Truth, Meaning and the Analysis of Natural Language

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By

Paolo Casalegno

Edited by Pasquale Frascolla, Diego Marconi and Elisa Paganini

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PREFACE

Paolo Casalegno (Torino, September 27, 1952 - Milano, April 12, 2009) was one of the best European philosophers working within the analytic tradition. He was well known in the analytic community, where many remember the clarity, efficacy, and originality of his presentations at conferences and workshops, his inexhaustible argumentative *verve*, and his uncommon ability in singling out a philosophical view's essential point and, often, fatal weakness. However, with the exception of a few articles in international journals such as *dialectica* and the *Proceedings of the Aristotelian Society*, much of his research work was published in Italian and dispersed in minor journals, collective volumes, and *Festschriften* for colleagues and friends (Paolo was never particularly picky about where and how his papers were printed). There lies the main motivation for the present volume: we hope it will contribute to better and more widespread knowledge of a philosopher we regard as one of the ablest and most profound in European philosophy of the last decades.

Casalegno graduated from the *Scuola Normale* of Pisa in 1975 and started his academic career as a logician. His much later textbook *Teoria degli insiemi* (*Set theory*, with Mauro Mariani, 2004) shows that he never lost interest for the discipline; indeed, logic was the permanent background of his philosophical research, which was mostly in the areas of philosophy of language (including formal semantics), the theory of truth, and, in his later years, epistemology. His work in formal semantics is here represented by two papers, "Approaches to Quantification" and "*Only*: Association with Focus in Event Semantics". The first is a very clear and thorough account of work in the theory of generalized quantifiers of the late Eighties. It includes several original hints, e.g. on the possibility of extending Hans Kamp's treatment of "donkey sentences" beyond the case of indefinite anaphoric antecedents and, particularly, on the weakness of attempts at getting rid of Jaakko Hintikka's ramified quantifiers by ultimately reducing the relevant cases to special cases of the collective reading of the quantifier.

"*Only*: Association with Focus in Event Semantics" (written with Andrea Bonomi) exemplifies Casalegno's skill in analyzing natural language with the tools of formal semantics. The problem is the analysis of sentences containing phrases of the form "Only[...]", where the brackets indicate focus: e.g. "John only [kissed Mary]" as distinct from "John only [kissed] Mary". After showing that the extant theories (proposed by Mats Rooth, Arnim von Stechow and J.A.G. Groenendijk) were inadequate in that they could not satisfactorily deal with several structures of the form "Only[NP]", Casalegno and Bonomi put forward an original theory within the framework of event semantics and showed that it applied to a large number of structures, including all counterexamples to previous theories; moreover, it could be successfully extended to sentences with multiple focus, such as "John only introduced [Bill] to [Sue]", and to "Only when" structures such as "Only [when John comes in] Mary goes out".

However, Casalegno's central philosophical preoccupation was with the notions of truth and reference. His deep-seated beliefs about them are synthetically expressed at the beginning of "Three Remarks on Truth and Reference". On the one hand, he believed that both truth and reference were puzzling, obscure notions: "The idea that words correspond to bits of reality and that, in virtue of this correlation between words and things, statements have well-defined truth conditions is [...] notoriously hard to make clear and precise. The traditional explanations of how language can attach to the world are inadequate, and general philosophical considerations seem to indicate that no adequate explanation exists [...] our image of the relation between language and reality is illusory and misleading". On the other hand, he also thought that we cannot do without truth or reference: "Not only are these two notions [...] integral to our pre-theoretical image of how language works, but they also seem to be indispensable when we come to theorise". One side of this "Nec tecum, nec sine te" attitude was reflected both in constant criticism of semantic naturalism and its attempts at accounting for the relation between language and reality (in "Three Remarks on Truth and Reference", in the last section of "The Modal Properties of Truth", and more fully in "The Referential and the Logical Component in Fodor's Semantics") and in his support and reinforcement of Quine's indeterminacy arguments ("Quinean inscrutability vs. total inscrutability"). The other side was visible in his later attempt at showing why the notion of truth is useful not just in semantic theorizing but in the context of human life ("Truth and Truthfulness Attributions"). Moreover, Casalegno was extremely suspicious of any view that he saw as compromising truth or reference with epistemological notions: while this is obvious in the papers he devoted to the discussion of Michael Dummett's philosophy of language (not included in the present collection), there is a trace of it in "Reasons to Believe and Assertion", the only article in epistemology he ever published. Indeed, there is no doubt that the notions of truth and reference that Casalegno regarded as both puzzling and indispensable were the *realistic* notions.

In the "Three Remarks on Truth and Reference" paper, Casalegno does (predictably) three things: first, he produces a very simple and clever *a priori* argument against the possibility of naturalizing reference. Secondly, he argues against the claim that the semantic notions, being *theoretical* notions (i.e., notions that are involved in the explanation but not in the description of empirical data), are justified by their success in accounting for the speakers' semantic intuitions. Casalegno argues that the semantic notions can only do their explanatory job by being supplemented with our pretheoretical intuitions about truth. Thirdly, he criticizes Donald Davidson's claim that while theories of truth presuppose a pretheoretical notion of truth, the notion of reference is satisfactorily accounted for once we are aware of its role in the characterization of truth. Against this, Casalegno insists that here, too, we need a pretheoretical notion of reference to check a truth theory's T-sentences. In a way, all three remarks make the same point: both justifications and "reductions" of truth and reference fail in that they presuppose the intuitive notions of truth and reference.

In "The Modal Properties of Truth" Casalegno showed that even Tarskian truth definitions do not fully capture the intuitive notion of truth, *not* because such definitions are not intensionally adequate (they are), *but* because the Tarskian notion of truth alone does not suffice to make sense of the idea that languages are characterized by their semantic rules: to achieve that result we must independently appeal to the intuitive notion of truth (see pp. 101-104). Thus he agreed with Hilary Putnam's argument against Carnap in *Representation and Reality* (1988) to the extent that Putnam's point was "to show that the intuitive notion of truth cannot be replaced everywhere with a predicate defined by Tarski’s method – and moreover that that method does not provide us with an exhaustive theoretical counterpart of the intuitive notion of truth"; but he disagreed with him if Putnam's argument was intended to prove the point by proving the intensional inadequacy of Tarskian definitions. Even though a Tarskian truth definition does not fully explain *what it is* for a language to be identified by its semantic rules, languages *are* identified by their semantic rules, hence biconditionals like "'Peter is happy' is true if and only if Peter is happy" are not just true but necessarily true. Consequently, Tarskian truth definitions are not just extensionally adequate but they are intensionally adequate as well.

Another way one might think of grounding the semantic notions is by having linguistic and non-linguistic behavior determine them: this is the view Quine intended to undermine by the thought experiment of radical translation and the arguments arising from it. In "Quinean Inscrutability vs. Total Inscrutability" Casalegno agreed with the gist of Quine's indeterminacy arguments, though not with the rationale that is often invoked to explain how they work. In his view, the inscrutability of reference does not depend on certain properties being necessarily coinstantiated (whenever there is a rabbit there is an undetached rabbit part, a rabbit stage, etc.) but rather on Quine's not imposing any constraint on how a translation manual translates individual words, except for the single requirement that stimulus-meaning of observation sentences be preserved. "Now—Casalegno observes—it turns out that this is compatible with *any* way of translating words, no matter how crazy" (p.135). Consequently, as Casalegno shows in great detail, inscrutability of reference is *total*, not restricted to *gavagai*-like cases; and it remains total even if one adds more demanding constraints on which translation manuals are admissible.

In "The Referential and the Logical Component in Fodor's Semantics" Casalegno defends inscrutability against Jerry Fodor's claim that reference is, indeed, scrutable. In *The Elm and the Expert* (1994) Fodor had tried to show that it is possible to determine whether by the words "square" and "triangle" a speaker means *square* and *triangle* or *part of a square* and *part of a triangle*, by asking the speaker questions that involve logical notions such as conjunction. The meaning the speaker assigns to the logical words expressing such notions (e.g. "and") can be determined, in turn, by finding out which argument forms he accepts, i.e. by observing the speaker's inferential dispositions. Casalegno attacks Fodor's argument on three counts: first, even if we could be sure that the speaker means conjunction by "and" we could not choose between the two hypotheses (*square* vs. *part of a square*, etc.); secondly, we could not determine by Fodor's method whether the speaker does mean conjunction by "and"; thirdly, even if we could choose between the two hypotheses inscrutability would still be there, for the kind of reasons Casalegno had spelled out in the "Quinean Inscrutability vs. Total Inscritability" paper.

The gist of Casalegno's refutation of Fodor's argument hinges on a controversial claim: that inference patterns do not fix the meanings of the logical words. A speaker may accept standard inference schemata for a connective '*' while assigning non-standard extensions to—say—'F * G'. In the paper on Fodor's semantics, Casalegno proved his point by offering counterexamples to the meaning fixation thesis. However, he also remarked that the fact that the meaning of the logical symbols is not fixed by the set of valid inferences "would deserve to be discussed much more thoroughly than I can do here" (p.153-154). Such a thorough discussion is exactly what he provided in "Logical concepts and logical inferences". There he intended to show that, contrary to what some philosophers such as Paul Boghossian and Christopher Peacocke have claimed, it is *not* the case that a subject knows the meaning of a logical constant if and only if she accepts a certain set of logical rules of inference. First of all, being disposed to use a logical constant according to certain inference rules is not sufficient to know its meaning. According to Casalegno, if this were the case then it would follow that a speaker is entitled to assert a logically complex sentence only if the sentence logically follows from some finite set of logically simple sentences the speaker accepts. But this is clearly false, as Casalegno shows by a few simple counterexamples. Rather, "our capacity to describe situations by means of logically complex sentences is a primitive capacity which does not appear to be reducible to anything else and certainly cannot be reduced to the mere readiness to perform logically correct inferences" (p.176).

Casalegno was also sceptical about the other direction of the biconditional, i.e. he doubted that semantic competence about logical words ("or", "every", etc.) *required* acceptance of a certain set of inference rules. In each single case, we may have legitimate doubts that acceptance of a certain rule—say, Modus Tollens—is necessary for competence about a logical idiom ("if...then..."); but aside from that, the main difficulty is that we have not been told what kind of data are supposed to settle the issue. No doubt, from a normally competent speaker we expect a certain amount of inferential ability; this does not entail, however, that we have a sufficiently precise notion of "rule acceptance of which is necessary to possess a given logical concept".

In "Truth and Truthfulness Attributions"—the last paper Casalegno devoted to the semantic notions, truth and reference—he did something entirely different: by an elegant thought experiment he showed why it is *good* for us to have the (intuitive) notion of truth. Availability of the truth predicate, he argued, significantly increases our capacity for acquiring true beliefs, and this is an end in itself. Notably, the truth predicate cannot be surrogated in this function by other predicates such as "verified", or by "truthful" or "reliable" as predicated of a speaker, or by full-fledged justifications of an assertion.

Underscoring the truth predicate's unique utility was Casalegno's last explicit tribute to those puzzling words, "is true". However, even in his very last paper, "Reasons for Belief and Assertion", he meant to show that in laying down the constitutive norm for assertion one cannot replace a truth involving notion (knowledge) by non-truth involving notions such as rationality or reasonableness. I.e., he wanted to show that Timothy

Williamson's knowledge rule, "One must: assert that p only if one knows that p", cannot be satisfactorily replaced by either Igor Douven's rationality rule, "X must: assert that p only if it is rational for X to believe that p", or by Jennifer Lackey's reasonableness rule, "X must: assert that p only if it is reasonable for X to believe that p". Casalegno's criticism is based on careful analysis of alleged counterexamples to the knowledge rule: eventually, he didn't claim to have proved Williamson right but only to have shown his proposal to be better than the competition.

We hope this short introduction to be helpful in giving the reader some idea of the breadth and import of Paolo Casalegno's work, and the centrality of the topics he dealt with for analytic philosophy. To get a feeling of the subtlety, originality and efficacy of his arguments, there is no alternative to reading the papers themselves.

CHAPTER I

APPROACHES TO QUANTIFICATION*

1. Two recent volumes of papers–*Generalized Quantifiers* (Gärdenfors 1987) and *Studies in Discourse Representation Theory and the Theory of Generalized Quantifiers* (Groenendijk, de Jongh & Stokhof 1987)[1]–allow one to form a fairly precise idea of the way in which the topic of quantification in natural language is addressed nowadays within logical semantics. Given its evident importance, this topic has naturally always received some attention, but, in these last few years, studies in the field have blossomed luxuriantly. The stimulus here has been the publication of a number of important works, notable for their richness in original and fruitful ideas.

The pages to follow can be read as a sort of introduction to these two recent volumes. I will illustrate some of the interesting problems that arise in the domain of natural language quantification, together with some of the theoretical approaches that have been proposed—starting with the "theory of generalized quantifiers" ably promoted by Jon Barwise and Robin Cooper, which features already in the titles of the two collections of papers. The picture that I sketch here will not be complete and will not be very thorough in its details, but it should be able to serve as a rough guide for initial purposes, or so I hope.

2. The theory of generalized quantifiers[2] is based on a very simple idea that is not in the least bit new. It is the idea that Frege expressed when he said that quantifiers are "second order concepts," and, translated into extensionalist terms, it comes out as: quantifiers denote sets of sets. This

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* I thank Maria Zinanni and Claudio Saccon for their generous aid in the preparation of this text. Eva Picardi and Ernesto Napoli read and commented on an earlier version of this work-my gratitude goes to them as well.

[1] From this point on I will refer to these two volumes as *GQ* and *SDRTTGQ*.

[2] The best introduction to the topic remains Barwise-Cooper 1981. For a synthetic presentation of the theory's basic concepts, see Sandri 1983 and the first chapter of van Benthem 1986. For a broad overview that takes into consideration numerous developments and applications, see Westerståhl 1986.

point might be obscured somewhat by the fact that elementary logic texts treat the symbols $\forall$ and $\exists$ syncategorematically, and do not endow them with any semantic values of their own. However, a moment's reflection suffices for it to become completely evident. To assert the truth of $\forall$ X φ– given a universe of discourse U and relative to a pre-established interpretation of the basic symbols–is to state that every element of U satisfies φ, or in other words that the set $[[\varphi]]$ of elements of U satisfying φ coincides with U itself, or in other words that $[[\varphi]] \in \{U\}$; and to assert the truth of $\exists$ X φ is to assert that some element of U satisfies φ, or in other words that $[[\varphi]]$ is not empty, or in other words that $[[\varphi]] \in \{X \mid X \subseteq U \;\&\; X \neq \varnothing\}$. In light of these obvious considerations, we can decide (departing from the letter, but certainly not from the spirit of the presentation in standard texts) to assign semantic values to $\forall$ and $\exists$ themselves and to posit that $[[\forall]] = \{U\}$ and $[[\exists]] = \{X \mid X \subseteq U \;\&\; X \neq \varnothing\}$. Following which, the truth conditions of sentences of the form Q X φ with Q = $\forall$ or Q = $\exists$ can be expressed in a unitary way via the following clause:

(1) Q X φ is true (given the universe of discourse U and relative to the pre-established interpretation) if and only if $[[\varphi]] \in [[Q]]$.

Now, once it has been established that the universal and existential quantifiers can be seen as denoting sets of sets, the question naturally arises if the notion of a quantifier can be generalized–that is, if one can introduce other expressions denoting sets of sets that would be of interest to the logician. The first to pose this question–with essentially logico-mathematical concerns–was Andrzej Mostowski in the fifties [3], who launched a research program that would subsequently undergo important developments. Barwise and Cooper, for their part, stress that the generalized notion of a quantifier is not only of interest to mathematics but also relevant for the logical analysis of natural language: according to these two researchers, all the expressions that linguists call N(oun)P(hrase)s deserve to be treated as generalized quantifiers. In fact, this is not a new idea either–it figures already in Montague 1973. The originality of Barwise and Cooper's work lies in the way in which they exploit this idea. But before entering into the heart of the matter, a couple of clarifying remarks are in order.

It is sometimes said that the symbols $\forall$ and $\exists$ roughly correspond to the words *every* and *some*. Now, *every* and *some* are not NPs: they give rise to an NP only once they are combined with an N(oun) (*every man,*

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[3] Cf. Mostowski 1957.

some book). The equation "quantifiers = NPs" might thus seem odd; for an instant, one might even entertain the idea of replacing it with the equation "quantifiers = DET(erminer)s" (where DET is the precise syntactic category that the words *some* and *every* belong to). So let us try to elucidate this point. The fact is that it is only in a loose sense that $\forall$ and $\exists$ "correspond" to the words *every* and *some*. It would be more accurate to say that they correspond to those NPs of the form *every* N and *some* N in which N denotes the universe of discourse–for example, *every thing* and *some thing.* Indeed, if we articulate our analysis of natural language in such a way as to have [[*every thing*]] = {U} and [[*some thing*]] = $\{X \mid X \subseteq U \ \& \ X \neq \varnothing\}$, and if we stipulate moreover that for sentences of the form NP V(erb)P(hrase) with NP = *every thing* or NP = *some thing* the following clause applies (in obvious analogy with (1)):

(2) NP VP is true if and only if [[VP]] $\in$ [[NP]],

we arrive at the correct truth conditions for the sentences under consideration, abstracting away from some exceptions.[4]

But how should we treat an NP of the form *every* N or *some* N when [[N]] $\neq$ U? As is well known, one of Frege's intuitions was that anything that can be expressed with NPs of this kind can equally be expressed by using the terms *every thing* and *some thing* (or by using $\forall$ and $\exists$, if we prefer a transcription into symbols). For example,

(3) *every man is mortal*

is equivalent to *every thing, if it is a man, is mortal,* and

(4) *some book is boring*

is equivalent to *some thing is a boring book*. However, it is in no way essential to proceed by way of these paraphrases. Nothing prevents us from treating *every man* and *some book* as quantifiers in their own right and imagining that [[*every man*]] = $\{X \mid X \subseteq U \ \&$ [[*man*]] $\subseteq X\}$ and [[*some book*]] = $\{X \mid X \subseteq U \ \&$ [[*book*]] $\cap X \neq \varnothing\}$, in which case (3) and (4) are true if and only if [[*mortal*]] $\in$ [[*every man*]] and [[*boring*]] $\in$ [[*some book*]] respectively. In general, we can posit that [[*every* N]] = $\{X \mid X \subseteq U \ \&$ [[N]] $\subseteq X\}$ and [[*some* N]] = $\{X \mid X \subseteq U \ \&$ [[N]] $\cap X \neq \varnothing\}$;

[4] I have in mind here cases where the subject NP falls within the scope of an NP contained within the VP, as well as the cases discussed in the two sections to follow.

once we do this, we find that the truth conditions of a sentence NP VP with NP = *every* N or VP = *some* N are just as clause (2) predicts.

At this point, the reader should be able to imagine how this kind of analysis could be extended further to NPs containing DETs different from *every* and *some*. Take for example the DETs *no*, *five*, *most*. Given the above discussion, it shouldn't come as much of a surprise that we can assign the following values to NPs containing these DETs: $[[\textit{no}\ \mathrm{N}]] = \{X \mid X \subseteq U \ \&\ [[\mathrm{N}]] \cap X = \varnothing\}$, $[[\textit{five}\ \mathrm{N}]] = \{X \mid X \subseteq U \ \&\ |[[\mathrm{N}]] \cap X| = 5\}$, $[[\textit{most}\ \mathrm{N}]] = \{X \mid X \subseteq U \ \&\ |[[\mathrm{N}]] \cap X| > |[[\mathrm{N}]]-X|\}$[5] Naturally, what justifies these choices is the fact that the truth conditions of sentences containing the NPs in question can then be derived correctly again from the schema in (2).

Now let us ask: what semantic value ought we to attribute to a DET D if we want to obtain the denotation of an NP of form D N compositionally on the basis of [[N]] and of [[D]]? The answer is obvious: since [[N]] is a subset of U and since [[NP]] is a set of subsets of U, [[D]] must be a function that maps each subset of U to some set of subsets of U.[6] For example, [[*every*]] will be the function that maps any $Y \subseteq U$ to the set $\{X \mid X \subseteq U \ \&\ Y \subseteq X\}$, [[*some*]] will be the function that maps any $Y \subseteq U$ to the set $\{X \mid X \subseteq U \ \&\ Y \cap X \neq \varnothing\}$, [[*most*]] will be the function that maps any $Y \subseteq U$ to the set $\{X \mid X \subseteq U \ \&\ |Y \cap X| > |Y-X|\}$, etcetera. In general, [[D]] will be defined in such a way that, for any N, [[D N]] = [[D]] ([[N]]).

One last remark before we proceed to more substantial issues. At first glance it might seem that something has to let proper names wiggle out somehow from the quantificational treatment that NPs are subject to: after all, we are used to thinking of proper names as denoting not sets of sets, but rather mere individuals. But here an old trick of Montague's comes to the rescue: if a is the individual named by the name ν, we can stipulate that ν denotes the set of sets containing a. For example, the proper name *Emily Dickinson* can be viewed as denoting the set of sets containing Emily Dickinson–and in that case, parallel with what we have seen, we can arrive at the correct truth conditions of a sentence like *Emily Dickinson is well-known* by saying that the sentence is true if and only if [[*well-known*]] ∈ [[*Emily Dickinson*]].

[5] I use the notation |Y| to indicate the cardinality of the set Y.

[6] It would actually be more accurate to say that every DET is associated with a functional F that applies to any structure <U, [[ ]]> and yields a function from *P*(U) to *P*(*P*(U)). But in the text I will try to keep to a level of maximal simplicity, at the cost of some approximation.

And we come at last to the crucial question: is there really some profit to be made by viewing NPs as quantifiers? Does this point of view genuinely lead to a better understanding of the logical structure of natural language? Going by Montague 1973 only, one would be tempted to give a frank "no." Montague's prevailing concern was with compositionality, understood in a certain austere way: for Montague, expressions of the same syntactic category are to be matched at the semantic level with entities of the same type. It was in order to keep to this principle that Montague resorted to the trick we mentioned just above, which raises the type of the entities denoted by proper names. As for the remaining NPs (and here the only ones included in the English fragment that Montague studied were NPs of the form *every* N and *a*(*n*) N, together with definite descriptions), the choice to treat them as sets of sets[7] seems like nothing more than a statement of the obvious. Well, as I mentioned earlier, the essence of Barwise and Cooper's originality is that, in an apparently obvious idea, they perceived surprising new possibilities for development. Barwise and Cooper limit their attention to simple linguistic constructions –for instance, they put aside issues concerning intensional contexts. But to compensate, rather than restricting themselves to a narrow class of NPs as Montague did, they undertake a systematic investigation of all of the NPs admitted by natural language. This enlarged perspective allows them to identify large-scale regularities that, surprisingly, turn out to be describable (and perhaps even explainable) once reference is made to structural properties of the set-theoretic objects associated with NPs and DETs. More precisely: Barwise and Cooper show how, once DET and NP denotations are characterized in the way we have seen, it is possible to (I) formulate restrictive conditions that it seems that the DETs and NPs of any natural language must satisfy ("semantic universals"); (II) describe linguistically significant classes of DETs and NPs (that is, classes of DETs and NPs that behave in a uniform way with respect to various linguistic phenomena–and the concern here is not only with behavior specific to individual languages but also with behavior across the range of possible human languages).

I will illustrate point (I)[8] with an example that is very simple but not devoid of interest. We say that a DET D is *conservative* when, for any X, $Y \subseteq U$, $Y \in [[D]](X)$ if and only if $X \cap Y \in [[D]](X)$. To get a sense of

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[7] In reality, as Montague wishes to deal with intensional contexts as well, he is forced to treat NPs as denoting properties of properties rather than sets of sets. But we can abstract away from this further complication here.

[8] For a presentation and critical discussion of some of the semantic universals proposed by Barwise and Cooper, see Delfitto 1986.

this definition, think about the equivalence of sentences like *Every man is mortal* and *Every man is a mortal man*, *No actor is shy* and *No actor is a shy actor*, *Most dogs are faithful* and *Most dogs are faithful dogs*: these equivalences testify to the conservativity of *every*, *no* and *most*. One of the semantic universals proposed by Barwise and Cooper is the following: in any natural language, the DETs are all conservative. The reader can consider those languages known to him, and verify to what extent they conform to this condition, running through the various DETs in the inventory of each. It isn't easy to find counterexamples to the conservativity universal. One might think about *only*: *Only Japanese tourists visit the Louvre* is not equivalent to *Only Japanese tourists are Japanese tourists who visit the Louvre*. But it is doubtful that *only* is a DET, for reasons we will not go into here.[9]

I won't dwell further on the topic of semantic universals, in part because I don't wish to go into overly technical details, in part because it seems to me that this aspect of Barwise and Cooper's work is significant more for its methodological implications than for the intrinsic interest of the specific hypotheses presented. However, I would like to mention that, among the articles in *GQ* and *SDRITQG* that talk about semantic universals, Keenan 1987a merits particular attention. Many of the universals proposed by Barwise and Cooper concern simple DETs, that is, those DETs that are not built out of other DETs[10]. (A very elementary example: in any natural language, the simple DETs are not trivial. That is, we do not find any DET D for which we would say that [[D]] maps any $Y \subseteq U$ to $\varnothing$, nor do we find any DET D for which we would say that [[D]] maps any $Y \subseteq U$ to the set of all subsets of U. For these kinds of DETs, the truth of D N VP would never depend on the N and VP chosen: in the first case, D N VP would invariably be false, and in the second case it would invariably be true.) Now, Keenan in his article sets out a very general picture in which these conditions find a natural place. Keenan distinguishes between categories that are *lexically free* and those that are not. In the case of lexically free categories, there is no principled distinction to be made between denotations that simple elements of the

[9] The so-called Keenan-Stavi Theorem–one of the most well-known contributions to the theory of generalized quantifiers–relates to the notion of conservativity (cf. Keenan and Stavi 1986). Keenan and Stavi take their result to show that, at least in a certain sense, all conservative DETs are expressible in natural language. For a concise presentation of the theorem and for a justified invitation not to interpret it as being stronger than it is, see van Benthem 1986, pp. 10-11.

[10] For example, *all*, *three* and *four* are simple DETs, while *not all* and *three or four* are not.

category can have, on the one hand, and denotations that complex elements of the category can have, on the other. By contrast, in the case of categories that are not lexically free, simple elements can have only some of the denotations available to complex elements. Against this background, the category of DETs and the category of NPs would be seen as categories that are not lexically free, and this would justify the possibility of formulating semantic universals for these categories of the kind we have touched on. Keenan moreover attempts to explain why certain categories are lexically free and others are not: here we find the thesis that the lexically free categories are the "small" ones, those matched with a small set of potential denotations. I am not sure that Keenan's reasoning coheres perfectly, but the attempt deserves appreciation.

I pass now to the second of the two points I mentioned above: the use of the conceptual apparatus of the theory of generalized quantifiers in order to characterize linguistically significant classes of NPs. Here a particularly effective example[11], one that I think is worth some discussion, concerns sentences of the form *there is/are NP*. It is well known that, in contexts of this kind, certain NPs are admissible and others are not. Thus, the sentences

> (5) *there is a boy / are some (ten, less than ten, more than ten, several, an odd number of) boys in the garden*

are fully acceptable, but the same is not true of the following sentences:

> (6) *there is every (the) boy / are all (most, one half of the) boys in the garden.*

The question is: is there a way to distinguish the two groups of NPs, other than by pure and simple enumeration? Before Barwise and Cooper, linguists had attempted to answer this question with the suggestion that "indefinite" NPs may appear in *there*-sentences but "definite" NPs may not. The trouble was that these terms were often used without any satisfactory definition. G. Milsark wrote in this regard:

> The notion "definite" [is] a notion whose status in linguistic theory is anything but clear. The term has been used for generations in the pedagogy and scholarly description of many of the Indo-European languages, but within that tradition it is usually used only in discussing the overt formal

[11] Not the only one: another nice example involves the use of the notion of monotonicity to characterize the distribution in English of words like *any* and *ever*. Moreover, this idea antedates Barwise and Cooper's work: cf. Ladusaw 1979.

> contrast between "definite" and "indefinite" determiners such as English *the* and *a/an*. In philosophical logic, the related notion "definite description" has a similarly narrow scope, referring really only to singular nominal expressions introduced by *the* and their logical equivalents.
>
> Linguists, on the other hand, tend to take the term "definite noun phrase" in a broader sense […] As far as I know, the only coherent motivation that has ever been given for the inclusion of [...] NP types under the rubric "definite" has concerned similarity of distribution. [...] Clearly, more than this is needed, however. (Milsark 1977, p. 5)

In the 1977 article from which this passage is taken, Milsark strives to remedy the situation. He attempts to characterize the NPs that can occur in *there*-sentences on the basis of explicit criteria that can be applied with some generality. But his analysis–which is conducted entirely in informal terms–cannot be said to be fully satisfying either. Despite his good intentions and despite the insightfulness of some of his observations, Milsark's discussion leaves various points shrouded in obscurity.

Things finally change with Barwise and Cooper. Using very modest formal equipment, they address the issue with perfect clarity and complete rigor. Let us say that a DET D is *positive strong* if, for every universe of discourse U and every $X \subseteq U$, $X \in [[D]](X)$, and that it is *negative strong* if, for every universe of discourse U and every $X \subseteq U$, $X \notin [[D]](X)$. A DET is *strong* if it is positive or negative strong, and is *weak* if it is not strong. Now, Barwise and Cooper's conjecture is that sentences of the form *there is/are* NP admit only NPs with weak DETs. For a first test of the adequacy of this proposal, consider again examples (5) and (6): a moment's reflection suffices to convince oneself that indeed the DETs in (5) are weak, while the DETs in (6) are strong. As for *why* these two classes differ in their behavior, Barwise and Cooper propose the following. A sentence of the form *there is/are* D N is interpreted as expressing that $U \in [[D\ N]]$ (where U is, as usual, the universe of discourse). But it is easy to show that, in the case where D is strong, $U \in [[D\ N]]$ is either a tautology or a contradiction[12]: the corresponding sentence with *there* therefore has no informational value, and this, according to Barwise and Cooper, is the reason why it sounds unnatural.

[12] By conservativity, $U \in [[D]]\ (X)$ if $X \cap U \in [[D]]\ (X)$ if $X \in [[D]]\ (X)$. Therefore, if D is positive strong, that is if $X \in [[D]]\ (X)$ is always true, then $U \in [[D]]\ (X)$ is also always true; if instead D is negative strong, that is if $X \in [[D]]\ (X)$ is always false, then $U \in [[D]]\ (X)$ is also always false.

Barwise and Cooper's solution is so simple that it could even give the impression of triviality: one might think that, if this is the kind of result that we get out of the theory of generalized quantifiers, then the theory can't be of much interest. But it would be wrong to reason in this way. Natural language semantics aims to be an empirical discipline. What makes a theoretical proposal in this field interesting, then, is not its degree of formal complexity but rather its capacity to account for facts of natural language. If, by adopting a certain point of view and by making use of a certain technical apparatus, we find that we can explain the facts of language in a simple way, then we can conclude that this point of view and these technical resources were well chosen.

Now, to say that Barwise and Cooper's solution is simple, clear and precise is certainly not to say that it is perfect, or that it leaves no room for improvement. The advantage of having a hypothesis that is formulated in simple, clear and precise terms is that one can perceive its implications more easily and see how to test it. It is unsurprising therefore that some researchers have returned to the problem of *there*-sentences convinced that they could do better than Barwise and Cooper. This is the case for Johnsen 1987 in particular. According to Johnsen, an NP can occur in sentences of the form *there is/are* NP[13] only if contains an *intersective* DET, that is, a DET D such that, for any X, Y $\subseteq$ U, to say that Y $\in$ [[D]] (X) is to say that Y $\in$ [[D]] (X $\cap$ Y). Keenan 1987b's analysis is still more interesting. Among other things, Keenan strongly–and not implausibly–criticizes Barwise and Cooper's idea (adopted by Johnsen as well) that the unacceptability of certain sentences can be explained on the basis of the triviality of their informational content. A detailed discussion of these developments would certainly be worthwhile but here I will not go beyond this brief mention of their existence.

3. For all its merits, the kind of analysis defended by Barwise and Cooper 1981 is far from solving all the problems related to quantification in natural language. Further tools are needed, for example, to treat so-called donkey-sentences, that is, sentences like

(7) *every farmer who owns a donkey beats it*[14]

[13] To be precise, Johnsen considers not only *there-sentences* of this kind, but also sentences like *there arrived some men at the airport*. Higginbotham 1987 also concerns himself with sentences of this sort.

[14] Examples of this kind were first brought up by Geach 1962.

where the relevant interpretation is one on which the pronoun *it* takes *a donkey* as its antecedent. That a sentence of this kind should create difficulty might puzzle those who view the logical analysis of language as an enterprise that consists in taking complete sentences and paraphrasing them with symbolic terms (an exercise that is not always trivial but that is of modest theoretical relevance). After all, any student who has a modicum of familiarity with logical notation is capable of proposing the following translation for (7) after a bit of thought.

(8) $\forall x \forall y\ ((\mathit{farmer}(x)\ \&\ \mathit{donkey}(y)\ \&\ \mathit{own}\ (x, y)) \rightarrow \mathit{beat}\ (x, y))$.

Where is the problem, then? The problem arises because the goal of logical semantics is not to analyze individual sentences considered in isolation from all other sentences, but rather to derive the logical forms of sentences using general principles that can be applied in a systematic way. It is from this standpoint that (7) poses difficulty. If we try to analyze (7) while keeping to the Fregean recipe for the translation of sentences containing NPs of the form *every* N and *an* N, we obtain something like

(9) $\forall x\ ((\mathit{farmer}(x)\ \&\ \exists y\ (\mathit{donkey}(y)\ \&\ \mathit{own}\ (x, y))) \rightarrow \mathit{beat}\ (x, z))$

which is certainly not equivalent to (8) and which is glaringly inadequate. Notice in particular that in (7) (on the interpretation that interests us) the pronoun *it* is anaphorically related to the NP *a donkey*, but in (9) the variable z–which ought to correspond to the pronoun–is free. In a moment of scant lucidity, one might think about improving (9) by replacing z with an occurrence of y, in the illusion that the variable corresponding to the pronoun in (7) would then end up bound by the existential quantifier; but this move would obviously be futile, as this new occurrence of y would remain outside the field of action of $\exists$. The problem that we are faced with is thus the problem of identifying general principles for arriving at a sentence's logical form that can do two things at once: on the one hand, in "normal" cases, they should yield results equivalent to those we get out of the Fregean recipe for translating NPs with *every* and *a* in terms of $\forall$ and $\exists$; on the other hand, they should also justify the attribution of a logical form like (8) to a sentence like (7). And the theoretical framework presented in § 2 does not seem to be of any help to us here.[15]

[15] Donkey-sentences also constitute a problem for the *binding theory* as formulated within Chomskian linguistics. Here the difficulty arises from the fact that the pronoun in a donkey-sentence is not c-commanded by its antecedent at the level of *S-structure*. For a recent discussion of this issue, see Reinhart 1987.

An ingenious attempt at a solution was made by Hans Kamp[16]. Kamp's solution is seated within an approach to semantics that is original in many ways and unquestionably deserving of attention: this approach is the Discourse Representation Theory referred to in the title of one of the two volumes that we are concerned with in these pages. In Kamp's theory, the ascription of truth conditions to sentences (which are considered not only in isolation but also in the context of sequences that form "discourses") is mediated by the construction of "discourse representations" (DRs), or, more generally, by the construction of "discourse representation structures" (DRSs). Kamp claims that the procedures for constructing DRs mirror the mental processes that take place in an individual who is actually interpreting a discourse. The sentences of a discourse get taken into consideration one after the other, in the order in which they are presented. Moreover, the treatment of each individual sentence is not compositional: while traditionally one seeks to determine a sentence's truth conditions on the basis of the semantic values assigned to the expressions that make it up, in the case of DR construction one starts from the complete sentence and one breaks it down progressively into parts. This process gradually turns out the informational content of the sentence in the form of clauses that, taken all together, furnish a sort of picture of a possible state of affairs. If this picture happens to be compatible with reality, the sentence that served as the starting point is true; otherwise, it is false.

By way of example, let us consider a discourse of the simplest kind, one that consists of a single sentence:

(10) *a boy loves a girl*

One of the salient characteristics of the generation procedure for DRs is that every occurrence of an NP of the form *an* N entails the introduction of a corresponding "discourse referent." In practice, one can think of a discourse referent as a free variable. Since (10) contains two occurrences of NPs of the relevant form, a DR for (10) will have to contain two discourse referents, say x and y. Beyond this, a DR like this will have to contain the following clauses (which result from the process that breaks (10) down into informational content and which are not further reducible):

[16] Cf. Kamp 1981. A related analysis of donkey-sentences was proposed by Irene Heim in Heim 1982. Unfortunately this text has remained unpublished, but see Heim 1983. [*Translator's note*. The dissertation by Irene Heim that Casalegno refers to here was subsequently published as : Heim, I. 1988, *The Semantics of Definite and Indefinite Noun Phrases*, Garland Publishing (*Outstanding Dissertations in Linguistics* Series), New York.]

boy (x), *girl* (y), *love* (x, y). These three clauses taken together depict, so to speak, a possible state of affairs. In technical terms, the compatibility of this possible state of affairs with reality (and thus the truth of (10)) can be identified with the existence of a function f from {x, y} to the universe of discourse such that f(x) and f(y) satisfy the conditions expressed by the clauses under consideration.

At this point one might object that all this is quite uninteresting. One might very well have the impression that the foregoing is just a pointlessly laborious way of saying that (10) is true if and only if the open formula *boy* (x) & *girl* (y) & *love* (x, y) is satisfiable–and in that case, why not simply say that (10) can be translated as ∃x∃y (*boy* (x) & *girl* (y) & *love* (x, y))? In fact, the decision to treat NPs of the form *an* N as "corresponding to" free variables has its advantages. A first advantage concerns the possibility of analyzing, within the framework of Kamp's theory, not only individual sentences but also–as we said above–sequences of more than one sentence structured so as to make up a discourse. Let's suppose that (10) is the first sentence of a discourse that continues with

(11) *she hates him*

The most natural reading is the one on which the pronouns *she* and *him* are interpreted as referring respectively, by way of anaphora, to the girl and the boy mentioned in (10). Now, it is very easy to capture this idea in terms of DRs: all one needs to do is to extend the DR previously constructed for (10) by adding the further clause *hate* (y, x). If instead one represents (11) by means of a closed formula in which the variables are existentially quantified, it becomes more difficult to account for the discourse constituted by (10) and (11) (presumably one would represent the entire discourse by means of a single closed formula that would *not* contain as a subformula the formula used to represent (10) in isolation). A second advantage of the treatment of NPs with the indefinite article in terms of free variables is that this allows us to solve the problem we started from, namely that of sentences like (7). But to see how, one needs to know something first about the way in which NPs of the form *every* N are treated in Kamp's theory. If (a discourse contains) a sentence (which) contains an NP of this form, the analysis in this case provides for the construction not merely of a single DR, but rather of a DRS–that is to say, a set of suitably ordered DRs. More specifically, when one has a sentence of the form *every* N VP, one has to introduce two distinct DRs, call them DR_1 and DR_2, where one corresponds to N and the other to VP. DR_1 and DR_2 are conceived in such a way that the starting sentence is true if and only if every assignment of values to the discourse referents contained in

DR_1 that satisfies DR_1 is extendable to some assignment of values to the discourse referents in DR_2 that satisfies DR_2. An elementary example: in the DRS associated with

(12) *every farmer is happy*

DR_1 and DR_2 will consist of the clauses *farmer* (x) and *happy* (x), respectively. Now for (7). In the case of (7), DR_1 is constructed in the following way: we begin by generating the clauses *farmer* (x) and *own a donkey* (x); subsequently, in accordance with the idea that NPs introduced by the indefinite article have to be treated in terms of free variables, the second of these two clauses gets broken down into *donkey* (y) and *own* (x, y). As for DR_2, it will be constituted of the single clause *beat* (x, y)–what is essential here is that the discourse referent introduced into DR_2 that corresponds to the pronoun *it* (y in the case at hand) is the same as the discourse referent introduced into DR_1 that corresponds to the NP *a donkey*. Now, let us ask with regard to a DRS constructed in this way: when will it be the case that every valuation of DR_1's discourse referents satisfying DR_1 can be extended to a valuation of DR_2's referents that satisfies DR_2? Since in this case DR_2 doesn't contain any discourse referent that isn't already in DR_1, we can reformulate this question as follows: when will it be the case that every valuation satisfying DR_1 satisfies DR_2 as well? The answer is obvious: this will happen precisely when (8) is true. But (8), as we pointed out right at the outset, constitutes an adequate translation of (7). Kamp's analysis of (7) therefore arrives at the right result.

This presentation was rather rough–and too much so, of course, to allow the reader to appreciate all the merits of Discourse Representation Theory. But on reading the articles collected in *SDRTTGQ* and *GQ*, one can easily see how influential Kamp's ideas have been. Even if only two authors–Ewan Klein 1987 and Henk Zeevat 1987–choose to work directly within Kamp's framework, there seems to be a general conviction that this theory furnishes an adequate solution to the problem posed by donkey-sentences (and other problems as well). There are some attempts at reformulation, among which the most interesting is unquestionably Barwise 1987. Barwise's goal in this article is to show how one can refashion what Kamp has done in a way that respects the principle of compositionality, without recourse to DRs and without departing very far from the spirit of the "theory of generalized quantifiers"[17]. While the

[17] As the problem of donkey-sentences falls outside the framework outlined by Barwise and Cooper, the reader might wonder if this fact constitutes a "rebuttal" to

article deserves a detailed examination, I will abstain from this in order to avoid an overly technical discussion. One piece of advice to the reader: before taking a stab at Barwise's text, it might be useful to read Mats Rooth's contribution (Rooth 1987) by way of introduction. Rooth presents a formalism that is analogous but much simpler.

One issue that is not addressed adequately either in *SDRTTGQ* or in *GQ*, and which however ought to be discussed thoroughly, is the issue of how to extend Kamp's theory to NPs that contain DETs other than *every* and *a*. Obviously, *every* and *a* are not the only DETs that can appear in donkey-sentences; extending the theory is therefore an inescapable duty. But performing this task requires untangling some knotty issues. I think it is worth dedicating a few words to this.

A preliminary problem first. It is well known that NPs introduced by certain DETs can't serve as antecedents for a donkey-anaphor. This is the case for NPs of the form *every N*: notice, for example that in

(13) *a gourmet who knows every neighborhood restaurant visits it regularly*

the pronoun *it* cannot take *every neighborhood restaurant* as its antecedent (and in fact Kamp's theory correctly excludes this possibility). But what general distinction can one draw between those NPs that can serve as

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the theory of generalized quantifiers. It is worthwhile emphasizing that it does not. The existence of donkey-sentences does show (as do the facts we will take up in § 4) that the procedure that Barwise and Cooper consider for evaluating sentences is applicable only within a limited domain-and that, if we assume that an expression's denotation has to include everything relevant for determining the truth conditions of all the sentences in which the expression appears, then we probably have to conclude that strictly speaking NPs do not "denote" sets of subsets of the universe of discourse. But this ultimately has little importance. The fact remains that, by associating NPs with sets of sets in the way we have seen and by then introducing simple criteria for classifying these set-theoretic objects, Barwise and Cooper manage to account for significant regularities within individual languages and even across all natural languages; this is proof enough that their point of view is valuable and fruitful.

This said, it should be added that in any event, when one looks at the current literature on quantification, one finds it rather fragmentary: the theoretical approaches to quantification that exist are heterogeneous and cannot directly be compared with one another (this is the case precisely for Barwise and Cooper's theory and Discourse Representation Theory), and one perceives the lack of a single uniform theoretical framework capable of integrating these different approaches. The exercise in formalization that Barwise undertakes in the work cited just above can be seen as an attempted step towards achieving just such an integration.

antecedents to pronouns in donkey-sentences and those that can't? Even if researchers have by no means neglected this question, I do not see that a persuasive answer has been given as yet. It is a widespread belief[18] that the class of possible antecedents to donkey anaphors coincides with the class of indefinite NPs–those which can occur in *there*-sentences and to which, as we have seen, the theory of generalized quantifiers seeks to provide a rigorous characterization. But is this really the case? I have my doubts. It seems to me, for instance, that one can find donkey anaphora with an antecedent of the form *all the* N, and an NP of this kind is certainly no more indefinite than an NP of the form *every* N. As evidence, consider the contrast between (13) and the sentence obtained from (13) by substituting *all the neighborhood restaurants* for *every neighborhood restaurant* and by putting the pronoun in the plural form: unlike (13), this new sentence can be read perfectly well as a donkey-sentence. Or take the sentence

(14) *every man who seduces all the women he meets treats them with little respect.*

Here too, it seems to me that nothing prevents us from interpreting the sentence in donkey style. (Naturally, if what I am saying is correct–that is, if the DETs *every* and *all* behave differently with respect to the phenomenon of donkey anaphora–the consequences of this go beyond the simple fact that the class of NPs capable of serving as donkey anaphor antecedents cannot be identified with the class of indefinites. It also follows that it is impossible to characterize this class using the criteria available within the theory of generalized quantifiers: after all, from the point of view of that theory, there is no difference whatsoever between *every* and *all* !) I will not dwell further on this point because I am not able to propose any hypothesis about what the right distinction is, and a few more rounds of the example-counterexample game would probably be as inconclusive as they would be easy to play. I am convinced that the issue requires further study, however.

Apart from this preliminary problem, there are some more specific problems that one runs into when one sets about extending Kamp's theory. We have seen that Kamp's analysis of a sentence like (7) is based on two assumptions: (I) NPs of the form *an* N do not have, so to speak, any quantificational value of their own–in the construction of a DRS, every occurrence of an NP of this kind leads to the introduction of a free variable, and it is only on the basis of the context that one can establish whether in the end this variable will be interpreted as if it were bound by an

[18] See for example Higginbotham 1987 and Reinhart 1987.

existential quantifier or in some other way instead; (II) by contrast, NPs of the form *every* N have a quantificational value of their own–it is as though *every* were associated with a universal quantifier of a kind that is sometimes called “unselective,” that is, a quantifier that can act on several variables at once, including any around that correspond to NPs of the form *a* N. Now, it isn’t clear how to extend (I) and (II) to the analysis of donkey-sentences in which we find DETs other than *every* and *a.* Consider

(15) *every farmer who owns at least five donkeys beats them*.

The temptation here is to say, on analogy with (I), that the NP *at least five donkeys* does not have any quantificational value of its own. And at first sight it seems reasonable to go about constructing the DRS for (15) by introducing a clause like *at least five donkeys* (x) to correspond to this NP, where x is a variable over sets or “groups” of individuals[19]. The trouble is that there also exist sentences like

(16) *at least five farmers who own a donkey beat it*.

In this case it seems impossible *not* to attribute a quantificational value of its own to the NP with the DET *at least five*. We thus face a dilemma: do NPs with the DET *at least five* have a quantificational value of their own, or don’t they? At this point, one could look for a way out by hypothesizing that NPs containing certain DETs behave on some occasions like true quantifiers and on other occasions in another way. In fact, an idea of this kind is suggested in *GQ* by Sebastian Löbner 1987 and by Jan Lønning 1987[20] (for reasons not directly connected with the problem of donkey-sentences). However, I am rather skeptical about the actual workability of this solution. Löbner’s and Lønning’s arguments are not very persuasive, and on top of this they do not offer an adequate formal implementation of their proposal. Worse still, there are complicated examples of donkey-sentences (like *if more than five farmers who own a donkey beat it, Pedro reports them to the Animal Protection Agency*) in which the reasons for and against the attribution of a quantificational value to a single occurrence of an NP seem to cancel each other out (or, better, sum together).

A further difficulty concerns sentences of the following kind:

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[19] The introduction of groups of individuals into the universe of discourse seems to be necessary in any event in order to account for the so-called “collective reading” of quantifiers–see § 4.

[20] The same idea already appears in Milsark 1977.