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Prolegomena to a Semantic Theory for Natural Languages Based on Recursive Arithmetic

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Prolegomena to a Semantic Theory for Natural Languages Based on Recursive Arithmetic

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....

A Celeste e alla sua mamma Francesca,
perché prima di loro nulla era chiaro

E a Bruno,
che meritava una laurea in lettere più di me

Sommario

In questa tesi si indaga la possibilità di impiegare una versione dell'aritmetica (primitiva) ricorsiva allo scopo di costruire le rappresentazioni semantiche degli enunciati delle lingue naturali. L'idea deriva dal fatto che un sistema formale di questo tipo si differenzia sotto diversi aspetti significativi dai formalismi tradizionalmente impiegati in semantica formale, basati sulla logica predicativa classica. In particolare, nel caso dell'aritmetica ricorsiva, i quantificatori non sono termini primitivi del linguaggio, ma vengono definiti come specifiche funzioni ricorsive; inoltre, essi non possono esservi definiti in modo analogo a come vengono classicamente concepiti, ossia come quantificatori "illimitati", il cui dominio non è necessariamente finito. Nell'aritmetica ricorsiva è possibile, tuttavia, esprimere l'equivalente di asserzioni generali, che riguardano cioè un qualunque individuo arbitrariamente scelto, mediante l'uso di variabili libere; crucialmente, queste variabili non stabiliscono rapporti di portata con altri termini del linguaggio, e la loro interpretazione è per gran parte assimilabile a quella dei quantificatori universali tradizionali con portata ampia. Alla luce di questo fatto, si sostiene che diversi fenomeni linguistici, attestati in lingue appartenenti a famiglie diverse, possono essere spiegati in modo particolarmente naturale assumendo che gli elementi lessicali e le strutture sintattiche che vi sono coinvolti siano correlati alla presenza di tali variabili libere con valore generico nella forma logica dell'enunciato. In particolare, vengono analizzati gli indefiniti generici, i condizionali e le frasi abituali, nella loro interazione con la negazione e, nei primi due casi, con i sintagmi nominali quantificati; in connessione con questi aspetti, viene toccato il problema della struttura interna degli indefiniti negativi; infine, in termini di variabili generiche viene offerta una possibile analisi del fenomeno del Neg-Raising. Molte delle proposte avanzate sviluppano idee già apparse nella letteratura, in Löbner (2000, 2013) e, soprattutto, in Goodstein (1951, 1957) e Hornstein (1984). Alcuni apparenti problemi della teoria delineata vengono spiegati facendo appello a uno specifico trattamento delle frasi incassate motivato indipendentemente. Si suggerisce quindi che i fenomeni analizzati, considerati nel loro complesso, confermino la validità del progetto iniziale, lasciando intravvedere nuovi potenziali scenari per un fecondo scambio tra filosofia della matematica e semantica linguistica.

Abstract

In this dissertation, the possibility of employing a version of (primitive) recursive arithmetic to build the semantic representations of natural language sentences is explored. This idea derives from the fact that such a formal system differs under several respects from formalisms which have been traditionally employed in formal semantics, based on classical predicate logic. Specifically, in the case of recursive arithmetic, quantifiers are not primitive terms of the language, but they are defined as peculiar recursive functions; additionally, within it they cannot be defined in a way which corresponds to how they have traditionally been conceived, i.e. as “unbounded” quantifiers, whose domain is not necessarily finite. In recursive arithmetic, however, it is possible to convey something equivalent to general assertions, regarding any arbitrarily chosen individual, by using free variables; crucially, such variables do not establish relations of scope with other terms of the language, and their interpretation can to a large extent be assimilated to that of wide scope standard universal quantifiers. In the light of this, it is argued that several linguistic phenomena, attested in natural languages of different families, can be explained in an especially natural way by assuming that the lexical elements and syntactic structures involved are correlated with the presence of these free variables with generic value in the logical form of the sentence. In particular, generic indefinites, conditionals and habitual clauses are analyzed, in their interaction with the negation and, as for the first two, with quantified noun phrases; in connection with these aspects, the problem of the internal structure of negative indefinite is also addressed; finally, a possible analysis of the Neg-Raising phenomenon in terms of generic variables is offered. Many of the proposals made here have already appeared in the literature, in Löbner (2000, 2013) and, moreover, in Goodstein (1951, 1957) and Hornstein (1984). Some apparent counterexamples to the theory outlined are explained by making appeal to an independently motivated treatment of embedded clauses. It is suggested that the analyzed phenomena, when collectively considered, confirm the validity of the initial project, letting one glimpse new potential scenarios for a fruitful exchange between the philosophy of mathematics and linguistic semantics.

Contents

1 Introduction **1**

I A system of recursive arithmetic **5**

2 Finitism (and predicativism) **7**

3 Primitive recursion **21**

4 Recursive arithmetic **31**
4.1 A logic-free system of recursive arithmetic 31
4.2 Grounding the formal system 37

II Recursive arithmetic and natural language semantics **39**

5 Terms of recursive arithmetic as semantic representations **41**
5.1 A remark on ontology 41
5.2 Semantic representations 44
5.3 The theoretical claim 49
5.4 The illocutionary operator 'ASSERT' 53

6 Neutral semantic representations **57**
6.1 Lexical words 58
6.1.1 Verbs 58
6.1.2 The Davidsonian account 61
6.1.3 Nouns and other lexical words 67
6.2 Connectives: conjunction and disjunction 68
6.3 Anaphoric elements 70
6.4 Indexicals 81
6.5 Plurals 84

6.5.1 Adjectives of quantity and similar modifiers 88
6.5.2 Distributivity 89
6.6 Negation 90
6.7 Dependent clauses 104
6.7.1 Relative clauses 114
6.7.2 Focus and *wh*-questions 116

7 Challenging semantic representations 121
7.1 Bounded quantifiers 121
7.1.1 Existential import 123
7.2 Generic indefinites 140
7.2.1 First *caveat*: epistemic indefinites 140
7.2.2 Second *caveat*: bare nouns 142
7.2.3 Third *caveat*: leaving islands aside and movement in peace . 154
7.2.4 Standard accounts of generic indefinites 157
7.2.5 Goodstein (1951) 162
7.2.6 Hornstein (1984) 164
7.2.7 Generic indefinites as free determiners 171
7.2.8 Generic indefinites in dependent clauses 190
7.3 Conditionals 193
7.4 Negative indefinites 201
7.5 Habituals 216
7.6 Neg-Raising 218

8 Conclusions 231
8.1 Summary 231
8.2 Open problems 234
8.3 Interdisciplinary perspectives 235

A Quantity implicatures 237

B Examples of derivations in recursive arithmetic 241
B.1 Derivation-schemata of general utility 241
B.2 A proof in $\mathcal{PR}$ 243
B.3 A proof of the commutativity of sum in $\mathcal{RA}$ 245

List of Symbols

S	successor function	22
v, u, w, v_1, v_2, ...	generic variable	22
p, p_1, p_2, ...	placeholder	23
A, B, C, A_1, A_2, ...	term	23
α, β, α_1, α_2, ...	equation	23
f, f_1, f_2, ...	p.r.u. function	23
$+$	sum	25
$\cdot$	product	25
$:=$	definitional identity	25
$\phi(A)$	equation containing A	25
$\phi(A/B)$	equation obtained from $\phi(A)$ by substituting all occurrences of A in it with a term B such that B does not contain placeholders contained in $\phi(A)$	25
$\varphi(A)$	term possibly containing A	25
$\varphi(A/B)$	term obtained from $\varphi(A)$ by replacing any occurrence of A in it with B	25
$\varphi(p)$	string obtained from $\varphi\langle p\rangle$ by replacing at least one term in it with an occurrence of p	25
$\varphi\langle p\rangle$	term not containing p	25
$\varphi(p/A)$	term obtained from $\varphi(p)$ by substituting all occurrences of p in it with A	25
$\vec{X}$	$X_1, \ldots, X_n$, for a given n	27
$\vdash_{\mathcal{S}}$	relation of derivation in the system $\mathcal{S}$ (holding from left to right)	27
$\dashv\vdash_{\mathcal{S}}$	relation of derivation in the system $\mathcal{S}$ (holding in both directions)	27
$\bigcup_{i=1}^{n}\{\alpha_i\}$	set of n equations	28
P	predecessor	29
$\dot{-}$	integer difference	29
$\wedge$	logical conjunction	29
$\vee$	logical disjunction	29

$\neg$	logical negation	29
$\rightarrow$	material implication	29
$\leq$	'less than or equal to'	29
$(\forall x \leq A)\varphi(x)$	bounded universal quantifier	30
$(\exists x \leq A)\varphi(x)$	bounded existential quantifier	30
t	indexical term	37
$[\![\ ]\!]$	interpretation function	48
$\Rightarrow$	entailment	48
$\in$	set appartenence	49
ASSERT	illocutionary operator of assertion	54
$\Leftrightarrow$	equivalence	55
$>>$	'presupposes'	76
$>/>$	'does not presuppose'	76
Π	'is an individual part of'	88
$Card(A, B)$	'the cardinality of A is B'	88
$Dist$	distributive operator	89
$Proof(A, B)$	'there is a proof of B of length at most $f(A)$'	115
$\rightsquigarrow$	'conversationally implicates'	170
$Acc(A, B)$	'A is accessible to B'	198

Chapter 1

Introduction

The aim of this dissertation is to investigate the possibility, and to argue for the opportunity, of applying a version of a certain mathematical formalism to the study of meaning in natural language. The formalism is that of Primitive Recursive Arithmetics, also simply Recursive Artihmetic (hereafter, also RA), first introduced in Skolem (1923) but improved in Goodstein (1954) and especially defended, from a foundational perspective, in Goodstein (1951, 1957).

In the first part of the dissertation, I will briefly explore the philosophical background underlying the proposal. First of all, in Ch. 2, I will deal with the main motivations that had led to the formulation and the development of RA. Then, I will introduce the system I am going to discuss in three successive steps: in Ch. 3, I will describe a system $\mathcal{PR}$ which can compute primitive recursive unary functions; in §4.1, I will extend the previous system to obtain a formal system $\mathcal{RA}$ which is a logic-free version of RA; in §4.2, I will define a minimal extension $\mathcal{RA}^t$ of $\mathcal{RA}$ obtained by simply adding a special term to its vocabulary.

In the second part, I will try to show how one could look at $\mathcal{RA}^t$ as a formal language with which to build suitable semantic representations for natural language sentences and I will argue for its superiority, for that purpose, over standard formalisms based on (extensions of) classical first order logic. After briefly addressing some general issues of natural language semantics in §5.1 and §5.2, I will illustrate somewhat more precisely the central claim of the dissertation in §5.3. Then, in §5.4, I will define the role of an illocutionary operator in shifting from semantic representations to equations of $\mathcal{RA}^t$.

In Ch. 6, I will define, as terms of $\mathcal{RA}^t$, the meaning of some classes of linguistic expressions in a way which closely parallels definitions which are already on the market or which, however, do not provide any evidence supporting the main theoretical claim: in §6.1, I will deal with verbs, nouns, adjectives and adverbs; in §6.2, I will deal with conjunction and disjunction; in §6.3, I will say something about the variegate class of anaphoric elements in natural languages; in §6.4, I will link the issue of indexicals and their function in communication, owing very much to Russell's theory, to the inferential properties of the term t introduced in $\mathcal{RA}^t$; in §6.5, I will deal with plural morphology and some features of numeral modifiers and similar; in §6.6, I will deal with negation, which will come to play a crucial role in the subsequent part of the dissertation and whose syntactic properties, for this reason, will be analyzed in more detail; finally, in §6.7, I will address the issue of dependent clauses, developing a proposal, inspired by sententialist accounts of attitude reports, which will prove necessary to articulate an important distinction made in the following central chapter.

Ch. 7 will be, in fact, the core of this dissertation: first of all, in §7.1, I will defend the viability of a treatment of natural language quantifiers as bounded quantifiers (the only special sort of quantifiers available for $\mathcal{RA}^t$); then, after addressing, in §7.2.1–7.2.3, three issues potentially interfering with the ongoing discussion, I will rapidly review two standard approaches to genericity widely attested in the literature and I will present a different approach independently advocated by Goodstein (1951, 1957) and, in some more length, by Hornstein (1984). In §7.2.7, I will develop their insights by formulating an analysis of generic indefinites in terms of free variables. I will then take into account, in §7.2.8, an apparent counterexample to the analysis just offered, relying on the semantic characterization of dependent clauses outlined in the previous chapter. Then, in §7.3, I will give a semantics for conditionals in terms of that peculiar notion of genericity. Finally, in §7.5 and §7.6 I will consider two issues concerning the verbal domain: habituals and Neg-Raising, respectively, suggesting how that conception of generic arguments can shed some light even on them.

Ch. 8 will draw the conclusions, set some open questions within the framework developed before and underline the interdisciplinary connections behind it.

Finally, in Appendix A, I will very briefly outline an account of quantity implicatures, one important and pervasive pragmatic phenomenon I alluded to at some points in the dissertation. Then, in Appendix B I will provide some simple examples of basic derivations both in the system $\mathcal{PR}$ and in $\mathcal{RA}$ (the latter holding also in $\mathcal{RA}^t$).

The linguistic data employed in this work are almost always taken from English and Italian, my first language.[1]

[1] As for the glosses in linguistic examples, I strived to follow the Leipzig Glossing Rules as stated in the version of February 2008, which can be found at the web address 'http://www.eva.mpg.de/lingua/resources/glossing-rules.php'. Furthermore, I decided to eventually follow all and only the following "optional" rules: 4A, 4B, 4C, 4D and 4E. See the glossary there for the meaning of the abbreviations employed here.

In any case, the glosses are minimal, showing only the grammatical features, if any, that are relevant to the ongoing analysis. To this extent, quite often I used ambiguous glosses (for instance, the verb *have* may appear in them, without any specification of mood, person or number): when this is case, I take it for granted that the translation suffices to disambiguate the gloss, under the implicit assumption that the translation and the glossed material share as many grammatical features as possible.

Part I

A system of recursive arithmetic

Chapter 2

Finitism (and predicativism)

Wszystko -
słowo bezczelne i nadęte pychą.
Powinno być pisane w cudzysłowie.
Udaje, że niczego nie pomija,
że skupia, obejmuje, zawiera i ma.
A tymczasem jest tylko
strzępkiem zawieruchy.

Wisława Szymborska, *Wszystko*[1]

This section deals with some rather philosophical issues in the foundations of mathematics and the history of logic. It cannot be nor is meant to be in any way an exhaustive treatment of such issues; its main purposes are to sketch the historical background which gave birth to the formal system I am going to base myself upon (which I will introduce in some detail in the next section), and, moreover, to describe some features of finitism, a philosophical doctrine about the

[1] Everything - / a bumptious, stuck-up word. / It should be written in quotes. / It pretends to miss nothing, / to gather, hold, contain, and have. / While all the while it's just / a shred of gale (Wisława Szymborska, *Everything*; translation from Polish by Stanislaw Baranczak and Clare Cavanagh).

Yes, I know that this quotation is something of a boomerang, for this dissertation!

nature of mathematics and reasoning strictly associated with that formal system and enlightening some of its most interesting properties.[2]

Two main notions of 'infinite' have been counterposed throughout history: that of an *actual* infinite, corresponding to an infinite amount of something which could be captured by the thought at once, and that of a *potential* infinite, i.e. a process developing indefinitely through time. The idea that only the latter, among the two notions, could find any correspondence in reality was probably, at least implicitly, held by most thinkers until the second half of the Nineteenth century. Just to cite a well-known example, the great mathematician Carl Friederich Gauss, in a letter to Schumacher written in 1831 (Gauss (1900a)), expressed his worries about his friend's mathematical work with the following words:

> I must protest most vehemently against your use of the infinite as something consummated, as this is never permitted in mathematics.

However, with the foundation of set theory by Georg Cantor in the 1870s, and in particular with his "discovery" of "transfinite" numbers, the notion of an infinite totality as an object which was capable of being grasped by the mind at once began, slowly but constantly, to gain acceptance among mathematicians and philosophers, and it is nowadays customary. This is not to say, of course, that the debate between sustainers and opposers of the actual infinite has come to an end. Since the very time when Cantor was making his views on infinite sets and transfinite numbers public, he found the strong opposition of one of his teachers at the University of Berlin, Leopold Kronecker. Kronecker is a key figure in the history of some ideas that lay at the core of RA. He too explicitly maintained

[2] For a recent brief but in-depth survey on the relations between RA and the foundations of mathematics, see Tait (2012).

that in mathematics, as more generally in any field of reality, there was no place for an actual infinite, and expressed his deep disappointment towards such alleged mathematical objects as so-called "transfinite" numbers, this position being well testified by the following quotation, probably the best known by him:

> *Die ganzen Zahlen hat der liebe Gott gemacht, alles andere ist Menschenwerk.*
>
> God created the natural numbers, all else is the work of man.[3]

This quotation testifies to Kronecker's ontological view inspiring his philosophy of mathematics (described by several scholars as "concrete", "algorithmic" and "computational"; see, for instance, Edwards (1987, 1988, 1989, 1995) and Gana (1986); Gana (1986), in particular, offers an in-depth reconstruction of Kronecker's attempts to realize his project of reducing all mathematical entities to natural numbers): aside from natural numbers, there are no mathematical entities at all, but, at best, ways to describe "some aspects of the 'natural' calculus" (Gana (1986: 260)).[4]

Another interesting quotation from Kronecker (1895-1930: 156) reflects the way he borrowed and elaborated Gauss' insight (English translation from Edwards (1989: 71)):

> The general concept of an infinite series itself, for example a power series, is in my judgement permissible only with the reservation that in each particular case the arithmetical rule by which the terms are given

[3]As it is said in Boniface (2005: 144), "[t]his sentence was stated in a lecture for the *Berliner Naturforscher-Versammlung* (1886) and was quoted by Weber in his obituary" (Weber (1893: 15)).

[4]Translation from Italian mine.

satisfies, as above, conditions which make it possible to deal with the series as though it were finite, and thus to make it unnecessary, strictly speaking, to go beyond the notion of a finite series.

Besides, Kronecker was the first, at least in the modern age (one possible predecessor of his in the ancient age being Proclus) to make the claim that the existence of a mathematical object bearing some specified properties could only be stated by actually providing an object of that kind, after having "constructed" it according to certain suitable mathematical rules (hence the label of *constructivism* attributed to the complex of philosophical theories of mathematics sharing this fundamental position).

> *Der Standpunkt, welcher mich von vielen andern Mathematikern trennt, gipfelt in dem Grundsatz, dass die Definitionen der Erfahrungswissenschaften, — d.h. der Mathematik und der Naturwissenschaften, welche man neuerdings unter jenem Namen von den übrigen Wissenschaften, den sogen. Geisteswissenschaften trennt, — nicht bloss in sich widerspruchsfrei sein müssen, sondern auch der Erfahrung entnommen sein müssen, und was noch wesenlicher ist, das Kriterium mit sich führen müssen, durch welches man für jeden speziellen Fall entscheiden kann, ob der vorliegende Begriff unter die Definition zu subsumieren ist, oder nicht. Eine Definition, welche dies nicht leistet, mag von Philosophen oder Logikern gepriesen werden, für uns Mathematiker ist sie eine bloße Wortdefinition und ohne jeden Wert.*[5]
>
> The point of view on which I d[i]sagree with most mathematicians

[5]From Kronecker's 1891 lectures, the last ones he did; in Boniface & Schappacher (2002: 240).

> resides in the basic assertion that mathematics and the natural sciences — which have recently been separated by this name from the remaining sciences, the so-called sciences of the mind [...] — must not only be free of contradiction, but must also result from experience and, what is even more essential, must dispose of a criterium by which one can decide, for each particular case, whether the presented concept is to subsume, or not, under the definition. A definition which does not achieve this, can be advocated by philosophers or logicians, but for us mathematicians, it is a bad nominal definition. It is worthless.[6]

The link between Kronecker's "algorithmic" standpoint towards the foundations of mathematics and his refusal of the notion of a completed infinite is further explored in Edwards (1987, 1988, 1989, 1995) (see also Gana (1986)). See, in particular, the following quotations from Edwards (1989):

> [B]ased on my reading of Kronecker over the years, I regard it as a certainty that he would have regarded it as essential to show that this problem [the problem of deciding whether two given A's are equivalent modulo a given set of M's]—which he put at the heart of his formulation of mathematics—can be solved algorithmically, that is, *by a computational procedure which can be shown to terminate after a number of steps for which an* a priori *(finite) upper bound can be given.*[7]
>
> Kronecker did not believe in arbitrary infinite series, only in infinite series explicitly and constructively given.[8]

[6] English translation from Boniface (2005: 149).
[7] Edwards (1989: 69); emphasis mine.
[8] Edwards (1989: 71).

> Kronecker would probably have said that it was ridiculous to let the ultimate reality be the field [generated by the square root of -3, or the cube root of unity, over the rationals]—a completed infinite set that could never be explained to anyone but a mathematician[...].
>
> Kronecker's insistence on the specific and the algorithmic led him to one position that is too extreme even for most modern constructivists. Kronecker believed that *a mathematical concept was not well defined until it had been shown how to decide, in each specific instance, whether the definition was fulfilled or not.*[9]
>
> Already in Greek times, mathematicians understood very well that infinity was, as Gauss said, a "façon de parler," and that limits can be described—in fact are best described—without any mention of infinites. [...] [Kronecker's] principles, [...] in his mind and in fact, were no different from the principles of his predecessors, from Archimedes to Gauss.[10]

Finitism can be related to a broader current in the philosophy of mathematics, namely *predicativism*. Predicativism was first advocated by Jules Henri Poincaré, who, like Kronecker, felt a huge disappointment towards the emergence and diffusion of set theory as a foundation for mathematical sciences and towards the idea of the so-called "logicist" school, mainly represented by Frege and Russell, that logic took precedence over mathematics and was the proper basis on which to found the latter.[11] Poincaré's main claims in the philosophy of mathematics,

[9] Edwards (1989: 72); emphasis mine.
[10] Edwards (1989: 74).
[11] For a survey in English of Poincaré's philosophical positions about mathematics, see Gold-

first expressed in written form in his Poincaré (1905) and Poincaré (1906), reduce essentially to two. The first is the idea that any attempt of founding arithmetic on something else would have been unsuccessful;[12] in several places he points out, in particular, that the principle of mathematical induction should be presupposed by any logical theory (Poincaré (1905: 817–8)):

> *«[L]e principe d'induction complète» me paraissait à la fois nécessaire au mathématicien et irréductible à la logique. On sait quel est l'énoncé de ce principe:*
>
> *«Si une propriété est vraie du nombre 1, et si l'on établit qu'elle est vraie de $n+1$ pourvu qu'elle le soit de n, elle sera vraie de tous les*

---

farb (1988), on which I mostly base myself. Poincaré's philosophy of mathematics was highly influenced by Kant's (1787), who conceived arithmetic as the science of pure time.

[12]Despite the fact that I feel sympathetic with the idea that logic cannot serve as a foundation for mathematics, I find quite misleading (and ungenerous) the picture that Poincaré offers of logic. In this regard, there is a sense by which I agree with the conclusion of the following passage from Poincaré (1906: 315–6), but, under this sense, the conclusion, to me but certainly not to Poincaré, extends also to mathematics.

> *Une démonstration vraiment fondée sur les principes de la Logique Analytique se composera d'une suite de propositions; les unes, qui serviront de prémisses, seront des identités ou des définitions; les autres se déduiront des premières de proche en proche; mais bien que le lien entre chaque proposition et la suivante s'aperçoive immédiatement, on ne verra pas du premier coup comment on a pu passer de la première à la dernière, que l'on pourra être tenté de regarder comme une vérité nouvelle. Mais si l'on remplace successivement les diverses expressions qui y figurent par leur définition et si l'on poursuit cette opération aussi loin qu'on le peut, il ne restera plus à la fin que des identités, de sorte que tout se réduira à une immense tautologie. La Logique reste donc stérile, à moins d'être fécondée par l'intuition.*

> A proof truly grounded on the principles of Analytical Logic will be compounded by a collection of propositions; some of them, which will play the role of premisses, will be identities or definitions; the others will be deduced from the premisses step by step; but even if the link between each proposition and the following one can be seen immediately, it won't be apparent at first how it has been possible to pass from the first proposition to the last one, which we could be tempted to view as a novel truth. But if we replace the different expressions that appear there in sequence by their definitions and if we repeat this operation as long as possible, nothing will remain other than some identities, so that everything will reduce to a huge tautology. Hence, Logic remains sterile, until it is fecundated by intuition.

> *nombres entiers.» J'y voyais le raisonnement mathématique par excellence.*
>
> «[T]he principle of complete induction» seems to me at the same time necessary to the mathematician and irreducible to logic. We know what the enunciation of this principle is:
> «If a property is true of number 1, and if we have established that it is true of $n+1$ provided that it is true of n, then it will be true for all integers.» I see here the mathematical argument *par excellence*.

Poincaré's second claim is his *vicious circle principle*, which forbids definitions that are "non predicative" or, as it is nowadays more common to say, *impredicative*. The notion of an impredicative definition that Poincaré has in mind is that of a definition where in the *definiens* essential reference is made to the *definiendum* (Poincaré (1906: 307)):

> [*L*]*es définitions qui doivent être regardées comme non prédicatives sont celles qui contiennent un cercle vicieux.*
>
> Definitions that must be seen as not predicative are those that contain a vicious circle.

Definitions of this kind had already been considered as illegitimate for a long time. However, it is unclear if, before Poincaré, there was the same awareness that the problematic cases were not simply definitions where the same predicate appears both in the *definiendum* and in the *definiens*, like perfectly acceptable (and even necessary) inductive definitions, but rather definitions where reference to the same (generic) individual that should be defined is also made in the *definiens*.

Poincaré derived his notion of impredicativity from Richard (1905), where the homonymous paradox was introduced. To Poincaré, the source of all paradoxes that were shaking the mathematical building by its foundations in those years was that peculiar illegitimate sort of definitions, and, interestingly, a special case of this definition was obtained, according to Poincaré, by making essential reference to infinite totalities (Poincaré (1906: 316)):

> *C'est la croyance à l'existence de l'infini actuel qui a donné naissance à ces définitions non prédicatives. Je m'explique: dans ces définitions figure le mot* tous [...]. *Le mot* tous *a un sens bien net quand il s'agit d'un nombre fini d'objets; pour qu'il en eût encore un, quand les objets sont en nombre infini, il faudrait qu'il y eût un infini actuel. Autrement* tous *ces objets ne pourront pas être conçus comme posés antérieurement à leur définition et alors si la définition d'une notion N dépend de tous les objets A, elle peut être entachée de cercle vicieux, si parmi les objets A il y en a qu'on ne peut définir sans faire intervenir la notion N elle-même.*
>
> Il n'y a pas d'infini actuel*; les Cantoriens l'ont oublié, et ils sont tombés dans la contradiction.*

> It is the belief in the existence of the actual infinity that gave birth to these non predicative definitions. Let me explain myself: in these definitions the word *all* appears [...]. The word *all* has a very precise meaning when it applies to a finite number of objects; in order for it to have one more, when the objects are an infinite number, an actual infinity would be necessary. Otherwise *all* these objects could not be

> known as established before their definition and so, if the definition of a notion N depends on all the objects A, it can be affected by a vicious circle, if among the objects A there is one which cannot be defined without the intervention of the notion N itself.
>
> *There is no actual infinite*; Cantorians forgot it, and they fell into contradiction.

At this point, it is worth mentioning another prominent figure in the history of constructivist philosophies of mathematics, namely the Dutch mathematician Luitzen Egbertus Brouwer (1881–1966), because he was the founding father of *intuitionism*, today by far the most common and widely followed variety of constructivism, which nevertheless should be viewed as separate from the philosophical perspectives embodied in RA.[13] Maybe, Brouwer's most original claim on the foundations of mathematics is his refusal of a logical principle with a respectable and long-dated pedigree, namely the so-called *Principle of Excluded Middle* (EM), which was first stated by Aristotle himself in his *Metaphysics* (see Aristotle (1984)) and which states, in one of its formulations, that, given a certain proposition, either the proposition or its negation must hold. Many people working on the philosophy of mathematics seem to endorse the view that this refusal is a coherent result of constructivism. To suggest that this may not be the case, it may be useful to quote the enlightening preface of Goodstein (1951: 9), where Goodstein assigns a crucial role to Ludwig Wittgenstein's *Tractatus Logico-Philosophicus* in contrasting Brouwer's refusal of EM:

[13] In his famous doctoral thesis, Brouwer, who stressed the kantian conception of arithmetic as the science of pure time extending it to all mathematics, endorsed constructivism in a peculiar way, by claiming that mathematical objects are the product of the activity of a creative subject. He shared Poincaré's disappointment with logicism and similarly defended the priority of mathematics over logic (and, in the same regard, over language).

The main forces which have shaped the foundations of mathematics over the past twenty-five years have been antithetical in purpose but complementary in effect. To Hilbert's formalism we owe the detailed analysis of the structure of mathematical systems and the imaginative conception of mathematics as its own object of discourse; to the constructivism of Brouwer, the critique of classical logic and the intuitive notion of a finitist proof.

The formalist-finitist controversy in the foundations of mathematics was resolved, in principle, by Wittgenstein's analysis of the characteristics of a formal language. Wittgenstein showed that in a formal language the *meaning* of the signs is a purely *functional* property of the language; it follows that Brouwer's denial of the *validity* of a formal axiom—the *tertium non datur*—was totally mistaken. The conclusion to be drawn from the finitist critique is not that certain parts of mathematics are incorrect but that the currently accepted interpretation of the signs, in particular the interpretation of the quantifiers A and E[14] in terms of universality and existence is *untenable*. One cannot dispute a formal equivalence like

$$\sim Ax(P(x)) = Ex(\sim P(x))$$ [15]

but one may well be able to show that the use of the quantifiers A and E in the formula is not consistent with the ordinary usage of the terms "for all" and "there exists", so that in a system in which this

[14]Today, they would normally be indicated as '$\forall$' and '$\exists$', respectively [A/N].

[15]Again, the sign '$\sim$' for negation would be more normally written, today, as '$\neg$' [A/N].

> formula holds, "A" and "E" are not synonymous with the universal and existential operators.

Wittgenstein too, who was Goodstein's teacher and was cited several times in his writings, can be viewed as endorsing finitism in his so-called "middle period", i.e. the period when he wrote his *Philosophical Remarks* (1929–30; Wittgenstein (1964)) and his *Philosophical Grammar* (1931–33; Wittgenstein (1969)). Relevant quotations testifying Wittgenstein's finitist attitude may be the following ones:

> An irrational number isn't the extension of a decimal fraction, [...] it's a law. (Wittgenstein (1975: §181))

> [T]he mistake in the set-theoretical approach consists time and again in treating laws and enumerations (lists) as essentially the same kind of thing. (Wittgenstein (1974: 461))

> It is senseless to speak of the whole infinite number series, as if it, too, were an extension. (Wittgenstein (1975: 144))

It is interesting to note that in his middle period, Wittgenstein rejected his own earlier view, expressed in the *Tractatus Logico-Philosophicus*, that universal and existential quantifiers amount, respectively, to infinitary conjunction and disjunction.[16] In other words, he came to reject the notion of an "unbounded quantifier" which is at the basis of classical first order logic (CL), in this way aligning with the spirit feeding RA:[17]

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[16] See the discussion on unbounded quantification in §7.1.
Curiously, this theory was quoted as a source of inspiration for GB theory by Chomsky in his Pisa lectures (see Graffi (2001: 451)).

[17] Wittgenstein probably went a step further with respect to RA in apparently rejecting generalizations *tout court*, as the following quotations seem to suggest:

> What is the meaning of such a mathematical proposition as '$(\exists n)4+n = 7$'? It might be a disjunction — $(4+0=7) \vee (4+1=7) \vee$ etc. *ad inf.* But what does that mean? I can understand a proposition with a beginning and an end. But can one also understand a proposition with no end? (Wittgenstein (1975: §127))

> The important point is that, even in the case where I am given that $3^2+4^2=5^2$, I ought not to say '$(\exists x, y, z, n)(x^n + y^n = z^n)$', since taken extensionally that's meaningless, and taken intensionally this doesn't provide a proof of it. No, in this case I ought to express only the first equation. (Wittgenstein (1975: §150))

The setting just described can explain why the emergence of paradoxes in naïve set theory and related formal systems (for instance, Frege's logical foundation of arithmetic), such as Cantor's (1895), Burali-Forti's (1897), Russell's (1902), Richard's (1905), König's (1905), Bernstein's (1905), Berry's (1906), Grelling & Nelson's (1908), together with the first mathematical results which formally reproduced some features of these paradoxes, starting with the famous Gödel incompleteness theorems (stated in Gödel (1931)), encountered little surprise among constructivist and finitist mathematicians. In the words of Hermann Weyl:

> According to [Brouwer's] view and reading of history, classical logic was abstracted from the mathematics of finite sets and their subsets. [. . .] Forgetful of this limited origin, one afterwards mistook that logic

[O]ne [should not] say a general proposition follows from a proposition about the nature of number. (Wittgenstein (1975: §126))

[I]t seems to me that we can't use generality—all, etc.—in mathematics at all. (Wittgenstein (1975: §129)) for something above and prior to all mathematics, and finally applied it, without justification, to the mathematics of infinite sets. This is the Fall and original sin of set theory, for which it is justly punished by the antinomies. *It is not that such contradictions showed up that is surprising, but that they showed up at such a late stage of the game.* [italics mine]

Chapter 3

Primitive recursion

One, two, three, four!
DEE DEE RAMONE

In 1888, Richard Dedekind published a seminal work on number theory, Dedekind (1888), where for the first time a suitable set of axioms for arithmetic was presented:[1] it was equivalent to a more famous set of axioms developed a little later by Giuseppe Peano and which, accordingly, are now both universally known under the label of Peano axioms or Dedekind-Peano axioms. Furthermore, Dedekind (1888) also introduced the notion of a *primitive recursive function* (at that time, simply called *recursive function*; henceforth, also *p.r. function*),[2] and showed

[1]Dedekind's work was actually a substantial improvement of the work done on the same subject by the mathematician and linguist Hermann Grassmann in Grassmann (1861).

[2]Actually, as Tait (2012) points out, "Dedekind proved the principle of definition by iteration, not the general principle of primitive recursive definition. As Peter Aczel pointed out in conversation, the latter principle does seem to have appeared explicitly for the first time in Skolem (1923)". However, as Tait (2012) further remarks, Robinson (1947) built a reduction of primitive recursion to pure iteration. In the same regard, Meyer & Ritchie (1967b,a), showed the equivalence between p.r. functions and functions computable by 'for' programs, programs in a programming language including instructions of the shape 'for Y do S', meaning 'iterate S for Y times', but not free iterations without an upper bound (see also Odifreddi (1989: 70 f.)). Much work by Giorgio Germano and Stefano Mazzanti is also devoted to this topic; see for instance Germano & Mazzanti (1988), where it is said that "the motivation for investigating prim$(\mathbb{N}, \mathbb{N})$

that many mathematical functions employed in everyday mathematical practice were definable in terms of primitive recursion.[3] The more general notion of 'primitive recursion' which mathematicians and logicians are acquainted with, however, was developed by Skolem (1923) more than thirty years later; for some years, this notion must have appeared so natural and elegant that Hilbert (1926) even formulated the conjecture that it corresponded formally to the intuitive notion of a computable function.[4]

I will describe here a first formal system, which I will call $\mathcal{PR}$, which allows one to formally derive equations with p.r. functions and which requires only a simple and natural extension to be transformed into a system of primitive recursive arithmetic, as we will see in the next chapter.

The alphabet $Alph_{\mathcal{PR}}$ of $\mathcal{PR}$ is the following one:[5]

$$Alph_{\mathcal{PR}} = \{0, g, \star, S, =, [,]\}.$$

The symbol 'S' corresponds to the *successor function*, which is assumed to be the only *basic function* of $\mathcal{PR}$; besides, we have also one and only one *basic term*, namely '0'.

I will call any sequence of 'g's *generic variable* and any sequence of stars

[i.e., the class of primitive recursive unary functions] may be found in the *beauty* and *simplicity* of the underlying data structure" (Germano & Mazzanti (1988: 218), italics mine; to be paired with Odifreddi's quotation at p. 35 below).

[3] Grassmann (1861) had already defined sum and product in primitive recursive terms.

[4] This was, however, before Gabriel Sudan in 1927 and, with greater notoriety, Wilhelm Ackermann in 1928 had described their homonymous alleged functions, which are not primitive recursive and would hence show the computational limitations of primitive recursion.

[5] Here and in the following, I often borrow the notation and terminology of set theory, since it is widespread and almost standard when dealing with such subjects. This could appear incoherent, given my sympathies for kroneckerian finitism; however, it should be noted that, since, when I use set-theoretical notions, I am always dealing with finite sets only, their employment is absolutely innocent in this respect.

placeholder.

A is a *term* iff one of the following cases holds:

a) A is 0;

b) A is a generic variable;

c) A is SB, where B is a term;

d) A is $[B\ \varphi(p)]^p C$, where B and C are terms, p is a placeholder and $\varphi(p)$ is a string obtained from a term $\varphi\langle p\rangle$ not containing p by replacing at least one term in it with an occurrence of p.

Something is an *equation* iff it is '$A = B$', where A and B are terms.

I will call both the symbol 'S' and any sequence of symbols '$[A\ \varphi(p)]^p$' *primitive recursive unary function* (hereafter, also simply *p.r.u. function*), where A is a term, p is a placeholder and $\varphi(p)$ is a string obtained from a term $\varphi\langle p\rangle$ not containing p by replacing at least one term in it with an occurrence of p. Given a term A containing a p.r.u. function, if f is the leftmost occurrence of a function contained in A and B is the rightmost occurrence of a term contained in A, I will say that B is the *argument* of f.

The notion of *scope* is defined here among terms, as simple containment: an occurrence $o(A)$ of a term A is in the scope of a term B iff B contains $o(A)$.

I think that the choice of restricting the field only to p.r.u. functions, i.e. (p.r.) functions with one single argument, has several welcome consequences: first of all, it allows for a more compact and elegant description of the whole formal system; secondly, it also allows for a much more natural treatment of quantification in natural language.[6]

But what do we lose when we decide to deal only with unary functions? Of course, the viability of such an option presupposes that there is a relevant sense under which the right answer to this question is "nothing". And so it is, in fact: given a suitable formal system $\mathcal{PR}'$ for p.r. functions without limitations of ariety, we can build an effective procedure $m : L_{\mathcal{PR}'} \mapsto L_{\mathcal{PR}}$ mapping equations of $\mathcal{PR}'$ into equations of $\mathcal{PR}$ such that, if '$\vdash_{\mathcal{PR}}$' is the relation of entailment between equations of $\mathcal{PR}$ and '$\vdash_{\mathcal{PR}'}$' is the relation of entailment between equations of $\mathcal{PR}'$ and α and β are equations of $\mathcal{PR}'$, then $\alpha \vdash_{\mathcal{PR}'} \beta$ iff $m(\alpha) \vdash_{\mathcal{PR}} m(\beta)$. Thus, we can say that the inferential properties of the system do not change by allowing functions of ariety greater than 1.

This fact can be seen as a primitive recursive version of an observation first made by Schönfinkel (1924). Moreover, it extends readily to the expansions of $\mathcal{PR}$ I am going to discuss below (in particular, to the system of recursive arithmetic $\mathcal{RA}$).

However, there is also a sense in which something gets lost. In fact, given again a suitable formal system $\mathcal{PR}'$ for p.r. functions without limitations of ariety, we cannot build an effective procedure $m : F_{\mathcal{PR}'} \cup \mathbb{N}^\times \mapsto F_{\mathcal{PR}} \cup \mathbb{N}$ mapping n-ary p.r. functions into p.r.u. functions, n-tuples of numbers into numbers and terms of $\mathcal{PR}'$ into terms of $\mathcal{PR}$ such that, if '$\vdash_{\mathcal{PR}}$' is the relation of entailment between equations of $\mathcal{PR}$ and '$\vdash_{\mathcal{PR}'}$' is the relation of entailment between equations of $\mathcal{PR}'$, then $0 = 0 \vdash_{\mathcal{PR}'} f^n(N) = A$ iff $0 = 0 \vdash_{\mathcal{PR}} m(f^n)(m(N)) = m(A)$ for any f^n, N and A. As Robinson (1947) showed, such a result would only be possible

[6] Besides, a connection can even be seen between this issue and the need for simply binary branching advocated in generative syntax in works like Kayne (1981, 1994), Larson (1988) and Chomsky (1993, 1995).

by adding at least one binary recursive function to the system. In any case, I cannot see how this last circumstance could be viewed as a limitation, especially for present purposes.

I will almost always use some abbreviations to designate terms of $\mathcal{PR}$. As an example, we may see the definitions of some elementary arithmetic operations such as sum (in symbols, '+') and product (in symbols, '$\cdot$'), which will play a crucial role in the following (the symbol ':=' is used here for definitional identity):

$$(A + B) := [A\ S\star]^\star B; \tag{D1}$$

$$\begin{aligned}(A \cdot B) &:= [0\ (\star + A)]^\star B \\ &:= [0\ [\star\ S \star\star]^{\star\star} A]^\star B.\end{aligned} \tag{D2}$$

I will call a term containing only 'S' and '0' as symbols *number*; I will call both a generic variable and a number *index*. I will say that a term of $\mathcal{PR}$ is a *closed* term if and only if it does not contain variables. Similarly, I will say that a p.r.u. function not containing variables is a *closed* p.r.u. function.

The central notion of $\mathcal{PR}$, as a deductive system, is that of a *rule of inference*, that I am going to define through some schemata. In those schemata, a capital Latin letter stands for a term, v is a generic variable, $\phi(A)$ is an equation containing A, $\phi(A/B)$ is the equation obtained from $\phi(A)$ by substituting all occurrences of A in it with a term B such that B does not contain placeholders contained in $\phi(A)$, $\varphi(A)$ is a term possibly containing A, $\varphi(A/B)$ is a term obtained from $\varphi(A)$ by replacing any occurrence of A in it with B, α is an equation, p is a placeholder, $\varphi(p)$ is a string obtained from a term $\varphi\langle p\rangle$ not containing p by replacing at least one term in it with an occurrence of p and $\varphi(p/A)$ is a term obtained from $\varphi(p)$

by substituting all occurrences of p in it with A.

So, each syntactic object obtainable by substitution of equals with equals from the following schemata is a rule of inference of $\mathcal{PR}$.

Rule 3.1 (Transitivity: Goodstein's (1954: 247) T)

$$\frac{A = B \qquad A = C}{B = C} \quad (T)$$

Rule 3.2 (Particularization: Goodstein's (1954: 247) Sb$_1$)

$$\frac{\phi(v)}{\phi(v/A)} \quad (P)$$

Rule 3.3 (Substitution: Goodstein's (1954: 247) Sb$_2$) [7]

$$\frac{A = B}{\varphi(C/A) = \varphi(C/B)} \quad (S)$$

Rule 3.4 (Function Application)

$$\frac{\alpha}{[A\ \varphi(p)]^p 0 = A} \quad (F_0)$$

$$\frac{\alpha}{[A\ \varphi(p)]^p SB = \varphi(p/[A\ \varphi(p)]^p B)} \quad (F_S)$$

A rule of inference is, thus, a syntactic object consisting of an horizontal line, an equation-schema immediately below it and one or more equation-schemata

[7] This rule may be seen as a formal version of Leibniz's Principle of the Indiscernibility of Identicals, despite contrary opinions. This principle is taken to be part of the meaning of what is known as Leibniz's Law, expressed in his *Discourse on Metaphysics*, Section 9; see Leibniz (1969: 308). For the problem of establishing if Leibniz explicitly held this principle attributed to him, see Feldman (1970).

immediately above it. The equation-schemata above the line are also-called the *upper equation-schemata* of the rule, while the one below the line is called the *lower equation-schema*.

A *derivation* consists of a rooted tree (in the technical sense this expression has in graph theory) in which the nodes are equations and the edges are labeled lines interposed to the equations they connect as in a rule of inference. Each equation α in a derivation, except from its leaves, at the top of the tree, is inferred by one of the rules of inference given above, i.e. there is a rule of inference r such that α is obtainable by substitution from the lower equation-schema of r and its daughters can be accordingly obtained by substitution from the upper equation-schemata of r[8] in such a way that, taking into account α and its daughters collectively, the same metavariables of r have been replaced by the same items. The tag of the rule of inference is the label of the edge connecting α to its daughters; I will write it on the left of the lines horizontally separating the equations.

If $\vec{\alpha}$ are all the equations, without repetitions, appearing in the leaves of a derivation in $\mathcal{PR}$ whose root is the equation β, then I will say that β has been *derived* in $\mathcal{PR}$ from $\vec{\alpha}$ and I will write '$\vec{\alpha} \vdash_{\mathcal{PR}} \beta$'; if in all the leaves of a derivation there is the equation '$0 = 0$' and its root is the equation α, then I will also call that derivation a *proof* and I will say that α has been *proved* in $\mathcal{PR}$.[9] If $\alpha \vdash_{\mathcal{PR}} \beta$ and $\beta \vdash_{\mathcal{PR}} \alpha$, I will also write '$\alpha \dashv\vdash_{\mathcal{PR}} \beta$'.[10]

[8]As for the upper equation-schemata, which are possibly more than one, this precisely amounts to saying that α's daughter in the i^{th} position from left to right can be obtained by substitution from the i^{th} upper equation-schema, again from left to right, of r.

[9]The equation '$0 = 0$' plays, in a system of RA employed to model human reasoning, the same role that '$\top$' plays in CL, i.e., more or less, that of an autoevident truth, from which any truism may be derived (see Goodstein (1957)).

[10]In the following, I will always use metavariables over syntactic objects of $\mathcal{PR}$, still speaking simply of "derivations" and "proofs" instead of *derivation-schemata* and *proof-schemata* respectively, as would be more correct.

An example of derivation in $\mathcal{PR}$ can be found in §B.2.

We could prove that there is an effective procedure to establish, for any (finite) set of equations $\bigcup_{i=1}^{n}\{\alpha_i\}$ of $\mathcal{PR}$ and any equation β of $\mathcal{PR}$, if $\bigcup_{i=1}^{n}\{\alpha_i\} \vdash_{\mathcal{PR}} \beta$. This result, even if limited to equations not containing terms other than closed ones, arises as a consequence of a theorem stated in Meyer (1965) and Ritchie (1965) and proved in Meyer & Ritchie (1967b,a) (based on results in Axt (1963, 1965) and Cobham (1964); see also Odifreddi (1999: 297)).

Now, we are in a position to define the behaviour of a p.r.u. function in accordance with the standard terminology: we can say that the *value* of a closed p.r.u. function f for the closed argument A is the number N iff $0 = 0 \vdash_{\mathcal{PR}} fA = N$.

In the following, I will conform to a quite standard usage by employing the first of the two notations below in place of the latter one:

(1) $$\begin{cases} f0 = A \\ fSx = \varphi(p/x) \end{cases} \quad := \quad f := [A\ \varphi(p)]^p$$ [11]

The usual notion of "function" of course does not correspond to that of a "p.r.u. function", nor does the usual notion of "operation". However, I will continue to employ both these terms quite freely, since they are very common, but with the specification that here they are conceived as simply denoting term-schemata. In accordance with the standard convention, I will also possibly employ the terms "functor" and "operator", meaning the syntactic object which is used to build, respectively, a certain function or a certain operation, but again with the implicit

[11] Note that the identity of two terms differing only in the placeholders they contain (i.e., such that each one can be obtained from the other by substitution of equals with equals among the placeholders it contains) cannot still be proven in $\mathcal{PR}$: it needs (U) (combined with (S)), the rule of inference introduced in $\mathcal{RA}$, to prove it.

assumption that the function and the operation are interpreted as above.

The following functions, called respectively the *predecessor* function (with functor 'P') and the *integer difference* function (with functor '$\dot{-}$'), are crucially employed, following Hilbert & Bernays (1934), to define in primitive recursive terms some central logical operators like connectives and bounded quantifiers (see §6.2 and §7.1, respectively):

(D3)
$$\begin{cases} P(0) = 0 \\ P(SA) = A \end{cases}$$

(D4)
$$\begin{cases} A \dot{-} 0 = A \\ A \dot{-} SB = P(A \dot{-} B) \end{cases}$$

It is useful to already introduce the following abbreviations, which I will employ further in many semantic representations. They are, respectively, the standard logical *conjunction*, logical *disjunction*, logical *negation* and *material implication* and the relation of being *less than or equal to*:

(2) $(A \wedge B) := (A + B);$

(3) $(A \vee B) := (A \cdot B);$

(4) $\neg A := 1 \dot{-} A;$

(5) $(A \rightarrow B) := (\neg A \vee B);$

(6) $(A \leq B) := (A \dot{-} B).$

The following ones are, instead, the definitions of new functors, namely bounded universal and existential quantifiers, respectively:

(D5)
$$\begin{cases} (\forall x \leq 0)\varphi(x) = \varphi(0) \\ (\forall x \leq SA)\varphi(x) = \varphi(SA) \wedge (\forall x \leq A)\varphi(x) \end{cases}$$

(D6)
$$\begin{cases} (\exists x \leq 0)\varphi(x) = \varphi(0) \\ (\exists x \leq SA)\varphi(x) = \varphi(SA) \vee (\exists x \leq A)\varphi(x) \end{cases}$$

Subsequently, I will adopt the usual conventions to reduce the number of parentheses. In particular:

a) the pair of outermost parentheses is omitted;

b) product takes precedence over sum and integer difference; hence, for instance, '$A + B \cdot C$' is shorthand for '$A + (B \cdot C)$';

c) conjunction and disjunction take precedence over material implication; hence, for instance, '$A \to B \wedge C$' is shorthand for '$A \to (B \wedge C)$';

d) in all cases not covered by (b) and (c), operators associate on the left; hence, if $\bullet$ and $\circ$ are binary operators, then '$A \bullet B \circ C$' is shorthand for '$(A \bullet B) \circ C$'.

Chapter 4

Recursive arithmetic

4.1 A logic-free system of recursive arithmetic

Building on Dedekind's work, Skolem (1923) developed a formal deductive system for arithmetic which was intended to avoid the paradoxes which emerged in the foundations of mathematics at the end of the Nineteenth century and the beginning of the Twentieth; this system and other formal systems equivalent to it are now known as (*Primitive*) *Recursive Arithmetic* (henceforth, also simply 'RA').

Basing himself on insights first formulated in Hilbert & Bernays (1934) and developed in Gödel (1931), Goodstein (1954) simplified Skolem's (1923) original system by formulating it in a logic-free shape, overcoming a philosophical problem raised by an analogous system of Curry.

The possibility of building RA dispensing with the incorporation of some logical notions as basic may be viewed as giving substance to Poincaré's claims about the priority of mathematics over logic and the irreducibility of the principle of mathematical induction to any set of logical principles. Something close to this view can also be found in the following quotation from Hilbert (1926: 376):[1]

> Kant already taught—and indeed it is part and parcel of his doctrine—that mathematics has at its disposal a content secured independently of all logic and hence can never be provided with a foundation by means of logic alone; that is why the efforts of Frege and Dedekind were bound to fail. Rather, as a condition for the use of logical inferences and the performance of logical operations, something must already be given to our faculty of representation, certain extra logical concrete objects that are intuitively present as immediate experience prior to all thought. If logical inference is to be reliable, it must be possible to survey these objects completely in all their parts, and the fact that they occur, that they differ from one another, and that they follow each other, or are concatenated, is immediately given intuitively, together with the objects, as something that neither can be reduced to anything else nor requires reduction. This is the basic philosophical position that I consider requisite for mathematics and, in general, for all scientific thinking, understanding, and communication. And in mathematics, in particular, what we consider is the concrete signs themselves, whose shape, according to the conception we have adopted, is immediately clear and recognizable.

Actually, for some scholars logic-free systems of RA not only show that there are mathematical concepts independent from logical ones, but, moreover, that the latter ones depend on the former.

[1] The reference is to the English translation.

The $\mathcal{RA}$ system of recursive arithmetic introduced here is an extension of the previous $\mathcal{PR}$ system, obtained by simply adding the following rule of inference (and modifying accordingly, when necessary, the definitions of the syntactic objects of $\mathcal{PR}$), where $\Phi(A)$ (with Φ equal to either φ or ψ or χ) is a term containing the term A and $\Phi(A/B)$ is a term obtained from $\Phi(A)$ by substituting all occurrences of A in it with a term B:

Rule 4.1 (Uniqueness of Recursion: Goodstein's (1954: 248) U_1) [2]

$$\frac{\varphi(0) = \psi(0) \qquad \varphi(S(v)) = \chi(A/\varphi(v)) \qquad \psi(S(u)) = \chi(A/\psi(u))}{\varphi(A) = \psi(A)} \quad (U)$$

The resulting system corresponds to Goodstein's (1954) system of RA, apart from the peculiar way I adopted to syntactically represent functions defined by recursion.

Examples of derivations in $\mathcal{RA}$ can be found in §B.3.

It is worth quoting the following passage from Goodstein (1951: 9–10), where the link between RA and Poincaré's idea that logic should be considered as reducible to mathematics, while not *vice versa*, is made clear:

[2]This is not precisely Goodstein's (1954) original formulation, but it can be immediately obtained from it by successive applications of (S) and is intended to make some derivations easier.

Given the other rules of inference, the Uniqueness of Recursion can be derived from a suitable formalization of the more common induction principle, and *vice versa* (see Goodstein (1945) for a proof). For the informal characterization of this principle given by Poincaré, see the quotation at p. 14 above (even if nowadays it is far more common to enunciate it by making reference to the number 0 instead of 1).

As far as I know, the induction principle, applied very early in the history of mathematics, was first explicitly stated in Grassmann (1861). The uniqueness of primitive recursion, however, appears to be, in Goodstein's (1972: 281) words, "a far more intuitively acceptable notion" than induction; Goodstein (1972: 280–1), besides, attributes the justification of induction in terms of the uniqueness of the function defined by the recursive definition to Wittgenstein (see Wittgenstein (1969)).

> Constructivism and formalism found a point of contact in *recursive number theory* which was developed by Skolem, and by Herbrand and Gödel in their construction of non-demonstrable propositions. Recursive number theory plays a fundamental part in the fusion of these two modes of thought in the present work.
>
> The aim of *constructive formalism* is to replace the intuitive notion of a finitist proof by the strictly formal property of demonstrability in a formal system. This is accomplished by the construction of a mathematical system—the equation calculus—which operates independently of the axioms and constants of logic. This system affords a means of proving certain types of logical formulæ and consequently effects a reduction of logic to mathematics.
>
> The necessity for some equivalent of the theory of types involves any system founded upon the concept of class in intolerable complications, but even apart from questions of expediency there are good grounds for denying the class concept a primary part in a mathematical system. The equation calculus gives to *function* the fundamental role that classical analysis assigns to related classes. A function is defined by the introductory equations of its sign, which by means of the transformation rules of the calculus, serve to transform the function-sign into a definite numeral, when definite numerals are assigned to the argument places in the function-sign.

Anticipating what will turn out to be one crucial issue in this dissertation, one main difference between $\mathcal{RA}$ and classical logic (hereafter, simply 'CL') is that, in $\mathcal{RA}$, quantifiers are no longer primitive symbols of the formal language, but can only be defined as special recursive functions and, moreover, they can only be defined in a way which makes them have a different property with respect to traditional quantifiers, namely that of being *bounded*. A bounded universal quantifier ranging over natural numbers, for instance, can be used to convey the information that all numbers up to a certain one have a given property: it cannot be used to say, however, that any natural number has that property. To say something like that, the only way that $\mathcal{RA}$ has at its disposal is that of employing generic variables (which, in fact, can be substituted by any number through Particularization): but, crucially, generic variables are not p.r.u. functions and, thus, they are scopeless.

Despite the apparent limitations derived from not having the unbounded versions of quantifiers at its disposal,[3] $\mathcal{RA}$ turns out to be a very powerful system. Tait (1968, 1981, 2002, 2005) defended the idea that $\mathcal{RA}$ can capture all finitist reasoning. Besides, Feferman & Strahm (2010) provided further technical support to that claim and Feferman (1990) demonstrated that one can design a theory which is expressive enough to capture computational practice but which has "only" the computational strength of $\mathcal{RA}$.[4] In a similar vein, Odifreddi (1999: 286) states

[3]Which, however, can be seen as meaningful only from a theory-dependent perspective.

[4]Actually, several scholars have advanced the idea that even strictly weaker systems can do the job. I should mention at least Harvey Friedman's "grand conjecture", which implies that many mathematical theorems, such as Fermat's last theorem, can be proved in Elementary Function Arithmetic (also-called "Exponential Function Arithmetic"; abbreviated, EFA), a system strictly weaker than $\mathcal{RA}$. The original statement of the conjecture, formulated in 1999, is the following:

> Every theorem published in the *Annals of Mathematics* whose statement involves only finitary mathematical objects (i.e., what logicians call an arithmetical statement) can be proved in EFA. EFA is the weak fragment of Peano Arithmetic based on the usual quantifier-free axioms for 0, 1, +, ×, exp, together with the scheme of induction for all formulas in the language all of whose quantifiers are bounded.

that "[t]he class of p.r. functions is probably the most natural subrecursive class".

Finally, notice that rule (P) of Particularization, essentially involving generic variables, formally corresponds to the familiar rule of Elimination of the Universal Quantifier $(\forall E)$ in CL; for this reason, and for the technical result in 4.1 below showing that, under certain conditions, generic variables behave exactly like universal quantifiers as definable in primitive recursive terms, I will say that generic variables are *universal-alike*. Besides, since generic variables are not functions, the rule can apply taking as its premise any equation containing a generic variable, unlike $(\forall E)$, which cannot directly apply if the universal quantifier is embedded under another logical operator: in other words, generic variables behave like widest scope quantifiers, while they behave, between themselves, as if they were scope insensitive (this last fact is analogous to what happens with traditional universal quantifiers, since, if there is no other logical operator taking intermediate scope between them, inverting their reciprocal scope makes the starting equation turn into an equivalent one).

Theorem 4.1 $v \leq n \rightarrow \varphi(v) \dashv\vdash_{\mathcal{RA}} (\forall x \leq n)\varphi(x)$.

The sense from right to left corresponds to Goodstein's (1957: 84) theorem 3.832, given his Deduction Theorem (Goodstein (1954: 255) and Goodstein (1957: 114)). The sense from left to right arises as an immediate consequence of Goodstein's (1957: 84–5) theorems 3.81 and 3.95 (*via*, again, the Deduction Theorem). □

4.2 Grounding the formal system

Finally, I also define an extension of $\mathcal{RA}$, leading to the definitive system which I will adopt as the background formal system below and which I will dub $\mathcal{RA}^t$. $\mathcal{RA}^t$ differs from $\mathcal{RA}$ only in a minimal way, starting from the introduction of a new symbol in the alphabet:

$Alph_{\mathcal{RA}^t} = Alph_{\mathcal{RA}} \cup \{t\}$.

t is called the *indexical term*, and it is a (basic) term of $\mathcal{RA}^t$: all the definitions of the other syntactic objects of $\mathcal{RA}^t$ are identical to those of the syntactic objects of $\mathcal{RA}$, since t does not play a particular role in any rule of inference; the reasons behind its introduction will be explained in §6.4.

However, we can already observe that, because its both playing the role of an argument within $\mathcal{RA}^t$ terms and also not being essentially involved in any rule of inference of the system, it is the only element of $\mathcal{RA}^t$ which has the features of a pure rigid designator (in Kripke's (1980) famous sense). For this reason, it is after its introduction that I want to define (somehow in a still mysterious fashion, for the moment) a notion of consistency for a set of formulæ $\bigcup_{i=1}^{n}\{\alpha_i\}$ of $\mathcal{RA}^t$: $\bigcup_{i=1}^{n}\{\alpha_i\}$ is *consistent* iff, for some number N and for any $1 \leq i \leq n$, $t = N \vdash_{\mathcal{RA}^t} \alpha_i$.

Note that this notion of consistency somewhat differs from the standard one, according to which a set of formulæ is consistent iff it does not derive both a formula and its logical negation. First of all, we should observe that, in the logic-free version of $\mathcal{RA}^t$ I am adopting, negation is not a primitive symbol and so needs to be defined as a p.r.u. function (as in §(4)): it would be, thus, quite odd if such a general metalinguistic notion should have to rely on a derived concept.

Secondly, and much more importantly, the problem with the standard definition is that, from a finitist point of view, no one sees how the relevant notion of derivation should be bounded there, i.e. up to what length (arguably defined in functional terms) derivations should be checked in order to decide if a given entity bears the property or not.

Part II

Recursive arithmetic and natural language semantics

Chapter 5

Terms of recursive arithmetic as semantic representations

5.1 A remark on ontology

Considerations of ontology may play a central role in developing a semantic theory for natural languages. In particular, this has been the case within the dominant paradigm of modern semantics, namely model-theoretic semantics, as conceived, for instance, by Davidson and Montague.

The first thing to keep in mind before trying to outline a semantic theory built upon RA is probably, thus, the kind of objects RA is designed to deal with, i.e. natural numbers. *Prima facie*, this feature may appear as a severe limitation affecting a similar enterprise from the very beginning. Natural numbers, however, have been shown for a long time to be capable of suitably representing a wide range of empirical data:[1] in particular, given whatever (finite) set of atomic entities (no

[1] Gödel's (1931) famous arithmetization of the syntax of Peano Arithmetic can be viewed as

matter, here, if they have to be conceived as perceptual atoms or not) and whatever (finite) set of operations combining tokens of these atoms to obtain (finite) combinations, the objects thus obtained may be effectively (i.e., by a specified algorithmic procedure) put in a bijective correspondence with natural numbers. We say that, in so doing, we have *coded* our objects through natural numbers, and that the procedure we have followed for that purpose is a *coding* function.[2] This can be done, for instance, by considering a total order of the objects and associating its ordering number to each of them. The most common total order employed for that purpose is by far the *shortlex order* (also, "quasi-lexicographic order", "radix order" and "length-plus-lexicographic order"), which assigns priority to the shortest objects (under a relevant specified parameter of length) and then, to the objects with the same length, assigns priority following a given alphabetical ordering (see for instance Quine (1946: 113), and Kantrowitz (2000: 41) for more explicit descriptions).

Coding procedures are entering into the folk culture, in an era where computers, which are able to handle an impressive amount of different data (some of which happen to have a perceptual correlate, given that computers make one see images in their monitors and hear sounds through their woofers) by ultimately coding them with natural numbers in a binary numerical base, play such an important role in everyday life.

Hence, given this state of affairs, the ontological problem significantly shifts to that of establishing if an ontology entirely based on discrete finite entities suffices

an early example of procedures designed for that purpose. I will come back to this subsequently.

[2]Given the standard terminology, this is not a precise way of stating. However, it seems to me that, under finitist assumptions, no other status can be assigned to functions than that of procedures (of a peculiar sort, maybe).

for the aim of building a suitable semantic theory for natural languages. If this problem and the ontological problem of determining if there are only discrete finite entities in reality coincide, it is a much more intricate and, thus, debatable subject; I suspect that they do, however, and this should be taken, I believe, as the default assumption. Moreover, note that, if they do, the problem is, once again, that of determining if a potential infinite is all we can get, or we effectively also need actual infinities, conceived as special objects populating our real world; in other words, the problem is still that of accepting or refusing the existence of classes as defined by modern set theory.

The assumption that an ontology entirely based on discrete finite entities suffices for the aim of building a suitable semantic theory for natural languages is not, of course, an innocent one, but it seems to me (as well as to many others) very reasonable. If one disagrees with this assumption and at the same time considers the ontological issue for a theory of meaning essential, then he should probably believe that the project of founding natural language semantics over RA is undermined from the very beginning; at best, he could look at a formalism like $\mathcal{RA}^t$ as possibly being employed to model a proper subpart of natural language semantics.

Most linguists, however, usually assign the precedence to features different from ontological ones, in evaluating the sins and virtues of a given semantic theory. In particular, a topic of major and still increasing interest is the way a certain semantic theory shapes the interface between semantics and syntax. This topic is strictly interlaced with that of the format of semantic representations, which I am addressing in the next section.

5.2 Semantic representations

Today, there are several different theories of meaning on the market. As I said before, probably the dominant paradigm in formal linguistics is that of model-theoretic semantics (also, "referential semantics" or "truth-conditional semantics"), originated in the work by Donald Davidson and Richard Montague in the Sixties, but rooted much earlier at least in some of Frege's, Wittgenstein's, Tarski's and Carnap's writings. This paradigm has come under attack from several linguists, especially some of those trained in the tradition of earlier generative syntax (one among many examples being Hornstein (1984), on which I will importantly rely in the following), for the essential use it makes of extralinguistic notions like those of "truth" and "reference".

It is also still alive, however, the tradition often labeled as the "meaning as use" paradigm, which can be traced back to the second Wittgenstein and which has recently found new life and moved from the philosophy of ordinary language to the field of formal semantics thanks to the increasing work in so-called proof-theoretic semantics, a branch of research rooted in Gentzen's, Prawitz's and Dummett's writings and now especially promoted by Peter Schroeder-Heister and, in linguistics, by Nissim Francez and Roy Dickhoff.

Finally, there is a wide and varied galaxy of so-called *representationalist* (sometimes, "*mentalist*") approaches, which encountered great success in the Sixties and Seventies among linguists and are still dominant among cognitive scientists and AI researchers,[3] and which can be traced back at least to Katz & Fodor (1963) and

[3]To mention but a few, see the works by Jerry Fodor, Steven Pinker, or William J. Rapaport. Also Kamp's Discourse Representation Theory (DRT), which attracted a considerable following in formal semantics, is a kind of representational theory of meaning.

Katz & Postal (1964): the fundamental idea of this paradigm is that meanings are nothing more than combinations of symbols in some special language physically realized in the human brain (with no exceptions I am aware of, scholars belonging to this school postulate that this language must be universal for all human beings; Katz & Fodor (1963) dubbed it "Markerese", while Steven Pinker "Mentalese").[4]

Even if I feel sympathetic with this latter approach, here, it is of course impossible to do even partial justice to the huge and difficult question of what meanings actually are; rather, I simply want to stress the fact that, in spite of such a variety of approaches in formal semantics (and the ones I have mentioned do not exhaust the list), these approaches all share one notable feature, namely that of associating natural language sentences to some semantic representations (usually expressed in some suitable formal language).[5] Moreover, independently of the fact that they identify these semantic representations with meanings or with syntactic objects isomorphic to meanings (like in the representationalist approach) or not (like in the model-theoretic and proof-theoretic approaches), all these theories make an essential use of these semantic representations; in other words, whether or not they assign the *status* of theoretically relevant objects to semantic representations, all these theories nevertheless must at least rely on their indispensable heuristic contribution.[6]

[4]Procedural semantics can be viewed as a special branch of this line of research.

I assume that an important consequence of a representationalist theory of meaning, which distinguishes it from both the model-theoretic and the proof-theoretic approach, is that, on the basis of its assumptions, two sentences established as logically equivalent under a suitable notion of logical equivalence do not necessarily have the same meaning.

[5]The requirement that the language in which representations are built be a formal one is not, however, essential.

[6]There seems to be a strong connection between finitism in the philosophy of mathematics and the representationalist paradigm in semantics, since typical non-representationalist notions of meaning are not (primitive) recursive.

See, in this regard, what Dowty (1994: 114) says:

> It is [. . .] a truism that humans do not carry anything analogous to infinite sets of possible worlds or situations around in their heads, so the study of deduction—inferential relations based on syntactic properties of some kind of "representations" of denotations—are potentially of relevance to the psychology of language and to computational processing of meaning in a way that model-theoretic semantics alone is not.

Needless to say, stressing this quite trivial fact does not imply that notions such as those of "entailment" or "truth" do not play any role in doing semantics: even if one is not forced to commit himself to the claim that such notions play a role in any proper definition of what meanings are, I think that nobody can deny their fundamental methodological contribution to the development of a theory of meaning.[7] To be less vague, consider the obvious fact that patterns of entailment relations holding between sentences in a certain natural language do also regularly hold between proper translations of those sentences into another language. This is clear evidence that the surface form of sentences does not play a direct role in determining its entailment relations with other sentences. But since there must be some property of the sentences which directly determines these relations (otherwise, we should call for an oracle to come to our aid), it seems natural to look at their meaning for that purpose. Hence, we can naturally expect that different inferential properties ultimately relate to different meanings.[8] As it is standard

[7]As for the notion of "reference", however, things are probably different, since it seems impossible to entirely abandon it at least when addressing the problem of the meaning of indexical expressions.

[8]The opposite is not at all obvious. In particular, under any plausible representationalist

practice, thus, I will freely make use of judgements about entailment relations to illustrate differences in meaning between sentences. Notice that judgements of this sort allow the derivation with absolute certainty of only negative conclusions, i.e. conclusions stating that the meanings of two sentences are not the same, and not positive ones, stating that the meanings of two sentences are the same.

Let me say also that, as I see it, judgements about so-called "truth conditions" are actually a special sort of judgements about entailment relations.[9] Anybody in semantics, notwithstanding his idiosyncratic preferences for one theory of meaning or another, commonly argues for two sentences having different meanings by showing that they have different "truth conditions", i.e. that there is a certain scenario in which one of them is true while the other is false. This is best understood, I believe, by saying that in this case what he does is simply build a certain consistent set S of premisses (it does not matter for this argument if they are sentences or logical forms), which collectively describe the intended scenario, and showing that the first sentence and the (logical) negation of the second both logically follow from it.[10]

For this reason, I also considered it necessary to define the rules of inference governing deductions between formulæ of $\mathcal{RA}^t$, whose language I will try to show as adequate for the building of semantic representations of natural language sentences, even if, strictly speaking, the specification of such rules pertains to logic

theory of meaning, it is plainly false, at least setting aside phenomena of pragmatic nature.

[9]In this respect, I think that I am closer to proof-theoretic semanticists in conceiving the notion of "truth" as less fundamental than, and dependent on, the notion of "proof". However, as it is clear from what I have just said, this does not imply that the notion of "proof" should be seen as taking part in the definition of meaning.

[10]The specification that S must be consistent is not a dispensable one, otherwise the Principle of Explosion would apply, trivializing the deductive machinery to the extent that all derivations from S would be valid. For a formalization of the relevant notion of "consistency" in primitive recursive terms, see §4.2 above.

rather than to semantics.

In particular, I assume that one of the goals of a semantic theory (be it the final one, as I believe, or a preliminary one) is that of providing, for any given natural language L, an explicit algorithm, namely a function $[\![\ ]\!]_L$,[11] such that $[\![\ ]\!]_L$ maps each syntactic object of L (from single morphemes up to whole discourses) into a semantic representation, i.e. a syntactic object of a suitable formal language, say $\mathcal{L}$. According to what I said before, $[\![\ ]\!]_L$ must be such that, given two discourses A and B of L, $A \Rightarrow_L B$ (i.e., A entails B) iff $[\![A]\!]_L \vdash_{\mathcal{L}} [\![B]\!]_L$. This is to be understood, however, as a merely necessary condition, not a sufficient one: impressionistically speaking, such a mapping should additionally be as harmonious as possible. I will call $[\![\ ]\!]_L$ the *interpretation function* for L.[12]

I assume, however, essentially in line with standard assumptions held in the literature, that, whatever formal language we choose to represent the meanings of syntactic units of a given natural language is, it should formally assign the kind of meanings which one can properly establish entailment relations between only to whole discourses, among the syntactic units which have an overt realization (consider, in this regard, the formal treatment of sentential boundaries as covert conjunctions which is familiar in dynamic semantic theories: see, for instance, Heim (1982: 40)).

[11]In the following, I will often omit the subscript denoting the particular language where no possible ambiguity arises, or where I want to refer succinctly to properties that I assume to be shared by such a function relative to any natural language.

[12]This is not, of course, an interpretation function in the sense of model-theoretic semantics.

5.3 The theoretical claim

Once we have acknowledged that semantic representations play a crucial role in any theory of linguistic meaning and we are inclined to neglect possible ontological objections, I think we are in a position to appreciate the relevance of the questions I am trying to address.

Actually, classical first order logic, which, however expanded or modified, is at the base of standard modelizations of natural language semantics, was originally designed to deal with exactly the same ontology as RA, i.e. with the set of natural numbers. In particular, formal systems of arithmetic based on CL and incorporating into their language non-logical symbols to define p.r. functions have been widely investigated and employed. Let FOA (First Order Arithmetic) be such a language where no restriction on formulæ instantiating its induction schema has been imposed[13] and let FOA^t be the system obtained from FOA by adding to its alphabet the individual term t, which behaves exactly like in $\mathcal{RA}^t$, i.e. as a name of an unspecified number: then we can build a function $f : L(\mathcal{RA}^t) \mapsto L(FOA^t)$, mapping equations of $\mathcal{RA}^t$ into formulæ of FOA^t, such that, for any two equations $\alpha, \beta \in L(\mathcal{RA}^t)$, $\alpha \vdash_{\mathcal{RA}^t} \beta$ iff $f(\alpha) \vdash_{FOA^t} f(\beta)$. However, the converse is not possible, i.e. we cannot build an analogous mapping function $g : L(FOA^t) \mapsto L(\mathcal{RA}^t)$ such that, for any two formulæ $\alpha, \beta \in L(FOA^t)$, $\alpha \vdash_{FOA^t} \beta$ iff $g(\alpha) \vdash_{\mathcal{RA}^t} g(\beta)$. In particular, what would remain beyond the expressive power of $\mathcal{RA}^t$ are, as I have already said, unbounded existential quantifiers and, when they are embedded within the scope of another operator, also unbounded universal ones.

[13] What I am going to say actually holds for much weaker formal systems imposing restrictions on formulæ instantiating the induction schema; in particular, systems where restrictions on the combinations of unbounded quantifiers appearing inside their formulæ have been well studied. This point, however, is completely orthogonal to the one I am making.

Now, if we could show that this extra expressive power is indeed needed once we employ logical formulæ of a certain formal system to build semantic representations of natural language sentences, then we would have shown *ipso facto* that $\mathcal{RA}^t$ cannot do the job. This would be the case, in particular, if we were able to show one of the following things:

a) that at least some natural languages have verbs, nouns, adjectives, adverbs or prepositions which cannot be translated as p.r.u. functions;

b) that at least some natural languages have determiners which must be translated as unbounded existential quantifiers;

c) that at least some natural languages have determiners which must be translated as unbounded universal quantifiers and which are within the scope of another operator.

As for the first circumstance, I have no evidence that it holds for any language. As for the other two, instead, I suspect that it would be pretty hard to empirically prove them, since it is a well-known fact that there is an abundance of covert arguments in natural language, and, besides, restrictors of DPs are known to be often incomplete and in need of being contextually constrained (see Kuroda (1982) and Sperber & Wilson (1986), among many others); so, it would be difficult to clearly show that there are cases like the ones just mentioned where the determiners in question truly come in an unbounded fashion.

Conversely, there is at least one way to suggest that a formalism like that of $\mathcal{RA}^t$ best serves natural language semantics rather than its CL competitors: to show that in natural languages there are elements corresponding to generic variables, i.e. scopeless elements which are universal-alike and are interpreted as if they had wide scope over functional operators while they were scope-insensitive between themselves. This will be what I will argue in Ch. 7.

One could maintain that, even if I succeeded in showing that proper semantic representations for natural language sentences need to have at their disposal something structurally analogous to generic variables, i.e. translating scopeless elements which are universal-alike, still this would not be, *per se*, an argument in favour of RA as the proper formal basis upon which to found natural language semantics, since scopeless elements of this kind could be, for instance, easily incorporated in a formal system like standard first-order or higher-order logic. However, I should reply that the really crucial way to argue against the application of RA to natural language is by showing that unbounded quantifiers (or, more generally, elements which are not defined as p.r.u. functions) are indeed present in natural languages: $\mathcal{RA}^t$, where (bounded) quantifiers are only some special p.r.u. functions and, thus, are not primitives of the system, is indeed a very simple formal system, arguably minimally simple and certainly much simpler than any possible version of CL; thus, if no extra expressive power is needed, it should be preferred on the grounds of general epistemological principles. Moreover, if we had a system with unbounded universal quantifiers, there would be no need for generic variables, since all their inferential behaviour could be reproduced by wide scope universal quantifiers: the presence of those free variables in the system would thus require an independent motivation showing what has been gained at the cost of such an anti-economical move: the least we can say is that the task of providing such an independent motivation would hardly be a trivial one.

The hypothesis I am going to partly explore, hence, is that the outputs of $[\![\ ]\!]$

are terms of $\mathcal{RA}^t$.[14]

Of course, here I will give only a partial characterization of the interpretation function: this is to say that I will equate the term resulting from the application of the interpretation function to a compound linguistic expression to another term where the interpretation function, whenever present, is applied only to components of the previous expressions.[15]

Note, as well, that we should assume that something corresponding to indices appears at the level of the syntactic representation which feeds the semantic component (LF, for instance): I will assume that indices themselves do. The existence of something playing this role at some suitable level of the syntactic representation is in line with most linguistic approaches to quantification, starting from the Quantifying-In device of Montague (1970a,b, 1973) to Lakoff's Quantifier Lowering or the most famous Quantifier Raising by May.

The semantic theory I am going to sketch is a top-down semantics in the technical sense defined, for instance, in Hodges (2012: §3): in other words, semantic derivation is taken to procede from the meaning of the largest expressions to that of their components, down to the semantic atoms.

[14] The assumption that what we need are terms of the system allows us to define the interpretation function in a much simpler way with respect to what we should do by assuming that the outputs were equations of the system, as in Goodstein (1957: Ch.3). Besides, the central intuition informing the theory of illocutionary operators (see §5.4 below) suggests that this treatment should be preferred also for independent reasons.

I believe that this assumption is in line with Partee's (2009) suggestion (ultimately based on Carstairs-McCarthy (1999)) of the viability of a semantic theory based on one single basic semantic type. But, again, Schönfinkel (1924) can be viewed as a forerunner.

[15] Note that this requirement does not imply that the semantic theory I am arguing for is a compositional one. In fact, I believe that, where fully developed, it would not be, like many others on the market. In particular, what I consider to be the crucial obstacle to a compositional theory of meaning, in agreement with nearly all scholars challenging compositionality, is the proper semantic treatment of intensionality.

5.4 The illocutionary operator 'ASSERT'

One last thing has to be done before moving to semantic analyses proper. As I have said before, judgements about entailments (among which, I argued, a prominent role is played by those concerning truth conditions) are a precious methodological tool in order to describe the meanings of natural language discourses or their subparts. However, it is often assumed, following a line of thought inaugurated by Frege (1918), that these judgements, rather than directly applying to the meanings of discourses themselves, apply to their assertions, i.e. to a specific speech act differing from others sharing which share with it the same semantic content.

To this extent, Frege (1918) introduced the notion of *illocutionary force* (*Behauptung*, in the German original) in order to be able to distinguish different statements that seem to share the same core content. This is the case, for instance, with the following sentences:

(7) a. Gianni ate the potatoes.
 b. Did Gianni eat the potatoes?

Frege said that sentences like (7a) and (7b) have the same content, but (7a) expresses it with *interrogative* force, while (7b) expresses it with *assertive* force.[16]

After Frege, it has oftenbeen assumed, in formal semantics, that illocutionary force is conveyed by a specific operator present in a suitable (pragmatic) representation of the speech act and which is standardly conceived as a unary non-embeddable operator (illocutionary force was expressed through operators first by

[16]This minimal list, of course, does not exhaust the inventory of illocutionary forces discussed in the literature.

Zaefferer (1982), whose analysis collocated itself within the framework of Montague's grammar; for a simplified treatment, more in line with the one proposed here and where the standard labels were introduced, see Jacobs (1984a,b)): in the case of assertive force, it is usually labelled "ASSERT", while for interrogative force we usually have "QUESTION". Besides than in Jacobs (1984a,b), 'ASSERT' has been adopted also by Krifka (1992, 1999) and, more recently, by Chierchia (2006).

To maintain the assumption that all outputs of $[\![\ ]\!]$ are terms of $\mathcal{RA}^t$, I treat illocutionary operators, and in particular 'ASSERT', as mere symbols denoting the operation by which one obtains a certain kind of speech act from a certain semantic representation of a discourse in some natural language.[17] In the case of assertions, the operation can be formally represented in the following way (where D is a discourse in the natural language under examination):[18]

(8) $\quad \text{ASSERT}(\alpha) \quad = \quad [\![\alpha]\!] = 0.$

Now, we are in a position to define a relation between terms of $\mathcal{RA}^t$ which is a formal equivalent of the intuitive notion of *semantic equivalence* between discourses. The definition goes this way:

[17]The presence of a non-embeddable operator 'ASSERT' in declarative sentences allows us to define the interpretation function in a simpler way compared with how we should do without it. One can look at Goodstein (1957: Ch.3), where the logical operators are defined in a rather more complex way as a consequence of having had to make propositions directly correspond to equations of RA, to convince himself of this. I take this to be evidence enough that we should assume the presence of such an operator even in the case it was not opposed to other illocutionary operators like 'QUESTION'.

[18]Notice that we could have made the machinery work also by choosing a number other than '0', in (8). However, the choice of '0' allows for the simplest treatment of the semantics and, thus, has a considerable theoretical appeal and an *a priori* greater empirical plausibility.

$$(9) \qquad \alpha \Leftrightarrow \beta \quad = \quad \mathrm{ASSERT}(\alpha) = 0 \dashv\vdash_{\mathcal{RA}^t} \mathrm{ASSERT}(\beta) = 0.$$

Chapter 6

Neutral semantic representations

One of the aims of this dissertation is to show that the proper definition of the semantic contribution of some classes of words or some syntactic structures in natural languages is best explained by appealing to the kind of semantic representations which can be provided by a formal system like $\mathcal{RA}^t$ and which no formalism of a different kind can provide. In doing so, I also need, first, to define in $\mathcal{RA}^t$ terms the semantic contribution of particular words, lexical categories and phrases whose semantic analysis recast in $\mathcal{RA}^t$ terms does not depart from standard ones present already in the literature in any vital respect.

Most, if not all, classes of linguistic expressions I will consider in the following have received several different syntactic analyses, sometimes without still having reached a consensus among the majority of linguists as to which one of them should be preferred; however, I will simply choose one of the analyses I am aware of (of course, the one I feel more likely to be on the right track), without discussing alternative proposals. I believe, however, that most syntactic frameworks are compatible with the semantic treatment I am going to develop (even if I will offer no proof supporting this claim).

6.1 Lexical words

6.1.1 Verbs

At least since Tesnière (1959), it has become customary in linguistics to think that each verb comes with a certain *valence*, corresponding to the number of arguments that must associate with it in the clause in order to preserve grammaticality (a correspondent idea already held in formal logic at least since De Morgan (1864)). However the syntactic relation between the verb and its arguments has to be specified, this does not imply, in natural languages, the overt realization of a certain argument (see, among others, Carlson (1984)). Consider for instance a verb like *give*, typically treated as a 3-valent verb, i.e. a verb associating with three different arguments, namely a subject, a direct object and an indirect object.[1] Look now at the following sentences:

(10) Gianni gave an umbrella to Luisa.

(11) #Gianni gave an umbrella.

(12) #Gianni gave to Luisa.

(13) #Gianni gave.

[1] When referring to the valence of a verb, here, I am deliberately not taking into account, for the sake of simplicity, the presence of at least the temporal argument and the event argument, besides the other ones. As for the event argument, I will introduce it shortly below in §6.1.2.

All the sentences where one or more arguments of the verb is missing (i.e., (11)–(13)) would sound odd, as the gate symbol signals, when given out of the blue. However, contexts that make them fine are not hard to find. Consider for instance the following scenario:

(14) There has been an event called the “Mutual Support Day”, and participants have been asked to give a present to someone living in their neighbourhood.

In the scenario described in (14) all the sentences (11)–(13) become felicitous. It is important to note, however, that, for sentences (11)–(13), uttered in the scenario given in (14), paraphrases where the covert arguments are overtly expressed through bare nominals are by far better than alleged paraphrases where the covert arguments are overtly expressed through indefinite DPs with overt determiners:

(15) a. Gianni gave an umbrella to people.
b. #Gianni gave an umbrella to someone.

(16) a. Gianni gave things to Luisa.
b. #Gianni gave something to Luisa.

(17) a. Gianni gave things to people.
b. #Gianni gave {something / things} to someone.
c. #Gianni gave something to {someone / people}.

I have intentionally chosen a verb like *give*, which has a strong tendency to be associated with overtly realized arguments in the clause, to illustrate the pragmatic nature of the possible lack of overt arguments in a more general way, since it is well-known that there are many verbs that can quite naturally dispense with having all their arguments expressed in the surface. Here are some examples involving the 2-valent verb *eat*:

(18) Polly has just eaten two tomato sandwiches.

(19) Polly has just eaten.

a. Polly has just eaten a typical meal of hers.

b. #Polly has just eaten {something / it / that thing}.

I will give an outline explanation for the contrast above in the following §6.1.2.

Even if some arguments can sometimes be omitted, not all of them can: subject, which always expresses an argument, in fact cannot (this fact may probably be seen already stated in Aristotle's *De Interpretatione*; see Aristotle (1831, 1984)). This is true, of course, even for *pro*-drop (*null subject*) languages, like Italian or Spanish, where the subject does not need to be overtly realized; as it is well-known, inflection, among other things, proves that a covert subject is still present. Moreover, on the interpretative side, in the absence of an overt subject we can never supply an existential quantifier in the semantic representation of the clause, but we always need to recover an element which is anaphoric to some proper antecedent in the discourse (and this is why we find *pro*, i.e. a pronoun, in ordinary syntactic representations within the generative framework). Here are three examples, with a transitive, an unaccusative and an inergative verb respectively, and with a third person pronoun to be recovered (with pronouns of other persons, the oddness of a paraphrase with an existential quantifier would be trivial):

(20) Ha prestato un aspirapolvere a Luisa.

`has lent a vacuum to Luisa`

a. ✓ '{He / She} lent a vacuum to Luisa.' It.

b. ✗ 'Someone lent a vacuum to Luisa.'

(21) È arrivato tardi.

`is arrived late`

a. ✓ '{He / She} arrived late.'

b. ✗ 'Someone arrived late.'

(22) Ha camminato lentamente.

`has walked slowly`

a. ✓ '{He / She} walked slowly.'

b. ✗ 'Someone walked slowly.'

6.1.2 The Davidsonian account

It has often been said, in the linguistic and philosophical literature, that verbs predicate something of a certain *event*. This view can be ultimately traced back to the ancient Indian grammarian Pāṇini (see Pāṇini (1989)). In more recent times, this idea has been revived and made formally precise by Donald Davidson in Davidson (1967), where he argued that we should assume that verbs provide a variable ranging over events to the logical forms of the clause which they are embedded in. Hence, for instance, he would have suggested something similar to the informal semantic representation in (23a) for (23).[2]

[2]In all the following logical forms, I will avoid features concerning verbal aspect or the *Aktionsart*: for these and other topics linked to the verb, the Italian reader is directed to Borgato (2012).

(23) Gianni quickly gave an umbrella to Luisa.

a. [some event: e] ([some umbrella: x] (e is a giving of x to *Luisa* by *Gianni*) $\wedge$ e is quick)

According to Davidson, logical forms of this kind are required to account for the pattern of entailment relations determined by VP modification. Consider the following (24)–(28), for instance:

(24) Polly buttered the cracker.

a. [some event: e] (e is a buttering of *the_cracker* by *Polly*)

(25) Polly buttered the cracker slowly.

a. [some event: e] (e is a buttering of *the_cracker* by *Polly* $\wedge$ e is slow)

(26) Polly buttered the cracker slowly in the bathroom.

a. [some event: e] (e is a buttering of *the_cracker* by *Polly* $\wedge$ e is slow $\wedge$ e is in *the_bathroom*)

(27) Polly buttered the cracker slowly in the bathroom with a knife.

The standard account remains that due to Vendler (1957, 1967) (based on Ryle (1949)), who identified four different classes of events: *activities*, *accomplishments*, *achievements* and *states*. Kenny (1963) added empirical support to this distinction and put together accomplishments and achievements in a unique class, that of what he called *performances*; even if its internal subdivision is required on syntactic grounds, nevertheless performances seem to form a natural class. Jespersen (1924: 273) formulated a two-fold classification of verbs anticipating some aspects of Kenny's taxonomy, and Garey (1957) did the same in a more accurate way. For the parallel between performances and count nouns, on one side, and both activities and states and mass nouns, on the other side, see Allen (1966) and Mourelatos (1978), while for the parallel between perfective aspect and count nouns, on one side, and imperfective aspect and mass nouns, on the other side, see Leech (1969) and again Mourelatos (1978).

Note, furthermore, that a completely formal semantic representation for sentences like (23) is still not possible, since I have not yet discussed the semantics of quantifiers, which needs to be addressed after that of predicates.

a. [some event: e] (e is a buttering of *the_cracker* by *Polly* $\wedge$ e is slow $\wedge$ e is in *the_bathroom* $\wedge$ [some knife: x] (e is performed with x))

(28) Polly buttered the cracker slowly in the bathroom with a knife at midnight.

a. [some event: e] (e is a buttering of *the_cracker* by *Polly* $\wedge$ e is slow $\wedge$ e is in *the_bathroom* $\wedge$ [some knife: x] (e is performed with x) $\wedge$ e is at midnight)

Intuitively, (28) entails (24), (25), (26) and (27); (27) entails (24), (25) and (26) but not (28); (26) entails (24) and (25) but not (26) and (27); (25) entails (24) but not (26), (27) and (28); (24) does not entail any of (25)–(28). The same pattern of entailment relations formally holds, by conjunction reduction, between the correspondent representations in (24a)–(28a).

It is essential to notice that all these modifiers cannot be semantically associated with any of the arguments of the verb, except, of course, the event argument. In (25), for instance, we cannot take Polly or the cracker to be slow, but only the action of buttering performed by Polly to the cracker.

Coming back to the contrast between bare NPs and overt DPs in (15)–(17), consider the following well-known contrast noticed by Dowty (1972) (see also Dowty (1979: §2.3.3), Hinrichs (1985), Moltmann (1991) and Carlson (1977a) (Portner & Partee (2002: 41–45)); see also Verkuyl (1972) for another early explicit observation that nominal expressions may have an impact on aspectual features of the VP):

(29) Gianni ate a sandwich {in an hour / for an hour}.

(30) Gianni ate some sandwiches {in an hour / for an hour}.

(31) Gianni ate sandwiches {* in an hour / for an hour}.

As the combinations of admissible adverbial modifiers suggest, sentences (29) and (30) are about an accomplishment, in Vendler's (1967) terms, and thus can have a telic reading, that induced by the adverbial modifier introduced by *in*. They can have atelic readings too, and, in accordance with my intuitions, there are two different such readings in both cases: as for (29), for instance, modified by the *for*-adverbial, it can mean either that Gianni spent an hour eating one and the same sandwich (possibly still not having finished eating it at the end of that interval of time) or that he completely ate one and the same sandwich several times during a whole hour; given the obvious physical fact that there is no food which, once it has been eaten, can rapidly (if at all) be brought back to its previous state, the latter is especially odd. Sentence (31), instead, is about an activity, in Vendler's (1967) terms, and thus can only have an atelic reading.

Notice that the atelic readings of sentence (31) require the bare noun to be an existential indefinite, not a kind-denoting definite.

Hence, we may assume that, when a bare noun is to be interpreted as an existential quantifier, it must be in the scope of some quantifier provided by the verb, for instance the existentially quantified event argument. This may well fit with the insight, which can be found expressed in Longobardi (1991, 1994), that bare nouns have a phonologically null determiner that is only licensed when it is properly governed by a verb (for governing of bare nouns by the verb, see also Contreras (1986); for the idea that reference to individuals is licensed only by a D layer, on which Longobardi bases his analysis, see Stowell (1991)). This may also explain the following contrast observed in Ihsane (2008: 201) concerning the anaphor *others*:

(32) John recommended {some / certain / three} books and Mary recommended others.

(33) *John recommended books and Mary recommended others.

However, a problem for such an account seems to arise, which has interesting links with the main topic of this dissertation and which I will come back to further on in §7.1. As I said, it is natural to assume that both the event argument and bare nouns introduce an existential quantifier in the logical form: but in standard logic, when we have two existential quantifiers[3] one of which takes scope directly over the other, such a configuration is indistinguishable, as for its inferential properties, from one where the scope of the two quantifiers is inverted. It would seem, thus, a move against economy if a natural language provided a specific syntactic construction which never differentiates on the inferential ground from other syntactic constructions present in the same language.

In any case, for the moment it is important to stress only these two things:

a) there seems to be a connection between the aspectual properties linked to bare nouns and the narrow scope features of the latter;

b) covert arguments behave like bare nouns.

As for this last remark, I will not hesitate to formulate the conjecture that covert arguments do in fact correspond to DPs with both a null determiner and

[3]The same, of course, also holds for universal ones.

an empty restrictor.

Now, let's come back to the main path. As a straightforward consequence of the introduction of the event variable, the valence of a verb in the davidsonian framework is increased by exactly one unit with respect to its valence in, say, Tesnière's one. Since I am not aware of clear counterevidence to the strongest assumption that all verbs do come equipped with an event argument in the semantic representation, I will make such an assumption and maintain that the davidsonian valence of any verb must be greater than 0. At this point, we could define the following semantic representation for a completely saturated n-valent IP^n, where $V^{n\prime}$ is a term of $\mathcal{RA}^t$ containing at least n different types of placeholders:

(34) $$[\![\mathrm{IP}^n]\!] \quad \equiv \quad V'^n.$$

Finally, it is worth remembering the quite standard assumption that the event argument is always existentially quantified and that its quantifier is always within the scope of those quantifiers possibly introduced by overt determiners in the same clause and also within the scope of negation. This last point, due to Krifka (1989a), can be expressed by saying that, while we can use negation to state that there are not events of *X-ing*, we cannot use it, instead, to state that there are events of *not-X-ing* (note that this does not exclude the possibility that an event of *not-X-ing* is expressible by other syntactic means, for instance by employing the English construction *abstaining from X-ing* or verbs encoding in their lexical meaning the meaning of *not-X-ing*).

6.1.3 Nouns and other lexical words

I will follow the tradition in formal semantics of translating nouns, adjectives, adverbs and prepositions (when they are not case-markers) with logical forms sharing the same structure of those I have just suggested for verbs (see, for instance, Higginbotham (1987) and Longobardi (1994), for a treatment of NPs as predicates). In doing so, I will assume that they too have an argumental structure.

However, another important assumption I will make here is that these lexical words, unlike verbs, have a fixed ariety depending on the lexical category they belong to, and in particular that nouns, adjectives and adverbs logically translate as unary predicates, while prepositions as binary ones.[4] This claim, despite being quite orthodox, would require an in-depth discussion I cannot even attempt here (see Higginbotham (1985: 563–7) for nouns and adjectives and for the intersective semantics of adjectival modification); in any case, it appears to be mostly controversial when applied to the case of nouns, since relative nouns like *daughter* or *father*, deverbal nouns like *teacher* or *admirer* and others typically seem to have more than one argument (*the daughter of Gianni*, *the father of Agnese*; *the teacher of Michael*, *the admirer of Sonia*). The solution I will implicitly adopt here is that of treating these cases as cases of NPs with a semantically poor N head modified by a relative clause, roughly along the following lines (for a semantic characterization of relative clauses, see 6.7.1 below):

(35) the daughter of Gianni ≈ the female human being who Gianni generated;

(36) the teacher of Michael ≈ the person who teaches things to Michael.

[4] Things are obviously more complex with gradable adjectives and comparatives; see Cresswell (1976), Hellan (1981), Kennedy (1997b,a) and Oda (2008) for some semantic insights.

Many things should be said in this regard: here, I will limit myself to highlighting two well-known facts. The first is that nouns, adjectives and adverbs, unlike verbs, do not assign case to their complements; besides, despite standard analyses of nominalizations, I think that one could maintain that the word *of* introducing complements in the noun phrases in (35) and (36) is the complementizer instead of the preposition, thus leaving open the possibility that a CP is at work and possibly suggesting some analogies with control structures.[5] Secondly, there are many languages where nominalizations are used to form relative clauses (the phenomenon is particularly pervasive in Tibeto-Burman languages: see Matisoff (1972, 2003) and DeLancey (1999), among many others; see the recent Ntelitheos (2012) for some cases in Malagasy).

6.2 Connectives: conjunction and disjunction

The semantic value of sentential *conjunction* and *disjunction* expressed in $\mathcal{RA}^t$ terms, in accordance with their boolean semantics (see Boole (1847: 51–3)), can be ultimately traced back to Hilbert & Bernays (1934). The definitions we are looking for are the following ones:

(37) $[\![\alpha \text{ and } \beta]\!] \quad = \quad [\![\alpha]\!] \wedge [\![\beta]\!];$

(38) $[\![\alpha \text{ or } \beta]\!] \quad = \quad [\![\alpha]\!] \vee [\![\beta]\!].$

[5] On the complementizer *of* in English, see Kayne (1997) and, for the phenomenon of degrading of particles shift in *of-ing* nominalizations, as well as for nominalizations in general, the classical Chomsky (1970) (reacting to Lees (1960), where, however, a first comprehensive classification of English nominalizations was offered).

On the phenomenon of recategorization of prepositions as complementizers, see Dubinsky & Williams (1995), among others.

The characterization of the meaning of connectives just given predicts their semantic behavior exactly in the same way as it is predicted in the classical propositional calculus.

Of course, in the linguistic and philosophical literature there has been a huge debate, still ongoing, concerning the proper semantic analysis of conjunction when it connects two DPs instead of two VPs or two sentences. The problem is how to deal with examples like the following one:

(39) Gianni and Luisa lifted the piano.

(39) has a reading saying that Gianni and Luisa managed to lift the piano together, thus not entailing either the following (40) nor the following (41).

(40) Gianni lifted the piano.

(41) Luisa lifted the piano.

The interpretation suggested for (39) is an example of a *collective* reading. However, to model such a collective reading, a semantic representation like the following (42) would not work, since it would correspond instead to sentence (43), actually entailing both (40) and (41).

(42) $[\![\text{Gianni lifted the piano}]\!] \wedge [\![\text{Luisa lifted the piano}]\!]$

(43) Gianni lifted the piano and Luisa lifted the piano.

There have been two general approaches to the problem of conjoined DPs: one, dating back to Aristotle and famously formalized in Link (1983) (see also Hoeksema

(1983b)), posits a lexical ambiguity for the conjunction; the other, instead, tries in different ways to reduce the meaning of DPs connecting conjunction to that of sentential conjunction.

There is of course an *a priori* reason which favours the last approach, namely the fact that positing an ambiguity is always a move which goes against theoretical parsimony and, hence, should be viewed as a last resort. Nevertheless, the ambiguity account seems to be supported, among other things, by the fact that languages like Japanese have conjunctions (*to*, *oyobi*, *toka* and others) which can only join nominal expressions (see Hayashishita & Bekki (2012)). I cannot address the question in all its complexity, but here I will assume the ambiguity theory as a base, thus restricting the semantic representation given above for conjunction only to the case of sentential (or VP) conjunction.

6.3 Anaphoric elements

As is well-known, several natural language expressions, like third-person pronouns, definite descriptions, proper names, tenses, certain temporal adverbs, presupposition triggers and possibly many others, are used to refer to some expression previously introduced in the discourse or to some entity or fact which is made salient in the context of utterance.[6] These two uses have sometimes been labeled,

[6]As is well-known too, third-person pronouns, the definite article and certain demonstratives are crosslinguistically related together even on the morphological level: in fact, a quite commonly held view is the one that takes pronouns to only be definite descriptions in disguise, i.e. definite descriptions with no descriptive content (or a "poor" one, given that, in English as well as in many other languages, they can nevertheless convey semantic features like '±human' or 'male/female'). The assimilation between pronouns and descriptions can be traced back at least to Postal (1966).

Of course, a unified analysis of these elements may well lead to different assumptions: Elbourne (2005), for instance, has argued in favour of a unified account of third-person pronouns and definite descriptions extending the russellian quantificational analysis of definite descriptions respectively, as “anaphoric” and “referential” (or “deictic”). However, a recent trend, mostly developed in the Lewis-Kamp-Heim framework of dynamic theories of meaning (obvious references are Kamp (1981) and Heim (1982, 1983)), strived to argue for an assimilation of the two (hopefully, even if not necessarily, for all the syntactic categories listed above), usually obtained by reducing the referential use to the anaphoric one by appealing to the phenomenon of *accommodation* (see Karttunen (1974) for the global case and Heim (1982) for the general notion). Quite often too, this has been understood as if in the semantic representation the anaphoric element is actually coindexed with a suitable antecedent which lacks a phonetic realization.

Standardly, anaphora is conceived as coindexing with a variable which is bound by a quantifier: hence, anaphoric elements are viewed as bound variables themselves. This is, of course, because it is usually assumed that a new discourse referent must be introduced by a linguistic expression (be it overtly expressed or recoverable through accommodation) which corresponds to a quantifier in the logical form or, at least, to a variable which is bound by a quantifier introduced in some place of the derivation (as it happens, in dynamic frameworks, with existential closure or unselective quantification). However, this is not the case in the present account, since there are linguistic expressions which introduce new referents but logically correspond to free variables instead of quantifiers and never come to be bound throughout the syntactic derivation. Under the present account, thus,

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also to the formers. Even a quick examination of his theories goes beyond the limits of this dissertation; here, I can only point out the fact that Elbourne’s analysis essentially requires a substantial amount of situation semantics at work.

Notice, finally, that in languages like Italian, in their substandard varieties, proper names too can bear the definite article, especially in the case of female ones; and, in general, in all languages I am aware of which have a definite article, all plural proper names must carry it.

anaphora simply comes to be coindexing with an expression occupying a suitable position (say, c-commanding it) at a certain level in the syntactic representation.[7]

There would be much to be said for properly defending an assumption like that; here, however, I simply assume it as a point of departure, limiting myself to quoting some early places in the literature where it has been advocated for with reference to at least one of the categories mentioned above.

Heim (1982) emphasizes the debt that dynamic semantic theories owe to the view held by some traditional grammarians like Christophersen (1939) and Jespersen about definites; this view is summarized by her in the following "nutshell" from Heim (1983) (Portner & Partee (2002: 223)):

> A definite is used to refer to something that is already *familiar* at the current stage of the conversation. An indefinite is used to introduce a *new* referent.
>
> This has been labeled the "familiarity theory of definiteness."

As Heim herself points out, a decisive step towards a tenable reformulation of the familiarity theory was made by Lauri Karttunen in Karttunen (1968a,b, 1976) (see also Krifka (1992)). Karttunen, in fact, introduced there the notion of a *discourse referent*, i.e. something which can play the role of an antecedent for anaphora but which does not necessarily correlate with a (unique) referent in the real world.

There seems to be, however, a requirement which must be fulfilled when the antecedent of the anaphoric element is provided through accommodation. I will illustrate it with two examples, the second of which is the classical example of a

[7] For the phenomenon of *backwards anaphora*, also-called "*cataphora*", see Postal (1970).

so-called *paycheck pronoun* (or *pronoun of laziness*, introduced in the linguistic and philosophical debate by Geach (1962)):

(44) Gianni has a wonderful recollection of his primary school teacher.

(45) John gave his paycheck to his mistress. Everybody else put it in the bank.

In its most natural reading out of the blue, (45) is understood as saying that everybody puts his own paycheck (not John's one) in the bank. However, this reading can only arise if it is presupposed that everyone, and John too, has one but only one paycheck. In the same way, (44) presupposes that Gianni had one and only one primary school teacher. In other words, in these cases anaphora seems to be possible only if the antecedent is uniquely identifiable (but not necessarily at the global level), in accordance with the classical analysis of definite descriptions in Russell (1905) (see also Cooper (1979)), extended to cover also the case of plural definites by Sharvy (1980) with his analysis in terms of maximal sets. Notice that the unique identification does not need to take place at the global level, as in (45) it is bound by the universal quantifier.[8]

I suspect that the reason behind this requirement is that, if it was not fulfilled, we would not be able to recover the right antecedent between more than one possible candidate. It remains to be understood if the particular case of donkey anaphora is a case of unaccommodated or accommodated anaphora (I believe that Elbourne's (2005) proposal may be recast in terms of the latter; see also Cooper (1979), Heim (1990) and Heim & Kratzer (1998) for other previous D-

[8]Given the well-known syntactic constraints limiting the interpretation of non-reflexive pronouns (see below), this seems to imply that the subject of the second sentence in (45) must be a topic, or, however, it must have a different local domain with respect to the pronoun.

type approaches, i.e. approaches based on the assimilation between pronouns and definite descriptions).

A unified treatment of pronouns as anaphoric elements can be found also in Hausser (1974, 1979) and Lasnik (1976). Proper names are conceived as anaphoric elements by Sommers (1982: 230) (but see already Burge (1973: 436)) and Yagisawa (1984) (who applied his analysis to solve the famous Frege's (1892) puzzle about identity; remind also Russell's (1905) assimilation of proper names to definite descriptions). The parallel between pronouns and tenses is illustrated and developed in Partee (1973). Finally, van der Sandt (1992) (actually based on his former van der Sandt (1989) and van der Sandt & Geurts (1991) and inspired by some of the main works on presupposition, in particular works on the problem of presupposition projection like Karttunen (1973) and the already quoted works on accommodation; see also Geurts (1999)) developed a theory of presupposition as anaphora.[9]

Even if the antecedent of an anaphoric element may be recovered in a wide variety of ways, with contextual factors often playing a crucial role, it is constrained by the linguistic environment not only through the descriptive content possibly carried by the anaphoric element itself. The point is illustrated by the following examples (which are Heim's (1990: 75) (57) and (58) original ones, respectively; see Heim (1982: 21–4, 80–1), crediting Barbara Partee for earlier examples of this kind, and Heim (1990: §5) for discussion of this issue):

[9] Only a few years later Saul Kripke independently made public more or less the same fundamental idea.
For the early history of the notion of "presupposition", the obvious reference is Strawson (1950), who argued against Russell's approach to definite descriptions relying on some suggestions already contained in the seminal Frege (1892).

(46) a. Every man who has a wife is sitting next to her.

b. *Every married man is sitting next to her.

Intended reading: 'every married man is sitting next to his wife'

An obvious avenue to be explored to account for such a contrast is that of hypothesizing that the anaphoric link between the pronoun and its antecedent must be established before the meanings of lexical words have been unpacked (it seems to me that this represents one further argument in favour of Chomsky's (1965) syntactic level of the deep structure as something distinct from the semantic representation of the sentence). However, I will leave this problem aside completely.

As for the interpretation of pronouns, already Lees & Klima (1963) and Ross (1967b) showed that the antecedent of a reflexive pronoun must be within the same clause and that, if a pronoun linearly precedes its antecedent, then it must be in a subordinate clause to that of the antecedent (see also Chomsky (1981)). Of course, also PRO (Chomsky (1973, 1977b)) should be treated as an anaphoric element.[10]

Finally, prominent linguistic expressions to be considered here are *wh*-pronouns in free relative clauses. It may be controversial to treat them as anaphoric elements, but, especially in view of the arguments I am going to develop in Ch. 7, there are several features which *wh*-items share with standard anaphoric elements that are worth highlighting.

wh-items are assumed to occupy a certain layer in a CP projection:[11] it is

[10] In this regard, I want to mention here at least the theories of raising as control (cf. Bresnan (1982a), Starosta (1997) and O'Grady (2005), this latter basing himself on Andrews (1982), Jacobson (1990) and Culicover & Jackendoff (2001), among others) and causatives as control structures (Bordelois (1988), among others).

[11] Sometimes interrogative sentences are analyzed as complements of performative verbs, overtly realized through an interrogative morpheme which in many languages reduces, in the

thus possible to conceive the CP as a restrictive relative clause whose head is the *wh*-pronoun itself, more or less working as descriptively poor definite descriptions like *the one*(*s*) or *the thing*(*s*). This is, in fact, the standard treatment at least for dependent interrogative clauses.[12]

Both definite descriptions and *wh*-clauses trigger a presupposition (this fact can also be seen in the answers to *wh*-questions, and it has been used to argue for the presuppositional behaviour of focus/background structures at least since Rooth (1985).):

(47) Gianni spoke to the one that was at the party.

$>>$ Someone was at the party.

(48) Gianni spoke to whom was at the party.

$>>$ Someone was at the party.

In both cases, there are linguistic environments where the properties of these items with respect to projection suggest the viability (and, I would say, even the desirability) of a treatment of their presuppositional features in van der Sandt's (1992) terms of anaphoric relations:

(49) If Luisa receives some roses, she knows the person who sent them.

$>/>$ Someone sent some roses to Luisa.

(50) If Luisa receives some roses, she knows who sent them.

$>/>$ Someone sent some roses to Luisa.

surface, to a prosodic feature: see Ross (1970), Karttunen (1977) and Higginbotham (1993).

[12] But see also the notion of "referentiality" to which Rizzi (1990) and Cinque (1990) appeal to explain the cases of violation of some weak islands.

Hence, under the present analysis, also *wh*-pronouns in free relatives must be bound by a quantifier somewhere, in the logical form of the discourse. This clearly appears to be the case when considering sentences like the following one (compare (51) with (208) at p. 183):

(51) Every girl introduced a boy to who wrote to him in chat.

There is an unmarked reading[13] under which the maximal set of people writing in chat is possibly different from boy to boy, and the boy himself to whom each girl introduced his chat-friends is possibly different from girl to girl. Hence, the *wh*-pronoun must be bound by a quantifier in the scope of the existential quantifier introduced by *a*, and *a* itself must introduce an existential quantifier which is in the scope of the universal quantifier (thus ruling out an interpretation as a specific indefinite, provided that it is semantically distinguishable at all from the interpretation as an existential quantifier).

Reference to maximal sets is arguably related to the property of embedded questions known, from Groenendijk & Stokhof (1981, 1982, 1983, 1984b,a) who first observed it, as *strong exhaustivity* (see also Rullmann (1995), partly based on Jacobson (1995), and Beck & Rullmann (1999), partly based on Heim (1994), for strong exhaustiveness in degree questions). *Weak exhaustivity* is illustrated by the following entailment pattern:

(52) a. John knows who was at the party.

b. Mary was at the party.

---

[13] I take the absence of markedness as an essential factor.

c. $\therefore$ John knows that Mary was at the party.

Strong exhaustivity, instead, is a stronger notion than weak exhaustivity: it implies weak exhaustivity, but also determines entailment patterns like the following one:

(53) a. John knows who was at the party.

b. Mary was not at the party.

---

c. $\therefore$ John knows that Mary was not at the party.

Notice, however, that pragmatic restrictions on the relevant maximal set may be in order also in the case of embedded questions, to the extent that they may make an inference even like the one exemplified in (52) fail. This would happen, for instance, in the following scenario: imagine that Ciro is some guy who became the protagonist of some horrible actions which especially injured John; imagine as well that the party has been organized by some of John's friends and that John could not join it because he was abroad during that period. Given this state of affairs, John would be very offended to know that Ciro had been invited to the party. Now, Ciro has been. Suppose that both Anna and Bice know this state of affairs and that Anna says to Bice (52a): it seems to me that, in this scenario, (52a) only entails that John knows that Ciro was at the party, possibly ignoring anything about other people and Mary in particular; hence, the inference from (52a) and (52b) to (52c) would fail in this case.

I will finish up the discussion on *wh*-pronouns here: what is important to retain is that I take free relative clauses to be anaphoric elements which are covertly bound somewhere in the syntactic structure of the sentence.

In the semantic representations I am going to give later, anaphoric elements will be possibly rendered in two different ways: in the first one, corresponding to what their specific function is, they will translate into variables (whether bound or free, but coindexed with the variable provided by a higher element in the syntactic structure); in the second one, they will translate as mere placeholders (not in the technical sense of $\mathcal{RA}^t$). This is because often it will not be possible to fully specify the linguistic or even contextual environment where a certain discourse is embedded and where one should look to find the proper antecedent of a certain anaphoric expression inside it: in this case, translations of the second kind will be needed, but with the warning that they should be viewed as incomplete, improper semantic representations, which may be safely employed because, in principle, they only require easy and straightforward modifications to be embedded in a proper one.

Notice that, in both cases, the implicit assumption is that the descriptive material (i.e., that in the NumP projection; see p. 85 below, for the structure of DP I am assuming here) of a definite DP does not contribute its usual semantic contribution to the semantic representation of the whole sentence: rather, this semantic contribution turns out to be relevant only for properly identifying the antecedent of the definite DP and, hence, plays a role in the semantic processing but does not manifest itself in the semantic representation.

Finally, note that this view of definites provides a simple explanation for the well-known facts discussed by Fodor (1979) in the context of her semantic comparison between the plural universal quantifier and the plural definite article. Fodor, in fact, noticed that, despite a certain closeness in meaning between (54a) and

(54b), (55a), while not (55b), gave rise to oddness:

(54) a. I saw the boys.

b. I saw all the boys.

(55) a. #I didn't see the boys but I did see some of them.

b. I didn't see all the boys but I did see some of them.

A related contrast emerges when observing that, among the following (57a) and (57b), only (57a) is a consistent answer to (56):

(56) Are the boys we met orphans?

(57) a. No, none of the boys we met are orphans.

b. #No, only some of them are.

Fodor (1979) explains these facts by pursuing a strategy which will prove quite pervasively explored to account for empirical puzzles like most of the ones I am going to address in this dissertation: namely, she postulates that plural definite DPs come in the lexicon with an "all-or-nothing" presupposition, or, in Gajewski's (2005: 14) more recent terms, a "presupposition of Excluded Middle" (see, below, pp. 182 and 222). In other words, if NPs$_x$ is the restrictor of the plural definite article and IP$_x$ is its nuclear scope, then we would have what follows:

(58) [the NPs$_x$]IP$_x$ $\vee$ [the NPs$_x$]$\neg$IP$_x$.

I take this explanation to be inferior to one based on the anaphoric character of definite DPs, for at least the following two reasons: first, it relies on a notion of presupposition which is not amenable to being reduced to that of anaphora (at least, it is not clear to me how it would be) and which, hence, I take to lack some interesting generalizations; secondly, and more importantly, it is not clear to me why natural languages, moreover crosslinguistically, would need items coming with such an *ad hoc* presupposition when they could obtain the same semantic effects with other items they nevertheless have at their disposal, suitably merged in the clause to obtain the desired scope configuration with respect to negation.

Despite some relevant differences, these objections also extend, as far as I can see, to the other applications of the presupposition of Excluded Middle, advanced in the literature, I am going to mention later.

6.4 Indexicals

In $\mathcal{RA}^t$, as we saw, there is a distinguished basic term of the system, namely t. t behaves differently from generic variables in that it plays no role in rule (P) of Particularization. t has, however, an important feature: that if $0 = 0 \vdash_{\mathcal{RA}^t} \varphi(v/t)$, then $0 = 0 \vdash_{\mathcal{RA}^t} \varphi(v)$. In other words, informally speaking, we cannot say anything about t which follows by logic alone that we cannot say also about any generic individual. Hence, conversely, everything we say about t which cannot be said of a generic individual, will not follow by logic alone: t is, thus, the basic ingredient to formulate contingent statements (or, in the kantian terminology, *synthetic judgements*).[14] For this reason, I assume that t corresponds in $\mathcal{RA}^t$ to

[14]Even if it has become famous from Kant's *Critique of Pure Reason*, the distinction between "analytic" and "synthetic" judgements (as its further elaboration, much less clear to me, distinguishing judgements which are "synthetic *a priori*" from those which are "synthetic *a posteriori*") can already be found in the writings by Locke (and, after him, in Hume): see Locke (1964). As this distinction is usually explained, *analytic* judgements are those judgements which are true

what in natural languages is an indexical argument.

The intimate nature of communication itself suggests that there must be at least one referent whose direct experience is available both to the speaker and to the hearer, something which could be considered as a sort of basic indexical, at least until we can use this expression in a restricted "cognitive" fashion, without committing to the position that this indexical has a surface realization in any natural language.[15]

If we take this fact as our point of departure, then we should explore the strongest hypothesis one can make, namely that one basic indexical is also sufficient. Actually, a reductionist approach to indexicals like that has been advocated by Bertrand Russell, who spent several pages on the issue of indexicals, which he called "egocentric particulars".

Already Frege, in his Frege (1918), argued in favour of an indispensable "private" core in a logical reconstruction of the meanings of sentences expressed in a natural language and, further, he too maintained that only indexicals, among natural language expressions, could play the role of providing such a core.

In both Russell (1940) and Russell (1948), however, Russell endorsed two stronger theses than that of the necessity of having at least one indexical for the description of the world: he claimed that indexicals were interdefinable and, moreover, that exactly one of them could have been taken as primitive in order to define

and whose truth value is such only in virtue of their meaning, while *synthetic* judgements are those judgements whose truth value also depends on facts in the world.

[15]Someone could object that the case of a communication which does not happen "in real time" is very common, typically when someone is reading something that another person wrote some time before, and that in this case it is hard to see what this common referent could amount to. However, it seems to me that even in that case what the hearer would actually do would be "translating" the written indexical expressions in a systematic way such that they come to pick up a unique referent related to the actual point of reference, as if the speaker had it as his point of reference too. For the relevant notion of "point of reference", see below in the text.

the others (he would have belonged, as Russell said, to the "minimum vocabulary" of the language under examination). It is important to note that neither in Russell (1940) nor in Russell (1948) does he explicitly state that one indexical in particular should have been chosen as basic; on the contrary, he claims that "*here-now*" and the demonstrative *this* would serve equally well for that purpose. Nevertheless, in the concrete examples from Russell (1948), he always employs the demonstrative pronoun *this* or one of the syncathegorematic and idiosyncratic expressions "*here-now*" or "*I-now*" to obtain definitions of other indexicals.[16] It is worth noting that *this* is the only real English word among these three expressions. The following ones are the first definitions suggested by Russell in the chapter titled "Egocentric Particulars" (Russell (1948: 85)):

> "This" might be taken as the only egocentric word not having a nominal definition. We could say that "I" means "the person experiencing this," "now" means "the time of this," and "here" means "the place of this."

And later (Russell (1948: 92)):

> "This" denotes whatever, at the moment when the word is used, occupies the center of attention. [...] We may define "I" as "the person attending to this," and "here" as "the place of attending to this." We could equally well take "here-now" as fundamental: then "this" would be defined as "what is here-now," and "I" as "what experiences this."

These quotations, even if clearly showing that Russell had the same kind of problem that I am addressing in this section in mind, strongly suggest that the kind

[16] Frege, instead, in Frege (1918), devoted his attention almost entirely on the first person pronoun *I*.

of logical symbol we are looking for, which is intended to represent what hereafter I will dub the "point of reference" of the discourse, does not have a corresponding expression in (plain) English (nor in Italian): *this*, in fact, in Russell's exposition, receives an undoubtedly idiosyncratic meaning, completely detached from the huge varieties of its actual usages; from the other side, "*I-now*" is of course an evocative expression which, however, does not belong to English and, moreover, is made up of two different English indexicals.

Hence, I will assume here that t denotes the point of reference and, additionally, that, even if thus essentially involved in the logical translation of any contingent statement of natural language, t lacks a proper lexical counterpart in the languages I am dealing with and it should always be taken as an implicit argument, in these languages.

Once we accept the idea that a point of reference does exist, then we can define all other indexicals, be they pronouns, or adjectives, or tenses, or covert arguments, as morphemes whose semantic representation contains t.[17]

6.5 Plurals

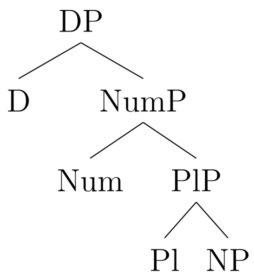
Before briefly addressing the problem of the semantics of plurals, and in consideration of the fact that the nominal domain will provide the neatest data on the basis

[17] It should be remembered, however, that indexical elements like demonstrative DPs can typically have a non-indexical use too; in this case, this use is neatly separated from the indexical one, since in the former the locative specification conveyed by the demonstrative changes into a metalinguistic specification, with different pragmatic effects on the process of anaphora resolution. This non-indexical use of demonstratives, which is of course crosslinguistically at the base of the process of formation of the definite article (see Lyons (1977)), was already implicitly subsumed in Russell (1919: 201).

For the relation between demonstrative DPs and definite descriptions, the Italian reader is directed to Vanelli (1989).

of which I will argue in favour of the application of RA to natural language semantics, it may be desirable to show the syntactic structure I am assuming (based on Abney (1987), Hudson (1989), Longobardi (1994), Zamparelli (2000), and others) for DPs:[18]

(59)



I will conform here to the view according to which plurals refer to somewhat special objects, namely plural objects; which is to say, in technically more precise terms, that each of them introduces a single discourse referent in the clause; this view has been dubbed by Barry Schein, one of its opponents, the *objectual* view about plurals. A position of this kind, which I will set out in a quite simple way (without implicating, so doing, that this is the most adequate one), is still probably the most popular in natural language semantics, and it can boast a long history starting at least as early as Russell (1903). A plural object could be described as something which is possibly formed by more than one individual of the same kind. As for the idea that plurals do not denote entities which are necessarily composed of more than one individual of the same kind (see Krifka (1986, 1989b,a), among many others; see also, in a partly different connection, Chomsky (1975: 202)), this is often referred to as the *inclusive* view on plurals, as

[18]This should not imply, I believe, that the arguments I am going to develop in the next part to defend the main claim of this dissertation depend in an essential way on the so-called "DP hypothesis" (see Szabolcsi (1983), a forerunner of this hypothesis usually ascribed to the aforementioned Abney (1987)).

opposed to the *exclusive* view, claiming, on the contrary, that at least two different individuals are necessary; the inclusive view is probably the one endorsed by most semanticists. Well-known supporting evidence displayed in favour of it is given by examples involving embedding of plurals under negation or in questions, like the following ones, from Schwarzschild (1996: 5):

(60) No doctors are in the room.

(61) Are there doctors in the room?

Of course, the truth conditions of sentence (60) and of any proper answer to question (61) (and, hence, *a fortiori* their meanings) are different, respectively, from those of the following (62) and of any proper answer to (63):

(62) No more than one doctor is in the room.

(63) Is there more than one doctor in the room?

Introduction of a single referent by plurals (even when they have a numeral determiner expressing ontological plurality, i.e. any integer from *two* up) allows for a much more natural syntax-semantics interface than we would have otherwise.

As it is well-known, plurals appear to share some features with mass nouns,[19] to the point that Bennett (1979: 264) even conjectured that the key to the semantics of mass nouns was the semantics of plural count nouns. Bunt (1979), for instance, observed that something similar to the property of *cumulative reference* that Quine (1960: 91) noticed about mass nouns, here exemplified by (64), also held for plurals:

[19] for the distinction between count and mass nouns in English, the earliest reference is Jespersen (1909a).

(64) If the content of this glass is water, and the content of that glass is water, then the content of the two glasses is water.

(65) If the animals in this camp are horses, and the animals in that camp are horses, then the animals in the two camps are horses.

I completely set aside any further consideration on this issue here. It is important, however, to stress the fact, no matter how obvious it is, that mass nouns lack a proper plural form, their plurals always having a derived denotation referring to kinds (see Mourelatos (1978: 424), for this point); hence, of course, they cannot be associated (unless they are kind-denoting) to determiners which require NPs in the plural form.[20]

Once we adopt an objectual standpoint on plurals, we also need to specify a suitable relation in order to express the fact that a plural object may be formed by a plurality of entities of a certain kind and that, at some proper level of subdivision, it must be formed only by entities of that kind: in other words, we want that, for instance, the plural noun *elephants* refers to a complex object which is possibly formed by more than one elephant and, moreover, which is formed only by elephants (unless we consider a forming relation which can be extended also to proper subparts of single elephants). Such a relation would be a mereological one and, hence, we could probably look at one of the many systems of mereology on the market in order to find useful insights towards a suitable definition in primitive recursive terms. Here I will not provide any such a definition, however, but,

[20]It has also been noticed, by Baker (1978), that the pronoun *one* may have as its antecedent a count noun but not a mass one. This should be taken, I believe, as evidence that *one* is not truly a pronoun by itself, but rather the standard numeral determiner which, as all numeral determiners, when it superficially appears in isolation always associates with a hidden partitive structure.

following Link (1983), I will dub it as the *individual part relation* and denote it with the symbol 'Π', with '$A\Pi B$' to be read as 'A is an individual part of B'.

In any case, I should at least assume that the definition of 'Π' is such that the following theorem holds:

Theorem 6.1 $A\Pi B \vdash_{\mathcal{RA}^t} A \leq B$.

We can thus specify the semantic contribution of plural morphology in the following way:[21]

(66)
$$\left[\!\!\left[\begin{array}{cc} \multicolumn{2}{c}{\text{PlP}_i} \\ \text{Pl} & \text{NP}_i \\ | & \triangle \\ [-\text{sg}] & \text{NPs} \end{array} \right]\!\!\right] \quad = \quad (\forall x \leq i)(x\Pi i \to [\![\text{NP}_x]\!]).$$

6.5.1 Adjectives of quantity and similar modifiers

Once we have defined in this way the semantic contribution of plural morphology, it becomes almost straightforward to define also that of cardinal numeral adjectives. I will do it by making use of a relation '$Card$' (with '$Card(A, B)$' to be read as 'the cardinality of A is B', or, more plainly, 'the total number of individual parts of A is B') whose definition in primitive recursive terms will remain unspecified here but does not present any difficulty in principle.

The definition is the following one (here, 'N' stands both for a numeral adjective and for a number; a similar treatment of numerals is given by Scha (1981) and is, for instance, implicit in Higginbotham (1986)):

[21] In accordance with the constraints imposed on quantification by $\mathcal{RA}^t$, the universal quantifier is of the bounded sort. In this particular case, however, this seems to be quite an innocent requirement, since the boundaries of the group denoted by A itself naturally circumscribe the domain of quantification. For the definition of the bounded universal quantifier, see p. 30 above.

(67) $$\left[\!\!\left[\begin{array}{c}\text{NumP}_i\\ \text{Num} \quad \text{PlP}_i\\ N\end{array}\right]\!\!\right] \quad = \quad \mathit{Card}(i, N) \wedge [\![\text{PlP}_i]\!]$$

Many natural language expressions of quantity arguably occupy the same layer Num within the syntactic structure of the DP; among them, in English, we can find *many*, *few*, *most*, *twice the*, *the two thirds of the*, etc.[22] Even if, for the sake of simplicity, I will avoid expressions of this kind in the following examples, again there are no principled difficulties in treating them, in the same vein as before, as denoting properties of a group.

6.5.2 Distributivity

I will assume here, in line with standard assumptions one can find in the literature and as a consequence of the syntactic structure assumed for DPs and the syntax-semantics mapping, that singular count DPs are inherently distributive, while no plural one is.[23] As it has been argued by much literature in formal semantics, the collective reading for plural DPs can be obtained in a natural way by positing a covert universal quantification over individual parts of the group denoted by the plural DP; this solution is due to Link (1983). The covert operator which carries over this universal quantification is sometimes labeled '*Dist*' and its semantics can

[22] For a recent account of the semantics of complex numeral adjectives, involving addition (*seventy-four*) or multiplication (*three thousands*), see Ionin & Matushansky (2006).

[23] As for what Scha (1981) dubbed "cumulative" readings, it seems to me that they would be better analyzed in connection with the information structure of the sentence. In particular, I take such cumulative readings to be instances of the phenomenon of discontinuous focus, which parallels that of multiple *wh*-s questions (see §6.7.2).

For an interesting correlation between the licensing of sentence-internal *different* by plural determiners like English *all* or *both*, see Brasoveanu (2011).

be given in the following way (this is not Link's (1983) original definition, but it differs from it under respects which I do not take to be essential here; note that, in the following definition, the argument i plausibly cannot be the event argument of the verb):[24]

(68) $$\left[\!\!\left[\begin{array}{c} \mathrm{IP}_{1i} \\ \diagup\!\!\diagdown \\ Dist_i \quad \mathrm{IP}_{2i}\end{array}\right]\!\!\right] \quad = \quad (\forall x \leq i)(x\Pi i \rightarrow [\![\mathrm{IP}_{2i}]\!])$$

It is not clear to me that genuine collective readings are available even for adjuncts or arguments other than those bearing the thematic role of agents of events expressed by transitive verbs.[25]

6.6 Negation

As is well-known (see in particular Horn (1989: §7.2)), the syntactic distribution of negation in natural languages does not match that of modern mathematical logic. In this tradition (and in Goodstein's formulation of RA too), negation is represented as a 1-place operator (or even, improperly, "connective") taking as its argument a proposition to yield another proposition. Horn (1989) (partly basing himself on Dahl (1979)) challenged this view by formulating a theory that he dubbed *Extended Term Logic* (ETL, for short), which was intended to do justice

[24] See n. 21 above for the presence of the bounded universal quantifier.

[25] Even if in general the correlation between thematic roles and Case can only be viewed as a tendency, while not as a systematic property of the language (at least for languages like English and Italian), such a correlation exists between the thematic role of Agent and the subject of transitive verbs in the active voice, as well as the adjunct introduced by *by*, in English, in the passive voice.

For the different thematic role of unaccusative verbs, the obvious references are Perlmutter (1978) and Burzio (1981, 1986).

to some assumptions of the aristotelian Term Logic, based on the bipartition of every sentence into a subject and a predicate.

It is true, indeed, for instance, that negation in English (as well as in Italian, among many other languages) cannot immediately precede a definite subject, at least unless it gives rise to a marked reading:[26]

(70) *Not {Gianni / {the / this} farmer} drank the lemonade.

Besides, negation, in natural languages, unlike in CL, cannot take scope over two coordinated sentences (again, unless it gives rise to a marked reading), a fact which can be illustrated by the following ungrammatical strings (where the subject is a quantified DP, thus ruling out the possibility that we are dealing with cases like those above):

(71) *Not every boy drank lemonade and every girl drank beer.
Intended reading: 'it is not true that both every boy drank lemonade and every girl drank beer'

(72) *Not every farmer beat his donkey or every donkey was lame.
Intended reading: 'it is not true that either every farmer beat his donkey or every donkey was lame'

[26] It seems that it cannot immediately precede a definite DP, whatever it is, without giving rise to such a reading, as the following (69) would show.

(69) *Gianni drank not the lemonade.

However, here my point is only to check if negation can adjoin to a whole sentence (and, thus, semantically negate a whole proposition) and this point is probably better illustrated, in a language like English, through sentences where the negation occupies the leftmost position in the surface.

As I have just said, negation may in effect immediately precede both definite subjects (and definite DPs in general) and coordinated sentences without being embedded in the first one, but only giving rise to a marked reading associated with the resulting sentence. This reading corresponds to what Horn dubbed *metalinguistic negation* (see Horn (1985) and Horn (1989: Ch.6)). One of Horn's assumptions is the following one (from Horn (1989: 472)):

> Apparent [...] instances of external negation[27] are [...] manifestations of metalinguistic negation, a means of objecting to an utterance on any grounds whatever, including its grammatical or phonetic form[.]

The following is a typical case where a metalinguistic negation is used to blame the fact that our addressee has just said something less informative than what he could have said or what we know to be the case, while the truth conditions of what he said are not denied (constituents within square brackets marked by a subscript 'F' are focused):

(73) Not [some]$_{\mathrm{F}}$ guys love Louise, [every]$_{\mathrm{F}}$ guy loves her!

Löbner (2000: 227), however, quoting an example in Horn (1989: 372), reasonably conjectures what follows:[28]

> [T]he metalinguistic, or non-propositional, quality of contrast is not due to some special mechanism of negation, but to a foregoing shift of focus to which, in a second step, standard negation is applied. *All these metalinguistic effects can as well be achieved without negation*

[27] For "external negation" Horn means here, more or less, negation as conceived in mathematical logic.

[28] Emphasis is mine.

accompanying them. This is obvious from the fact that the same metalinguistic quality is to be observed with the non-negative rectification clauses appropriate after such "metalinguistic negations".

I will continue to employ, in any case, the widespread label of "metalinguistic negation".[29]

Horn's claim, however, justifying his ETL, appears to be too strong (and Löbner (2000: §1.4), at least implicitly, is of the same opinion), as far as we can maintain that examples like the following ones do not necessarily involve a metalinguistic use of negation ((74) is Löbner's (2000: 223) (12c)):

(74) Not every city was destroyed.

(75) Not more than three boys drank lemonade.

As for the impossibility of negation taking scope over two coordinated sentences, instead, this is what Horn (1989: 476) says:

> [A]ny negation which takes scope over a conjunction, disjunction, or conditional must be metalinguistic. This is a result I already argued for in the case of the negated (i.e., rejected) conditionals [...]. With the other connectives, there is nowhere for a wide-scope descriptive negation to surface, given that conjunctive and disjunctive sentence types have no main verb, VP, or auxiliary as such. The form of wide-scope negation we do find cross-linguistically (= *It's not ({true / the case}) that Chris won and Sandy lost*) are precisely what we would

[29] Further, I depart from Löbner when he, following McCawley, states that "there appear to be good reasons not to consider contrastive focusing as a necessarily metalinguistic operation".

identify elsewhere in the same language as reflexes of the metalinguistic use of the negation operator.[30]

This is the portion I want to maintain of Horn's syntactic account of negation (except for what concerns the conditional, about which I have a slightly different story to tell: see §7.3). As it should appear clear from below, the ban on negation taking scope over coordinated sentences plays a crucial role in the explanation I am trying to offer for some of the fundamental data on which I base the present proposal. For this reason, even if I take as quite uncontroversial ungrammaticality judgements like those for (71) and (72), I want to examine this issue a bit further.

First of all, it should be noted that, crosslinguistically, there is another striking constraint on the syntactic distribution of negation, which again puts natural languages apart from mathematical logic but which appears to be independent from the issue of external negation. This is the ban on double negation (recognized, among others, by both Horn and Löbner; see Horn (1989: 470) and Löbner (2000: §1.8)).[31] It is interesting to also draw attention to this ban because it suggests a possible correlation between pragmatic and syntactic facts related to negation: given that $\neg\neg A = 0 \dashv\vdash_{\mathcal{RA}} A$ (as well as, in CL, $\neg\neg\alpha \dashv\vdash_{CL} \alpha$), i.e. a sentence A and its double negation $\neg\neg A$ are logically equivalent, and given addi-

[30] Immediately afterwards, Horn adds:

> [I]t is striking that even these metalinguistic negations are expressed by gathering the rejected conjunction / disjunction into the predicate expression with a pseudo-logical predicate (*be true*, *be the case*) invoked (and denied) for the occasion. What we do not find even here is the straightforward *not: Chris won and Sandy lost*, expressed in the canonical sentence-negation form dear to the hearts of Stoics, Fregeans, and transformationalists alike.

In this case, however, I do not share Horn's judgements: as I have said before, I take sentences like (71)–(72) to possibly receive a metalinguistic interpretation, and it seems to me that this state of affairs also extends to Horn's example in the quotation above.

[31] For the Law of Double Negation recast in primitive recursive terms, see Goodstein's (1957: 59) equation 3.31.

tionally that the $\neg\neg A$ is syntactically more complex than A, it is not surprising if, in accordance with the general pragmatic Q Principle (see Appendix A for a brief survey of the relevant pragmatic notions), only A has an unmarked syntactic realization.[32] The link between pragmatic and syntactic facts may also be at work in the case of negation over coordinate structures.

A curious fact to be noted (even if, taken in isolation, does not provide a sufficient argument for the existence of the ban under examination) is that n-connectives (i.e., connectives incorporating a negative morpheme) appear, crosslinguistically, to be formed by a negative morpheme plus the morpheme of conjunction or a morpheme somehow reducible to it.[33]

The Italian n-connective *né* can appear in two different constructions, namely the type *non* A *né* B 'not *A* nor *B*' and the correlative type *né* A *né* B 'neither *A* nor *B*'. As the paraphrases show, the same situation also holds in English, with the only difference that in the correlative construction two different negative connectives appear. The existence of two types of constructions, however, is common also to conjunction: in Italian, next to the type A *e* B '*A* and *B*', we also have the correlative type *e* A *e* B 'both *A* and *B*' (even if it is now more common to say *sia* A *che* B) and, as well as A *o* B '*A* or *B*', we also have *o* A *o* B 'either *A* or *B*'.

Given that the two elements of each series share the same meaning, but given also that the two constructions do not seem freely interchangeable in the discourse, we should look for some distinction on pragmatic grounds. I cannot address this issue here, however: what counts for the present discussion is only the semantics,

[32]Again, however, I assume that cases of double negation in natural languages are available as marked options.

[33]As Haspelmath (1997) points out, n-words in general are present in many natural languages, but not all: several Asian languages, for instance, like Japanese or Hindi, lack them.

rather than the pragmatics, of n-connectives when compared to their morphology. The etymology of Italian *né*, in fact, makes it derive from the Italian conjunction *e* 'and', as we can see, for instance, looking at the lemma *né* in Nocentini (2010):

> **né** cong. [fine sec. XII] ∼ e non.
>
> FORMAZIONE LATINA DI ORIGINE INDOEUROPEA: **lat.** *nec*, cong. coordinativa con valore negativo ▶ panromanzo: **fr.** *ni*, **occit.** *ne*, *ni*, **cat. sp.** *ni*, **port.** *nem*, **sardo** *nei*, **rum.** *nici* (dalla var. *neque*).
>
> ⧫ Il **lat.** *nec* è la var. ridotta di *neque* ricorrente davanti a parola che inizia per consonante, comp. della neg. *ne* e della cong. enclitica *que* 'e'.
>
> **né** conj. [end of XIIth cent.] ∼ and not.
>
> LATIN FORMATION OF INDO-EUROPEAN ORIGIN: **Lat.** *nec*, coordinating conj. with negative value ▶ panromance: **Fr.** *ni*, **Occit.** *ne*, *ni*, **Cat. Sp.** *ni*, **Port.** *nem*, **Sardinian** *nei*, **Rom.** *nici* (from var. *neque*).
>
> ⧫ **Lat.** *nec* is the contracted var. of *neque* occurring before a word beginning with a consonant, comp. from neg. *ne* and the enclitic conj. *que* 'and'.[34]

It is important to pay attention to the paraphrase *e non* 'and not', where the conjunction precedes the negation.

The same, *mutatis mutandis*, holds for English *neither* and *nor*, even if an element of opacity has diachronically intervened in the latter. Here are the etymologies that we can find in Klein (1966-7):

[34]Translation is mine.

> **neither,** adj., pron., adv., and conj., not either. — ME. *neither*, *neyther*, *nother*, *nouther*, *naither*, fr. OE. *nāwðer*, contraction of *nāhwæðer*, lit. 'neither of two', fr. *ne*, 'not', and *āhwæðer*, 'either of two', which is compounded of *ā-*, 'ever, always', and *hwæðer*, 'which of two'. See **no** and **whether** and cp. **either.** Cp. also **nor.**

> **nor,** conj. correlative to *neither*. — ME., contraction of *nother*, a var. of *neither*; see **neither.** The negative conjunction *nor* was influenced in form by the affirmative *or*.

As we can see, the lemma for *nor* clearly shows that the claim made by some that *nor* should be morphologically divided into a negative mark and the disjunction *or* is, at best, imprecise. So, let's also take a look at the etymology for *either*:

> **either,** adj., pron., conj. and adv. — ME. *aither*, *either*, fr. OE. *ǣghwæðer*, *ǣgðer*, 'each of two, both', for **a-gi-hwǣðer*, which is formed fr. *ā*, 'ever', pref. *gi-* (for *ge-*) and *hwæðer*, 'whether'. See **aye,** 'ever', pref. **y-** and **whether** and cp. **neither.** Cp. also Du. *ieder*, OHG. *eogiwedar*, *iowedar*, MHG. *iegeweder*, *ieweder*, *ieder*, G. *jeder*, 'either, each, every'.

Thus, the original meaning of the root of *neither* corresponds to that of *both* and, hence, can be assimilated to that of the conjunction *and* rather than that of the disjunction *or* (maybe through some intensional construction of the kind of, for instance, *Both the followings hold: A and B*).

In other words, the following rough equations in meaning between sentential schemata seem to be valid:

(76) *non A né B* $\approx$ *né A né B* $\approx$ *non A e non B*;

(77) *neither A nor B* $\approx$ *not A nor B* $\approx$ *not A and not B.*

Summarizing, Italian, Latin and English (as the other languages which have n-connectives, to the best of my knowledge) have n-connectives whose form is *n-&*, where *n-* is a negative marker and *&* a connective semantically related, at least through its etymology, to the conjunction; but the meaning of this n-connective corresponds to 'and not', where the conjunction takes scope over a negation, thus reversing the surface order of the two morphemes.[35] Now, if negation could take scope over connectives, the fact that so many languages came to have at their disposal an n-connective where, nevertheless, the scope relation between the corresponding positive connective and the negation is reversed with respect to the superficial order would be quite unexpected and require an explanation: in other words, we should ask why such n-connectives whose semantic content roughly corresponds to 'and not' do not take either the form *&-n* or the form *n-OR*, with the negative marker followed by a morpheme related to disjunction. In fact, once we have assigned a suitable meaning to negation in natural languages (as the one I am going to assign below in (85)), maintaining for the conjunction and the disjunction the semantic characterizations given above and assuming, for the sake of the argument, that no restriction on the syntactic distribution of negation works (at least, no restriction involving connectives), we obtain the following well-known semantic equivalences:[36]

[35]The same holds in the case of It. *neanche* 'not even', compound of the negative marker *ne-* and *anche* 'also', and Eng. *not even*. See Lahiri (1998) for related facts in Hindi.

[36]Known as De Morgan's Laws, after the name of the logician who rediscovered them in the Nineteenth century, but actually explicitly formulated by William of Ockham and presupposed in the work of the Pseudo-Scot. For their proofs in $\mathcal{RA}^t$, see Goodstein (1957), where they

(78) *not* [*A and B*] $\Leftrightarrow$ *not A or not B*;

(79) *not* [*A or B*] $\Leftrightarrow$ *not A and not B*.

The structures corresponding to *&-n* and *n-OR* are the ones in (79), but, even if they are semantically akin to these structures, the forms we can find in natural languages for n-connectives all instantiate the first member of (78) (see also Jaspers (2005), for some particular cases). This would be quite strange if negation could freely take wide scope over connectives.

Reversing the argument, we can conjecture that in n-connectives the negative marker has precisely the function of signalling that, at some suitable level of the syntactic representation of the sentence, we can reconstruct a negation immediately embedded under a conjunction, in both the conjuncts; the presence of negation would hence be anticipated in the linear order of the constituents.[37] This would possibly be a consequence of the well-known Neg First principle formulated by Jespersen (1917: 5) and described there as "the natural tendency, [...] for the sake of clearness, to place the negative first, or at any rate as soon as possible, very often immediately before the particular word to be negatived (generally the verb)".[38]

Notice, at least apparently supporting this conjecture, that the simultaneous occurrence of preverbal n-words and predicate negation with NC reading was possible in Old Italian, and indeed very common in the case of negative conjunction;

correspond, respectively, to Goodstein's (1957: 60) equations 3.32 and 3.321.

[37]Remember that, starting with Kutas & Hillyard (1980), a considerable amount of experimental evidence in favour of the hypothesis that interpretation is an incremental process has been collected; see also Radó & Bott (2012) for some important refinements of this general idea.

[38]Note that a similar function is assigned also to negative markers of concord items in the quite recent analysis of Negative Concord (NC) given by Zeijlstra (2004, 2006); see §7.4 below.

the following examples are from Zanuttini (2010):[39]

(80) Epicurio fue un filosofo che non seppe lettera *nè* *non*
`Epicurio was a philosopher that not knew letter nor not`
seppe disputare ...
`knew to_dispute`
'Epicurus was a philosopher who neither knew of letters nor was able to dispute'

(*Fiori e vita di filosafi*, Ch. 11, ll. 2–3)

(81) Fue accusato, ma non si trova neuna legge scritta sopra
`was accused but not IMPERS finds no law written over`
cosìfatto maleficio [reato], *né* convenevole *non* era che nne
`such made misdeed nor convenient not was that from_it`

[39]There is, besides, the well-known problem of explaining why natural languages can have at their disposal only connectives associated with the structures represented in (79) but not to those in (78). This question is an instance (actually, I believe, the only clear instance; see the observations on n-indefinites below and on Neg-raising in §7.6) of the so-called puzzle of the O corner of the aristotelian Square of Oppositions; the puzzle, in its general form, dates back to Thomas Aquinas, who noticed it in the following passage (the English translation, with slight modifications, is by J. Oesterle, quoted in Horn (1989: 253)):

> In negativis autem non est aliqua dictio posita, sed possumus accipere, *non omnis*; ut sicut, *nullus*, universaliter removet, eo quod significat quasi diceretur, *non ullus*, idest, *non aliquis*, ita etiam, *non omnis*, particulariter removeat, in quantum excludit universalem affirmationem.
>
> [80398] Expositio Peryermeneias, lib. 1 l. 10 n. 13

> There is no designated word, but *non omnis* ['not all'] can be used. Just as *nullus* ['no'] removes universally, for it signifies the same thing as if we were to say *non ullus* ['not any'; 'not some'], so also *non omnis* removes particularly inasmuch as it excludes universal affirmation.

The puzzle has been extended to connectives by Horn (1972: §4.23) and Zwicky (1973: 477). See Horn (1989: §4.5) and, more detailed, Horn (1972: Ch.4) for a famous explanation of the general case on pragmatic grounds.

scampasse sanza pena ...

`escape:SBJ;PST:3SG without punishment`

'he was charged, but nobody was able to find any written law about such a crime, nor it was right that he escaped the punishment'

(Brunetto Latini, *Rettorica*, p. 128, ll. 1–3)

(82) De la cosa del prossimo tuo non farai furto,

`of the thing of_the fellow your not do[IMP]:FUT:2SG theft`

né in tal modo *non* gliele torrai, *né* *non*

`nor not in such manner not to_him=it you_will_take nor`

l'userai contra sua voluntade.

`it=you_will_use against his will`

'you won't steal your neighbour's goods, nor will you take them in such a way, nor will you use them against his will'

(Bono Giamboni, *Libro*, Ch. 17, §31)

(83) Li veraci amici *nè* per forza d'arme *nè* per ricchezza

`the true friend neither by force of-arms nor by richness`

d'oro *non* si possono avere, ma per servigio e per fede

`of-gold nor IMPERS can have but by service and by faith`

s'acquistano.

`IMPERS-buy`

'true friends can neither be obtained through force of arms, nor thanks to abundance of gold, but they can be obtained only by devotion and faithfulness'

(*Fiori e vita di filosafi*, Ch. 21, ll. 24–25)

I should point out that, if the story about n-connectives goes the way I have just described, it could not easily also be extended to the case of n-indefinites, given that I did not rule out the possibility that negation can take scope over an indefinite DP (and the impossibility of negation taking scope over connectives played a crucial part in that story). However, at a closer inspection, n-indefinites appear crosslinguistically to be morphologically more complex than one could *prima facie* assume, being compound not only by a negative marker and an indefinite morpheme, but also by a third morpheme, usually referred to as an (overt or covert) "EVEN" element; the overt realizations of this element (like morphemes related just to *even*) arguably introduce in the semantic representation of the clause a connective, i.e. an element belonging to one of those syntactic categories which must locally precede negation in unmarked structures (see, again, Lahiri (1998) for scope inversion between negation and such EVEN element in NPIs in Hindi).

Of course, we are still lacking an explanation of why such a ban on negation over connectives should hold. Here, I will not attempt to address this issue. One possible answer could be related to the fact that in the right members of (78) and (79), unlike in the left ones, both clauses bear a syntactic mark that they are globally in a negative environment (such a line of reasoning would resemble the one pursued by Dowty (1993, 1994), basing himself on the earlier Sánchez Valencia (1991), to account for the function of NPIs and negative concord items in human reasoning; see also Hoeksema (1986)).

Since it will play an important role in the analysis of many of the crucial empirical data discussed below, it is worth stating the ban on negation taking scope over coordinate structures in the following more precise way:

(84) **BNCS (Ban on Negated Coordinate Structures)**

In the deep syntactic structure, negation is never in a position such that it is interpreted as having immediate scope over a coordinate structure.

For a coordinate structure I mean here a syntactic constituent which semantically translates as either a logical conjunction, or a logical disjunction, or a material implication.

The fact must be stressed that the ban is intended to apply already to the syntactic structure; this is because, among other things, we do not want it to cause semantic representations containing a material implication whose first argument is itself a coordinate structure to become illegitimate. The material implication, in fact, is defined as a disjunction whose first disjunct is a negated term (see definition (5) at p. 29): we do not want that logical negation, lacking a syntactic counterpart, to be considered illegitimate, otherwise we would be in trouble defining the semantic contribution of conditionals, universal quantifiers, generic indefinites and possibly other linguistic expressions.

Now, we can very simply characterize the meaning of negation in $\mathcal{RA}^t$ terms in the following expected way:

(85) $[\![\text{not IP}]\!] \quad = \quad \neg[\![\text{IP}]\!].$

To conclude this section, I want to stress the fact that the impossibility of unmarked negation taking scope over a connective may, *prima facie*, make those RA systems which are "logic-bearing" appear more natural, i.e. those systems which extend the classical propositional calculus, like the original Skolem's (1923) one or Schwartz's (1987a), which could easily be modified in order to retain only connectives from the propositional language (thus, negation should be defined as a p.r.u. function and would only have narrow scope with respect to sentential connectives). However, we should remember that logic-free and logic-bearing systems of RA are logically equivalent (see Schwartz (1987b)) and, in the light of the greater conceptual parsimony of logic-free systems, the ban illustrated above is probably best understood as a merely syntactic constraint. Hence, the choice of building semantic representations for natural languages through the formalism of a logic-free system of RA instead of a logic-bearing one is still coherent with the considerations just developed.

6.7 Dependent clauses

The semantics of dependent clauses will play an important role in the next part of this dissertation for at least two reasons: first, since all intensional verbs have a clausal complement, it turns out to be useful to somehow specify the semantic contribution of complements of that kind before addressing Neg-Raising, one of the linguistic phenomena which I take to provide support for my claim (in this regard, however, the particular semantic analysis of dependent clauses which I am arguing for plays an essential role only in explaining some quite marginal semantic effects); secondly, and more importantly, the semantic analysis I will give, even if independently motivated, sheds some light on some apparently strange scope behaviours of generic indefinites.

The semantics I am going to outline is a unified one for complements of attitude verbs, complements of modal verbs, relative clauses, antecedents of conditionals, subordinate clauses and clausal complements in topicalizations, focalizations, dislocation structures (see Postal (1971), Cinque (1977, 1990), Rizzi (1997) and Zubizarreta (1998), among others) and *wh*-questions.[40] All these cases arguably involve a CP projection, since a complementizer is realized in the surface. Thus, at least *prima facie*, a unified semantic account of theirs could find a bit of support from the syntax.[41]

I think that the discussion should start from the complements of attitude verbs, since they probably are, among the syntactic structures just listed, those which posit the hardest challenges to a theory of meaning.

As is well-known, complements of attitude verbs not only do not obey substitution of extensionally equivalent expressions, as in all intensional contexts, but they seem not to obey substitution of intensionally equivalent expressions either. The point is illustrated by the following examples:[42]

(86) a. Gianni knows that any walrus is a mammal. ?$\Leftarrow$

b. ?$\Rightarrow$ Gianni knows that any bachelor is male.

(87) a. Gianni believes that 2+2=4. ?$\Leftarrow$

b. ?$\Rightarrow$ Gianni believes that Poincaré's conjecture is true.

---

[40] This list is by no means exhaustive.

[41] This observation presupposes that main clauses are not to be conceived as CPs themselves, at least not in all cases or not in all languages. For positions of this kind held in the syntactic literature, see, for instance, Travis (1984, 1991), Ottósson (1989), Diesing (1990), Röngvaldsson & Thráinsson (1990), Zwart (1991, 1993, 1996a,b), Cardinaletti & Starke (1996) and Kiparsky (1996).

Even if it seems to me that the account I am describing best fits with such a position, it should not be considered to be incompatible, as far as I can see and when suitably adapted, with the opposite position where also main clauses are (always) CPs.

[42] The example is admittedly very involved, but I decided to add it in order to show concretely that the problem does not affect only analytic statements, like in (86), or mathematical truths, like in (87), which are by far the most common kind of examples employed in the literature to illustrate this point.

(88) a. Gianni wants his zoo to be organized in this way: there are exactly three walruses, each of them living in a different cage; for each walrus there is one and only one seal living in the cage where it lives and in love with it and, for each pair of walruses, there is one and only one seal living in the cage where the first one lives and in love with the second one; there are no other seals; besides which, there are also some baboons, each of them living in a different cage; for each baboon there is one and only one macaque living in the cage where it lives and in love with it and, for each pair of baboons, there is one and only one macaque living in the cage where the first one lives and in love with the second one; there are no other macaques; there are some giraffes too, each of them living in a different cage; for each giraffe there is one and only one zebra living in the cage where it lives and in love with it and, for each pair of giraffes, there is one and only one zebra living in the cage where the first one lives and in love with the second one; there are no other zebras; finally, each zebra likes also one and only one among seals and macaques and no two zebras like the same seal or macaque. ?$\Leftarrow$

b. Gianni wants there to be exactly four baboons and exactly five giraffes in his zoo.

The alleged entailment relations between the two sentences within each couple are marked with the interrogative dot because they have sometimes been considered as indeed holding in the literature (usually by appealing to some notion of "implicit" attitude, namely of "implicit" knowledge, belief, will, etc.), but here I assume without further discussion that they simply do not hold at all in any intuitive sense; however, the following entailment relations, between the clausal complements of the sentences above, quite uncontroversially do hold:

(89) a. Any walrus is a mammal. $\Leftarrow$

b. $\Rightarrow$ Any bachelor is male.

(90) a. 2+2=4. $\Leftarrow$

b. $\Rightarrow$ Poincaré's conjecture is true.

(91) a. Gianni's zoo is organized in this way: there are exactly three walruses, each of them living in a different cage; for each walrus there is one and only one seal living in the cage where it lives and in love with it and, for each pair of walruses, there is one and only one seal living in the cage where the first one lives and in love with the second one; there are no other seals; besides which, there are also some baboons, each of them living in a different cage; for each baboon there is one and only one macaque living in the cage where it lives and in love with it and, for each pair of baboons, there is one and only one macaque living in the cage where the first one lives and in love with the second one; there are no other macaques; there are some giraffes too, each of them living in a different cage; for each giraffe there is one and only one zebra living in the cage where it lives and in love with it and, for each pair of giraffes, there is one and only one zebra living in the cage where the first one lives and in love with the second one; there are no other zebras; finally, each zebra likes also one and only one among

seals and macaques and no two zebras like the same seal or macaque.

b. $\Rightarrow$ In Gianni's zoo there are exactly four baboons and exactly five giraffes.

The logical equivalence between (89a) and (89b) holds in virtue of them being both analytical truths, i.e. statements which are true only in virtue of the relevant meaning of the words they contain (and in particular of the relevant definitions of the words *walrus* and *bachelor*, respectively).

The logical equivalence between (90a) and (90b) derives from the fact that they are both true mathematical statements,[43] and true mathematical statements may be viewed as analytic statements too, in that they do not express a contingent truth, but a necessary one, one which holds independently of any fact in the world.

Finally, as a matter of logic, (91b) is a logical consequence of (91a), since the cardinalities of the sets of animals in Gianni's zoo turn out to be related in the following way, in accordance to what (91a) says:

$$\frac{\begin{array}{c}|\{x : x \text{ is a walrus}\}| = 3 \\ |\{x : x \text{ is a seal}\}| = |\{x : x \text{ is a walrus}\}|^2\end{array}}{|\{x : x \text{ is a seal}\}| = 9}$$

$$\frac{\begin{array}{c}|\{x : x \text{ is a macaque}\}| = |\{x : x \text{ is a baboon}\}|^2 \\ |\{x : x \text{ is a zebra}\}| = |\{x : x \text{ is a giraffe}\}|^2 \\ |\{x : x \text{ is a seal}\}| + |\{x : x \text{ is a macaque}\}| = |\{x : x \text{ is a zebra}\}|\end{array}}{9 + |\{x : x \text{ is a baboon}\}|^2 = |\{x : x \text{ is a giraffe}\}|^2}$$

It could be proved that the last equation with two variables has only the following solutions:

- $|\{x : x \text{ is a baboon}\}| = 4$;

[43] Poincaré's conjecture is a conjecture in geometric topology first formulated by Poincaré's in 1904. It was first proved by Grigori Perelman in 2002 as a particular case of Thurston's geometrization conjecture.

- $|\{x : x \text{ is a giraffe}\}| = 5$.

The only solution to the problem of logical omniscience I am aware of is that of assuming that the objects of attitude reports are actually representations of some kind.[44] This view has been widely explored in the literature and seems to have a certain affinity with the project of a representationalist theory of meaning (see, for instance, Field (1978); however, the two are not necessarily tied together).

The view that complements of verbs of attitude report denote representations can be traced back at least to Carnap (1947) (see also, in part, Quine (1956)), while the same idea for complements of modal predicates expressing necessity may be found already in Gödel (1933). Carnap's (1947) account predicts the following interpretation for belief reports (Carnap (1947: 61ff.)):

> [T]he sentence "John believes that D" in [a semantical system] **S** can be interpreted by the following semantical sentence: "There is a sentence $\mathrm{S_i}$ in a semantical system $\mathbf{S}'$ such that (a) $\mathrm{S_i}$ in $\mathbf{S}'$ is intensionally isomorphic to "D" in **S** and (b) John is disposed to an affirmative response to $\mathrm{S_i}$ as a sentence of $\mathbf{S}'$."

Carnap's rather cumbersome paraphrase is intended, of course, to avoid any sort of commitment of the agent with a specific sentence in the language of the attitude report, since it may well be the case that the agent does not even know that language, the attitude report possibly being true all the same. The problem of the semantic commitment with particular languages, even if in different forms,

[44]To embrace theories of attitude reports which share such an assumption, the label of "sententialism" is quite widespread. I prefer, however, the label of "representationalism", since, in its common use, the word "sentence" has a narrower meaning (which is arguably properly subsumed by that of the word "representation") than the one which is required for more robust theories of attitude reports.

is a problem threatening many representationalist accounts of attitude reports. Coming back to Carnap's solution, a similar but more subtle difficulty for it has been detected by Church in 1950.[45] Church considered a couple of sentences like the following ones, with (92b) being the translation in German of the English sentence (92a):

(92) a. Seneca believed that man is rational.

b. Seneca glaubte, daß der Mensch vernunftbegabt sei.

As Church observes, when we apply Carnap's scheme quoted above to (92a) and (92b), we obtain the following paraphrases (93a) and (93b), respectively:

(93) a. There is a sentence S_i in a semantical system **L** [Latin] such that (a) S_i in **L** is intensionally isomorphic to "Man is rational" in English and (b) Seneca is disposed to an affirmative response to S_i as a sentence of **L**.

b. Es gibt einen Satz S_i in einem semantischen System **L**, so daß gilt: (a) S_i in **L** ist intensional isomorph zu "Der Mensch ist vernunftbegabt" im Deutschen und (b) Seneca ist dazu disponiert, auf S_i als Satz von **L** zustimmend zu reagieren.

Church argues that, although (92b) translates (92a) and even letting aside the precise properties of Carnap's crucial notion of "intensional isomorphism", (93b) is not a good translation for (93a); a suitable translation would be, instead, the following one:

[45] Actually, Church had two different arguments against Carnap, but here I am content to illustrate only the first one, since I agree with M. Dusche that the second is plainly incorrect.

(94) Es gibt einen Satz S_i in einem semantischen System **L**, so daß gilt: (a) S_i in **L** ist intensional isomorph zu “Man is rational” im Englisch und (b) Seneca ist dazu disponiert, auf S_i als Satz von **L** zustimmend zu reagieren.

Church’s point is indeed quite intuitive and ultimately relies on a concept of translation in accordance with the requirement expressed in the following passage by Langford (1937: 53 f.):

> There is a simple test which helps us to determine whether a word is being used or talked about, namely, that of translation. A word that is being used is to he translated, while a word that is being talked about must not be (subject matter must remain unchanged under translation).

Hence, according to this criterion, (93b) cannot be a translation of (93a) because in it the quoted sentence *“Man is rational”* does not appear, while in its place *“Der Mensch ist vernunftbegabt”* does, and, besides, there is no longer the reference to English, replaced by reference to German; both these ill correspondences are amended in (94).

Despite an attempt by Burge, followed by Higginbotham and Dusche, to argue that sentences like (93b) do translate, in fact, a sentence like (93a) (hence relaxing Langford’s (1937) claim above), I believe that Church’s argument is on the right track and should be taken seriously. Of course, Carnap’s theory is only one early representationalist theory of attitude report: after him, other theories in this stream have been proposed, and I should mention at least Davidson’s “paratactic account” and Harman’s (1972) “interpreted logical forms” (“ILFs”; especially developed by Higginbotham, Larson and Ludlow and Dusche). But I think that the core ingredient to overcome Church's argument is offered by those theories elaborated in the more general framework of LOTH (the Language of Thought Hypothesis; see the discussion on mentalese in §5.2 above), especially by Field's (1978) account, relying on the classical Fodor (1975). After all, once we have at our disposal a language of thought which can be conceived as a semantic medium among different natural languages, we could well assume that the representation we want to commit our agent of the attitude to is built precisely in this language, and so we could employ it in both the paraphrases for (92a) and (92b), thus overcoming the translation problem.

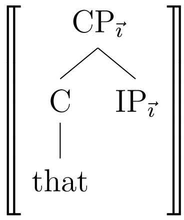
In my definition of the semantic contribution of a complementizer phrase, I will base myself essentially on Montague & Kalish's (1959) account of complements of attitude verbs and modal predicates,[46] whose main goal was that of combining a representational treatment of clausal complements with the possibility of establishing a link between linguistic material within them and variables bound by a quantifier external to them. As in their case, I will posit no limit to the number of such variables related to any embedded clause, except of course that it must be at most equal to the number of overt and covert arguments in that clause (this is partly related to my theoretical preferences on the issue of islands; see §7.2.3 below).

Hence, if $\mathrm{IP}_{\vec{\imath}}$ is a sentence containing the indices $\vec{\imath}$ (possibly among others), I will denote as '$\ulcorner [\![\mathrm{IP}_{\vec{\imath}}]\!] \urcorner(\vec{A})$' the code number that a certain bijective function encoding semantic representations assigns to the term obtained from $[\![\mathrm{IP}_{\vec{\imath}}]\!]$ by replacing each index i_j (with $0 \leq j \leq n$) with the corresponding term A_j in all its occurrences

[46]The fact that my view on definite descriptions differs sensibly from theirs does not matter here.

within $[\![\mathrm{IP}_{\vec{\imath}}]\!]$. The possibility of arithmetizing the syntax of a certain formal system has been famously put to work for the first time in Gödel (1931) for the version of Peano Arithmetic given in Russell and Whitehead's *Principia Mathematica*. Here, I omit any precise characterization of such a function for $\mathcal{RA}^t$, since it would be quite tedious while it does not present any difficulty in principle. It is fundamental to realize that the term $[\![\mathrm{IP}_{\vec{\imath}}]\!]$ is not itself part of the semantic representation when appearing in $\ulcorner[\![\mathrm{IP}_{\vec{\imath}}]\!]\urcorner(\vec{A})$; hence, rule (S) of substitution can never apply directly to a term into '$\ulcorner\ \urcorner$'.[47]

Now, I assume the following structure for $\mathrm{CP}_{\vec{\imath}}$:

(95)  $= \quad \ulcorner[\![\mathrm{IP}_{\vec{\imath}}]\!]\urcorner(\vec{\imath})$

Note that it is the syntax that determines which indices within $\mathrm{IP}_{\vec{\imath}}$ must be replaced in the semantic representation of the embedding CP: this means that possibly some generic variables contained in $\mathrm{IP}_{\vec{\imath}}$ are not among $\vec{\imath}$ and they are, so to speak, semantically "inactive", insofar as they cannot provide the input for an application of rule (P). If an index i contained within a sentence $\mathrm{IP}_{\vec{\imath}}$ also appears as an argument of $\ulcorner[\![\mathrm{IP}_{\vec{\imath}}]\!]\urcorner(\vec{A})$, I will say that i is *transparent* to the matrix clause embedding $\mathrm{IP}_{\vec{\imath}}$, otherwise I will say that it is *opaque* to it.

The picture just drawn seems to be reminiscent of some features of the notion of phase which has entered in recent times in the vocabulary of generative grammar (see Chomsky (2001)).

[47]It is precisely for this reason that the semantic theory I am arguing for cannot be compositional, still being, of course, (primitive) recursive.

The only crucial assumption on which definition (95) relies upon is that a dependent clause ultimately refers to a (semantic) representation; however unortodox it may be, this assumption, at least in some slightly different fashion, has already appeared in the literature and misses any special connection with RA.

I will address now the issue of how relative clauses could be handled within a general theory of dependent clauses along these lines.

6.7.1 Relative clauses

From a semantic point of view, the most important fact about relative clauses is probably that they behave like adjectives in possibly being intersective modifiers of the NP, something which can be rendered in logical form through the conjunction of the property denoted by the modified NP and the property denoted by its modifier.[48] But, assuming this view, a problem arises when trying to extend the representationalist account of dependent clauses laid out before also to relative clauses, namely that the semantic representation given above for dependent clauses does not express a property of any argument. This state of affairs may of course suggest that, after all, it is not a very good idea to try to extend the representationalist account also to relative clauses. However, it seems to me that the risk in not pursuing this strategy is that of underestimating the indication towards a partially uniform treatment provided by syntax.[49]

[48] I am setting aside, here, the issue of *non-restrictive* (also *appositive*) relative clauses; see Ross (1967a, 1986) and Cinque (1978, 1981) on this topic.

[49] Of course, a solution which would be immune to this objection would be that of abandoning a representationalist account for all kind of dependent clauses. But this move would be at the expense of having the problem of logical omniscience come back. Besides, as I will argue in §7.2.8 below, there is also another important reason which points towards a representationalist analysis of all dependent clauses, but since, unlike the one just mentioned, it is dependent on the main theoretical claim of this dissertation, I will not consider it here.

In order to combine the virtues of both the intersective view of nominal modification and the representationalist view of dependent clauses, I will assume that, in the case of relative clauses, the operation of merging a relative clause CP_i with a noun phrase NP_i not only introduces a conjunction in the associated logical form, but it also introduces, maybe located in a layer within a (tentative) AdjP projection, a covert predicate Op. In the semantic representation, '$Proof(A, B)$' is shorthand for '$(\exists x \leq \varphi(A))proof'(x, B)$', which holds for two terms A and B iff B is the code of a term of $\mathcal{RA}^t$ such that there is a proof of '$B = 0$' in $\mathcal{RA}^t$ of length at most $f(A)$ (under some suitable measurement of the length of proofs and with $f(A)$ a specified term containing A).[50] Again, Gödel (1931) showed, for the version of Peano Arithmetic of *Principia Mathematica*, that the bounded notion of provability (i.e., the notion of provability through a proof not exceeding a certain length, possibly determined in functional terms) in that system could be captured by a primitive recursive function, and again his result extends to our system without any principled difficulty. Schematically, so, I assume the following semantic representation of a NP modified by a relative clause:[51]

[50] It seems to me that, as for the presence of the unbounded existential quantifier here, considerations analogous to those made relative to the unbounded universal one in nn. 21 and 24 are valid also in this case.

[51] Syntactically, I have in mind the quite classical head external analysis of relative clauses, assumed in Montague (1970a) and which can be drawn back to Quine (1960). However, it seems to me that also the head raising and the matching analysis are compatible with such a semantic account.

Note, further, that the binding argument of '$Proof$' in the semantic representation could also have been different from t. From here on, however, I will make this assumption for all extensional contexts; in the intensional ones, instead, I will take the binding argument to be the situation (or world) argument introduced by the intensional operator. Simply, it seems to me that these arguments are particularly salient for that purpose, but admittedly I have no robust evidence supporting this impression (see also n. 10 below).

(96) $$\left[\!\left[\Tree [.\text{NP}_{1i} \text{NP}_{2i} [.\text{AdjP}_i [.\text{Adj} \text{Op}] \text{CP}_i]] \right]\!\right] = [\![\text{NP}_{2i}]\!] \wedge \mathit{Proof}(t, [\![\text{CP}_i]\!])$$

Informally speaking, this amounts to saying that the NP *cat which scratched Gianni* is assumed to be roughly equivalent to the NP *cat such that a proof exists that it scratched Gianni*.

Notice that in cognitive sciences and AI it is quite customary to assume that a cognitive agent whatever always has upper bounds for the computations he displays; Step Logic (whose first appearance is in Drapkin & Perlis (1986)), for instance, formally develops the assumption that an agent's beliefs are indexed by time points or steps corresponding to stages in the agent's reasoning, thus allowing for a definition of a non-omniscient time-bounded reasoner.

6.7.2 Focus and *wh*-questions

The syntax and semantics of focus is strictly intertwined with those of questions and *wh*-items. Fundamental studies on the semantics of questions are Hamblin (1958, 1973) and Karttunen (1977) (the last one partly relying on Ross (1970)); besides, see also Bolinger (1978) for matching and alternative questions, Groenendijk & Stokhof (1981, 1982, 1983); Groenendijk et al. (1984), Heim (1994), Rullmann (1995) and Beck & Rullmann (1999), already cited, for strong exhaustivity in embedded questions, Groenendijk & Stokhof (1984b,a) and Krifka (2001) for coordinated and pair-list questions and Isaacs & Rawlins (2008) for conditional questions. The investigation of the semantics and pragmatics of focus ultimately springs from Paul (1880) and Jackendoff (1972); an influential analysis in terms of presupposition, based on Hamblin's alternative semantics, is developed in Rooth (1985, 1992, 1996) (compare Karttunen & Peters (1979) for differences with clefts, which are of course related); see also Chomsky (1976), Jacobs (1983), von Stechow (1985, 1991), Rochemont (1986), Krifka (1991), É. Kiss (1998), Zubizarreta (1998) and Herburger (2000), for different analyses, Szabolcsi (1981) for exhaustivity of focus (at least in Hungarian) and Taglicht (1984) for an early discussion of discontinuous focus. The syntactic parallelism between *wh*-items and focused constituents is explicitly established, for instance, in Chomsky (1977b).

As is well-known, there is a systematic correlation between *wh*-items in questions and constituents receiving a marked intonation (i.e., *focused* constituents) in answers, which is illustrated by the following examples.

(97) a. Who gave two umbrellas to Luisa?
b. [Gianni]$_F$ (gave {two umbrellas / them} to {Luisa / her}).

(98) a. What did Gianni give to Luisa?
b. [Two umbrellas]$_F$ ({Gianni / he} gave to {Luisa / her}).

(99) a. Who did Gianni give two umbrellas to?
b. To [Luisa]$_F$ ({Gianni / he} gave {two umbrellas / them}).

(100) a. How many umbrellas did Gianni give to Luisa?
b. [Two]$_F$ ({umbrellas / of them} ({Gianni / he} gave to {Luisa / her})).

(101) a. How did Gianni give two umbrellas to Luisa?

b. [Roughly]$_{F}$ ({Gianni / he} gave {two umbrellas / them} to {Luisa / her}).

(102) a. When did Gianni give two umbrellas to Luisa?

b. [Yesterday]$_{F}$ ({Gianni / he} gave {two umbrellas / them} to {Luisa / her}).

(103) a. Where did Gianni give two umbrellas to Luisa?

b. [At the railway station]$_{F}$ ({Gianni / he} gave {two umbrellas / them} to {Luisa / her}).

(104) a. Why did Gianni give two umbrellas to Luisa?

b. [Because it was raining]$_{F}$ ({Gianni / he} gave {two umbrellas / them} to {Luisa / her}).

Again in a parallel way, as we may have *multiple wh-s* within the same sentence, we also have sentences with a so-called *discontinuous focus*:[52]

(105) a. Who did Gianni introduce to whom?

b. [Filippo]$_{F}$ to [Susanna]$_{F}$ ({Gianni / he} introduced).

Finally, we may have embedded *wh*-questions as well as embedded focus/background structures, and as a particular case ((108a) and (108b), respectively) even self-embedded ones (*wh*- embedded questions are nothing other than free relative clauses, while focus/background self-embedded structures are the cases usually referred to in the literature as *multiple focus*; see Krifka (1991) and n. 52 above):[53]

[52]Because of a widespread choice of terms which I consider not entirely felicitous, the existing label of *multiple focus* is associated with a phenomenon which is not the parallel of multiple *wh*-s.

[53]I should warn the reader that examples (107b) and (108b) with focus display an ambiguity

(107) a. Luisa told Gianni who was at the party.

b. Luisa told Gianni that [Susanna]$_F$ was at the party.

(108) a. Who told Gianni who was at the party?

b. [Luisa]$_F$ (told {Gianni / him} {that / that [Susanna]$_F$ was at the party}).

It is widely agreed that both questions and answers project a CP layer in the syntactic structure: the reason why I included focus and related phenomena within the present section about dependent clauses is precisely that I share this rather uncontroversial assumption. Moreover, I believe that this syntactic fact, in conjunction with the apparently bizarre semantic analysis I suggested for all dependent clauses, may provide the key towards a deeper understanding of Rooth's (1985) insight (see also Rooth (1992, 1996)) that focus (and questions) triggers a presupposition of its correspondent background, despite this not being an existential presupposition (i.e., the presupposition that at least one true instantiation of the sentence-schema provided by the background exists). Rooth (1985, 1992, 1996), furthermore, offers an analysis not only of focus, but also of association with focus, which involves several adverbial modifiers known as *focalizing adverbs* like Eng. *only*, *even*, *also*, *at most*, *at least*, *exactly*, etc.

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which does not find an analogous one in the corresponding examples (107a) and (108a) with *wh*-s, respectively. The ambiguity is between an embedded (the one intended) and an unembedded reading of the rightmost focused element (with the unembedded reading making (108b) turn into a case of discontinuous focus). As for (107b), for instance, the two readings may find the following corresponding rough paraphrases with cleft structures:

(106) a. Luisa told Gianni that it was Susanna who was at the party.

b. It was Susanna who Luisa told Gianni was at the party.

Chapter 7

Challenging semantic representations

7.1 Bounded quantifiers

In this part, I will restrict my attention only to universal and existential (covert or overt) quantifiers in natural languages. This may be viewed as a strong limitation, given the apparent wide range of determiners with different meanings which natural languages have at their disposal. I have already said, however, that here, following the tradition of dynamic semantics, I am treating definite DPs as mere anaphoric elements (see §6.3). Besides, as I said in §6.5.1, many other determiners are treated here as adjectives of groups, following, for instance, Verkuyl (1981). Once we decide to treat words like these as adjectives, it is even more difficult to treat them as determiners, in line with the tradition of generalized quantifiers theory (Barwise & Cooper (1981)), some complex expressions like Eng. *more than* n, *less than* n, *at most n*, *at least n*, *only n*, *even n*, compound by a focalizing adverb [1] and a numeral n (see Krifka (1999)). Finally, so-called negative quantifiers have received sound analyses treating them as syntactically complex elements, in particular as elements introducing or relating to a negation in the syntactic structure (as an early reference, see Jacobs (1980), who analyzed German negative quantifiers as existential quantifiers within the immediate scope of a negation; but see §7.4).

As for conjunction, disjunction and negation, I already said that they behave in the same way in RA and in classical first order logic, provided that we accept the formal translations I gave before. What is, instead, intrinsically beyond the limits of any version of RA are quantifiers conceived in the customary way. Notice that, actually, under consistent finitist assumptions, we had better say, with Goodstein (1951) (see the quotation at p. 17 above), that standard logic fails to provide the right interpretation to natural language quantifiers.

The way quantifiers are defined in RA is based on the notion of *bounded* quantification. The idea lying behind it is the one, common also to standard logic and famously expressed in Wittgenstein's *Tractatus*, that universal and existential quantifiers correspond respectively to plural conjunction and disjunction. The difference between standard logic and RA consists in the cardinality which is assigned to these pluralities: in standard logic it is an infinite one (at least the infinite of natural numbers, $\aleph_0$), i.e. universal and existential quantifiers are conceived respectively as an infinitary conjunction and disjunction;[2] while in RA there is an upper bound to the number of conjuncts and disjuncts, respectively. In other words, and

[1]Early analyses of some of these modifiers, offering seminal insights for further developments, are Fillmore (1965) on *even* (see also Fillmore et al. (1988)) and, moreover, Horn (1969, 1972) on both *only* and *even* and Horn (1996) and Geurts & van der Sandt (2004) on *only*.

[2]Remember, however, that, as I said in §2, later Wittgenstein abandoned this view to embrace a full-fledged finitism. See the discussion on p. 18 and in particular n. 16.

remembering that our domain of quantification is the set $\mathbb{N}$ of natural numbers, a quantifier which is conceived classically is unbounded, in that it is assumed that it can range over all natural numbers, while a quantifier defined as a primitive recursive function must be bounded, i.e. it can only range over natural numbers up to a certain one.

As I have said before (p. 36), what behaves like unbounded universal quantifiers in $\mathcal{RA}^t$, under certain circumstances, are generic variables, but they are crucially not functions (hence, not binding terms) and thus they cannot be embedded in the scope of any operator, something which natural language quantifiers, instead, appear able to do.

Of course, since all constraints on the domain of quantification can be put explicitly in the scope of the quantifier (quite naturally, when considering restricted quantifiers, within the restrictor itself), technically we can express primitive recursive quantifiers in terms of the standard ones. The converse is obviously impossible, but we can still maintain (and this is what I am actually doing) that the infinitary interpretation of quantifiers simply does not make sense at all.

7.1.1 Existential import

Before coming to semantic representations of natural language quantifiers, there is one important issue I should address, even if only briefly: the problem of so-called *existential import* of quantifiers. An example of existential import is given by the following sentence (109), which seems to presuppose the information in (109a):

(109) Every mouse in this room is grey.

a. There is at least one mouse in this room.

That the content of (109a) is not actually asserted by (109) is proven, in the usual manner (see Strawson (1950)), by considering its negative counterpart (110) and realizing that it seems too to presuppose (109a):

(110) Not every mouse in this room is grey.

Notice that the label of "existential import", even if it is probably the most widespread in the literature, may be somewhat misleading, since the phenomenon does not necessarily imply the existence of an individual of a certain kind in the real world. This is well illustrated by the following example:

(111) If there are mice in this room, then every mouse in this room is grey.

In this case, the existence of at least one individual of the relevant kind (i.e., of at least one mouse in the room) holds only at a local level, not necessarily at a global one. If we assume that existential import is a genuine case of presupposition, this fact is, of course, not surprising at all, since consequents of conditionals are a typical case of *filters*, in Karttunen's (1973) terminology, i.e. of linguistic environments which do not always allow presuppositions to project in larger environments they are embedded within.

In the literature, it is sometimes said that determiners having existential import are *partitive*, in a technical sense which is of course related, but still different from, the sense in which we speak of true partitive constructions (i.e., complex DPs which in English appear in the form *DP of* Det_{def} *NPs*). However, since it seems to me that nothing really forces a global interpretation of the label "existential import", since it is probably the most common means used to refer to the phenomenon and since its most accredited competitor, namely the adjective "partitive", is itself not completely immune to risks of misunderstandings, I will maintain the standard label without further hesitations.

There has been a lot of discussion, among linguists and philosophers, whether this phenomenon is better understood as a genuine case of presupposition or rather as an implicature, i.e. arising as a by-product of independent pragmatic principles, maybe ultimately grounded on processing strategies.[3] It seems to me that it would be hard to reconcile this latter position with data like, for instance, the following, where the oddness of both the question and the answer appears to be independent from any property of the context other than the one specified:

(112) (*Scenario*: There is no mouse in the room.)
A: # Is every mouse in this room grey?
B: # Yes.

Furthermore, notice that, under the assumption that the existential import of the universal quantifier *every* arises only as a by-product of some pragmatic principles, the answer in (112) would be true!

The standard gricean pragmatic account of the existential import of the universal quantifier is a typical case of a quantity implicature (see Geurts (2010) and Appendix A). However, Geurts (2007: 256 f.) shows, *contra* Abusch & Rooth (2004), that the implicature story simply does not work. In a non-presuppositional account, a sentence like the following (113) (Geurts's (2007: 257) (8)) would unilaterally entail both, say, (114) and (115) (Geurts's (2007: 256) (6a)); hence, (113)

[3] Some historical remarks on the existential import of universal quantifiers can be found in Kneale & Kneale (1962) and, moreover, Horn (1989: §1.1.3).

is a stronger statement of both (114) and (115), and thus, having uttered either (114) or (115), the speaker conveys the information that he was not in a position to utter the more informative (113), i.e. he conveys the information in (116).

(113) There are no Swiss matadors.

(114) No Swiss matador adores Dolores del Rio.

(115) Every Swiss matador adores Dolores del Rio.

(116) It is possible that there are Swiss matadors.

However, as Geurts notices, a genuine conversational implicature should be cancellable by suitable prosecutions in the discourse: this is actually the case with (114), as the following (117a) and (117b) show, but it it is not the case with (115), as shown by the corresponding (118a) and (118b) (Geurts's (2007: 257) (10a) and (10b), respectively):

(117) a. No Swiss matador adores Dolores del Rio — in fact, there are no Swiss matadors (at all).

b. No Swiss matador adores Dolores del Rio, and maybe there are no Swiss matadors (at all).

(118) a. #Every Swiss matador adores Dolores del Rio — in fact, there are no Swiss matadors.

b. #Every Swiss matador adores Dolores del Rio, and maybe there are no Swiss matadors.

In other words, Abusch & Rooth's (2004) pragmatic story seems to work for *no*, but should face serious problems with determiners like *every*.

Here, I will simply assume that several determiners in English (and in many other languages as well) do have an existential import, quoting some studies in the relevant literature where the reader can find more in-depth observations supporting this point of view. In particular, the first scholar who argued that universal quantifiers have existential import conceived as a presupposition was Hart (1951),[4] who claimed the following (Hart (1951: 207)):

> [A]ny one who in normal discourse asserts such a sentence as, e.g., 'All taxi drivers are well-read', and appears to be making on this occasion a serious assertion will be properly taken to believe the corresponding existential sentence to be true. For otherwise he could have no reasons for asserting it. [...] If we want a word we can say that the [universal] form in the absence of a special indication 'presupposes' or 'strongly suggests' the truth of the existential form. But these psychological terms ill convey the conventional character of the connection.

The same view was expressed in Strawson (1952), even if relative to a list of different English determiners (see Heim & Kratzer (1998: 160 f.)). This work has been the source for the strongest hypothesis on existential import formulated in the literature, due to McCawley (1972), according to which all determiners would have existential import. This hypothesis has proved barely tenable: in particular, de Jong & Verkuyl (1985) (followed by Diesing (1992), among others)

[4]see the discussion in Horn (1989: §1.1.3) and in particular the views expressed by Apuleius and reported by Sullivan (1967).

For experimental evidence supporting the existential import of *every*, see Rips (1994) showed that there seems to be a clear correlation between *strong* determiners and presuppositional determiners, on one side, and *weak* determiners and determiners which are not necessarily presuppositional, on the other side, where the opposition between strong and weak determiners is to be understood in Milsark's (1974) terms (see also Milsark (1977)). In Milsark's terminology, strong determiners are those which are bad in *there be* sentences (like *every*, *most*, *both*, *each*), while weak ones are those which are fine in such a linguistic environment (like numerals, *no*, *few*, *many*):[5]

(120) a. There is {a / no} unicorn.

b. There are {two / few / many} unicorns.

(121) a. #There is {every / each / neither} unicorn.

b. #There are {all / most / both} unicorns.

[5]Here, I am simply using the terminology used by Milsark himself: following on from the previous discussion in 6.5.1 and at the beginning of this section, it should be evident that I am implicitly assuming that some of these words are not true determiners.

It is quite customary, in the literature, to find sentences like those which are marked here as simply odd marked as plainly ungrammatical. I think that such sentences are not, in fact, really ungrammatical. Consider, for instance, one of the variants of (121a) in the specified scenario:

(119) (*Scenario*: B read to A the descriptions, made by a certain explorer who many take to be mad, of three unicorns, each one with its own distinguishing features, living in a remote island in the Eastern portion of the Pacific Ocean. Both A and B are in doubt whether to trust the story or not. A decides to reach that island himself and, after coming back, runs breathless to B and tells him what follows.)
A: There is every unicorn!

Of course, this is not a scenario which appears to be relevant when a sentence like (119) is uttered out of the blue. Out of the blue, the locative argument in *there be* sentences is normally understood as covering the whole universe of discourse and, under this assumption, (119) turns to be a tautology (this is the same explanation given by Barwise & Cooper (1981: §4.3); see Portner & Partee (2002: 96)).

Note that in all the Italian counterparts of (121b) complex determiners incorporating the definite article are involved (this is of course connected to the fact, repeated below in §7.2.2, that English bare nouns may be definite DPs, while Romance ones cannot; see Heim & Kratzer (1998: 161) on the hidden partitive structure associated with Eng. *all*: for this reason, hereafter, when speaking of the universal quantifier I will only refer to singular determiners like *every* or its counterparts in other languages):

(122) #Ci sono {tutti gli / la maggior parte degli / entrambi gli} unicorni.
`there are all the the greater part of_the both the unicorns`
'there are {all / most / both} unicorns' It.

It is often assumed that strong determiners are all presuppositional (see de Jong & Verkuyl (1985) and Diesing (1992), among others), and here I align myself with this view. It is more controversial, instead, the treatment of weak determiners, as far as existential import is concerned (see Heim & Kratzer (1998: §6.8) and in particular Heim & Kratzer (1998: §6.8.4)). It seems reasonable to assume at least that they do not mandatorily have existential import. From this point of departure, we have two alternatives: stating that they are ambiguous between presuppositional and non-presuppositional interpretations (as, again, in de Jong & Verkuyl (1985) or Diesing (1992), the latter arguing for a structural rather than a lexical ambiguity), or maintaining that they are never presuppositional and trying to explain presuppositional effects in terms of the intervention of independent factors.[6] Here, I will endorse the last position, directing the reader to the indicated references for arguments supporting it.

One needs to carefully distinguish between weak determiners and persistent determiners, in Barwise & Cooper's (1981) sense. Here is a formulation of the relevant definition of persistence for determiners:

Definition 7.1 *A determiner Det is* persistent *iff, for any noun phrases* NP_{1x} *and* NP_{2x} *and any verb phrase* VP_x*, if* NP_{1x} *is a hyponym of* NP_{2x} *(in symbols,* $\mathrm{NP}_{1x} \subseteq \mathrm{NP}_{2x}$*), then* $[\mathit{Det}\ \mathrm{NP}_{1x}]\mathrm{VP}_x$ *entails* $[\mathit{Det}\ \mathrm{NP}_{1x}]\mathrm{VP}_x$ *(in symbols,* $[\mathit{Det}\ \mathrm{NP}_{1x}]\mathrm{VP}_x \Rightarrow [\mathit{Det}\ \mathrm{NP}_{1x}]\mathrm{VP}_x$*).*

Given this definition, both Eng. *some* and *a* turn out to be persistent determiners,[7] as the following example may serve to illustrate:

(123) a. black cat $\subseteq$ cat

b. (i) {A / Some} black cat loves beer.

(ii) $\Rightarrow$ {A / Some} cat loves beer.

Here, I want to argue that not all persistent determiners are also weak. In particular, one counterexample is represented by determiners like It. *qualche* 'some (sg.)' and *alcuni* 'some (pl.)', as opposed, respectively, to the indefinite article *un(o) / una / un'* 'a(n)' and to the so-called *partitive article dei / degli / delle*

[6] Among others, topicality and focalization; but simply consider also the widely independently attested possibility of implicit content associated with the restrictor discussed, for instance, in Kuroda (1982) and Sperber & Wilson (1986)).

[7] Actually, I would be inclined to say that none of them occupy the head position in a DP (see the beginning of this section and n. 11 below), but, given that they are very commonly treated as such, at least in the semantic literature, and that it seems to me that trying to be more precise would result in increased complexity of exposition, for now and henceforth I will quite freely speak about elements like that as "determiners".

'sm' (see Chierchia (1997) and Zamparelli (2002b) on the morphological structure and the semantics of the Romance partitive article).[8] All three determiners are persistent, as illustrated by the following examples:

(124) a. pugile miope ⊆ pugile
boxer short-sighted boxer
'short-sighted boxer' ⊆ 'boxer'

b. (i) {Un / Qualche} pugile miope studia linguistica.
a some boxer short-sighted studies linguistics
'{a / some} short-sighted boxer studies linguistics'

(ii) ⇒ {Un / Qualche} pugile studia linguistica.
a some boxer studies linguistics
'{a / some} boxer studies linguistics'

c. (i) {Dei / Alcuni} pugili miopi studiano linguistica.
sm some boxers short-sighted study linguistics
'{sm / some} short-sighted boxers study linguistics'

(ii) ⇒ {Dei / Alcuni} pugili studiano linguistica.
sm some boxers study linguistics
'{sm / some} boxers study linguistics'

[8] *sm* is here, in accordance with the standard usage in the literature, the written form corresponding to the unstressed variant of *some*; see the discussion on p. 147 below.

However, *un* and *qualche*, on one side, and *dei* and *alcuni*, on the other side, are not freely interchangeable in *there*-sentences and, in general, in existential assertions:

(125) a. In questo villaggio, {c'è / esiste} un unicorno.
`in this town there_is exists a unicorn`
'in this town, there {is / exists} a unicorn'

b. #In questo villaggio, {c'è / esiste} qualche unicorno.
`in this town there_is some unicorn`
'in this town there {is / exists} some unicorn'

(126) a. In questo villaggio, {ci sono / esistono} degli unicorni.
`in this town there are exist sm unicorns`
'in this town, there {are / exist} sm unicorns'

b. #In questo villaggio, {ci sono / esistono} alcuni unicorni.
`in this town there are exist some unicorns`
'in this town, there {are / exist} some unicorns'

I believe that Italian illustrates this point better than English would do, since in Italian the opposition between *un* and *qualche*, on one side, and *dei* and *alcuni*, on the other, seems indeed to correspond to minimal couples whose members only differ, in synchrony, for the determiner having existential import or not. In English, on the contrary, the difference between *sm* and *some* may not be entirely clear.[9] Besides, despite the glosses and paraphrases given above, Eng. sg. *some* is far from being an exact equivalent of It. *qualche* (the point is illustrated in

[9]Note, as well, that some overlap between the partitive article and *some* (arguably, *sm*) should be however reasonably assumed, given that Romance mass NPs, if not used as kind-referring, cannot restrict the existential presuppositional determiner, still possibly being the restrictor of the partitive article, while in English they can appear introduced by *some*:

Zamparelli (2007)) and is, instead, closer to It. *un qualche* 'a some' (usually, It. *qualche* is translated by pl. *some*, instead of singular one, but still this would be the same translation of It. pl. *alcuni*, while there are undoubtedly both semantic and pragmatic differences between *qualche* and *alcuni*; see Alonso-Ovalle & Menéndez-Benito (2008, 2010) on Spanish sg. *algún* and Alonso-Ovalle & Menéndez-Benito (2011, 2013) on its plural counterpart *algunos*, at least partly paralleling the distinction between *qualche* and *alcuni*).

Nevertheless, I will assume, following Strawson (1952), that Eng. *some*, in its stressed variant (probably not the same thing as the focused variant of *some* which Büring (1996) takes to be the exclusive source of its partitive readings), do have existential import.

Now, the time has come to give the semantic representations for some English syntactic structures whose outermost operator is a quantifier (here, in line with all syntactic theories I am aware of, I am implicitly assuming a ban on vacuous quantification, as the one proposed by Chomsky (1982)):[10]

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(127) *Dammi qualche vino, per favore.
`give_me some wine please`

(128) Dammi del vino, per favore.
`give_me sm wine please`
'give me some wine, please' It.

(129) Give me some wine, please.

[10]For the sake of simplicity, from here on, I will take t itself to be the binding argument of each bounded quantifier whenever the quantifier is not embedded in an intensional context; in this last case, instead, I will take the binding argument to be the world (or situation) argument introduced by the intensional operator. I am not entirely sure that this is always a viable option (even if I am inclined to think so), but, in any case, it makes things simpler and, moreover, does not affect the arguments I am trying to develop (see n. 51 above).

As one can see, with the following semantic representations I do not want to commit myself to the view that all the layers within DP must always project.

(130) $$\left[\!\left[\left[_{\mathrm{IP}_{1i}} \left[_{\mathrm{DP}_{i,x}} \left[_{\mathrm{D}}\ \text{every}\right] \ \mathrm{NP}_{i,x}\right] \ \mathrm{IP}_{2x}\right] \right]\!\right] \quad = \quad (\forall x \leq t)(x \Pi i \rightarrow [\![\mathrm{IP}_{2x}]\!]);$$

(131) $$\left[\!\left[\left[_{\mathrm{IP}_{1i}} \left[_{\mathrm{DP}_{i,x}} \left[_{\mathrm{D}}\ \exists\right] \ \left[_{\mathrm{PlP}_{i,x}}\ \text{some NPs}\right]\right] \ \mathrm{IP}_{2x}\right] \right]\!\right] \quad = \quad (\exists x \leq t)(x \Pi i \wedge [\![\mathrm{IP}_{2x}]\!]);$$

(132) $$\left[\!\left[\left[_{\mathrm{IP}_{1}} \left[_{\mathrm{DP}_{x}} \left[_{\mathrm{D}}\ \exists\right] \ \left[_{\mathrm{NumP}_{x}} \left[_{\mathrm{Num}}\ \text{a}\right] \ \mathrm{NP}_{x}\right]\right] \ \mathrm{IP}_{2x}\right] \right]\!\right] \quad = \quad (\exists x \leq t)([\![\mathrm{NP}_x]\!] \wedge [\![\mathrm{IP}_{2x}]\!]).$$ [11]

The semantic representations above reflect the fact that, in the case of DPs having existential import, I take the restrictor to be only a tool to recover the proper antecedent for the anaphoric link. However, since logical forms where the property conveyed by the restrictor is supplied also at the local level (i.e., in the case of universal quantification, within the antecedent of a material implication;

[11] This semantic representation reflects the fact that I take the English indefinite article to simply be the unstressed non-pronominal variant of the numeral *one*; there are, of course, morphological facts possibly justifying this assumption (the same considerations also apply to the indefinite article in Italian, where the morphologic link is even more evident).

On generic *one*, see Moltmann (2006).

while in the case of existential quantification, within the first conjunct of a conjunction) rather than only at the global level where the antecedent is merged into the structure which is input to interpretation are logically equivalent, hereafter I will conform to the standard practice of making these properties appear (also) at the local level in the semantic representations. This should avoid useless complications when comparing universal quantifiers and generic indefinites in the next sections.

Of course, when dealing with quantifiers in natural language semantics, of particular importance is the problem of how they can take scope, especially given the availability of so-called *inverse scope* readings, where two operators are interpreted with a reciprocal scope which does not reflect their order in the surface.[12] One of the earliest solutions to the problem of quantifier scope in natural languages proposed in the literature has been the so-called *Quantifying-in*, first formulated in Montague (1970a) (see Montague (1974: 204–5)). Essentially, Quantifying-in is a syntactic approach to quantifier scope, in that it establishes a connection between different scope configurations in the semantic representation of a sentence and different syntactic representations corresponding to it. Its main difference with the other major syntactic approach to quantifier scope, namely the one based on *Quantifier Raising* (QR; see Chomsky (1976) and May (1977, 1985)), is that, since it does not assume, unlike the QR approach, that quantifiers take scope only after they have obeyed some movement rule at a certain level of the syntactic rep-

[12] At least starting with Ioup (1975), it has often been argued that the information structure of inverse scope readings is marked. I believe that this may be indeed a reasonable conclusion in some cases of inverse scope readings. However, there is evidence enough from experimental studies (see references at p. 155) that inverse scope is more difficult to process than direct one; hence, several judgements of markedness may be ascribed to a hard processing rather than to a complex information structure.

resentation, the Quantifying-in approach does not predict that quantifier scope is constrained by the same kind of limitations which constrain overt movement (on this issue, see already Chomsky (1975)); as I will go into further in §7.2.3, the empirical tenability of many, if not most, of such constraints has been challenged from several sides.[13] However, I follow Hofmeister & Sag (2010) in maintaining that at least the Coordinate Structure Constraint (CSC; see Ross (1967a, 1986) and Schmerling (1972)) holds as a genuine syntactic constraint.

It is worth remembering here what the CSC amounts to, since it will play an important role in the analysis of some crucial examples in the next section. Here I will formulate it in the following manner:

(133) **Coordinate Structure Constraint (CSC)**

An element may be extracted from a coordinate structure only if it is extracted from both the coordinated clauses and it is either an adjunct or it receives the same case in both of them.[14]

It is plausible that the CSC, as well as the BNCS stated above, can find a pragmatic motivation to the extent of making inferences simpler; in this way, there would also be a uniform principle of economy applied to reasoning justifying both these syntactic restrictions.

[13]Quantifier Raising is often advocated to explain phenomena such as Antecedent Contained Deletion (ACD); but see already Cormack (1984) for an account of ACD as simple V-ellipsis instead of VP-ellipsis, compatible with other syntactic assumptions.

[14]For the theory of Case, the obvious references are Chomsky & Lasnik (1977) and Rouveret & Vergnaud (1980).

This formulation is intended to cover also the well-known case of so-called "across-the-board" (ATB) extraction (see already Ross (1967a, 1986) himself).

The need for implicit arguments, such as the one introduced as the boundary argument in the definition of bounded quantifiers, is supported by massive independent evidence collected in the semantic literature: I have already cited Kuroda (1982) and Sperber & Wilson (1986) among the earliest studies addressing this topic. The first indirect argument at supporting that, however, has probably been put forward already by Aristotle, when he defends, by introducing his notion of the *distinction of respects*, the possibility of consistently asserting a superficial contradiction in natural language. Aristotle makes this point in the celebrated and *ad nauseam* discussed passage from *Metaphysics* (1005b 19–23) (Aristotle (1984); italics in the English translation mine):

> τὸ γὰρ αὐτὸ ἅμα ὑπάρχειν τε καὶ μὴ ὑπάρχειν ἀδύνατον τῷ αὐτῷ καὶ κατὰ τὸ αὐτό (καὶ ὅσα ἄλλα προσδιορισαίμεθ᾽ ἄν, ἔστω προσδιωρισμένα πρὸς τὰς λογικὰς δυσχερείας) [...].
>
> It is impossible that the same thing can at the same time both belong and not belong to the same object and *in the same respect (and all other specifications that might be made, let them be added to meet local objections)* [...].

Finally, I want to also outline here a semantic analysis for the English universal quantifier *each*, in order to give some clarifications about an alleged contrast between it and *every* in terms of distributivity.

The etymology for the Italian counterpart of *each*, {*ciascun*(*o*) / *ciascuna*}, seems to be particularly telling; the following lemmas are from Devoto (1968):[15]

ciascuno, lat. volg. **cisque* (class. *quisque*) *unus*; cfr. CIASCHEDUNO,

[15]Translations are mine.

> incr. con *catuno* [. . .].
>
> **ciascuno,** Vulgar Lat. **cisque* (Classical Lat. *quisque*) *unus*; cf. CIASCHEDUNO, crossed with *catuno* [. . .].

> **ciascheduno,** lat. volg. **cisque* (class. *quisque*) *et unus*, it. **cischeduno*, incr. con *cata unum*, it. arc. *catuno* e *cadauno* [. . .].
>
> **ciascheduno,** Vulgar Lat. **cisque* (Classical Lat. *quisque*) *et unus*, It. **cischeduno*, crossed with *cata unum*, Old It. *catuno* and *cadauno* [. . .].

> **catuno** 'cadauno' (arc.), lat. volg. **cata*-; [. . .] cfr. CADAUNO.
>
> **catuno** 'cadauno' (archaic), Vulgar Lat. **cata*-; [. . .] cf. CADAUNO.

> **cadauno,** dallo sp. *cadauno* e questo dal lat. *cata*- distributivo (preso dal gr. *katá*) e *unus* [. . .].
>
> **cadauno,** from Spanish *cadauno* and this from Lat. *cata*- distributive (taken from Greek *katá*) and *unus* [. . .].

The diachronic data (see in particular the lemma for *cadauno*) suggest a way to give substance to the commonplace, quite ubiquitous in the literature (see Beghelli & Stowell (1997: §5.1), for instance) according to which it is *each*, not *every*, which is the true English distributive universal quantifier. Such a claim needs, I believe, to be better specified, since there is a technical meaning of the adjective "distributive", that opposes it to the adjective "collective" and that is presupposed by default here, according to which it can hardly be denied that *every* is also distributive. Sentence (134) below is not compatible with a scenario where the students painted the room together, each one painting only a limited portion of it.

(134) Every student completely painted the room.

But the etymology of *each* and, moreover, that of some of its counterparts in other languages (like It. *ciascuno* or French *chaque*) seem compatible with the idea that *each* is a universal quantifier that needs to have an existential quantifier in its immediate scope and that receives the following semantic representation:

(135)
$$\left[\!\!\left[\ [_{\mathrm{IP}_{1i}}\ [_{\mathrm{DP}_{1i,x}}\ [_{\mathrm{D}}\ \text{each}]\ \mathrm{NP}_{1i,x}]\ [_{\mathrm{IP}_{2x}}\ [_{\mathrm{DP}_{2y}}\ [_{\mathrm{D}}\ \exists]\ \mathrm{NP}_{2y}]\ \mathrm{IP}_{3x,y}]]\ \right]\!\!\right] =$$

$$= \quad (\forall x \leq t)(x\Pi i \rightarrow (\exists y \leq t)([\![\mathrm{NP}_{2y}]\!] \wedge [\![\mathrm{IP}_{3x,y}]\!] \wedge (\forall z \leq t)([\![\mathrm{IP}_{3z,x}]\!] \rightarrow$$
$$\rightarrow z = y))).$$

Note that the existential quantifier required to satisfy such a semantic representation does not need to be provided by a DP; it may well be triggered in the logical form by, for instance, the inflection, i.e. it may be an existential quantifier over time intervals. This can be seen, I believe, by comparing the following sentences:

(136) a. Every boy ran down the hill.

b. Each boy ran down the hill.

As far as I can see, (136b), while not (136a), entails that no two relevant runnings took place simultaneously.

7.2 Generic indefinites

7.2.1 First *caveat*: epistemic indefinites

It is sometimes claimed in the literature (and Hornstein (1984) is one example) that certain prenominal adjectival modifiers, like English *certain*, can force wide scope readings of indefinites.

However, already Fodor & Sag's (1982: 362 f.) (see also Hintikka (1986)) saw that, even if it is undoubtable that such modifiers can favour wide scope, nevertheless they are compatible also with different readings. Unfortunately, in my opinion, they failed to provide a clear example supporting their claim, because they discussed only one single sentence where the indefinite modified by *certain* is embedded within a CP (see the considerations at p. 104 above). This is the relevant passage:

> The modifiers *certain* and *particular* [...] favor a referential understanding of an indefinite. [...] The stronger claim is sometimes made that *certain* and *particular* force maximally wide scope interpretations of a quantifier with respect to higher predicates, negation, and so forth. [...] However, it seems clear to us that sentence [(137)] can have an interpretation which implies that Sandy, but not Tom, has a boy in mind.
>
> (137) Tom said that Sandy believes that a certain boy has been cheating.
>
> [...] The semantics of modifiers like *certain* is completely obscure, and

> our impression is that these modifiers correlate with scope in only a very rough way. [...] It looks to us as if *certain* is used in discourse as a loose cue to the fact that an identification of a relevant individual could be given by *someone*, either by the speaker or by one of the people whose propositional attitudes are being reported.

Alonso-Ovalle & Menéndez-Benito (2003, 2011) refer to determiners modified by adjectives of this kind as "*epistemic indefinites*" (Alonso-Ovalle & Menéndez-Benito (2010) speak instead of "*modal indefinites*"). The literature on these modifiers has become quite huge in recent years: one can see, for instance, Zamparelli (2000, 2007) and Chierchia (2006) on It. *un* {*qualsiasi* / *qualunque*} 'a whatever' and Zamparelli (2007) on It. (*un*) *qualche* '(a) sm'; Krifka (1991) and Kratzer & Shimoyama (2002) on German *irgendein* 'a whatever'; Jayez & Tovena (2002, 2006) on French *un quelconque* 'a whatever', *quelque* 'sm' and *un certain* 'a certain'; already cited Alonso-Ovalle & Menéndez-Benito (2008, 2010) on Spanish *algún* 'sm' and Gutiérrez-Rexach (2001) and Alonso-Ovalle & Menéndez-Benito (2011, 2013) on Spanish *algunos* 'some'. The adjectives of the *whatever*-type are often analyzed in terms of some notion of "non-individuation" or "indifference", while those of the *certain*-type in terms of "individuation" or "referentiality". Adjectives of the first type present some interesting and well-known correlations with so-called Free Choice Items (FCIs) as Eng. *any* (in positive environments).[16]

[16]For some deep analyses of FCIs, see Kadmon & Landman (1993), Eisner (1994), Dayal (1995, 1997, 1998), Tovena & Jayez (1997, 1999) and Giannakidou (1997b,a, 1998). This issue interrelates in a non-trivial manner with the problem of the ambiguous or univocal status of *any*, between its free choice and its negative polarity readings; the ambiguity theory follows quite straightforwardly from the arguments provided in the literature supporting the claim that NPI *any* is an existential quantifier: see Ladusaw (1979a), Carlson (1980a), Linebarger (1980, 1987) and Jackson (1995).

Besides, there seem to be expected correlations, in Romance languages, between the differences

All this recent literature agrees on not recognizing any special connection between these epistemic modifiers and scope, if not as a side-effect of pragmatic nature arising in certain circumstances. Hence, in trying to show the existence, in natural language, of wide scope DPs, I will not look at such modifiers, unlike Hornstein (1984).

7.2.2 Second *caveat*: bare nouns

Genericity is a pervasive phenomenon of natural language and, *prima facie*, it can be described as a way to say something about any individual whatever belonging to a certain kind, instead of about a particular individual of that kind. Put it this way, we could also say that genericity is a way to refer to individuals of a special, abstract sort, namely "kinds". This was the notion of genericity proposed by Carlson (1973, 1977a,b, 1980b) (see also Carlson (1979)), for instance. However, expressions that may be argued to be generic in this sense semantically differ very much from one another. In particular, we shall distinguish here, slightly modifying a terminology introduced by Longobardi (2001), between *generic indefinites* (hereafter, GIs) and *kind-referring definites*.[17] As for kind-referring definites, I consider them to belong to a subclass of anaphoric expressions, as any other definite noun

in meaning of the two types exemplified by It. *un {qualunque / qualsiasi} NP* and *un NP {qualunque / qualsiasi}* and the differences in meaning between prenominal and postnominal adjectives. On this last issue, the seminal work is Bolinger (1967a); see also Cinque (1990) and, for some more recent references, Knittel (2005) on French, Demonte (2008) on Spanish and Alexiadou et al. (2007).

[17] Longobardi (2001) actually speaks, in a more symmetric way, of "generic indefinites" and "generic definites". Given that, according to the analysis I want to defend, the latter are distinguishable from other definites not because of their semantic representation but rather because of the ontological properties attributed to their antecedent, while the former are distinguishable from other indefinites on a purely semantic ground, and given that "generic" is probably nevertheless the most widespread label in treating the kind of phenomena I am interested in, I will use this label for indefinites only, while making overtly clear what I assume to be the peculiarity of the subclass of definites which I am comparing them with.

phrase.

The difference between these two kinds of DPs is quite sharp in languages like Italian, where they are always differently marked on the surface (needless to say, the following English translations are highly inaccurate in several respects; however, they seem to me to be a good compromise to convey in plain language the relevant aspects of the meaning of the corresponding original sentences):

(138) a. *Le* *patate* *verdi* sono state trovate per la prima volta
`the potatoes green are been found for the first time`
in America del Sud.
`in America of South`
✓ 'the first group of green potatoes to be found was found in South America'

b. *Patate* *verdi* sono state
`the:F;PL potatoes green:PL be;IND;3PL`
trovate per la prima
`stay:PTCP;PST:F;PL find:PTCP;PST:F;PL for the:F;SG`
volta in America del Sud.
`first:F;SG time:SG in America of:the;M;SG South`
✓ ∃: 'a certain group of green potatoes was first found in South America'

✓ Gen: 'a group whatever of green potatoes was first found in South America'

✗ Kind: 'the first group of green potatoes to be found was found in South America'

(139) a. *Il* *vino* *in* *cartone* è stato
`the;M;SG wine in carton is stay:PTCP;PST:M;SG`
immesso nel mercato
`introduce:PTCP;PST:M;SG in:the;M;SG market`
negli anni Ottanta.
`in:the:M;PL years Eighties`
✓ 'the first quantity of wine in cartons to have been introduced to the market was introduced in the Eighties'

b. *Vino* *in* *cartone* è stato
`wine in carton is stay:PTCP;PST:M;SG`
immesso nel mercato
`introduce:PTCP;PST:M;SG in:the;M;SG market`
negli anni Ottanta.
`in:the:M;PL years Fifty`
✓ ∃: 'a certain quantity of wine in cartons was introduced to the market in the Eighties'
✓ Gen: 'a quantity whatever of wine in cartons was introduced to the market in the Eighties'
✗ Kind: 'the first quantity of wine in cartons to have been introduced to the market was introduced in the Eighties'

As the contrasts between (138a) and (139a), on one side, and, respectively, (138b) and (139b), on the other side, show, Italian bare nouns cannot have a kind-denoting reading, which, instead, is available with definite DPs like the subjects of (138a) and (139a).

The pattern of Italian is also shared by the other Romance languages (see Longobardi (2001));[18] English bare plurals, however, as it is well-known, are equally fine as existential indefinites, as GIs and as kind-referring definites. The following examples illustrate the English case:

(140) *Green potatoes* were first found in South America.

✓ ∃: 'a certain group of green potatoes was first found in South America'

✓ Gen: 'a group whatever of green potatoes was first found in South America'

✓ Kind: 'the first group of green potatoes to be found was found in South America'

(141) *Wine in cartons* was introduced to the market in the Eighties.

✓ ∃: 'a certain quantity of wine in cartons was introduced to the market in the Eighties'

✓ Gen: 'a quantity whatever of wine in cartons was introduced to the market in the Eighties'

✗ Kind: 'the first quantity of wine in cartons to have been introduced to the market was introduced in the Eighties'

The semantic parallel between bare plural count nouns and bare singular mass nouns in English was noticed a long time ago, at least by Cartwright (1975) and Carlson (1980b: §7.6.0) (see also Benincà (1980) and Gillon (1992), for some syntactic remarks).[19]

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[18]For the peculiar syntactic constraints on Romance bare nouns, see Contreras (1986) and Delfitto & Schroten (1991).

[19]There is, in addition, the symmetric circumstance that both Romance and Germanic languages lack bare plural mass nouns (they lack in general plural mass nouns) and also bare singular

Carlson's (1977a) is a non-quantificational approach to bare plurals, where they are treated as proper names, in particular as proper names of kinds, and their ambiguity between a generic and an existential reading is derived from the presence or absence of an operator possibly encoded in the lexical meaning of the predicate (see 7.2.4 below; some analogies between bare plurals and proper names had already been noticed by Postal (1969)).

The fact that English bare nouns cover semantic functions which in Romance languages are played by two distinct constructions is one of several arguments which led many scholars to reject Carlson's unified account of English bare plurals and to argue in favour of the hypothesis of a genuine ambiguity of English bare nouns. Among the advocates of this position, see Krifka (1987), Gerstner-Link & Krifka (1993), Schubert & Pelletier (1987), Wilkinson (1988, 1989, 1991, 1995)and Longobardi (1991, 1994, 2001).[20]

One could ask why languages like Italian, where an indefinite non-presuppositional overt determiner is available (the so-called *partitive article*) also have bare plurals. This question may find an answer through another central question. Let's assume that bare plural count nouns have a hidden determiner, say φ, as it is sometimes labeled in the literature. The question, then, is the following: can φ be viewed as the plural counterpart of the indefinite article in English, in the cases where the bare noun cannot be analyzed as a kind-denoting definite? The viability

count ones (this very well-known feature is stressed, for instance, in Chierchia (1998: 341)). Of course, bare singular count nouns may be easily found, with both definite and indefinite interpretations, in articleless languages like Russian and the other Slavic languages (see Chierchia (1997)).

[20]For differences in the syntactic encoding of the two sorts of genericity in French see Carlier (1989). The distinction has superficial manifestations also in some African languages: see Welmers (1974) for the relevant data and Greenberg (2003) and Greenberg (2002: 121) (n. 5) for an analysis along these lines.

of this option was, for instance, the motivation underlying a specific transformation rule in Chomsky (1965). Others, like Sweet (1898), instead, identified the indefinite plural of *a* with the unstressed variant of *some*, (sometimes written '*sm*'; see Carlson (1977a) (Portner & Partee (2002: 37)) and n. 8 above).

The problem is widely discussed in Carlson (1977a, 1980b), who came to a negative answer. First of all, Carlson (1977a) (Portner & Partee (2002: 38 f.)) observes that bare plurals, unlike DPs introduced by the indefinite article *a*, cannot give rise to transparent readings; the examples he considers are the following ones (Carlson's (1977a) (8) and (10), respectively):

(142) a. Minnie wishes to talk with a young psychiatrist.

b. Minnie wishes to talk with young psychiatrists.

(143) a. Minnie wishes [(a young psychiatrist: x) [Minnie talks with x]]

b. (a young psychiatrist: x) [Minnie wishes [Minnie talks with x]]

While (142a) can receive both the interpretations represented in (143), the reading (143b) is unavailable for (142b). In other words, (142b) rules out a *de re* reading compatible with the possibility that Minnie wishes to talk with one or more psychiatrists even ignoring the fact that they are psychiatrists (the interpretation corresponding to (143b)).

Further, Carlson noticed that English bare nouns, unlike DPs introduced by the indefinite article *a*, cannot take wide scope over universal quantifiers (Portner & Partee (2002: 40 f.); the following examples are Carlson's (1977a) (22) and (23)):

(144) a. Everyone read a book on caterpillars.

b. Everyone read books on caterpillars.

(144a) can receive, in fact, both the interpretations represented in (145), while only (145a) is available for (144b), according to him.[21]

(145) a. (everyone: x) [(a book on caterpillars: y) [x read y]]

b. (a book on caterpillars: y) [(everyone: x) [x read y]]

(144b) cannot receive an interpretation necessarily requiring that there is one and the same book on caterpillars read by everybody. It is easy to see that this state of affairs also holds for Italian:

(146) a. Ogni musicista di questo conservatorio ha suonato uno
`every musician of this conservatoire has played an`
strumento conservato in questa stanza.
`instrument stored in this room`
'every musician of this conservatoire has played an instrument stored in this room'

b. Ogni musicista di questo conservatorio ha suonato
`every musician of this conservatoire has played`
strumenti conservati in questa stanza.
`instruments stored in this room`
'every musician of this conservatoire has played instruments stored in this room'

[21] I ignore here the fact that Carlson assumes plural morphology to entail the cardinality of the relevant set to be at least 2; see §6.5 above for classical counterarguments to this claim already made available in the literature.

(147) a. (every musician: x) [(an instrument stored in this room: y) [x has played y]]

b. (an instrument stored in this room: y) [(every musician: x) [x has played y]]

Again, while both interpretations in (147) are accessible to (146a), only (147a) is available for (146b).

Note that the plural variants of (144a) and (146a) obtained by using the determiners *some* and *degli*, respectively, share with the correspondent sentences containing the singular indefinite article the same interpretative ambiguity between a narrow scope reading of the indefinite and a wide scope one:

(148) Everyone read some books on caterpillars.

(149) Ogni musicista di questo conservatorio ha suonato degli
`every musician of this conservatoire has played of:the`
strumenti conservati in questa stanza.
`instruments stored in this room`
'every musician of this conservatoire has played some instruments stored in this room'

Another argument against the analysis of bare plurals as the plural form of the singular indefinite article is the one I illustrated in §6.1.2, based on the contrast between telicity induced by overt determiners and atelicity induced by the null one.

Finally, look at the following contrast in Italian, which, as far as I know, has remained unnoticed in the literature up to now:

(150) a. Abbiamo fatto una scoperta sensazionale: una foresta
`have;1PL made a discovery sensational a forest`
tropicale produce ossigeno!
`tropical produces oxygen`
'we made a sensational discovery: a tropical forest produces oxygen!'

b. #Abbiamo fatto una scoperta sensazionale: foreste
`have;1PL made a discovery sensational forests`
tropicali producono ossigeno!
`tropical produce oxygen`
'we made a sensational discovery: tropical forests produce oxygen!'

As a native speaker, if I was asked to judge the acceptability of sentences (150a) and (150b) taken out of the blue, I would be strongly inclined to consider (150a) fine but (150b) quite odd. Note that such a judgement would not depend on the role of singular morphology, since the previous contrast replicates with the following sentences, where all relevant DPs are plural:[22]

(151) a. Abbiamo fatto una scoperta sensazionale: dei gemelli
`have;1PL made a discovery sensational PART:the twins`
omozigoti talvolta si odiano!
`homozygous sometimes RECP hate`

[22]Apparently, the generic use of plural indefinites with numeral adjectives is much more constrained than that of indefinites with the singular article. However, this can quite easily be explained in purely pragmatic terms, such as those provided by Corblin (1987: 57 f.) (see also Dobrovie-Sorin (2004: 58) for a brief English description of Corblin's analysis and for some criticism).

'we made a sensational discovery: homozygous twins sometimes hate each other!'

b. #Abbiamo fatto una scoperta sensazionale: gemelli
`have;1PL made a discovery sensational twins`

omozigoti talvolta si odiano!
`homozygous sometimes RECP hate`

'we made a sensational discovery: homozygous twins sometimes hate each other!'

As far as I can see, the problem seems to be that, when they are capable of a generic reading, Italian bare plurals, unlike indefinite correspondents with numeral modifiers, seem to be topics. I do not want to go into this pretty informal intuition further, here; however, it is worth noting that all the examples of generically interpreted Italian bare noun phrases quoted in Longobardi (1994) (a paper which is quite open, even if not as much as Longobardi (2001), to the possibility that Italian bare nouns can receive a generic interpretation) contain a demonstrative in the bare noun phrase, which necessarily refers to some contextually salient entity. See below:[23]

(152) a. Foreste di tali dimensioni sono ormai difficili da
`forests of such dimensions are by_now difficult to`

[23]Note, further, that the structure with the frequency adverb preceding the modal verb is marked in Italian, and that this marked structure makes the presence of the generic bare noun far more acceptable, thus indirectly confirming that a complex information structure plays a role in licensing generic bare nouns in Italian. For the unmarked structure of Italian sentences, see Benincà et al. (1988).

trovare.

`find`

'forests of that size are now hard to find'

b. Castori di questo tipo non costruiscono mai dighe.

`beavers of this sort not build ever dams`

'beavers of this sort never build dams'

c. Acqua di quel colore raramente può essere bevuta.

`water of that color rarely can be drunk`

'water of that color can rarely be drunk'

Note that it is hard to see how one could directly check in English as well the presence of such a constraint on generically interpreted bare nouns, given that English bare nouns, unlike Romance ones, can be kind-referring definites and kind-referring definites allow for perfectly acceptable sentences in Italian too, whether uttered out of the blue or not, as the following examples derived from (150b) and (151b), respectively, show:

(153) a. Abbiamo fatto una scoperta sensazionale: le foreste

`have made a discovery sensational the forest`

tropicali producono ossigeno!

`tropical produce oxygen`

'we made a sensational discovery: tropical forests produce oxygen!'

b. Abbiamo fatto una scoperta sensazionale: i gemelli

`have made a discovery sensational the twins`

omozigoti talvolta si odiano!

`homozygous sometimes` RECP `hate`

‘we made a sensational discovery: homozygous twins sometimes hate each other!’

Let’s conclude this brief overview on bare nouns by giving a look at, following Longobardi (2001: 339), to the four major semantic proposals formulated to account for the behaviour Romance bare nouns:

a) Casalegno (1987) (followed by Zamparelli (2002a)): Romance bare nouns, unlike in English, are only existential;

b) Longobardi (1991, 1994): Romance bare nouns can be existential, but sometimes also GIs, even if in this case their distribution is more restricted than in English; they can never be kind-referring definites;

c) Chierchia (1998): Romance and English bare nouns have essentially the same distribution;

d) Longobardi (2001) (see also Delfitto (2002) and Dobrovie-Sorin (2004)): Romance bare nouns can be existential or GIs, with the same distribution of GIs with an overt Num head and without any difference, as for both uses, between Romance and English; they can never be kind-referring, however.

The few phenomena I illustrated above point towards an explanation akin to Longobardi’s (1994), even if, unlike him, I would take the difference illustrated by examples (150a)–(151b) as the core feature semantically distinguishing bare GIs (both in Italian and English) from overt generic ones and constraining the distribution of the formers with respect to the latters.

This picture about bare nouns, even as incomplete as it is, still suffices, I believe, to convince one that bare nouns, even when they appear to share some properties with GIs with an overt Num head, at best can only be seen as an atypical case of GIs. Thus, we can set them aside in the next part of this dissertation, without worrying too much that the analysis of GIs we are going to outline below may turn out too incomplete.

7.2.3 Third *caveat*: leaving islands aside and movement in peace

In the following sections, I am going to examine some data illustrating the different scope behaviour of GIs with respect to quantified DPs relative to some other operators: that will be the core data hopefully providing support for my main claim. At least some of this data have already been discussed in the literature. However, these differences in scope behaviour has often been argued for on the basis of a different kind of evidence, ultimately reducible to two partly related phenomena:

a) scope inversion of operators within the same clause;

b) wide scope reading of operators across (scope) islands.

Actually, what I am going to suggest is that none of these phenomena can provide clearcut evidence that some expressions of natural language should translate in logical form as free (generic) variables. Arguments based on these kinds of phenomena are the chief ones in Fodor & Sag's (1982) classic paper about specific indefinites and the only ones for some of the issues addressed in Hornstein (1984), which is one source of inspiration for this dissertation.

The arguments based on scope reversing are essentially intended to show that scope reversing is at least easier when the element embedded under the operator on which it should be interpreted as having wide scope is a free DP (see p. 165 below) rather than a quantified one. Since the possibility of quantified DPs (paradigmatically, universals) taking wide scope through scope reversing has been known for a long time (see, for instance, Montague (1970a, 1973) for an early treatment of the problem), the sloppy and barely definitive character of such arguments is usually acknowledged. There are, however, several experimental studies proving that scope reversing has an impact in terms of increased processing costs with respect to linear scope (see, among others, Ioup (1975), Fodor (1982), Crain & Steedman (1985), Altmann & Steedman (1988), Kurtzman & McDonald (1993), Tunstall (1998), Anderson (2004), Filik et al. (2004), O'Grady (2006), Pylkkänen & McElree (2006), Reinhart (2006), AnderBois et al. (2012) and Dotlačil & Brasoveanu (2012)).

Usually, evidence from scope islands violation is taken to be much more robust; however, almost all proposed scope islands have been challenged in the literature by providing convincing counterexamples, and the intervention of semantic and pragmatic factors has been shown to be often decisive.

Further, under many accounts of the ambiguity between opaque and transparent readings (*de dicto vs. de re*) of DPs embedded under intensional predicates, the ambiguity is attributed to the relative scope of the DP and the intensional predicate, which can come in two distinct possible combinations. This analysis was first advocated in Russell (1905) (see Zamparelli (2000: §6.1.1)) and illustrated here with the following example:

(154) Gianni thinks that every British detective is after him.

a. Gianni thinks that [(every x: x is a British detective) [x is after

Gianni]]

b. (every x: x is a British detective) [Gianni thinks that [x is after Gianni]]

One way to account for data of this kind would be of course that of relaxing the close parallel which has been assumed to hold between extraction islands and scope islands (at least since Lakoff (1970); for some earlier relevant literature see Fodor & Sag (1982: 369)).[24] However, it is a remarkable fact that, in a parallel way, also extraction islands have come under intensive attack in the last few decades. More or less as in the case of scope islands, the main strategy to cast doubts on the existence of (some) extraction islands has been that of ascribing them not to syntax but rather to processing limitations; this idea is, at least *prima facie*, promising in order to explain the widely attested grey zones in grammaticality judgements, the interference of molecular factors in increasing or decreasing the degree of acceptability and the important oscillations of judgements between different speakers of the same language.

As is well-known, the starting point for the syntactic studies of islands, where the term itself made its first appearance in the linguistic literature, was Ross (1967a, 1986), basing himself on the previous Chomsky (1964), while Chomsky himself, in turn, made further notable developments in Chomsky (1973) (see Boeckx (2012) for a recent overview on the topic of syntactic islands). But in recent years, a new approach to syntactic islands has gained respectability, one which looks at them (or at least at part of them) not as constraints on the syntac-

[24]Note, however, that this parallel is predicted in a principled way once we assume that quantifiers take scope through movement at LF. Historically, in fact, such a parallel had been the primary motivation underlying the QR account.

tic structure, but rather as a by-product of syntactic constructions which are too difficult to process (with different degrees of difficulty depending on several factors, corresponding to the different degrees of acceptability of the island violation; see Boeckx (2012: §2.3) for a critical opinion). The most prominent recent study developing an analysis like that is probably Hofmeister & Sag (2010),[25] based on, among others, Deane (1991), Pritchett (1991), Kluender (1991, 1992, 1998, 2004) and Kluender & Kutas (1993); but see already Givon (1979) for an early statement of the core idea.[26]

Even if I feel sympathetic with both processing-oriented reductionist attempts, the semantic one of deriving scope island effects through processing considerations and the syntactic one of deriving extraction island effects in these terms too,[27] the only point here is that of stressing the fact that possibly any argument based on a greater or smaller predisposition of some classes of DPs to violate scope islands may fall under attack on the basis of some counterexample. For this reason, I will try to develop some arguments relying on different bases.

7.2.4 Standard accounts of generic indefinites

There are two main families of approaches to genericity in natural language on the market and both have also been put to work for the particular case of the analysis of generic indefinites. Here, I will briefly comment on some fundamental ideas underlying each one.

[25] As I said before (p. 136), Hofmeister and Sag concede that at least the Coordination Structure Constraint (CSC) is an irreducibly syntactic island effect.

[26] As Boeckx (2012: 33) remarks, one main source of inspiration for all these reductionist accounts has been Chomsky & Miller (1963); see also Chomsky (1961).

[27] And, moreover, I think that the we should maintain that the two kinds of phenomena proceed on a par.

The first approach is rooted in Carlson (1977a, 1980b) and was primarily designed to address the issue of the semantics of bare plural noun phrases. As I said before, Carlson analyzed bare plural NPs as definite descriptions of kinds. The most interesting feature of this approach, in the light of the empirical data I am going to discuss in the following sections, is that it treats BPs as non-quantificational, a feature that will prove crucial, in my analysis, to account for that data.

However, there are at least two problems for also extending such an approach to GIs. First of all, if GIs were anaphoric elements, I cannot see how one could find, in whatever context, any proper overt antecedent for them without falling into a *regressus ad infinitum*. Hence, it seems that an analysis of GIs in terms of kind-referring definite descriptions requires abandoning the idea that definites are always anaphoric elements.

The second problem of a carlsonian approach extended to GIs would be that of how to obtain particularization in a non-stipulative way. GIs, in fact, always allow us to derive, as a logical consequence of the sentence they are embedded in, a corresponding sentence obtained from that sentence by replacing the GIs themselves with a referential item. This is not always the case, however, with BPs. The following examples illustrate this contrast (of course, the indefinite article is to be interpreted generically in (155a)):

(155) a. A dodo is funny.

b. $\Rightarrow$ That dodo is funny.

(156) a. Dodos are extinct.

b. *That dodo is extinct.

Thus, we would have the remarkable problem of distinguishing between the two types of kind-reference, something which is enough to make the original project appear no longer feasible. Sentences containing GIs (or habituals) are usually referred to as *characterizing sentences*, to distinguish them from sentences whose generic character entirely relies on the presence of kind-denoting elements.

Approaches belonging to the second family attempt to model genericity by assimilating it to some (modal) quantificational structure. This is, for instance, the analysis offered by Heim (1982: §II-4.3). Heim took inspiration from works such as Katz (1972), where it is said that GIs are akin to *if*-clauses, structures which are usually analyzed in modal terms in formal semantics. In the same vein, she also cites Nunberg & Pan (1975: 415), who say that a generic statement of the form *An F is G* means that *G* is a property that holds of *Fs* "in virtue of [their] classmembership". The main technical tool employed by Heim to articulate her proposal, however, is the notion of an unselective quantifier famously put to work in Lewis (1975) to account for the semantic behaviour of what he called "adverbs of quantification" and, following him, by Kratzer (1979) for her account of conditionals (see §7.3 below).

Lewis (1975) explicitly considered the following "adverbs" (see Portner & Partee (2002: 178)):[28]

a) *always*, *invariably*, *universally*, *without exception*;

b) *sometimes*, *occasionally*;

c) *never*;

[28] Actually, some of them are not morphologically adverbs, but this has no relevance for the present discussion.

d) *usually*, *mostly*, *generally*, *almost always*, *with few exceptions*;

e) *often*, *frequently*, *commonly*;

f) *seldom*, *infrequently*, *rarely*, *almost never*.

He noticed that, despite them being temporal adverbs, in certain sentences these modifiers behave as if they were quantifiers binding variables provided by indefinite DPs within the same clause. The following examples are slightly modified versions of Heim's (1982: §II-4.3) ones (precisely, of Heim's (1982: 126) original examples (16) and (17), respectively):

(157) If a cat has been exposed to 2,4-D, it {(a) never / (b) seldom / (c) sometimes / (d) often / (e) usually / (f) always} goes blind.

(158) A cat that has been exposed to 2,4-D {(a) never / (b) seldom / (c) sometimes / (d) often / (e) usually / (f) always} goes blind.

Heim assumes, extending the analysis of cases like (157) already offered in Lewis (1975) to the case of GIs, that both (157) and (158) can be paraphrased in the following way:

(159) {(a) No / (b) Few / (c) Some / (d) Many / (e) Most / (f) All} cats that have been exposed to 2,4-D go blind.

In other words, in Heim's account (which can be seen more or less as the starting point of all other quantificational accounts of GIs), a GI works like the restrictor of a possibly covert universal-alike quantifier which I will refer to as the generic operator (usually represented as "Gen" or "GEN", in the literature), whose meaning is closely related, if not identical, to that of Eng. *always* in its use as an adverb of quantification just illustrated above.

Adverbs of quantification (as well as the generic operator in Heim's original proposal) are unselective quantifiers, insofar as they do not necessarily bind one single variable, as ordinary quantifiers of first order logic, but they bind all free variables in their domain:

(160) a. A cat which finds a mouse {(a) never / (b) seldom / (c) sometimes / (d) often / (e) usually / (f) always} runs after it.

b. ({(a) No / (b) Few / (c) Some / (d) Many / (e) Most / (f) All} $\langle x, y\rangle$: x is a cat and y is a mouse) [x runs after y]

Note that, in Lewis's (1975) original theory, there is no need to stipulate a (suspect) ambiguity, inherent to adverbs belonging to the category under examination, between their reading as temporal adverbs and the one as restrictors of participants in a certain event: the temporal reading, in fact, may well arise as the only available reading when there are no other free variables except from the temporal one in the clause.

The unselective binding hypothesis, however, has to face several empirical and conceptual problems, which led most linguists to embrace the view that adverbs of quantification are better uniformly analyzed as standard selective quantifiers ranging over events or situations (see, among others, Rooth (1985, 1995), Schubert & Pelletier (1987) and Krifka et al. (1995)). In §7.2.7, however, I will discuss some problems which affect any quantificational account of GIs.[29]

[29] Another one, which I cannot discuss here, is presented in Hintikka & Carlson (1979), where GIs are treated as wide scope universal quantifiers.

7.2.5 Goodstein (1951)

There was, however, another approach to GIs that needs to be discussed. This approach dates back at least to the work of Goodstein himself. Unfortunately, Goodstein, as a mathematician, was not primarily interested in problems about natural language and his deep linguistic insights were not properly developed to show the full range of empirical consequences they could lead to. On the linguistic side, instead, a strikingly similar analysis was proposed by Hornstein (1984) for GIs. Hornstein, on the contrary, seemed to be unaware of the possibility of connecting the analysis he was advocating to the formalism of recursive arithmetic; moreover, the linguistic community apparently paid less attention than deserved to his analysis.

The chief aim of this dissertation can be viewed as the attempt to establish a connection between these two analyses, Goodstein's and Hornstein's, and to show that they may find an application for a solution to some apparently unrelated well-known linguistic puzzles.

The examples of genericity in natural language discussed by Goodstein are centred on English bare nouns. Because of what I said above in §7.2.2, this has not really been a felicitous choice. However, the relevance of his considerations for the semantic analysis of natural language emerges all the same through the following passage from Goodstein (1951: 21 f.):

> The numeral variable is an instance of a general noun, and as such its use is akin to that of a blank space which may be filled by certain signs. The college rules forbidding *students* to leave their place of residence after a certain hour is expressed by means of the variable sign

“student”. If the rule reads “students in residence from January 1944 to January 1945”, the variable term “student” could be replaced in the formulation of the rule, by a list of names, the names of the students in residence 1944–45, and it is this fact which has tempted mathematical philosophers to believe that a variable is a *class* of objects, but if the rule is intended to apply not only to present, but also to future students, the replacement of the variable sign by a list of names is no longer possible and we are obliged to find an interpretation of variable signs which does not identify a variable with a class of names given in extension. The truth value of a sentence containing a variable sign is unchanged if the variable sign is replaced by some certain other sign, for instance, in the sentence “metal is found in the earth”, the variable “metal” may be replaced by names of particular metals, such as “iron” or “gold”, so that the term “metal” may be regarded as a space to be filled in by the name of a metal. What is unsatisfactory about this analysis of the variable is that we require a knowledge of all the particulars which come under the variable sign before the variable itself is defined; that is to say we are no better able to define a variable sign as a blank space to be filled by certain signs than to identify the variable with a class of objects, for just as we cannot enumerate an infinite class we cannot say just *which* are the signs that may fill a certain variable-sign space.

The definition of a variable is not given in terms of the objects whose signs may take the place of the variable sign but by the rules for the use of the variable sign itself.

7.2.6 Hornstein (1984)

Hornstein (1984) is a monograph which deals with the general issue of elaborating a theory of meaning in natural languages which is shaped on the syntactic structure of languages as predicted by generative grammar on the basis of a sound analysis of the logical problem of language acquisition, in contrast with approaches to semantics making essential use of notions like "truth" or "reference". This project is by itself, I believe, of considerable interest; however, there is a more specific reason why Hornstein (1984) deserves special attention here. An important part of that book, in fact, investigates a distinction first introduced by Hornstein in 1981 between two types of "quantifiers" that matches quite well the one between free (generic) variables and quantifiers in $\mathcal{RA}^t$.

Essentially, I could say that the crucial linguistic notions investigated in this dissertation were already correctly identified in Hornstein (1984). What was still missing was: (a) the description of a formal system making the different inferential behaviour of the linguistic elements involved (and, thus, providing a further confirmation for their relevant properties through a functional justification for them) predictable in a precise way; (b) a discussion of their interaction with clause boundaries; (c) the consideration of adequate empirical data neatly supporting the distinction under examination.

In Hornstein (1984), especially in Ch. 2, it is argued that natural languages have essentially three different types of quantified NPs, which Hornstein simply dubs *type I*, *type II* and *type III quantifiers*. As for type III quantifiers, mostly discussed in Hornstein (1984: 67–8), I think that Hornstein's analysis appears to be particularly controversial; hence, I will focus only on the opposition between type

I and type II quantifiers, which Hornstein himself seems to consider more basic and to which he devoted the entire Ch. 2 (entitled *Two Types of Quantifiers*). The two types are defined, in Hornstein (1984: 17), respectively in the following ways:

> I. a set of NP expressions whose interpretive scope domain is always wide;
>
> II. a set whose interpretive scope domain is restricted to the clause in which the quantified NP is situated [. . .].

Since linguistics derived the word *quantifier* from logic, where of course it is still part of the basic vocabulary of the discipline, and since my point (as well as Hornstein's) is that of trying to show that certain DPs, among which those that Hornstein dubbed "type I quantifiers", should not be conceived as translating in logical form as logical quantifiers, I think that Hornstein's choice of terms here turns out to be quite infelicitous. Hence, hereafter I will speak, when not explicitly stated otherwise, of *free determiners*, meaning Hornstein's "type I quantifiers", and of *quantifiers*, meaning his "type II quantifiers"; in a parallel fashion, I will speak of a *free DP*, meaning a DP whose head is a free determiner, and of a *quantified DP*, meaning a DP whose head is a quantifier.

What is of considerable importance, here, is especially the content of Hornstein (1984: Ch.5), where the author argues that GIs are to be viewed among free determiners. To make this point, Hornstein employs the same diagnostics he put to work in Ch. 2, mainly based on examples of scope inversion or coindexing across clause boundaries; as I have said, however, arguments built upon this sort of empirical data cannot easily be employed to account for the differences between the determiners under examination. In the case of GIs, however, Hornstein added one notable new kind of example supporting, according to him, the hypothesis that GIs are free determiners: they are instances of donkey sentences. These sentences should be analyzed, according to Hornstein's account, as essentially involving a GI which is syntactically embedded in the scope of another operator; in the prototypical case reported here as (161), Hornstein's analysis could be represented as in (161a):

(161) Every farmer who owns a donkey beats it.

a. (a$_{\exists}$ x: x is a donkey) [(every y: y is a farmer who owns x) [y beats x]]

Notice that this is indeed a new kind of argument with respect to those displayed in Hornstein (1984: Ch.2), because, as is well-known, donkey sentences represent a puzzle not only for the apparently unusual scope properties displayed by the embedded indefinite, but further and foremost for its unexpected universal-alike interpretation. Of course, once we analyze it as a GI, this interpretation is instead the expected one.

The analysis of donkey sentences in terms of GIs, however, was already taken into account and criticized by Heim in her dissertation (Heim (1982: 36–37)). In Heim (1982: 36), in fact, we can read what follows:[30]

> it will be important to realize that the indefinites in donkey sentences are not instances of "generic" indefinites [...]. I am not denying, of course, that our understanding of indefinites in the context of donkey

[30] Emphasis is in the original.

> sentences [. . .] in effect amounts to a "generic" understanding [. . .]. What I *am* denying is that the observed truth conditions are a result of disambiguating the relevant indefinites in favor of their generic readings [. . .].

There are, according to Heim, two obstacles against an analysis of donkey sentences involving generic reading of the embedded indefinite. The first is the following one (Heim (1982: 37); examples renumbered):

> [N]ot all indefinites have generic readings. "Someone," for instance, does not (unless it is part of a larger NP, e.g., modified by a relative clause), as the impossibility of a generic reading for (162) shows:
>
> (162) Someone is grey.
>
> However, "someone" can act just like "a donkey" in donkey sentences, as (163) shows.[31]

It seems to me that this argument is quite weak: a DP whose head is *some* has, in accordance with the semantic representation I gave above in (131), existential import, a feature which involves greater semantic complexity (and maybe also syntactic) with respect to the indefinite article, which in turn lacks it. Hence, if existential import does not turn out to be relevant, it seems quite reasonable that pragmatic principles should make one use the indefinite article instead of *some*. Now, this circumstance is probably quite common with generic statements,

[31] This is the example Heim is making reference to (Heim's (1982: 35) (1)):

(163) If $\left\{\begin{array}{c}\text{someone}\\ \text{anyone}\end{array}\right\}$ is in Athens, he is not in Rhodes.

i.e., when speaking generically, we are more inclined, I believe, to say something about a generic individual of some sort than of a generic member of some set of individuals which is salient in the discourse. In other words, I think that a sort of pragmatic story along the lines of that formulated in Corblin (1987) would also work in the case of *someone*, as used in examples like Heim's ones.

The second argument by Heim is, in my view, plainly ill-formed. In Heim (1982: 37), she says (examples renumbered):

> [T]here are certain linguistic environments that, for whatever reason, preclude a generic reading for indefinites, most notably the subject-position of "there"-insertion sentences, and also the object position of the verb '[']have." Neither of the following sentences can be read as a generic statement about donkeys:
>
> (164) John has a donkey.
>
> (165) There is a donkey in the yard.
>
> However, indefinites in these environments do serve as antecedents for donkey anaphora:
>
> (166) If John has a donkey he beats it.
>
> (167) If there is a donkey in the yard John will chase it away[.]

A generic interpretation of the indefinite in (164) and (165) results in oddness, rather than in ungrammaticality, especially in the case of (164) (for (165), we need to share the spirit of Barwise & Cooper's (1981: §4.3) analysis of the badness of universal quantifiers in existential statements; see again Portner & Partee (2002: 96)): John could not plausibly own any donkey in the universe, nor any conceivable donkey could be in the yard now. But similar odd interpretations would not arise if we interpret (166) and (167) as if the indefinite was generic, in accordance with Hornstein's analysis, since we would obtain the following logical representations, which are perfectly consistent with the facts in the actual world:

(168) (a$_\exists$ x: x is a donkey) [[if John has x] John beats x];

(169) (a$_\exists$ x: x is a donkey) [[if there is x in the yard] John will chase x away].

However, even if I am not persuaded by Heim's arguments, I am still persuaded by her conclusion. It seems to me, in fact, that quite a strong argument against a GI account of donkey sentences comes from considering the embedding DP, instead of the embedded one.

There is nothing strange, from a pragmatic point of view, in a sentence like (161), while the following (170) is, instead, pretty odd:

(170) #Three farmers own Ih-Oh.

It is not a common state of affairs, in fact, that donkeys have more than one single owner. However, it is well-known that the universal quantifier triggers a scalar implicature to the extent that it ranges over a domain with possibly more than one single individual; otherwise, a fully cooperative speaker should use the definite article in its place:

(171) a. Every farmer is rough.

b. $\rightsquigarrow$ It is possible that there is more than one farmer.

(172) a. The farmer is rough.

b. $\Rightarrow$ There is one and only farmer.

But then a correspondent implicature should arise from (161), here repeated as (173a), while the following (174a) implies the information in (174b) (where we assume for both (173a) and (174a) that the embedded indefinite is interpreted generically):

(173) a. Every farmer who owns a donkey beats it.

b. ?$\rightsquigarrow$ # It is possible that there is a donkey who is owned by more than one farmer.

(174) a. The farmer who owns a donkey beats it.

b. $\Rightarrow$ For any donkey, there is one and only farmer who owns it.

And if we map the intended scope hierarchy of the elements of (173a) on the surface, we come up with the following rough paraphrase, which in fact has the same implicature in (173b):

(175) a. #For any donkey, every farmer who owns it beats it.

b. $\rightsquigarrow$ # It is possible that there is a donkey who is owned by more than one farmer.

But (175a) is far from being a good paraphrase for the donkey reading of (173a), since the former, but not the latter, turns into pragmatic oddness in virtue of the odd implicature it triggers. If the embedded indefinite in (173a) was indeed a GI, under normal circumstances we would have used (174a) instead of (173a). It seems to me that this strongly suggests that, in donkey sentences, we are not dealing with GIs, as Heim correctly maintains.

There is, however, at least one example which Hornstein quotes to argue for GIs being free determiners and which is worth repeating here, compared with its corresponding sentence containing a universal quantifier in the place of the GI (Hornstein's (1984: 82) (19a)):

(176) Its$_i$ family is important to a raccoon$_i$.

(177) *Its$_i$ family is important to every raccoon$_i$.

In (176), the GI is able to coindex a pronoun to its left, something which the universal quantifier cannot do. As far as I can see, this is the pattern of grammaticality judgements which arises when the antecedent DP is interpreted as the topic of the sentence, while the rest is the focus. Under the assumption that topicalization may only involve free DPs, this would become the expected result; I will not explore this idea here, however.

Now, I move to what I take to be the crucial empirical data showing that GIs are indeed free determiners, as argued by Hornstein (1984).

7.2.7 Generic indefinites as free determiners

The literature, especially the more philosophically oriented, contains plenty of passages where GIs are semantically assimilated to universal quantifiers. And, *prima facie*, there seem to be good reasons for such an assimilation, as the following examples show:

(178) a. Every cat scratches Gianni.

b. A cat scratches Gianni.

(179) a. Gianni beats every cat.

b. Gianni beats a cat.

(180) a. Some dog-sitters beat every cat.

b. Some dog-sitters beat a cat.

(181) a. Every dog-sitter beats every cat.

b. Every dog-sitter beats a cat.

If the indefinite article is interpreted in a generic way, the (a)-sentences above have more or less the same meaning of the corresponding (b)-sentences. However, things change dramatically if we are in a negative environment:

(182) a. Not every cat scratches Gianni.

b. Not a cat scratches Gianni.

(183) a. Gianni does not beat every cat.

b. Gianni does not beat a cat.

In this case, no matter how we try to interpret the indefinite: the (a)-sentences systematically have a weaker interpretation than the corresponding (b)-sentences. (182a), in fact, is consistent with a situation where the following (184) is true, while (182b) is not; and the same pattern holds between (183a), (185) and (183b):

(184) There is some cat who scratches Gianni.

(185) There is some cat who Gianni beats.

The stronger reading conveyed by sentence (183b) with the GI can be roughly paraphrased by the following sentence displaying a universal quantifier in its place:

(186) For every cat, Gianni does not beat it.

This effect is discussed in Löbner (2000: §4.2) (see also Löbner (2013)), who noticed that "[t]his question is, apparently, neglected in almost all the literature on generics. It is not, for example, discussed throughout the whole *Generic Book* [Carlson & Pelletier (1995)]. The only work I know of which addresses the question is Fodor (1970). She arrives at the same result as I will below". Further, Löbner (2000: 282) rightly argues what follows:

> The fact that negation of a simple CS [characterizing sentence] yields an all-or-nothing contrast provides an argument against analyses in terms of universal quantification that is much stronger than the exceptions argument.[32]

Now, the following appears to be a reasonable semantic representation in $\mathcal{RA}^t$ for (183a):

(188) $\neg(\forall x \leq t)(x\Pi i \wedge cat'(x) \rightarrow beat'(Gianni, x))$

[32] Note, however, that Löbner, again correctly, would take the following (187), instead of (182b), as the true negation of (178b); additionally, he does not explicitly consider GIs other than those in subject position. However, there are no appreciable differences in truth conditions between (187) in its generic reading and (182b).

(187) A cat does not scratch Gianni.

Given the difference just observed in interpretation between the universal quantifier and the GI, we should strongly doubt that (188) can be a good semantic representation also for (183b), since a purely pragmatic explanation of that difference is hard to imagine. But let's look now at the following two terms of $\mathcal{RA}^t$:

(189) $(\forall x \leq t)(x\Pi i \wedge \mathit{cat}'(x) \rightarrow \mathit{scratch}'(x, \mathit{Gianni}))$

(190) $v\Pi i \wedge \mathit{cat}'(v) \rightarrow \mathit{scratch}'(v, \mathit{Gianni})$

Given theorems 4.1 and 6.1, it can be easily proved that (189) and (190) are logically equivalent in $\mathcal{RA}^t$.[33] Hence, we could easily hypothesize that, if (189) is the semantic representation associated with (178a), then (190) could be the semantic representation associated with (178b). If things are so, let's see what kind of semantic representation we would get for (183b) in accordance with this hypothesis:

(191) $\neg(v\Pi i \wedge \mathit{cat}'(v) \rightarrow \mathit{beat}'(\mathit{Gianni}, v))$

With such a representation, we immediately obtain a welcome result: through the rule of inference (P), in fact, we may substitute the generic variable v in (191) with whatever term even if the outmost operator is the negation, something which is however impossible in the case of the universal quantifier. If we replace the variable with a term which denotes an individual which belongs to the relevant set of cats, in particular, we get the semantic representation of a right logical

[33]This equivalence is obtained by assuming that the restrictor of the GI provides a covert argument in order to obtain the same partitive interpretation which, under our assumptions, is mandatory in the case of the universal quantifier (see §7.1.1).

For the view that nothing prevents GIs from being contextually restricted, see Greenberg (1998) (anticipated in part by Condoravdi (1997)).

consequence of the corresponding sentence. In fact, after substituting v in (191) with whatever number n denoting a cat in the relevant set, the resulting logical form entails that, since the antecedent of the conditional is true but the whole conditional needs to be false (given that it is in the scope of the negation), the consequent must be false too; hence, for any number denoting a cat in the relevant set, we obtain that Gianni does not beat it, i.e. we obtain precisely the stronger reading conveyed by (186).

However, the semantic representation (191) has at least two main (related) problems. The first one is syntactic: under reasonable assumptions about the mapping between syntax and semantics, this logical form implies that the negation takes direct scope over a connective, something which would violate the BNCS.

Moreover, I have previously only considered substitution of v by a number denoting a cat in the relevant set, arguing that, in this case, the resulting semantic representation correctly reflects the intuitive truth conditions, unlike what we would have through (188). But once we come to consider numbers denoting individuals which are not cats in the relevant set, things completely change. If we particularize the generic variable v in (191) with a number n which does not denote a cat in the relevant set, then the antecedent of the conditional would turn out to be false and hence the whole conditional would automatically be true and its negation false: by *reductio ad absurdum*, we could conclude that all individuals must be cats in the relevant set! Of course, such an odd conclusion is not entailed by (183b). This is an instance of what has been known, after Reinhart (1997), as the Donald Duck problem.[34]

[34]This name is due to the way the following example is commented by Reinhart:

(192) If we invite a certain philosopher to the party, Max will be annoyed.

A better candidate as a semantic representation for sentence (183b) may be the following:[35]

(193) $cat'(v) \rightarrow \neg beat'(Gianni, v)$

(193) no longer violates the BNCS and it seems to carry the right truth conditions. Note that, under this semantic analysis and assuming the BNCS, we are forced to analyze (182b) as not containing a GI but the standard existential quantifier; this analysis is also semantically unproblematic, since it leads to the right truth conditions as well.

As I have said, Löbner (2000: §4) comes very close to these same conclusions about the interaction between negation and GIs. He scrutinizes and rules out both the carlsonian and the quantificational accounts of CSs. It is especially interesting what he has to say about the second one (Löbner (2000: 282–3); examples renumbered):

> Many present analyses of CSs agree in assuming a genericity operator in the semantic representations of CSs. The genericity operator, written as "GEN" in Carlson and Pelletier (1995)[,] is unanimously given a

a. ✗ (a certain x) [[if [x is a philosopher] and [we invite x to the party]] Max will be annoyed]

b. ✓ (a certain x: x is a philosopher) [[if we invite x to the party] Max will be annoyed]

The fact that Donald Duck is not a philosopher, under the reading represented by (192a), would make (192) true as about him. However, a much more reasonable reading for (192) is the one represented by (192b).

[35]Here, I omitted that part of the semantic representation which corresponds to the partitive interpretation. First, in fact, under many natural readings of the sentences involved such covert information is not conveyed; secondly, I think it should now be clear enough that it is not the presence or absence of existential import which is the main source of the semantic differences between universal quantifiers and GIs.

semantics in terms of some variants of universal quantification.

The inadequacy of any account of genericity in terms of universal quantification shows up in the following problem, which is immediately related to the problem of intrasentential negation. Consider a question-answer pair such as

(194) Is sushi delicious? — No.

We would certainly want to be able to analyze sentential *no* in general as the negation of the proposition of the question, i.e., as an equivalent of the negation of the corresponding declarative sentence. 'No', in (194) must be equivalent to

(195) Sushi is not delicious.

Thus, the only plausible semantic representation of sentential *no* in this function appears to be something like "$\neg \mathbf{p}$", where $\mathbf{p}$ is a free variable for a proposition provided by the context. Now, if we represent the meaning of *sushi is delicious* roughly (omitting all variables) by

(196) GEN(sushi; is delicious)

we would have to represent the meaning of sentential *no* in (194) *ad hoc* by (197) – in disagreement with the general interpretation '$\neg$p', which would result in (198):

(197) GEN(sushi; $\neg$ is delicious)

(198) ¬GEN(sushi; is delicious)

> This is certainly an unwelcome consequence. Independently of the question whether a hidden genericity operator is admissible or not, the negation argument developed here is an argument against the *kind* of operator used in the analyses mentioned.

I think that Löbner's (2000) criticism of quantificational accounts of characterizing sentences is indeed on the right track. Note, further, that there is little probability that theories advocating a treatment of genericity along the lines of Lewis (1975) can fall outside the quantificational family, since the generic operator is usually assumed to have the adverb *always* as its almost exact overt counterpart, and *always* is without a doubt a quantificational adverb, as its interaction with negation shows; the following sentence (199), which is logically compatible with (200), illustrates this latter claim:

(199) Gianni is not always sick.

(200) Gianni is sick most times.

As for Löbner's positive alternative proposal (which I cannot examine in depth here, since Löbner, in that paper, has many other fishes to fry), it rests on a broad conception of genericity along the lines of the following quotation (Löbner (2000: 290–2); emphasis is in the original; examples are renumbered):

> Common to the generic constructions discussed here [...] is [...] a certain mode of language use. This mode is characterized as a predication about hypothetical cases involving at least one parameter that is

not referentially anchored in the situation of utterance. Generic talk is talk on the level of categories rather than individuals referred to [...]. However, it is not *explicit* talk about categories, as no categories are mentioned as such (and therefore I do not agree with Carlson's *reference*-to-kinds analysis). Let me illustrate what I mean by referential anchoring with a simple example.

(201) a. dogs bark

b. dogs are chasing Joan

(201b) is a sentence about Joan, a report about some event taking place during the present time. Both tense (plus the progressive form) and the reference to Joan anchor the event to particular components of the world. Since the event itself is anchored, its agent is part of reality as well: the referent of the indefinite NP can be anchored. No such possibility exists in the case of (201a). The sentence is somehow anchored by tense, but the non-progressive form of the event verb *bark* in its present tense does not allow its referential anchoring to any particular event. Hence, its agent role cannot be anchored either.

Thus, the category level quality of such statements is brought about by the fact that the generic NP, generic conditional or generic relative provides no more information than just an explicit categorization of an unanchorable parameter. Being left with this information alone, without the possibility of connecting any other information that we might have about any real, i.e., particular, values of parameters in case

> they were anchored, we have to take the predication as relevant on the basis of the explicit categorization alone. It is the lack of anchoring that makes these statements general.
>
> [...] I assume that the availability of an unanchored reading of the indefinite is crucial.
>
> (S) **Hypothesis**
>
> Generic predication is predication about referentially unanchored cases.
>
> [...] If this line of analysis is correct, we obtain at the same time an explanation of the truth-value gaps resulting from simple generic predication. [...] [T]he truth-value gaps of generic quantification are due to a predication in terms of abstract cases. If I state a predication in terms of an abstract case specified by the generic NP or clause, I attribute the truth of the predication to the sort of case specified. Hence it carries over to the whole category. It is important to note in this regard that indefinites inevitably provide *sortal* information about possible referents as objects with certain characteristics they exhibit for themselves.

The claim that generic predication essentially involves "referentially unanchored cases" can be interesting, in the present framework, at least insofar as it can be matched with the fact that, under present assumptions, natural language quantifiers logically translate as bounded quantifiers, which, as their name reveals, have an argument "anchoring" them to an upper boundary of their domain of quantification, while logical translations of GIs lack such a boundary. However, as I said, this boundary may be "simulated" even for GIs, by simply imposing, explicitly or with the essential contribution of the context of utterance, some suitable specifications on their restrictor.

Moreover, Löbner's claim seems to parallel the one, which is quite ubiquitous in the literature, that there are no "episodic" generics, as his speculations about tense suggest. However, Greenberg (1998) already showed that this claim is simply false, providing the following examples:

(202) a. A Jew is in synagogue tonight.

b. A faithful Catholic is in Church today.

Further, he observes that there are factors which seem to considerably improve the degree of acceptability of such temporally restricted GIs, like the presence of modal expressions *would* or *should* ((203)), the addition of what he calls "modifying "normative" adjectives" ((204)) and, finally and more expected, context ((205)):

(203) a. An Italian restaurant would be closed today.

b. A lion would be very aggressive today.

c. A Catholic should be nervous today.

d. A child should be especially polite today.

(204) a. A decent Italian restaurant is closed today.

b. A true Clinton supporter is happy tonight.

c. A decent accountant is busy this week.

d. A well-behaved child is especially polite today.

(205) (*Scenario*: Me and my friend decide to eat tonight in an Italian restau-

rant, but an hour later my friend calls me and says "We better go eat in an Indian restaurant tonight. I just remembered that it's Italy's independence day")[36]

An Italian restaurant is closed tonight.

Besides, even if he rightly argues against quantificational approaches to GIs, Löbner concretely strives to account for their interaction with negation by appealing to a presupposition of Excluded Middle, along the lines of Fodor (1979) (acknowledged by Löbner himself, as we saw before) mentioned above (p. 80). This is precisely his formulation (Löbner's (2000: 294) condition (120a), where D is "the domain of cases the predication is about"):

(206) $\forall x(x \in \mathrm{D} \rightarrow \mathrm{P}(x)) \vee \forall x(x \in \mathrm{D} \rightarrow \neg\mathrm{P}(x))$.

All the objections raised to a solution of this kind for the case of plural definite DPs also extend to the present case of GIs.

As regards GIs, Löbner only addresses their interaction with negation. It may seem, until now, that the different behaviour between GIs and universal quantifiers is limited to cases involving negation and could, thus, be related to some features of negation rather than to features of those determiners. Things are different, however. Consider the following examples:[37]

[36] *Sic*!

[37] Here, I needed a verb with three DP arguments in order to have one which both precedes the GI in the linear order and is postverbal: in this case, its interpretation as a topicalised DP is ruled out, or, at least, strongly disfavoured. Topicalization can, in fact, interfere with the phenomena I am addressing, since it involves a CP projection and, hence, can give rise to those apparent counterexamples I am going to discuss in §7.2.8. Further, I chose a quantified subject in order to avoid any possible interference of a referential interpretation of the non-generic indefinite by embedding this latter in its scope.

(207) Every sailor shows some shell to every blonde girl.

a. ✓ 'For every sailor, there is at least one shell such that, for every blonde girl, he shows it to her.'

b. ✓ 'For every sailor and for every blonde girl, there is at least one shell such that he shows it to her.'

(208) Every sailor shows some shell to a blonde girl.

a. ✗ 'For every sailor, there is at least one shell such that, for any blonde girl, he shows it to her.'

b. ✓ 'For every sailor and for any blonde girl, there is at least one shell such that he shows it to her.'

The (a)-readings are the readings where the determiners, provided that they all translate in logical form into scope-bearing logical operators, take scope parallel to their surface order; in the (b)-readings, instead, the last determiner in the surface appears to have outscoped the existential quantifier in the logical form; other readings are of course available for both sentences, but are not relevant for the present purposes and do not need to be taken into account.

Now, the unexpected fact for a theory of GIs as universal-alike quantifiers is that the (a)-reading is only possible for sentence (207) containing as its last determiner a universal quantifier, while it is not available for sentence (208) with the GI in its place. Again, thus, there is an unexpected mismatch between the universal quantifier and the GI, even if in this case it goes in the opposite direction with respect to the couple of examples (183a) and (183b),[38] because it is now the

[38] Here I do not consider (182a) and (182b), since, as I argued above, in the case of (182b), it is likely that the only possible interpretation for the indefinite is the existential one.

sentence without the GI, (207), which possibly expresses the strongest reading, while the weakest one is the only one available for (208); this is easily seen by realizing that, if (207) is interpreted in the reading represented by (207a), then only (208) is compatible with the following scenario:

(209) There is no shell such that at least one sailor showed it to every blonde girl.

If we consider, then, the two following logical forms, corresponding to readings (207a) and (207b) respectively, this means that (207a) cannot be the semantic representation of (208).

(210) a. $(\forall x \leq t)(x \Pi i \wedge \mathit{sailor}'(x) \rightarrow (\exists y \leq t)(y \Pi j \wedge \mathit{shell}'(y) \wedge$
$\wedge (\forall z \leq t)(z \Pi h \wedge \mathit{girl}'(z) \wedge \mathit{blonde}'(z) \rightarrow \mathit{show}'(x, y, z))))$

b. $(\forall x \leq t)(x \Pi i \wedge \mathit{sailor}'(x) \rightarrow (\forall z \leq t)(z \Pi h \wedge \mathit{girl}'(z) \wedge \mathit{blonde}'(z) \rightarrow$
$\rightarrow (\exists y \leq t)(y \Pi j \wedge \mathit{shell}'(y) \wedge \mathit{show}'(x, y, z))))$

Again, we can try, as a first approximation, to translate (208) by using a generic variable for the GI; the result is arguably the following:

(211) $(\forall x \leq t)(x \Pi i \wedge \mathit{sailor}'(x) \rightarrow (\exists y \leq t)(y \Pi j \wedge \mathit{shell}'(y) \wedge$
$\wedge (v \Pi h \wedge \mathit{girl}'(v) \wedge \mathit{blonde}'(v) \rightarrow \mathit{show}'(x, y, v))))$

(211) immediately does the job: from it, applying rule (P) to any n, we can derive that, if n denotes something which is a blonde girl, then every sailor shows a shell to her (with nothing forcing the interpretation that there must be one and the same shell shown to each girl by each sailor): this corresponds exactly to the

expected interpretation of (208) paraphrased in (208b).

Notice that, in this case, no Donald Duck problem arises, since we could formally prove that this problem is limited to environments which are not upward entailing (henceforth, also simply 'UE'),[39] while the nuclear scope of any determiner within (211) is. However, (211) would be the translation of an alleged deep syntactic structure where the existential quantifier takes immediate scope over a material implication, i.e. a coordinate structure, without possibly receiving case from the antecedent; hence, it would violate the CSC. This fact suggests the following logically equivalent semantic representation for (208) (again, I set aside the possible partitive interpretation of the generic):

(212) $$girl'(v) \wedge blonde'(v) \rightarrow (\forall x \leq t)(x \Pi i \wedge sailor'(x) \rightarrow \rightarrow (\exists y \leq t)(y \Pi j \wedge shell'(y) \wedge show'(x, y, v)))$$

In order not to reproduce, relative to the universal quantifier, the problem of the violation of the CSC, in (212) the antecedent of the conditional introduced by the restrictor of the GI also outscopes the universal quantifier: the interpretation, however, matches the one represented by (208b). Both the following theorems, in fact, hold, with the first one corresponding to a familiar result of CL and the second, only minimally differing from the previous one for the substitution of a universal quantifier with a generic variable, showing once more the universal-alike character of generics:

Theorem 7.1 $(\forall x \leq A)(\varphi(x) \rightarrow (\forall y \leq B)(\psi(y) \rightarrow \chi(x, y))) = 0 \dashv\vdash_{\mathcal{RA}^t} \dashv\vdash_{\mathcal{RA}^t} (\forall y \leq B)(\psi(y) \rightarrow (\forall x \leq A)(\varphi(x) \rightarrow \chi(x, y))) = 0$

[39]See Fauconnier (1975b,a), Ladusaw (1979b, 1980) and Barwise & Cooper (1981), for instance, for the relevant definition of "upward entailment".

In both senses, the derivation follows from Goodstein's (1957: 84) theorem 3.834, Goodstein's (1957: 85) 3.982 and Goodstein's (1957: 82) 3.06, given also the Deduction Theorem (Goodstein (1954: 255) and Goodstein (1957: 114)). $\square$

Theorem 7.2 $(\forall x \leq A)(\varphi(x) \rightarrow (\psi(v) \rightarrow \chi(x, v))) = 0 \dashv\vdash_{\mathcal{RA}^t}$

$\dashv\vdash_{\mathcal{RA}^t} \psi(v) \rightarrow (\forall x \leq A)(\varphi(x) \rightarrow \chi(x, v)) = 0$

In both senses, the derivation follows from Goodstein's (1957) theorems 3.06 and 3.834 alone, again *via* the Deduction Theorem. $\square$

Note that the same contrast observed between (207) and (208) is also replicated in the case of the following two slightly more complex sentences,[40] where pragmatic factors should favour a reading of the direct object as taking scope over the indirect object; notwithstanding these factors, such a reading is still absent with the GI.[41]

(214) Every sailor showed with pride some of his shells to every girl passing there.

a. ✓ 'For every sailor, there is at least one shell such that, for every girl passing there, he showed it to her with pride.'

[40]The reason why I developed the argument above starting from (207) and (208) instead of from (214) and (215) is precisely that the formalization of the latter would have resulted in even more complicated logical forms.

[41]It seems to me, as well as to my English informants, that the judgement of impossibility for the reading correspondent to (215b) becomes more fuzzy when we replace *some* with, for instance, a numeral:

(213) Every sailor showed with pride two of his shells to a girl passing there.

But this may well be a pragmatic effect due to a functional interpretation of the existential indefinite (i.e., two of his shells which are such and such: for instance, the two biggest shells he had), while such an interpretation is semantically blocked in the case of *some* (again, see Zamparelli (2007) and Alonso-Ovalle & Menéndez-Benito (2008, 2010, 2011) for the counterparts of *some* in other languages). The important fact is, in my view, that despite the non-functional preferences of *some*, its wide scope reading over a universal quantifier, as exemplified by (214a), is perfectly accessible, while it is not in the case of a GI.

b. ✓ ‘For every sailor and for every girl passing there, there is at least one shell such that he showed it to her with pride.’

(215) Every sailor showed with pride some of his shells to a girl passing there.

a. ✗ ‘For every sailor, there is at least one shell such that, for any girl passing there, he showed it to her with pride.’

b. ✓ ‘For every sailor and for any girl passing there, there is at least one shell such that he showed it to her with pride.’

One natural way to definitively check if, even in UE-environments, we should assume that the restrictor of a GI, translating in logical form as the antecedent of a material implication, takes wide scope over quantifiers, is by looking at sentences where the indefinite is anaphorically linked to a quantified DP: we should expect, in these cases, that if the restrictor cannot be interpreted *in situ*, then the indefinite cannot be interpreted generically. This prediction is actually borne out when looking, for instance, at the following sharp contrast (where one should try to interpret the last indefinite in (216a) generically):[42]

(216) a. *Gianni shows some shells$_i$ he found to a blonde girl who likes them$_i$.

b. Gianni shows some shells$_i$ he found to every blonde girl who likes them$_i$.

(217) a. *Gianni shows every shell$_i$ he found to a blonde girl who likes it$_i$.

[42]See also n. 37 above about the need for three DP arguments.
Note, as well, that, under the controversial assumption that specific indefinites conceived as scopeless elements exist (like, classically, in Fodor & Sag (1982)), (216a) should be able to receive such an interpretation where the second indefinite is a GI. Since this is not the case, (216a) seems to provide an argument against an account of specific indefinites as scopeless elements and thus, plausibly, also in general against specificity as a distinguished semantic feature in its own right.

b. Gianni shows every shell$_i$ he found to every blonde girl who likes it$_i$.

The pattern differentiating the universal quantifier from the GI can be detected also in connection with other, semantically more complex, environments:

(218) a. No dog-sitter beats every cat.

b. No dog-sitter beats a cat.

(219) a. Exactly two dog-sitters beat every cat.

b. Exactly two dog-sitters beat a cat.

Again, (218a) but not (218b) is compatible with (220), and, again, (219a) but not (219b) (in one of its readings) is compatible with (221):

(220) There is a dog-sitter such that there is a cat that he beats.

(221) There is no dog-sitter who beats every cat.

The readings conveyed by sentences (218b) and (219b) with GIs can be roughly paraphrased, respectively, by the following sentences displaying widest scope universal quantifiers in their place:

(222) For every cat, no dog-sitter beats it.

(223) For every cat, there are exactly two dog-sitters who beat it.

Now, the following ones appear to be reasonable semantic representations for (218a) and (219a), respectively:

(224) $\neg(\exists x \leq t)(dog\_sitter'(x) \wedge (\forall y \leq t)(y\Pi j \wedge cat'(y) \rightarrow beat'(x,y)))$

(225) $(\exists x \leq t)(Card(x,2) \wedge (\forall y \leq t)(y\Pi x \rightarrow dog\_sitter'(y)) \wedge$
$\wedge (\forall y \leq t)(y\Pi x \rightarrow (\forall z \leq t)(z\Pi j \wedge cat'(z) \rightarrow beat'(y,z)))) \wedge$
$\wedge \neg(\exists x \leq t)(Card(x,3) \wedge (\forall y \leq t)(y\Pi x \rightarrow dog\_sitter'(y)) \wedge$
$\wedge (\forall y \leq t)(y\Pi x \rightarrow (\forall z \leq t)(z\Pi j \wedge cat'(z) \rightarrow beat'(y,z))))$

In accordance with what I argued before for (183b) and (208), the following terms are instead good candidates to be the semantic representations of (218b) and (219b), respectively:

(226) $cat'(v) \rightarrow \neg(\exists x \leq t)(dog\_sitter'(x) \wedge beat'(x,v))$

(227) $cat'(v) \rightarrow (\exists x \leq t)(Card(x,2) \wedge (\forall y \leq t)(y\Pi x \rightarrow dog\_sitter'(y)) \wedge$
$\wedge (\forall y \leq t)(y\Pi x \rightarrow beat'(y,v))) \wedge \neg(\exists x \leq t)(Card(x,3) \wedge$
$\wedge (\forall y \leq t)(y\Pi x \rightarrow dog\_sitter'(y)) \wedge (\forall y \leq t)(y\Pi x \rightarrow beat'(y,v)))$

I believe that, conjunctively, examples (183b), (208), (218b) and (219b) provide strong empirical evidence supporting the claim that GIs provide generic variables to the logical form of the sentence (*plus* the structure of a material implication). This is, hence, the semantic representation that I am suggesting for them:

(228) $$[\![\, [_{\mathrm{IP}_{1v}}\ [_{\mathrm{DP}_v}\ [_{\mathrm{D}}\ \mathrm{Gen}]\ \mathrm{NumP}_v]\ \mathrm{IP}_{2v}] \,]\!] \quad = \quad [\![\mathrm{NumP}_v]\!] \rightarrow [\![\mathrm{IP}_{2v}]\!].$$

Now, I move to an apparent counterexample which seems to threaten this relatively simple picture.

7.2.8 Generic indefinites in dependent clauses

There is, indeed, a simple way to obtain sentences whose meaning resembles very much that of (182a) or (183a) but where, instead of the universal quantifier, a GI appears: it is by embedding the GI within a CP. Look at the following examples:

(229) It is not true that a cat scratches Gianni.

a. ✓ it is not true that [(a_{Gen} v: v is a cat) [v scratches Gianni]]

b. ✓ (a_{Gen} v: v is a cat) [it is not true that [v scratches Gianni]]

(230) It is not true that Gianni beats a cat.

a. ✓ it is not true that [(a_{Gen} v: v is a cat) [Gianni beats v]]

b. ✓ (a_{Gen} v: v is a cat) [it is not true that [Gianni beats v]]

We can easily see that the readings represented by (229a) and (230a) roughly correspond to the weak interpretation of (182a) and (183a), respectively, and are compatible, thus, with the scenarios in (184) and (185), unlike (182b) and (183b).

Actually, there is nothing really surprising in this phenomenon: as I argued in §6.7 above, dependent clauses provide a number to the semantic representation of their matrix clause, namely the number of the semantic representation of a sentence; variables within that sentence may well be accessible from outside, as implied by the semantic representation I gave, but crucially they do not need to. In the (a)-readings, the variables provided by the GIs are opaque, to use the terminology introduced in §6.7, while in the (b)-readings they are still transparent. The distinction is formally represented through the following logical forms, corresponding to (229a)–(230b):

(231) a. $\neg true'(\ulcorner cat'(v) \to scratch'(v, Gianni)\urcorner(Gianni))$

b. $cat'(v) \to \neg true'(\ulcorner scratch'(v, Gianni)\urcorner(v, Gianni))$

(232) a. $\neg true'(\ulcorner cat'(v) \to beat'(Gianni, v)\urcorner(Gianni))$

b. $cat'(v) \to \neg true'(\ulcorner beat'(Gianni, v)\urcorner(Gianni, v))$

This is instead an example designed to obtain an interpretation similar to that of (208a) by using a GI within a relative clause:

(233) Every sailor owns some shell that he shows to a blonde girl.

a. ✓ (every x: x is a sailor) [(some y: y is a shell such that [(a$_{Gen}$ v: v is a blonde girl) [x shows y to v]]) [x owns y]]

b. ✓ (a$_{Gen}$ v: v is a blonde girl) [(every x: x is a sailor) [(some y: y is a shell such that [x shows y to v]) [x owns y]]]

Again, we can see that a reading like (233a), which was impossible for (208), is now available for (233): this is, of course, the opaque reading of the GI, while the transparent one, represented by (233b), is close to the only one which was possible for (208), namely the one represented by (208b). Formally, the contrast between the two readings reveals itself through the following logical forms:

(234) a. $(\forall x \leq t)(x\Pi i \wedge sailor'(x) \to (\exists y \leq t)(y\Pi j \wedge shell'(y) \wedge$
$\wedge\, Proof(t, \ulcorner girl'(v) \wedge blonde'(v) \to show'(x, y, v)\urcorner(x, y))))$

b. $girl'(v) \wedge blonde'(v) \rightarrow (\forall x \leq t)(x \Pi i \wedge sailor'(x) \rightarrow$

$\rightarrow (\exists y \leq t)(y \Pi j \wedge shell'(y) \wedge Proof(t, \ulcorner show'(x,y,v) \urcorner(x,y,v))))$

Given that the semantic representation suggested here for dependent clauses is independently motivated (on the basis of the problem of logical omniscience *plus* considerations of parallelism between syntax and semantics) and that it suffices to explain the different behaviour of GIs when they are embedded within them with respect to their behaviour when they are not, I conclude that dependent clauses do not constitute a real counterexample to my claim that GIs translate in logical form as generic variables. On the contrary, once we had independently proved this claim, the asymmetry between dependent and independent clauses seems to add further evidence supporting sententialism.

As far as I can see, GIs which are complements of deverbal nouns can receive an interpretation with narrow scope with respect to quantifiers in the same sentence: this would be coherent with the analysis of nominalizations as relative clauses in disguise which I suggested in §6.1.3.

(235) Gianni knows some ridiculous admirer of an artwork made by him.

a. ✓ (some x: (a$_{Gen}$ v: v is an artwork made by x) [x is a ridiculous admirer of v]) [Gianni knows x]

(236) Every novelist introduced some of his poet friends to some admirer of a work of his written in hendecasyllabic verses.

a. ✓ (every x: x is a novelist) [(some y: y is a poet friend of x) [(some z: (a$_{Gen}$ v: v is a work of y written in hendecasyllabic verses) [z is an admirer of v]) x introduced y to z]]

7.3 Conditionals

In §6.6, I discussed the BNCS deliberately avoiding making reference to the case of conditional sentences, in this way departing from Horn's (1989) treatment. The reason why Horn (1989), instead, also took into account conditional sentences is that he presupposed an account of conditionals like the one in standard logic, i.e. as coordinated structures triggered by an operator whose English counterpart would be the "connective" *if (... then)*; the semantic treatment of this connective, besides, is given in terms of what is known as "material implication" (starting with Russell, who borrowed its logical definition from Frege (1879)), corresponding to the definition of the standard symbol '$\rightarrow$' given above in (5) (which does not show any difference between CL and RA).

Even if the structure defined in (5) may of course be relevant (and I assume that, in fact, it is) for the semantic characterization of natural language conditionals (and for that of universal quantifiers and GIs as well, along the lines we saw above), Kratzer (1977, 1978, 1979, 1981, 1986, 1991) provided a much sounder semantic analysis for these constructions than the one based on pure material implication. First of all, Kratzer pays serious attention to the syntactic circumstance that the antecedent of the conditional is introduced by a complementizer, i.e. an element that has the same syntactic category as the head of standard relative clauses. Additionally, following previous analyses by Lewis (1973) and Stalnaker (1975) (ultimately affording in Ramsey (1931a)), she strives to build an account covering as uniformly as possible both indicative and subjunctive conditionals (for some irreducible differences, on the semantic side, see already Adams (1970)).[43]

[43]There is an enormous literature on the semantic properties of the conditional and its subspecies. I can mention here only some studies I am aware of and consider particularly original:

In a nutshell, Kratzer, developing the insight in Lewis (1975) which I already mentioned above (see §7.2.4 above), analyzes a conditional as a tripartite structure where the antecedent provides the restriction for a (possibly) covert modal operator, and the consequent is its nuclear scope. Her proposal is actually more articulate, but this is what I consider its most important and solid portion. The following is a famous quotation from Kratzer (1991: 656):

> The history of the conditional is the story of a syntactic mistake. There is no two-place *if* ... *then* connective in natural language. *If*-clauses are devices for restricting the domains of various operators.

Now, the problem is the same as above: such a modal operator is again conceived as some variant of the (unbounded) universal quantifier, possibly GEN itself, ranging over possible worlds or possible situations or possible states of affairs or possible whatever else.[44] But a quantifier of whatever kind must face the same sort of objections raised for GEN by Löbner (2000) and quoted above. In particu-

Jespersen (1909b) and Bolinger (1967b) on conditional conjunction and disjunction and Keshet (2013) on conditional conjunction; Palmer (1974), Thomason (1984), Iatridou (2000), Ogihara (2000) and Ippolito (2003) (the latter formulates a theory of the interaction between modality and temporality analogous to that in Condoravdi (2002)) on the role of past tense in counterfactuals; McCawley (1988) and Beck (1997) deal with the problem of comparative conditionals, i.e. constructions which in English follow the schema *The X-er A, the X-er B*; Biezma (2011) (partly based on Iatridou & Embick (1994) and Schwarzschild (1999)) on subject-auxiliary inversion in the antecedent of conditionals; Schwarz (1998) and Romero (2000) on reduced conditionals; Schwager (2005, 2006) on conditionalized imperatives; Schwager (2005) and von Fintel & Iatridou (2007) on sufficiency modal constructions.

[44]Even if here I am trying to set aside as much as possible ontological questions, I feel that speaking of possible worlds in connection with indicative conditionals is particularly problematic, since it seems to me that what we are considering when evaluating an indicative conditional is not possible worlds different from the actual one (as we may be obliged to consider, instead, when evaluating a subjunctive conditional), but rather situations of the actual world which possibly we have not directly experienced.

For the semantics of mood see especially Farkas (1985, 1992b,a) and, for some comparisons between English and Italian, Portner (1992, 1997), but already Quine (1956) and, in connection with conditionals, Anderson (1951).

lar, it would not be easy to explain, under the assumption that a covert operator GEN is restricted by the antecedent of a conditional, why the logical negation of a conditional is impossible to obtain in natural language, if not negating an intensional predicate (possibly covert) whose complement contains the conditional, while the logical negation of a universal quantifier is always fine. The point is illustrated, for both indicative and subjunctive conditionals, by the following examples (grammaticality judgements, again, do not extend to marked information structures):[45]

(237) a. *Not if Gianni is at home, every sandwich is on the table.

b. *Not if Gianni was at home, every sandwich would be on the table.

(238) a. Not every sandwich is on the table, if Gianni is at home.

(i) ✓ 'if Gianni is at home, not every sandwich is on the table'

(ii) ✗ 'it is not the case that every sandwich is on the table if Gianni is at home'

b. Not every sandwich would be on the table, if Gianni was at home.

(i) ✓ 'if Gianni was at home, not every sandwich would be on the table'

(ii) ✗ 'it is not the case that every sandwich would be on the table if Gianni was at home'

(239) a. The sandwich is not on the table, if Gianni is at home.

[45] I am leaving aside here from the semantic or pragmatic differences induced by the reciprocal order of antecedent and consequent. I chose a quantificational subject in order not to have the ban of negation over definites interfere; however, with sentences in (239), I also checked the case where the negation is attached to the main verb and tries to take scope over the alleged covert operator from that position.

(i) ✓ 'if Gianni is at home, the sandwich is not on the table'

(ii) ✗ 'it is not the case that the sandwich is on the table if Gianni is at home'

b. The sandwich would not be on the table, if Gianni was at home.

(i) ✓ 'if Gianni was at home, the sandwich would not be on the table'

(ii) ✗ 'it is not the case that the sandwich would be on the table if Gianni was at home'

(240) a. It is not the case that, if Gianni is at home, every sandwich is on the table.

b. It is not the case that, if Gianni was at home, every sandwich would be on the table.

(241) a. It is not the case that every sandwich is on the table if Gianni is at home.

b. It is not the case that every sandwich would be on the table if Gianni was at home.

The contrast between possible and impossible interpretations of sentences in (238) and (239) is made more apparent by the fact that both (238a) and (238b), on one side, and (239a) and (239b), on the other, are inconsistent with the following scenarios (242) and (243), respectively, while they should not be if the negation behaved like a logical negation with wide scope over the covert head of the antecedent:

(242) It is possible that every sandwich is on the table and Gianni is at home.

(243) It is possible that the sandwich is on the table and Gianni is at home.

The point was already made by Dummett (1973) and Grice (1975). Dummett (1973: 330) (as quoted in Horn (1989: §6.2.2)) pushed himself to conclude that “we have no negation of the conditional of natural language, that is, no negation of its sense: we have only a form for expressing refusal to assent to its assertion” (the context from which this passage has been extracted makes it clear that, when speaking of the refusal to assent to an assertion, Dummett here has in mind the utterance of a negative statement where negation attaches to a *verum* predicate or similar).

Now, if, repeating the same argument developed above for GIs, we assume that the antecedent of the conditional does not restrict a variable bound by a generic operator, but instead, simply, a generic variable, i.e. a scopeless element, we may analyze the pattern of the interaction between negation and conditionals as a mere instantiation of the BNCS again. Thus, we get the same ban on negation taking immediate scope over a conditional as Horn’s, but without committing ourselves to the material implication account of conditionals, while endorsing, instead, the more widely accepted account based on genericity and tripartite structure.

Further evidence that the antecedent of the conditional is to be interpreted as having wide scope within the clause on which it depends comes, again, from observations on anaphora (note that, in (244), the pronoun should not be interpreted as generically referring to the kind of shells; besides, once more, topicalizations should be avoided):

(244) *Gianni shows some shells$_i$ to Luisa if she likes them$_i$.

(245) *Gianni shows every shell_i to Luisa if she likes it_i.

I assume, for concreteness, that the generic variable introduced in indicative conditionals ranges over situations accessible from t (see n. 45 above); the case of subjunctive conditionals is notoriously more complicated. It seems to me that, in subjunctive conditionals, the generic variable ranges over situations which are accessible to the perceptual experience of the speaker up to a certain point in time (possibly also the present instant).[46]

As for the relevant notion of accessibility, here it is impossible to give any precise characterization of it, assuming that a suitable one can even be found somewhere in the literature. However, at least concerning the particular case of epistemic accessibility, it is worth signalling the growing amount of studies on inductive inference taking as their starting point the rigorous formalization of the principle of Ockham's Razor developed in Solomonoff (1960, 1964) and Kolmogorov (1998) (see also Odifreddi (1999: §VII.5)): inductive inference is of course at the core of epistemic modality, which is, I believe, the most difficult subspecies of modality to constrain.[47]

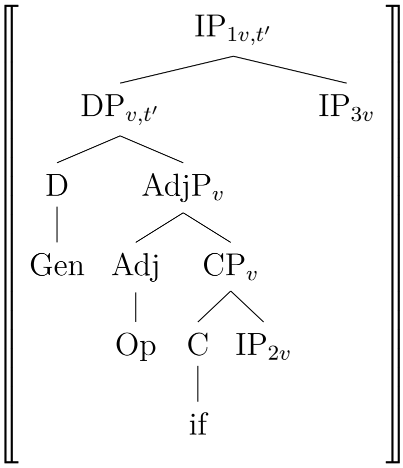
The semantic representation of a conditional sentence, thus, turns out to be the following one (where '*Acc*' is the relation of accessibility between situations and $t' = t$ in the particular case of the indicative conditional; as before, the syntactic

[46] The possibility of the relevant point in time being the present is related to the reason why I prefer to use the label of "subjunctive conditionals" instead of that of "counterfactuals". If conditionals of this kind are in fact needed when we want to speak of scenarios which are contrary to facts, the converse does not hold: i.e., we are not forced to use them only in that case. This assumption is in agreement with most literature on the topic.

[47] When dealing with, say, deontic modality, we could just concentrate on a given set of relevant conventional laws and derive logical consequences from them by deductive inferences, while no such conventional character can be found in natural laws, since we derive them from sensorial experience through that peculiar kind of inferential process, neatly distinct from deduction, which is induction.

structure is simplified):

(246)



$=$

$$= \quad Acc(v,t') \wedge Proof(v, \ulcorner [\![\mathrm{IP}_{2v}]\!] \urcorner(v)) \rightarrow [\![\mathrm{IP}_{3v}]\!]$$

Now that we have sketched such a semantic analysis of conditionals, we can address a puzzle raised by Winter (2001) relative to the following sentence:

(247) If three workers in our staff have a baby soon we will have to face some hard organisational problems.

Winter noticed that (247) cannot have the reading in (248):

(248) There are three workers such that for each x of them, if x has a baby, there will be problems. $3 > Dist > \text{if} > 1$

In the present account, the unavailability of (248) as a reading for (247) is not surprising, since the covert argument restricted by the *if*-clause is a generic variable and, hence, cannot be interpreted as if it had narrow scope under a genuine universal quantifier like *Dist*.

Lewis (1975) also dealt with *when*-clauses, in the margin of his influential theory about adverbs of quantification, viewing them as “stylistic variations” of *if*-clauses restricting an adverb of quantification, in a paragraph titled just in this way (Portner & Partee (2002: 185–6)): in Lewis’s (1975) own words (Portner & Partee (2002: 185)), “canonical restrictive if-clauses may, in suitable contexts, be replaced by when-clauses” (see the discussion on Lewis’s (1975) proposal in §7.2.4 above). In other words, *when*-clauses would play the function of restricting the variable bound by the quantifier introduced by a (possibly covert) adverb of quantification. Letting aside problems of syntactic nature (partly acknowledged by Lewis himself), Löbner’s (2000) criticism, reported above in §7.2.7, to proposals inspired by Lewis (1975) employing (unselective) generic quantifiers extends also to this particular application. Löbner (2000: 285 f.) too, however, treats *when*-clauses, as well as other adverbial clauses and headless relatives, as instances of those linguistic structures whose negation “is formed by VP negation in the main clause”. Even if I completely agree with this claim, I strongly believe that, in accordance with what I said in §6.3 above, free relatives are anaphoric elements, unlike what I argued the head of an *if*-clause to be. Hence, the behaviour of free relatives with respect to negation should not have the same source of that of *if*-clauses, and in particular it should probably be put in relation to the constraint on binding of *wh*-pronouns *via* accommodation (see again §6.3).

Besides the morphological differences between the two types of clauses, I also take the contrast in interpretation between (244) above, repeated here as (249), and (250) to be significant:

(249) *Gianni shows some shells$_i$ to Luisa if she likes them$_i$.

(250) Gianni shows some shells$_i$ to Luisa when she likes them$_i$.

7.4 Negative indefinites

I want now to briefly address the complex issue of negative indefinites (hereafter, also simply 'NIs'), i.e. indefinite determiners which morphologically incorporate a negative mark, like Eng. *no* or It. {*nessun* / *nessuna* / *nessuno*}, because certain puzzling properties of theirs may receive some clarity once we put generic variables into the semantic toolbox.

First of all, consider the following well-known laws of CL:

(251) a. $\neg\forall x\varphi(x) \Leftrightarrow \exists x\neg\varphi(x)$

b. $\neg\exists x\varphi(x) \Leftrightarrow \forall x\neg\varphi(x)$

These laws already appeared in the writings of William of Shyreswood[48] and have the following corresponding versions with bounded quantifiers ((252a) corresponds to Goodstein's (1957: 71) 3.964 or Goodstein's (1957: 83) 3.222, while (252b) can be derived from it through Goodstein's (1957: 59) 3.31 and Goodstein's (1957: 60) 3.34):

(252) a. $\neg(\forall x \leq A)\varphi(x) \Leftrightarrow (\exists x \leq A)\neg\varphi(x)$

b. $\neg(\exists x \leq A)\varphi(x) \Leftrightarrow (\forall x \leq A)\neg\varphi(x)$

Generalizations of these are the following (where '$Q\vec{x}$', with Q an unbounded or

[48]Parallel laws also hold for the classical modal operators of necessity and possibility and they are essentially due to Peter of Spain. In the light of the modern interpretation of necessity in terms of universal quantification and possibility in terms of existential quantification (see p. 225 below), they simply arise as a special case of (251a) and (251b).

bounded quantifier of whatever force, is a shorthand for '$Qx_1 \dots Qx_n$'), which should be of interest once we are dealing with NIs which are also NPIs:

(253) a. $\neg\forall\vec{x}\varphi(\vec{x}) \Leftrightarrow \exists\vec{x}\neg\varphi(\vec{x})$

b. $\neg\exists\vec{x}\varphi(\vec{x}) \Leftrightarrow \forall\vec{x}\neg\varphi(\vec{x})$

(254) a. $\neg(\forall\vec{x} \leq A)\varphi(\vec{x}) \Leftrightarrow (\exists\vec{x} \leq A)\neg\varphi(\vec{x})$

b. $\neg(\exists\vec{x} \leq A)\varphi(\vec{x}) \Leftrightarrow (\forall\vec{x} \leq A)\neg\varphi(\vec{x})$

Developing the parallel between logical quantifiers and connectives I already mentioned above (see p. 122), these laws correspond to standard De Morgan laws (see p. 98):

(255) a. $\neg(\varphi \wedge \psi) \Leftrightarrow \neg\varphi \vee \neg\psi$

b. $\neg(\varphi \vee \psi) \Leftrightarrow \neg\varphi \wedge \neg\psi$

Here are some instantiations of the laws given above in English (notice that those in (256) are not precise instantiations of the laws in (255), since such precise instantiations would violate the BNCS and thus are not available; besides, the indefinites in the examples of (257) are of course to be interpreted as existential quantifiers):

(256) a. It is not the case that both John is bald and Harry is hairy. $\Leftarrow$ $\Rightarrow$ John is not bald or Harry is not hairy.

b. It is not the case that either John is bald or Harry is hairy. $\Leftarrow$ $\Rightarrow$ John is not bald and Harry is not hairy.

(257) a. Not every man is bald. $\Leftarrow$

⇒ A man is not bald.

b. Not a man is bald. ⇐

⇒ Every man is not bald.

Now, because of the logical equivalences in (253)–(254), when dealing with NIs and with negative polarity determiners (henceforth, 'NPDs'),[49] it is impossible to decide on purely truth-conditional grounds if they were to be analyzed as existential quantifiers within the scope of a negation or, on the contrary, as universal quantifiers taking scope over a negation. This fact has led, in many cases, to different analyses of different NIs (and sometimes even to different analyses of the same one) coexisting in the literature.

While NPDs in English have most frequently been analyzed as existential quantifiers in the (immediate) scope of a negation (see Ladusaw (1979a, 1980), Linebarger (1980, 1987)), several other analyses dealing with different languages, in particular recent ones, have taken the opposite path: starting with Szabolcsi (1981) on Hungarian n-words, through Giannakidou (2000) on emphatic n-words in Greek, ending up with Sells (2001), Sells & Kim (2006) and Kim & Sells (2007) on Korean NPDs and Kataoka (2006b,a), and Shimoyama (2008, 2011) on Japanese NPDs,[50] all these studies analyzed some items as universal-alike quantifiers taking

[49]For the licensing conditions of NPDs, the standard analysis is the one based on the notion of *downward entailment*, due to Fauconnier (1975b,a) and Ladusaw (1979b, 1980). See also Klima (1964), Linebarger (1980, 1987), Zwarts (1981, 1986, 1995, 1996a,b, 1998), Hoeksema (1983a, 1986), Heim (1984) (following Schmerling (1971)), Kadmon & Landman (1990, 1993), Giannakidou (1998) and Lahiri (1998) for problems, refinements and extensions.

Regarding the complex issue of the licensing of NPDs in questions, see in particular Higginbotham (1993) (partly relying on Higginbotham (1991)), relating the issue to downward-entailingness; see also Guerzoni & Sharvit (2007) for some criticisms.

[50]Remember that Asian languages in general lack n-words, as reported by Haspelmath (1997); see n. 33 at p. 95 above.

(immediate) scope over a negation.[51]

Here it is impossible to carry out any serious discussion on the quantificational force of NIs and NPDs even in one single language. However, I believe that it is worth stressing the role that the analysis outlined above for GIs may play in suggesting a solution to a puzzle about NIs and the conditional.

First of all, we should emphasize the trivial fact that, if one wants to pursue an analysis of certain NIs in terms of something semantically close to universal quantification, then genericity seems to be the best candidate, since indefinites, in positive environments, are never standard universal quantifiers, while they can be generic. Taking this simple fact as our starting point, a reasonable taxonomy for NIs and NPDs on the basis of their quantificational force may *a priori* be the following one:

I) NIs or NPDs that are always existential quantifiers within the (immediate) scope of a negation;

II) NIs or NPDs that are always GIs taking (immediate) scope over a negation;

III) NIs or NPDs that are ambiguous between existential quantifiers within the (immediate) scope of a negation and GIs taking (immediate) scope over a negation.

I will call these possible items "type-I", "type-II" and "type-III" NIs or NPDs, respectively.

It is interesting to consider, I believe, the case of Italian. Italian has a series

[51] See also the logical forms employed by Zanuttini (1991, 1997) and Haegeman & Zanuttini (1996), with universal quantifiers taking scope over negation, but apparently only committing themselves to the resulting truth conditions of the sentence.

of NIs and a series of NPDs which are not also NIs: they are summarised in the following table:

	Determiner form		**Null-nominal form**	
NIs	*nessun(o)* `no:M;SG` 'no'	*nessuna* `no:F;SG` 'no'	*nessuno* `none` '{nobody / anybody}'	*niente* `nothing` '{nothing / anything}'
NPDs	*alcun(o)* `any:M;SG` 'any'	*alcuna* `any:F;SG` 'any'	∅ *alcunché* `anything` 'anything'	

Table 7.1: NPDs in Italian.

The items in the NIs series, when more than one appears in the same local domain, give rise to the phenomenon of Negative Concord, as the following examples illustrate (in order to be consistent with the hypothesis that the NIs appear in the same local domain, marked information structures must not be taken into account; it is well-known that, in that case, double negation readings may arise):

(258) Gianni non ha regalato niente a nessuno.
`Gianni not has gifted nothing to nobody`
'Gianni did not gift anybody anything'

(259) Nessuno ha regalato niente a nessuno.
`nobody has gifted nothing to nobody`
'nobody gifted anybody anything'

Further, contrary to what happens in non-NC languages like English, in Italian there cannot be a postverbal NI which has not been licensed by a preverbal negation:

(260) John ate nothing.

(261) *Gianni ha mangiato niente.[52]
`Gianni has eaten nothing`

I assume, following Laka Mugarza (1990) and Ladusaw (1992), that data like this is to be taken as evidence that postverbal NIs in NC languages like Italian are NPIs. As is well-known, furthermore, Italian is a Non-Strict NC language, differing from Strict NC languages like, for instance, Czech in that the former does not allow surface sentential negation with a NI subject, while the latter, on the contrary, require it. The pattern is illustrated below:

(263) a. Nessuno ha mangiato {la torta / niente}.
`nobody has eaten the cake nothing`
'nobody ate {the cake / anything}' It.

b. *Nessuno non ha mangiato {la torta / niente}.
`nobody not has eaten the cake nothing`

[52]Sentences like the following (262) are indeed possible in Italian, and they were even more frequent in Old Italian. However, a suitable paraphrase of it easily shows that the meaning of the n-word has somehow shifted:

(262) Questo è niente.
`this is nothing`
'this is a thing of very small value'

At first glance, cases like this one seem to be relicts specular to the so-called Emphatic Negation (see van der Wouden (1994)). This expression refers to a phenomenon attested in some non-NC languages and consists of two words, which must appear strictly adjacent in the sentence, both containing one negative morpheme but conveying a single negative meaning; examples are Dutch *nooit niet* 'never not' or *niets geen* 'nothing no'. For an analysis of Emphatic Negation as a by-product of loss of Negative Concord, see Zeijlstra (2010).

(264) a. *Nikdo {snědl dort / nic snědl}.
`nobody ate cake nothing ate`
b. Nikdo {nejedl dort / nic nejedl}.
`nobody not_ate cake nothing not_ate`
'nobody ate {the cake / anything}' Czech

Following Zeijlstra (2004, 2006), I further assume that the only difference between Italian and Czech is that Italian has, besides an overt negation, also a covert negation at its disposal: in both languages, thus, the negative marker of NIs would be an abstract, uninterpretable negative feature, while the interpretable negation (whether covert or overt) occupies its standard position in the clause: hence, in both Strict and Non-Strict NC languages, all concord items would be NPIs.[53]

Note, as well, that the simultaneous occurrence of preverbal NIs and sentential negation with NC reading was possible in Old Italian, even if less frequent than the case, mentioned above in §6.6, of the negative conjunction cooccurring with standard negation. The following examples are again from Zanuttini (2010):

[53]Note that, on the basis of the examples above alone, one could argue, in an equally plausible way, that the Italian preverbal NI contains a negative morpheme with a truly negative semantic value (and this is, in fact, Ladusaw's (1992) idea); however, we may have as well more than one preverbal NI argument in the same clause, as in the following example:

(265) A nessuna ragazza nessun marinaio ha mostrato {una / nessuna} conchiglia.
`to no girl no sailor has showed a no shell`
'to no girl any sailor showed {a / any} shell'

In this case, we have two preverbal NIs, with the sentence obligatorily lacking an overt standard negation; but the meaning of the sentence requires that at most one of the two contains a negative morpheme which is also semantically negative. Given this state of affairs, it seems to me that Zeijlstra's account explains the empirical facts in a more parsimonious way.

(266) . . . e comandò a' baroni che *neuno* *non* li

and he_ordered to barons that no_one not him

insegnasse spendere questo oro . . .

teach:SBJ;PST:3SG to_spend this gold

'. . . and he told his barons not to instruct him how to spend that gold. . . '

(*Novellino*, 7, ll. 8–10)

(267) . . . sicché si spegnesse l'umana generazione

so_that MID fade_away:SBJ;PST:3SG the-human generation

e *neuno* *non* andasse poscia in paradiso . . .

and no_one not go:SBJ;PST:3SG then in heaven

'. . . so that the human generation faded away and then no one went to heaven. . . '

(Bono Giamboni, *Libro*, Ch. 44, §4)

(268) . . . e che *neuno uomo non* sapea che ne fosse

and that none man not knew what of_him be:SBJ;PST:3SG

adivenuto.

become:PTCP:PST

'. . . and no one knew what had become of him'

(*Novellino*, 64, ll. 54–55)

Now, if we are to classify the Italian items under examination according to the taxonomy just given, I consider the following example rules out for both the determiners the hypothesis that they can belong to type II NPDs:

(269) Ogni marinaio non ha mostrato a qualche ragazza {alcuna / nessuna} conchiglia.
`every sailor not has shown to some girl any no shell`
'every sailor did not show to some girl any shell'

According to one of the readings for (269), there is at least one girl for each sailor such that the sailor did not show to her any shell.[54] This reading is available with items of both series and is possible only if these items are interpreted as existential quantifiers under negation, otherwise negation would be interpreted as having wide scope over the other arguments and over *qualche ragazza* in particular, which would plainly lead to wrong truth conditions.

Additionally, the items in the NPDs series which are not NIs cannot appear in the preverbal position, independently of the fact that a preverbal negation is present or not:

(270) *Alcun ragazzo (non) ha mangiato la torta.
`any boy not has eaten the cake`

In other words, even if it has an element corresponding to Eng. NPI *any*, modern Italian lacks something formally corresponding to Eng. FC *any*, thus it is reasonable to assume that It. {*alcun*(*o*) / *alcuna*} is a type I NPD. However, this state of affairs only represents the actual endpoint of a diachronic process, since in Old Italian, {*alcun*(*o*) / *alcuna*} were not (exclusively) NPIs, but could also appear in positive environments and, among other uses, as FC determiners as well

[54]The complication of the existential quantifier in the scope of a universal one is again intended, as in p. 182 before, to avoid a referential reading of the indefinite.

(see (274) below), as the following examples from Zanuttini (2010) show:

(271) Pubbliche questioni son quelle nelle quali si tratta il
`public questions are those in-the which IMPERS treats the`
convenentre d'*alcuna* cittade o comunanza di genti.
`utility of-any city or community of people`
'public questions are those in which the utility of whatever city or community of people is dealt with'

(Brunetto Latini, *Rettorica*, p. 5, ll. 8–10)

(272) Compagno è quelli che per *alcuno* patto si congiunge
`comrade is the_one that for any pact himself conjoins`
con un altro ad *alcuna* cosa fare.
`with an other to any thing do`
'a comrade is one who, through whatever pact, joins another one to do something'

(Brunetto Latini, *Rettorica*, p. 13, ll. 13–14)

(273) Dimmi *alcuna* cosa de la natura de' Vizî che nascono
`tell=me any thing of the nature of vices that originate`
di lei.
`from her`
'tell me something about the nature of vices which originate from it'

(Bono Giamboni, *Libro*, Ch. 30, §7)

(274) ... in Campo Marzio, nel quale s'asemblava la
`in Campo Marzio in-the which IMPERSgot_together the`

comunanza a llodare *alcuna* persona ch'era degna d'avere
`community to praise any person that-was worthy of-getting`

dignitade e signoria …
`dignity and lordship`

'… in Campo Marzio, where people got together to praise one who was worthy of getting dignity and lordship'

(Brunetto Latini, *Rettorica*, p. 59, ll. 12–14)

Finally, notice that the determiner forms of both the series are strong NPIs (for the notion of strength relevant in this case, see, for instance, Hoeksema (1983b, 1986) or Zwarts (1998)), since they are bad, for instance, in questions, as examples in (275) show, while their pronominal forms, which instead are fine in such environments, seem to be weak NPIs for this reason:

(275) a. %Gianni ha incontrato nessuna ragazza?
`Gianni has met no girl`

b. *Gianni ha incontrato alcuna ragazza?
`Gianni has met any girl`

(276) a. Gianni ha incontrato nessuno?
`Gianni has met nobody`
'did Gianni meet anybody?'

b. Gianni ha mangiato {niente / alcunché}?
`Gianni has eaten nothing anything`
'did Gianni eat anything?'

It still has to be decided if the *nessuno* series groups type I or type III items. Reasoning by uniformity with *alcuno*, we should expect that the former is the right answer. However, there is a puzzle also involving the conditional which suggests that things could be different.

The puzzle was first formulated by Higginbotham (1986) (and further discussed especially in Dekker (2001)) and illustrated through the following English sentences:

(277) a. Every student will succeed if he works hard.

b. No student will succeed if he goofs off.

As Dekker (2001: 118) puts it (examples are renumbered),

> [e]xample (277a) can be given an intuitively correct interpretation, if it is taken to state that for every student the following holds: if he works hard he will succeed. However, a similar analysis of example (277b) would give rather disastrous results. For suppose example (277b) is rendered as stating that for no student this holds: if he goofs off he will succeed. If we read the latter implication as a material one, then the sentence would turn out to state that every student goofs off and no student succeeds, which is way too strong. Alternatively, if the sentence is taken to state that for no student there is a rule-governed connection between goofing off and success, then this is way too weak. Rather, example (277b) seems to state that no student who goofs off will succeed, that is, that goofing off implies failure.

The first case taken into account by Dekker, namely that of material implication, requires, I believe, no further discussion: it parallels the instances of the Donald Duck problem already encountered above. As for the second case, it may formally arise, under the standard assumptions implicit in the quotation above, in two ways: either we have a Kratzer-style modal operator embedded under the NI, or we have, in the same position, an embedded *verum* predicate or similar, leading to a reading of sentence (277b) which could be paraphrased more or less as *For no student it is the case that, if he goofs off, he will succeed* or *No student is such that, if he goofs off, he will succeed*. Readings like that are actually available, I believe, as marked options for sentence (277b), but, nevertheless, as Dekker rightly maintains, they are by no means its most natural readings.

Now, let's see which readings should be expected in the present framework which would not be expected, however, unless without *ad hoc* stipulations, under standard semantic assumptions. As for the semantic representation of the conditional, we have only one possibility; the choice only weighs upon the NI, which we may analyze as an existential quantifier under a negation or as a GI over it. It should be remarked that the former choice encounters an obstacle of syntactic nature, because it configures itself as a violation of the CSC, given that the antecedent must remain within the scope of the negative quantifier, since its subject is anaphorically linked to it, and in this position the material implication (i.e., a connective) would be in the immediate scope of the quantified main subject without the latter having been extracted from the antecedent. However, even setting aside this problem, such an analysis also leads to wrong semantic results. The two semantic representations corresponding to the two different analyses are thus the following ones:[55]

[55] Actually, we would need to formulate a suitable adaptation of Linebarger's (1980) Immediate

$$(278) \quad \neg(\exists x \leq t)(student'(x) \wedge (Acc(w,t) \wedge$$
$$\wedge\, Proof(t, \ulcorner goof\_off'(x,w) \urcorner(x,w)) \rightarrow will\_succeed'(x)))$$

$$(279) \quad student'(v) \rightarrow (Acc(w,t) \wedge Proof(t, \ulcorner goof\_off'(v,w) \urcorner(v,w)) \rightarrow$$
$$\rightarrow \neg will\_succeed'(v))$$

Now, (278) differentiates from the too strong reading with the conditional interpreted in terms of material implication Dekker alluded to for only one aspect: that now the sentence is no longer simply stating that every student goofs off, but rather that every student in any accessible situation does; but this is a completely negligible difference, and the reading remains far too strong with respect to the actual meaning of the sentence. No such an objection, however, can be raised against (279), which seems to correspond correctly to the intuitive truth conditions of (277b).

Italian may provide even more suggestive evidence in favour of a solution to Higginbotham's puzzle along these lines. Consider the following Italian version of the puzzle where direct objects, instead of subjects, are coindexed with the anaphoric pronoun in the antecedent of the conditional and remember that the items in the *alcuno* series, in modern Italian, are all NPIs and are not homophonic with any other item, unlike English *any*:

Scope Constraint (ISC) in order to allow the antecedent of the conditional to intervene between the NI and the negation licensing it. However, such an adaptation would probably not cause big theoretical discomforts, since the logical form of the antecedent has the same structure of that of the DP whose head is the NI, and hence a certain uniformity, at least at the semantic level, is guaranteed. Besides, we could also push ourselves to hypothesize that the covert head restricted by the antecedent can come also in an NPI variant; such an assumption may not be so inventive as it may appear at first glance, considering for instance that the complementizer *if* also introduces embedded yes/no questions, which can license weak NPIs.

(280) Gianni non promuoverà nessuno studente$_i$ se *pro*$_i$ sarà stato
`Gianni not will_pass no student if will_be been`
pigro.
`lazy`
'Gianni will pass no student if he goofed off'

(281) *Gianni non promuoverà alcuno studente$_i$ se *pro*$_i$ sarà stato
`Gianni not will_pass any student if will_be been`
pigro.
`lazy`

(282) Gianni non promuoverà alcuno studente se Mario sarà stato
`Gianni not will_pass any student if Mario will_be been`
pigro.
`lazy`
'Gianni will pass no student if Mario goofed off'

Now, the impossibility of (281) clearly depends on the indicated coindexing pattern, as the comparison with (282) immediately shows. This impossibility is particularly telling, since I have already argued that, for independent reasons, *alcuno* is a type I NPI, i.e. it always introduces in the logical form an existential quantifier; hence, it is not surprising if that coindexing pattern results in ungrammaticality, since it has the same syntactic problems of the syntactic representation that we needed in order to get the (also semantically untenable) semantic representation (278) above. This seems to confirm that our syntactic worries were indeed justified. But, moreover, it seems to suggest that items in the *nessuno* series are best understood as type II NPIs, i.e. NPIs which are ambiguous between existential quantifiers under negation and GIs over it. Remember, in fact, that we were wondering if these items are type I, like *alcuno*, or type II NPIs. Now, the grammaticality of (280) with the indicated coindexing pattern rules out an interpretation of *nessuno* as an existential quantifier, for both the syntactic and the semantic reasons just seen, and strongly suggests the viability of a reading where *nessuno* is interpreted as a GI, like *no* in the reading represented by (279).

Note that such a solution to Higginbotham's puzzle and its Italian version would have not been available if, instead of a GI, we had had whatever quantifier (not necessarily an existential one, but also a universal or universal-alike one, i.e. even one semantically closer to GIs): in this case, in fact, the same syntactic problem of (281) and of the alleged syntactic representation leading to logical form (278) would have been reproduced; additionally, there would have been an unpleasant semantic mismatch between the indefinite morpheme in NIs and that in standard indefinites.[56]

7.5 Habituals

A sentence like the following one may be of course understood as expressing a general trend:

(283) Gianni jokes with Luisa.

[56] The idea that NIs bear some kind of reference to a set of possibly abstract, or generic, objects is not new in the literature, since it has been advocated in Geurts (1996). However, Geurts there did not characterize generics as I did, nor even as universal-alike elements.

This is what is also known as an *habitual* reading of (283). Note that (283) has only proper names as DPs: hence, if we want to try to formalize its generic character by means of a generic variable, the only possible candidate to trigger one in the logical form is the temporal argument. This is in accordance (apart, of course, from the issue of quantification) with two related positions usually held in the literature about habitual sentences, namely that they arise as a consequence of generic quantification over time intervals (see Fox (1995: 316) and Giannakidou (1995: 104); but, before them, also McCawley (1981) and Farkas (1992b) for the related view that habituals are intensional operators) and that there is a close correlation between habituality and imperfective aspect (see Bonomi (1995)).[57]

Further, a sentence like (283) can hardly be interpreted as saying that in any interval of time Gianni is actually joking with Luisa. Most likely, it will express the fact that in any relevant interval of time he is so doing: (pragmatic) restriction on the domain of the generic variable is of course in order. Besides, this restriction may be made explicit by employing a *when*-clause, something which strongly confirms that what we are dealing with is a generic temporal argument (remember that, under present assumptions, I take such clauses to be anaphoric elements, looking at a bound or generic variable already present in the discourse to be coindexed with):

(284) Gianni jokes with Luisa {when / whenever} a funny situation creates.

[57] In this regard, it should be pointed out that languages like French (Kleiber (1987)), Czech and Polish (Filip & Carlson (1997)) do also appear to have perfective habituals; however, Klimek-Jankowska (2012) analyzes these cases as instances of kind-reference, thus extending the opposition between two different sorts of genericity already detected within the nominal domain to the verbal one as well.

Without any pretence of accuracy and departing completely from the issue of domain restriction, hence, the semantic representation for (283) may be something along the following simple lines (where the generic variable v in the first argumental place of the predicate $joke'$ is a variable over time intervals):

(285) $joke'(v, Gianni, Luisa)$

Hence, it comes as no surprise that the negation of (283), translated in logical form as (286a), produces a stronger statement than the one that would result from the negation of a universal quantifier:

(286) Gianni does not joke with Luisa.

a. $\neg joke'(v, Gianni, Luisa)$

Informally, (286) means that in any relevant interval of time it is not the case that Gianni jokes with Luisa, not merely that there are some relevant intervals of time in which he does not.

There would be, of course, much more to say on the issue of habituality, but I prefer to move on to an arguably more puzzling issue, namely Neg-Raising, to conclude this overview on the possible applications of RA to natural language semantics.

7.6 Neg-Raising

Neg-Raising is a well-known puzzling linguistic phenomenon involving negation and some verbs selecting a sentential complement (i.e., some intensional verbs): it amounts to the possibility, with such verbs, that preverbal negation is interpreted as if it was embedded within the sentential complement, i.e. as if it had raised from a position within the embedded clause, which it occupied in the deep structure, to gain the position in the matrix where it appears on the surface.

One of the puzzling facts about Neg-Raising is that this phenomenon regards some but not all intensional verbs. This fact is illustrated by the following two series of examples: in (287) we have verbs which do not show Neg-Raising effects, while in (288) there are verbs that do; the list is by no means exhaustive (the exclamative dot in the paraphrases of sentences in the second series signals that a non-Neg-Raising reading is also available for them, but, out of the blue, it seems to be a marked reading):[58]

(287) a. Gianni {can / could / may} not watch the match.
✓ $\neg\Diamond$: 'it is not the case that Gianni {can / could / may} watch the match'
✗ $\Diamond\neg$: 'it is possible that Gianni does not watch the match'

b. Luisa does not {hope / dream} {to watch / that Gianni watches} the match.
✓ ¬Hop: 'it is not the case that Luisa {hopes / dreams} {to watch / that Gianni watches} the match'
✗ Hop¬: 'Luisa {hopes / dreams} {not to / that Gianni does not} watch the match'

c. Luisa does not {say / affirm / assert / declare / guess / maintain / tell} {to have watched / that Gianni watched} the match.

[58]This feature, namely the possibility of having also a non-Neg-Raising reading but associated with some markedness, seems to accomunate all Neg-Raising verbs; see Gajewski (2005: 16).

✓ ¬Say: ‘it is not the case that Luisa {says / affirms / asserts / declares / guesses / maintains / tells} {not to have watched / that Gianni did not watch} the match’

✗ Say¬: ‘Luisa {says / affirms / asserts / declares / guesses / maintains / tells} {not to have watched / that Gianni did not watch} the match’

d. Luisa does not {know / realize / understand} {to have watched / that Gianni watched} the match.

✓ ¬Know: ‘it is not the case that Luisa {knows / realizes / understands} {not to have watched / that Gianni did not watch} the match’

✗ Know¬: ‘Luisa {knows / realizes / understands} {not to have watched / that Gianni did not watch} the match’

e. Luisa does not love {to watch / that Gianni watches} the match.

✓ ¬Lov: ‘it is not the case that Luisa loves {to watch / that Gianni watches} the match’

✗ Lov¬: ‘Luisa loves {not to / that Gianni does not} watch the match’

(288) a. Gianni {must / should} not watch the match.

! $\neg\Box$: ‘it is not the case that Gianni {must / should} watch the match’

✓ $\Box\neg$: ‘it is necessary that Gianni does not watch the match’

b. Gianni does not {seem / appear} to watch the match.

! ¬Seem: ‘it is not the case that Gianni {seems / appears} to watch the match'

✓ Seem¬: 'Gianni {seems / appears} not to watch the match'

c. Luisa does not {want / desire / wish} {to watch / that Gianni watches} the match.

! ¬Want: 'it is not the case that Luisa {wants / desires / wishes} {to watch / that Gianni watches} the match'

✓ Want¬: 'Luisa {wants / desires / wishes} {not to / that Gianni does not} watch the match'

d. Luisa does not {believe / expect / suppose / think} {to watch / that Gianni watches} the match.

! ¬Bel: 'it is not the case that Luisa {believes / expects / supposes / thinks} {not to / that Gianni does not} watch the match'

✓ Bel¬: 'Luisa {believes / expects / supposes / thinks} {not to / that Gianni does not} watch the match'

e. Luisa does not like {to watch / that Gianni watches} the match.

✓ ¬Lik: 'it is not the case that Luisa likes {to watch / that Gianni watches} the match'

✗ Lik¬: 'Luisa likes {not to / that Gianni does not} watch the match'

The contrast between non-Neg-Raising and Neg-Raising readings also appears with modal adjectives in predicative position:

(289) a. It is not possible that Gianni watches the match.

✓ $\neg\Diamond$: 'it is not the case that Gianni can watch the match'

✗ $\Diamond\neg$: 'it is possible that Gianni does not watch the match'

b. It is not X-ble that Gianni watches the match.[59]

✓ $\neg\Diamond$: 'it is not the case that one can X that Gianni watches the match'

✗ $\Diamond\neg$: 'it is X-ble that Gianni does not watch the match'

c. It is not necessary that Gianni watches the match.

✓ $\neg\Box$: 'it is not the case that Gianni must watch the match'

✗ $\Box\neg$: 'it is necessary that Gianni does not watch the match'

(290) It is not convenient that Gianni watches the match.

! $\neg\Box$: 'it is not the case that it is convenient that Gianni watches the match'

✓ $\Box\neg$: 'it is convenient that Gianni does not watch the match'

Theories attempting to explain the phenomenon of Neg-Raising essentially fall into three major groups: syntactic, semantic and pragmatic theories. Syntactic theories have been the first ones to be proposed: Fillmore, in particular, argued that Neg-Raising is a movement rule, since it has the appearance of being cyclic; Fillmore's proposal, however, has been strongly criticized on empirical grounds, especially by Horn (1972); see also Gajewski (2005: §2.1.6).

Another interesting insight about Neg-Raising comes from Kiparsky & Kiparsky (1970), who argued that factive verbs never obey Neg-Raising effects, and further offer a syntactic explanation for this fact by postulating that factive complements are embedded under a (possibly silent) nominal *fact*.

[59] X stands for a propositional attitude verb like *think*, *desire*, *conceive*, etc., provided, of course, the necessary phonological adaptations.

Semantic accounts of Neg-Raising stem from Bartsch (1973) and probably find their most representative recent advocate in Gajewski (2005, 2007) (relying also on Heim (2000)): in a nutshell, the idea behind these semantic accounts is that Neg-Raising verbs come with a (lexically encoded, since Heim (2000)) presupposition of Excluded Middle, formally represented in Gajewski (2005: 14) in the following terms (where NRP is the semantic representation of a Neg-Raising predicate and S that of its sentential complement):[60]

(292) $\mathrm{NRP(S)} \vee \mathrm{NRP}(\neg \mathrm{S})$.

Once again, this is the same kind of presupposition advocated by Fodor (1979) and Löbner (2000) to account for the behaviour of plural definites (p. 80) and, in the case of the latter, also GIs (p. 182) with respect to negation. As I said before, I take the objections raised when discussing the first case to extend also to the one under examination here.

Finally, I will say only a few words about the chief pragmatic account of Neg-Raising appearing in the literature, namely Horn's (1989: §5.2). Horn's idea is essentially that Neg-Raising readings arise as R-implicatures (see Appendix A for a definition) with some intensional predicates. Gajewski (2005) presents some arguments against Horn's account; a quite compelling one hits pragmatic accounts of Neg-Raising in general and is based on the empirical observation that there are cases, within one same language, of predicates sharing more or less the same

[60] Actually, there is a material error in Gajewski's (2005: 14) original formulation of this presupposition, since it appears as the following (291), i.e. as an instance of the Law of Excluded Middle, which is of course not in question and does not need to be added as a presupposition:

(291) $\mathrm{NRP(S)} \vee \neg \mathrm{NRP(S)}$.

meaning but such that one is Neg-Raising while the other is not, and also cases of a predicate which is Neg-Raising in a language while its (closest) translation in another language is not. The following couples are taken from Gajewski (2005: 90) (with the exception of the Italian one, which exactly parallels the French one, and the one comparing It. *amare* with Eng. *love*):

	Neg-Raiser	**Non Neg-Raiser**	
Eng.	*suppose* *expect*	*guess* *anticipate*	Eng.
French	*souhaiter* 'hope'	*expérer* 'hope'	French
It.	*augurarsi* 'hope'	*sperare* 'hope'	It.
Hebrew	*xosev* 'think'	*maamin* 'believe'	Hebrew
German	*hoffen* 'hope'	*hope*	Eng.
It.	*amare* 'love'	*love*	Eng.

Table 7.2: Neg-Raiser and Non Neg-Raiser verbs in some languages.

In this regard, furthermore, the contrast between (288a) and (290) above, repeated here as (293) and (294) respectively, is particularly telling, since the verb and the adjective seem to share essentially the same meaning:

(293) Gianni must not watch the match.

! $\neg\Box$: 'it is not the case that Gianni {must / should} watch the match'

✓ $\Box\neg$: 'it is necessary that Gianni does not watch the match'

(294) It is not necessary that Gianni watches the match.

✓ $\neg\Box$: 'it is not the case that Gianni must watch the match'

✗ $\Box\neg$: 'it is necessary that Gianni does not watch the match'

One should observe, however, that the etymology of *necessary* makes it derive from a structure of the type *not-possible*, hence it is not surprising if its negation amounts to something of the type *possible-not*; this is what we find in Klein (1966-7):

> **necessary,** adj., certain to happen, inevitable, requisite. — ME. *necessarie*, fr. L. *necessārius*, 'unavoidable, indispensable, necessary', fr. *necesse*, 'unavoidable, necessary', which stands for **ne-cezd-tis*, **ne-cēd-tis*, lit. '(there is) no evasion, (it is) inevitable', fr. negative pref. *ne-* and *cēdere*, 'to go away, yield'. For the first element see **no,** adj., and cp. **nay,** for the second see **cede,** for the ending see adj. suff. **-ary**

Within the tradition of modern philosophical logic and analytic philosophy, the subclass of intensional verbs which has attracted scholars first was that of modal predicates. At least since Carnap (1946) (his ideas were further articulated in Kanger (1957), Prior (1957), Montague (1960) and, moreover, Kripke (1963); see also Barcan (1946) and Carnap (1947)),[61] the standard way to express modal notions has been through quantification over accessible worlds or analogous abstract objects (here, as I did before when discussing conditionals, I will also speak of situations, instead of worlds, but the specific ontological preferences do not matter for present purposes). In particular, necessity has been understood to mean holding in all accessible situations, while possibility holding in some possible situations.

Hintikka (1962) also extended this approach to other intensional predicates. Hence, the notion of *modal basis* came to play a central role in this respect, since

[61] To take a look at the first modern insights on the semantics of modal operators, see MacColl (1880), Lewis (1920), Becker (1930) and Lewis & Langford (1932).

belief has been analyzed in terms of universal quantification over worlds accessible from a *doxastic* modal basis relative to the bearer of the belief, will has been analyzed in terms of universal quantification over worlds accessible from a *bouletic* modal basis relative to the bearer of the will, human laws have been analyzed in terms of universal quantification over worlds accessible from a *deontic* modal basis relative to a certain collection of norms, natural laws in terms of universal quantification over worlds accessible from an *epistemic* modal basis relative to a certain amount of perceived evidence, logical and mathematical laws in terms of universal quantification over worlds accessible from an *alethic* modal basis, etc.[62] Even if analyses of this kind have usually been developed under assumptions rejecting the sententialist approach to dependent clauses I endorsed before, recasting them in sententialist terms does not give rise, as far as I can see, to any principled difficulties.

The essential idea behind the present proposal on Neg-Raising is quite simple (and, at this point, maybe easy to figure out): in the case of Neg-Raising verbs, the traditional analysis in terms of universal quantification over situations or accessible worlds should be replaced by an analysis in terms of generic variables ranging over situations; in the case of non-Neg-Raising verbs, on the other hand, existential quantification would be at work and, hence, negation could take scope over the situation argument.

Let's see how this idea can work for some particular cases of both sentences containing non-Neg-Raising verbs and sentences containing Neg-Raising ones (a

[62]As this largely incomplete list suggests, universal force in the quantification over accessible worlds of various kinds has been advocated in a predominant way. However, there are robust exceptions to this trend, represented, to name but a few, by predicates expressing possibility, as I said before, or predicates introducing indirect discourse, as we will see in a while.

situation argument has been added here to the logical forms of verbs; I believe it likely that such an argument coincides with the temporal one, even if the proposal is also consistent with different assumptions):

(295) a. Gianni cannot watch the match.

(i) not [(some x: x is a situation {deontically / epistemically} accessible from t) [there is a proof of length at most $f(x)$ of $\ulcorner$[[Gianni watches the match in x]]$\urcorner$]]

(ii) $\neg(\exists x \leq t)(Acc_{\{\text{Deont/Ep}\}}(x,t) \wedge Proof(x, \ulcorner watch'(Gianni, the\_match, x)\urcorner(Gianni, the\_match, x)))$

b. Luisa did not say that Gianni watched the match.

(i) not [Luisa said $\ulcorner$[[Gianni watched the match]]$\urcorner$]

(ii) $\neg said'(\ulcorner watch'(Gianni, the\_match)\urcorner(Gianni, the\_match))$

c. Luisa does not know that Gianni watched the match.

(i) not [Luisa has a proof of length at most $f(t)$ of $\ulcorner$[[Gianni watched the match]]$\urcorner$]

(ii) $\neg(\exists x \leq t)(proof'(x, \ulcorner watch'(Gianni, the\_match)\urcorner(Gianni, the\_match)) \wedge have'(Luisa, x, t)))$

(296) a. Gianni must not watch the match.

(i) (a$_{Gen}$ w: w is a situation {deontically / epistemically} accessible from t) [not [there is a proof of length at most $f(w)$ of $\ulcorner$[[Gianni watches the match in w]]$\urcorner$]]

(ii) $Acc_{\{\text{Deont/Ep}\}}(w,t) \rightarrow \neg Proof(w, \ulcorner watch'(Gianni, the\_match, w)\urcorner(Gianni, the\_match, w))$

b. Luisa does not want that Gianni watches the match.

(i) (a$_{Gen}$ w: w is a situation bouletically accessible to Luisa) [not [Luisa has a proof of length at most $f(w)$ of $\ulcorner$[[Gianni watches the match in w]]$\urcorner$]]

(ii) $Acc_{\text{Boul}}(w, Luisa) \rightarrow \neg(\exists x \leq t)(proof'(x, \ulcorner watch'(Gianni, the\_match, w)\urcorner(Gianni, the\_match, w)) \wedge have'(Luisa, x, t))$

c. Luisa does not believe that Gianni watched the match.

(i) (a$_{Gen}$ w: w is a situation doxastically accessible from t) [not [Luisa has a proof of length at most $f(w)$ of $\ulcorner$[[Gianni watches the match in w]]$\urcorner$]]

(ii) $Acc_{\text{Dox}}(w, Luisa) \rightarrow \neg(\exists x \leq t)(proof'(x, \ulcorner watch'(Gianni, the\_match, w)\urcorner(Gianni, the\_match, w)) \wedge have'(Luisa, x))$

Of course, the logical forms above could (actually, should) be refined under several respects; however, they formally give substance to the idea I mentioned before, namely that Neg-Raising verbs differ from non-Neg-Raising ones because they contain a lexically encoded generic variable ranging over accessible situations (or similar). Note that in the logical forms in (296), i.e. the logical forms of sentences with Neg-Raising verbs, negation could not have been inserted in a higher position without violating the BNCS: this is, again, the syntactic constraint I claim to be at the basis of the attested semantic effects.

There is another consequence of an analysis of Neg-Raising verbs along these lines which I consider to be quite nice: is the fact that it posits a distinction on truth-conditional grounds between sentences containing a Neg-Raising predicate and the corresponding ones with the negation appearing within the embedded clause. I will illustrate this point with the verb *believe*, repeating here (296c) as

(297a) and comparing it with (297b):

(297) a. Luisa does not believe that Gianni watched the match.

(i) $Acc_{\mathrm{Dox}}(w, Luisa) \rightarrow \neg(\exists x \leq t)(proof'(x, \ulcorner watch'(Gianni,$ $the\_match, w)\urcorner(Gianni, the\_match, w)) \wedge have'(Luisa, x))$

b. Luisa believes that Gianni did not watch the match.

(i) $Acc_{\mathrm{Dox}}(w, Luisa) \rightarrow (\exists x \leq t)(proof'(x, \ulcorner \neg watch'(Gianni,$ $the\_match, w)\urcorner(Gianni, the\_match, w)) \wedge have'(Luisa, x))$

The condition expressed by (297b-i) is actually stronger than the one expressed by (297a-i), since, for the former to be true, it suffices that, for any situation which Luisa is ready to acknowledge as possibly corresponding to the actual state of affairs, she does not have any proof that Gianni watched the match in that situation: it is not necessary, even if plausible, that for all and every situation (and not even necessary for a single one) Luisa has at her disposal any proof that Gianni did not watch the match in that situation, something which corresponds to the meaning conveyed by (297b-i). It seems to me that this distinction reflects indeed a real intuitive semantic difference between (297a) and (297b); besides, this difference would remain slight enough to account for that closeness in meaning ultimately justifying the label of “Neg-Raising” itself.

Chapter 8

Conclusions

8.1 Summary

This dissertation has been structured in two major parts. In the first one, I gave a brief outline of the philosophical background of finitism, a peculiar philosophical view on mathematics which rejects the idea of an actual infinite and which, at least in its most coherent derivations, also views traditional unbounded quantification over infinite sets as meaningless. Then, I moved on to describe a formal system which can be viewed as a foundation for arithmetic built on finitist assumptions, namely (primitive) recursive arithmetic (RA); I further provided a version of RA incorporating a special symbol which I anticipated to be required in order to establish the "anchoring" of semantic representations to the actual world once we had applied RA to the modelization of meanings in natural language. I stressed the *a priori* appeal of RA (especially when compared with systems of arithmetic based on first order logic), which consists of theoretically desirable well-recognized features of simplicity and elegance, accompanying themselves with expressive richness and flexibility.

In the second part, I first addressed the general problem of building a semantic theory for natural languages, emphasizing the fact that one common feature shared by all different approaches to natural language meaning on the market has been that of employing some (more or less) formal semantic representations with which to translate natural language sentences. Moreover, under the reasonable assumption that differences in entailment relations with other sentences and different truth conditions should also bring together differences in meaning, all semantic theories (at least implicitly) acknowledge the relevance, for the empirical investigation, of defining a formal notion of "entailment" between those semantic representations. I argued that these rather uncontroversial assumptions suffice to make the use of RA as the background system for building semantic representations in linguistic semantics *a priori* worthy of being tested, in place of standard widespread systems ultimately based on classical first order logic (CL). I also argued that the key features distinguishing RA from CL were the absence of unbounded quantifiers (which a sound philosophical interpretation of RA rejects as lacking a proper counterpart in human reasoning and natural language as well) and the presence of generic variables, i.e. variables which are always free and trigger the application of the rule of inference of Particularization and, for this reason, can be partly assimilated to widest scope universal quantifiers in CL. As a consequence of that, I identified as the main goal of this dissertation that of showing the existence of some linguistic constructions, possibly across different languages, which should undergo a semantic analysis involving generic variables instead of classically conceived universal quantification in order to get a more natural mapping from syntax to semantics.

Before concretely exploring this possibility, I defined a notion of entailment in

RA terms corresponding to the intuitive one; in order to do this in a straightforward way, I assumed, following much literature, the existence of the illocutionary operator 'ASSERT'. Soon after, I moved on to some rough semantic descriptions of many different linguistic categories and expressions, for which I proposed some semantic representations in RA terms, which I globally qualified as "neutral" insofar as they do not show any relevant difference between a formalization in RA or CL terms. I also discussed some syntactic features regarding some expressions I considered: in particular, aligning myself with observations previously made in the literature, I formulated a ban on negation taking scope over coordinate structures under unmarked readings (BNCS). I also gave an account of dependent clauses, by extending the fundamental insights proper of the sententialist approach to complements of attitude reports: by endorsing the sententialist standpoint, after having argued in its favour on independent grounds, I thus provided myself with a principled motivation for an apparent counterexample threatening the validity of the account I was going to develop in the subsequent part (being in a position to explain the effect of apparently having embedded generic variables in the scope of other operators), moreover in a way seemingly in accordance with some central notions of recent generative syntactic theory.

Then, I moved to the core of the dissertation, by discussing those linguistic phenomena and puzzles I took to provide empirical evidence in favour of my claim about the superiority of RA over CL as a background formal system for natural language semantics. First of all, I set aside some classes of data previously discussed in the literature to defend related claims, since they had already been attacked with apparently sound arguments. Once this was done, I took into account the relevant empirical data, addressing such topics as generic indefinites (for which I strongly relied upon Goodstein (1951, 1957), Hornstein (1984) and Löbner (2000, 2013)), conditionals, negative indefinites, habituals and Neg-Raising. Essentially, I provided arguments of two general sorts: arguments based on the interaction with sentential negation (bringing into play the previously stated BNCS) and arguments based on anaphora (bringing into play the CSC). In both cases, thus, I felt entitled to derive conclusions on the general format of semantic representations by making reference to some (minimal and rather uncontroversial) assumptions of syntactic nature. I maintained the account of the phenomena considered to be superior to standard ones already pursued in the literature: to quantificational ones (partly with arguments borrowed from Löbner (2000)) and to those based on the Presupposition of Excluded Middle, which essentially I took to be an *ad hoc* solution.

8.2 Open problems

There are, of course, plenty of problems left unresolved by the present account. I take the following to be the most prominent ones:

a) even if offering a (unified) explanation to several still puzzling linguistic facts, the account I developed is based on some syntactic assumptions (despite them being quite minimal) which do not find an immediate correlation with features of the formal system I employed. A more in-depth investigation of the possible relations between those syntactic constraints and the language of $\mathcal{RA}^t$, arguably also relying on independently motivated cognitive assumptions about computational efficiency in human reasoning, would be strongly desirable in order to perhaps add plausibility to the main claim of this dissertation;

b) since, under the account just offered, clausal boundaries play a crucial role in the explanation of some fine-grained phenomena involving generic variables in the semantic representation, a much more detailed account of CP should be provided, in order to carefully test predictions of the present theory and compare them with those of its competitors; particular attention should be payed to an analysis of those CPs not displaying an overt complementizer (as with focus, focalizing adverbs and topic);

c) the range of empirical data covered here could hopefully be extended: all natural language constructions displaying some correlations with modality (just to mention but a few, future tense and imperative mood) are possible candidates for a treatment based on the availability of elements with features like those of generic variables in RA.

8.3 Interdisciplinary perspectives

As I have already said, the empirical data discussed in this dissertation is still logically compatible with a semantic account where semantic representations are logical forms in an extension of CL with free variables displaying a universal-alike inferential behaviour. However, there would be no motivation at all behind a system of this kind, given that universal quantifiers, as they are characterized in CL, already cover the same functions that those free variables would play; hence, we would need an explanation of why natural languages having determiners and possibly other syntactic tools devoted to expressing universal quantification crosslinguistically also employ syntactically distinguished forms to mark elements corresponding in the semantic representation to wide scope universal quantifiers.

Reversing the argument, if natural languages crosslinguistically display expressions and constructions of this kind, it is plausible that they need them in order to convey meanings which they could not otherwise convey. In other words, if the picture just outlined is on the right track, it seems to provide an argument in favour of the view that human reasoning can be successfully modelled through (some version of) RA and that the philosophical standpoint about the nature of mathematical entities related to it, namely finitism, is presupposed by any proper foundation of mathematics itself. This should contribute to keeping alive an interest towards a philosophical view which was commonly shared, implicitly or not, by most mathematicians until the second half of the Nineteenth century. Hence, not only linguistics, and natural language semantics in particular, would benefit from achievements and insights coming from the philosophy of mathematics, as has happened, through the mediation of logic and philosophy of language, for a long time: it also could provide evidence to favour or disfavour specific philosophical theories about mathematics and reasoning, moreover on solid empirical grounds; and this would be, I believe, a welcome result.

Appendix A

Quantity implicatures

The most important achievement of gricean pragmatic is probably the explanation in terms of so-called *quantity implicatures* of some linguistic phenomena which previously were only accounted for in terms of semantic ambiguity (see Geurts (2010) for a recent overview on this issue).

The first explicit formulation of an argument based on a quantity implicature was given by the Nineteenth century logician John Stuart Mill. This is a passage of his (from Mill (1865: 501)) quoted in Horn (1989: 212):

> If I say to any one, 'I saw some of your children to-day', he might be justified in inferring that I did not see them all, not because the word mean it, but because, if I had seen them all, it is most likely that I should have said so: even though this cannot be presumed unless it is presupposed that I must have known whether the children I saw were all or not.

Grice's first formulation of the pragmatic principle which lies at the base of Mill's argument has been given in Grice (1961), from which the following passage, which anticipates the famous Quantity maxim, is taken:

> [O]ne should not make a weaker statement rather than a stronger one unless there is a reason for so doing.

"[R]eason[s] for so doing" amount especially, if not exclusively, to reasons connected with the notion of *relevance*, one central notion of all pragmatics. The following quotation from Suppes (1973) is likely to show in a very immediate manner the importance of this notion in the regulation of everyday discourse:

> Suppose Gianni and Luisa are walking and Gianni notices a spider close to Luisa's shoulder. He says "Watch out for the spider". He does not say, "Watch out for the black, half-inch long spider that has a green dot in its centre and is about six inches from your left shoulder at a vertical angle of about sixty degrees."

In the classic framework given in Grice (1975), relevance alone is responsible for one conversational maxim, i.e. the maxim of Relation. However, as Horn pointed out in Horn (1984), some other gricean principles can arguably be reduced, at least in part, to that. Horn himself, in this way, offered a highly parsimonious pragmatic framework to derive conversational implicatures, entirely based on the following two simple principles:

(298) *Q Principle*

Contribute as much as you can [given R].

(299) *R Principle*

Contribute no more than you must [given Q].

This formulation, as stated, is clearly affected by circularity, and so it always leads to clashes. In order to avoid this consequence, we can reasonably rely on the following suggestion by Huang (in Huang (2007)), which permits the assignment of a priority ordering between the two principles (in particular by omitting the part between square brackets in the formulation of R):[1]

> [T]he R-principle generally takes precedence until the use of a contrastive linguistic form induces a Q-implicature to the non-applicability of the pertinent R-implicature [. . .].

Once we have obtained a sufficiently satisfactory formulation of these general pragmatic principles, through them we can explain some widespread inferences that one usually makes when he is the addressee of the utterances of certain sentences. Suppose in fact that you are the addressee of an utterance of sentence α and that there is a sentence β which, in the current context, implicates α while α does not implicate β; in other words, in the given context, β is more informative than α. So, if 1) β would be relevant whenever true, 2) you have at your disposal enough evidence to maintain that the speaker knows if β is true or not, and 3) you can maintain that the speaker is willing to be cooperative, i.e. to mould his communicative behavior to something along the lines of Q and R, then you can infer from the utterance of α, in the given circumstances, that β is false. In gricean terms, you can derive $\neg\beta$ from α as an implicature, which means that, even if it is not an entailment of α, it is nevertheless conveyed by it in the given circumstances. So, in these circumstances, α and $\alpha \wedge \neg\beta$ are pragmatically, even if not semantically, equivalent.

[1] Note, however, that this priority order is partly, even if not completely, implicit in Levinson's influential schema to derive quantity implicatures, given in Levinson (1983).

Appendix B

Examples of derivations in recursive arithmetic

B.1 Derivation-schemata of general utility

In this section, I wish to introduce some derivation-schemata which can be useful for reducing the length of derivations. I will treat, in fact, these derivation-schemata as if they were new rules of inference, labelling them with names which are written between angle brackets, in order to distinguish them from true rules of inference.

Theorem B.1 $\alpha \vdash_{\mathcal{PR}} A = A$.

$$\dfrac{(F_0)\,\dfrac{\alpha}{A+0=A} \qquad (F_0)\,\dfrac{\alpha}{A+0=A}}{A=A}\,(T) \qquad \langle SI \rangle$$

Theorem B.2 $A = B \vdash_{\mathcal{PR}} B = A$.

$$\dfrac{A=B \qquad \langle SI \rangle\,\dfrac{\top}{A=A}}{B=A}\,(T) \qquad \langle K \rangle$$

Theorem B.3 $\varphi(A/B) = \psi(A/B), B = C \vdash_{\mathcal{PR}} \varphi(A/C) = \psi(A/C)$.[1]

$$\dfrac{\dfrac{\varphi(A/B)=\psi(A/B) \qquad (S)\,\dfrac{B=C}{\varphi(A/B)=\varphi(A/C)}}{\psi(A/B)=\varphi(A/C)}\,(T) \qquad (S)\,\dfrac{B=C}{\psi(A/B)=\psi(A/C)}}{\varphi(A/C)=\psi(A/C)}\,(T) \qquad \langle R_l \rangle$$

Theorem B.4 $\varphi(A/C) = \psi(A/C), B = C \vdash_{\mathcal{PR}} \varphi(A/B) = \psi(A/B)$.

$$\dfrac{\varphi(A/C)=\psi(A/C) \qquad \langle K \rangle\,\dfrac{B=C}{C=B}}{\varphi(A/B)=\psi(A/B)}\,\langle R_l \rangle \qquad \langle R_r \rangle$$

If in a derivation we apply $\langle R_l \rangle$ to the premises $A = B$ and $B = C$ to obtain $A = C$ (this is the particular case of the *Cut* rule; see n. 1), we could obtain an equivalent derivation (i.e., one establishing the same '$\vdash_{\mathcal{PR}}$' relation) by inverting the order of the two subtrees having the two premises as their roots and applying $\langle R_r \rangle$ instead of $\langle R_l \rangle$; in these cases, I will always choose the $\langle R_l \rangle$ option.

[1] The important rule of inference usually known under the name of *Cut* arises, in the present system, as the particular instantiation of this schema with $\psi(A/B) = B$.

Theorem B.5 $\phi(A/[B\ \varphi(p)]^p 0) \vdash_{\mathcal{PR}} \phi(A/B)$.

$$\langle R_l \rangle \frac{\phi(A/[B\ \varphi(p)]^p 0) \qquad (F_0)\dfrac{\phi(A/[B\ \varphi(p)]^p 0)}{[B\ \varphi(p)]^p 0 = B}}{\phi(A/B)} \qquad \langle F_0 R_l \rangle$$

Theorem B.6 $\phi(A/[B\ \varphi(p)]^p SC) \vdash_{\mathcal{PR}} \phi(A/\varphi(p/[B\ \varphi(p)]^p C))$.

$$\langle R_l \rangle \frac{\phi(A/[B\ \varphi(p)]^p SC) \qquad (F_S)\dfrac{\phi(A/[B\ \varphi(p)]^p SC)}{[B\ \varphi(p)]^p SC = \varphi(p/[B\ \varphi(p)]^p C)}}{\phi(A/\varphi(p/[B\ \varphi(p)]^p C))} \qquad \langle F_S R_l \rangle$$

B.2 A proof in $\mathcal{PR}$

Here and in the following, I will employ the usual abbreviations for numbers; i.e., $1 := S0$, $2 := S1 = SS0$, etc. The following schema, which closely reflects standard computational practice, can easily be transformed into a proper derivation of $\mathcal{PR}$ given these conventions:

- if A is the term appearing in the first row, then the first row corresponds to the following derivation-schema:

$$\langle SI \rangle \frac{\alpha}{A = A}$$

- if A is the term appearing in the first row, then the following correspondence holds (where $\langle F_x R_l \rangle$ is either $\langle F_0 R_l \rangle$ or $\langle F_S R_l \rangle$):

$$\begin{array}{l} = B = \\ = C \end{array} \qquad := \qquad \langle F_x R_l \rangle \frac{A = B}{A = C}$$

- rule $\langle F_x R_l \rangle$ is always applied to the leftmost possible function.

Theorem B.7 $\alpha \vdash_{\mathcal{PR}} 4 \cdot 1 + 2 \cdot 3 = 10$.

$$
\begin{aligned}
& 4 \cdot 1 + 2 \cdot 3 = \\
= \ & 3 \cdot 0 + 4 + 2 \cdot 3 = \\
= \ & 0 + 4 + 2 \cdot 3 = \\
= \ & S(0+3) + 2 \cdot 3 = \\
= \ & SS(0+2) + 2 \cdot 3 = \\
= \ & SSS(0+1) + 2 \cdot 3 = \\
= \ & SSSS(0+0) + 2 \cdot 3 = \\
= \ & 4 + 2 \cdot 3 = \\
= \ & 4 + (2 \cdot 2 + 2) = \\
= \ & 4 + (2 \cdot 1 + 2 + 2) = \\
= \ & 4 + (2 \cdot 0 + 2 + 2 + 2) = \\
= \ & 4 + (0 + 2 + 2 + 2) = \\
= \ & 4 + (S(0+1) + 2 + 2) = \\
= \ & 4 + (SS(0+0) + 2 + 2) = \\
= \ & 4 + (2 + 2 + 2) = \\
= \ & 4 + (S(2+1) + 2) = \\
= \ & 4 + (SS(2+0) + 2) = \\
= \ & 4 + (4 + 2) = \\
= \ & 4 + S(4+1) = \\
= \ & 4 + SS(4+0) = \\
= \ & 4 + 6 = \\
= \ & S(4+5) = \\
= \ & SS(4+4) = \\
= \ & SSS(4+3) = \\
= \ & SSSS(4+2) = \\
= \ & SSSSS(4+1) = \\
= \ & SSSSSS(4+0) = \\
= \ & 10
\end{aligned}
$$

B.3 A proof of the commutativity of sum in $\mathcal{RA}$

In this section, I will establish a few further derivation-schemata, the last one of which corresponds to a proof of the well-known commutativity property of the sum operation. Differently from the schemata established in §B.1 above, however, in these an essential role is played by some particular recursive functions. To distinguish the latter from the former, then, I will write their labels within square brackets, instead of angle ones.

Lemma B.1 (Goodstein's (1954: 249) (6)) $\alpha \vdash_{\mathcal{RA}} 0 + A = A$.

$$(U)\,\dfrac{(P)\,\dfrac{(F_0)\,\dfrac{\alpha}{A+0=0}}{0+0=0} \qquad (P)\,\dfrac{(F_S)\,\dfrac{\alpha}{A+SB=S(A+B)}}{0+SB=S(0+B)} \qquad \langle SI\rangle\,\dfrac{\alpha}{SA=SA}}{0+A=A} \qquad [B.1]$$

Lemma B.2 $\alpha \vdash_{\mathcal{RA}} SA = A + 1$.

$$(T)\,\dfrac{(S)\,\dfrac{(F_0)\,\dfrac{\alpha}{A+0=A}}{S(A+0)=SA} \qquad \langle K\rangle\,\dfrac{(F_S)\,\dfrac{\alpha}{A+1=S(A+0)}}{S(A+0)=A+1}}{SA=A+1} \qquad [B.2]$$

Lemma B.3 (Goodstein's (1954: 250) (7)) $\alpha \vdash_{\mathcal{RA}} A + SB = SA + B$.

$$(U)\,\dfrac{(T)\,\dfrac{[B.2]\,\dfrac{\alpha}{SA=A+1} \qquad \langle K\rangle\,\dfrac{(F_0)\,\dfrac{\alpha}{SA+0=SA}}{SA=SA+0}}{A+1=SA+0} \qquad (P)\,\dfrac{(F_S)\,\dfrac{\alpha}{A+SB=S(A+B)}}{A+SSB=S(A+SB)} \qquad (P)\,\dfrac{(F_S)\,\dfrac{\alpha}{A+SB=S(A+B)}}{SA+SB=S(SA+B)}}{A+SB=SA+B} \qquad [B.3]$$

Lemma B.4 $\alpha \vdash_{\mathcal{RA}} SA + B = S(A + B)$.

$$
\dfrac{[B.3]\ \dfrac{\alpha}{A + SB = SA + B} \qquad (F_S)\ \dfrac{\alpha}{A + SB = S(A + B)}}{SA + B = S(A + B)}\ (T) \qquad [B.4]
$$

Theorem B.8 (Goodstein's (1954: 18) (8)) $\alpha \vdash_{\mathcal{RA}} A + B = B + A$.

$$
\dfrac{\dfrac{\langle S \rangle\ \dfrac{(F_0)\ \dfrac{\alpha}{A + 0 = A}}{A = A + 0} \qquad [B.1]\ \dfrac{\alpha}{0 + A = A}}{A + 0 = 0 + A} \qquad [B.4]\ \dfrac{(F_S)\ \dfrac{\alpha}{A + SB = S(A + B)}}{SA + B = S(A + B)}}{A + B = B + A}\ (U) \qquad [B.8]
$$

Bibliography

Abner, Natasha & Jason Bishop (eds.). 2008. *Proceedings of WCCFL*. 27 Somerville, MA: Cascadilla Press.

Abney, Steven P. 1987. *The English Noun Phrase in its Sentential Aspect*. Cambridge, MA: MIT dissertation.

Abraham, Werner & Sjaak de Meij (eds.). 1986. *Topic, Focus, and Configurationality. Papers from the 6th Groningen Grammar Talks, Groningen, 1984*. Amsterdam: John Benjamins.

Abusch, Dorit & Mats Rooth. 2004. Empty-domain effects for presuppositional and non-presuppositional determiners. In Kamp & Partee (2004), 7–27.

Adams, Ernest. 1970. Subjunctive and indicative conditionals. *Foundations of Language* 6. 89–94.

Aguilar Guevara, Ana, Anna Chernilovskaya & Rick Nouwen (eds.). 2012. *Proceedings of Sinn und Bedeutung*. 16 Cambridge, MA: MIT Working Papers in Linguistics.

Alexiadou, Artemis, Liliane Haegeman & Melita Stavrou. 2007. *Noun Phrase in the Generative Perspective*. Berlin & New York: Mouton de Gruyter.

Allen, Robert Livingston. 1966. *The Verb System of Present-Day American English*. The Hague: Mouton.

Aloni, Maria, Vadim Kimmelman, Floris Roelofsen, Galit Weidman Sassoon, Katrin Schulz & Matthijs Westera (eds.). 2012. *Proceedings of the 18th Amsterdam Colloquium*. Amsterdam: Springer.

Alonso-Ovalle, Luis & Paula Menéndez-Benito. 2003. Some epistemic indefinites. In Kadowaki & Kawahara (2003), 1–12.

Alonso-Ovalle, Luis & Paula Menéndez-Benito. 2008. Minimal domain widening. In Abner & Bishop (2008), 36–44.

Alonso-Ovalle, Luis & Paula Menéndez-Benito. 2010. Modal indefinites. *Natural Language Semantics* 18(1). 1–31.

Alonso-Ovalle, Luis & Paula Menéndez-Benito. 2011. Domain restrictions, modal implicatures and plurality: Spanish *algunos*. *Journal of Semantics* 28(2). 211–40.

Alonso-Ovalle, Luis & Paula Menéndez-Benito. 2013. Indefinites, dependent plurality, and the viability requirement on scalar alternatives. *Journal of Semantics* 30(1). 65–102.

Altmann, Gerry & Mark Steedman. 1988. Interaction with context during human sentence processing. *Cognition* 30. 191–238.

Ambrose, Alice & M. Lazerowitz (eds.). 1972. *Ludwig Wittgenstein: Philosophy and Language*. London: Allen and Unwin.

AnderBois, Scott, Adrian Brasoveanu & Robert Henderson. 2012. The pragmatics of quantifier scope: a corpus study. In Aguilar Guevara et al. (2012), 15–28.

Anderson, Alan Ross. 1951. A note on subjunctives and counterfactual conditionals. *Analysis* 11. 35–8.

Anderson, Catherine. 2004. *The Structure and Real-Time Comprehension of Quantifier Scope Ambiguity*. Evanston, IL: Northwestern University dissertation.

Anderson, Stephen R. & Paul Kiparsky (eds.). 1973. *A Festschrift for Morris Halle*. New York: Holt, Rinehart, and Winston.

Andrews, Avery. 1982. The representation of case in Modern Icelandic. In Bresnan (1982b), 427–503.

Aristotle. 1831. *Aristotelis Opera*, vol. 1. Berlin: G. Reimerum. Edited by Immanuel Bekker.

Aristotle. 1984. *Complete Works of Aristotle*, vol. 1. Princeton, NJ: Princeton University Press edited by jonathan barnes. edn.

Ashton, N., A. Chereches & D. Lutz (eds.). 2011. *Proceedings of SALT*. 21 Ithaca, NY: Cornell University Press.

Aspray, William & Philip Kitcher (eds.). 1988. *History and Philosophy of Modern Mathematics*. Minneapolis: University of Minnesota Press.

Axt, Paul. 1963. Iteration of primitive recursion. *Notices of the American Mathematical Society* 10. 113. Abstract 597-182.

Axt, Paul. 1965. Iteration of primitive recursion. *Zeitschrift für mathematische Logik und Grundlagen der Mathematik* 11. 253–5.

Bach, Emmon, E. Jelinek, Angelika Kratzer & Barbara H. Partee (eds.). 1995. *Quantification in Natural Languages*. Dordrecht: Kluwer.

Baker, Carl Lee. 1978. *Introduction to Generative-Transformational Syntax*. Englewood Cliffs, NJ: Prentice-Hall.

Bar-Hillel, Yeoshua (ed.). 1964. *Proceedings of the International Congress for Logic, Methodology, and the Philosophy of Science*. Amsterdam: North-Holland.

Barcan, Ruth C. 1946. A functional calculus of first order based on strict implication. *Journal of Symbolic Logic* 11(1). 1–16.

Barker, Chris & David R. Dowty (eds.). 1992. *Proceedings of SALT*. 2 Columbus, OH: Ohio State University.

Bartsch, Renate. 1973. "Negative Transportation" gibt es nicht. *Linguistische Berichte* 27. 1–7.

Bartsch, Renate, Johan van Benthem & Peter van Emde Boas (eds.). 1989. *Semantics and Contextual Expression*. Dordrecht: Foris.

Barwise, Jon & Robin Cooper. 1981. Generalized quantifiers and natural language. *Linguistics and Philosophy* 4(2). 159–219. Reprinted in Portner & Partee (2002: 75–126).

Bäuerle, Rainer, Urs Egli & Arnim von Stechow (eds.). 1979. *Semantics from Different Points of View*. Berlin: Springer.

Bäuerle, Rainer, Christoph Schwarze & Arnim von Stechow (eds.). 1983. *Meaning, Use and Interpretation of Language*. Berlin: Walter de Gruyter.

Beals, Katharine (ed.). 1993. *Papers from the 29th Regional Meeting of the Chicago Linguistic Society*. Chicago: Chicago Linguistic Society.

Beaver, David, Luis Casillas, Brady Clark & Stefan Kaufmann (eds.). 2002. *The Construction of Meaning*. Stanford, CA: CSLI.

Beck, Sigrid. 1997. On the semantics of comparative conditionals. *Linguistics and Philosophy* 20(3). 229–71.

Beck, Sigrid & Hotze Rullmann. 1999. A flexible approach to exhaustivity in questions. *Natural Language Semantics* 7(3). 249–98.

Becker, Oskar. 1930. Zur Logik der Modalitäten. *Jahrbuch für Philosophie und Phänomenologische Forschung* 11. 497–548.

Beghelli, Filippo & Tim Stowell. 1997. Distributivity and negation: the syntax of *each* and *every*. In Szabolcsi (1997), 71–107.

Belletti, Adriana & Luigi Rizzi (eds.). 1996. *Parameters and Functional Heads*. Oxford: Oxford University Press.

Benincà, Paola. 1980. Nomi senza articolo. *Rivista di Grammatica Generativa* 5. 51–63.

Benincà, Paola, Giampaolo Salvi & Loredana Frison. 1988. L'ordine degli elementi della frase e le costruzioni marcate. In Renzi & Salvi (1988), vol. I, 115–225.

Bennett, Michael. 1979. Mass nouns and mass terms in Montague Grammar. In Davis & Mithun (1979), 263–85.

Bertinetto, Pier Marco, Valentina Bianchi, James Higginbotham & M. Squartini (eds.). 1995. *Temporal Reference, Aspect and Actionality*, vol. 1. Torino: Rosenberg & Sellier.

Berwick, Robert C., Steven P. Abney & Carol Tenny (eds.). 1991. *Principle-Based Parsing: Computation and Psycholinguistics*. Dordrecht: Kluwer.

Beyssade, Claire (ed.). 2002. *Romance Languages and Linguistic Theory 2000: Selected Papers from "Going Romance" 2000*. Amsterdam: John Benjamins.

Bierwisch, Manfred & Karl Erich Heidolph (eds.). 1970. *Progress in Linguistics*. The Hague: Mouton.

Biezma, María. 2011. Conditional inversion and GIVENNESS. In Ashton et al. (2011), 552–71.

Binnick, Robert I., Alice Davidson, Georgia M. Green & Jerry L. Morgan (eds.). 1969. *Papers from the 5th Regional Meeting of the Chicago Linguistic Society*. Chicago: Chicago Linguistic Society.

Boeckx, Cedric. 2012. *Syntactic Islands*. Cambridge: Cambridge University Press.

Bolinger, Dwight. 1967a. Adjectives in English: Attribution and predication. *Lingua* 18. 1–34.

Bolinger, Dwight. 1967b. The imperative in English. In Halle et al. (1967), 335–62.

Bolinger, Dwight. 1978. Asking more than one thing at a time. In Hiz (1978), 107–50.

Boniface, Jacqueline. 2005. Leopold Kronecker's conception of the foundations of mathematics. *Philosophia Scientiæ* CS 5. 143–56.

Boniface, Jacqueline & Norbert Schappacher. 2002. Sur le concept de nombre dans la mathématique. Cours inédit de Leopold Kronecker à Berlin (1891). *Revue d'Histoire des mathématiques* 7. 207–75.

Bonomi, Andrea. 1995. Aspect and quantification. In Bertinetto et al. (1995), 93–110.

Boole, George. 1847. *The Mathematical Analysis of Logic*. Cambridge: Macmillan, Barclay, & Macmillan. Reprinted: Oxford. 1948.

Bordelois, Ivonne. 1988. Causatives: from lexycon to syntax. *Natural Language and Linguistic Theory* 6(1). 57–94.

Bordelois, Ivonne, Heles Contreras & Karen Zagona (eds.). 1986. *Generative Studies in Spanish Syntax*. Dordrecht: Foris.

Borgato, GianLuigi. 2012. *Un profilo del verbo* (Quaderni Patavini di Linguistica Monografie 23). Padova: Unipress.

Borgato, GianLuigi & Alberto Zamboni (eds.). 1989. *Dialettologia e varia linguistica per Manlio Cortelazzo*. Padova: Unipress.

Brasoveanu, Adrian. 2011. Sentence-internal *different* as quantifier-internal anaphora. *Linguistics and Philosophy* 34(2). 93–168.

Bresnan, Joan. 1982a. Control and complementation. In Bresnan (1982b), 282–390.

Bresnan, Joan (ed.). 1982b. *The Mental Representation of Grammatical Relations*. Cambridge, MA: MIT Press.

Buchalla, Rohnna & Anita Mittwoch (eds.). 1994. *Proceedings of IATL*. 1 Jerusalem: Akademon.

Bunt, Cornelis Hendrik. 1979. Ensembles and the formal semantic properties of mass terms. In Pelletier (1979), 249–78.

Burge, Tyler. 1973. Reference and proper names. *Journal of Philosophy* 70. 425–39.

Büring, Daniel. 1996. *The 59th Street Bridge Accent*: University of Tübingen dissertation.

Burzio, Luigi. 1981. *Intransitive Verbs and Italian Auxiliaries*. Cambridge, MA: MIT dissertation.

Burzio, Luigi. 1986. *Italian Syntax: A Government and Binding Approach*. Dordrecht: Foris.

Cardinaletti, Anna & Michael Starke. 1996. Deficient pronouns: a view from Germanic. A study in the unified description of Germanic and Romance. In Thráinsson et al. (1996), 21–65.

Carlier, Anne. 1989. Généricité du syntagme nominal sujet et modalité. *Travaux de Linguistique* 19. 33–56.

Carlson, Greg N. 1973. *Superficially Unquantified Plural Count Noun Phrases in English*: University of Iowa MA thesis.

Carlson, Greg N. 1977a. A unified analysis of the English bare plural. *Linguistics and Philosophy* 1(3). 413–58. Reprinted in Portner & Partee (2002: 35–74).

Carlson, Greg N. 1977b. *Reference to Kinds in English*. Amherst, MA: University of Massachusetts dissertation. Reprinted with revisions as Carlson (1980b).

Carlson, Greg N. 1979. Generics and atemporal *when*. *Linguistics and Philosophy* 2(1). 49–98.

Carlson, Greg N. 1980a. Polarity *any* is existential. *Linguistic Inquiry* 11. 799–804.

Carlson, Greg N. 1980b. *Reference to Kinds in English*. New York: Garland. Reprint with revisions of Carlson (1977b).

Carlson, Greg N. 1984. Thematic roles and their role in semantic interpretation. *Linguistics* 22. 259–79.

Carlson, Greg N. & Francis J. Pelletier (eds.). 1995. *The Generic Book*. Chicago: The University of Chicago Press.

Carnap, Rudolf. 1946. Modalities and quantification. *Journal of Symbolic Logic* 11(1). 33–64.

Carnap, Rudolf. 1947. *Meaning and Necessity*. Chicago: Chicago University Press.

Carstairs-McCarthy, Andrew. 1999. *The Origins of Complex Language: An Inquiry into the Evolutionary Beginnings of Sentences, Syllables, and Truth.* Oxford: Oxford University Press.

Carter, Juli & Rose-Marie Dechaine (eds.). 1989. *Proceedings of NELS.* 19 University of Massachusetts Amherst, MA: GLSA.

Cartwright, Helen. 1975. Some remarks about mass nouns and plurality. *Synthese* 31. 395–410. Reprinted in Pelletier (1979: 15–30).

Casalegno, Paolo. 1987. Sulla logica dei plurali. *Teoria* 2. 125–43.

Chand, Vineeta, Ann Kelleher, Angelo J. Rodríguez & Benjamin Schmeiser (eds.). 2004. *Proceedings of WCCFL.* 23 Somerville, MA: Cascadilla Press.

Chereches, Anca (ed.). 2012. *Proceedings of SALT.* 22 Ithaca, NY: Cornell University Press.

Chierchia, Gennaro. 1997. Partitives, reference to kinds and semantic variation. In Lawson (1997), 73–98.

Chierchia, Gennaro. 1998. Reference to kinds across languages. *Natural Language Semantics* 6(4). 339–405.

Chierchia, Gennaro. 2006. Broaden your views. Implicatures of domain widening and the "logicality" of language. *Linguistic Inquiry* 37. 535–90.

Chomsky, Noam. 1961. On the notion "rule of grammar". In Jakobson (1961), 6–24. Reprinted with revisions in Fodor & Katz (1964: 155–210).

Chomsky, Noam. 1964. *Current Issues in Linguistic Theory.* The Hague: Mouton.

Chomsky, Noam. 1965. *Aspects of the Theory of Syntax.* Cambridge, MA: MIT Press.

Chomsky, Noam. 1970. Remarks on nominalization. In Jacobs & Rosenbaum (1970), 184–221.

Chomsky, Noam. 1973. Conditions on transformations. In Anderson & Kiparsky (1973), 232–86. Reprinted in Chomsky (1977a: 79–160).

Chomsky, Noam. 1975. Questions of form and interpretation. *Linguistic Analysis* 1(1). 75–109.

Chomsky, Noam. 1976. Conditions on rules of grammar. *Linguistic Analysis* 2. 303–51.

Chomsky, Noam. 1977a. *Essays on Form and Interpretation*. Amsterdam: North-Holland.

Chomsky, Noam. 1977b. On wh-movement. In Culicover et al. (1977), 71–132.

Chomsky, Noam. 1981. *Lectures on Government and Binding*. Dordrecht: Foris.

Chomsky, Noam. 1982. *Some Concepts and Consequences of the Theory of Government and Binding*. Cambridge, MA: MIT Press.

Chomsky, Noam. 1993. A minimalist program for linguistic theory. In Hale & Keyser (1993), 1–52.

Chomsky, Noam. 1995. *The Minimalist Program*. Cambridge, MA: MIT Press.

Chomsky, Noam. 2001. Derivation by phase. In Kenstowicz (2001), 1–52.

Chomsky, Noam & Howard Lasnik. 1977. Filters and control. *Linguistic Inquiry* 8. 425–504.

Chomsky, Noam & George A. Miller. 1963. Introduction to the formal analysis of natural languages. In Luce et al. (1963), 269–321.

Christophersen, Paul. 1939. *The Articles. A Study of their Theory and Use in English*. Copenhagen: Munksgaard.

Cinque, Guglielmo. 1977. The movement nature of left dislocation. *Linguistic Inquiry* 8(2). 397–411.

Cinque, Guglielmo. 1978. La sintassi dei pronomi relativi 'cui' e 'quale' nell'italiano moderno. *Rivista di Grammatica Generativa* 3. 31–126.

Cinque, Guglielmo. 1981. On the theory of relative clauses and markedness. *The Linguistic Review* 1. 247–94. Reprinted as Cinque (1995: Ch.2).

Cinque, Guglielmo. 1990. *Types of $\bar{A}$ Dependencies*. Cambridge, MA: MIT Press.

Cinque, Guglielmo. 1995. *Italian Syntax and Universal Grammar*. Cambridge: Cambridge University Press.

Cobham, Alan. 1964. The intrinsic computational difficulty of functions. In Yeoshua Bar-Hillel (ed.), Bar-Hillel (1964), vol. 2, 24–30.

Cole, Peter & Jerry L. Morgan (eds.). 1975. *Syntax and Semantics. Speech Acts.* 3 New York: Academic Press.

Comorovski, Ileana & Klaus von Heusinger (eds.). 2007. *Existence: Semantics and Syntax*. Dordrecht: Springer.

Condoravdi, Cleo. 1997. *Descriptions in Context*. New York: Garland.

Condoravdi, Cleo. 2002. Temporal interpretation of modals: modals for the present and for the past. In Beaver et al. (2002), 59–87.

Contreras, Heles. 1986. Spanish bare NPs and the ECP. In Bordelois et al. (1986), 25–49.

Cooper, Robin. 1979. The interpretation of pronouns. In Heny & Schnelle (1979), 61–92.

Corblin, Francis. 1987. *Indéfini, défini et démonstratif. Constructions linguistiques de la référence*. Genève: Droz.

Corblin, Francis, Carmen Dobrovie-Sorin & Jean-Marie Marandin (eds.). 1999. *Empirical Issues in Formal Syntax and Semantics*, vol. 2. The Hague: Thesus.

Corblin, Francis & Henriette de Swart (eds.). 2004. *Handbook of French Semantics*. Stanford, CA: CSLI.

Cormack, Annabel. 1984. VP anaphora: variables and scope. In Landman & Veltman (1984), 81–102.

Crain, Stephen & Mark Steedman. 1985. On not being led up the garden path: the use of context by the psychological syntax processor. In Karttunen et al. (1985), 320–58.

Cresswell, Maxwell J. 1976. The semantics of degree. In Partee (1976), 261–92.

Culicover, Peter W. & Ray S. Jackendoff. 2001. Control is not movement. *Linguistic Inquiry* 32. 493–512.

Culicover, Peter W. & Louise McNally (eds.). 1998. *Syntax and Semantics. The Limits of Syntax*. 29 New York: Academic Press.

Culicover, Peter W., Thomas Wasow & Adrian Akmajian. 1977. *Formal Syntax*. New York: Academic Press.

Dahl, Östen. 1979. Typology of sentence negation. *Linguistics* 17. 79–106.

Davidson, Donald. 1967. The logical form of action sentences. In Rescher (1967), 81–95.

Davidson, Donald & Gilbert Harman (eds.). 1972. *Semantics of Natural Language*. Dordrecht: Kluwer.

Davis, Martin (ed.). 1965. *The Undecidable. Basic Papers on Undecidable Propositions, Unsolvable Problems and Computable Functions*. Hewlett, NY: Raven Press.

Davis, Steven & Marianne Mithun (eds.). 1979. *Linguistics, Philosophy and Montague Grammar*. Austin: University of Texas Press.

Dayal, Veneeta. 1995. Licensing *any* in non-negative/non-modal contexts. In Simons & Galloway (1995), 72–93.

Dayal, Veneeta. 1997. Free relatives and *ever*: *identity* and *free choice* readings. In Lawson (1997), 99–116.

Dayal, Veneeta. 1998. *Any* as inherently modal. *Linguistics and Philosophy* 21(5). 433–76.

De Morgan, Augustus. 1864. On the Syllogism IV and on the logic of relations. *Cambridge Philosophical Transactions* 10. 331–58.

Deane, P. 1991. Limits to attention: a cognitive theory of island phenomena. *Cognition* 2. 1–63.

Dedekind, Richard. 1888. *Was sind und was sollen die Zahlen?* Braunschweig: Vieweg. Eng. transl.: The nature and meaning of numbers. In Dedekind (1901: 31–115).

Dedekind, Richard. 1901. *Essays on the Theory of Numbers: Continuity and Irrational Numbers. The Nature and Meaning of Numbers*. Chicago: Open Court.

Dekker, Paul. 2001. On *if* and *only*. In Hastings et al. (2001), 114–33.

Dekker, Paul, Martin Stokhof & Yde Venema (eds.). 1997. *Proceedings of the 11th Amsterdam Colloquium*. Amsterdam: University of Amsterdam.

DeLancey, Scott. 1999. Relativization in Tibetan. In Yadava & Glover (1999), 231–49.

Delfitto, Denis. 2002. *Genericity in Language. Issues of Syntax, Logical Form and Interpretation*. Alessandria: Edizioni dell'Orso.

Delfitto, Denis & Jan Schroten. 1991. Bare plurals and the number affix in DP. *Probus* 3(2). 155–85.

Demonte, Violeta. 2008. Meaning-form correlation and adjective position in Spanish. In McNally & Kennedy (2008), 71–100.

Devoto, Giacomo. 1968. *Dizionario etimologico. Avviamento alla etimologia italiana*. Firenze: Le Monnier.

Diesing, Molly. 1990. Verb movement and the subject position in Yddish. *Natural Language and Linguistic Theory* 8(1). 41–79.

Diesing, Molly. 1992. *Indefinites*. Cambridge, MA: MIT Press.

Dinneen, Francis P. (ed.). 1966. *Report on the Seventeenth Annual Round Table Meeting on Linguistics and Language Studies*. Washington, DC: Georgetown University Press.

Dobrovie-Sorin, Carmen. 2004. Generic plural indefinites and (in)direct binding. In Corblin & de Swart (2004), 55–70.

van der Does, Jaap & Jan van Eijck (eds.). 1996. *Quantifiers, Logic and Language*. Stanford, CA: CSLI.

Dotlačil, Jakub & Adrian Brasoveanu. 2012. The online interpretation of sentence-internal *same* and distributivity. In Chereches (2012), 104–24.

Dowty, David R. 1972. *Studies in the Logic of Verb Aspect and Time Reference in English*. Austin: University of Texas dissertation.

Dowty, David R. 1979. *Word Meaning and Montague Grammar. The Semantics of Verbs and Times in Generative Semantics and Montague's PTQ*. Dordrecht: Reidel.

Dowty, David R. 1993. Deductive versus semantic accounts of reasoning: the relevance of negative polarity and negative concord marking. In Beals (1993), 91–124.

Dowty, David R. 1994. The role of negative polarity and concord marking in natural language reasoning. In Harvey & Santelmann (1994), 114–44.

Drapkin, Jennifer & Donald Perlis. 1986. A preliminary excursion into step-logics. In Ras & Zemankova (1986), 262–9.

Dubinsky, Stanley & Kemp Williams. 1995. Recategorization of prepositions as complementizers: the case of temporal prepositions in English. *Linguistic Inquiry* 26. 125–37.

Dummett, Michael. 1973. *Frege: Philosophy of Language*. London: Duckworth.

Dybjer, Peter, Sten Lindström, Erik Palmgren & Göran Sundholm (eds.). 2012. *Epistemology versus Ontology. Essays on the Philosophy and Foundations of Mathematics in Honour of Per Martin-Löf*. Dordrecht: Springer.

É. Kiss, Katalin. 1998. Identificational focus versus information focus. *Language* 74(2). 45–273.

Ebert, Christian & Cornelia Endriss (eds.). 2006. *Proceedings of Sinn und Bedeutung*. 10 Berlin: ZAS.

Edwards, Harold M. 1987. An appreciation of Kronecker. *The Mathematical Intelligencer* 9(1). 28–35.

Edwards, Harold M. 1988. Kronecker's place in history. In Aspray & Kitcher (1988), 139–44.

Edwards, Harold M. 1989. Kronecker's views on the foundations of mathematics. In Rowe & McCleary (1989), 67–77.

Edwards, Harold M. 1995. Kronecker on the foundations of mathematics. In Hintikka (1995), 45–52.

Ehlich, Konrad & Henk van Riemsdijk (eds.). 1983. *Connectedness in Sentence, Discourse, and Text*. Tilburg: Tilburg University Press.

Eickmeyer, Hans J. & Hannes Rieser (eds.). 1981. *Words, Worlds and Contexts: New Approaches to Word Semantics*. Berlin: Mouton de Gruyter.

Eisner, Jason. 1994. '∀'-less in Wonderland? Revisiting *any*. In Fuller et al. (1994), 92–103.

Elbourne, Paul D. 2005. *Situations and Individuals*. Cambridge, MA: MIT Press.

Farkas, Donka F. 1985. *Intensional Descriptions and the Romance Subjunctive Mood*. New York: Garland.

Farkas, Donka F. 1992a. Mood choice in complement clauses. In Kenesei & Pleh (1992), 77–103.

Farkas, Donka F. 1992b. On the semantics of subjunctive complements. In Hirschbühler & Koerner (1992), 69–104.

Farley, Anne M., Peter T. Farley & Karl-Erik McCullough (eds.). 1986. *Papers from the Parasession on Pragmatics and Grammatical Theory*. Chicago: Chicago Linguistic Society.

Fauconnier, Giles. 1975a. Polarity and the scale principle. In Grossman et al. (1975), 188–99.

Fauconnier, Giles. 1975b. Pragmatic scales and logical structure. *Linguistic Inquiry* 6. 353–75.

Fee, E. Jane & Katherine Hunt (eds.). 1989. *Proceedings of WCCFL*. Stanford, CA: Stanford Linguistic Association.

Feferman, Solomon. 1990. Polymorphic typed lambda calculi in a type-free axiomatic framework. *Contemporary Mathematics* 106. 101–36.

Feferman, Solomon & Thomas Strahm. 2010. Unfolding finitist arithmetic. *Review of Symbolic Logic* 3(4). 665–89.

Feldman, Fred. 1970. Leibniz and 'Leibniz' Law. *Philosophical Review* 79. 510–22.

Field, Hartry H. 1978. Mental representation. *Erkenntnis* 13(1). 9–61. Reprinted in Stich & Warfield (1994).

Filik, Ruth, Kevin B. Paterson & Simon P. Liversedge. 2004. Processing doubly quantified sentences: Evidence from eye movements. *Psychonomic Bulletin & Review* 11(5). 953–9.

Filip, H. & Greg N. Carlson. 1997. Sui generis genericity. *Penn Working Papers in Linguistics* 4. 91–110.

Fillmore, Charles J. 1965. Entailment rules in a semantic theory. Tech. Rep. POLA 10 Ohio State University Columbus.

Fillmore, Charles J., Paul Kay & Catherine O'Connor. 1988. Regularity and idiomaticity in grammatical constructions: the case of *let alone*. *Language* 64. 501–38.

von Fintel, Kai & Sabine Iatridou. 2007. Anatomy of a modal construction. *Linguistic Inquiry* 38. 445–83.

Fodor, Janet Dean. 1970. *The Linguistic Description of Opaque Contexts*. Cambridge, MA: MIT dissertation. Published as Fodor (1979).

Fodor, Janet Dean. 1979. *The Linguistic Description of Opaque Contexts*. New York & London: Garland. Published version of Fodor (1970).

Fodor, Janet Dean. 1982. The mental representation of quantifiers. In Peters & Saarinen (1982), 129–64.

Fodor, Janet Dean & Ivan A. Sag. 1982. Referential and quantificational indefinites. *Linguistics and Philosophy* 5(3). 355–98.

Fodor, Jerry A. 1975. *The Language of Thougth*. Cambridge, MA: Harvard University Press.

Fodor, Jerry A. & Jerrold J. Katz (eds.). 1964. *The Structure of Language: Readings in the Philosophy of Language*. Englewood Cliffs, NJ: Prentice-Hall.

Fox, Danny. 1995. Economy and scope. *Natural Language Semantics* 3(3). 283–341.

Frege, Gottlob. 1879. *Begriffschrift, eine der arithmetischen nachgebildete Formelsprache des reinen Denkens*. Nebert: Halle. Eng. transl.: *Begriffsschrift*, a formula language, modeled upon that of arithmetic, for pure thought. In van Heijenoort (1967: 1–82).

Frege, Gottlob. 1892. Über Sinn und Bedeutung. *Zeitschrift für Philosophie und philosophische Kritik* 100. 25–50. Eng. transl.: On sense and reference. In Geach & Black (1952: 56–78).

Frege, Gottlob. 1918. Der Gedanke. Eine Logische Untersuchung. *Beiträge zur Philosophie des deutschen Idealismus* 1. 58–77. Eng. transl.: The Thought: A Logical Inquiry. In Strawson (1967: 17–38).

Freidin, Robert (ed.). 1991. *Principles and Parameters in Comparative Grammar*. Cambridge, MA: MIT Press.

Friedman, Tova & Satoshi Ito (eds.). 2008. *Proceedings of SALT*. 18 Ithaca, NY: Cornell University Press.

Fuller, Janet M., Ho Han & David Parkinson (eds.). 1994. *Proceedings of ESCOL*. Ithaca, NY: Cornell University Press.

Gabbay, Dov M. & Franz Guenthner (eds.). 1984. *Handbook of Philosophical Logic: Extensions of Classical Logic*, vol. 2. Dordrecht: Reidel.

Gajewski, Jon Robert. 2005. *Neg-Raising: Polarity and Presupposition*. Cambridge, MA: MIT dissertation.

Gajewski, Jon Robert. 2007. Neg-Raising and polarity. *Linguistics and Philosophy* 30(3). 289–328.

Gana, Francesco. 1986. Dio e l'uomo nella matematica di Kronecker. *Historia Mathematica* 13. 255–76.

Garey, Howard B. 1957. Verbal aspects in French. *Language* 33. 91–110.

Gauss, Carl Friederich. 1900a. Letter to Schumacher, 12 july 1831. In Gauss (1900b), 215–8.

Gauss, Carl Friederich. 1900b. *Werke*, vol. 8. Leipzig: Teubner.

Geach, Peter T. 1962. *Reference and Generality: An Examination of Some Old Medieval Theories*. Ithaca, NY: Cornell University Press.

Geach, Peter T. & Max Black (eds.). 1952. *Translations From the Philosophical Writings of Gottlob Frege*. Oxford: Blackwell.

Germano, Giorgio & Stefano Mazzanti. 1988. Primitive iteration and unary functions. *Annals of Pure and Applied Logic* 40. 217–56.

Gerstner-Link, Claudia & Manfred Krifka. 1993. Genericity. In Jacobs et al. (1993), 966–78.

Geurts, Bart. 1996. On *no*. *Journal of Semantics* 13(1). 67–86.

Geurts, Bart. 1999. *Presuppositions and Pronouns*. Oxford: Elsevier.

Geurts, Bart. 2007. Existential import. In Comorovski & von Heusinger (2007), 253–71.

Geurts, Bart. 2010. *Quantity Implicatures*. Cambridge: Cambridge University Press.

Geurts, Bart & Rob van der Sandt. 2004. Interpreting focus. *Theoretical Linguistics* 30(1). 1–44.

Giannakidou, Anastasia. 1995. Subjunctive, habituality and negative polarity items. In Simons & Galloway (1995), 94–111.

Giannakidou, Anastasia. 1997a. Linking sensitivity to limited distribution. In Dekker et al. (1997), 139–44.

Giannakidou, Anastasia. 1997b. *The Landscape of Polarity Items*. Groningen: University of Groningen dissertation.

Giannakidou, Anastasia. 1998. *Polarity sensitivity as (non)veridical dependency*. Amsterdam: John Benjamins.

Giannakidou, Anastasia. 2000. Negative . . . concord? *Natural Language and Linguistic Theory* 18. 457–523.

Gibson, Masayuki & Jonathan Howell (eds.). 2006. *Proceedings of SALT*. 16 Ithaca, NY: Cornell University Press.

Gillon, Brendan S. 1992. Towards a common semantics for English count and mass nouns. *Linguistics and Philosophy* 15(6). 597–639.

Givon, Talmy. 1979. *On Understanding Grammar*. New York: Academic Press.

Gödel, Kurt. 1931. Über formal unentscheidbare Sätze der Principia mathematica und verwandter Systeme I. *Monatshefte für Mathematik und Physik* 38. 173–98. Eng. transl.: On formally undecidable propositions of *Principia Mathematica* and related systems. I. In Davis (1965: 5–38).

Gödel, Kurt. 1933. Eine Interpretation des Intuitionistichen Aussagenkalküls. *Ergebnisse eines Mathematischen Kolloquiums* 4. 39–40. Eng. transl.: An interpretation of the intuitionistic propositional calculus. In Gödel (1986: 296–303).

Gödel, Kurt. 1986. *Collected Works*, vol. 1. Oxford: Oxford University Press. Edited by Solomon Feferman, J. W. Dawson, Stephen C. Kleene, G. H. Moore, R. M. Solovay and Jean van Heijenoort.

Goldfarb, Warren. 1988. Poincaré against the logicists. In Aspray & Kitcher (1988), 61–81.

Gonzàlez, Mercè (ed.). 1994. *Proceedings of NELS*. 24 Amherst, MA: GLSA.

Goodluck, Helen & Michael S. Rochemont. 1992. *Island Constraints: Theory, Acquisition, and Processing*. Dordrecht: Kluwer.

Goodstein, Reuben Louis. 1945. Function theory in an axiom-free equation calculus. In *Proceedings of the London Mathematical Society* (2 48), 401–34.

Goodstein, Reuben Louis. 1951. *Constructive Formalism. Essays in the Philosophy of Mathematics*. Leicester: University College Leicester.

Goodstein, Reuben Louis. 1954. Logic-free formalisations of recursive arithmetic. *Mathematica Scandinavica* 2. 247–61.

Goodstein, Reuben Louis. 1957. *Recursive Number Theory. A Development of Recursive Arithmetic in a Logic-Free Equation Calculus*. Amsterdam: North-Holland.

Goodstein, Reuben Louis. 1972. Wittgenstein's philosophy of mathematics. In Ambrose & Lazerowitz (1972), 271–86.

Graffi, Giorgio. 2001. *200 Years of Syntax. A Critical Survey*. Amsterdam: John Benjamins.

Grantham O'Brien, Mary, Christine Shea & John Archibald (eds.). 2006. *Proceedings of the 8th Generative Approaches to Second Language Acquisition Conference (GASLA 2006)*. Somerville, MA: Cascadilla Proceeding Project.

Grassmann, Hermann. 1861. *Lehrbuch der Arithmetik für höhere Lehranstalten*. Berlin: Enslin.

Greenberg, Yael. 1998. Temporally restricted generics. In Strolovitch & Lawson (1998), 55–73.

Greenberg, Yael. 2002. *Manifestations of Genericity*: Bar-Ilan University dissertation. Published as Greenberg (2003).

Greenberg, Yael. 2003. *Manifestations of Genericity*. New York & London: Routledge. Published version of Greenberg (2002).

Grice, H. Paul. 1961. The causal theory of perception. In *Proceedings of the Aristotelian Society. Supplementary*, vol. 35, 121–53. The Aristotelian Society.

Grice, H. Paul. 1975. Logic and conversation. In Cole & Morgan (1975), 41–58.

Groenendijk, Jeroen, Theo Janssen & Martin Stokhof (eds.). 1981. *Formal Methods in the Study of Language*. Amsterdam: Mathematisch Centrum.

Groenendijk, Jeroen, Theo Janssen & Martin Stokhof (eds.). 1984. *Truth, Interpretation and Information*. Dordrecht: Foris.

Groenendijk, Jeroen & Martin Stokhof. 1981. Semantics of *wh*-complements. In Groenendijk et al. (1981), 153–81.

Groenendijk, Jeroen & Martin Stokhof. 1982. Semantic analysis of *wh*-complements. *Linguistics and Philosophy* 5(2). 175–233.

Groenendijk, Jeroen & Martin Stokhof. 1983. Interrogative quantifiers and Skolem-functions. In Ehlich & van Riemsdijk (1983), 71–110.

Groenendijk, Jeroen & Martin Stokhof. 1984a. On the semantics of questions and the pragmatics of answers. In Landman & Veltman (1984), 143–70.

Groenendijk, Jeroen & Martin Stokhof. 1984b. *Studies on the Semantics of Questions and the Pragmatics of Answers*: University of Amsterdam dissertation.

Groenendijk, Jeroen, Frank Veltman & Martin Stokhof (eds.). 1987. *Proceedings of the 6th Amsterdam Colloquium*. Amsterdam: University of Amsterdam.

Grossman, Robin E., L. James San & Timothy J. Vance (eds.). 1975. *Papers from the 11th Regional Meeting of the Chicago Linguistic Society*. Chicago: Chicago Linguistic Society.

Guerzoni, Elena & Yael Sharvit. 2007. A question of strength: on NPIs in interrogative clauses. *Linguistics and Philosophy* 30(3). 361–91.

Gutiérrez-Rexach, Javier. 2001. The semantics of Spanish plural existential determiners and the dynamics of judgements types. *Probus* 13. 113–54.

Gutiérrez-Rexach, Javier (ed.). 2003. *Semantics: Critical Concepts in Linguistics*. London: Routledge.

Haegeman, Liliane (ed.). 1997. *Elements of Grammar*. Dordrecht: Kluwer.

Haegeman, Liliane & Raffaella Zanuttini. 1996. Negative Concord in West Flemish. In Belletti & Rizzi (1996), 117–79.

Hale, Kenneth & Samuel Jay Keyser (eds.). 1993. *The View from Building 20: Essays in Linguistics in Honor of Sylvain Bromberger*. Cambridge, MA: MIT Press.

Halle, Morris, Horace G. Lunt & Hugh McLean (eds.). 1967. *To Honor Roman Jakobson: Essays on the Occasion of his Seventieth Birthday: 11 October 1966*. The Hague: Mouton.

Hamblin, Charles L. 1958. Questions. *Australasian Journal of Philosophy* 36. 159–68.

Hamblin, Charles L. 1973. Questions in Montague English. *Foundations of Language* 10. 41–53.

Hamm, Fritz & Erhard Hinrichs (eds.). 1998. *Plurality and Quantification*. Dordrecht: Kluwer.

Harman, Gilbert. 1972. Logical form. *Foundations of Language* 9. 38–65.

Hart, Herbert L. A. 1951. A logician's fairy tale. *Philosophical Review* 60. 98–212.

Harvey, Mandy & Lynn Santelmann (eds.). 1994. *Proceedings of SALT*. 4 Ithaca, NY: Cornell University Press.

Haspelmath, Martin. 1997. *Indefinite pronouns*. Oxford: Clarendon Press.

Hastings, Rachel, Brendan Jackson & Zsofia Zvolenszky (eds.). 2001. *Proceedings of SALT*. 11 Ithaca, NY: Cornell University Press.

Hausser, Ronald R. 1974. *Quantification in Extended Montague Grammar*. Austin: University of Texas dissertation.

Hausser, Ronald R. 1979. How do pronouns denote? In Heny & Schnelle (1979), 93–139.

Hayashishita, J.-R. & Daisuke Bekki. 2012. Conjoined nominal expressions in Japanese: Interpretation through monad. In Okumura et al. (2012), 54–67.

van Heijenoort, Jean (ed.). 1967. *From Frege to Gödel. A Source Book in Mathematical Logic. 1879–1931*. Cambridge, MA & London: Harvard University Press.

Heim, Irene. 1982. *The Semantics of Definite and Indefinite Noun Phrases*. Amherst, MA: University of Massachusetts dissertation.

Heim, Irene. 1983. File change semantics and the familiarity theory of definiteness. In Bäuerle et al. (1983), 164–89. Also in Portner & Partee (2002: 223–48).

Heim, Irene. 1984. A note on negative polarity and downward entailingness. In Jones & Sells (1984), 98–107.

Heim, Irene. 1990. E-type pronouns and donkey anaphora. *Linguistics and Philosophy* 13(2). 137–77.

Heim, Irene. 1994. Interrogative semantics and Karttunen's semantics for *know*. In Buchalla & Mittwoch (1994), 128–44.

Heim, Irene. 2000. Degree operators and scope. In Jackson & Matthews (2000), 40–64.

Heim, Irene & Angelika Kratzer. 1998. *Semantics in Generative Grammar*. Oxford: Blackwell.

Hellan, Lars. 1981. *Towards an Integrated Analysis of Comparatives*. Tübingen: Narr.

Heny, Frank & Helmut S. Schnelle (eds.). 1979. *Syntax and Semantics. Selections from the Third Groningen Round Table.* 10 New York: Academic Press.

Herburger, Elena. 2000. *What Counts: Focus and Quantification*. Cambridge, MA: MIT Press.

Herzog, Otthein & Claus-Rainer Rollinger (eds.). 1991. *Text Understanding in LILOG*. Berlin: Springer-Verlag.

Higginbotham, James. 1985. On semantics. *Linguistic Inquiry* 16. 547–93.

Higginbotham, James. 1986. Linguistic theory and Davidson's program. In LePore (1986), 29–48.

Higginbotham, James. 1987. Indefiniteness and predication. In Reuland & ter Meulen (1987), 43–70.

Higginbotham, James. 1991. Either/or. In Sherer (1991), 143–55.

Higginbotham, James. 1993. Interrogatives. In Hale & Keyser (1993), 195–227.

Hilbert, David. 1926. Über das Unendliche. *Mathematische Annalen* 95. 161–90. Eng. transl.: On the infinite. In van Heijenoort (1967: 367–92).

Hilbert, David & Paul Bernays. 1934. *Grundlagen der Mathematik*, vol. 1. Berlin: Springer.

Hinrichs, Erhard W. 1985. *A Compositional Semantics for Aktionsarten and NP Reference in English*: The Ohio State University dissertation.

Hintikka, Jaakko. 1962. *Knowledge and Belief: An Introduction to the Logic of Two Notions*. New York: Cornell University Press.

Hintikka, Jaakko. 1986. The semantics of *a certain*. *Linguistic Inquiry* 17. 331–6.

Hintikka, Jaakko (ed.). 1995. *From Dedekind to Gödel*. Dordrecht: Kluwer.

Hintikka, Jaakko & Lauri Carlson. 1979. Conditionals, generic quantifiers, and other applications of subgames. In Saarinen (1979), 179-214.

Hintikka, Jaakko, Julius Moravcsik & Patrick Suppes (eds.). 1973. *Approaches to Natural Language: Proceedings of the 1970 Stanford Workshop on Grammar and Semantics*. Dordrecht: D. Reidel.

Hirschbühler, Paul & E.F. Konrad Koerner (eds.). 1992. *Romance Languages and Modern Linguistic Theory: Papers from the 20th Linguistic Symposium on Romance Languages*. Amsterdam: John Benjamins.

Hiz, Henry (ed.). 1978. *Questions*. Dordrecht: Reidel.

Hodges, Wilfrid. 2012. Requirements on a theory of sentence and word meanings. In Schantz (2012), 583–608.

Hoeksema, Jack. 1983a. Negative polarity and the comparative. *Natural Language and Linguistic Theory* 1. 403–34.

Hoeksema, Jack. 1983b. Plurality and conjunction. In ter Meulen (1983), 63–83.

Hoeksema, Jack. 1986. Monotonicity phenomena in natural language. *Linguistic Analysis* 16(1-2). 235–50.

Hofmeister, Philip & Ivan A. Sag. 2010. Cognitve constraints and island effects. *Language* 86. 366–415.

Horn, Laurence R. 1969. A presuppositional approach to *only* and *even*. In Binnick et al. (1969), 98–107.

Horn, Laurence R. 1972. *On the Semantic Properties of Logical Operators in English*. Los Angeles: UCLA dissertation.

Horn, Laurence R. 1984. Toward a new taxonomy for pragmatic inference: Q-based and R-based implicature. In Schiffrin (1984), 11–42.

Horn, Laurence R. 1985. Metalinguistic negation and pragmatic ambiguity. *Language* 61. 121–74.

Horn, Laurence R. 1989. *A Natural History of Negation*. Chicago: University of Chicago Press.

Horn, Laurence R. 1996. Exclusive company: *only* and the dynamics of vertical inference. *Journal of Semantics* 13(1). 1–40.

Hornstein, Norbert. 1984. *Logic as Grammar*. Cambridge, MA: MIT Press.

Huang, Yan. 2007. *Pragmatics*. Oxford: Oxford University Press.

Hudson, Wesley. 1989. Functional categories and the saturation of noun phrases. In Carter & Dechaine (1989), 207–22.

Iatridou, Sabine. 2000. The grammatical ingredients of counterfactuality. *Linguistic Inquiry* 31(2). 231–70.

Iatridou, Sabine & David Embick. 1994. Conditional inversion. In Gonzàlez (1994), 189–203.

Ihsane, Tabea. 2008. *The Layered DP. Form and Meaning of French Indefinites.* Amsterdam: John Benjamins.

Ionin, Tania & Ora Matushansky. 2006. The composition of complex cardinals. *Journal of Semantics* 23. 315–60.

Ioup, Georgette. 1975. *The Treatment of Quantifier Scope in a Transformational Grammar*: City University of New York dissertation.

Ippolito, Michela. 2003. Presuppositions and implicatures in counterfactuals. *Natural Language Semantics* 11(2). 145–86.

Isaacs, James & Kyle Rawlins. 2008. Conditional questions. *Journal of Semantics* 25(3). 269–319.

Jackendoff, Ray S. 1972. *Semantic Interpretation in Generative Grammar.* Cambridge, MA, London: MIT Press.

Jackson, Brendan (ed.). 2002. *Proceedings of SALT.* 12 Ithaca, NY: Cornell University Press.

Jackson, Brendan & Tanya Matthews (eds.). 2000. *Proceedings of SALT.* 10 Ithaca, NY: Cornell University Press.

Jackson, Eric. 1995. Negative polarity and general statements. In Simons & Galloway (1995), 130–47.

Jacobs, Joachim. 1980. Lexical decomposition in Montague-Grammar. *Theoretical Linguistics* 7. 121–36.

Jacobs, Joachim. 1983. *Fokus and Skalen.* Tübingen: Niemeyer.

Jacobs, Joachim. 1984a. Funktionale Satzperspective und Illokutionssemantik. *Linguistische Berichte* 91. 25–8.

Jacobs, Joachim. 1984b. The syntax of focus and adverbials in German. In Abraham & de Meij (1986), 103–27.

Jacobs, Joachim, Arnim von Stechow, W. Sternefeld & Venneman Th. (eds.). 1993. *Handbuch der Syntax.* Berlin: de Gruyter.

Jacobs, Roderick A. & Peter S. Rosenbaum (eds.). 1970. *Readings in English Transformational Grammar.* Waltham, MA: Ginn.

Jacobson, Pauline. 1990. Raising as functional composition. *Linguistics and Philosophy* 13(4). 423–75.

Jacobson, Pauline. 1995. On the quantificational force of English free relatives. In Bach et al. (1995), 451–86.

Jaeger, Jeri (ed.). 1978. *Proceedings of the 4th Annual Meeting of the Berkeley Linguistics Society*. Berkeley, CA: University of California Press.

Jaisser, Annie (ed.). 1988. *Proceedings of the 14th Annual Meeting of the Berkeley Linguistics Society*. Berkeley, CA: Berkeley Linguistics Society.

Jakobson, Roman (ed.). 1961. *Structure of Language and its Mathematical Aspects: Proceedings of the 12th Symposium in Applied Mathematics*. 12 Providence: American Mathematical Society.

Jaspers, Dany. 2005. *Operators in the lexicon: On the negative logic of natural language*. Utrecht: LOT dissertation.

Jayez, Jacques & Lucia M. Tovena. 2002. Determiners and (un)certainty. In Jackson (2002), 164–83.

Jayez, Jacques & Lucia M. Tovena. 2006. Epistemic determiners. *Journal of Semantics* 23(3). 217–50.

Jespersen, Otto. 1909a. *A Modern English Grammar on Historical Principles*, vol. 2. London: Allen and Unwin.

Jespersen, Otto. 1909b. *A Modern English Grammar on Historical Principles*, vol. 5. London: Allen and Unwin.

Jespersen, Otto. 1917. *Negation in english and other languages*. Copenhagen: A. F. Høst.

Jespersen, Otto. 1924. *The Philosophy of Grammar*. London: Allen and Unwin.

Jones, Charles & Peter Sells (eds.). 1984. *Proceedings of NELS*. 14 Amherst, MA: GLSA.

de Jong, Franciska & Henk J. Verkuyl. 1985. Generalized quantifiers: the properness of their strength. In ter Meulen & van Benthem (1985), 21–43.

Kadmon, Nirit & Fred Landman. 1990. Polarity sensitive *any* and free choice *any*. In Stokhof & Torenvliet (1990), 227–51.

Kadmon, Nirit & Fred Landman. 1993. 'Any'. *Linguistics and Philosophy* 16(4). 353–422.

Kadowaki, Makoto & Shigeto Kawahara (eds.). 2003. *Proceedings of the North East Linguistic Society.* 33 Amherst, MA: GLSA.

Kamp, Hans. 1981. A theory of truth and semantic representation. In Groenendijk et al. (1981), 277–322. Reprinted in Groenendijk et al. (1984: 1–41) and Portner & Partee (2002: 189–222).

Kamp, Hans & Barbara H. Partee (eds.). 2004. *Context Dependence in the Analysis of Linguistic Meaning.* Amsterdam: Elsevier.

Kanger, Stig. 1957. *Provability in Logic.* Stockholm: Almqvist and Wiksell.

Kant, Immanuel. 1787. *Critik der reinen Vernunft.* Rīga: Hartknoch. Eng. transl. as Kant (1998).

Kant, Immanuel. 1998. *Critique of Pure Reason.* Cambridge: Cambridge University Press. Edited by Paul Guyer and Allen W. Wood. Eng. transl. of Kant (1787).

Kantrowitz, Robert. 2000. A principle of countability. *Mathematics Magazine* 73(1). 40–42.

Karttunen, Lauri. 1968a. What do referential indices refer to? Tech. Rep. P3854 RAND Corporation.

Karttunen, Lauri. 1968b. What makes definite noun phrases definite? Tech. Rep. P3871 RAND Corporation.

Karttunen, Lauri. 1973. Presuppositions of compound sentences. *Linguistic Inquiry* 4. 167–93.

Karttunen, Lauri. 1974. Presupposition and linguistic context. *Theoretical Linguistics* 1(1). 181–94.

Karttunen, Lauri. 1976. Discourse referents. In McCawley (1976), Reprinted in Gutiérrez-Rexach (2003: 20–39).

Karttunen, Lauri. 1977. Syntax and semantics of questions. *Linguistics and Philosophy* 1(1). 3–44.

Karttunen, Lauri, David R. Dowty & Arnold Zwicky. 1985. *Natural Language Parsing: Psychological, Computational and Theoretical Perspectives.* Cambridge: Cambridge University Press.

Karttunen, Lauri & Stanley Peters. 1979. Conventional implicature. In Oh & Dinneen (1979), 1–56.

Kataoka, Kiyoko. 2006a. Neg-sensitive elements, neg-c-command, and scrambling in Japanese. In Vance & Jones (2006), 221–33.

Kataoka, Kiyoko. 2006b. *Nihongo hitei-bun-no koozoo: Kakimazebun-to hitei-kooo-hyoogen*. Tokyo: Kurosio.

Katz, Jerrold J. 1972. *Semantic Theory*. New York: Harper & Row.

Katz, Jerrold J. & Jerry A. Fodor. 1963. The structure of a semantic theory. *Language* 39. 170–210. Reprinted in Fodor & Katz (1964: 479–518).

Katz, Jerrold J. & Paul M. Postal. 1964. *An Integrated Theory of Linguistic Descriptions*. Cambridge, MA: MIT Press.

Kayne, Richard. 1981. Unambiguous paths. In May & Koster (1981), 143–83.

Kayne, Richard. 1994. *The Antisymmetry of Syntax* (Linguistic Inquiry Monographs 25). MIT Press.

Kayne, Richard. 1997. The English complementizer *of*. *Journal of Comparative Germanic Linguistics* 1. 43–54. Reprinted in Kayne (2000: 212–20).

Kayne, Richard. 2000. *Parameters and Universals*. Oxford: Oxford University Press.

Keenan, Edward (ed.). 1975. *Formal Semantics of Natural Language*. Cambridge: Cambridge University Press.

Kenesei, I. & E. Pleh (eds.). 1992. *Approaches to Hungarian*, vol. 4. Szeged: JATE.

Kennedy, Christopher. 1997a. Comparison and polar opposition. In Lawson (1997), 240–57.

Kennedy, Christopher. 1997b. *Projecting the Adjective: The Syntax and Semantics of Gradability and Comparison*. Santa Cruz: University of California dissertation.

Kenny, Anthony. 1963. *Action, Emotion and Will*. London: Routledge and Kegan Paul.

Kenstowicz, Michael (ed.). 2001. *Ken Hayle. A Life in Language*. Cambridge, MA: MIT Press.

Keshet, Ezra. 2013. Focus on conditional conjunction. *Journal of Semantics* 30(2). 211–56.

Kim, Shin-Sook & Peter Sells. 2007. Generalizing the immediate scope constraint on NPI licensing. In Zeijlstra & Sohen (2007), 85–91.

Kimball, John P. (ed.). 1972. *Syntax and Semantics*, vol. 4. New York: Academic Press.

Kiparsky, Paul. 1996. The shift to head-initial VP in Germanic. In Thráinsson et al. (1996), 140–79.

Kiparsky, Paul & Carol Kiparsky. 1970. Fact. In Bierwisch & Heidolph (1970), Reprinted in Steinberg & Jacobovits (1971: 345–69).

Kleiber, Georges. 1987. *Du côté de la référence verbale: Les phrases habituelles*. Berne: Peter Lang.

Klein, Ernest. 1966-7. *A Comprehensive Etymological Dictionary of the English Language: Dealing with the Origin of Words and Their Sense Development Thus Illustrating the History of Civilization and Culture*. Amsterdam: Elsevier.

Klima, Edward S. 1964. Negation in English. In Fodor & Katz (1964), 246–323.

Klimek-Jankowska, Dorota. 2012. Imperfective and perfective habituals in Polish: A bi-directional OT account of variation and ambiguity. *Journal of Logic, Language and Information* 21(1). 31–52.

Kluender, Robert. 1991. *Cognitive Constraints on Variables in Syntax*. San Diego: University of California dissertation.

Kluender, Robert. 1992. Deriving island constraints from principles of predication. In Goodluck & Rochemont (1992), 223–58.

Kluender, Robert. 1998. On the distinction between strong and weak islands: a processing perspective. In Culicover & McNally (1998), 241–79.

Kluender, Robert. 2004. Are subject islands subject to a processing account? In Chand et al. (2004), 475–99.

Kluender, Robert & M. Kutas. 1993. Subjacency as a processing phenomenon. *Language and Cognitive Processes* 8. 573–633.

Kneale, William & Martha Kneale. 1962. *The Development of Logic*. Oxford: Clarendon Press.

Knittel, Marie Laurence. 2005. Some remarks on adjective placement in the French NP. *Probus* 17. 185–26.

Kolmogorov, Andrey. 1963. On tables of random numbers. *Sankhyā Ser. A* 25. 369–75. Reprinted as Kolmogorov (1998).

Kolmogorov, Andrey. 1998. On tables of random numbers. *Theoretical Computer Science* 207(2). 387–95. Reprint of Kolmogorov (1963).

Kratzer, Angelika. 1977. What 'must' and 'can' must and can mean. *Linguistics and Philosophy* 1(3). 337–55.

Kratzer, Angelika. 1978. *Semantik der Rede, Kontexttheorie-Modalwörter, Konditionalsätze*. Königstein: Scriptor.

Kratzer, Angelika. 1979. Conditional necessity and possibility. In Bäuerle et al. (1979), 117–47.

Kratzer, Angelika. 1981. The notional category of modality. In Eickmeyer & Rieser (1981), 38–74. Reprinted in Portner & Partee (2002: 289–323).

Kratzer, Angelika. 1986. Conditionals. In Farley et al. (1986), 1–15.

Kratzer, Angelika. 1991. Conditionals. In von Stechow & Wunderlich (1991), 651–6.

Kratzer, Angelika & Junko Shimoyama. 2002. Indeterminate pronouns: the view from Japanese. In Otsu (2002), 1–25.

Krifka, Manfred. 1986. *Nominalreferenz und Zeitkonstitution. Zur Semantik von Massentermen, Pluraltermen und Aspektklassen*: University of Munich dissertation. Published as Krifka (1989b).

Krifka, Manfred. 1987. An outline of genericity. SNS-Bericht 87-25 Univesity of Tübingen.

Krifka, Manfred (ed.). 1988. *Genericity in Natural Language*. SNS-Bericht 88-42 University of Tübingen.

Krifka, Manfred. 1989a. Nominal reference, temporal constitution and quantification in event semantics. In Bartsch et al. (1989), 75–115.

Krifka, Manfred. 1989b. *Nominalreferenz und Zeitkonstitution. Zur Semantik von Massentermen, Pluraltermen und Aspektklassen*. Munich: Fink. Published version of Krifka (1986).

Krifka, Manfred. 1991. A compositional semantics for multiple focus constructions. In *Proceedings of SALT*, 127–58.

Krifka, Manfred. 1992. Definite NPs aren't quantifiers. *Linguistic Inquiry* 23. 157–62.

Krifka, Manfred. 1999. At least some determiners aren't determiners. In Turner (1999), 257–91.

Krifka, Manfred. 2001. Quantifying into question acts. *Natural Language Semantics* 9(1). 1–40.

Krifka, Manfred, Francis J. Pelletier, Greg N. Carlson, Alice ter Meulen, Gennaro Chierchia & Godehard Link. 1995. Genericity: an introduction. In Carlson & Pelletier (1995), 1–124.

Kripke, Saul A. 1963. Semantical analysis of modal logic I. Normal modal propositional calculi. *Zeitschrift für mathematische Logik und Grundlagen der Mathematik* 9. 67–96.

Kripke, Saul A. 1972. Naming and necessity. In Davidson & Harman (1972), 253–355. Reprinted as Kripke (1980).

Kripke, Saul A. 1980. *Naming and Necessity*. Cambridge, MA: Harvard University Press. Reprint of Kripke (1972).

Kronecker, Leopold. 1895-1930. *Werke*. Leipzig: Teubner. Reprinted in New York: Chelsea. 1968.

Kuno, Susumu, John Whitman, Young-Se Kang, Ik-Hwan Lee, Joan Maling & Young-Joo Kim (eds.). 2001. *Harvard Studies in Korean Linguistics*. 9 Harvard: Harvard University.

Kuroda, Sige-Yuki. 1982. Indexed predicate calculus. *Journal of Semantics* 1(1). 43–59.

Kurtzman, Howard & Maryellen McDonald. 1993. Resolution of quantifier scope ambiguities. *Cognition* 48. 243–79.

Kutas, Marta & Steven A. Hillyard. 1980. Reading senseless sentences: brain potentials reflect semantic anomaly. *Science* 207. 203–5.

Ladusaw, William A. 1979a. *Polarity Sensitivity as Inherent Scope Relations*. Austin: University of Texas dissertation. Published as Ladusaw (1979b).

Ladusaw, William A. 1979b. *Polarity Sensitivity as Inherent Scope Relations*. New York: Garland. Published version of Ladusaw (1979a).

Ladusaw, William A. 1980. On the notion *affective* in the analysis of negative-polarity items. *Journal of Linguistic Research* 1. 1–16. Reprinted in Portner & Partee (2002: 457–70).

Ladusaw, William A. 1992. Expressing negation. In Barker & Dowty (1992), .

Lahiri, Uptal. 1998. Focus and negative polarity in Hindi. *Natural Language Semantics* 6(1). 57–123.

Laka Mugarza, Miren Itzar. 1990. *Negation in Syntax: On the Nature of Functional Categories and Projections*. Cambridge, MA: MIT dissertation.

Lakoff, George. 1970. Repartee, or a reply to "Negation, conjunction, and quantifiers". *Foundations of Language* 7. 389–422.

Landman, Fred & Frank Veltman (eds.). 1984. *Varieties of Formal Semantics*. Dordrecht: Foris.

Langford, Cooper Harold. 1937. Review: E. W. Beth, *The Signifies of Pasigraphic Systems. A Contribution to the Psychology of the Mathematical thought Process*. *Journal of Symbolic Logic* 2(1). 53–54.

Lappin, Shalom (ed.). 1996. *The Handbook of Contemporary Semantic Theory*. Oxford: Blackwell.

Larson, Richard K. 1988. On the double object construction. *Linguistic Inquiry* 19. 335–92.

Lasnik, Howard. 1976. Remarks on coreference. *Linguistic Analysis* 2. 1–22. Reprinted in Lasnik (1989: 90–109).

Lasnik, Howard. 1989. *Essays on Anaphora*. Dordrecht: Kluwer.

Lawson, Aaron (ed.). 1997. *Proceedings of SALT*. 7 Ithaca, NY: Cornell University Press.

Leech, Geoffrey N. 1969. *Towards a Semantic Description of English*. London: Longman.

Lees, Robert B. 1960. *The Grammar of English Nominalizations*. The Hague: Mouton de Gruyter.

Lees, Robert B. & Edward S. Klima. 1963. Rules for English pronominalization. *Language* 39. 17–28.

Leffel, Katherine & Denis Bouchard (eds.). 1991. *Views on Phrase Structure*. Dordrecht: Kluwer.

Leibniz, Gottfried Wilhelm. 1969. *G. W. Leibniz: Philosophical Papers and Letters*. Dordrecht: Reidel.

LePore, Ernest (ed.). 1986. *Inquiries into Truth and Interpretation*. Oxford: Blackwell.

LePore, Ernest (ed.). 1987. *New Directions in Semantics*. London: Academic Press.

Levinson, Stephen C. 1983. *Pragmatics*. Cambridge: Cambridge University Press.

Lewis, Clarence Irving. 1920. Strict implication. An emendation. *Journal of Philosophy, Psychology and Scientific Methods* 17. 300–2.

Lewis, Clarence Irving & Cooper Harold Langford. 1932. *Symbolic Logic*. London: Century.

Lewis, David. 1973. *Counterfactuals*. Oxford: Blackwell.

Lewis, David. 1975. Adverbs of quantification. In Keenan (1975), 3–15. Reprinted in Portner & Partee (2002: 178–88).

Linebarger, Marcia C. 1980. *The Grammar of Negative Polarity*. Cambridge, MA: MIT dissertation.

Linebarger, Marcia C. 1987. Negative polarity and grammatical representation. *Linguistics and Philosophy* 10(3). 325–87.

Link, Godehard. 1983. The logical analysis of plurals and mass terms: a lattice-theoretical approach. In Bäuerle et al. (1983), 303–23. Reprinted in Portner & Partee (2002: 127–46).

Löbner, Sebastian. 2000. Polarity in natural language: predication, quantification and negation in particular and characterizing sentences. *Linguistics and Philosophy* 23(3). 213–308.

Löbner, Sebastian. 2013. *Understanding Semantics*. London & New York: Routledge 2nd edn.

Locke, John. 1964. *An Essay Concerning Human Understanding*. Cleveland: Meridian Books.

Longobardi, Giuseppe. 1991. Proper names and the theory of N-movement in syntax and Logical Form. *University of Venice Working Papers in Linguistics* 9.

Longobardi, Giuseppe. 1994. Reference and proper names. *Linguistic Inquiry* 25(4). 609–65.

Longobardi, Giuseppe. 2001. How comparative is semantics? A unified parametric theory of bare nouns and proper names. *Natural Language Semantics* 9(4). 335–69.

Luce, R. Duncan, Robert R. Bush & Eugene Galanter (eds.). 1963. *Handbook of Mathematical Psychology*. New York: Wiley.

Lyons, John. 1977. *Semantics*, vol. 1. Cambridge: Cambridge University Press.

MacColl, Hugh. 1880. Symbolical reasoning. *Mind* 5. 45–60.

Mailing, Joan & Annie Zaenen (eds.). 1990. *Syntax and Semantics. Modern Icelandic Syntax*. New York: Academic Press.

Matisoff, James A. 1972. Lahu nominalization, relativization, and genetivization. In Kimball (1972), 237–57.

Matisoff, James A. 2003. *Handbook of Proto-Tibeto-Burman: System and Philosophy of Sino-Tibetan Reconstruction*. Berkeley & Los Angeles: University of California Press.

May, Robert. 1977. *The Grammar of Quantification*. Cambridge, MA: MIT dissertation.

May, Robert. 1985. *Logical Form, Its Structure and Derivation*. Cambridge, MA: MIT Press.

May, Robert & J. Koster (eds.). 1981. *Levels of Syntactic Representation*. Dordrecht: Kluwer.

McCawley, James D. 1972. A program for logic. In Davidson & Harman (1972), 498–544.

McCawley, James D. (ed.). 1976. *Syntax and Semantics. Notes from the Linguistic Underground*. 7 New York: Academic Press.

McCawley, James D. 1981. *Everything that Linguists Have Always Wanted to Know about Logic but Were Ashamed to Ask*. Chicago: University of Chicago Press. Second edition as McCawley (1993).

McCawley, James D. 1988. The comparative conditional constructions in English, German and Chinese. In Jaisser (1988), 176–87.

McCawley, James D. 1993. *Everything that Linguists Have Always Wanted to Know about Logic but Were Ashamed to Ask*. Chicago: University of Chicago Press. Second revised edition of McCawley (1981).

McNally, Louise & Christopher Kennedy (eds.). 2008. *Adjectives and Adverbs. Syntax, Semantics, and Discourse*. Oxford: Oxford University Press.

ter Meulen, Alice (ed.). 1983. *Studies in Modeltheoretic Semantics*. Dordrecht: Foris.

ter Meulen, Alice & Johan van Benthem (eds.). 1985. *Generalized Quantifiers: Theory and Applications*. Dordrecht: Foris.

Meyer, Albert R. 1965. Depth of nesting and the Grzegorczyk hierarchy. *Notices of the American Mathematical Society* 12. 342. Abstract 622-56.

Meyer, Albert R. & Dennis M. Ritchie. 1967a. The complexity of loop programs. In *Proceedings of the ACM National Conference*, vol. 22, 465–9. Association for Computing Machinery.

Meyer, Albert R. & Dennis M. M. Ritchie. 1967b. Computational Complexity and Program Structure. Research report RC-1817 IBM Research.

Mill, John Stuart. 1865. *An Examination of Sir William Hamilton's Philosophy and of the Principal Philosophical Questions Discussed in His Writings*. London: Longman, Green and Co. 2nd edn.

Milsark, Gary L. 1974. *Existential Sentences in English*. Cambridge, MA: MIT dissertation.

Milsark, Gary L. 1977. Toward an explanation of certain peculiarities of the existential construction in English. *Linguistic Analysis* 3. 1–29.

Moltmann, Friederike. 1991. Measure adverbials. *Linguistics and Philosophy* 14(6). 629–60.

Moltmann, Friederike. 2006. Generic *one*, arbitrary PRO, and the first person. *Natural Language Semantics* 14(3). 257–81.

Montague, Richard. 1960. Logical necessity, physical necessity, ethics, and quantifiers. *Inquiry* 3. 259–69.

Montague, Richard. 1970a. English as a formal language I. In Visentini (1970), 189–224. Reprinted in Montague (1974: 188–221).

Montague, Richard. 1970b. Universal grammar. *Theoria* 36. 373–98. Reprinted in Montague (1974: 222–46).

Montague, Richard. 1973. The proper treatment of quantification in ordinary English. In Hintikka et al. (1973), 221–42. Reprinted in Montague (1974: 247–70) and Portner & Partee (2002: 17–34).

Montague, Richard. 1974. *Formal philosophy*. New Haven & London: Yale University Press.

Montague, Richard & Donald Kalish. 1959. 'That'. *Philosophical Studies* 10. 54–61. Reprinted in Montague (1974: 84–94).

Mourelatos, Alexander P. D. 1978. Events, processes, and states. *Linguistics and Philosophy* 2(3). 415–34.

Nocentini, Alberto. 2010. *L'Etimologico. Vocabolario della lingua italiana*. Firenze: Le Monnier.

Ntelitheos, Dimitrios. 2012. *Deriving Nominals: A Syntactic Account of Malagasy Nominalizations*. Leiden: BRILL.

Nunberg, Geoffrey & Chiahua Pan. 1975. Inferring quantification in generic sentences. In *Proceedings of the Chicago Linguistic Society*, vol. 11, 412–22.

Oda, Toshiko. 2008. *Degree Constructions in Japanese*. Mansfield, CT: University of Connecticut dissertation.

Odifreddi, Piergiorgio. 1989. *Classical Recursion Theory*. Amsterdam: Elsevier.

Odifreddi, Piergiorgio. 1999. *Classical Recursion Theory. Volume II*. Amsterdam: Elsevier.

Ogihara, Toshiyuki. 2000. Counterfactuals, temporal adverbs, and association with focus. In Jackson & Matthews (2000), 115–31.

O'Grady, William. 2005. *Syntactic Carpentry. An Emergentist Approach to Syntax*. Mahwah, NJ: Lawrence Erlbaum Associates.

O'Grady, William. 2006. The syntax of quantification in SLA: an emergentist approach. In Grantham O'Brien et al. (2006), 98–113.

Oh, Choon-Kyu & David A. Dinneen (eds.). 1979. *Syntax and Semantics. Presupposition.* 11 New York: Academic Press.

Okumura, Manabu, Daisuke Bekki & Ken Satoh (eds.). 2012. *New Frontiers in Artificial Intelligence.* Dordrecht: Springer.

Otsu, Yukio (ed.). 2002. *Proceedings of the Third Tokyo Conference on Psycholinguistics.* Tokyo: Hituzi Syobo.

Ottósson, Kjartan G. 1989. VP-specifier subjects and the CP/IP distinction in Icelandic and Mainland Scandinavian. *Working Papers in Scandinavian Syntax* 44. 89–100.

Pāṇini. 1987. *Aṣṭādhyāyīof Pāṇini.* Austin: University of Texas Press. Reprinted as Pāṇini (1989).

Pāṇini. 1989. *Aṣṭādhyāyīof Pāṇini.* Delhi: Motilal Banarsidass. Reprint of Pāṇini (1987).

Palmer, Frank Robert. 1974. *The English Verb.* London: Longman.

Parret, Herman, Marina Sbisà & Jef Verschueren (eds.). 1982. *Possibilities and Limitations of Pragmatics. Proceedings of the Conference on Pragmatics, Urbino, July 8–14, 1979.* Amsterdam: John Benjamins.

Partee, Barbara H. 1973. Some structural analogies between tenses and pronouns in English. *Journal of Philosophy* 70. 601–9.

Partee, Barbara H. (ed.). 1976. *Montague Grammar.* New York: Academic Press.

Partee, Barbara H. 2009. Do we need two basic types? *Snippets* 20. 37–41.

Paul, Hermann. 1880. *Prinzipien der Sprachgeschichte.* Tübingen: Niemeyer.

Pelletier, Francis J. (ed.). 1979. *Mass Terms: Philosophical Problems* Synthese Language Library. Dordrecht: Reidel.

Perlmutter, David M. 1978. Impersonal passives and the Unaccusative Hypothesis. In Jaeger (1978), 157–89.

Peters, Stanley & Esa Saarinen (eds.). 1982. *Processes, Beliefs and Questions.* Dordrecht: Reidel.

Poincaré, Jules Henri. 1905. Les mathématiques et la logique. *Revue de Métaphysique et de Morale* 13. 815–35.

Poincaré, Jules Henri. 1906. Les mathématiques et la logique. *Revue de Métaphysique et de Morale* 14. 294–317.

Portner, Paul. 1992. *Situation Theory and the Semantics of Propositional Expressions*. Amherst, MA: University of Massachusetts dissertation.

Portner, Paul. 1997. The semantics of mood, complementation, and conversational force. *Natural Language Semantics* 5(2). 167–212.

Portner, Paul & Barbara H. Partee (eds.). 2002. *Formal Semantics: The Essential Readings*. Oxford: Blackwell.

Postal, Paul M. 1966. On so-called "pronouns" in English. In Dinneen (1966), 177–206.

Postal, Paul M. 1969. Anaphoric islands. In Binnick et al. (1969), 209–39.

Postal, Paul M. 1970. On coreferential complement subject deletion. *Linguistic Inquiry* 1. 439–500.

Postal, Paul M. 1971. *Cross-over Phenomena*. New York: Holt, Rinehart and Winston.

Prior, Arthur Norman. 1957. *Time and Modality*. Oxford: Clarendon Press.

Pritchett, Bradley L. 1991. Subjacency in a principle-based parser. In Berwick et al. (1991), 301–45.

Pylkkänen, Liina & Brian McElree. 2006. The syntax-semantic interface: On-line composition of sentence meaning. In Traxler & Gernsbacher (2006), 537–77.

Quine, Willard Van Orman. 1946. Concatenation as a basis for arithmetic. *Journal of Symbolic Logic* 11(4). 105–14.

Quine, Willard Van Orman. 1956. Quantifiers and propositional attitudes. *Journal of Philosophy* 53. 177–87.

Quine, Willard Van Orman. 1960. *Word and Object*. Cambridge, MA: MIT Press.

Radó, Janina & Oliver Bott. 2012. Underspecified representations of quantifier scope? In Aloni et al. (2012), 180–9.

Ramsey, Frank P. 1931a. General propositions and causality. In Ramsey (1931b), 237–55. Reprinted in Ramsey (1990: 145–63).

Ramsey, Frank P. 1931b. *The Foundations of Mathematics and Other Logical Essays*. London: Routledge and Kegan.

Ramsey, Frank P. 1990. *Philosophical Papers*. Cambridge: Cambridge University Press.

Ras, Zbigniew W. & Maria Zemankova (eds.). 1986. *Proceedings of ISMIS86*. New York: ACM.

Reinhart, Tanya. 1997. Quantifier scope: how labor is divided between QR and choice functions. *Linguistics and Philosophy* 20(4). 335–97.

Reinhart, Tanya. 2006. *Interface Strategies: Optimal and Costly Computations*. Cambridge, MA: MIT Press.

Renzi, Lorenzo & Giampaolo Salvi (eds.). 1988. *Grande grammatica italiana di consultazione*. Bologna: il Mulino.

Rescher, Nicholas (ed.). 1967. *The Logic of Decision and Action*. Pittsburgh: University of Pittsburgh Press.

Reuland, Eric & Alice ter Meulen (eds.). 1987. *The Representation of (In)definiteness*. Cambridge, MA: MIT Press.

Richard, Jules. 1905. Les principes des mathématiques et le problème des ensembles. *Revue générale des sciences pures et appliquées* 16. 541. Eng. transl.: The principles of mathematics and the problem of sets. In van Heijenoort (1967: 142–4).

Rips, Lance J. 1994. *The Psychology of Proof: Deductive Reasoning in Human Thinking*. Cambridge, MA: MIT Press.

Ritchie, Dennis M. 1965. Complexity classification of primitive recursive functions by their machine programs. *Notices of the American Mathematical Society* 12. 343. Abstract 622-59.

Rizzi, Luigi. 1990. *Relativized Minimality*. Cambridge, MA: MIT Press.

Rizzi, Luigi. 1997. The fine structure of the left periphery. In Haegeman (1997), 281–337.

Robinson, Raphael M. 1947. Primitive recursive functions. *Bulletin of the American Mathematical Society* 53. 925–42.

Rochemont, Michael S. 1986. *Focus in Generative Grammar*. Amsterdam: John Benjamins.

Romero, Maribel. 2000. Reduced conditionals and focus. In Jackson & Matthews (2000), 149–66.

Röngvaldsson, Eirikur & Höskuldur Thráinsson. 1990. On icelandic word order once more. In Mailing & Zaenen (1990), 3–40.

Rooth, Mats. 1985. *Association with Focus*. Amherst, MA: University of Massachusetts dissertation.

Rooth, Mats. 1992. A theory of focus interpretation. *Natural Language Semantics* 1(1). 75–116.

Rooth, Mats. 1995. Indefinites, adverbs of quantification, and focus semantics. In Carlson & Pelletier (1995), 265–99.

Rooth, Mats. 1996. Focus. In Lappin (1996), 271–97.

van Rootselar, B. & J. F. Staal (eds.). 1968. *Logic, Methodology and Philosophy of Science*. 3 Amsterdam: North-Holland.

Ross, John R. 1967a. *Constraints on Variables in Syntax*: MIT dissertation. Published as Ross (1986).

Ross, John R. 1967b. On the cyclic nature of English pronominalization. In Halle et al. (1967), 1669–82.

Ross, John R. 1970. On declarative sentences. In Jacobs & Rosenbaum (1970), 222–72.

Ross, John R. 1986. *Infinite Syntax!* Norwood, NJ: Ablex. Published version of Ross (1967a).

Rouveret, Alain & Jean-Roger Vergnaud. 1980. Specifying reference to the subject: French causatives and conditions on representations. *Linguistic Inquiry* 11. 97–202.

Rowe, David E. & John McCleary (eds.). 1989. *The History of Modern Mathematics*, vol. 1: Ideas and Their Reception. London: Academic Press.

Rullmann, Hotze. 1995. *Maximality in the Semantics of Wh-Constructions*. Amherst, MA: University of Massachusetts dissertation.

Russell, Bertrand. 1903. *The Principles of Mathematics*. Reprinted as Russell (2010).

Russell, Bertrand. 1905. On denoting. *Mind* 14. 479–93.

Russell, Bertrand. 1919. The philosophy of logical atomism. *Monist* 29. 190–222.

Russell, Bertrand. 1940. *An Inquiry into Meaning and Truth*. London: George Allen and Unwin.

Russell, Bertrand. 1948. *Human Knowledge: Its Scope and Limits*. London: George Allen and Unwin & Simon and Schuster.

Russell, Bertrand. 2010. *Principles of Mathematics*. London: Routledge.

Ryle, Gilbert. 1949. *The Concept of Mind*. London: Hutchinson.

Saarinen, Esa (ed.). 1979. *Game-Theoretical Semantics*. Dordrecht: D. Reidel.

Salvi, Giampaolo & Lorenzo Renzi (eds.). 2010. *Grammatica dell'italiano antico*. Bologna: il Mulino.

Sánchez Valencia, Víctor. 1991. *Studies on Natural Logic and Categorial Grammar*. Amsterdam: University of Amsterdam dissertation.

van der Sandt, Rob. 1992. Presupposition as anaphora resolution. *Journal of Semantics* 9(4). 333–77.

van der Sandt, Rob A. 1987. Presupposition and discourse structure. In Groenendijk et al. (1987), 312–30. Reprinted with revisions as van der Sandt (1989).

van der Sandt, Rob A. 1989. Presupposition and discourse structure. In Bartsch et al. (1989), 267–94. Revised version of van der Sandt (1987).

van der Sandt, Rob A. & Bart Geurts. 1991. Presupposition, anaphora, and lexical content. In Herzog & Rollinger (1991), 259–96.

Scha, Remko. 1981. Distributive, collective and cumulative quantification. In Groenendijk et al. (1981), 483–512.

Schantz, Richard (ed.). 2012. *Prospects for Meaning*. Berlin: Walter de Gruyter.

Schiffrin, Deborah (ed.). 1984. *Meaning, Form, and Use in Context: Linguistic Applications* GURT. Washington, DC: Georgetown University Press.

Schmerling, Susan F. 1971. A note on negative polarity. *Papers in Linguistics* 4(1).

Schmerling, Susan F. 1972. Apparent counterexamples to the Coordinate Structure Constraint. *Studies in the Linguistic Sciences* 91–104.

Schönfinkel, Moses. 1924. Über die Bausteine der mathematischen Logik. *Mathematische Annalen* 92. 305–16. Eng. transl.: On the building blocks of mathematical logic. In van Heijenoort (1967: 357–66).

Schubert, Lenhart K. & Francis J. Pelletier. 1987. Problems in the representation of the logical form of generics, plurals, and mass nouns. In LePore (1987), 385–451.

Schwager, Magdalena. 2005. *Interpreting Imperatives*. Frankfurt: University of Frankfurt/Main dissertation.

Schwager, Magdalena. 2006. Conditionalized imperatives. In Gibson & Howell (2006), 241–58.

Schwartz, Daniel G. 1987a. A free-variable theory of primitive recursive arithmetic. *Zeitschrift für mathematische Logik und Grundlagen der Mathematik* 33. 147–57.

Schwartz, Daniel G. 1987b. On the equivalence between logic-free and logic-bearing systems of primitive recursive arithmetic. *Zeitschrift für mathematische Logik und Grundlagen der Mathematik* 33. 245–53.

Schwarz, Bernard. 1998. Reduced conditionals in German: event quantification and definiteness. *Natural Language Semantics* 6(3). 271–301.

Schwarzschild, Roger. 1996. *Pluralities*. Dordrecht: Kluwer.

Schwarzschild, Roger. 1999. Givenness, AVOIDF and other constraints on the placement of accent. *Natural Language Semantics* 7(2). 141–77.

Sells, Peter. 2001. Negative polarity licensing and interpretation. In Kuno et al. (2001), 3–22.

Sells, Peter & Shin-Sook Kim. 2006. Korean NPIs scope over negation. *Linguistic Research* 42. 275–97.

Sharvy, Richard. 1980. A more general theory of definite descriptions. *The Philosophical Review* 89(4). 607–24.

Sherer, Tim (ed.). 1991. *Proceedings of NELS*. 21 University of Massachusetts Amherst, MA: GLSA.

Shimoyama, Junko. 2008. Indeterminate NPIs and scope. In Friedman & Ito (2008), 711–28.

Shimoyama, Junko. 2011. Japanese indeterminate negative polarity items and their scope. *Journal of Semantics* 28(4). 413–50.

Sieg, Wilfried, Richard Sommer & Carolyn Talcott (eds.). 2002. *Reflections on the Foundations of Mathematics: Essays in Honor of Solomon Feferman* Lecture Notes in Logic. Natick, MA: A.K. Peters/CRC Press.

Simon-Vandenbergen, Anne-Marie, Kristin Davidse & Dirk Noël (eds.). 1997. *Reconnecting Language: Morphology and Syntax in Functional Perspectives.* Amsterdam: John Benjamins.

Simons, Mandy & Teresa Galloway (eds.). 1995. *Proceedings of SALT.* 5 Ithaca, NY: Cornell University Press.

Skolem, Thoralf. 1923. Begründung der elementaren Arithmetik durch die rekurrierende Denkweise ohne Anwendung scheinbarer Veränderlichen mit unendlichem Ausdehnungsbereich. *Videnskapsselskapets skrifter, I. Matematisk-naturvidenskabelig klasse* 6. Eng. transl.: The foundations of elementary arithmetic established by means of the recursive mode of thought, without the use of apparent variables ranging over infinite domains. In van Heijenoort (1967: 303–33).

Solomonoff, Ray J. 1960. A preliminary report on a general theory of inductive inference. Tech. Rep. ZTB-138 Zator Co.

Solomonoff, Ray J. 1964. A formal theory of inductive inference I. *Information and Control* 7. 1–22.

Sommers, Fred. 1982. *The Logic of Natural Language.* Oxford: Clarendon Press.

Sperber, Dan & Deirdre Wilson. 1986. *Relevance: Communication and Cognition.* Oxford: Blackwell.

Stalnaker, Robert. 1975. Indicative conditionals. *Philosophia* 5. 269–86.

Starosta, Stanley. 1997. Control in constrained dependency grammar. In Simon-Vandenbergen et al. (1997), 99–138.

von Stechow, Arnim. 1985. Focusing and backgrounding operators. Fachgruppe Sprachwissenschaft 6 University of Konstanz.

von Stechow, Arnim. 1991. Current issues in the theory of focus. In von Stechow & Wunderlich (1991), 804–25.

von Stechow, Arnim & Dieter Wunderlich (eds.). 1991. *Semantik/Semantics: An International Handbook of Contemporary Research.* Berlin: Walter de Gruyter.

Steinberg, Danny D. & Leon A. Jacobovits (eds.). 1971. *Semantics: An Interdisciplinary Reader*. Cambridge: Cambridge University Press.

Stich, Stephen P. & Ted A. Warfield (eds.). 1994. *Mental Representation: A Reader*. Oxford: Blackwell.

Stokhof, Martin & Leen Torenvliet (eds.). 1990. *Proceedings of the 7^th Amsterdam Colloquium*. Amsterdam: University of Amsterdam.

Stowell, Tim. 1991. Determiners in NP and DP. In Leffel & Bouchard (1991), 37–56.

Strawson, Peter F. 1950. On referring. *Mind* 59. 520–44.

Strawson, Peter F. 1952. *Introduction to Logical Theory*. London: Metheun.

Strawson, Peter F. (ed.). 1967. *Philosophical Logic*. Oxford.

Strolovitch, Devon & Aaron Lawson (eds.). 1998. *Proceedings of SALT*. 8 Ithaca, NY: Cornell University Press.

Sullivan, Mark W. 1967. *Apuleian Logic*. Amsterdam: North-Holland.

Suppes, Patrick. 1973. Semantics of context-free fragments of natural languages. In Hintikka et al. (1973), 370–94.

Sweet, Henry. 1898. *A New English Grammar*. Oxford.

Szabolcsi, Anna. 1981. The semantics of topic/focus articulation. In Groenendijk et al. (1981), 513–40.

Szabolcsi, Anna. 1983. The possessor that ran away from home. *The Linguistic Review* 3. 89–102.

Szabolcsi, Anna (ed.). 1997. *Ways of Scope Taking*. Dordrecht: Kluwer.

Taglicht, Josef. 1984. *Message and Emphasis*. London: Longman.

Tait, William W. 1968. Constructive reasoning. In van Rootselar & Staal (1968), 185–99.

Tait, William W. 1981. Finitism. *Journal of Philosophy* 78. 524–46.

Tait, William W. 2002. Remarks on finitism. In Sieg et al. (2002), 410–9.

Tait, William W. 2005. *The Provenance of Pure Reason: Essays in the Philosophy of Mathematics and Its History*. Oxford: Oxford University Press.

Tait, William W. 2012. Primitive recursive arithmetic and its role in the foundations of arithmetic: Historical and philosophical reflections. In Dybjer et al. (2012), chap. 8, 161–80.

Tesnière, Lucien. 1959. *Éléments de syntaxe structurale*. Paris: Klincksieck. Eng. transl. as Tesnière (2015).

Tesnière, Lucien. 2015. *Elements of Structural Syntax*. Amsterdam: John Benjamins. Eng. transl. of Tesnière (1959).

Thomason, Richmond H. 1984. Combinations of tense and modality. In Gabbay & Guenthner (1984), 135–65.

Thráinsson, Höskuldur, Samuel David Epstein & Steve Peter (eds.). 1996. *Studies in Comparative Germanic Syntax*, vol. 2. Dordrecht: Kluwer.

Tovena, Lucia M. & Jacques Jayez. 1997. Any as a Finian quantifier. In Dekker et al. (1997), 295–300.

Tovena, Lucia M. & Jacques Jayez. 1999. *Any*: from scalarity to arbitrariness. In Corblin et al. (1999), 39–57.

Travis, Lisa. 1984. *Parameters and Effects of Word Order Variation*. Cambridge, MA: MIT dissertation.

Travis, Lisa. 1991. Parameters of phrase structure and verb-second phenomena. In Freidin (1991), 339–64.

Traxler, Matthew J. & Morton Ann Gernsbacher (eds.). 2006. *Handbook of Psycholinguistics*. Amsterdam: Elsevier.

Tunstall, Susanne. 1998. *The Interpretation of Quantifiers: Semantics and Processing*. Amherst, MA: University of Massachusetts dissertation.

Turner, Ken (ed.). 1999. *The Semantics/Pragmatics Interface from Different Points of View*. Oxford: Elsevier.

Vance, Timothy J. & Kimberly Jones (eds.). 2006. *Japanese/Korean Linguistics*. 14 Stanford, CA: CSLI.

Vanelli, Laura. 1989. Dimostrativi e articoli: deissi e definitezza. In Borgato & Zamboni (1989), 369–81.

Vendler, Zeno. 1957. Verbs and times. *Philosophical Review* 66. 143–60.

Vendler, Zeno. 1967. *Linguistics in Philosophy*. Ithaca, NY: Cornell University Press.

Verkuyl, Henk J. 1972. *On the Compositional Nature of the Aspects*. Dordrecht: Kluwer.

Verkuyl, Henk J. 1981. Numerals and quantifiers in X-bar-syntax and their semantic interpretation. In Groenendijk et al. (1981), vol. 2, 567–99.

Visentini, Bruno et al. (ed.). 1970. *Linguaggi nella società e nella tecnica*. Milan: Edizioni di Comunità.

Wansing, Heinrich (ed.). 1996. *Negation: A Notion in Focus*. Berlin: Walter de Gruyter.

Weber, Heinrich. 1893. Leopold Kronecker. *Mathematische Annalen* 43. 1–25.

Welmers, William Everett. 1974. *African Language Structures*. Berkeley, CA: University of California Press.

Wilkinson, Karina. 1988. The semantics of the common noun 'kind'. In Krifka (1988), 407–29. Reprinted with revisions as Wilkinson (1989).

Wilkinson, Karina. 1989. (In)definites and *kind* NPs. In Fee & Hunt (1989), 414–28. Reprint with revisions of Wilkinson (1988). Reprinted with revisions as Wilkinson (1995).

Wilkinson, Karina. 1991. *Studies in the Semantics of Generic Noun Phrases*. Amherst, MA: University of Massachusetts dissertation.

Wilkinson, Karina. 1995. The semantics of the common noun *kind*. In Carlson & Pelletier (1995), 383–97. Reprint with revisions of Wilkinson (1989).

Winter, Yoad. 2001. *Flexibility Principles in Boolean Semantics*. Cambridge, MA: MIT Press.

Wittgenstein, Ludwig. 1964. *Philosophische Bemerkungen*. Oxford: Blackwell. Eng. transl. as Wittgenstein (1975).

Wittgenstein, Ludwig. 1969. *Philosophische Grammatik*. Oxford: Blackwell. Eng. transl. as Wittgenstein (1974).

Wittgenstein, Ludwig. 1974. *Philosophical Grammar*. Oxford: Blackwell. Eng. transl. of Wittgenstein (1969).

Wittgenstein, Ludwig. 1975. *Philosophical Remarks*. Blackwell. Eng. transl. of Wittgenstein (1964).

van der Wouden, Ton. 1994. *Negative contexts*. Groningen: Rijksuniversiteit dissertation.

Yadava, Yogendra P. & Warren W. Glover (eds.). 1999. *Topics in Nepalese Linguistics*. Kamaladi, Kathmandu: Royal Nepal Academy.

Yagisawa, Takashi. 1984. Proper names as variables. *Erkenntnis* 21. 195–208.

Zaefferer, Dietmar. 1982. On a formal treatment of illocutionary force indicators. In Parret et al. (1982), 779–98.

Zamparelli, Roberto. 1995. *Layers in the Determiner Phrase*: University of Rochester dissertation. Published with revisions as Zamparelli (2000).

Zamparelli, Roberto. 2000. *Layers in the Determiner Phrase*. New York: Garland. Published revised version of Zamparelli (1995).

Zamparelli, Roberto. 2002a. Definite and bare kind-denoting noun phrases. In Beyssade (2002), 305–42.

Zamparelli, Roberto. 2002b. *Dei ex machina*: a note on plural/mass indefinite determiners. Università di Bergamo.

Zamparelli, Roberto. 2007. On singular existential quantifiers in Italian. In Comorovski & von Heusinger (2007), 293–328.

Zanuttini, Raffaella. 1991. *Syntactic Properties of Sentential Negation*: University of Pennsylvania dissertation.

Zanuttini, Raffaella. 1997. *Negation and Clausal Structure: A Comparative Study of Romance Languages*. Oxford: Oxford University Press.

Zanuttini, Raffaella. 2010. La negazione. In Salvi & Renzi (2010), 569–82.

Zeijlstra, Hedde H. 2004. *Sentential Negation and Negative Concord*. Utrecht: LOT dissertation.

Zeijlstra, Hedde H. 2006. How semantics dictates the syntactic vocabulary. In Ebert & Endriss (2006), 421–36.

Zeijlstra, Hedde H. 2010. Emphatic multiple negation expressions in dutch. *The Linguistic Review* 27. 37–73.

Zeijlstra, Hedde H. & Jan-Philipp Sohen (eds.). 2007. *Proceedings of the Workshop on Negation and Polarity*. Tübingen: University of Tübingen.

Zubizarreta, Maria Luisa. 1998. *Topic, Focus and Word Order*. Cambridge, MA: MIT Press.

Zwart, Jan-Wouter. 1991. Clitics in Dutch: evidence for the position of INFL. *Groninger Arbeiten zur Germanistischen Linguistik* 33. 71–92.

Zwart, Jan-Wouter. 1993. *Dutch Syntax: A Minimalist Approach*. Groningen: University of Groningen dissertation.

Zwart, Jan-Wouter. 1996a. *Morphosyntax of Verb Movement: A Minimalist Approach to the Syntax of Dutch*. Dordrecht & Boston: Kluwer.

Zwart, Jan-Wouter. 1996b. N-feature checking in Germanic Verb Second configurations. In Thráinsson et al. (1996), 257–75.

Zwarts, Frans. 1981. Negatief polaire uitdrukkingen 1. *GLOT* 4. 35–132.

Zwarts, Frans. 1986. *Categoriale Grammatica en Algebraische Semantick*. Groningen: University of Groningen dissertation.

Zwarts, Frans. 1995. Nonveridical contexts. *Linguistic Analysis* 25. 286–312.

Zwarts, Frans. 1996a. Facets of negation. In van der Does & van Eijck (1996), 385–421.

Zwarts, Frans. 1996b. A hierarchy of negative expressions. In Wansing (1996), 169–94.

Zwarts, Frans. 1998. Three types of polarity. In Hamm & Hinrichs (1998), 177–238.

Zwicky, Arnold M. 1973. Linguistics as chemistry: the substance theory of semantic primes. In Anderson & Kiparsky (1973), 467–85.

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[2] Gli devo anche l'idea di inserire come *explicit* la citazione di Marziale riportata di seguito…

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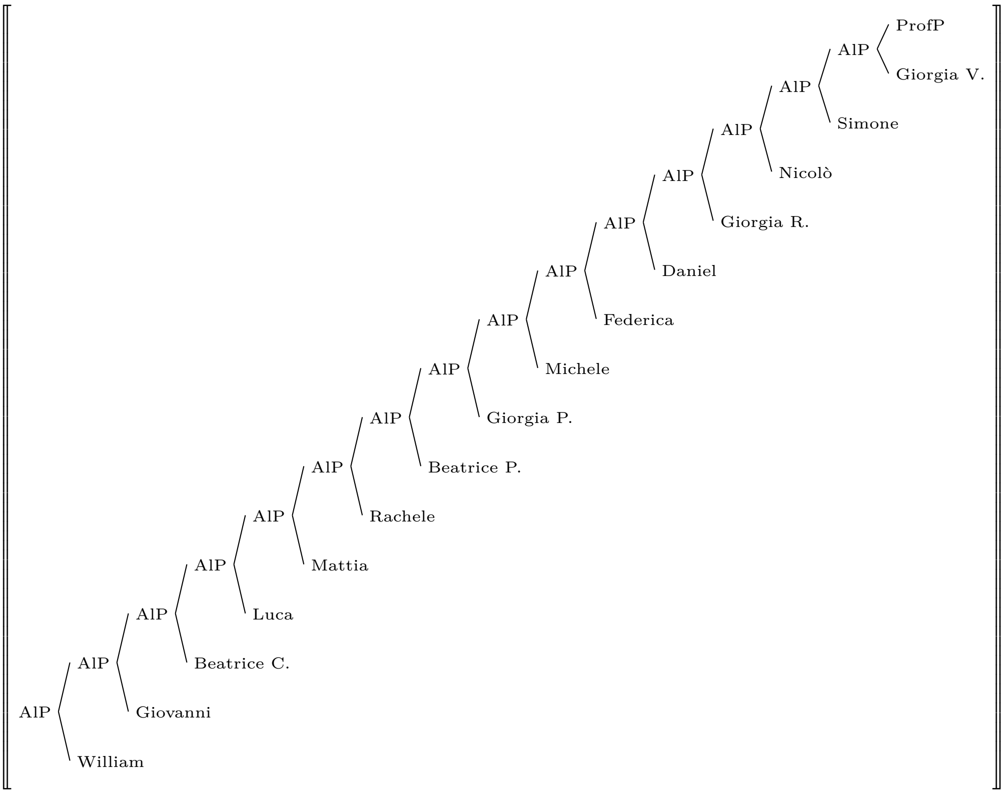

= III B

Ohe, iam satis est, ohe, libelle,
iam pervenimus usque ad umbilicos.
. . .
iam lector queriturque deficitque,
iam librarius hoc et ipse dicit
"ohe, iam satis est, ohe, libelle."

MARTIALIS iv. 89