* $\mathbb{V}(\mathrm{L}_2) \subseteq \mathbb{V}(\mathrm{L}_1)$

    If $\mathrm{V}_1 \subseteq \mathrm{V}_2$, *then* $\mathbb{L}(\mathrm{V}_2) \subseteq \mathbb{L}(\mathrm{V}_1)$

(2) $L \subseteq \mathbb{L}(\mathbb{V}(L))$

    $V \subseteq \mathbb{V}(\mathbb{L}(V))$

Proof. We only show selected cases. (1a) Assume $L_1 \subseteq L_2$ and that $v_0 \in \mathbb{V}(L_2)$. Since v_0 is L_2-consistent, it confirms every argument in L_2. But, then v_0 confirms all arguments in L_1 as well, so v_0 is L_1-consistent.

(2b) Assume that $v_0 \in V$. For reductio, let $v_0 \notin \mathbb{V}(\mathbb{L}(V))$. Then v_0 is not $\mathbb{L}(V)$-consistent. Thus, there is an argument $\langle \Gamma, A \rangle \in \mathbb{L}(V))$ that v_0 refutes. But, all $\mathbb{L}(V)$ are V-valid, i.e., there is no valuation $v \in V$ that refutes it. Contradiction.

$\square$

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[9] A function cl is a closure operator on a set S if for all $X, Y \subseteq S$:

- $X \subseteq cl(X)$;
- $X \subseteq Y$ implies $cl(X) \subseteq cl(Y)$;
- $cl(cl(X))$ implies $cl(X)$.

The conditions are related to structural properties of provability relations and consequence operators.

6.2.4   Absoluteness

By Lem. (6.1) we already know that for any valuation space, if we first induce a logic, and subsequently a valuation space from the resulting logic, then the induced valuation space will contain all the valuations of the original valuation space. Correspondingly, the same result holds for arguments and logics. However, there is no guarantee that the induced set will be identical with the original, i.e., that the converse result $V \supseteq \mathbb{V}(\mathbb{L}(V))$ holds. For Galois connections in general this property is known as (Galois) *completeness*. For logics and valuations we will follow Hardegree (2005) and call it *absoluteness*.

Definition 6.7 (Absoluteness)**.** Let L be a logic, V a valuation space:

- If $L = \mathbb{L}(\mathbb{V}(L))$, say that L is *absolute*;

- If $V = \mathbb{V}(\mathbb{L}(V))$, say that V is *absolute*.

Why care about absoluteness? Roughly put, without absoluteness there is no guarantee that a logic which is sound and complete with respect to a particular valuation space V_1 is not also sound and complete with respect to some distinct but undesirable valuation space V_2. In particular, this might be a valuation space including valuations not in V_1, and which potentially ruin the target truth-conditions. Presumably, it is because this is reminiscent of non-standard models for arithmetic that Shoesmith & Smiley (1978) later chose to talk about categoricity, and made the connection more rigid by defining categoricity with respect to different semantic *matrices*. More about this below in Section 6.4.3.

For the case of L-absoluteness we only state a general result of Hardegree's:

Theorem 6.2 (L-absoluteness, Hardegree (2005))**.** *A logic* L *is absolute iff* $\Vdash_L$ *is reflexive and transitive, i.e.*

- *If $B \in \Gamma$ then $\Gamma \Vdash_L B$;*

- *If $\Gamma \Vdash_L \forall \Delta$ and $\Delta \Vdash_L B$, then $\Gamma \Vdash_L B$, where $\Gamma \Vdash_L \forall \Delta =_{df} \forall C \in \Delta$, $\Gamma \Vdash_L C$.*

With this we leave behind the notion of L-absoluteness. In what follows we will primarily be concerned with V-absoluteness. The question of V-absoluteness is closely related to the question of categoricity originally raised in Carnap (1943) and developed in Shoesmith & Smiley (1978), Smiley (1996), and Rumfitt (1997). Informally, we can say that some logics give rise to *non-standard valuations*. Of course, for there to be a non-standard valuation, there has to be some background expectation as to what the valuations ought to look like. We might consider the admissible valuations for **CPL** as the *target* valuation space. For the inferentialist, the task is to provide a logic (and, in turn, a proof system) to which the target valuation space is absolute. What is surprising is that even in a mundane case like classical logic, this is a non-trivial result. In fact, Carnap's observation was precisely that in a standard SET-FRML framework classical logic is non-categorical. More precisely, the target valuation space is not absolute.

Interestingly, we can compare this to Belnap's notions of *existence* and *uniqueness* (see Section 2.2.3). On the present framework some inference rules might simply fail to induce a valuation space which specifies a truth-function for the connective in question—indeed, this is precisely the problem with `tonk` in a Boolean setting. In contrast, the question of absoluteness is about the uniqueness of the target valuation space. We can then put our worry with **CPL** as one of uniqueness for the semantics.

To see this, we need only turn to a concrete counter-example. We introduce a further definition for brevity.

Definition 6.8 (V-consistency)**.** Let V be a valuation space, and v a valuation. v is *V-consistent* iff v confirms every V-valid argument.

Lemma 6.3. *A valuation v is V-consistent iff $v \in \mathbb{V}(\mathbb{L}(V))$.*

Proof. By Def. 6.4, 6.5, $\mathbb{L}(V)$ consists of all V-valid arguments; $\mathbb{V}(L)$ consists of all valuations that confirm every argument in L. Thus, $\mathbb{V}(\mathbb{L}(V))$ consists of all valuations that confirm every V-valid argument. $\square$

Lemma 6.4. *V is absolute iff whenever v is V-consistent, $v \in V$.*

Proof. By the fact that the maps form a Galois connection and Lem. 6.3. $\square$

We can then give a simple counter-example showing that the admissible valuations for **CPL** form a non-absolute valuation space.

Example 6.1 (**CPL** SET-FRML). Let V_{CPL} be the admissible valuations of **CPL**. V_{CPL} is not absolute.

Proof. Set a valuation v^* such that for every $A \in$ *WFF*, $v^*(A) = 1$. Obviously, $v^* \notin V_{CPL}$. Yet, v^* is V_{CPL}-consistent since it confirms every **CPL**-argument (indeed, every argument). Thus, by Lem. 6.4, V_{CPL} is not absolute with respect to SET-FRML. $\square$

More intuitively, we can put it as follows: We know that **CPL** is sound and complete with respect to V_{CPL}. But, adding v^* will not change this fact. For, v^* cannot provide any new counter-models, so anything that was valid before is still valid; and, v^* leaves all other counter-models intact, so anything previously invalid is still invalid.

In other words, v^* is an example of what Carnap (1943) called a non-standard interpretation. Note that it clearly violates the standard truth-conditions for classical connectives, e.g., since there is now a valuation where a formula and its negation are both true, neither *contradictoriness* nor *contrariness* can be expressed with the negation.[10] Again, since **CPL**-arguments are sound and complete with respect to both V_{CPL} and $V_{CPL} \cup \{v^*\}$, the logic fails to uniquely pick out a semantics for the logical constants.

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[10]This was the primary concern of Smiley (1978).

What about $v^@$ such that $v^@(A) = 0$, for every $A \in WFF$? Granted that there are tautologies around (as there are in classical logic), we can rule out the valuation that takes everything to false. Clearly, if the logic L contains an argument of the form $<\emptyset, A>$, $v^@$ will not be L-consistent. Hence, as was pointed out to me by Elia Zardini, there is a related valuation $v^\top$ such that $v^@(A) = 0$, for every $A \in WFF$, except if $\Vdash A$. Such a valuation, although more involved, would do the same trick as v^*.

As a result of Carnap's observation about non-standard valuations, it has been concluded—by Raatikainen (2008) amongst others—that there is an underdetermination problem: The logic does not suffice to determine the semantic content of the involved logical connectives. The presence of non-standard valuations spells trouble for the inferentialist who takes meaning-determination to involve carving out truth-conditions. Nevertheless, the moral is not necessarily that the inferentialist must give up on meaning-fixing as truth-condition-fixing. Rather, the inferentialist should explore the structural resources in the logics employed in the proof-theoretic semantics. In the end, by increasing the expressive power of the structural language, absoluteness can be achieved. To do this, we must revise the proof-theoretic framework.

6.2.5   Multiple-Conclusion and Absoluteness

In fact, it turns out that the above lack of absoluteness can be remedied in several ways. It is well-known that when we move from single-conclusion to multiple-conclusion (SET-FRML to SET-SET) the result is an absolute logic (for the Boolean valuations). Thus, not only can we save classical logic from the unfortunate fate bestowed upon it by single-conclusion, we can prove that for any Boolean valuation space, we can find a logic that yields absoluteness.[11]

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[11]Shoesmith & Smiley (1996, p. 4): "Indeed one would have to conclude that classical logicans, like so many Monsieur Jourdanis, have been speaking multiple conclusion all their lives without knowing it."

In order to provide this result let us briefly reset some of the definitions in an appropriate way.

Definition 6.9 (SET-SET Logics)**.** Given a language $\mathcal{L}$ and a set of *WFF*s over $\mathcal{L}$, a *multiple-conclusion argument* is a pair $\langle \Gamma, \Delta \rangle$ such that $\Gamma, \Delta \subseteq$ *WFF*. A SET-SET *logic* is any collection L^m of arguments (on WFF). If $\langle \Gamma, \Delta \rangle \in L^m$ we say that $\langle \Gamma, \Delta \rangle$ is L^m-*valid*, $\Gamma \Vdash \Delta$.

Definition 6.10. For a valuation v and $\Gamma, \Delta \subseteq$ *WFF*:

- v *refutes* an argument $\langle \Gamma, \Delta \rangle$ iff v verifies Γ but falsifies B for each $B \in \Delta$;

- v *confirms* $\langle \Gamma, \Delta \rangle$ iff v does not refute $\langle \Gamma, \Delta \rangle$.

Definition 6.11. $\mathbb{L}^m(V) =_{df} \{\langle \Gamma, \Delta \rangle : \langle \Gamma, \Delta \rangle$ is V-valid$\}$

With these definitions in mind, we can now give a general proof of absoluteness for any valuation space in the universe of valuations. Note also that Lem. (6.3) and Lem. (6.4) both carry over to SET-SET (with $\mathbb{L}^m$).

Theorem 6.5 (SET-SET, Dunn & Hardegree (2001))**.** *For any valuation space* $V \subseteq \mathcal{U}$, $V = \mathbb{V}(\mathbb{L}^m(V))$.

Proof. We only need to prove one direction; the other direction follows from the fact that the we are dealing with a Galois connection. The proof proceeds by contraposition. Suppose that $v_0 \notin V$. We show that $v_0 \notin \mathbb{V}(\mathbb{L}^m(V))$. Define two sets T, F as follows:

- $T = \{A \in$ *WFF* $: v_0(A) = 1\}$

- $F = \{A \in$ *WFF* $: v_0(A) = 0\}$

For each $v \neq v_0$, either $v(B) = 0$ for some $B \in T$ or $v(B) = 1$ for some $B \in F$ (from the definition of a valuation). Thus, v confirms the argument $T \Vdash F$. It follows

that the argument is V-valid (since it is confirmed by every valuation except v_0). Since v_0 does not confirm the argument, however, v_0 is not V-consistent. So, by Lem. 6.4, $v_0 \notin \mathbb{V}(\mathbb{L}^m(V))$. $\square$

As an immediate corollary the SET-SET framework yields an absoluteness result for the set of **CPL** admissible valuations, V_{CPL}. Note that the proof will not go through on SET-FRML as the argument in question must include cases where the cardinality of the conclusion set (the succedent) is 0 or strictly greater than 1. For instance, the argument $<\{A, \neg A\}, \emptyset>$ rules out the non-standard valuation v^*: The argument is *refuted* by v^* since the premises are verified (by the definition of the valuation) and every conclusion is falsified (there is no conclusion). Thus, v^* cannot be L^m-consistent for the relevant SET-SET logic L^m for classical logic.

For similar reasons the less common framework FRML-SET is not absolute, i.e., multiple-conclusion but single-premise. Observe a valuation v_* such that for every $A \in \mathit{WFF}$, $v_*(A) = 0$. This is simply the dual of the original problem with SET-FRML. (There is also a dual of $v^\top$, call it $v^\perp$, that takes everything to true, except contradictions.) Even more obvious, frameworks of the type $\emptyset$-SET, e.g., Gentzen-Schütte sequent calculus, where all the action is on the right-hand (succedent) side. Obvious that is, because Gentzen-Schütte systems work by mimicking the left-hand side by negated formulae on the right-hand side. Importantly, $\emptyset$-SET frameworks function with an Identity axiom $\vdash \Gamma, P, \neg P$. Specifically, such logics can only rule out limit valuations which take everything to false, i.e., $v^@$.

What we have seen, then, is that the inferentialist who accepts multiple-conclusion has more leverage than the single-conclusion counterpart. Perhaps this is a welcome result for some sequent-calculus aficionados, but for those who have expressed concerns about the interpretation of multiple conclusion (see Section 3.3), such a move is not available. Rather, they must look elsewhere for a generalisation of the single-conclusion framework that yields absoluteness. As it happens, this is

precisely one of the advantages of the bilateral framework where assertion and denial are primitive signs in the structural language (see Section 3.2.3).

6.2.6   Bilateralism

This way of proving absoluteness for **CPL** was explored in Smiley (1996).[12] To rehearse, the idea is, broadly speaking, to introduce assertion and denial as primitive signs in the logical framework. With $+$ and $-$ we indicate that a formula is asserted and denied respectively. As pointed out in Section 3.3, multiple-conclusion can then be interpreted along the lines of an argument being valid if one cannot (rationally) assert all the premises and deny all the conclusions.

Again, we reset the preliminary definitions in order to prove a general result of absoluteness.

Definition 6.12 (SET-FRML$^+$ Logics)**.** Given a language $\mathcal{L}$ and a set of *WFF*s over $\mathcal{L}$, an *argument* is a pair $\langle \Gamma,\, A \rangle^+$ such that $\Gamma \subseteq \mathit{WFF}^+$ and $A \in \mathit{WFF}^+$, where WFF^+ is the set of well-formed formulae signed with an assertion or denial sign (e.g., $+B$, $-B$). A SET-FRML$^+$ *logic* is any collection L^+ of arguments. If $\langle \Gamma, A \rangle^+ \in L^+$ we say that $\langle \Gamma, A \rangle^+$ is L^+-*valid*, $\Gamma \Vdash^+ A$.

By introducing signed formulae we also need to reinterpret other properties of the logic. First, assertion and denial as pragmatic roles (speech acts) are thought of as either *correct* or *incorrect*. Without invoking too fine-grained considerations about the norms of assertion, we will here simply assume a naïve relationship between truth-values and correctness.[13] Of course, the point is not that the below clauses somehow exhaust the concepts of assertion and denial. Rather, these connections are all we need for the present purposes of defining a logical framework

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[12]Rumfitt (1997) is an attempt at expanding Smiley's work to (strong) Kleene logic. Even if Rumfitt's aim is orthogonal to the more general project outlined here, his discussion of the possibility of introducing several notions of denial has heavily influenced the present work.

[13]It is a complicating factor that norms of speech acts are also often argued to be tied up with doxastic or epistemic states.

that provides absoluteness. Later we will investigate further how more fine-grained notions can be implemented for similar purposes (see Section 6.4).[14]

Hence, we give the following connection between correctness and verfication:

Definition 6.13 (Correctness)**.** For $A \in$ *WFF*:

- $+A$ is *correct* with respect to v iff $v(A) = 1$;

- $-A$ is *correct* with respect to v iff $v(A) = 0$.

Furthermore, there is now a superstructure on the arguments: over and above *truth-preservation* (over valuations) there is *correctness-preservation* (over valuations together with Def. 6.13). We make this idea precise by calibrating the definition of refutation and confirmation.[15]

Definition 6.14. A valuation v *refutes* an argument $\langle \Gamma, A \rangle^+$ iff B is correct for each $B \in \Gamma$ and A is incorrect. Otherwise v *confirms* the argument.

Definition 6.15. $\mathbb{L}^+(V) =_{df} \{\langle \Gamma, A \rangle^+ : \langle \Gamma, A \rangle^+ \text{ is V-valid}\}$

We now have the means to prove a result analogous to the SET-SET Thm. (6.5) above. The result was originally proved in Smiley (1996); we merely modify the proof strategy to fit that used by Dunn & Hardegree. For reasons of uniformity, we use a slightly different terminology.

Theorem 6.6 (SET-FRML$^+$, Smiley (1996))**.** *For any valuation space* $V \subseteq \mathcal{U}$, $V = \mathbb{V}(\mathbb{L}^+(V))$.

Proof. Again, let $v_0 \notin V$. Define the sets T, F as before:

- $T = \{A \in \textit{WFF} : v_0(A) = 1\}$

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[14]See also Rumfitt (1997) for a further discussion of the logical nature assertion and denial.

[15]One consideration that will be left undiscussed here is whether there are revenge issues at the level of *correctness-valuations*. For an argument to this effect, see Murzi & Hjortland (2009). For an insightful criticism, see Incurvati & Smith (2009).

- $F = \{A \in \mathit{WFF} : v_0(A) = 0\}$

There are three cases: (i) $F = \emptyset$; (ii) F is a singleton; and (iii) F is non-empty, non-singleton. We show the first case and indicate the structure of the other three.

For (i), take the argument $\langle +(T/\mathrm{Q}), -Q \rangle$, where $+(T/\mathrm{Q})$ is the set of $+$-signed formulae in T except Q. For each $v \neq v_0$, there will be some $B \in \mathit{WFF}$ such that $v(B) = 0$. Thus, either v verifies Q or it falsifies some $B \in T/\mathrm{Q}$. That is, either some $B^+ \in +(T/\mathrm{Q})$ is incorrect or $-Q$ is correct. Either way, v confirms $+(T/\mathrm{Q}) \Vdash^+ -Q$. So, the argument is V-valid, and thus in $\mathbb{L}^+(V)$. Yet, as is evident, v_0 does not confirm $\langle +(T/\mathrm{Q}), -Q \rangle$, hence v_0 is not $\mathbb{L}^+(V)$-consistent. In other words, $v_0 \notin \mathbb{V}(\mathbb{L}^+(V))$.

Case (ii). Take the argument $\langle +T, +Q \rangle$ where $F = \{Q\}$. For each, $v \neq v_0$, v confirms the argument, but v_0 refutes it.

Case (iii). Take the argument $\langle +T \cup \{-A_0, -A_1, ...\}, +Q \rangle$, where $F = \{Q, A_0, A_1, ...\}$. □

We can now see that the details in the proof simply implements the insight about how the SET-FRML$^+$ framework corresponds to the SET-SET framework that we outlined in Section 3.3. By applying signs to the formulae in T and F we force the succedent to have a cardinality of 1. Yet, rejecting on the antecedent side is merely a circumlocution for multiple-conclusion (or the other way around, as the case may be).

Let us try the example of v^* again. We cannot take the conclusion side as the empty-set as in SET-SET, but we can shift one of the premises over to the conclusion side by reversing the sign. So, instead of $A, \neg A \Vdash \emptyset$, we have $+A \Vdash -\neg A$ or $+\neg A \Vdash -A$. Since both these sequents are classically provable, our non-standard valuation is ruled out.

The insight this affords is that the issue of absoluteness comes down to having sufficient "locations" in the structural language of the logic: Right- and left-hand side in SET-SET, or right- and left-hand side with signs in SET-FRML$^+$. What is required of the structural language is for there to be sufficient distinctions in the sequents (or rules) to yield the full *sentential output* of the semantics, i.e., a partition of valuations as appropriate or inappropriate. The aim is for this partition to fit admissible valuations, i.e., to yield the target output valuation space. Analogously, we can infer from this that the one-sided systems (Gentzen-Schütte) cannot be sufficient for the absoluteness of **CPL**. There is, in a word, no location for the undesignated value 0, i.e., left-hand side. For now, it is not the intention to give any formal characterization of what a location in a logic is, but it might enhance the intuitive understanding of the situation. Soon we will try to make the idea more precise by looking at generalisations of the valuation-spaces and corresponding n-sided frameworks (see Section 6.4).

6.3   Many-valued logics

6.3.1   Non-Boolean Valuation Spaces

So far we have only varied the framework of the arguments and logic. But what happens when we modify the framework of the valuation spaces, e.g., we introduce non-Boolean values and, perhaps, more than one designated value? Needless to say, it would be a disappointment if the inferentialist could do no better than provide absoluteness for the Boolean universe of valuations. The literature on the topic has been more or less exclusively focusing on the Boolean universe (perhaps with the exception of Rumfitt 1997), and as far as I know, no one has considered cases with more than one designated value. As it turns out, such a generalisation forces on us more sophisticated frameworks for logics in order to preserve absoluteness.

In the spirit of the above remarks, it is natural to expect that an increase in the so-called locations will be an increase in the expressive power of the structural language. With further distinctions in the logics, we ought to be able to make finer discriminations between values and designation in the sentential output.

Definition 6.16 (Valuations Generalised)**.** Let WFF be the well-formed formulae over a language $\mathcal{L}$, and let $\mathbf{V}$ be the set of truth-values. A *valuation* is a map v: $WFF \rightarrow \mathbf{V}$. Let $\mathcal{D} \subseteq \mathbf{V}$ be the set of *designated values.*

Along these lines we must generalise the concepts of falsification/verification and refutation/confirmation so as to take into account that there might be multiple designated values. In general, we get the following definitions:

Definition 6.17. For a valuation v, and $A \in WFF$, $\Gamma \subseteq WFF$:

- *v verifies A* iff $v(A) \in \mathcal{D}$ (Γ iff $v(B) \in \mathcal{D}$ for each $B \in \Gamma$);

- *v falsifies A* iff $v(A) \notin \mathcal{D}$;

- *v refutes* an argument $\langle \Gamma, A \rangle$ iff v verifies Γ but falsifies A;

- *v confirms* $\langle \Gamma, A \rangle$ iff v does not refute $\langle \Gamma, A \rangle$.

Similarly, the generalisation extends to the SET-SET framework:

Definition 6.18. For $\Gamma, \Delta \subseteq WFF$:

- *v refutes* an argument $\langle \Gamma, \Delta \rangle$ iff v verifies Γ but falsifies B for each $B \in \Delta$;

- *v confirms* $\langle \Gamma, \Delta \rangle$ iff v does not refute $\langle \Gamma, \Delta \rangle$.

Of course, the above is merely a framework for a range of many-valued logics. In order to study the particular problem arising from absoluteness of many-valued logic we need to first look at a particular case.[16]

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[16]For comprehensive material on many-valued logics, see Rescher (1969) and Priest (2008)

6.3.2   FDE and Absoluteness

As a case study, let us pick a generalisation of classical logic, namely *First-Degree Entailment*, **FDE**.[17] **FDE** takes the powerset of values from **CPL**, for brevity we call them 1, 0, *b* (both), *n* (neither). Following a typical interpretation, we can say that the logic is both *gappy* and *glutty* (or *paracomplete* and *paraconsistent*).[18] The set of designated values is now any member of the powerset with the original designated value *1*, so *b* is a designated value ($b = \{1, 0\}$).

We quickly introduce the truth-functions from which admissible **FDE** valuations are normally produced. Notice that if we ignore the columns and rows for *b* and *n*, we regain classicality. Consequently, we can already see that the task of proving absoluteness will have a finer fabric than in the Boolean universe.

Definition 6.19 (FDE). Let $\mathbf{V} = \{1, 0, \{1,0\}, \emptyset\}$ (for short we write *b*, *n* for the latter two), $\mathcal{D} = \{1, b\}$. Let the following induce the admissible valuations, V_{FDE} from assignments of values to atomics:

$f_\wedge$	1	*b*	*n*	0
1	1	*b*	*n*	0
b	*b*	*b*	0	0
n	*n*	0	*n*	0
0	0	0	0	0

$f_\vee$	1	*b*	*n*	0
1	1	1	1	1
b	1	*b*	1	*b*
n	1	1	*n*	*n*
0	1	*b*	*n*	0

$f_\neg$	
1	0
b	*b*
n	*n*
0	1

Our question then becomes whether or not we can give an absoluteness result for valuation spaces over the **FDE** (powerset) universe of valuations. In other words,

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[17]For details, see Priest (2008, ch. 8.4).

[18]There is a subtlety here we overlook. Some people talk about gaps as propositions that are neither true nor false, without thinking of them as having a third value, *neither*. This is a tricky difference, especially since the question of whether or not there is semantic value at all has ramifications for central semantic considerations about truth-bearers, or rather, semantic-value-bearers (e.g., what a proposition is). Similarly, one might wonder whether the glutty case has two distinct semantic values, or some third fusion of these.

how does the inferentialist expand the methods of Thms. 6.5 and 6.6 to carve out truth-conditions in an **FDE** landscape? Clearly, SET-FRML will not give absoluteness for **FDE**. This follows straightforwardly from the fact that it fails for **CPL**, and the **CPL**-admissible valuations live within the new universe of **FDE** valuations. But what about SET-SET? And what about SET-FRML$^+$?

Consider the valuation v^{**} such that $v^{**}(A) = $ b for each $A \in \textit{WFF}$. Obviously, this is just a variation of the non-standard valuation v^* from Example (6.1) above. The problem is that in SET-SET the 'characteristic' partition of formulae, $\langle \Gamma, \emptyset \rangle$, is the same as for v^*. More precisely, an argument rules out v^{**} iff it rules out v^*. But that is not good enough. For absoluteness, we might want to allow a valuation where everything is *both* without allowing the valuation where everything is (just) true. The SET-SET framework runs the two designated values together: Even if v^{**} refutes an argument based on the above partition, it is not the case that any valuation u such that $v^{**} \neq u$ confirms it. Just let u be such that $u(A) = 1$ for each $A \in \textit{WFF}$. Nor, as one ought to expect, will SET-FRML$^+$ provide absoluteness. As we will see below, both frameworks fail to provide locations that distinguish between the two designated values.

Where do we go from here? Let us propose the obvious fusion of the SET-SET and SET-FRML$^+$ frameworks. The idea is simply to gain new locations by adding both multiple-conclusion *and* signed formulae (assertion and denial). Granted, this moves excludes the possibility of *interpreting* multiple-conclusion as assertion and denial in disguise (or vice versa).[19] Later we will explore whether an alternative interpretation of the joint framework can be found. At least the advocate of multiple-conclusion *sui generis* still has the option of reading signed formulae as assertion and denial independent of the SET-SET structure.

To proceed, we yet again tweak the basic definitions to include further structure:

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[19]I am indebted to Aaron Cotnoir for pressing this point on me in discussion. See also Section 3.3 again.

Definition 6.20 (SET-SET$^+$ Logics)**.** Given a language $\mathcal{L}$ and a set of *WFF*s over $\mathcal{L}$, an *argument* is a pair $\langle \Gamma, \Delta \rangle^+$ such that $\Gamma, \Delta \subseteq WFF^+$, where WFF^+ is the set of well-formed formulae signed with an assertion or rejection sign (e.g., $+B, -B$). A SET-SET$^+$ *logic* is any collection L_m^+ of arguments. If $\langle \Gamma, \Delta \rangle^+ \in L_m^+$ we say that $\langle \Gamma, A \rangle^+$ is L_m^+-*valid*, $\Gamma \Vdash_m^+ \Delta$.

A vexed issue arises when we consider the interpretation of the signs with respect to correctness and incorrectness. Understood as pragmatic roles, it is not straightforward to give an intuitive reading of the interaction between $+$ and $-$ and the **FDE** values. Even if one thinks that the connection between norms of assertion/denial and truth/falsity is clear-cut (which it presumably is not), there is no getting around the fact that one is faced with difficult logical choices when detailing such a connection for gaps and gluts. For now, we merely stipulate the connection in a way that is dictated not by an analysis of the pragmatic roles in the contexts of gaps and gluts, but rather by the demands of the logics to guarantee absoluteness. The details of the connection are reminiscent of—but not the same as—the external/internal distincion for denial found in Rumfitt (1997).

One particular comment on the following definition is warranted. Note that it is at odds with the way that denial is understood by the dialetheists (cf. Priest 2006a, ch. 6). For Priest, it is vital for the solution to semantic paradoxes that one is correct in asserting everything that is true (including true things which are also false), but that, asymmetrically, one only denies things that are strictly false. In other words, the dialetheist resists an interpretation where denial can be interchanged with assertion of a negation.[20] In contrast, our definition will treat assertion and denial symmetrically, for no other reason than that it is what works with the proof strategy.

Definition 6.21 (Correctness)**.** For $A \in WFF$:

- $+A$ is *correct* with respect to v iff $v(A) \in \mathcal{D}$;

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[20]See also Humberstone (2000) for a further elaboration on rejection and negation.

- $-A$ is *correct* with respect to v iff $v(A) = 0$ or $v(A) = b$.

It is then a trivial matter to extend these choices into a definition for refutation and confirmation of arguments:

Definition 6.22. A valuation v *refutes* an argument $\langle \Gamma, \Delta \rangle^+$ iff A is correct for each $A \in \Gamma$ and B is incorrect for each $B \in \Delta$. Otherwise v *confirms* the argument.

Definition 6.23. $\mathbb{L}_m^+(V) =_{df} \{\langle \Gamma, \Delta \rangle^+ : \langle \Gamma, \Delta \rangle^+ \text{ is V-valid}\}$

All that remains is to follow the proof strategy from the earlier theorems, now incorporating the added structure. Although a bit more involved, the recipe is still essentially the same: Working by contraposition, we take an arbitrary valuation not in the valuation space, and produce sets corresponding to the values assigned to formulae. The trick is to construct a SET-SET$^+$ argument that is refuted by the valuation but confirmed by any other.

Theorem 6.7 (SET-SET$^+$). *For any valuation space* $V \subseteq \mathcal{U}$, $V = \mathbb{V}(\mathbb{L}_m^+(V))$.

Proof. Let $v_0 \notin V$. Define the sets T, F, B, N:

- $T = \{A \in \textit{WFF} : v_0(A) = 1\}$

- $F = \{A \in \textit{WFF} : v_0(A) = 0\}$

- $B = \{A \in \textit{WFF} : v_0(A) = b\}$

- $N = \{A \in \textit{WFF} : v_0(A) = n\}$

Take the argument $\langle \{+T, +B, -B, -F\}, \{+F, +N, -N, -T\} \rangle$. v_0 refutes the argument since the premises are all correct and the conclusions are all incorrect. However, for each $v \neq v_0$, v confirms the argument. To see this note that if $v \neq v_0$, there must be a formula A in at least one of T, F, B, N such that $v(A) \neq v_0(A)$.

(i) If $A \in T$, then $v(A) \neq 1$. If $v(A) = b$, then A would be correctly rejected in the succedent, so the argument is confirmed. If $v(A) = n$ it is incorrectly asserted in the antecedent. If $v(A) = 0$ it is again incorrectly asserted in the antecedent.

(ii) If $A \in F$, then $v(A) \neq 0$. If $v(A) = b$, then A is correctly asserted in the succedent. If $v(A) = n$, it is incorrectly rejected in the antecedent. If $v(A) = 1$, it is incorrectly rejected in the antecedent.

(iii) If $A \in B$, then $v(A) \neq b$. If $v(A) = 1$, A is incorrectly rejected in the antecedent. If $v(A) = 0$, then it is incorrectly asserted in the antecedent. If $v(A) = n$, then A is both incorrectly asserted and incorrectly rejected in the antecedent.

(iv) If $A \in N$, then $v(A) \neq n$. If $v(A) = 1$, then A is correctly asserted in the succedent. If $v(A) = 0$, then A is correctly rejected in the succedent. If $v(A) = b$, then A is correctly rejected and correctly asserted in the succedent.

So, the argument $\langle \{+T, +B, -B, -F\}, \{+F, +N, -N, -T\} \rangle$ is V-valid, and thus in $\mathbb{L}_m^+(V)$. Yet, as is evident, v_0 does not confirm the argument, hence v_0 is not $\mathbb{L}_m^+(V)$-consistent. In other words, $v_0 \notin \mathbb{V}(\mathbb{L}_m^+(V))$. $\qquad\square$

What we have is a general argument for any four-valued logic with two designated values. Under such a universe of valuations, any valuation-space can be proved absolute. As a corollary of Theorem (6.7), we know that the valuation space corresponding to the admissible valuations of **FDE** is absolute.

Furthermore, it is straightforward that the framework is also powerful enough to yield absoluteness for some related 3-valued logics (with either one or two designated values). Take for instance the admissible valuations of the logics **LP** (the Logic of Paradox) and **K3** (strong Kleene logic).[21] These logics are obtained

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[21]See Example 6.3 and 6.4 in Section 6.4.3 for details.

from **FDE** by adding one of two constraints: no gluts (i.e., ignore the value b) gives **K3**; no gaps (i.e., ignore the value n) gives **LP**. In other words, all other values remain the same as in the truth-tables from Def. 6.19. The proofs for these cases will proceed in a similar fashion, but ignoring the value which does not figure in the respective semantics.

6.4   Proof-Theoretic Generalisations

6.4.1   Infinitary Arguments

Undoubtedly, we could extend the strategy to deal with n-valued logics with higher number of designated values as well. But for the inferentialist, these result are of limited interest. Hitherto, arguments with possibly infinite premise- and conclusion-sets have been applied in order to prove that, for any valuation in a universe, there is an argument in the framework that would rule it out. However, that is the wrong currency for PTS: What we need are genuine proof-systems whose inference rules yield the correct valuation space. For, even if proof-systems induce a class of arguments, and the class of arguments might yield absoluteness with respect to the target valuation space, we have no reason so far to believe that for any class of arguments, there is an appropriate proof-system. After all, proof-systems—and their inference rules—are normally finite entities, whereas the arguments discussed above are infinitary.[22] More, the very proof strategy that the Theorems have in common relies on an infinitary argument being available. The reason is simply that the argument is defined over the class of well-formed formula, which is itself infinite.

The challenge, then, is to identify proof-theoretic systems that determine valuation spaces in the required sense. Not a lot of ingenuity is involved in seeing

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[22]We set aside issues about infinitary rules. Although there might be something to be said about the connection with absoluteness, it is not clear that it is relevant for proof-theoretic semantics.

the relationship between SET-SET and sequent calculi for classical logic, or SET-FRML$^+$ and a Smiley-Rumfitt-style signed natural deduction system. But, when we move into the realm of many-valued logics and absoluteness, the growing number of locations put pressure on standard proof-theoretic frameworks. Incidentally, many-valued logics often do not have proof-systems, and to the extent that they do, there is no systematic way of telling whether they are absolute with respect to the particular logic's admissible valuations.

6.4.2   Compactness

A natural thought is to investigate not only SET-SET frameworks and absoluteness, but FSET-FSET frameworks, where FSET indicates that the succedent and antecedent sets are finite (alternatively, FSET-FRML frameworks). In fact, one might suspect that there is a connection between absoluteness with respect to FSET-FSET logics and *compactness* of the valuation space in question. Such a connection has already been investigated by Peregrin (2008), but restricted to Boolean valuation spaces. His findings might serve as a guide to further generalisations.

Definition 6.24 (Compactness)**.** Let WFF be the well-formed formulae, $\Gamma \subseteq WFF$, and V a valuation space. Say that V is *compact* if whenever for every finite subset $\Gamma^{'} \subseteq \Gamma$ there is valuation $v \in V$ that is a model of $\Gamma^{'}$, then there is a valuation $v^{'} \in V$ that is a model of Γ.

Definition 6.25 (Finiteness)**.** Say that V has the *finiteness property* when $\Gamma \vDash \Delta$ iff there are finite subsets $\Gamma^{'} \subseteq \Gamma$, $\Delta^{'} \subseteq \Delta$ such that $\Gamma^{'} \vDash \Delta^{'}$.

It is well-known that the valuation space consisting of **CPL** admissible valuations is compact and has the finiteness property. In general, however, there is no guarantee that a Boolean valuation space has these properties. But, Peregrin has shown that there is a significant connection between the finiteness property

and absoluteness. Note that we do not stick to Peregrin's notation nor his proof strategy in what follows.[23] (For details on his approach, see ibid., p. 152-56.)

Theorem 6.8. *If a Boolean valuation space V has the finiteness property, then V is absolute with respect to an FSET-FSET logic.*

Proof. Assume that V has the finiteness property. We prove by contraposition that $\mathbb{V}(\mathbb{L}(V)) \subseteq V$. Assume that $v_0 \notin V$. Again, we define two sets T, F in the following way:

- T $= \{A \in WFF \mid v(A) = 1\}$

- F $= \{A \in WFF \mid v(A) = 0\}$

By definition, v_0 refutes the argument $\langle T, F \rangle$. Any $v \neq v_0$ confirms $\langle T, F \rangle$. So, in particular every $v \in V$ confirms $\langle T, F \rangle$, i.e., T $\vDash$ F. By Def. 6.25, there are finite subsets $\Gamma \subseteq$ T, $\Delta \subseteq$ F such that $\Gamma \vDash \Delta$. Obviously, every $v \in V$ confirms $\langle \Gamma, \Delta \rangle$, so $\langle \Gamma, \Delta \rangle \in \mathbb{L}(V)$. But, v_0 refutes $\langle \Gamma, \Delta \rangle$ since it refutes $\langle T, F \rangle$. Thus, $v_0 \notin \mathbb{V}(\mathbb{L}(V))$. $\square$

Summed up, Thm. 6.8 is little more than an extension of Thm. 6.5. No surprise, of course, given that it is all on the backdrop of the Boolean universe.

Peregrin continues by studying the connection between other frameworks and semantic properties. The result is a hierarchy of logical frameworks for the Boolean universe. We will not stop to do justice to the details here. It suffices to say that he makes related observations about the FSET-FRML and SET-SET frameworks.

One caveat about the above type of result is worth mentioning. Peregrin is also in the business of ensuring that absoluteness results can be transferred to genuinely proof-theoretic systems (rather than just logical frameworks of arguments). This

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[23]In particular, Peregrin has a different way of classifying logical frameworks. FSET-FSET is what he calls a PQI-system.

is why he wants a measure on precisely what can be done with finite arguments in a Boolean universe. However, he makes a pertinent remark: In addition to the question of what a framework of finite arguments like FSET-FSET can do with respect to absoluteness, there is a question of whether or not a finite set of such arguments is sufficient. Even if the valuation space has the finiteness property, the above result (6.8) does not ensure that there is a finite axiomatisation of the logic in question. For that we must dig deeper. (See the discussion in ibid., pp. 157-58.)

6.4.3 Matrix logics

Given what we have seen in Section 6.3 we have reason to suspect that Peregrin's observations about absoluteness for the Boolean universe have counterparts in the land of many-valued logics. Of course, we know that the frameworks like SET-SET and SET-FRML$^+$ are not sufficient for absoluteness, so their finite versions will not do either. Nonetheless, the question of finitary arguments and, ultimately, finite axiomatisations, still applies.

Fortunately, there are results that might point us in the right direction. Shoesmith & Smiley (1996) have discussed many-valued logics and compactness in some detail (see especially ch. 13). We start with some helpful definitions:

Definition 6.26 (Matrix)**.** A *matrix M* is defined by a structure $\langle \mathcal{V}, \mathcal{D}, \mathcal{F} \rangle$ where $\mathcal{V}$ is the (non-empty) set of truth-values, $\mathcal{D} \subseteq \mathcal{V}$ is the set of designated truth-values, and $\mathcal{F}$ is the set of truth-functions denoted by the logical connectives in the language.

Definitions of valuations, verification, refutation, etc., works as in Section 6.3.1 (see Def. 6.17, 6.18). An example of a matrix is **FDE** as defined in Section 6.19; another is the **CPL** with its obvious truth-tables.[24] Some further examples:

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[24] Standardly, classical logic is counted as a matrix-logic, albeit not a many-valued one in the strict sense.

Example 6.2. *Kleene matrix* **K3**: $\mathcal{V} = \{0, \frac{1}{2}, 1\}$, $\mathcal{D} = \{1\}$, and the truth-functions are given by the following truth-tables:[25]

$f_\wedge$	1	$\frac{1}{2}$	0
1	1	$\frac{1}{2}$	0
$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	0
0	0	0	0

$f_\vee$	1	$\frac{1}{2}$	0
1	1	1	1
$\frac{1}{2}$	1	$\frac{1}{2}$	$\frac{1}{2}$
0	1	$\frac{1}{2}$	0

$f_\to$	1	$\frac{1}{2}$	0
1	1	$\frac{1}{2}$	0
$\frac{1}{2}$	1	$\frac{1}{2}$	$\frac{1}{2}$
0	1	1	1

$f_\neg$	
1	0
$\frac{1}{2}$	$\frac{1}{2}$
0	1

Example 6.3. *3-valued Łukasiewicz matrix* **Ł₃**: $\mathcal{V} = \{0, \frac{1}{2}, 1\}$, $\mathcal{D} = \{1\}$, and the truth-functions are given by the same truth-tables as in Example 6.2 except that:[26]

$f_\to$	1	$\frac{1}{2}$	0
1	1	$\frac{1}{2}$	0
$\frac{1}{2}$	1	1	$\frac{1}{2}$
0	1	1	1

Example 6.4. *Logic of Paradox* **LP**: $\mathcal{V} = \{1, \frac{1}{2}, 0\}$, $\mathcal{D} = \{1, \frac{1}{2}\}$. Same basic truth-functions as in Example 6.2.

Example 6.5. n-*valued Łukasiewicz matrices* **Ł$_n$**: $\mathcal{V} = \{0, \frac{1}{n-1}, \frac{2}{n-1}, ..., 1\}$, $\mathcal{D} = 1$, and the basic truth-functions can be given for any n as x $\wedge$ y = min(x,y), x $\vee$ y = max(x,y), x $\to$ y = min(1, 1 − x + y), and ¬x = 1 − x.[27]

Example 6.6. *Infinite Łukasiewicz matrices*: Countably infinite Łukasiewicz logic has the set of rationals as truth-values; continuum valued Łukasiewicz logic has the set of reals as truth-values. The basic truth-function are the same as in Example 6.5.

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[25]See Kleene (1952a). The system is called *strong* Kleene logic. By letting every function be such that if there is an input $\frac{1}{2}$, the output is $\frac{1}{2}$, we get *weak* Kleene logic.

[26]See Łukasiewicz (1920, 1930).

[27]Note that Example 6.3 is a special case.

Definition 6.27. If a matrix M has a finite number of truth-values, we say that M is *finitely many valued.*

Shoesmith & Smiley prove an important result about this latter class of matrix logics:

Theorem 6.9 (Shoesmith & Smiley (1996))**.** *Every finitely many-valued matrix logic has the finiteness property.*[28]

6.4.4   Interlude: Broken Words Never Meant to Be Spoken

A closer look at matrix logics also reveals that they offer a route back to `tonk`. We return to the tradition in Section 2.2.2, in which Stevenson and Wagner offered reasons to think that `tonk` was *truth-functionally* suspect, rather than proof-theoretically suspect. We will see that, as with the traditional proof-theoretic diagnosis (Section 2.2.3), the misgivings must be sensitive to the type of matrices in question.[29] Less prosaically, there is more between heaven and earth than the Boolean universe.

Let us first see how `tonk` behaves with respect to a Boolean matrix. With the natural deduction rules we saw above in Section 2.2.2, what can we say about its Boolean truth-tables?

$$\frac{A}{A\,\mathtt{tonk}\,B}\,(\mathtt{tonk}-I) \qquad \frac{A\,\mathtt{tonk}\,B}{B}\,(\mathtt{tonk}-E)$$

As we should expect, there are no such tables. If we take $\mathbb{V}(L)$ of the collection of arguments formed by the above rules, we see that for any Boolean valuation, first, that if A is true then so is $A\,\mathtt{tonk}\,B$, and, second, if B is false, $A\,\mathtt{tonk}\,B$ is false. Thus, if both A and B are true, there is no functional output for $A\,\mathtt{tonk}\,B$. The

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[28]We do not include the proof. See ibid., pp. 251-52 for details.

[29]Of course, being truth-functionally suspect cannot be a general objection against logical constanthood. Quantifiers, modalities, and other uncontroversial cases are all non-truth-functional.

problem appears to be that `tonk` both over- and undergenerate with respect to Boolean valuations: No truth-functional content is carved out by Prior's suggested rules. If anything, it looks like a *prima facie* case for questioning the semantics of the expression.

$f_{\texttt{tonk}}$	1	0
1	1	$^1/_0$
0	?	0

But the death of truth-functional `tonk` is greatly exaggerated. Roy Cook (2005) suggested a many-valued matrix in which `tonk` can function non-trivially. The idea is to **FDE** from Def. 6.19 to produce a 4-valued matrix which induces a *non-transitive* consequence relation. Both `tonk`-rules can be made valid in the semantics, but without allowing the transitive step from the first to the second rule.

How can this be done? Keeping the matrix identical to **FDE**, we can still investigate other options for altering the logic by varying the corresponding notions of validity. Such variation can take advantage of the fact that in n-valued matrices we can define new preservation properties. Cook suggests introducing a second notion of model-theoretic consequence, *non-falsity preservation*:

Definition 6.28. $\Gamma \vDash_{NF} \Delta$ iff for every valuation v, whenever $v(A) \in \{0, b\}$, for every $A \in \Delta$, $v(B) \in \{0, b\}$ for some $B \in \Gamma$.

In contrast to standard truth-preserving validity, we can think of non-falsity preservation as preservation of falsity *upwards*, that is, from conclusions to premises.[30] The critical point is that in the **FDE** matrix the two notions of validity do not come to the same thing.

We define *Tonk Logic* as follows:

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[30]By taking **K3** together with non-falsity preservation we get a logic equivalent to *Analethic Logic* in Beall & Ripley (2004).

Definition 6.29. The matrix is as in Def. 6.19 but with validity defined disjunctively as follows:

$\Gamma \vDash \Delta$ iff either

- $\Gamma \vDash_T \Delta$ (truth-preserving), or;

- $\Gamma \vDash_{NF} \Delta$ (non-falsity preserving).

It is now clear why, in principle, transitivity might fail for `Tonk` Logic: Even if both $\Gamma \vDash \Delta$ and $\Delta \vDash \Theta$, it might be that $\Gamma \vDash \Theta$ fails on both accounts. Say, a valuation in which $v(A) = 1$ for every $A \in \Gamma$, $v(B) = $ b for every $B \in \Delta$, and $v(C) = 0$ for every C in Θ.

However, the failure of transitivity is a language-dependent fact, in particular, it only comes about with the right type of connective. What Cook shows is that this feature of the new notion of model-theoretic validity is triggered by `tonk`.[31] To see this we need a four-valued truth-table for `tonk` which validates the `tonk`-rules.

Definition 6.30. Truth table for tonk:

f_t	1	b	n	0
1	1	b	1	b
b	1	b	1	b
n	n	0	n	0
0	n	0	n	0

In a `Tonk` Logic with `tonk` as its only connective, and the truth-table of Def. 6.30, the two rules are both valid (by Def. 6.29). We have $A \vDash_T A$ `tonk` B (not $\vDash_{NF}$) and A `tonk` $B \vDash_{NF} B$ (not $\vDash_T$), but it is not the case that $A \vDash B$, for any A, B.

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[31]Incidentally, Cook observes that the $\wedge$, $\vee$, $\neg$-fragment of the language (i.e., without `tonk`) is still transitive. See ibid., p. 222.

Cook's `Tonk` Logic has thus already provided us with a matrix for which `tonk` does not trivialise. Yet, it is evident that `tonk` is no more harmonious (in the GE sense) than before. We have simply observed that non-harmonious rules may find suitable non-Boolean habitats.

Where does this leave the diagnosis offered by Stevenson and Wagner? Their way of understanding what it is for inference rules to confer meaning on a logical connective does converge with ours, but the details are different. Without a hegemony for Boolean valuations, there is no argument from meaning-determination that singles out `tonk` as ill-formed. This is no vindication of `tonk`, of course. First, its inference rules might yet be bad for non-semantic reasons; but, more importantly, even if there is a matrix that validates the `tonk`-rules, these rules might not induce the target truth-table. In other words, there might be a failure of absoluteness.

From what we have seen above about **FDE** and absoluteness in Section 6.3.2, it should already be obvious that the `tonk`-rules, cast in a SET-FRML framework, will not uniquely induce the truth-table in Def. 6.30. In fact, Cook remarks that there are 2^{16} distinct **FDE** truth-tables that validate the two `tonk`-rules. Hence, again, we have an example of a logic that is sound and complete with respect to distinct semantics. The only charge left against `tonk` in Cook's logic is that it underdetermines, precisely in the sense which we have explored above (e.g., for classical logic in SET-FRML).

6.4.5 n-Sided Sequent Calculus

There are, evidently, a great number of logics that can be semantically investigated by n-valued matrices. The fact that Thm. 6.9 holds for such logics suggests that we ought to be able to find frameworks with *finite* arguments that can provide absoluteness for these finitely many-valued matrix logics. How then can we produce proof systems that implement the idea of locations discussed for frameworks

above? It turns out that the inferentialist can help herself to some ready-made proof-theoretic structures that precisely capture the idea of locations.

In the spirit of Gentzen's original systems, Schröter (1955), Rousseau (1967, 1970) and others have generalised sequent calculus to a range of many-valued logics. The insight they employ is simple but brilliant: If we return to the sequent system for classical logic, we can observe a straightforward but often ignored connection with the truth-tables of the **CPL**-matrix. Take, for instance, the $\wedge$-rules:

$$\frac{\Gamma, A, B \Rightarrow \Delta}{\Gamma, A \wedge B \Rightarrow \Delta} \ (L\wedge) \qquad \frac{\Gamma \Rightarrow A, \Delta \quad \Gamma \Rightarrow B, \Delta}{\Gamma \Rightarrow A \wedge B, \Delta} \ (R\wedge)$$

For simplicity we will treat the antecedent and succedent as sets rather than multisets, so there is no use for contraction in the system. As we have seen above, in an FSET-FSET framework like this, we standardly define validity as follows: A sequent $\Gamma \Rightarrow \Delta$ is *valid* ($\vDash \Gamma \Rightarrow \Delta$) iff for every valuation $v \in V$, v confirms $\langle \Gamma, \Delta \rangle$, i.e., whenever $v(A) \in \mathcal{D}$, for every $A \in \Gamma$, then there is some $B \in \Delta$ such that $v(B) \in \mathcal{D}$. As the conditional here is material implication, we can paraphrase the definition disjunctively: $\vDash \Gamma \Rightarrow \Delta$ iff for every valuation $v \in V$, either some $v(A) \notin \mathcal{D}$ for some $A \in \Gamma$ or $v(B) \in \mathcal{D}$ for some $B \in \Delta$.[32] More informally, we can think of the traditional sequents as Janus-like: Two-faced with the antecedent side being the false-location, and the succedent side the true-location.

What about the sequent rules? Since the rules operate on sequents we will not say that they preserve truth. Rather, the rules preserve confirmation: For every valuation $v \in V$, if v confirms every premise, then v confirms the conclusion. From this we can see an immediate connection between sequent rules and truth-tables. What $R\wedge$ tells us is that every valuation v must be such that if $v(A) = v(B) = 1$ (from the two premises), then $v(A \wedge B) = 1$ (from the conclusion). Similarly, $L\wedge$ tells us that if either $v(A) = 0$ or $v(B) = 0$ (the formulae are now at the antecedent (false) location), then $v(A \wedge B) = 0$. An exact rendering of the truth-table.

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[32]This is in line with Gentzen's informal reading of sequents as $A_1 \wedge ... \wedge A_n \rightarrow B_1 \vee ... \vee B_m$.

Of course, one might interject that in fact the (single-conclusion) natural deduc-
tion rules for $\wedge$ do the trick as well: One can read the rules downwards as truth-
preserving and upwards as falsity preserving (to at least one premise), and conse-
quently read off the truth-table. Yet, the failure of absoluteness for SET-FRML
(and, needless to say, FSET-FRML) for Boolean valuation spaces already informs
us that this cannot generally be the case. Indeed, the standard negation rules are
an obvious example. In the sequent calculus, however, the negation flip-flop rules
(see Appendix A.10) induce the truth-table.

Again, the same holds for the implication-rules:

$$\frac{\Gamma \Rightarrow A, \Delta \quad \Gamma, B \Rightarrow \Delta}{\Gamma, A \to B \Rightarrow \Delta} \ (L\to) \qquad \frac{\Gamma, A \Rightarrow B, \Delta}{\Gamma \Rightarrow A \to B, \Delta} \ (R\to)$$

For $R\to$, if $v(A) = 0$ or $v(B) = 1$ (sequent read disjunctively), then $v(A \to B) =
1$. With $L\to$ we the final line of the truth-table: If $v(A) = 1$ and $v(B) = 0$, then
$v(A \to B) = 0$, as expected.

Let us then prepare to generalise this relationship between sequent rules and ma-
trices. First, take the identity Axiom from **G1cp**, and rewrite it to indicate the
two-sidedness clearly:

$$(IdAxiom)$$
$$A|A$$

The axiom simply says that A is either false (left-located) or true (right-located):
It underwrites the *bivalence principle* in classical semantics. We can write rules in
a similar way:

$$\frac{\Gamma|A, \Delta}{\Gamma, \neg A|\Delta} \qquad \frac{\Gamma, A|\Delta}{\Gamma|\neg A, \Delta}$$

We can now introduce a three-sided sequent where the locations are as follows:

$$false \mid intermediate \mid true$$

Correspondingly, we write the new 3-sided sequents as:

$$\Gamma_1 \mid \Gamma_2 \mid \Gamma_3$$

This is the novelty offered by Schröter (1955) and Rousseau (1967, 1970) which allows us to formalise 3-valued matrix logics. In the sequel, we will follow the notation from Baaz et al. (1993) and Zach (1993). Let us give some definitions and an example:

Definition 6.31. An n-*valued sequent* is an n-tuple of finite sets Γ_i where $\Gamma \subseteq WFF$:

$$\Gamma_1 | \Gamma_2 | ... | \Gamma_n$$

We say that $A \in WFF$ has *location i* if it is a member of the ith set Γ_i. The ith location corresponds to the the truth-value $v_i \in \mathcal{V}$.

Definition 6.32. A valuation v *confirms* a sequent $\Gamma_1 \mid \Gamma_2 \mid ... \mid \Gamma_n$ if there is an $1 \leq i \leq n$ and a formula $A \in \Gamma_i$ such that $v(A) = v_i$.

Definition 6.33. An n-sided sequent rule for a connective λ is a schema of the form:

$$\frac{\langle \Gamma_1^j, \Delta_1^j | ... | \Gamma_m^j, \Delta_m^j \rangle_{j \in I}}{\Gamma_1 | ... | \Gamma_i, \lambda(A_1, ..., A_n) | ... | \Gamma_m} \; {\scriptstyle (\lambda:i)}$$

where the arity of λ is n, I is a finite set, $\Gamma_l = \bigcup_{j \in I} \Gamma_l^j$, $\Delta_l^j \subseteq \{A_1,..., A_n\}$, and the following condition holds: Let v be a valuation. Then

(i) $\lambda(A_1,..., A_n)$ takes the truth value v_i under v, and;

(ii) For $j \in I$, v confirms the sequent $\Delta_1^j \mid ... \mid \Delta_m^j$

are equivalent.

Let us add a brief explanatory remark to Def. 6.33. The template gives us one sequent rule per truth-value for each connective. Thus, for a 3-valued matrix, the corresponding proof-system will have three sequent-rules for each connective λ. Obviously, each rule λ_i will conclude with λ in the ith location, the only real task is plotting out the correct premises. Baaz et al. (1993) points out that there is not in general a unique set of sequent rules satisfying the condition of Def. 6.33. Any conjunctive normal form $\bigwedge_{j \in I} \bigvee_{l=1}^{m} \bigvee_{A \in \Delta_l^j} A^{v_l}$, where A^{v_l} is short for 'A takes the truth-value v_l', will yield a possible set of sequent-rules.

The sequent-rules are situated in a general framework, described by the following definition:

Definition 6.34. An n-*sided sequent calculus* contains the following elements:

(i) Axioms of the form $A \mid ... \mid A$;

(ii) For every connective λ and every truth-value v_i an introduction rule λ_i (Def. 6.33);

(iii) Weakening rules K_i for every location i:
$$\frac{\Gamma_1|...|\Gamma_i|...|\Gamma_n}{\Gamma_1|...|\Gamma_i, A|...|\Gamma_n} \ (K_i)$$

(iv) Cut rules C_i for every two truth-values $v_i \neq v_j$:
$$\frac{\Gamma_1|...|\Gamma_i, A|...|\Gamma_n \quad \Delta_1|...|\Delta_j, A|...|\Delta_n}{\Gamma_1, \Delta_1|...|\Gamma_n, \Delta_n} \ (C_{i,j})$$

A couple of remarks. First, weakening is motivated by the fact that since elements at a location are disjunctive, adding new ones will not turn a confirming valuation into a refuting valuation. Second, since the valuations are functional, i.e., any formula can only have one truth-value, a result which has $v(A) = v^i = v^j$ where $i \neq j$ is eliminated: Cut simply removes the formula in question.[33] (Note also that the Cut rules, in contrast to the operational rules, are context-sharing.)

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[33]There is an interesting question about *relation semantics*, i.e., where a valuation can assign two different values to the same formula. For instance, **FDE** has such a semantics (see Priest 2008, p. 142-43). We leave it as an open issue how to deal with such semantics in a similar fashion.

For a concrete n-sided sequent calculus, we turn to Example 6.3 above: 3-valued Łukasiewicz logic. We only include sequent rules for $\neg$ and $\rightarrow$ as the procedure for producing the other rules should be transparent. Note that the locations 1, 2, 3 correspond to the truth-values 0, $\frac{1}{2}$, 1 respectively.

Definition 6.35. Sequent system for 3-valued Łukasiewicz logic:

Axioms:

$$A|A|A$$

Structural Rules:

$$\frac{\Gamma_1|\Gamma_2|\Gamma_3}{\Gamma_1, A|\Gamma_2|\Gamma_3} \ (K_1) \qquad \frac{\Gamma_1|\Gamma_2|\Gamma_3}{\Gamma_1|\Gamma_2, A|\Gamma_3} \ (K_2) \qquad \frac{\Gamma_1|\Gamma_2|\Gamma_3}{\Gamma_1|\Gamma_2|\Gamma_3, A} \ (K_3)$$

$$\frac{\Gamma_1, A|\Gamma_2|\Gamma_3 \quad \Gamma_1|\Gamma_2, A|\Gamma_3}{\Gamma_1|\Gamma_2|\Gamma_3} \ (Cut_{1,2}) \qquad \frac{\Gamma_1|\Gamma_2, A|\Gamma_3 \quad \Gamma_1|\Gamma_2|\Gamma_3, A}{\Gamma_1|\Gamma_2|\Gamma_3} \ (Cut_{2,3})$$

$$\frac{\Gamma_1, A|\Gamma_2|\Gamma_3 \quad \Gamma_1|\Gamma_2|\Gamma_3, A}{\Gamma_1|\Gamma_2|\Gamma_3} \ (Cut_{1,3})$$

Operational rules:

$\rightarrow$:

$$\frac{\Gamma_1|\Gamma_2|\Gamma_3, A \quad \Gamma_1, B|\Gamma_2|\Gamma_3}{\Gamma_1, A \rightarrow B|\Gamma_2|\Gamma_3} \ (\rightarrow_1) \qquad \frac{\Gamma_1|\Gamma_2, A, B|\Gamma_3 \quad \Gamma_1, B|\Gamma_2|\Gamma_3, A}{\Gamma_1|\Gamma_2, A \rightarrow B|\Gamma_3} \ (\rightarrow_2)$$

$$\frac{\Gamma_1, A|\Gamma_2, A|\Gamma_3, B \quad \Gamma_1, A|\Gamma_2, B|\Gamma_3, B}{\Gamma_1|\Gamma_2|\Gamma_3, A \rightarrow B} \ (\rightarrow_3)$$

$\neg$:

$$\frac{\Gamma_1|\Gamma_2|\Gamma_3, A}{\Gamma_1, \neg A|\Gamma_2|\Gamma_3} \ (\neg_1) \qquad \frac{\Gamma_1|\Gamma_2, A|\Gamma_3}{\Gamma_1|\Gamma_2, \neg A|\Gamma_3} \ (\neg_2) \qquad \frac{\Gamma_1, A|\Gamma_2|\Gamma_3}{\Gamma_1|\Gamma_2|\Gamma_3, \neg A} \ (\neg_3)$$

In essence, what Baaz et al. (1993) and Zach (1993) have shown us is that we can produce a sound and complete n-sided sequent system for any given finitely-valued matrix logic. In effect, this is not only the V to $\mathbb{L}(V)$ induction step, it is giving a full-blown proof-theory rather than collection of arguments. What we now observe is that since the induced logic is *location-based*, it also underwrites the absoluteness result, $V = \mathbb{V}(\mathbb{L}(V))$, by ensuring that the proof-theoretic framework preserves enough of the semantic structure to induce a valuation space which is precisely the target valuation space. *Infinite riches in a little room.*

Put differently, whereas before we only had an *existence proof* of a FSET-FSET framework that would provide absoluteness for finitely many-valued matrix logics, with n-sided sequents we have a *procedure* for generating proper proof-systems. That is a major expansion of PTS territory.

Yet, the n-sided sequent systems are not yet fully sufficient for what we promised above. The literature focuses on matrix logics with only one designated value, whereas we started out by investigating **FDE** which has two designated values, 1 and b. Worse, comparing Example 6.2 and 6.4, we see that there are *distinct* logics **K3** and **LP**, i.e., $\vDash_{K3} \neq \vDash_{LP}$, which share the same truth-tables. Of course, the matrices are different, but only because **LP** has another designated truth-value. Hence, we can infer that these matrices will produce the very same sequent-systems.

The result might be initially puzzling, but there is a perfectly sensible way in which the resulting logics are different. True, the mechanics of the induced sequent systems are the same; operational and structural rules have the same form, etc. However, the ensuing sequent rules are not embedded in the same metalogical framework. Take an example: The sequent $\langle \emptyset \mid \emptyset \mid A \vee \neg A \rangle$ is not provable in either **K3** or **LP**. As expected in the former case, since **K3** has no theorems. The latter case is worth noting, however. In fact, $A \vee \neg A$ *is* a logical truth in **LP**, i.e., for every valuation $v \in V_{LP}$, $v(A \vee \neg A) \in \mathcal{D}$. However, this is witnessed by

the provability of a different (*n*sided) sequent, namely $\langle \emptyset \mid A \vee \neg A \mid A \vee \neg A \rangle$. This sequent is provable in both systems, but, importantly, the result takes on completely different meanings when we see them on the backdrop of the matrices. For, whereas in **K3** the proved sequent merely states that, for every **K3**-valuation, $A \vee \neg A$ is either true or intermediate, in **LP** it tells us that it is always designated (i.e., either 1 or b).[34]

The example indicates that we need to reinterpret the relationship between model-theoretic validity and provability in *n*-sided sequent systems. By generalising we gave up on the sequent-arrow and thus the intuitive sense in which the mechanics operated on a 'follows from' relation. However, the disjunctive form may still be reformulated to a material implication which will help us understand the above difference. Let M be an *n*-valued matrix where $\mathcal{V} = \{v_1, ..., v_n\}$, and $\mathcal{D} = \{v_i, ..., v_n\}$ where $1 \leq i \leq n$. The we rewrite an *n*-sided sequent $\Gamma_1 \mid ... \mid \Gamma_i \mid ... \mid \Gamma_n$ in two-sided form as:

$$\Gamma_1 \cup ... \cup \Gamma_{i-1} \Rightarrow \Gamma_i \cup ... \cup \Gamma_n$$

and say that the sequent is *valid* iff for every $v \in V$, whenever $v(A) \in \mathcal{D}$, for every $A \in \Gamma_1 \cup ... \cup \Gamma_{i-1}$, then $v(B) \in \mathcal{D}$, for some $B \in \Gamma_i \cup ... \cup \Gamma_n$.[35] Of

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[34]Since the model-theoretic consequence relation is disjunctive and non-transitive, the relationship between derivability and consequence becomes more complicated when we consider Tonk logic. Correspondingly, the *n*-sided sequent rules for tonk (or at least a smattering of tonk)

$$\frac{\Gamma_1|\Gamma_2|\Gamma_3,A|\Gamma_4,A \quad \Gamma_1|\Gamma_2,B|\Gamma_3|\Gamma_4,B}{\Gamma_1|\Gamma_2|\Gamma_3|\Gamma_4,A\mathtt{tonk}B} \ (\mathtt{tonk}_1)$$

$$\frac{\Gamma_1|\Gamma_2|\Gamma_3,A|\Gamma_4,A \quad \Gamma_1,B|\Gamma_2|\Gamma_3,B|\Gamma_4}{\Gamma_1|\Gamma_2|\Gamma_3,A\mathtt{tonk}B|\Gamma_4} \ (\mathtt{tonk}_b)$$

$$\frac{\Gamma_1,A|\Gamma_2,A|\Gamma_3|\Gamma_4 \quad \Gamma_1|\Gamma_2,B|\Gamma_3|\Gamma_4,B}{\Gamma_1|\Gamma_2,A\mathtt{tonk}B|\Gamma_3|\Gamma_4} \ (\mathtt{tonk}_n)$$

$$\frac{\Gamma_1,A|\Gamma_2,A|\Gamma_3|\Gamma_4 \quad \Gamma_1,B|\Gamma_2|\Gamma_3,B|\Gamma_4}{\Gamma_1,A\mathtt{tonk}B|\Gamma_2|\Gamma_3|\Gamma_4} \ (\mathtt{tonk}_0)$$

must be situated in a different framework of derivation than the one looked at so far.

[35]Recall that Γs are sets not multisets.

course, moving back to a two-sided sequent involves loss of structure: For a given valid two-sided sequent, there is no procedure for finding the valid n-sided sequent since we are now working with a less fine-grained distinction, namely designated vs undesignated.

6.5   Objections and Conclusions

6.5.1   Scope

An obvious misgiving about inducing truth-conditions as an avenue to PTS are the apparently limited applications. Even if finite truth-functional matrices can be dealt with in a systematic fashion with n-sided sequents, there is a range of logical constants that falls outside the scope. The most obvious request is that there be a treatment for quantifiers. An approach to PTS that is without an adequate story about quantifiers might be considered too narrow. This is a complaint that must be taken seriously. In the above discussion of many-valued logics we have only considered the propositional fragments, but all systems have natural first-order extensions for which the question of absoluteness arises again.

It is worth taking a brief look at the prospects for finding a similar link between truth-conditions and inference rules for quantifiers. Looking at the V to $\mathbb{L}(V)$ direction investigated in Baaz et al. (1993), we see that quantifiers in first-order many-valued logics can be dealt with in a truth-functional manner: A quantifier Q denotes a truth-function $\hat{Q}: \wp(V) \setminus \{\emptyset\} \to V$ (for details, see e.g., Zach 1993, pp. 32-5). In such a form, we can get a truth-functional handle on quantifiers that will hopefully help to extend the current project. However, this remains essentially an open problem. For, even if the inference rules can distinguish between different sorts of many-valued quantifiers, the fact still remains that there is nothing at the PTS level which obviously corresponds to the quantifier domain element at the

MTS level. Carving out first-order content requires more finesse than carving out propositional (0th-level) content.[36]

6.5.2   Semantics and Pragmatics

A more foundational concern with the present PTS approach is that it is premised on a heavy reliance on model-theoretic parameters, e.g., what the appropriate truth-values are, which universe of valuations one is concerned with, etc. It is a well-known fact that some logics have more than one semantics (sometimes both perfectly natural). An instructive example for our purposes is classical logic, which has both a two-valued and a many-valued semantics. Obviously, the inferentialist cannot insist that the proof-theory alone picks the appropriate semantics for us. The classical inferences are equally compatible with, say, a probabilistic interpretation.[37] There is simply nothing at the proof-theoretic level (in the axiomatisation) which tells us what sort of interpretation a logical connective takes, even if we, given such an interpretational framework, would be able to induce the correct semantic content. In particular, the inferentialist might be worried that the identification of truth-values now plays a prevalent role in carving out the content of a logical connective. After all, sidestepping the centrality of truth in semantics was a motivating factor in traditional PTS.

Here we want to propose a compromise position. First, the theory of meaning should not shy away from delivering truth-conditional semantics as an explication of content. Second, the theory of meaning must implement traditional inferentialist scrouples about truth-conditions. Specifically, the content cannot ultimately be grounded on truth-conditional semantics on pain of reintroducing worries about *concept-acquisition*.[38] The strategy has at least two advantages: Inferentialists,

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[36]Interestingly, Smiley (1996, p. 9) reports that he had categoricity results for both classical first-order logic and normal modal logics. However, no details are provided.

[37]I am indebted to Hannes Leitgeb for making me realise the importance of this point.

[38]I have in mind Dummett's well-known arguments from manifestability and learnability (see Dummett 1973b, p. 216-17).

although sensitive to questions about understanding and learning langauge (and, thus, knowledge of meaning), are typically elusive about the nature of meaning. We need to ask the question 'What is semantic content?', not only 'How does an expression get its semantic content?'. In that sense, our aim differs from that of Dummett, which is nicely summed up in the mantra that "[a] model of meaning is a model of understanding" (Dummett 1973b, p. 217), i.e., an individual's knowledge of meaning. To this we say, a model of meaning is more than a model of understanding. The inferentialist can address epistemic issues independently of concerns about the ontology of meaning.[39]

So far we have oscillated between truth-condition talk and proof-condition talk. We now need to find an interpretation of the interaction between the two levels that does justice to the above compromise. On the one hand we cannot let valuation spaces be the semantic foundation, and, on the other, we cannot expect the proof-theory to carry the burden of interpretation on its own. The proposal we want to sketch here, even if only with a broad brush, picks up the idea of interpreting sequents in terms of pragmatic roles (discussed in Section 3.3). We saw that there is an analysis of sequents and multiple-conclusion sequent-calculus in terms of the interaction between the speech acts assertion and denial. Restall (2005) defines *states*, [X : Y], and *incoherence* of states, to produce a corresponding interpretation of sequent rules as rational constraints imposed by the norms of the speech acts.

In Restall (2009b), the analysis is put to use in a way pertinent to our project. Restall envisages a connection between coherent states and valuations which is reminiscent of the connection between logics and valuation spaces discussed throughout this chapter. We can think of constraints on states, e.g., if $[X, \neg A : Y]$ is coherent, then $[X : Y, A]$ is coherent, as preceding and producing a valuational constraint set by the L vs $\mathbb{V}(L)$ link from the foregoing sections. In particular, the principle for states has a corresponding sequent rules, namely the familiar $L\neg$ rule, which instructs us that for any valuation, if A is in the truth-location, then $\neg A$ is in

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[39]Hence, for exactly that reason, we avoid the misleading terminology *anti-realist semantics*.

the false-location. This in turn is simply the familiar truth-functional clause for Boolean negation in a different guise.

Once we follow Restall in drawing up such a pragmatics-semantics interface, we can help ourselves to generalisations which correspond to the proof-theoretic generalisations in Section 6.4.5. For, one might think to object that the connection between states and valuations still work on the backdrop of facts about matrices. However, the improvement is supposed to reside in the fact that it is now the norms of speech acts which ground truth-conditional assumptions.[40] On the simple picture, Boolean valuations are associated with the assertion and denial in a straightforward manner. However, no one suggests, I take it, that such a picture forms an exhaustive or even accurate picture of the involved speech acts. Rather, just like the semantics of logical connectives, the pragmatics of logical connectives are nuanced and varied. In that spirit, we can enhance the notion of a state to carry a heavier workload.

As an example, let us revisit 3-valued Łukasiewicz logic, Ł_3. The described notion of a state, being two-sided, is not a sufficient match for the 3-sided sequents associated with Ł_3. Nevertheless, there is a corresponding notion that might help. Let a *tri-state* be a triple [X : Y : Z] where we think of X as a collection of asserted statements, Z, as a set of denied statements, and, finally, Y as a set of *doubted* statements. Alternative interpretations are available: Y might be a set of *ignored* statements, or statements one demurs from, etc. The point here is not to sell any particular interpretation, but to indicate the direction in which to take the analysis. Of course, n-sided matrices and n-sided sequents, for an arbitrary n, outstrips any pragmatic interpretation, but that is besides the point. In interesting cases, there is hope for finding connections between the norms governing speech acts and the inference rules governing logical constants.

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[40]Compare the connection between signs and valuations in the Smiley-Rumfitt framework, Section 3.2.3.

In particular, the analysis offers an interpretation of the structural language, and the logic that governs structural connectives (e.g., commas), which is not itself merely another level of semantics. Instead, we consider the structural language a representation of a pragmatic level. A suitable, but controversial, metaphor would be 'making it explicit'. Pragmatic roles are made explicit by the semantic content of logical constants. In other words, it is the *metasemantic* role of proof-conditions. Here is the spirit of inferentialism: The use-theoretic tradition reenters the picture as a pragmatic contribution to the theory of meaning. With the risk of resurrecting an inferentialist cliché, *Im Anfang war die Tat*—the speech act.

Chapter 7

Conclusion

In PTS, the concept of formal logical consequence is grounded on the meaning of logical constants through the meaning-determining function of inference rules. We have offered a discussion of proof-theoretic harmony, the main success criterion for a set of inference rules to fix the meaning of a connective. It has been argued extensively that PTS and harmony should not be restricted to a revisionary debate about classical and intuitionistic logic. Rather, it has the capacity to be a more encompassing semantic project, complementing, but not supplanting, traditional model-theoretic semantics. However, expanding PTS beyond its traditional borders involves generalising the proof-theoretic framework of the semantics.

We have argued that the best account of PTS for classical logic involves a multiple-conclusion framework. With such a generalisation, however, comes the difficulties caused by bringing PTS into the substructural era of proof-theory. The presence and absence of structural properties reveals a semantic lacuna in the notion of harmony; for, the minimalist idea that structural properties can be isolated and treated as extra-semantic in PTS appears unconvincing. Rather, what is needed is a theory that integrates structural properties in PTS. In other words, unless the formal accounts of harmony can be developed to incorporate the growing corpus

of substructural logics, the semantic importance of harmony as a concept is undermined. There is prospect for rethinking harmony with more structural flexibility, for example to incorporate multiple-conclusion and a distinction between additive and multiplicative rules. However, this requires that the inferentialist looks beyond the confines of natural deduction to sequent calculus and other formalisms that guide the study of such structural extensions.

In order to properly understand what it is for inference-rules to determine the meaning of logical constants, we have developed a new and more specific theory of PTS. The idea is to give content to the gloss that the meaning of a connective is 'read off' the inference rules, by looking at how rules can induce valuation in model-theoretic semantics. By ensuring that a logic uniquely specifies a valuation space, we have a precise sense in which the truth-conditional content of logical constants are determined by rules. The critical formal counterpart is that absoluteness holds for the valuation space in question. As it turns out, we can then extend PTS to a range of logics through the formal relationship between matrices for finitely many-valued logics on one side, and n-sided sequent calculi on the other.

Even though the notion of truth-conditions are reintroduced as a core constituent of the formal semantics, we argue that this does not jeopardise the inferentialist spirit. One can still think of the inference rules as our portal to grasping the sense of logical constants, and thus as the connection between validity and entitlement to inference. We offer an interpretation of the proof-theoretic frameworks in terms of speech acts, instituting a metasemantic role for structural properties in the proof systems. The pragmatic character of the interpretation grounds semantic content of logical constants in the inferential practice.

Picking up where we began, we ask with Marlowe's *Faust*:

      Is, to dispute well, logic's chiefest end?
      Affords this art no greater miracle?

It does. But, for logic to offer philosophy 'more matter with less art', it must provide insights into the epistemological significance of deduction. How we acquire knowledge by reasoning alone, remains one of philosophy's greatest mysteries. If logic is left powerless to contribute to this question, it is of little consequence to philosophy. PTS, we argue, has more to offer as part of this bigger picture.

Appendix A

Proof-Systems

A.1 Nip (Nmp)

This is Prawitz's intuitionistic system (see Prawitz (1965, pp. 20-1). *Minimal logic*, **Nmp**, (Johansson 1936) results from omitting $E\bot_I$ (or, as it is more commonly called, *EFQ*).

$$\frac{A \quad B}{A \wedge B} \; (I\wedge) \qquad \frac{A \wedge B}{A} \; (E\wedge_1) \qquad \frac{A \wedge B}{B} \; (E\wedge_2)$$

$$\frac{A}{A \vee B} \; (I\vee_1) \qquad \frac{B}{A \vee B} \; (I\vee_2) \qquad \frac{A \vee B \quad \overset{[A]^u}{\overset{\vdots}{C}} \quad \overset{[B]^v}{\overset{\vdots}{C}}}{C} \; (E\vee_{(u,v)})$$

$$\frac{\overset{[A]^u}{\overset{\vdots}{B}}}{A \to B} \; (I\to)_{(u)} \qquad \frac{A \to B \quad A}{B} \; (E\to)$$

$$\frac{\bot}{A} \; (E\bot_I)$$

Another way of extending **Nmp** to **Nip** is by adding *Disjunctive Syllogism* instead of $E\bot$:

$$\frac{A \vee B \quad \neg A}{B} \; (DS)$$

A.2   Ncp

Prawtiz's classical system replaces $E\perp_I$ with $E\perp_C$ (sometimes called Classical Reductio ad Absurdum, CRA):

$$\frac{\begin{array}{c} [\neg A]^u \\ \vdots \\ \perp \end{array}}{A} \; (E\perp_C)_{(u)}$$

There is a related rule which is also referred to as Classical Reductio ad absurdum, but which does not use $\perp$.

$$\frac{\begin{array}{cc} [\neg A]^u & [\neg A]^u \\ \vdots & \vdots \\ B & \neg B \end{array}}{A} \; (CRA^\neg)_{(u)}$$

When we talk about **Ncp** we refer to a system with the former rule. Recall that in Prawitz's formulation, negation is always defined in terms of $\perp$ and $\rightarrow$.

A.3   Nip$^\neg$

As **Nip** above, but with the following negation rules (*in addition to* $E\perp_I$):

$$\frac{\begin{array}{c} [A]^u \\ \vdots \\ \perp \end{array}}{\neg A} \; (I\neg)_{(u)} \qquad \frac{A \quad \neg A}{\perp} \; (E\neg)$$

A.4   Ncp$^\neg$

As **Nip**$^\neg$ above but without $E\bot_I$, and with *Double Negation Elimination*:

$$\frac{\neg\neg A}{A} \ {\scriptstyle(DNE)}$$

A.5   Ncp^T

As **Ncp**$^\neg$ but with $E\bot_C$ instead of *DNE*.

A.6   Ncp^{LEM}

Gentzen's **NJ** was just **Nip**$^\neg$ above, but with the addition of the *Law of Excluded Midde Rule*. Although this is strictly speaking an axiom, we think of it as a degenerate type of rule (i.e., with empty premise set).

$$\frac{}{A \vee \neg A} \ {\scriptstyle(LEM)}$$

A.7   Ncp^{CDil}

A way of getting classical logic with $\neg$ but not $\bot$ is by adding *Classical Dilemma* and a $\bot$-free version of EFQ:

$$\cfrac{\begin{matrix}[A]^u & [\neg A]^v \\ \vdots & \vdots \\ B & B\end{matrix}}{B} \ {\scriptstyle(CDil)_{(u,v)}} \qquad \frac{A \quad \neg A}{B} \ {\scriptstyle(EFQ)}$$

Note that LEM follows straightforwardly from this rule by letting $B = A \vee \neg A$.

A.8 $\mathbf{Ncp}^{+-}$

Operational rules:

$$\frac{+A \quad +B}{+(A \wedge B)} \; {\scriptstyle (+I\wedge)} \quad \frac{+(A \wedge B)}{+A} \; {\scriptstyle (+E\wedge_i)} \quad \frac{+(A \wedge B)}{+B} \; {\scriptstyle (+E\wedge_{ii})}$$

$$\frac{-(A \vee B)}{-A} \; {\scriptstyle (-E\vee_i)} \quad \frac{-(A \vee B)}{-B} \; {\scriptstyle (-E\vee_{ii})} \quad \frac{-A \quad -B}{-(A \vee B)} \; {\scriptstyle (-I\vee)}$$

$$\frac{+(A \to B) \quad +A}{+B} \; {\scriptstyle (+E\to)} \quad \frac{-(A \to B)}{+A} \; {\scriptstyle (-E\to_i)} \quad \frac{-(A \to B)}{-B} \; {\scriptstyle (-E\to_{ii})}$$

$$\frac{-A}{+(\neg A)} \; {\scriptstyle (+I\neg)} \quad \frac{+(\neg A)}{-A} \; {\scriptstyle (+E\neg)}$$

Co-ordination principles:

$$\begin{array}{c} [\alpha]^u \\ \vdots \\ \dfrac{\bot}{\alpha^*} \; {\scriptstyle (RED^*)(u)} \end{array} \qquad \frac{\alpha \quad \alpha^*}{\bot} \; {\scriptstyle (LNC^*)}$$

A.9 NC

Structural Rules:

$$\frac{\Gamma}{\Gamma, A} \; {\scriptstyle (K)} \quad \frac{\Gamma, A, A}{\Gamma, A} \; {\scriptstyle (C)} \quad \frac{\Gamma, A, B, \Delta}{\Gamma, B, A, \Delta} \; {\scriptstyle (Ex)}$$

Operational Rules:

$$\begin{array}{c} [A]^u \\ \vdots \\ \dfrac{\Gamma, B}{\Gamma, A \to B} \; {\scriptstyle (I\to^{NC})(u)} \end{array} \qquad \frac{\Gamma, A \quad \Delta, A \to B}{\Gamma, \Delta, B} \; {\scriptstyle (E\to^{NC})}$$

$$[A]^u$$
$$\vdots$$
$$\frac{\Delta, \bot}{\Delta, \neg A} \; (I\neg_{NC})(u) \quad \frac{\Gamma, A \quad \Delta, \neg A}{\Gamma, \Delta} \; (E\neg_{NC})$$

$$\frac{\Gamma, A \quad \Delta, B}{\Gamma, \Delta, A \wedge B} \; (I\wedge) \quad \frac{\Gamma, A \wedge B}{\Gamma, A} \; (E\wedge_1) \quad \frac{\Gamma, A \wedge B}{\Gamma, B} \; (E\wedge_2)$$

$$\frac{\Gamma, A}{\Gamma, A \vee B} \; (I\vee_1) \quad \frac{B}{\Gamma, A \vee B} \; (I\vee_2) \quad \frac{\Gamma, A \vee B}{\Gamma, A, B} \; (E\vee)$$

A.10 G1cp

Axioms:

$$\begin{array}{cc} (Id) & (L\bot) \\ A \Rightarrow A & \bot \Rightarrow \end{array}$$

Structural rules:

$$\frac{\Gamma \Rightarrow \Delta}{\Gamma, A \Rightarrow \Delta} \; (LK) \quad \frac{\Gamma \Rightarrow \Delta}{\Gamma \Rightarrow A, \Delta} \; (RK)$$

$$\frac{\Gamma, A, A \Rightarrow \Delta}{\Gamma, A \Rightarrow \Delta} \; (LW) \quad \frac{\Gamma \Rightarrow A, A, \Delta}{\Gamma \Rightarrow A, \Delta} \; (RW)$$

Operational rules:

$$\frac{\Gamma, A_i \Rightarrow \Delta}{\Gamma, A_0 \wedge A_1 \Rightarrow \Delta} \; (L\wedge) \quad \frac{\Gamma \Rightarrow A, \Delta \quad \Gamma \Rightarrow B, \Delta}{\Gamma \Rightarrow A \wedge B, \Delta} \; (R\wedge)$$

$$\frac{\Gamma, A \Rightarrow \Delta \quad \Gamma, B \Rightarrow \Delta}{\Gamma, A \vee B \Rightarrow \Delta} \; (L\vee) \quad \frac{\Gamma \Rightarrow A_i, \Delta}{\Gamma \Rightarrow A_0 \vee A_1, \Delta} \; (R\vee)$$

$$\frac{\Gamma \Rightarrow A, \Delta \quad \Gamma, B \Rightarrow \Delta}{\Gamma, A \rightarrow B \Rightarrow \Delta} \; (L\rightarrow) \quad \frac{\Gamma, A \Rightarrow B, \Delta}{\Gamma \Rightarrow A \rightarrow B, \Delta} \; (R\rightarrow)$$

A.11   G1ip

As **G1cp** but with succedent restricted to the emptyset or a singleton.

A.12   G1cp$^\neg$

We can replace the $L\bot$ axiom with rules for negation:

$$\frac{\Gamma \Rightarrow A, \Delta}{\Gamma, \neg A \Rightarrow \Delta} \ (L\neg) \qquad \frac{\Gamma, A \Rightarrow \Delta}{\Gamma \Rightarrow \neg A, \Delta} \ (R\neg)$$

A.13   Cut rules

Multiplicative cut for **G1[ic]**:

$$\frac{\Gamma \Rightarrow A, \Delta \quad \Gamma', A \Rightarrow \Delta'}{\Gamma, \Gamma' \Rightarrow \Delta, \Delta'} \ (Cut_m)$$

Additive Cut for **G1cp**:

$$\frac{\Gamma \Rightarrow A, \Delta \quad \Gamma, A \Rightarrow \Delta}{\Gamma \Rightarrow \Delta} \ (Cut_a)$$

The version for **G1ip** takes single-succedent.

A.14   G3cp

Axioms:

$$\overset{(Id)}{\Gamma, p \Rightarrow p, \Delta} \qquad \overset{(L\bot)}{\Gamma, \bot \Rightarrow \Delta}$$

Operational rules:

$$\frac{\Gamma, A, B \Rightarrow \Delta}{\Gamma, A \wedge B \Rightarrow \Delta} \ _{(L\wedge)} \qquad \frac{\Gamma \Rightarrow A, \Delta \quad \Gamma \Rightarrow B, \Delta}{\Gamma \Rightarrow A \wedge B, \Delta} \ _{(R\wedge)}$$

$$\frac{\Gamma, A \Rightarrow \Delta \quad \Gamma, B \Rightarrow \Delta}{\Gamma, A \vee B \Rightarrow \Delta} \ _{(L\vee)} \qquad \frac{\Gamma \Rightarrow A, B, \Delta}{\Gamma \Rightarrow A \vee B, \Delta} \ _{(R\vee)}$$

$$\frac{\Gamma \Rightarrow A, \Delta \quad \Gamma, B \Rightarrow \Delta}{\Gamma A \to B \Rightarrow \Delta} \ _{(L\to)} \qquad \frac{\Gamma, A \Rightarrow B, \Delta}{\Gamma \Rightarrow A \to B, \Delta} \ _{(R\to)}$$

A.15   G3ip

Axioms:

$$\overset{(Id)}{\Gamma, P \Rightarrow P} \qquad \overset{(L\bot)}{\Gamma, \bot \Rightarrow A}$$

Operational rules:

$$\frac{\Gamma, A, B \Rightarrow C}{\Gamma, A \wedge B \Rightarrow} \ _{(L\wedge)} \qquad \frac{\Gamma \Rightarrow A \quad \Gamma \Rightarrow B}{\Gamma \Rightarrow A \wedge B} \ _{(R\wedge)}$$

$$\frac{\Gamma, A \Rightarrow C \quad \Gamma, B \Rightarrow C}{\Gamma, A \vee B \Rightarrow C} \ _{(L\vee)} \qquad \frac{\Gamma \Rightarrow A_i}{\Gamma \Rightarrow A_0 \vee A_1} \ _{(R\vee)}$$

$$\frac{\Gamma, A \to B \Rightarrow A \quad \Gamma, B \Rightarrow C}{\Gamma, A \to B \Rightarrow C} \ _{(L\to)} \qquad \frac{\Gamma, A \Rightarrow B}{\Gamma \Rightarrow A \to B} \ _{(R\to)}$$

A.16   Gentzen-Schütte system, GS1p

This is the one-sided system corresponding to **G1cp** (other connectives are defined):

Axioms:

$$\overset{(Id)}{P, \neg P}$$

Structural rules:

$$\frac{\Gamma}{\Gamma, A} \ {\scriptstyle (RK)} \qquad \frac{\Gamma, A, A}{\Gamma, A} \ {\scriptstyle (RW)}$$

$$\frac{\Gamma, A \quad \Delta, \neg A}{\Gamma, \Delta} \ {\scriptstyle (Cut)}$$

Operational rules:

$$\frac{\Gamma, A}{\Gamma, A \vee B} \ {\scriptstyle (R\vee_L)} \qquad \frac{\Gamma, B}{\Gamma, A \vee B} \ {\scriptstyle (R\vee_R)} \qquad \frac{\Gamma, A \quad \Gamma, B}{\Gamma, A \wedge B} \ {\scriptstyle (R\wedge)}$$

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