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Absolute Generality
Towards a Modal Approach

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Summary

INTRODUCTION

This dissertation defends the idea that absolute general discourses are possible, i.e. sometimes the quantifiers in our sentences range over absolutely everything. However, it also defends the idea that there are indefinitely extensible concepts, i.e. concepts associated with an indefinitely extensible sequence of objects falling under them. *Prima facie,* the two ideas seem incompatible and, as a matter of fact, indefinite extensibility has been used to challenge absolute generality[1]. The reason of this apparent incompatibility is that the standard theory of quantification requires a domain for the quantifiers to range over: if the quantifier is absolutely general, then its domain[2] of quantification must contain absolutely everything. But if concepts as those of set, ordinal, cardinal etc. are indefinitely extensible, then also the concept of object (or the concept of 'being self-identical') is indefinitely extensible, since sets, ordinals and cardinals are objects. But this implies that no all-inclusive domain exists: given a domain, it is possible to find new objects not present in it. My strategy to argue for absolute generality and indefinite extensibility consists in arguing that standard quantification is not the only form of generality. One of the main thesis of the whole work is the claim that there exists a form of generality which behaves differently from standard quantification. This generality, which is formally captured by means of a modal approach to quantification, is firstly introduced in chapter 2, and then developed in chapters 6 and 7.

The general context in which this work has been developed is the debate on absolute generality. This is a highly interesting debate on quantifiers: is it possible to quantify over everything? Is there a totally unrestricted quantification over an all-inclusive domain? In the contemporary discussion, one of the first scholar to raise doubts about the possibility of meaningful sentences about everything was Russell. Speaking about the paradoxes (both set-theoretic and semantics) Russell writes:

> Thus it is necessary [...] to construct our logic without mentioning[3] such things as "all propositions" or "all properties", and without even having to say that we are excluding such things. The exclusion must result naturally and inevitably from our positive doctrines, which must make it plain that "all propositions" and "all properties" are meaningless phrases. (Russell [1908], p. 226).

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[1] For instance, Glanzberg [2004], Hellman [2006] and Button [2011] uses ideas connected to indefinite extensibility to challenge the possibility of absolute generality.

[2] As I shall make clear in the following chapters (especially chapter 6), when I say that no all-inclusive domain exists, the word 'domain' denotes either a set or a plurality (in the sense of plural logic).

[3] It is well-known that Russell was very sloppy with the use-mention distinction. Here he says that we should not mention things as "all propositions", but by saying "all propositions" he is just mentioning it! What he should have said is that, although we cannot *use* such expressions, we can certainly mention it.

If we cannot speak of all propositions or all properties, *a fortiori*, we cannot meaningfully speak of everything.

Russell's doubts did not meet many supporters. The standard view during the twentieth century was to regard standard first-order unrestricted quantification as unproblematic. Quine's essay *On what there is* is a case in point: to the question "what is there?" Quine answered 'Everything', where "Everything" is an unrestricted first-order quantifier.

At a naïve sight, it seems weird to question the possibility of absolute general discourses. How is it possible to fail to refer to everything, if I intend to refer to everything? Of course, everybody acknowledges that many occurrences of 'everything' in our sentences are restricted to some particular domain. If I say, "all bottles of beers are empty", it is unlikely that 'all bottles' refers to absolutely all bottles on this earth; rather it is likely it refers to some restricted domain of bottles, i.e. the bottle in my house[4]. But certainly, we can speak of everything, if we intend to. Or at least this seems to be the case. Williamson [2003] fully articulates what he dubs as naïve absolutism, i.e. the intuitive view according to which there are no problem in quantifying over everything. Naïve absolutism is supported by a number of different examples where it seems clear that we manage to achieve absolute generality. For instance, ontological and metaphysical discourses seem to presuppose absolute general claims[5]. Quine's answer above presupposes that 'everything' is to be taken as totally unrestricted. But also, logic requires unrestricted quantification. The laws of logic are valid not just in some restricted domain, but in all domains. Therefore, it seems that we need absolute generality to state them. In any case, there are also examples from ordinary talk. If you deny that there exists something, let's say Pegasus, then you are committed to the sentence 'Everything is such that it is not Pegasus'. Here the quantifier must be taken as absolutely unrestricted, if that sentence has its intended meaning; otherwise, if the quantifier has only a restricted domain, then the truth of that sentence is compatible with the existence of (an object equal to) Pegasus in a broader domain[6].

In addition, standard logic seems to suggest the possibility that unrestricted quantification is present also when the quantifiers are restricted. Suppose you say that 'all dogs bark' or that 'there exist a black swan'. These are examples of restricted quantification: the first quantifier is restricted to the domain of dogs, while the second one is restricted to the domain of swans. These sentences are usually formalized as follows: $\forall x(Dx \rightarrow Bx)$ and $\exists x(Sx \wedge Blx)$, where D =being a dog; B =barking; S =being a swan; Bl =being black. In both cases, the quantifier lies outside the parenthesis, and binds the whole formula inside the parenthesis (it binds, respectively, the formulas $Dx \rightarrow Bx$ and $Sx \wedge Blx$), not only the formulas that respectively say that we are speaking

[4] Stanley and Szabo [2000] provides an insightful analysis of how quantifier domain restriction works in natural language. I shall not bother with this topic in the present work.
[5] For a different opinion, see Glanzberg [2004].
[6] This is a form of the inexpressibility objection, which we have deeply analyzed in chapter 3. More reasons to support the plausibility of unrestricted quantification can be found in Williamson [2003].

of dogs (Dx) and swans (Sx). This standard way of formalization leads quite naturally to think that the range of the quantifier is always everything, and the predicate D and S select a sub-domain of everything. If this is the case, then every restricted quantification should be analyzed as composed by an unrestricted quantifier, and a predicate that restricts its domain[7].

What we have just seen suggests that naïve absolutism enjoys *prima facie* plausibility. However, it has been challenged in many ways. In the literature, there are four big objections that have been raised. They are the objection from paradox, the objection from the indeterminacy of reference, the objection from ontological relativity and the objection from sortal restriction. We shall now briefly explain what these objections amount to.

The objection from paradox. This is the most important objection, and the one more discussed in this dissertation. So, we just outline here his general aspect. The set theoretic paradoxes show that no universal set exists. Because, if it existed, then we could consider the set R of all non-self-membered sets. But if R belongs to itself, it does not belong to itself; if it does not belong to itself, it belongs to itself. We thus have a contradiction. The universal set thus cannot exist. But since standard model theoretic semantics is based on set theory, unrestricted quantification over everything needs the universal set as its own domain of quantification. The non-existence of this set implies that no unrestricted quantification is possible.

A slightly different version of the argument exploits the paradoxes to show that any domain can be expanded. Suppose you consider a domain D, which purports to be the domain of everything (the universal set). Then you can derive Russell's paradox. But at this point you can exploit the paradox to argue that the set R is not one of the objects included in D. Therefore, you can expand D by adding R to it. In this way, you find a more comprehensive domain $D' = D \cup R$. But D was arbitrary. Any domain that presents itself as the all-inclusive domain can be expanded. This is the argument at the root of indefinite extensibility. More on this in chapters 1 and 2.

The objection from indeterminacy of reference. This objection is based on the Löwenheim-Skolem theorem for first-order logic. Since the theorem claims that a first-order theory with an infinite model has models of any other infinite cardinality, and in particular it has a countable model, the objection claims that if we have a first-order theory whose quantifiers are supposed to be absolutely unrestricted, then it is undetermined if they range over everything or over a countable subset of everything. This objection is discussed at length in chapter 5, §1.4. There I argue that the objection raised a big issue for first-orderism, i.e. the thesis that first-order logic is the only legitimate form of logic, and therefore I exploit it as an argument for the appeal of higher-order resources in the absolute generality debate.

[7] In chapter 7 I propose a different interpretation of the quantifiers, closer to the reading of quantifiers in the theory of generalized quantifiers.

The objection from ontological relativity. This objection is based on the idea that ontology is always dependent on a certain conceptual schema. Different schemas give rise to different ontologies, in the sense that each of these ontologies will "carve up" the world in different objects. As a consequence, it is senseless to speak of an all-inclusive domain of objects which is independent from each schema. But unrestricted quantification requires the possibility of referring to this all-inclusive domain of objects. No unrestricted quantification is thus possible, if ontology is relative to a conceptual schema. This sort of neo-carnapian objection has been raised by Hellman [2006]; however, the most detailed development of such a line of thought is given by some recent work of Rayo [2013, 2016]. What it is interesting in Rayo's view is that it combines these ideas within a realistic framework: even if ontology is relative to a schema, reality is not reducible to a conceptual schema[8]. However, I am not going to deal with such an objection. In any case, I think that this objection raises a problem for standard quantification, not for the form of generality I introduce in this work and I claim to be absolute. In fact, this new form of generality allows the existence of absolute general claim without the presence of an absolute domain of quantification; but since the present objection just raises doubts on the existence of an all-inclusive domain, the relativity of ontology from a conceptual schema is compatible with absolute generality in the sense to be made clear below.

The objection from sortal restriction. This is considered to be the weakest objection against absolute generality. The objection claims that we cannot achieve absolute generality because we can only generalize over sortal predicates. Therefore, no general sentence can be about all objects whatsoever. Let me say something about this objection in this place, because I am not going to say anything else on it in what follows. The weakness of this objection derives from the fact that quantification can be employed with trans-sortal categories (and, as noted by Priest 2007, trans-conceptual schemas):

> Thus, we may say that Russell thought of everything. He thought of Cambridge, Whitehead, and the number three, even though these are of different sorts. And he thought of phlogiston, of oxygen, and of the poison oracle, even though these come from different conceptual schemes. 'Anything' can mean anything of any sort, and of any conceptual scheme. (Priest 2007)

I agree with Priest that 'anything' can mean anything of any sort; however, I find his rejection of the objection too quick. His counter-example ('Russell thought of Cambridge, Whitehead and the number three') shows that there are cases where we can generalize over things of different sorts. But a defender of the objection could notice that these cases are those in which we can produce a list of the things on which we generalize, as in Priest's example. When we try to generalize over everything, it is out of our possibility to list the elements over which we generalize, so we must use some concepts to specify the domain of quantification. Here the sortal objection amounts to the claim that we cannot use the concept of 'being an object' to produce such generalization. The reason is that

[8] Different schemas will make legitimate different questions to pose to reality, but the answers are provided by reality.

quantification is intimately connected with identity: quantification presupposes a domain of objects for which we must have a criterion of identity that tells us when (what appears to be) two objects are actually the same object. This criterion serves to distinguish an object from all other objects[9]. However, the concept 'being an object' cannot give us any criterion, exactly because it is not sortal (the fact that different conceptual schemas give rise to different objects shows that the concept of object is not sortal). So, we can generalize only over domains specified by means of sortal concepts, which implies that unrestricted quantification is not possible. Again, the problem is with the necessity of an all-inclusive domain of quantification, which means that this objection is compatible with the different form of generality that we introduce in this work. Whether the objection succeeds is doubtful; in any case, it does not pose any problem for the account of generality defended in this work.

What to do in front of these objections? One possibility is to deny the possibility of absolute generality, and to opt for a relativist position which claims that every generalization is restricted to a less than all-inclusive domain[10]. But this position faces a great problem, the inexpressibility objection (see chapter 3). Moreover, the relativist must also explain what appears to be absolutely general claims as the one we saw above. A standard response makes use of schematic generality (see chapter 4). As argued in chapter 3 and 4, these approaches are not very appealing for a number of difficulties they face, which suggests that we should find a non-naïve absolutist position that manages to defend itself from the previous objections.

The standard non-naïve absolutist position makes appeal to plurals, and so to plural logic. Boolos here is the main author, even if many philosophers have followed him in his plural interpretation of absolutism[11] (Cartwright, Burgess, Oliver and Smiley, the first Rayo, Uzquinano). The plural approach is treated at length in chapter 5, §2, where also some objections are raised against it.

Plural logic can be interpreted as a way of interpreting higher-order logic independently from set theory. A more direct appeal to higher-order logic to save first-order absolute generality is given by Williamson's predicativist approach to higher-order logic. Chapter 5, §3 is entirely devoted to Williamson's proposal, which is at the end dismissed for some deep problems concerning ideological hierarchies.

The position I try to develop in chapters 6 and mostly in chapter 7 is a non-standard form of absolutism. What I argue is that absolute generality is possible even in the absence of a domain (set or plurality) that comprehends everything. My argument

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[9] We can state the point in a slightly different way: standard quantification requires a *definite domain* for the quantifier. But without a criterion of identity for the objects in the domain, it cannot be clear what these objects are, which may also render unclear which objects are present in the domain (as an example, think of a vague case). When this is the case, we cannot say to have specified a *definite* domain.

[10] This position has been defended in different ways by a number of authors. For instance, Grim [1991], Glanzberg [2004, 2006], Hellman [2006], Lavine [2006], Parsons [2006] and Button [2011].

[11] Examples of such philosophers are Cartwright [1994, 2001], Burgess [2004], Oliver & Smiley [2013] chapter 14, Rayo [2006], and Uzquiano [2003, 2006, 2009].

against the existence of a maximal plurality of everything is based on a defense of indefinite extensibility. This defense is developed in three rounds: first of all, in chapter 1 I introduce the idea of indefinite extensibility and I argue that it is the most natural approach to take in front of the set theoretic paradoxes. Secondly, in chapter 5, §2 I criticize the rival plural theory: in particular I show that it is forced to introduce *ad hoc* elements to claim that there are no indefinite extensible concepts. Moreover, at the end of that paragraph I develop an argument based on the idea of 'universal applicability' to give a direct support to indefinite extensibility. However, the most important defense of indefinite extensibility just consists in showing that this very idea is consistent. This amounts to giving a solution to the problem of absolutely general claims over an indefinitely extensible sequence of objects. The third and last part of the defense of indefinite extensibility consists in explaining how generality over such concepts works. This is done especially in chapter 7.

My position with regards to the four objections before is as follows: the third and fourth objections do not seem to apply to it, and therefore they do not constitute a problem. Also, the second objection does not apply, as it is explained in chapter 5, §1.4. The reason is that my theory of concepts is developed in a higher-order framework. Concerning the first objection I believe it works against standard quantification. Consequently, I think that standard quantification is always restricted to a less than all-inclusive domain. My position can be seen as an attempt to show that absolute generality is possible even thought that objection succeeds.

I now provide an abstract of each chapter, and a suggestion of what to read with regards to the reader's time and interests.

Summary of the chapters

Chapter 1: chapter 1 is an introduction to the set theoretic paradoxes and to Basic Law V. It provides a derivation of Russell's paradox from the law, and it discusses all the ingredients present in the paradoxes. Moreover, it presents a detail analysis of the structure of the paradoxes. The general aim of the chapter is to have a look at the different strategies one might take to avoid the antinomies, and to explain why I have opted for the indefinite extensibility's interpretation.

Chapter 2: in the first part of the chapter, after discussing and rejecting Shapiro's and Wright's definition of indefinite extensibility, I propose my own definition which is based on a modal plural logic. The second part of the chapter is devoted to the study of the relationships between indefinite extensibility, the vicious circle and impredicativity. This will give us different important results: 1) impredicative definitions are compatible with indefinite extensibility; 2) in an expanding universe, impredicative definitions must be clearly distinguish from Russell's vicious circle; 3) we will find a more fine-grained classification of the paradoxes than the one individuated in the first chapter, depending on different forms of impredicativity.

Chapter 3: I discuss one of the most important objection against a relativist position in the absolute generality debate. This is the inexpressibility objection, which accuses the relativist of not being able to coherently express her own position. We are going through different formulations of the objection and different replies relativists have given. The general result of the chapter will be that the objection in fact succeeds; however, we shall also arrive at three more particular, but not less interesting results: 1) the objection only depends on denying the possibility of absolute generality, while nothing depends on the fact that this generality is considered to be expressed by an unrestricted quantification or by means of another logical device; 2) that relativism is not coherently expressible does not imply that it is false; 3) a modal version of absolutism – as the one defended in this work - finds itself in a better position with regard to the challenge posed by a certain form or relativism than standard (plural) absolutism.

Chapter 4: In the debate on absolute generality, many authors have defended a relativistic position, namely that quantifiers are always restricted to a less than all-inclusive domain. Consequently, they hold that an unrestricted quantification over everything is not possible. One problem for such a view is the need to explain the apparent absolute generality of logical laws, like $\alpha = \alpha$ or $\sim(\alpha \wedge \sim\alpha)$. The standard response appeals to schemas. In this chapter, I begin by examining the reasons why schematic generality has such a strong appeal in this debate, before raising an objection to show that schemas cannot be a good substitute for quantificational generality. What ultimately the chapter shows is that to express absolute generality over an indefinitely extensible sequence, we need a form of generality that is both open-ended (as schematic generality) and express a proposition with a determined truth-value (as quantificational generality).

Chapter 5: this chapter studies the presence of a first-order totally unrestricted quantification within different logical systems; in particular it focuses its attention on semantic theorizing in the presence of absolute generality. The first part deals with absolute generality within first-order logic: here some arguments are presented to the effect that semantic theorizing in such a setting is very unstable; the second part is devoted the plural approach to the logical antinomies and to absolute generality; the third part is dedicated to Williamson's predicativist approach to higher-order logic, which should guarantee the possibility of unrestrictedly quantifying over everything. Ultimately, this chapter shows that all these three approaches suffer from some deep problems that prevent us to be satisfied with any of them.

Chapter 6: I shall present three different accounts of the Domain Principle and I shall argue that one of them is compatible with the claim that there are indefinitely extensible concepts. This is interesting, because there is a well-known argument by Graham Priest according to which the Domain Principle implies the existence of "absolute totalities" (as the totality of all sets, ordinals, and so on), whose existence is denied by the defenders of indefinite extensibility. Moreover, I shall argue that this account explains why it is possible to have absolutely general claims concerning indefinitely extensible sequences of objects, which means that it is possible to have absolute generality without an all-comprehensive plurality of objects.

Chapter 7: this chapter sketches a theory of concepts that should explain how generalization works in the absence of a maximal plurality of everything. I shall start by introducing this new form of generality and by defending it from some common objections; then I introduce a higher-order modal theory of concepts to study in detail this form of generality. I shall sketch a semantics for a non-modal fragment of the language introduced, which can be seen as an attempt to model the self-referentiality of natural language. I then defend my account from a revenge phenomenon; moreover, I argue in favour of the notion of concept I have been working with. The chapter ends with some considerations on the role of abstraction (and abstraction principles) in my theory of concepts.

Appendix: the aim of this chapter is to examine Dummett's argument for intuitionistic logic in mathematics from the existence of indefinitely extensible concepts. After presenting the argument in detail, we will demonstrate that indefinite extensibility alone does not suffice to establish the conclusion and that the argument requires more and not trivial assumptions to work. We will suggest that Dummett smuggles some constructivist ideas into his interpretation of indefinite extensibility, which have the effect of preventing the argument from being a *new* case for constructivism in philosophy of mathematics.

How to read this dissertation

Each chapter has been thought and developed independently from the others. The reader should thus be able to read any chapter in any order, and still comprehend the whole work. However, I suggest the following reading plan.

If the reader has plenty of time at disposal, then I strongly suggest reading the whole dissertation. However, if the reader's time is very limited, then I suggest reading directly chapter 7. This is far the most important chapter, and the most original part of the work. After that, I suggest reading the first part of chapter 2 concerning the definition of indefinite extensibility, and chapter 6. Both these parts are strongly linked with chapter 7, and in a way, we could say that chapter 7 complete them. At this point, if there is still some time left, I suggest reading chapter 5, and maybe the Appendix. Chapter 1 serves to settle the stage, so the expert reader may skip it. Chapter 3 and 4 develops some interesting considerations that have a certain importance for the main thesis, but they are not essential to fully appreciate its defense.

CHAPTER 1

PRELIMINARIES. BASIC LAW V AND THE ORIGIN OF PARADOX

Introduction

It is well-known that Frege's Basic Law V is inconsistent, giving rise to Russell's paradox within the frame of second order logic. Basic Law V can be stated as follows:

(BLV) $\forall F \forall G(\varepsilon(F) = \varepsilon(G) \leftrightarrow \forall x(Fx \leftrightarrow Gx))$

where $\varepsilon(\ldots)$ is to be read as "the extension of", x is a variable for individuals (first-order objects) and *F, G* are variables for predicates (second-order objects)[12]. BLV is a bi-conditional, and it can thus be factorized into the conjunction of two conditionals:

(Va) $\forall F \forall G(\forall x(Fx \leftrightarrow Gx) \rightarrow \varepsilon(F) = \varepsilon(G))$

(Vb) $\forall F \forall G(\varepsilon(F) = \varepsilon(G) \rightarrow \forall x(Fx \leftrightarrow Gx))$.

In the following I will take predicates as referring to properties or, in a more Fregean way, concepts. The aim of this chapter is to explain why BLV along with SOL are inconsistent and, consequently, to explain what is the root of the set theoretic paradoxes.

§1. The derivation of the contradiction

We shall work within *second-order logic* (SOL, from now on), with identity[13] and BLV[14]. In our vocabulary, we have a primitive functional symbol $\varepsilon(F)$[15] to be read as "the extension of the concept F" and a derived membership symbol $\in$ so defined: $x \in y =_{df} \exists P(y = \varepsilon(P) \wedge Px)$ (informally: x belongs to y if there is a concept P such that y is the extension of P and x falls under P).

Relying only on SOL and the primitive functional symbol $\varepsilon(X)$ (without BLV), by means of a standard deductive system (see for example Uzquiano [2014], pp. 28-29), we can derive what Zalta calls *"the Existence of Extensions",* a principle which says that every concept gets correlated with an extension (which might be empty).

1. $x = x$ Axiom

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12 This is a second order formulation of the law. If we work within first order logic, the law can be expressed by a schema, $\varepsilon(\alpha) = \varepsilon(\beta) \leftrightarrow \forall x(\alpha x \leftrightarrow \beta x)$, where the Greek letters are meta-variables.

13 Strictly speaking, it is not necessary to add this qualification, since in second-order logic we can define identity in the following way: $x = y \leftrightarrow \forall F(Fx \leftrightarrow Fy)$. However, we have preferred to make it explicit because we will use explicit identity in the following derivations.

14 In the derivation of the contradiction I have based myself on Zalta [2015].

15 This functional symbol is a second-order symbol that takes first-order predicates as arguments. We may define a correspondent first-order predicate $Ext(x)$, to be read as "x is an extension", by means of the second-order functional symbol in the following manner: $Ext(x) =_{def} \exists F(x = \varepsilon(F))$.

2. $\forall x(x = x)$ by Universal Generalization
3. $\varepsilon(F) = \varepsilon(F)$ by instantiating x with *ε(F)* in 2.
4. $\exists x(x = \varepsilon(F))$ by $I\exists$
5. $\forall F \exists x(x = \varepsilon(F))$ by $I\forall$.

So, we have just proved – by means of pure logic - that every concept has an extension. This inference might be block in a free logic[16]. However, I do not consider this option an appealing one, since it strikes me as an undeniable fact that once concepts or properties are allowed, then also their extensions must be allowed.

The second step towards the contradiction is the derivation of the *Law of Extensions*. The Law of extensions says that the extension of a concept is given by the objects that fall under it: $\forall P \forall x(x \in \varepsilon(P) \leftrightarrow Px)$. By the laws of SOL we can eliminate both the universal quantifiers and so get: $c \in \varepsilon(Q) \leftrightarrow Qc$, with c and Q arbitrary. We now have to derive this biconditional.

Proof:

(from left to right): suppose $c \in \varepsilon(Q)$. By definition of $\in$, we have that $\exists H(\varepsilon(Q) = \varepsilon(H) \wedge Hc)$. Let's now instantiate H with an arbitrary F: $\varepsilon(Q) = \varepsilon(F) \wedge Fc$. By BLV (Vb) $\varepsilon(Q) = \varepsilon(F)$ implies $\forall x(Qx \leftrightarrow Fx)$, but since the second conjunct is Fc, we have Qc.

(from right to left): assume Qc. By *Existence of Extensions* $(\forall G \exists x(x = \varepsilon(G))$, we get that Q has an extension: ε(Q). By the law of Identity ε(Q)= ε(Q); putting together the two assumptions: $\varepsilon(Q) = \varepsilon(Q) \wedge Qc$. By existential introduction on Q: $\exists H(\varepsilon(Q) = \varepsilon(H) \wedge Hc)$; by definition of $\in$, we get $c \in Q$.

Take notice that in the previous derivation we have used only Vb and not Va.

We can now derive the principle of Extensionality, one of the two axioms of what has been called "naïve set theory". The principle says that if two extensions have the same members, then they are the same: $Ext(x) \wedge Ext(y) \rightarrow [\forall z(z \in x \leftrightarrow z \in y) \rightarrow x = y]$.

1. Assume $Ext(x)$ and $Ext(y)$. By definition of $Ext(x)$ and $Ext(y)$ (see footnote 12), we get $\exists F(x = \varepsilon(F))$ and $\exists G(y = \varepsilon(G))$. If P and Q are arbitrary such concepts, we have that $x = \varepsilon(P)$ and $y = \varepsilon(Q)$.
2. Assume $\forall z(z \in x \leftrightarrow z \in y)$
3. $\forall z(z \in \varepsilon(P) \leftrightarrow z \in \varepsilon(Q))$ by substituting 1 in 2
4. $a \in \varepsilon(P) \leftrightarrow a \in \varepsilon(Q)$ by E∀
5. $P(a) \leftrightarrow Q(a)$ by Law of Extensions
6. $\forall z(P(z) \leftrightarrow Q(z))$ by I∀

[16] A free logic is a logic which allows for empty singular (or plural) terms. Within such a logic we may stop the inference from 3 to 4, because the term "ε(F)" may be empty, that is it may lack denotation. The reason to adopt a free logic is the belief that logic should be topic-neutral and should be applied to evaluate reasoning over objects whose existence we doubt. See Oliver and Smiley [2013], pp. 181-189 for a deep defense of a free logic approach. On the contrary, it follows from the principle of standard logic that there must exist something.

7. $\varepsilon(P) = \varepsilon(Q)$ by BLV (Va). Therefore $x = y$.

The principle of Extensionality expresses the condition of identity between extensions. In contemporary set theory, extensionality is regarded as the condition of identity between sets (Boolos [1971]). In this sense, extensions can be seen as sets. However, while in the iterative conception of set (which lies behind standard Zermelo-Fraenkel set theory), sets are determined 'bottom-up', that is by starting from some elements (or the empty set) and applying the *set of* operation, in Frege's system is the other way around, because extensions are determined 'top-down'. A concept divides the whole universe between the objects that fall under it and the objects that do not fall under it. This way of determining sets has been called the *logical conception of set*. What it is important to underline is the fact that, according to Frege, sets (i.e. extensions of concepts) are determined by concepts, and therefore they inherit the logicality proper of concepts (see Boccuni 2011)[17].

It is now time we derived the contradiction. There are many ways of carrying the derivation; however, we will focus only on one of them (the second derivation in Zalta [2015]). This is – more or less – the derivation to be found in the Appendix of Frege's *Grundgestezte der Arithmetik,* which proceeds by deriving the *naïve comprehension axiom.* This derivation (along with the previous one) has the benefit of showing that naïve set theory (the theory whose axioms are Extensionality and Naïve Comprehension) is derivable in Frege's System.

1. From BLV (Vb) and SOL we derive the Law of Extensions: $\forall P \forall x(x \in \varepsilon(P) \leftrightarrow Px)$ (see derivation above)
2. $\forall x(x \in \varepsilon(F) \leftrightarrow Fx)$ by instantiated 1 with a free variable F
3. $\exists y \forall x(x \in y \leftrightarrow Fx)$ by I$\exists$
4. $\forall P \exists y \forall x(x \in y \leftrightarrow Px)$ by I$\forall$

$\forall P \exists y \forall x(x \in y \leftrightarrow Px)$ is the naïve comprehension principle (NCP), which says that for every property/concept, there is a set (or extension) whose elements are all and only those elements that instantiate that property. At this point we need the property of "not belonging to itself": $\lambda z. z \notin z$. How can we make sure that there is such a property? What we need is a *Impredicative Comprehension Principle for Concepts/Properties*: $\exists G \forall x(Gx \leftrightarrow \phi)$ where ϕ has no free occurrences of Gs. The principle says that for every open formula ϕ there is the correspondent concept (which in turn means that every predicate expresses a concept). Thanks to this principle we know that the Russell's property exists and we can instantiate the NCP with it.

5. $\exists y \forall x(x \in y \leftrightarrow x \notin x)$ by instantiating 4 with the property $\lambda z. z \notin z$. Let's call this set b.
6. $\forall x(x \in b \leftrightarrow x \notin x)$ by instantiating 5 with b

[17] This is the heart of Frege's logicism: concepts are logical entities that determines extensions, which are consequently logical entities. But then numbers are defined by means of extensions, and so they are logical entities too.

7. $b \in b \leftrightarrow b \notin b$ by instantiating the universal quantifier in 6 with b.

From 7 a contradiction is easily derivable.

BLV together with SOL give rise to paradox. Notice that in the derivation of the paradox we used only the Law of Extensions and not the principle of Extensionality. To derive the former, we use Vb: consequently, it is Vb that along with SOL is to be charged of the contradiction. This was already noticed by Frege, who considered Va to be doubtless also after the paradoxes came up.

§2. Whence the contradiction?

The derivation of the contradiction above is built on different principles which we should analyze closer to understand what is going on. First of all, there is SOL, which is essential to the derivation because, as we shall see, within the framework of FOL no contradiction can be derived from BLV; secondly there is the Impredicative Comprehension Principle for Concepts, which allows the formation of the Russell's property; thirdly, there is BLV, which implies "the Existence of Extensions" and "the Law of Extensions".

2.1 Second-order logic

SOL is essential for the derivation of the paradox. In fact, T. Parsons has proved that the system composed by first-order logic (FOL) and BLV is consistent[18]. However, consistency is gained thanks to the loss of expressive power and, consequently, the theory is not strong enough to carry out Frege's logicist project. One might beat the bullet, declared Frege's project unrealizable and charge second-order logic of the origin of contradiction. But this last movement would be too quick. First of all, FOL is a weak logical system and, even though philosophers from Quine on have looked at it as a safe harbor with regards to SOL, it suffers from severe limitations[19]; secondly, this movement would not consider the central fact that SOL alone does not give rise to any contradiction, rather it is the mixture of BLV *and* SOL that is inconsistent. This last point suggests that the culprit is to be found in *their interaction* and not in one of the two horns taken alone.

With regard to FOL, SOL is an extension which adds quantification into predicate position. This means that in a second-order domain there are not only first-order objects, but also second-order objects (sets or extensions), which are the correspondents of the predicates over which the quantifiers ranges. Now, BLV might be seen as a sort of description of the behavior of such entities: this suggests that it is this description upon which the contradiction hangs.

[18] Parsons [1987]. The consistency of BLV within FOL should warn us about putting too much emphasis on the law: there are context in which it does not give rise to any contradiction.
[19] See Chapter 5, §1 where we underline some of these limitations with regard to the possibility of having absolute general first-order quantifiers.

2.2. *Impredicative Comprehension Principle for Concepts/Properties*

The Impredicative Comprehension Principle for Concept/Properties (ICPC) is the following principle:

(ICPC) $\exists G \forall x (Gx \leftrightarrow \phi)$

where ϕ has no free occurrences of Gs. If we substitute the property $\lambda z. z \notin z$ for ϕ, we get Russell's property: $\exists G \forall x (Gx \leftrightarrow x \notin x)$. Consequently, the existence of this property directly depends on ICPC.

Since the open formula ϕ does not have any constraint (apart from not having free occurrences of the predicate over which the principle quantifies), one may ask if this lack of constraints is legitimate. As to each open formula corresponds a predicate in the language, the principle asserts that each predicate expresses a property (or a concept). Is this a legitimate principle? It seems the answer should be negative. A first problem is that the principle commits us to the existence of conjunctive and disjunctive properties. Is there such a propertie as "being 5 meters high *and* eating a pizza"? or a property as "being 5 meters high *or* eating a pizza"? If Giovanni is 1,70 meters high and is eating a pizza, then the sentence "Giovanni is 5 meters high or is eating a pizza" comes out true; does this mean that Giovanni has the property of "being 5 meters high *or* eating a pizza"? This seems quite implausible[20]. Moreover, there are further difficulties as shown by Goodman's predicate "*being gruen*", which gives rise to a contradiction.

The previous points suggest that ICPC cannot be taken without any restriction. A possible suggestion would be to restrict the open formulas to simple formulas, where a simple formula does not contain any propositional connective. Composed formulas would then correspond to bunch of properties. Even if we limit ICPC to simple predicates, this would not be enough, because the movement – by itself – does not exclude that the problematic predicates will correspond to bunch of properties. In any case, what interests us is the Russell's property: is this property legitimate? To propose a restriction only to avoid this property would be a totally *ad hoc* movement. Russell's proposal[21] of ruling out the property because of its impredicative nature suffers from such a defect, because there are impredicative definitions which are perfectly permissible. Moreover, it is not clear that a complete elimination of impredicative definitions are possible: consider for example the predicate *'being a predicative concept'*, which is defined as the predicate that applies to *all and only* predicative predicates: is such a predicate predicative or impredicative? If it is predicative, then it applies to itself, which implies that it is one of the members of the totality of predicative predicates that is used in its definition, and so it turns out to be impredicative. On the other hand, if it is impredicative, then it must be defined by means of itself. But since it is defined by means of all and only predicative predicates, it turns out to be predicative. A standard reply would just claim that the predicate is in fact impredicative, and so it is

[20] The examples might be multiplied: for instance, are there conditional properties?
[21] Before him Poicarè made the same point.

defined by means of itself. To keep things consistent, it is enough to rule out the fact that the predicate *only applies* to predicative predicates. This is in fact how things should go: if there is a predicate that applies to all predicative predicates, this is surely the predicate "being a predicative predicate", and if this predicate turns out to be impredicative, then it is natural to claim that no predicate can apply to all and *only* predicative predicates. This argument suggests that impredicativity is a phenomenon we should learn to leave with, because it suggests that it is not possible to rule completely out impredicativity.

In any case, impredicativity plays a fundamental role in the derivation of the contradiction, because a predicative formulation of BLV within SOL does not give rise to the paradox. We are going to analyze deeper the role of impredicativity below in this chapter and in chapter 2.

2.3. Basic Law V

The third component is BLV. Since we have already established that it is the interaction between BLV and SOL which gives rise to paradox, it is now time to look in more depth at it.

BLV states a condition of identity for concepts/properties. Va (right to left) is the following principle:

(Va) $\forall F \forall G(\forall x(Fx \leftrightarrow Gx) \rightarrow \varepsilon(F) = \varepsilon(G))$.

By contraposition, this means that whenever we have two different extensions, then the correspondent concepts are different. If we define a function *f* from extensions to concepts (*f(extension)=concept*), this condition means that the function will assign different concepts to different extensions: $extension1 \neq extension2 \rightarrow f(extension1) \neq f(extension2)$. The function is therefore injective.

Vb (left to right) is the following principle:

(Vb) $\forall F \forall G(\varepsilon(F) = \varepsilon(G) \rightarrow \forall x(Fx \leftrightarrow Gx))$

By contraposition, whenever two concepts differ, then the correspondent extensions differ too. If we define a function g from concepts to extensions ($g(concept) = extension$), this means that the function will assign different extensions to different concepts: $concept1 \neq concept2 \rightarrow g(concept1) \neq g(concept2)$. The function g is injective.

Since the functions *f* and g are inverse and both are injective, from the theorem of Cantor-Bernstein-Schroeder it follows that there is a one-to-one correspondence between concepts and extensions. The morale is therefore that BLV impose a condition on the cardinality of our second-order domain: concepts and extensions must have the same cardinality.

2.4. The root of the contradiction and the importance of impredicativity

We know that from BLV follows NCP, according to which for every concept there is a correspondent set (or extension). Extensions are subsets of the original second-order domain[22]. However, it is a result of Cantor's theorem that the subsets of a given set are more than its elements and therefore the subsets of a given set C cannot all belong to C. In this context, this means that there are more concepts than objects in the domain, but since extensions are objects in the second-order domain, there should be more concepts than extensions, which contradicts BLV[23]. BLV and Cantor's theorem thus contradict one another.

At this point a key observation is that Cantor's theorem requires impredicative definitions. Cantor's theorem says that the Powerset $P(A)$ of an arbitrary set A is strictly bigger than A: $|P(A)| > |A|$. The classical Powerset Axiom is impredicative because to select subsets from the set A we can use both predicative and impredicative definitions. If we used a predicative version of BLV (that is if we admit only predicative definitions), then we could not prove Cantor's theorem. This can be shown by looking at the way Cantor proved it. Cantor started supposing a bijection from A to P(A). Then he considers the set B of all elements of A that are not contained in their images (in the bijection). B is a subset of A, so it is a member of P(A). But there cannot be any element of A associated with B in the bijection on pain of contradiction. As a consequence, there is no bijection. Notice two things: firstly, the definition of B is impredicative and, secondly, if we take the supposed bijection to be the identity function, and we suppose that $B \subseteq A$ is associated with an element of P(A), B would be associated with itself and thus will become the set of all sets that do not belong to themselves. What we get is the Russell set.

As we said before, a quite natural suggestion would be to abandon impredicative definitions. However, this does not work. First of all, classical mathematics is full of

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[22] We can think of a concept as selecting the objects that follow under it in the domain, as it happens with the axiom of separation.

[23] Boolos [1993], p. 230: ‹‹we cannot explain how the serpent entered Eden except to say: it is a brute fact that you cannot inject the power set of a set into that set […]. Frege simply failed to notice that he was trying to do precisely that››. In his reply to Boolos, Dummett [1994], p. 244 reckons that this presupposes a objectual reading of quantification (excluding a substitutional one), but it is far from clear that this was the reading Frege had in mind: ‹‹I have not been converted to his [Boolos] view that an objectual interpretation is to be preferred: rather, I have come to think that no answer can be given to the question. Frege does not appear to have been conscious of the distinction between the two ways of understanding quantification. Now if we put an objectual interpretation, classically understood, on the second-order quantification of Frege's theory, there is […] a unique and obvious explanation for the inconsistency, namely that Axiom V, so understood, makes an inconsistent demand on the cardinality of the domain››. We are here not interested in Frege exegesis; however, for our general aims - the problem of absolute generality - the quantifiers must be taken as objectual. The reason is straightforward: substitutional quantification depends too strongly on the expressive resources of a language: a universal quantified sentence, $\Pi x P(x)$, if read susbstitutionally, can be true, even if there is (in the objectual reading of the quantifier) an object d, for which it is not true that $P(d)$. This may happen if there is no linguistic expression for d. Since a language can have at most countably many linguistic expressions, but there are more than countably many objects (in virtue of Cantor's theorem), substitutional quantification is not apt as an interpretation of unrestricted quantification over everything.

impredicative definitions which do not produce any paradox; secondly, since impredicative definitions are present in natural language, we would gain consistency by declaring a perfect comprehensible notion illegitimate, which can be seen as a sort of paradox's revenge (Beall [2007]). Therefore, it is better to find a solution that does not declare impredicative definitions illegitimate.

§3. The structure of contradiction

In the last paragraph, we have found the cause of the contradiction: BLV along with SOL impose a constraint on the cardinality of the second-order domain that contradicts Cantor's theorem. We now need to look in a more specific way at how this happens in the derivation of the contradiction (what follows heavily relied on the derivation of the contradiction exposed in §1).

It is worth remembering that we are working inside a universe, which might be thought of as the maximal extension or the universal set (this was Frege's supposition before the appearance of the paradoxes). The first four passages were a derivation of NCP from Vb and SOL. NCP ($\forall P \exists y \forall x (x \in y \leftrightarrow Px)$) consists in an equivalence between the membership relation and the relation of instantiation of an object to a concept. Each concept/property determines the set (extension) of objects falling under it. It is clear that a concept determines only one extension. This is due to the fact that a concept divides the entire universe in two subsets: the first made up of the objects that instantiated it; the second, which is its complement, made up of objects that do not instantiated it. The law of Excluded Middle holds and, therefore, for an arbitrary object x and an arbitrary concept C either $x \in C$ or $x \notin C$. The objects that fall under a concept are fixed, because the universe is thought of as fixed.

But what about different concepts? Could two different properties/concepts determine the same set? The answer must be negative, due to the fact that we have derived NCP from Vb, which asserts that if two concepts differ, then their extensions differ. To understand better the point, let's take as example the two properties "having a heart" and "having a kidney". As a matter of fact, they determine the same set of animals. So it seems that they are two different properties that determine the same set. What allows us to say that the two concepts differ? We say that they differ (even though their instances are the same), because it is not *necessary* that everything with a heart has a kidney. We can easily imagine animals with a heart and no kidneys, which do not actually exist, but that *could* have existed. We need to introduce modalities in order to distinguish different concepts with the same extension. But no modality is available to us, because we are working within a standard domain (a set) and not in a possible worlds framework. Therefore, we are not in a position to recognize that "having a heart" and "having a kidney" are different concepts.

Since in our framework we work with a fixed universe, we cannot distinguish different properties with the same extensions and, consequently, NCP makes properties/concepts collapse into sets/extensions. Now, what happens in the derivation of the contradiction from NCP? Recall that derivation:

4. $\forall P \exists y \forall x(x \in y \leftrightarrow Px)$ NCP
5. $\exists y \forall x(x \in y \leftrightarrow x \notin x)$ by instantiating 4 with the property $\lambda z. z \notin z$. Let's call this property b.
6. $\forall x(x \in b \leftrightarrow x \notin x)$ by instantiating 5 with b
7. $b \in b \leftrightarrow b \notin b$ by instantiating the universal quantifier in 6 with b.

Sentence 5 affirms the existence of a set y which contains all and only the sets that do not contain themselves (Russell set). Then we called this set y *'b'* and we instantiated y with b in 6. The last passage corresponds to the question: the set b belongs or does not belong to itself? But the instantiation produces the contradiction in virtue of the meaning of the property that defines the Russell set. This general structure might be captured by the Russell's schema[24]

1) $\Omega = \{y;\ \varphi(y)\}$ exists
2) if x is a subset of Ω : a) $\delta(x) \notin x$: Trascendence
   b) $\delta(x) \in \Omega$: Closure

The first point corresponds to the affirmation of the existence of the set in question (in our example, of the Russell's set: so φ is the property of not belonging to itself and Ω is the set of all sets that do not belong to themselves). The second point introduces what Priest calls "the diagonaliser" δ, which - if applied to an arbitrary subset x of Ω - has the effects described by a) and b). In our case, the diagonaliser is the same property φ "not belonging to itself". The contradiction arises when we apply the diagonaliser to Ω itself (that is when we ask if the set of all and only sets that do not belong to themselves belongs to itself), in fact we get a) $\delta(\Omega) \notin \Omega$ and $\delta(\Omega) \in \Omega$. The last passage corresponds to the derivation of 7 from 6 above.

Russell's schema is useful to see what goes on with the interaction between BLV and SOL. The universe of discourse is Ω; BLV requires a one-to-one correspondence between concepts and extensions in Ω, that is BLV requires all concepts to be in the universe Ω (point 2b of the Russell's schema). But Cantor's theorem shows that the concepts outrun the number of elements of Ω and, therefore, the set of all of them does not belong to Ω (point 2a of the schema). In other words, BLV (Vb) requires closure; Cantor's theorem requires transcendence.

But why Cantor's theorem requires transcendence? As the schema above makes clear, this is due to the mixture of two different aspects: a circular, self-referential aspect,

[24] Russell [1905]. The terms *Trascendence* and *Closure* comes from Priest's reformulation of the schema. Priest called it "inclosure schema".

which is embedded in the impredicative definitions, and negation (point 2a of the schema). Now, there are at least two kinds[25] of impredicative definitions:

1) a definition is impredicative if it defines an entity in terms of a totality to which this entity belongs;
2) a definition is impredicative if the entity is defined by reference to the totality that the entity presupposes in order to exist[26].

The first definition is not problematic, rather it is the second one which is present in the paradoxes. Consider the Russell set R, the set of all sets that do not belong to themselves. This set is defined by reference to the totality of non-self-membered sets. But in this case the definition is not merely a way of individuating an object, *because in order for the Russell set to exist, all non-self-membered sets must exist*. This is because a set is defined by its elements[27] (remind that the condition of identity for sets is the axiom of extensionality) and R is defined as the set whose elements are *all* the non-self-membered sets. Therefore, this is a definition of type 2), where "presuppose" means "presuppose for existence".

It is the fact that the Russell set is the set of *all* sets that do *not* belong to themselves that forces it not to be in the same domain as the its elements. If we take the set of all sets that belong to themselves, either it belongs or it does not belong to itself, but in neither case, we have a contradiction. So, negation is fundamental, because - along with this kind of impredicativeness - has the effect of *not allowing the set to be one of its element*. However, since this means that the set does not belong to itself and it is the set of *all* sets that do not belong to themselves, we have the contradiction. In the case of Cantor's theorem, the negation presented in the definition "the set of all elements *not* contained in their images" forces this set not to be included in the starting set; however, here we do not get a contradiction because we did not suppose that the starting set contained all sets.

To sum up, three are the ingredients that make up the paradox:

1. Self-reference;
2. Classical negation;
3. A fixed universe of discourse.

§4. Possible ways out?

[25] See Chapter 2, §2 for a wider classification of different kinds of impredicativity.

[26] This definition corresponds to the third form of vicious circle in Gödel [1944], p. 125. The third vicious circle claims that no entity can presuppose a totality to which this entity belong. I interpret the verb presuppose as "presupposing for existence".

[27] This is true for the iterative conception of set. According to the logical conception, a set is not defined by means of its elements, rather by means of a property that determines which elements are members of the set. However, also in this case, it remains true that all non-self-membered sets must exist, in order for the Russell set to exist. This is because the property '*being the set of all non-self-membered sets'* (which defines the Russell set) is explicitly built up by reference to all the non-self-membered sets.

In the last paragraph, we individuated three ingredients that lead to contradiction: a particular form of impredicativity (that is circularity), negation and the idea that we work within a fixed domain. Therefore, we can avoid the paradoxes by disallowing one of the three elements.

We have already dismissed the approach that abandons impredicative definitions. A similar approach would consist in allowing impredicative definitions, but in imposing some other restrictions to the permitted properties in the NCP. For instance, this is the approach of those who wants to maintain a universal set in their theory. The definition of the universal set is clearly impredicative; so, if we want the universal set, we must accept impredicative definitions and we must find some more specific limitations to impose to the NCP. There are more possibilities here in play, but in any case, what we get is a *non-well-founded* set theory: a theory which allows sets to be members of themselves. These are interesting mathematical theories, fully legitimate from a mathematical perspective: however, there are at least three reasons why we should not be happy with such theories in our present context. First of all, ZF has had a terrific success both in mathematics and in philosophy, so we should decide to abandon it only if no alternative solution is available; secondly, and more importantly, there is a clear conception of set (the iterative conception) that explains the notion of well-founded set presupposed by ZF. On the contrary, no clear conception of non-well-founded set is known[28]; thirdly, as mentioned above, theories with a universal set suffers from a big philosophical problem. In fact, they are consistent as far as some properties are regarded as illegitimate (or some other limitations are introduced)[29]. But these properties are perfectly understandable in natural language. This solution seems therefore *ad hoc* (it disallows some properties because they produce contradictions) and, moreover, the fact that perfectly understandable notions cannot be expressed in the theory is usually recognized to be a revenge phenomenon[30].

Another possibility is to change the meaning of negation, that is to change logic. What gives problem in the classical notion of negation is that it *excludes* that Russell set belongs to itself. So, we need a negation that does not produce this exclusion. This is a paraconsistent negation. Within a paraconsistent logic, we can accept all the ingredients, because we can now accept the derivation of contradictions without making the system trivial. However, in this context, this makes sense only if we are willing to embrace dialetheism, the thesis that some contradictions are true. Russell set would give rise to a true contradiction. This approach can have an appeal only if all other alternatives fail.

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[28] An exception that might turn out to revel fruitful in the future is the 'Graph' conception of set defended by Incurvati [2014], which aims to provide a conception of set that also grounds non-well-founded sets.

[29] An example of the first is Quine's *New Foundations*, which formulates the comprehension principle only for stratified properties; an example of the second is the set theory developed in Church [1974], which formulates the Separation axiom, Replacement and the Powerset axiom only for well-founded sets.

[30] See for instance JC Beall [2007], pp. 1-29. In particular the properties which are not allowed in the theory are to be admitted in the meta-theory when speaking of the limited expressive power of the theory in question.

But if one manages to find a different solution that does not give rise to contradiction, then there is no reason to embrace a dialetheist solution.

However, dialetheism is only one option: we could change negation in a way such that the Law of Non-Contradiction holds, while the Law of Excluded Middle fails to be a logical law. For instance, one might opt for intuitionism or for a paracomplete logic[31]. But also this path seems to be very revisionary, since the Law of Excluded Middle is used everywhere in mathematics. Since set theory is a mathematical theory, we should look for the best logic we can find for mathematics. And an important role in this regard is played by mathematical practice: we should look with suspect to all accounts which imply that some aspect of mathematical practice is not legitimate. For this reason, we prefer not to change the meaning of negation, and try to find a solution within a classical framework.

So, it seems that, from a philosophical point of view, the best thing to do is to acknowledge the non-existence of the universal set. If we look at the logical structure of the paradoxes that we have individuated – following Russell and Priest - in the inclosure schema, what immediately appears is that paradoxes arise because an element of the starting totality is at the same time outside the totality. But, wait a minute!, doesn't this mean that the totality Ω with which we started did not comprehend all elements of a certain kind φ? Was not that supposition simply wrong? If we suppose that that supposition was simply wrong, then the paradoxes disappear and what remains is a more comprehensive totality than the one we had before. We believed we started with the totality of all ordinals, but now we have a more comprehensive totality of ordinals. Of course, the process can be iterated indefinitely. If we apply the same reasoning to the second-order universe, we should conclude that given every universe of discourse there is always a more comprehensive universe of discourse. According to this line of thought, the mistake behind Frege's work was to work inside a single universe and not considering the possibility of enlarging it[32].

The idea is simply that the reason why there is no Russell set, no sets of all ordinals or of all cardinals, is that given any arbitrary definite totality of such entities, we can find a more comprehensive totality of those kinds. That is, sets, ordinals and cardinals are *indefinitely extensible* concepts. The idea of indefinite extensibility explain why NCP fails: it is derived by BLV and its formulation presupposes a fixed domain of discourse making the existence of such concepts impossible to be accepted.

This interpretation of the paradoxes shows what is wrong in the derivation of them: in the above derivation we cannot go from 6 to 7, because b is not in the range of the

[31] A paracomplete logic is a logic that admits truth-values gaps, i.e. sentences that are neither true nor false. Many such approaches have been developed to deal with semantic paradoxes as the Liar. Paracomplete logics are clearly the duals of paraconsistent logic. For a presentation of the key ideas' behind paracompleteness and its relationship with paraconsistency see D'Agostini [2013], chapter 4.

[32] This is the reason why Cantor's theorem is a theorem, even if it exploits the same reasoning behind Russell paradox. As we have seen, the contradiction is simply avoided by claiming that the Power set $P(A)$ of A is strictly more comprehensive than A.

universal quantifier of sentence 6: the reason is that b is the Russell set, and the indefinitely extensible interpretation consists exactly in claiming that, from one side, this set does not belong to itself and, from the other side, it is not one of the sets over which it has been defined (*b* is not in the range of the universal quantifier in 6). This is the idea we are going to explore in the next chapters.

CHAPTER 2

INTRODUCTION TO INDEFINITE EXTENSIBILITY

§1. Defining Indefinite Extensibility

1.1 Shapiro' and Wright's definition of indefinite extensibility

The term 'indefinite extensibility' (IE) was firstly introduced by Dummett [1963]. In Dummett [1993], p. 441 he characterizes an indefinitely extensible concept as follows:

> An indefinitely extensible concept is one such that, if we can form a definite conception of a totality all of whose members fall under the concept, we can, by reference to that totality, characterize a larger totality all of whose members fall under it.

In other words, an indefinitely extensible concept C is a concept associated with a principle of extension which takes a *definite totality* of objects *c* falling under C and produces a new object that is a C but it is not one of the *cs* in the definite totality.

The problem with such a characterization consists in making sense of the meaning of 'definite conception of a totality'. When do we have a definite conception of a totality? Shorter: when can we speak of a *definite totality*? Of course, a totality is definite if it is not indefinitely extensible[33], but this explanation is not of much help, since it is obviously circular.

However, similar ideas concerning indefinite extensibility can be found in Russell [1906][34], who spoke of the 'self-reproducibility' of some concepts. Russell conjectured that if a concept does not have a fixed extension, then it is possible to find an injection from the ordinals into it (see Shapiro and Wright [2006], pp. 258-259). Interestingly, Cantor had already proposed the converse of Russell's conjecture: if there is an injection form the ordinals to the extension of a concept, then the concept does not constitute a consistent multiplicity[35]. Putting together Russell's conjecture and its converse we have the following characterization of indefinite extensibility:

A concept is indefinitely extensible if, and only if there is an injection from the ordinals into its extension.

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[33] It is tempted to speak of an indefinitely extensible totality, but this is just loose talk to say that to a concept corresponds an indefinitely extensible sequence of definite totalities. In particular, if a totality is considered to be a set or a plurality, then – properly speaking - no totality is extensible, because adding an element to a set or to a plurality has the effect of 'producing' a new set and a new plurality. What is properly indefinitely extensible is a concept, and as a consequence, the sequence of its extensions.

[34] And also in Poincaré, as explained in §2 of this chapter.

[35] In a letter to Dedekind dated 03.08.1899, Cantor defines a multiplicity as inconsistent if it is not possible to think of all its members taken together, because this implies a contradiction. When it is possible to think of all the elements of a multiplicity together, then the multiplicity is consistent. See Cantor [1991], p. 399. Dedekind rightly complained that this definition is rather obscure.

The characterization of IE given by Russell's conjecture and its converse is different from the one given by Dummett. First of all, the two definitions are extensionally not equivalent. For instance, Dummett's characterization implies that a concept as 'arithmetical truth' is indefinitely extensible: given an arbitrary set T of arithmetical truths, one can find a truth – the gödelian sentence for T – which is not one of the truths in T. (Dummett [1963], Shapiro and Wright [2006], p. 263). But there is no injection from the ordinals (classically conceived) into arithmetical truths. In fact, the set of all arithmetical truth is countable, while the ordinals are not countable. According to Dummett, Gödel's theorem also shows that the concept of arithmetical proof is indefinitely extensible, but also in this case it is likely that its extension is at most countable[36]. All such cases are dubbed by Shapiro and Wright as cases of '*bounded* indefinite extensibility', while concepts as set, cardinal and ordinal are said to be *unbounded* indefinitely extensible. In this latter case, the adjective 'unbounded' expresses the fact that there is no limit ordinal λ which constitutes an upper bound to the process of increasing the extension of a concept; if there is such an ordinal, then the concept is indefinitely extensible up to the limit ordinal λ, and so it is bounded. The case of arithmetical truth is a case in point: given a set of arithmetical truths, we find, by reference to it, new arithmetical truths; however, the resulting sequence has an upper bound, and so it is bounded indefinitely extensible.

According to Shapiro and Wright, the first desiderata of a proper definition of indefinite extensibility is the capacity of distinguishing bounded from unbounded indefinite extensibility, which is not the case either with Dummett's proposal or with Russell's conjecture. The second desideratum is that the definition should *explain* why certain concepts – as the one of ordinal – are indefinitely extensible. Notice that this second desiderata is not met by the definition based on Russell's conjectured. In fact, if we define a concept as indefinitely extensible when there is an injection from the ordinals into its extension, then we are making the ordinal trivially indefinitely extensible, since it is a triviality that the ordinals are injectable into themselves (it is enough to consider the identity function). Moreover, a definition should avoid the kind of circularity present in Dummett's characterization of the phenomenon.

To overcome these problems, Shapiro and Wright gives a new definition of indefinite extensibility. They begin with a *relativized* notion of indefinite extensibility. Let's take a concept P of items of type t. Now, Π is a concept of concepts of type t items. P is said to be *indefinitely extensible with regard to Π* if and only if there is a function F from items of the same type as P to items of type t such that if X is any sub-concept of P such that ΠP then

1) FX falls under P

[36] Other examples of 'small' indefinite extensibility are given by the concept involved in Richard paradox (being a real number defined by means of a finite number of words), which cannot be greater than the power of the real numbers, and the concept involved in Berry paradox (being the smallest natural number not denoted by any expression of English of fewer then seventeen words), which cannot exceed the size of the countable.

2) FX does not fall under X
3) ΠX', (where X' is the concept instantiated only by FX and the items that instantiated X, that is $X \cup \{FX\}$).

What the definition intuitively expresses is that the sub-concepts of P which have the property Π do not have a maximal extension.

Many concepts respect this definition. Shapiro and Wright give twelve examples. We are going to have a look at just few of them (for the other examples see Shapiro and Wright [2006], pp. 266-268).

Example 1: finite numbers. Px iff x is a finite ordinal (or cardinal) and Πx iff there are only finitely many X's; FX is the function that, once applied to X, gives us the successor of the largest element in X. Being a finite ordinal (or cardinal) is indefinitely extensible with regard to (the property of being) "finite";

Example 2: countable sets. Px iff x is a countable ordinal and Πx iff there are only countably many X's; FX is the successor of the union of the X's. Being a countable ordinal (or cardinal) is indefinitely extensible with regard to "countable";

Example 3: ordinals. Px iff x is an ordinal and Πx iff each of the X's is an ordinal and the X's are themselves isomorphic to an ordinal (under the natural ordering); FX is the successor of the union of the X's. Being an ordinal is indefinitely extensible with regard to the property of being isomorphic to an ordinal.

In the first two cases, we are dealing with bounded indefinite extensibility, whilst in the last case with an unbounded indefinite extensibility.

Shapiro and Wright generalize these definitions in the following way (Shapiro & Wright [2006], p. 269):

a) For any ordinal λ, P is "up to λ extensible with respect to Π" just in case P and Π meet the conditions for the relativized notion as defined above, but λ places a limit on the length of series of Π-preserving applications of F to any sub-concept X of P such that ΠX;
b) P is *properly indefinitely extensible* with respect to Π just if P meets the conditions for the relativized notion as above and there is no λ such that P is up to λ-extensible with respect to Π;
c) P is indefinitely extensible (*simpliciter*) just in case there is a Π such that P is properly indefinitely extensible with respect to Π.

The generalized definition explains why in some cases we face a paradox: this happens when P itself has the property Π (in the case of the concept of ordinal, this happens when we ask if ordinals have the property Π of forming a well-order). So, when P itself has the property Π, then we have a case of unbounded indefinite extensibility; otherwise there will be an upper bound to the process:

> So what is the connection with paradox [...]? The immediate answer is that in each of these cases there is powerful intuitive reasons to regard *P* itself as having the property Π. For example, in case *P* is an *ordinal,* and Π*P* holds just if the *X's exemplify a well-order-type,* it seems irresistible to say that *ordinal* itself falls under Π. [...] The question, them, is what leads us to fix our concepts of *set, ordinal,* and *cardinal* so that they seem to be indefinitely extensible with respect to Π's which are, seemingly, characteristic of those very concepts themselves. (Shapiro & Wright [2006], pp. 269-270).

1.2 Problems for such a definition

How does this definition perform? The definition surely performs well with regard to the distinction between bounded and unbounded indefinite extensibility; in addition, it seems that it provides an interesting diagnosis of the paradoxes, which underlines the presence of a self-referential structure. However, there are still many critical points to examine.

First of all, it does not manage to completely avoid the circularity present in Dummett's characterization. It only makes it a bit more refined:

> Our suggestion, then, is that the circularity involved in the apparent need to characterize indefinite extensibility by reference to *Definite* sub-concepts/collections of a target concept *P* can be finessed by appealing instead at the same point to the existence of some species – Π – of sub-concepts of *P*/collections of *P's* for which Π-hood is limitless preserved under iteration of the relevant operation (S. & W. p. 269).

Maybe the fact that the circularity is not completely avoided is not a great problem; it might be that the circularity is in fact not eliminable. The problem is that talk of definite totality is substituted by talk of "species – Π – of sub-concepts of *P*/collections of *P's*", which is quite vague. Are these sub-collections sets? Or pluralities? However, worse than this, it is the fact that the definition seems to presuppose that we understand the fact that "Π-hood is limitless preserved under iteration": does not this presuppose that we already understand the notion of indefinite extensibility ('limitless' seems to indicate the same phenomenon described by the adjective 'indefinite')? It would be better to make explicit what it is intended with sub-collection, which may also shed light on how such limitless series work.

The second, and far more serious, problem regards the ordinals: it seems that this definition is not able to meet the desiderata according to which the ordinals should not be trivially indefinitely extensible. In fact, what distinguishes a bounded from an unbounded indefinitely extensible concept is the fact that the first has an upper bound, while the latter no. But, in this context, an upper bound is an ordinal, and so the definition must presuppose that ordinals are unbounded indefinitely extensible.

Maybe this worry can be overcome by defining a concept as unbounded indefinitely extensible with respect to Π just if P itself has the property Π. The idea would be to exploit the characteristic of the paradoxical cases that emerges from the above definition to define unbounded indefinite extensibility. The concept of being an ordinal is unbounded indefinitely extensible, because given any definite totality of ordinals, the totality is itself well-order, and so it has an ordinal (Π). Definition b) above should be rewritten as follows:

> b') P is *properly indefinitely extensible* with respect to Π just if P meets the conditions for the relativized notion as above, and P itself falls under the property Π.

This definition does not rely on the ordinals, and so it is better than the previous one. However, it still makes concepts as set, ordinal, and cardinal trivially indefinitely extensible. If we proceed in this way, then such concepts would be indefinitely extensible just by definition, since all of them are such that they fall under their respective property Π. Of course, it is better to avoid a definition that makes such concepts trivially indefinitely extensible, the reason being that there is a debate about the legitimacy of acknowledging such concepts as indefinitely extensible (for instance, Boolos's plural approach denies the existence of indefinitely extensible concepts[37]), and consequently, that such concepts are or are not indefinitely extensible seems to be a substantial thesis which cannot be settled only by means of a definition.

Finally, there is a worry about the consistency of such a definition. Shapiro and Wright work within the framework of standard classical logic. This leads them to the conclusion that there is no acceptable solution of the problem of absolute generality, once indefinite extensibility has been accepted. The reason is that there cannot be a standard quantification over all elements of an indefinitely extensible sequence. However, the problem for the definition arises by the fact that the same definition requires to generalize over all elements of an indefinitely extensible concept. Consider Example 3 above. That definition acquires its intended generality only if the range of application of the X (which stays for ordinals) comprehends all ordinals, not just some of them. Similarly, in the definition of the relativized notion of indefinite extensibility, all three points must be understood as implicitly bounded by universal quantifiers: for instance, point 1 (FX falls under P) must be understood as $\forall X$ (FX falls under P).

This problem is just an example of the inexpressibility objection that we are discussing in chapter 3. If one denies the possibility of absolute general discourse, it seems that she is not able to state coherently this same denial. In this case, if one uses indefinite extensibility to argue against the possibility of absolute generality, it seems that she is not able to coherently define the same concept of indefinite extensibility. What this situation clearly outlines is that a consistent definition of indefinite extensibility already requires the solution of the problem of generality over an indefinitely extensible sequence. For this reason, the definition we are going to propose can be fully understood in connection with the solution we are going to give to this problem in chapter 7.

1.3 A better definition of indefinite extensibility

[37] For the plural approach to absolute generality see chapter 5, §2.

In virtue of the problems of the previous definition, it is better to look for a different definition of IE. The discussion above showed some desiderata of such definition: first of all, the definition should account for the difference between bounded and unbounded indefinite extensibility; secondly, it should not make the ordinals (or other concepts) trivially indefinitely extensible, which means that it should be a neutral definition that could be also accepted by someone – as Boolos – who rejects the existence of such concepts.

It is useful to go back to Dummett's characterization of the notion, which is roughly the follows: a concept is indefinitely extensible if, given a *definite totality* of objects falling under it, by reference to this totality we find new objects that fall under the concept, but are not members of the starting totality.

Before dealing with the problem of the notion of definite totality, we shall focus on other features hidden in this characterization of indefinite extensibility. Suppose that P is an indefinitely extensible concept (IEC). Then we have the following feature:

a) If, given a domain of objects, a member *d* of such a domain is recognized as falling under P, then *d* will fall under P in any further expansion of the extension of P.

The second feature is that,

b) If, given a domain of objects, a member *d* of such a domain is recognized as *not* falling under P, then *d* will not fall under P in any further expansion of the extension of P.

The third feature of an IEC is that its extension indefinitely expands. In other words, we have the following feature:

c) when we expand an extension U of an IEC P into a more comprehensive extension U', we have that $U \subseteq U'$ (no objects have been lost in the passage from U to U').

This feature seems to be presupposed in Dummett's characterization: in particular, they imply that an indefinitely extensible concept maintains its *intension* through all the expansions of its extension. The concept remains the same while its extension grows[38].

Let us now turn to the clarification of the notion of 'definite totality' in Dummett's characterization. A first proposal would be to identify a definite totality with a *set*. What we obtain is the following definition

(IE-1) A concept is indefinitely extensible if, given a *set* of objects falling under it, by reference to this set we find new objects that fall under the concept, but are not members of the starting set.

The problem with such a definition is that it makes the concept of indefinite extensibility a consequence of Cantor's theorem. IE implies that given a set, we can find a

[38] A different conception of indefinite extensibility is discussed in §6 of chapter 7.

more comprehensive set, which is trivially true in virtue of Cantor's theorem that the Power set of a set A is always strictly bigger than A. Moreover, this definition is compatible with Boolos's plural approach: IE-1 implies that there cannot be a maximal set containing all the instances of a concepts, but it is compatible with the existence of a maximal plurality of all instances of a concept.

A far better suggestion consists in identifying a definite totality with a *plurality*:

(IE-2) A concept is indefinitely extensible if, given a *plurality* of objects (some objects) falling under it, by reference to this plurality (to these objects) we find new objects that fall under the concept, but are not members of the starting plurality (of the starting objects).

This is certainly a more interesting characterization of IE: firstly, it is not a consequence of Cantor's theorem (which is a theorem about sets, not pluralities)[39], and thus it is not a trivially true statement in virtue of a mathematical theorem, but an interesting philosophical thesis; secondly, IE-2 is incompatible with a plural approach to absolute generality: for the latter, there must be a maximal plurality of objects, while IE-2 implies that there cannot be a maximal plurality of objects falling under an indefinitely extensible concept[40].

Since it employs plural resources, this characterization must be formulated within plural logic. It is natural to formulate it within PFO (plural first-order logic). This feature of an IEC P may be expressed as follows:

(IE-2) $$\forall xx\ \exists u(u \nprec xx \wedge P(u))$$

But IE-2 yields an inconsistency: in fact, it is theorem of PFO-logic that there is a plurality that comprehends every object (this is just an instance of plural comprehension: $\exists xx\ \forall u(u \prec xx \leftrightarrow \varphi(u))$, where the meta-variable $\varphi(u)$ has been substituted by the predicate $x = x$). Therefore, the PFO-formulation of IE-2 above is inconsistent.

This result is nothing terribly surprising. Standard PFO-logic is just standard FOL with the addition of plural variables: in particular, it shares with standard FOL the fact that the logic works within a fixed universe of discourse (in FOL, the specification of a domain for the quantifiers is usually the specification of a set; in PFO-logic it is the specification of a plurality. In both cases a domain consists in a fixed - i.e. not extensible - universe of discourse). This suggests that the definition of IE-2 requires the introduction of new resources in the language; in particular what we need is a more intensional

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[39] For the first formulation of the generalization of Cantor's theorem to proper classes see Bernays (1942). For a discussion of it in a plural framework see Hawthorn & Uzquiano (2011).

[40] What if one rejects plurals? If one rejects plural, then she has also to reject the Boolos's plural approach to absolute generality. In this setting, (IE-1) turns to be a perfectly acceptable characterization of indefinite extensibility.

approach that keeps track of the fact that the same concept can have different (increasing) extensions. A modal logic is what we need:

$$(IE-2)^{\Diamond} \qquad \Box\forall xx\,\Diamond\exists u(u \nprec xx \wedge P(u)).$$

It must be noticed that this formulation is consistent with plural comprehension. The fact that plural comprehension has as an instance the existence of a plurality which comprehends every object is accommodated, in this modal logic, simply by the fact that, in each world (domain), plural comprehension turns out to be (trivially) true[41].

$(IE-2)^{\Diamond}$ asserts the potential existence of an object u, given the existence of objects xx. This simply means that going from the xx to the u, which is not one of the xx, we are expanding the domain.

Now that the modal approach is in play, we can give a proper formalization of the features of an IEC given by conditions a), b) and c) above. We obtain (the necessitation of the universal closure of the following conditions):

a) $P(x) \rightarrow \Box P(x)$
b) $\sim P(x) \rightarrow \Box{\sim}P(x)$
c) if U is expanded in U', then $D(U) \subseteq D(U')$, where D denotes the domain of U or U'.

To sum up, we have the following definition of IE:

Definition of IE: *A concept P is indefinitely extensible if it satisfies conditions a), b) and c), and moreover it satisfies condition* $(IE-2)^{\Diamond}$.

How does this definition perform with regard to the previously stated desiderata? Let us start with the difference between bounded and unbounded indefinitely extensibility. If a concept is indefinitely extensible with regard to $(IE-2)^{\Diamond}$, then it is clearly unbounded indefinitely extensible, because given any object that satisfy it, it is possible to find more objects satisfying it. So, it seems that our definition fails to satisfy the first desiderata. However, I do not think this is a great problem. In fact, a bounded indefinitely extensible concept is *not really* indefinitely extensible, since it has an upper bound. All its instances can be collected within a set, and so these concepts can be treated as standard concept with a fixed extension. For this reason, the fact that our definition cannot be applied to bounded indefinitely extensible concepts simply means that bounded IE is not really IE.

If one objects that our position rules out – by definition – Dummett's position according to which concepts as 'arithmetical truth' or 'arithmetical proof' are indefinitely extensible, then the reply would simply be that this complain is misleading. Because Dummett championed a constructivist approach to mathematics, which means

[41] See Chapter 7, §1 for the justification of the modal approach. Moreover, it must be noticed that the modal operator must be taken as primitive. This issue is fully discussed in Chapter 7.

that he would not have considered the ordinals (classically conceived) as legitimate mathematical objects. For Dummett, there is no distinction between bounded and unbounded indefinite extensibility, because there cannot be a set of all mathematical truths or a set of all mathematical proofs in his constructivist approach. Our definition would apply to those concepts, if we were following him in abandoning classical set theory.

Concerning the second desiderata – not making the ordinals trivially indefinitely extensible-, notice that our definition does not imply that the concept of ordinal is indefinite extensible. In fact, one could argue that the ordinals fail to satisfy the definition: in particular, a champion of the plural approach could argue that the there is no ordinal that corresponds to the plurality of all ordinals (maybe by appealing to the idea of Limitation of Size)[42]; in this way, given some ordinals, it is not always possible to find more ordinals, which means that the concept of ordinal does not respect the definition above. Our definition does not make the concept of ordinal to be trivially indefinitely extensible, but it leaves open the possibility of arguing for or against the idea that such a concept is an IEC or not.

The third desiderata concerned the need of avoiding the circularity present in Dummett's characterization. We saw that Shapiro and Wright's definition makes progress on this point, but they do not manage to completely avoid the circularity. What is the situation with regard to our definition? I think that for sure, on this specific point, it constitutes an improvement with respect to Shapiro and Wright's definition: they spoke of sub-concept of P/sub-collection of P without specifying what kind of collection it is. On the contrary, we have made clear that this kind of collection must be taken as a plurality. So, the ambiguity in their definition completely disappears in ours.

At a first sight, our definition does not make appeal to the notion of a definite totality, and so it seems to avoid any form of circularity. However, a plurality is for sure a definite totality: if it turned out that our comprehension of the nature of a plurality is based on the notion of definiteness, then we still have a circularity. The possibilities are two.

1) The notion of plurality is *prior* to the notion of definiteness: we know what definiteness means because we know what a plurality is. If this is the case, then our definition is completely circular-free. Notice here that, if this is the really the case, then the notion of plurality is the best notion we have to explain the notion of 'definiteness'. The reason is that, whatever one takes a collection to be (a set, a plurality, a proper class, a mereological sum, etc.), it seems necessary that for a collection to be completed, and therefore to be fully definite, there must be at least all its elements. The presence of all elements of a collection is a necessary condition to claim that this collection is definite. But a

[42] This position is deeply discussed in chapter 5, §2. I do not think this is a good position to defend, but the possibility of such a position shows that our definition does not make the concept of ordinal to be trivially indefinitely extensible.

plurality is (nothing over and above) its members. So the notion of a plurality is the most simple notion of a definite collection/totality. If one wants to explain the meaning of 'definite totality' by means of a more primitive notion, one must go for the notion of plurality[43].

2) it is the other way around: we understand the notion of plurality in virtue of the notion of definiteness. In this case, our definition would be plainly circular. However, the situation would not be different for a defender of the plural approach, who uses the notion of plurality. If such a notion presupposes the notion of definiteness, it seems plausible that it also presupposes the notion of indefiniteness[44], and this would obviously hold both for the defender of indefinite extensibility and for the champion of the plural approach. In this case, both would find themselves in a circularity.

As a matter of fact, I suspect that the last possibility is the right one: we understand the notion of plurality in virtue of the notion of definiteness. In any case, whether this is the case, this circularity would not be eliminable, and consequently we must learn to live with it. This circularity just points to the fact that such notions are primitive.

There is a further aspect to deal with. The last desideratum claims that the definition should be neutral, in the sense that it should be also acceptable by the opponent of the existence of indefinitely extensible concepts. Let us clarify this with an example. Suppose two opponents are arguing against each other on the problem of the existence of abstract objects. Contender A argues that there are in fact abstract objects, while contender B argues that all objects are concrete, and so there is no abstract objects. In order to have a proper disagreement between them, it is necessary that they use the term 'abstract object' in the same way. In other words, they must agree on the definition of the locution 'abstract object'. The same should happen in our present case: our definition of indefinite extensibility implies that, if there are indefinitely extensible concepts, the plural approach to absolute generality fails. So, to have a proper disagreement, it is necessary that the two opponents agree on the definition of indefinitely extensibility. Once they have agreed on the definition, they can start developing arguments in favor or against the existence of indefinitely extensible concepts. But the problem is that the definition is stated by means of primitive modal operators, which are not recognized by the pluralist (who works within PFO or some higher-order extension of PFO). Therefore, the two opponents do not share the same definition.

[43] All other notions of collection listed here are more complex than the notion of plurality. For instance, a set is a further object to regards to the plurality of its members; the same is true for the notion of proper class. The notion of mereological sum is usually taken not to bring a commitment to a further object with regard to *the objects* that compose the sum; however, the plural expression shows that we need the notion of plurality to state what a mereological sum is.

[44] It will not do to take the notion of definiteness as primitive and to define 'indefiniteness' as its negation: because we can do exactly the opposite: one can define 'definiteness' as the negation of a supposedly primitive notion of 'indefiniteness'.

That this is not a casual situation may be seen from the fact that the modal operators were introduced to give a consistent definition of indefinitely extensibility, and so it is in no way eliminable. Even if the defender of IE would like to opt for a different definition, framed within a different logic (for instance within intuitionistic logic, as argued by Dummett), the problem will not disappear. The pluralist will not recognize that definition exactly because it is framed within a different logic, and so, also in that case, the two opponents will not agree on it.

It is clear that the disagreement depends on the fact that the two opponents use different logics to argue for their thesis. Debates about what is the right logics are notoriously difficult to settle and evaluate without begging the question, and it seems to me that our present situation is not much different. However, I believe it is possible to find a common ground between the two positions. In fact, they both recognize that the non modalized formula IE-2 - $\forall xx \ \exists u(u \nprec xx \wedge P(u))$ - is false. According to the pluralist, its falsity can be seen as a consequence of the non-existence of indefinitely extensible concepts; according to the champion of IE, its falsity is due to a logic that works within a fixed domain of objects, and so cannot recognized the existence of indefinitely extensible concepts. The formula $\forall xx \exists u(u \nprec xx \wedge P(u))$ works here as common ground that guarantees that the two opponents are really speaking of the same phenomenon. I think this is the maximum we could hope for.

§2. Indefinite extensibility, impredicativity and the vicious circle

One of the key feature of the set theoretic paradoxes is the presence of impredicative definitions, i.e. the presence of circular definitions. Roughly speaking, a definition is impredicative if it defines an object by means of the totality of objects to which it belongs (a definition which is not impredicative, is called 'predicative'). Since this circularity, it is not surprising that many authors have proposed to block the paradoxes exactly by abandoning such kind of definitions. More specifically, impredicative definitions were considered to be the culprit of a vicious circle present in the definition of the so-called 'inconsistent multiplicities'. In particular, all the three major proponents of the existence of indefinitely extensible concepts – Poincaré, Russell and Dummett – rejected the legitimacy of impredicative definitions. Therefore, it is important to clarify the exact relations between indefinite extensibility, impredicativity and 'the vicious circle'. The first aim of this paragraph is to carry on this clarification, by underling the assumptions that make them compatible or incompatible. The second aim is to show that indefinite extensibility is not, by itself, incompatible with impredicative definitions; however, it is incompatible with the vicious circles: our conclusion is thus that, in an indefinitely extensible universe, it is not possible to identify impredicativity and the vicious circularity. You can have the former, but not the latter.

The structure of this paragraph is as follows: the first part concerns the relation between indefinite extensibility and impredicativity; by looking at Poincaré's and Russell's positions, we will try to understand if it is possible to formulate an argument to the claim that indefinite extensibility and impredicativity are incompatible; after having established the impossibility of such an argument and, therefore, the compatibility of indefinite extensibility and impredicativity, we shall distinguish three different types of impredicative definitions, and we shall argue that only the last two produce a vicious circularity (and therefore play a role in the derivation of the paradoxes). Finally, we shall see that, in an indefinitely extensible universe, one can accept the last two types of impredicativity whilst abandoning the correspondent forms of vicious circularity.

2.1 Impredicativity and indefinite extensibility

2.1.1 The case of Poincaré, that is: are indefinite extensibility and impredicativity incompatible?

The case of Poincaré is very interesting, because it seems to argue that indefinite extensibility and impredicative definitions are incompatible. Let us see exactly what he says. In the chapter *The last efforts of the logisticians* of the book *Science and Method* [45] he argues that the paradoxes are due to the presence of a *vicious circle* in their definition. In particular, whilst considering Richard paradox[46] concerning the set E of all real numbers definable by means of a finite number of words, he argues that the number N, defined by means of diagonalization on E, is defined by means of itself, since it is definable in a finite number of words, and thus it belongs to E. By involving the collection E, the number N involves its own existence. This vicious circle is present, according to Poincaré, when we have an impredicative definition: ‹‹thus, *the definitions that must be regarded as non-predicative are those which contain a vicious circle*›› (p. 190). But what is the "true solution"[47] of the paradoxes? Poincaré is the first to suggest the idea of indefinite extensibility as a general solution[48] for the paradoxes. He writes:

> Let us refer to what was said of this antinomy in Section V. E is the aggregate of *all* the numbers that can be defined by a finite number of words, *without introducing the notion of the aggregate E itself* otherwise the definition of E would contain a vicious circle, for we cannot define E by the aggregate E itself.
>
> Now we have defined N by a finite number of words, it is true, but only with the help of the notion of the aggregate E, and that is the reason why N does not form a part of E (pp. 189-190).

It is clear that, as we defined N by means of E, we can define a new number N' by means of the new totality E∪N. The process can then be iterated without an end. In

[45] Poincaré [1908], pp. 185-186. This chapter is an adaptation of his already published essay *Mathematics and logic.*
[46] Richard's paradox is one of the paradoxes of 'definability': consider the set E of all real numbers definable in a finite number of words. Since the words of a language are finitely many, there can be at most a countable number of such definable reals. But we can apply to E a diagonal procedure similar to the one that lead Cantor to prove the uncountability of the reals, to find a number N that cannot belong to E. If the n^{th} decimal of the n^{th} number in E is respectively 0, 1, 2, 3, 4, 5, 6, 7, 8 or 9 the n^{th} of N will be 1, 2, 3, 4, 5, 6, 7, 8, 1 or 0. In this way N is different from each number in E. So, N does not belong to E. The problem is that we have just defined N in a finite number of words, so N belongs to E. Contradiction.
[47] "True solution" is the title of the paragraph in which Poincaré introduces his solution to the paradoxes.
[48] In a related passage, he claims that the solution he is going to present can be generalize to all paradoxes.

contemporary terms, the idea that emerges from Poincaré's text is that the concept "being a real definable in a number of finite words" is indefinitely extensible. So, it seems that Poincaré is holding both the indefinite extensible thesis and the illegitimacy of impredicative definitions. Moreover, it is clear that the vicious circularity is produced by the impredicative definitions and indefinite extensibility is called into play to solve this circularity. It seems clear, thus, that Poincaré is suggesting that indefinite extensibility is incompatible with impredicativity.

What it is less clear is in what sense the two figures may be said to be incompatible. One might suggest[49] the following: the paradoxes depend on a vicious circularity, which is due to an impredicative definition (in the sense that without an impredicative definition, we would not have the vicious circularity). Indefinite extensibility is called into the play exactly to avoid this circularity, so we cannot have both indefinite extensibility and this circularity. Therefore, indefinite extensibility and impredicativity are incompatible. Unlikely, this idea does not work. Consider again Richard paradox. If we consider the concept "being a real definable in a number of finite words" as indefinitely extensible, the number N, defined by means of diagonalization on E, does not belong to E, which means that its definition is *predicative*: N is defined by quantifying over E, but N does not belong to E. What this shows is simply that once you allow the indefinite extensibility of the concept in question, then the definition of N turns out to be predicative. Indefinite extensibility implies predicativity ($Ind.ext. \rightarrow \sim imp.$). But this is not enough to conclude that indefinite extensibility and impredicativity are incompatible, at least if we cannot provide an argument to the effect that predicativity implies indefinite extensibility ($\sim Imp. \rightarrow Ind.ext.$).

Maybe a better argument is the following: consider (a schematic version of) the naïve comprehension axiom: $\exists y \forall x (x \in y \leftrightarrow \varphi(x))$, with the meta-variable φ standing for an impredicative value. Suppose now that the range of the universal quantifier $\forall x$ is indefinitely extensible, which means that the totality of *xs* is indefinitely extensible. The values *xs* are the values of the predicate φ. Since the latter is impredicative, $\varphi(x)$ is defined by means of the totality of the *xs.* But there is no totality of the *xs* because for each totality we may consider, we can find a more comprehensive totality. The morale is that if we want to keep $\varphi(x)$ impredicative, we must impose that the domain of *xs* is not extensible. So, impredicativity implies the existence of a totality, which is banned by indefinite extensibility. Therefore, indefinite extensibility does not ban directly impredicativity, rather it directly bans the existence of a definite totality, which is presupposed by an impredicative definition. In other words, impredicativity implies a definite totality, that is a totality that is not extensible ($Imp \rightarrow def.totality \rightarrow \sim ind.ext$). The problem with such an argument is that the result it gives us is the contraposition of the result of the above argument, which means that the two arguments prove the same, and thus it does not constitute an improvement over the latter result.

[49] The reader should bear in mind that here and in what follows below our interest is not in an historical reconstruction of Poincaré's thought, rather in understanding the connection between indefinite extensibility and impredicativity.

That the supposed incompatibility between indefinite extensibility and impredicativity is due to the fact that the latter presupposes a definite totality (a totality which is not extensible) is probably what Poincaré had in mind, is shown by the fact that he believes both in the natural numbers to form a definite totality and in the legitimacy of impredicative definition over them[50]. This suggests that what makes impredicative definitions legitimacy is the presence of a definite totality of objects. However, if this is all what we can extract from Poincaré's position, it is not enough to prove the incompatibility, because we do not have an argument to show that predicativity implies indefinite extensibility. We shall deal in a later section with a counterexample to this implication, which shows the impossibility of proving the incompatibility between indefinite extensibility and impredicativity[51]. For the time being let us see what we can extract from Russell's position.

2.1.2 The case of Russell, that is ramified type theory is too much!

The case of Russell is somehow similar. He took from Poincaré the idea that the paradoxes were due to the presence of a vicious circle and that we should avoid impredicative definitions to account for them. His solution, the ramified type-theory, bans impredicative definitions and does not allow to have a universal type, a type of all types. Ramified type theory is simple type theory with the supplementation of orders (sometimes called 'levels'). The universe is divided in infinitely many types: a type 0 there are individuals (things that are not classes); at type 1 there are classes of individuals (classes of type 0 object); at type 2 there are classes of classes of type 1, and so on. There is no universal type: each type has an immediately upper type. The type-stratification is not enough to ban impredicativity: for instance, at type 2 it is possible to quantify over all subclasses of type 1 (that is all subclasses determined by a predicative or impredicative formula). To avoid impredicativity, Russell adds 'orders', which introduce a stratification in the definitions of the classes. Apart from type 0 (where there are no classes), each type has infinitely many orders: at order 0 there are *predicative classes* (classes defined by means of individuals – if we are in type 1 – or by means of classes of an inferior type for each type >1); at order 1, there are classes defined by means of order 0 classes; at order 2, classes defined by means of order 1 classes and so on. In this way impredicative definitions are banned: no class can be defined by means of the totality of classes to which it belongs.

Russell was lead to the ramified theory of types exactly by the paradoxes of definability, because the latter cannot be solved by means of the simplified type theory[52]. At the same time, he believed that there was a unique logical structure behind all the paradoxes, and therefore he was looking for a unique solution for all of them. The logical structure which was culprit of the antinomies was the vicious circle. Ramified

[50] Feferman [2002].
[51] Of course, if you give a constructivist interpretation of indefinite extensibility, then the two become incompatible. On this point, I am going to say more in a later section.
[52] Giaquinto [2002], pp. 70-71.

type theory with the notion of "order", which is introduced to avoid impredicative definitions, was the solution to this vicious circularity. As Poincaré, Russell tried to block the paradoxes abandoning impredicativity.

Russell's ramified theory of types shares with indefinitely extensibility an important feature: the universe is open-ended, since for the former there cannot be a universal type, while for the latter there cannot be a universal domain[53]. In this sense, Russell position is very close to the one of Poincaré: both of them used an open-ended picture of the universe to ban impredicative definitions. However, from our standpoint this is quite disappointing, because it provides us with the same result of Poincaré's position: if we ban impredicativity, the resulting universe is open-ended. But, we already known that this is not enough to claim the incompatibility between indefinite extensibility and impredicativity[54].

2.1.3 Indefinite extensibility and impredicativity are compatible.

To show that indefinite extensibility and impredicative are incompatible we need to show that predicativity implies indefinite extensibility. This means that if we allow only predicative definitions, there cannot be a maximal universe. One could try to argue for that by appealing to the fact that we cannot define a maximal universe (a universal set) by means of only predicative definitions. In fact, the universal set is defined as the set of all sets: so it is defined by quantifying over a totality (all sets) to which it belongs (if it did not belong to it, it would not be the universal set, because it would lack an element: itself). However, this argument is a *non-sequitur*. The fact that we cannot predicatively define the universal set does not mean that predicativity and the universal set are incompatible; it simply means that to *state* their compatibility we need further expressive resources, that is impredicative definitions.

This situation has been clearly stated by Hellman [2004]. Hellman notices that predicative mathematics (mathematics that starts from the natural numbers considered as given – or by a weak system governing finite sets from a countable domain of individuals – and iteratively applies arithmetical comprehension to ordinals which are defined in a predicative way) has an upper bound such that all constructions over it become impredicative. This upper bound is the countable limit ordinal known as Γ_0. All predicative mathematics belongs to it. Since Γ_0 is a set (every ordinal is a set), it is the universe inside which we can have all the predicative definitions. In turn, this shows that predicativity does not imply indefinite extensibility, because every possible predicative construction can be carried out inside a fixed universe. However, Γ_0 itself is definable only in an impredicative way: "theorem requiring consistency strength measure by Γ_0

[53] Despite this similarity, there is a great difference between this two approaches: type theory constitutes an ideological hierarchy, since going up in the hierarchy our language becomes more and more powerful, while indefinite extensibility constitutes an ontological hierarchy: the language is always the same, what changes is the domain of objects of which the language can speak.

[54] Russell's solution was too drastic, at least for Ramsey. Ramsey's simplified type theory avoids the notion of order, and in this way, allows for impredicative definitions. In fact, we already know that introducing a hierarchy of types is not enough to ban impredicativity.

(or greater) for their proof are regarded as essentially impredicative" (Hellman [2004], p. 3). This is not a surprising result: since it is provable that all predicative constructions can be carried out inside Γ_0 and Γ_0, as all sets in standard ZF, does not belong to itself, it follows that Γ_0 cannot be 'constructed' in a predicative way.

Since predicativity is compatible with a fixed universe, it does not imply indefinite extensibility. This shows that indefinite extensibility and impredicativity are compatible. However, since indefinite extensibility implies predicativity (as we argued above), the compatibility with impredicativity must be spelt out carefully. In fact, what we have not done yet is to explain how indefinite extensibility and impredicativity interact. This task will be fulfilled in the next section by looking at the role that impredicativity plays in the standard argument for indefinite extensibility. Moreover, we are going to analyze in detail the relation between impredicativity and the vicious circle; what we shall argue is that vicious circles cannot be always identified with impredicative definitions: in an indefinitely extensible universe, we can accept the use of impredicative definitions, but we must ban vicious circles.

2.2 Varieties of impredicativity and vicious circles.

Let's start by noticing that there are many kinds of impredicative definitions. Russell was never very clear about what he takes the Vicious Circle Principle to be; however, following the famous paper *The mathematical logic of Bertrand Russell* of Gödel, we can distinguish three different forms of Russell's vicious circle, which in turn correspond to three different forms of impredicative definitions. These are the following:

1) No entity can be defined in terms of a totality to which this entity belongs;

2) no entity can involve a totality to which this entity belongs;

3) no entity can presuppose a totality to which this entity belongs.

2.2.1 Definition number 1

It might be not so clear what "defined in terms of a totality to which the entity belongs" means; I shall explain what I take point 1 to mean with a simple example. Consider the definition of the *least upper bound property* in classical analysis: any non-empty set of real numbers that has an upper bound has a least upper bound. In this definition, the least upper bound is defined by means of a totality of real numbers to which it belongs. The idea behind point 1 is that the definition of a mathematical entity consists in considering the totality of objects to which this entity belongs and in individuating the entity by means of a particular property that it possesses. Now, there are two possible ways in which the definition might refer to the totality[55]: first of all, with a term in the definition which explicitly refers to the totality as in the following example:

[55] Giaquinto [2002], pp. 72-73. The second example comes from this text, p. 73.

$F = \{x : x \text{ is in } G \text{ and } \varphi(x)\}$.

In this example, F has been defined by means of the totality G and the reference to the latter is given by means of a term. The second possibility is that in the definition there is a quantifier and the object being defined is in the range of it. Consider the following example taken from Gianquinto 2002, p. 73: the oldest visible galaxy. This means "x is a visible galaxy such that for every visible galaxy y, x is at least as old as y". Here the variable x is in the range of the quantifier "for every galaxy". In both the cases, despite the difference of reference to the totality to which the entity being defined belongs, we have *the same pattern*: the definition considers a totality of objects and defines one of them by means of a property or a characterization that identifies it.

Gödel's analysis of this account of the vicious circle is famous. He argues that it ‹‹applies only if we take a constructivistic (or nominalistic) standpoint toward the objects of logic and mathematics››[56]. According to a constructivist view, a definition constructs the object being defined and, consequently, we cannot define an object by means of the totality to which it belongs, because not having constructed the object yet, we have not constructed the totality that encompasses it. Now, it is well-known that Dummett championed a constructivist point of view in mathematics, and he interpreted indefinite extensibility as showing that there are cases in which the truth values of mathematical statements can be considered as undetermined. In other words, he gives a constructivist interpretation of indefinite extensibility[57]. Therefore, his version of indefinite extensibility is incompatible with impredicative definitions as 1 (but also with the other kinds of impredicativity). In general, if one takes a constructive attitude towards indefinite extensibility, then the latter is incompatible with impredicativity. However, this last result clearly depends on the constructive attitude, and not on indefinite extensibility by itself[58], as we have shown in the previous paragraphs. In fact, impredicative definitions of this kind are compatible with ZFC. As Zermelo (1908) pointed out, if we consider the objects of mathematics as determined independently of ourselves, then we can define an entity x by means of the totality to which it belongs, because the definition is only a means of individuating it, as happens when we individuated Luca as the tallest boy in the classroom (or a certain galaxy as the oldest galaxy).

Concerning the first point, the acceptance of which, according to Gödel, leads to the destruction of a great deal of mathematics, Gödel writes

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[56] Gödel [2001], Vol. 2, p. 127.

[57] On Dummett argument for intuitionistic logic from indefinite extensibility see the Appendix *Indefinite extensibility without intuitionism*.

[58] Dummett [1991], chapter 17 would not have agreed with such a claim, in fact he gave an argument to the effect that indefinite extensibility implies intuitionistic logic, and therefore it implies a constructivist standpoint towards mathematics. However, I have shown that the argument does not work in the Appendix: *Indefinite extensibility without intuitionism.*

> First of all, one may [...] deny that reference to a totality necessarily implies reference to all single elements of it or, in other words, that "all" means the same as an infinite logical conjunction[59].

2.2.2 Definition number 2

Then he continues:

> If, however, it is a question of objects that exist independently of our constructions, there's nothing absurd in the least absurd in the existence of totalities containing members which can be described (i.e. uniquely characterized) only by reference to this totality. Such state of affairs would not even contradict the second form of the vicious circle principle, since one cannot say that an object described by reference to a totality "involves" this totality, although the description itself does; nor would it contradict the third form, if "presuppose" means "presuppose for the existence" not "for the knowability"[60].

Here it is not clear at all what "involve" is supposed to mean. I propose to take as an example of definition that "involves" a totality of objects to which it belongs the case of Richard paradox[61]. The number N involves E because it is defined by diagonalization on E itself; at the same time E involves N because N should belong to E. Here the situation is rather different from the one of point 1, in fact we just have to consider the fact that if we change the enumeration of the elements in E or if we add or take away even only one element from E, the definition of N will always individuate a different object. What the definition of N denotes strictly depends on each single element of E. So, in defining N, we must refer to each single element of E and not simply to E, as happened with point 1[62]. In the case of the oldest galaxy above, if the totality of all galaxies had been different from the actual totality, it would not be necessary that the definition "the oldest galaxy" would have individuated a different object (this is only a possibility). But in the case of Richard paradox a slightly modification of E immediately modifies the referent of the definition of N. Therefore, there is a clear sense for which N depends on E in a deeper way

[59] Gödel [2001], Vol. 2, p. 128. Here Gödel is considering the possibility that universal generalization over a totality of objects does not behave as classical universal quantification does. The latter is equivalent to a (finite or infinite) conjunction. Now, a conjunction is a truth-conditional connective: its truth-value depends on the truth-values of the conjuncts. This presupposes that the conjunctions are given, in the sense that there is a domain which contains all conjuncts. Gödel is thus considering the possibility of a universal generalization that does not behave truth-conditionally, because the conjuncts may not be given. He might be thinking of intuitionistic universal quantification, in fact in intuitionistic logic we can have a universal quantified sentence when we have a rule that applies recursively to an object producing an object of the same sort. In this sense, we do not have to presuppose that these objects form a determined totality, as a set. Dummett will propose exactly this type of quantification to deal with indefinitely extensibility.

[60] Gödel [2001], Vol. 2, p. 128.

[61] Since I really do not know what Gödel has in mind with this second definition, I do not want to claim that this is a faithful reconstruction of his classification. I am just using his classification to underline a difference between impredicative definitions.

[62] A similar point is made by P. Clark [1994], p. 231. He underlines that while with Richard paradox the number N depends on each single element of E, in the case of Burali-Forti paradox, the ordinal bigger than all the ordinals depends only on the well-ordering of the ordinals, and not on each single ordinals. Clark then concludes that this undermined the thesis that the ordinals are indefinite extensible. We agree on the difference between the two cases, but not on Clark's conclusion: what the difference shows is simply that in the two cases we are dealing with two different principles of extensibility.

compared to the dependence of an object to the totality to which it belongs in point 1. If you consider again the definition of Luca as the tallest boy in the classroom (definition of kind 1), it is clear that Luca can exist even though the other boys in the class had never existed. This is not the case with number N (if E were empty, then there would be no element to diagonalize over and, consequently, there would be no number N). We could express this dependence as follows:

a) for each α, where α is an element of E, the number N involves α.

The problem is that the number N is one of the values of α. So, the definition of N involves N itself, which is a clear circle: we would like to define N, but this is possible only if we have already N. In other words, we must already have a definition of N in order to define N. The circularity here is vicious. To claim that the concept "being a number definable in a finite number of words" is indefinitely extensible is a possible way of avoiding this vicious circularity[63].

2.2.3 Definition number 3

However, the most interesting point is the third. Point 3 expresses a different kind of vicious circle, which in turn corresponds to a different kind of impredicative definitions. Consider the Russell set R, the set of all sets that do not belong to themselves. This set is defined by reference to the totality of sets that do not belong to themselves. But in this case the definition is not merely a way of individuating an object, because in order the Russell set to exist, *all* non-self-membered sets must exist. This is because a set is defined by its elements (remind that the condition of identity for sets is the axiom of extensionality) and R is defined as the set whose elements are *all* and only the non-self-membered sets. Since a set is determined by its elements, it seems fair enough to say that a set presupposes the existence of its elements. This simply means that we can have the elements of a set without having the set, but not vice versa. If we consider the cumulative hierarchy of sets, all this means that if we take an arbitrary set A of a rank α, in that rank there will also be all its elements; but if we take some elements in α the set containing them does not need to belong to the rank α (it may belong to α+1). Therefore, we may conclude, the definition of R is a definition of kind 3, where "presuppose" means "presuppose for existence": in order a set to exist, its elements must exist.

Notice that it is this kind of impredicativity that is involved in the set theoretic paradoxes (Burali-Forti paradox and Cantor's paradox, and in the proof of Cantor's theorem). What is problematic is not that the Russell set is *defined* by quantifying over all sets that do not belong to themselves (this feature is shared by impredicative

[63] I would also classify Berry paradox as a paradox that presents such kind of impredicativity. If D is the set of all integers definable in less than 19 words, "the smallest number not in D" is definable in less than 19 words and so should be in D. This number does not correspond to D itself, and by its definition it should be different from each member of D: for each α, where α is a number in D, this number is different from α. So, the circularity is exactly the same of Richard paradox.

definitions of kind 1); what is problematic is that, at the same time, Russell set must contain *all* these sets. Not only is it in the range of the quantifier present in his definition, but furthermore it is the set that contains all sets in the range of that quantifier. The difference with point 2 consists in the fact that number N does not contain all numbers of E, but is simply defined with reference to each single element of E. In other words, Russell set presupposes for its existence each non-self-membered set, since it is the set of all of them[64]. Thus, we have the following situation:

b) for each α, where α is a variable for non-self-membered sets, Russell set presupposes α.

The problem is that Russell set is one of the values of the variable α. For suppose otherwise: it is a self-membered set. Then, by its definition, it is a non-self-membered set. The result is that *Russell set presupposes itself for its own existence*. This sounds as a bad conclusion: how can something presuppose itself for its own existence?

2.2.4 Accepting impredicativity, whilst abandoning the vicious circle.

The classification above has shown three different forms of impredicativity. We argued that the first definition is problematic only if one accepts a constructivist approach to mathematics; otherwise its circularity is not vicious at all. Things are different for the second and the third definitions. In these two cases, we argued that the underlined circularity is vicious. The problem now is to avoid the latter two versions of the vicious circle, but not the first one. Of course, if at this point one proposed to ban impredicative definitions, just as Poicaré and Russell did, one would ban also definitions of the first kind, which are legitimate. But we do not need to be so drastic. Consider the case of Russell set. One of the presupposition of the derivation of the paradox is that we are working inside a fixed universe. In this case, if Russell set is a non-self-membered set, then the quantifier present in its definition must range over it. But if we take into consideration the possibility of an expansion of the universe, then Russell set may be a non-self-membered set and not be in the range of the quantifier present in its definition. In this scenario impredicative definitions are perfectly allowed: for all non-self-membered sets of the universe there is a set that contains all of them; however, this set cannot be one of the element of the universe: we have a means to expand the universe in a more comprehensive one.

The argument just given is the standard version of the argument for indefinite extensibility, which exploits impredicative definitions. One must notice that as soon as we expand the universe and we claim that R is not a member of itself and, at the same time, is not one of element in the range of the quantifier which is present in its

[64] It is straightforward to see that Burali-Forti's paradox presents a circularity of this kind, in fact to the totality of all ordinals corresponds an ordinal, which is the order type of the set of *all ordinals*. For the same reason, I would also classify König's paradox as an example of this third kind of impredicativity: the first non-definable ordinal is the order type of the set of *all definable ordinals*; but since it can be defined by the sentence "the first non-definable ordinal" it must belong to itself.

definitions, the definition of R becomes predicative. So, one might argue, we are rejecting impredicative definitions. But this conclusion is too quickly. Because the claim that the universe is indefinite extensible is called into the game exactly because we used an impredicative definition (of kind 2 or 3). So, it is the legitimacy of impredicative definition of such kind that force the universe to expand. It is true that after the expansion the definition of R results to be predicative, but now we are in a new universe where we can define a new set R' in an impredicative way. In turn, this would force another expansion and so on. In this sense, impredicative definitions are essential for indefinite extensibility. We cannot apply an impredicative definition to an indefinitely extensible universe, but we can apply it to a fixed universe to make it expand. However, the result of the expansion is not only that of making an impredicative definition predicative, but it also eliminates the third form of the vicious circle: since Russell set is not in the range of the quantifier present in its definition, Russell set does not presuppose its own existence anymore. *Therefore, the champion of indefinite extensibility is in the position of accepting as fully legitimate impredicative definitions of type 2 or 3 and, at the same time, she can avoid the corresponding version of the vicious circle*. An interesting thing to notice is that this is not the case with definition 1. In such a case, we cannot accept the impredicative definition and reject the vicious circle: if we believe that 1 states a vicious circle, then the corresponding impredicative definition must be avoided.

Discussing Poincaré's position we found an argument to the claim that we cannot use an impredicative definition over an indefinitely extensible domain, because the former requires the domain to be definite. But the considerations in the last paragraph shows the problem of that argument. The argument already presupposes that the universe is indefinitely extensible, while impredicative definitions must be used to expand the universe. It is impredicativity (of the second or the third kind) that, defined over a definite universe, forces this universe to expand. We define N or the Russell set in a fixed universe; the consequence is that we are forced to expand the universe. In this expanded universe, N does not belong to E and R does not belong to itself so from the point of view of this new universe, their definition is predicative; however, we can impredicatively defined a new N or a new R, which will force a further expansion. It is true that we cannot use an impredicative definition over an indefinitely extensible totality, but we can use an impredicative definition to show that a definite totality is, in reality, indefinitely extensible. It is in this sense that impredicativity is compatible with indefinite extensibility.

Another important aspect to notice is that we were able to eliminate the vicious circularity without recurring to a request of well-foundedness. If we had imposed a constriction of well-foundedness, we would have come to the same conclusion: no set can be one of its own elements. But we do not need this further assumption: that the problematic sets of the paradoxes cannot be elements of themselves is a consequence of the indefinite extensibility thesis.

This has an important consequence for an opponent of impredicativity. The fact that we can dismiss the third form of the vicious circle without dismissing the correspondent impredicative definition shows that one cannot argue – as Poincaré and Russell – that impredicativity must be dismissed because it leads to a vicious circularity. The Poicaré's quotation above («thus, *the definitions that must be regarded as non-predicative are those which contain a vicious circle*») is thus simply false. It is true that predicativism can be defended by other arguments, for instance by a constructivist approach to mathematics. However, the classical russellian argument from the presence of a vicious circularity is not valid. Of course, the difference between the second or the third form of the vicious circle and the correspondent form of impredicative definition can be seen only in a framework were the universe expands. In a fixed universe, there is no gap between the vicious circle and impredicativity, and, in the cases where this circularity is present, we face the paradox.

2.2.5 An interesting example: Basic Law V

An exactly similar phenomenon happens with BLV. We know that a predicative form of BLV is consistent within SOL. But we also know that impredicative BLV and FOL are together consistent. Therefore, the contradiction must arise in the connection between impredicativity and SOL. BLV is the following law: $\forall F \forall G \big(\varepsilon(F) = \varepsilon(G) \leftrightarrow \forall x(Fx \leftrightarrow Gx)\big)$. Here the explanation: the right-hand side of the law ($\forall x(Fx \leftrightarrow Gx)$) states an equivalence relation between concepts that introduces new objects (the extensions of the left-hand side), but these new objects were already in the range of the quantifier $\forall x$. The reason why this is called an abstraction principle is that the objects that it introduces, the extensions, are obtained by abstraction from an equivalence relation. The presence of such a relation shows that the two concepts are equal with regards to a particular aspect: the quantity of elements that instantiate them. By expressing an identity statement between these two aspects, the left-hand side of the law treats them as *objects* of predication. The existence of extensions thus depends on the equivalence relation. But the problem is that the equivalence relation is defined on a range of objects between which there are already the objects that it is supposed to introduce (this is due to the fact that extensions are between the objects over which the quantifier $\forall x$ ranges): therefore, these objects exist because of the abstraction in the equivalent relation, which in turn requires them as elements of its domain. These objects involve each element in the range of the quantifier in order to exist and, consequently, they involve themselves. The circularity seems to be exactly the one that is banned by the second form of the vicious circle (this is not a third form circularity because these extensions do not comprehend all the elements in the range of the quantifier).

What we said above on the relation between impredicativity and the second form of the vicious circle can now be exploited to save an impredicative version of BLV. We just have to claim that the new objects introduced by the equivalence relation are not in the range of the quantifier $\forall x$. This, in turn, means that their presence produces an expansion of the starting domain, which is due to the abstraction aspect of the law. In fact, we can avoid the paradox by claiming that the nominalization of the equivalence relation produces new objects that were not present in the starting domain. Again, by allowing the universe to expand we can save an impredicative version of BLV together with SOL without falling in the paradox[65].

2.3 Conclusion

In this paragraph, we have defended two theses: firstly, impredicativity is compatible with indefinite extensibility; secondly, in an expanding universe we can distinguish impredicativity and the vicious circle: while we can accept the former, we cannot accept the latter. This is possible because in an indefinitely extensible universe, impredicativity and the vicious circle cannot be identified.

Of course, one can argue for indefinite extensibility even without appealing to impredicative definition: for instance, given a domain D of objects, it is enough to consider the domain obtained by the union of D with its singleton to find a more comprehensive domain. Or, given some objects, it is enough to apply the operation of *set of* to those objects to find a more comprehensive domain of objects. However, the interest towards indefinite extensibility towards impredicativity relies on the fact that it does not presuppose a certain conception of set (in fact, the standard argument for IE works within the naïve conception of set), and so can teach us something important about the nature of concepts and their extensions, as we shall explain in chapters 6 and 7.

[65] This is admittedly just a rough sketch. For a development of these considerations see chapter 7, §7.

CHAPTER 3

THE INEXPRESSIBILITY OBJECTION

In this chapter, we discuss one of the most important objection against a relativist position in the absolute generality debate. This is the inexpressibility objection, which accuses the relativist of not being able to coherently express her own position. We are going through different formulations of the objection and different replies relativists have given. The general result of the chapter will be that the objection in fact succeeds; however, we shall also arrive at three more particular, but not less interesting results: 1) the objection only depends on denying the possibility of absolute generality, while nothing depends on the fact that this generality is considered to be expressed by an unrestricted quantification or by means of another logical device; 2) that relativism is not coherently expressible does not imply that it is false; 3) a modal version of absolutism finds itself in a better position with regard to the challenge posed by a certain form or relativism than standard absolutism.

1. Introduction: a wittgensteinian problem

In the *Tractatus Logico-Philosophicus* Wittgenstein puts very nicely the problem we are going to deal in this chapter: "in order to be able to set a limit to thought, we should have to find both of the limit thinkable (i.e. we should have to be able to think what cannot be thought)" (Proposition 3). Later on, Wittgenstein adds to this picture the propositions 5.632: "The subject does not belong to the world, rather it is a limit to the world". Here, if we interpret 'the subject' as 'the thought' and the world as indicating the ordinary world made up of ordinary objects (concrete objects), we could say that the proposition expresses the idea that the thought is not an object of the world, i.e. an object with a certain limit that differentiates it from other ordinary objects. Rather the thought is the same limit (the general background) within which we *are aware of* ordinary objects. If so, then when we try to think of the thought, we delimitate it and, therefore, we treat it as if it were an object of the world; but in this way, we fall into contradiction, because as the proposition says the thought is not a part of the world.

The problem we are going to deal in this chapter is exactly the one Wittgenstein here recognizes: to set a limit to what is thinkable or to what it is expressible is possible only by thinking or expressing what, according to the limit set, cannot be thought of or expressed. In this chapter, we shall look at different ways in which this problem has emerged in the discussion about 'absolute generality'[66].

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[66] This dialectic has been famously studied by Graham Priest, in his book *Beyond the limits of thought*. It is possible to interpret our whole dissertation as an attempt to shows that Priest's thesis – that the limit of thoughts are true contradictions – is false.

2. A first formulation of the objection

The relativist is someone who does not believe that there is an unrestricted quantification. Consequently, he will claim that each quantification is restricted to a particular (not all-inclusive) domain. But a famous objection can be raised against such a position. With the words of Lewis, the objection runs as follows: "Maybe the singularist [here the relativist] replies that some mystical censor stops us from quantifying over absolutely everything without restriction. Lo, he violates his own stricture in the very act of proclaiming it!" (Lewis [1991], p. 68). If one denies that an unrestricted quantification is possible, then the resulting position entails (or expresses) something like this: "there is no unrestricted quantification".

(1) $\sim\exists x\ (x = unrestricted\ quantification)$

Or, which is the same:

(1') $\sim\exists x\ (x = absolute\ domain)$

Is the quantifier in the last sentence restricted or unrestricted? Well, if that sentence is true, then no unrestricted quantification exists and, consequently, the quantifier is restricted. But if it is restricted, the sentence cannot deny that in a more comprehensive domain there is an unrestricted quantifier (simply because the sentence is silent about this further domain). So, the sentence can be true and, at the same time, an unrestricted quantifier may exist. Hence, if the relativist would like to use such a sentence to express her position, it seems that the quantifier should be considered as unrestricted. But then the sentence is false: in fact, in this case, it would assert that no unrestricted quantification exists by using an unrestricted quantification. One can conclude that it is not coherently possible to deny the existence of an unrestricted quantification.

Another way of appreciating the objection is by underling that (2) is a logical consequence of (1):

(2) There is an *x* over which we cannot quantify.

If sentence (1) is true, then no matter what domain we consider, it turns out to be restricted and, consequently, there is something not in the domain of quantification, which is what (2) says. But sentence (2) is explicitly self-defeating: to say that we cannot quantify over *x*, we must quantify over *x!* Therefore, the relativist position does not seem to be coherently expressible[67].

2.1 Williamson's refinements of the objection

Williamson ([2003], section V) exploits in a deep and wide way the objection just stated to argue that the relativistic position is not coherently expressible. He considers different ways the relativist may try to express his position, and he concludes that none

[67] See Williamson [2003] for a deep development of these considerations.

of them succeed. The first way the relativist may express his position is the more direct way, i.e. by claiming:

(3) It is impossible to quantify over everything.

This implies that the relativist is committed to

(4) I am not quantifying over everything.

This sentence implies (5):

(5) Something is not being quantified over by me.

Suppose that the generality-relativist utters (5) at a time *t*. He is likely to express something he believes to be true. So, by standard semantic principles for truth, we have:

(6) 'Something is not being quantified over by me' is true as uttered by the relativist at *t*.

Now, according to the standard semantics principles for quantifiers and predicates, we have:

(7) 'Something Fs' is true as uttered by *s* at *t* if and only if something over which *s* is quantifying at *t* satisfies 'Fs' as uttered by *s* at *t*.

(8) Something satisfies 'is not being quantifying over by me' as uttered by *s* at *t* if and only if it is not being quantified over by *s* at *t*.

From (6) and (7) we obtain:

(9) Something over which the generality-relativist is quantifying at t_0 satisfies 'is not being quantifying over by me' as uttered by the relativist at t_0.

From (8) and (9) we get:

(10) Something over which the generality-relativist is quantifying over at t_0 is not being quantified over by the generality-relativist at t_0.

(10) is inconsistent, and so also (3) from which it was derived. Williamson puts the point very nicely: for the relativist (3) is always false. If its domain is restricted, then for the relativist it is possible to quantify over everything, because here 'everything' has a restricted domain, and for her it is certainly possible to quantify over a restricted domain. If the domain is unrestricted, then for her the sentence is false because there is no unrestricted domain.

This argument is just a more complex way of exposing the argument we gave at paragraph 2. How can the relativist reply? A second attempt may consist in transforming the contradiction in (10) into a limitative result, by appealing to a meta-language. In a language L' the relativist may utter:

(3’) It is impossible to quantify in L over everything.

From (3’) by a parallel derivation of the one before, we can derive:

(11) Something over which the generality-relativist is quantifying over in L’ at t_0 is not being quantified over in L by the generality-relativist at t_0.

This is not a contradiction, but it is *a too weak* claim. The relativist’s aim was to express that unrestricted quantification was not possible at all, and not only that it was not possible for a certain limited language L. In general, what is contradictory is a sentence as:

(12) It is impossible in my current language to quantify over everything.

The third attempt of the relativist to express her position that Williamson considers is a really interesting one[68], which consists in just shifting the language talk to the less drastic context-talk. Now, suppose the relativist, in a context C’, utters (of a different context C):

(3’’) Not everything is quantified over in C.

Again, the problem is that (3’’) is too weak. How to generalize it? The natural suggestion is (13):

(13) For any context C_0, there is a context C_1 such that not everything that is quantified over in C_1 is quantified over in C_0.

Also this sentence won’t do. The reason is that the quantifier ‘for any context’ must ranges over all contexts (although it is not necessary that it ranges over everything). If so, it will also range over an arbitrary context C. Then, according to (13) there is a further context C’ such that not everything that is quantified over in C’ is quantified over in C, which means that not everything is quantified over in C. We know by (12) that to say that C does not quantifying over everything is self-defeating if uttered at C. So (13) is also not true if uttered at C. But C was an arbitrary context, which means (13) is not true if uttered at an arbitrary context.

The fourth attempt Williamson considers is the one by semantic ascent. The relativist may utter in C:

(14) ‘Not everything is quantified over in C’ is true as uttered in C*

where ‘C’ uttered in C* refers to C. The price to pay for this move is to abandon a homophonic account of the truth conditions. In fact, *in the context C*, the relativist must reject the following bi-conditional:

[68] The form of the objection we are going to discuss will come up later on in different contexts. So, it is important to keep it in mind.

(15) 'Not everything is quantified over in C' is true as uttered in C* if and only if not everything is quantified over in C.

The reason is simply that, in this approach 'everything' is indexical: it expresses different contents in different contexts.

The problem with this strategy is that one reason to be relativist is given by Russell's paradox applied to the notion of interpretation: the idea being that meta-linguistic reflection on a language interpretation forces an expansion of the universe[69]. But here, the relativist is trying to have a semantic ascent while remaining in the narrow context C. So, he cannot exploit Russell's paradox to argue for relativism. But in this way, he undermines the main argument for her own position.

The last approach Williamson considers is the one in which the context parameter is treated as a primitive operator, and not, as in the cases seen so far, as meta-linguistic devices. The basic idea of this proposal is to express relativism by means of sentences as follows:

(16) In C*, not everything is such that, in C, it is something

where we cannot derive from (16) the following sentence:

(17) Not everything is such that, in C, it is something.

The context parameter 'In C*' is primitive, i.e. it is an irreducible operator (and for this reason it does not allow the derivation of (17) from (16)). That also this approach fails is shown by Williamson with a comparison with the modal case. Those who consider modal operators as primitive want to avoid commitment to possible worlds. In asserting that it could have been the case that Y, they are not asserting that there is world such that Y. Analogously, in asserting (16), the relativists "do not assert outright that there is something other than what, in the current context, there is. That a list is such that, in some context, it is incomplete does not imply that it is incomplete" (Williamson [2003], section V). Exactly for this reason, this strategy does not seem to adequately articulate relativism: relativism is committed to the idea that there is something outside the current context of quantification, which is exactly what is not expressed by the present strategy.

Generally speaking, Williamson's arguments make a strong case against the expressibility of the relativistic position. However, Williamson concludes that his arguments are not enough to declare the relativist defeated. Even if she cannot express her position, the relativist may try to exploit Russell's paradox to show that the quantifier the absolutist believed to be totally unrestricted did not range over everything after all. The relativist should abstain herself from exposing a positive doctrine; rather she should just focus her efforts in showing that the putative absolute

[69] See Chapter 5, § 1.5.

domain of the absolutist can be expanded. This idea has been developed further by Button [2009] and we are going to deal with it in §4. For the time being, we are going to look at different defenses of relativism.

3. Relativists at work!

It is now high time to look at the way relativists have tried to defend the expressibility of their position. We shall argue that in none of the following cases they succeed.

3.1. Glanzberg's position

A reply to the inexpressibility objection is given by Michael Glanzberg [2004], who claims that the sentence "there is no unrestricted quantification" does not need to have an unrestricted quantifier to deny the possibility of an unrestricted quantification. In fact, Glanzberg argues, it is enough that that quantifier ranges over a *restricted* domain that comprehends *all* quantifiers and nothing else. Similarly, when we say that there is no domain that comprehends everything, it is enough that the domain of this quantifier comprehends every domain and nothing else. Since this domain is restricted, the relativist does not have any problem in expressing her own position.

First of all, notice that this works only if the domains of quantification are seen as sets (or as set-like objects) and not as pluralities. A plurality of things is simply *the things* and not an additional object that comprehends its members. If domains were pluralities, then "all domains" would indicate all pluralities, that is the totality of all things[70]. In this case the quantifier would be totally unrestricted, contradicting the relativist's position.

The domains must therefore be sets (or classes, if we understand them as set-like objects). But then we can ask Glanzberg if these sets are well-founded or not (the disjunction can be read as an inclusive disjunction). Let's suppose that these domains are all well-founded sets, which means that no set belongs to itself. But this has a bad consequence for Glanzberg proposal: in fact, the domain of all domains does not belong to itself. Consequently, we can extend it by considering the union of all its members with itself, which means that if domains are considered to be well-founded sets, there cannot be the domain of all domains, as the solution needs[71]. Let's now consider the other option: these domains are non-well-founded sets. This second case divides into two sub-cases: the first sub-case is the one in which some domains are well-founded, while the others not. Of course, the domain of all domains must be not well-founded. But then we can consider the domain of all domains that are well-founded. Is it well-founded or not? It is clear that this produces a version of Russell's paradox. The reply would simply be that this domain does not belong to the starting domain, which implies that there cannot be a domain of all domains. Again, this option does not work. The second sub-case is the

[70] Here I consider even single objects as forming a plurality: a plurality of only one thing. This may seem strange if compared with the usual notion of plurality in natural language; however, this is perfectly coherent for theoretical purposes.

[71] The reasoning here corresponds to Mirimanoff's paradox, which can be exploited to argue that the concept 'being a well-founded set' is indefinitely extensible.

one where we have only non-well-founded sets. Here we cannot exploit a sort of Russell's paradox reasoning. But this is very problematic for a relativist. Firstly, if the relativist is working with a non-well-founded set theory, it is likely that her theory can admit the universal set; if so, she can actually quantify over everything (in the absolute sense of the word). And to have the universal set, one has to exclude some properties as the property of being the set of all non-self-remembered sets, otherwise the theory gives rise to paradox. But if she excludes this property, she cannot exploit Russell's paradox to argue for relativism anymore. Of course, there is the option of accepting only some non-well-founded sets, without a universal set. But this would require also the admission of well-founded sets (if we only had non-well-founded sets, it seems that there is no problem to have the universal set), with the consequence that we are back at the first sub-case.

There is a further problem for Glanzberg's strategy. Remind that his idea is that the sentence: "there is no unrestricted quantification" requires only quantification over all domains (not over all objects). His argument exploits Russell's paradox to show that given a domain, we can find a more comprehensive domain. So, it is committed with the following:

(18) For any domain D_0, there is a more comprehensive domain D_1.

That there is a domain D_1 more comprehensive that D_0 means that not everything that is quantifying over in D_1 is quantifying over in D_0. So (18) is equivalent to

(18') For any domain D_0, there is a domain D_1 such that not everything that is quantified over in D_1 is quantified over in D_0.

But (18') is equivalent to (13). And we already know that (13) is self-defeating[72]. We can conclude that Glanzberg's proposal does not work.

3.2 Grim's proposal

Patrick Grim [1990, 1993] argues that the distinction use-mention is enough to solve the problem[73]. If we say: *"unrestricted quantification" is not possible*, then we are not using the words "unrestricted quantification", but only mentioning them. We can mention them and deny that they refer as we mention the expression "the round-square" and deny that it has a referent. Grim gives this reply to an objection that was raised against his argument for the non-existence of a set of all truths. The objection is a particular case of the objection of inexpressibility we are considering. Grim's well-

[72] (13) is self-deafening just with the supposition that its quantifier ranges over all domains, not everything.

[73] Hellman [2006] starts with the same proposal of just mentioning the words 'unrestricted quantification', in the sentence "unrestricted quantification is not possible". However, he does not think that the distinction use-mention is enough. He believes that one should take a more intensional path to express the fact that every domain can be incremented. This is really close to our view. However, Hellman does not introduce a new form of generality: the result is that even if he manages, in natural language, to find a way of denying the existence of an unrestricted quantification, as soon as he formalizes it, he must express it in a quantificational way, contradicting himself.

known argument runs as follows[74]: suppose there is the set T of all truths. Consider $P(T)$, its Power-set, which contains all its subsets. To each subset of T there corresponds a truth. For example, given a particular truth t_1, either it belongs or it does not belong to a certain subset. In either case we will have a truth. So, there are at least as many truths as subsets of T, that is as many truths as elements of $P(T)$. But by Cantor's theorem $P(T)$ is strictly bigger than T, so there are more truths than truths contained in the set of all truths, which is absurd. Grim concludes that there is no set of all truths, and therefore – Grim adds - there cannot be a proposition about all truths. In other words, we cannot speak of all truths. The problem is not that his argument began by supposing that there is a set of all truth (the argument is a kind of reduction[75]); rather the problem is that the conclusion is a universal sentence about all truths. It seems, therefore, that the argument is self-defeating, because its conclusion is a universal sentence about all truths that claims that there cannot be a universal sentence about all truths! Grim's initial strategy is to argue that the conclusion is not a universal sentence about all truths, exactly because the word 'all truths' present in this conclusion are just mentioned, not used. If we were to use those words, then we would quantify over all truths and, so, contradicting ourselves. But if we just mention them, then we are just saying that those words have no reference. So, Grim's proposal is that we should be very careful by using scare quotes, when mentioning the words 'all truths' or, in our present case, 'unrestricted quantification': ‹‹I think such an objection could be avoided, however, by judiciously employing scare quotes in order to phrase the entire arguments in terms of mere mentions of supposed 'quantification over all propositions'›› (Grim in Grim and Plantiga [1993], p. 271). It is clear that these scare quotes cannot be eliminated, otherwise the sentence, in Grim's argument, would quantify over all propositions. This is Grim's strategy to make his argument sound.

Does this strategy work? Can the relativist just appeal to the use-mention distinction to express her position? The problem is that, in the standard use of scare quotes to mention some expression, if the sentence that mentions these words expresses a proposition (that is, if it expresses a meaning), then we can always drop the quotes and express the same proposition by using those words. In other words, the sentence "'the round-square' does not refer" and the sentence "there is no round-square" are taken to be equivalent (they express the same proposition). But this cannot happen in our case, otherwise we are back into contradiction. What such a proposal must deny is that the sentence

(5) "unrestricted quantification" is not possible

which mentions the words "unrestricted quantification", is equivalent to (or entails) sentence (3):

(3) $\sim\exists x\ (x = unrestricted\ quantification)$

74 Grim [1988], p. 356. See also Grim [1991].

75 See Plantiga in Grim & Plantiga [1993] p. 285.

which is formulated with an unrestricted quantifier. But if we cannot go from (5) to (3), then it is not clear at all how the scare quotes are supposed to work. In virtue of what do they prevent us going from (5) to (3)?[76]

To stop the derivation from (5) to (3), one should argue that the two kinds of sentences are not really equivalent, in the sense that the sentence that only mention the words "unrestricted quantification" expresses the real logical structure of the denial, while the other sentence represents only a superficial grammatical phenomenon. If so, then (3) would just be an inadequate way of expressing (5): the supposed unrestricted quantification in (3) would only be a misleading phenomenon due to its grammatical form. The problem with such a defense is that it is really hard to see how this could be the case. First of all, there seems to be no argument for claiming that quantification in (3) is just a grammatical phenomenon; secondly the way contemporary logic has been developed seems to indicate that it is the other way around: the sentence with the quantifier is likely to be seen as the one which expresses the real logical structure of a sentence and not vice versa. All in all, this line of defense looks quite implausible and sterile.

However, Grim seems not to be completed satisfied by the scare quotes' argument, and therefore he proposes a different reply: we can draw a positive conclusion from the argument, which should sound something like that: "unrestricted quantification is an incoherent notion", but we must stop here; in particular from this conclusion we cannot derive the sentence "there is no unrestricted quantification". But this reply does not seem much better than the one before; in fact, it has the same problem of explain why we should stop at the first sentence and not derive the second.

At this point, Plantiga came out with a really strong reformulation of the inexpressibility objection, which is worth examining in detail. I report the objection as Grim reported it, since his formulation is clearer and more insightful than in the original Plantiga's formulation:

> Were there a sound Cantorian argument with the conclusion that there can be no universal propositions – so the argument goes - would require at least one universal proposition as a premise. But if sound, its conclusion would be true, and thus there could be no such proposition. If sound its premises would not all be true, and thus it would not be sound. There can then be no sound Cantorian argument with the conclusion that there can be no universal propositions. Very nice.

What is interesting of the argument is that Plantiga develops it in order to show that Grim's argument does not work, even if it is used a mere *reductio*, without any commitment to a positive conclusion. In our present context, Plantiga's argument runs as follows: even if the relativist avoids to explicitly state her own position, she needs absolute general sentences as premises of her argument.

[76] Plantiga stresses a similar point in the discussion with Grim: ‹‹But how is that supposed to work? The conclusion will be expressed in a sentence, presumably one involving scare quotes. Either that sentence expresses a proposition or it does not. If it does not, we won't make any advance by using the sentence; if it does, we should be able to remove the scare quotes. But how can we remove the scare quotes?›› (Plantiga in Grim and Plantiga, p. 285).

Grim's answer consists in denying that we need a universal sentence about all propositions in the premises: we can run the argument only by means of particular propositions (we consider a particular set of truths, a proposition about only those truths, and so on), while avoiding generalizations to an arbitrary set of truths. Here, Grim has clearly accused the point: he has abandoned the idea of expressing a positive conclusion from the argument, and also of stating a general argument to the effect that any set of truths is incomplete. The position that seems to emerge is a sort of skeptical position: we cannot generalize the relativist position, but we can show that the absolutist fails to quantify over everything. This is the position we saw above at the end of the section concerning Williamson's objection against the relativist and we are going to analysis it in a later section of this chapter.

3.3 Studd against Williamson: a case for relativism

One of the most important defense of relativism, and of the fact that it is coherently expressible is given by James Studd [2015, 2017], relying on some ideas formulated by Kit Fine[77]. In particular, it is worth examining the reply that Studd [2015] gives to an argument developed by Williamson [2003], section VII.

Williamson develops an argument (Williamson 2003, Section VII), which aims to show that the relativist faces big problems when she tries to give the semantics for her object language (where the object language's quantifiers range – of course – over restricted contexts). Let's suppose L to be a first-order language with such features, i.e. the quantifiers always range over limited domain (contexts), and therefore their truth-conditions are context sensitive. The standard Tarski-Davidson[78] semantics for the universal quantifiers is as follows:

($\forall C$) For every context C, $\forall x\alpha$ is true in C under an assignment A if and only if every member d of the domain of C is such that α is true in C under A$[x/d]$.

If the meta-language universal quantifier is absolutely unrestricted, then there is no problem with such a way of expressing the semantics of the object-language quantifier.

[77] Fine position is quite weird. He claims that he wants to defend relativism; but to generalize the Russellian argument to an arbitrary domain, he uses a primitive modal operator, which has the effect of enabling him to express general claims over an indefinitely extensible domain. So, while defending relativism, Fine ends up with a particular form of absolutism: an absolutism that does not need a universal domain. This is in fact really close to our position; however, there are some problems with Fine's interpretation of the modality, which places his view not so close as ours as it may at first seem.

[78] Williamson only considers this kind of semantics in his 2003 paper. Studd [2015] correctly notices that there are other kinds of semantics, and argues that the absolutist faces difficult problems within the Mostowski-Barwise-Cooper's semantics (See Barwise & Cooper [1981]; Westerstahl [2014]). The reason is that such semantics is based on set theory, and therefore can be used by the absolutist only in cases where the universe of the object language forms a set. If the universe does not form a set, the absolutist cannot encode many predicates as set-extensions. Studd also argues that the expansionist relativist (see below) has no problem with such semantics, because given a certain set, it is always possible to find more comprehensive sets. However, it is difficult to look at this situation as a strong critic against absolutism: the problem for the absolutist just come from the fact that this semantics is based on set theory: she could simply argue that, exactly for this fact, it is an inadequate semantics for absolute general discourses.

But of course, the relativist will claim that also in the meta-language quantification is restricted. So $(\forall C)$ will be uttered in a context C* with a restricted domain D^*. Of course, in order for $(\forall C)$ to have its intended meaning, the domain of any context C – say D^C – must be included in D^*: $D^C \subseteq D^*$. Therefore, the first commitment the relativist must accept in order to be able to give the right semantics for the quantifier above is towards the following sentence:

1. The utterance of $(\forall C)$ in D^C specifies the intended truth-conditions for the quantifier if and only if $D^C \subseteq D^*$.

Here there is a double limitation of the object-language quantifier: for every context C, $\forall x\alpha$ is true in C under A only if every member of D* which is also a member of D^C is such that α is true in C under A$[x/d]$, which means that $(\forall C)$ has its intended meaning only if $D^* \cap D^C = D^C$.

The second commitment the relativist should make is a general one: even if there is no domain that comprehends everything, each thing must be in (at least) one domain. Otherwise she could not avoid 'semantic pariahs': to claim that there is something that cannot be expressed (which implies expressing it):

2. Each thing is one of the things of a domain D^C of at least one context C.

The last commitment is just a consequence of the relativist's claim that no domain comprehends everything:

3. Not everything is in D^*.

Sentence 3 can be truly uttered only within a context C** which must be more comprehensive than C*, the context in which we utter $(\forall C)$. If we utter 3 in C*, it will express the falsity that not every element of D^* is in D^*.

Williamson now argues that the conjunction of 1, 2 and 3 leads to contradiction: by 2 each thing is in a domain D^C of a context C; by 3 in D* there are not everything. Therefore, we have $D^C - D^* \neq \emptyset$, which contradicts 1: $D^C \subseteq D^*$.

Studd's reply to Williamson's challenge addresses the kind of relativism the argument presupposes. Relying on Fine [2006], Studd individuates two ways to enlarge a domain. The first way consists in just de-restricting a domain which is restricted by means of a predicate. This can be done by generalizing the property that restricts the domain. However, there is a limit to this way of enlarging domain: when we come to a property as 'being a thing'. In such case, the domain cannot be enlarged further. A relativist that endorses such a picture of domain restriction would just claim that it is not possible to relax the property that restricts a domain until reaching the maximal extension. Let's call this relativist a 'restrictivist'. To explain in what the second way consists, Studd introduces an important distinction between domain and universe. A domain is bounded to a quantifier, and can be conceived as a context. While a universe is linked to a language, and therefore it is not relative to a particular context. The universe of the language collects each object from whatever domain (context). So, the relativist will claim that no domain is as large as the universe of the language, while the absolutist will claim that, in some cases, some domains are as large as the universe of the language. Given a fixed interpretation, the domains of the quantifiers are always sub-domains of the universe.

Premise 2 above forces to accept that the universe of a language contains each object contained in each domain of quantification. If this were not the case, then there would be semantic pariahs for the language. Both the restrictivist relativist and the absolutist accept that the universe of a language contains every object, because they accept premise 2. But, according to the restrictivist relativist, no domain of quantification is as large as the universe of the language, while, according to the absolutist, some domain is as comprehensive as the universe itself[79].

The second way of enlarging a certain domain is by expanding the universe of a language. Let's call this relativist an 'expansionist'. According to the expansionist, the quantifier can be unrestricted (so its domain can be as large as the universe), but it is always possible to expand the same universe. We can expand the universe by reinterpreting the meanings of our words to allow a more liberal language.

With these two different kinds of relativism, Studd argues that Williamson's argument certainly works for the restrictivist relativist, but not for the expansionist. In the restrictivist case, since each object is in one of the domain D^C of the contexts C and since the domain $D^*$is restricted, there must be at least one object which is in one of the D^C but not in D^*. But in the expansionist case, the argument is not valid anymore, because the expansionist will deny premise 2. For the expansionist, since the universe of a language can be expanded, there are, for each language, semantic pariahs. However, they are only temporary pariahs, because they can be expressed in a more comprehensive language. The expansionist relativist can therefore give the semantics for a language that can be expanded, provided that its meta-language has a universe at least as comprehensive as the universe of the object language (remind that the expansionist has no problem in allowing an unrestricted quantification over the universe of a language). Of course, such relativist must prove that, given a language, we can expand its universe, otherwise the positions would collapse into the standard absolutist position (Studd's argument relies on a certain reinterpretation of the indefinitely extensible argument which he develops in Studd [2017]).

Let's start with a language L with a domain D_L. The relativist wants to claim that D_L is not absolute: she can use Russell's paradox on D_L so as to expand it to a language L^+ with

[79] Notice that the restricted relativist seems not able to express that no domain contains everything in the universe; to express it, the quantifier 'everything' should range over the whole universe, that is there must be a domain as comprehensive as the universe.

a domain D_{L^+}. In this shift, it is fundamental that no object of D_L gets lost. In L^+, she can claim:

(6) Not *everything* which is quantified over in D_{L^+} is quantified over in D_L.

The quantifier in 6 rages over the domain D_{L^+}. If we suppose that 6 is true, then there is no self-defeating problem for the relativist: by means of a quantifier defined on D_{L^+} she is claiming that no quantifier defined in D_L is absolute. Moreover, if 6 is true, then all elements of D_L are also elements of D_{L^+}: so, the relativist will have no difficulty in stating the truth-conditions for quantified sentences of L in L^+.

However, at this point there are at least two reasons why the relativist should not be completely satisfied with this solution: firstly, since the reply is meant to address the last of Williamson's argument above, she wants to claim the also the domain D_{L^+} of L^+ is not absolute; secondly, the relativist position consists in claiming that no domain whatsoever is absolute. For both these reasons, she need to generalize her argument.

One possibility is just to reiterate the argument: if we want to claim that also D_{L^+} is not absolute, we perform the same argument on D_{L^+}, and we get an even more expressive language L^{++} with an expanded domain $D_{L^{++}}$ such that $D_{L^+} \subseteq D_{L^{++}}$. From $D_{L^{++}}$ we can coherently claim that D_{L^+} is not absolute. But for this new domain, the problem raises again. And again, we can reiterate the strategy. However, by reiterating the strategy we can never claim that there is no absolute domain at all, because given a certain language L* we *cannot* coherently claim in L* that its domain is not absolute. But if it is not possible to generalize Studd's reply, then the reply cannot be seen as a coherent case for relativism.

3.4 Generalizing Studd's reply

How to generalize the reply, without being committed to a language with an absolute domain of quantification? Recall that the general inexpressibility objection consists in showing that the negation of unrestricted quantification needs unrestricted quantification to be stated. The two elements that are responsible for that result are the followings:

A) The reference to everything

B) The fact that this reference is expressed by means of a (standard) quantifier.

The central part of the argument is played by point 1. *If we deny being able to refer to everything, then we are referring to everything*. Notice that this last sentence does not say anything about the form of reference. Of course, if we interpret the "everything" in the italic sentence as a standard quantifier, then also point B is operating in the argument and we end up with the original objection. However, it is not necessary to accept point B. If it possible to show that absolute generality requires a form of generality different from the standard one, then the original argument is stopped and we can claim, without contradiction, that there is no absolute domain of quantification.

The expansionist relativist may thus try to generalize Studd's suggestion above by means of a different form of generality. Following Fine [2006], she can use a primitive modal operator to express cross-domain generality. A sentence as

(7) $\forall x\ (x = x)$

becomes

(8) $\Box\forall x\ (x = x)$.

The last sentence does not need an absolute domain, because it makes an affirmation that is true however you can expand the domain. It says something that must be *structurally* true if we want to have a domain in general. The fact that sentences of this kind does not need an absolute domain of quantification simply means that the true or falsity of their claims does not depend on the objects that are present in a certain domain, rather they depend on structural features of domains: if the relativist is right in claiming that no domain is absolute, then 'being non absolute' expresses a structural condition on domains.

The expansionist relativist can reply to the objection above exactly by showing that the reference to everything is not a quantificational reference, but it needs the work of modalities. So, she can deny the existence of an unrestricted quantification, without using an unrestricted quantifier. The sentence (3) and (3') respectively become now

(9) $\sim\Diamond\exists x\ (x = unrestricted\ quantification)$

(9') $\sim\Diamond\exists x\ (x = absolute\ domain)$

Again, the latter express a structural truth (according to the relativist) depending on the nature of the domains. So, it does not need any domain of quantification.

Of course, the merely introduction of modalities does not solve the problem. How do we have to understand sentences with these modalities? If we can reduce these sentences to sentences without the modal operator, then we are back into the contradiction. In order for this solution to be effective, this reduction must be impossible: therefore, the modalities must be *primitive*. What the relativist here is doing is to introduce a new form of generality, different from the quantificational one. If we do not introduce a new form of generalization and we maintain that the quantificational generality is the only form of generality at our disposal, then there is no room to deny an unrestricted quantification without presupposing it. The only way of making this denial coherent is with a different form of generality, not reducible to the quantificational one.

The primitive character of these modalities indicates the presence of an autonomous form of generality[80].

The relativist can give an elegant answer to the inexpressibility objection; indeed, her answer is the only which is not immediately self-defeating and manages to say that there is no absolute domain of quantification. This is possible by means of a primitive modality. If we have strong arguments against the existence of an absolute domain (as the relativist claims) the fact that these modalities can help her avoiding the inexpressibility's objection constitutes a good reason in favor of them, otherwise she would find herself in a dilemma: no absolutely unrestricted quantification is possible because there is no absolute domain, and she cannot say what we have just said because the same claim presupposes an unrestricted quantification.

However, things are not so easy for the relativist, as you may have noticed. The reason is that the inexpressibility objection has been overcome by means of a new form of generality, which makes generalizations over any domains possible. But this means that absolute generality has been reinstated. In this scenario, no standard quantification can be absolutely general; however, the modalized quantifier manages to make absolutely general claims, as claim 8 above. The resulting position is therefore not relativistic, if the relativist wants to claim not only that unrestricted quantification is not possible, but also that absolute general claims are not possible.

We may dub this non-standard form of absolutism "modal absolutism", or "expansionist absolutism" or "potentialism".

The modal strategy allows to reinstate absolute general claims without the necessity of having an absolute domain of quantification. Even if this position denies that there is an unrestricted quantification over everything, it is better not to consider it a relativistic position, since it allows absolutely general claims. But this means that this strategy cannot be used by a relativist, who denies absolute generality.

4. Incoherence yes, but falsity?

The examination of the possibility of expressing the relativist position has ended up in a rather negative way for the relativist. We have found no way to coherently express the relativistic position. However, this cannot exclude that there may be other ways, not examined so far, that may allow the relativist to state her position. In any case, until somebody comes out with such a solution, since the abundance of arguments for the inexpressibility of relativism, we conclude that relativism is not coherently expressible.

[80] We mentioned above the position of Hellman [2006], who proposes a more intensional way to express the negation of an absolutely unrestricted quantification. Since modal operators are intensional operators, this proposal may be really close to what Hellman has in mind. However, he does not go so far as introducing a new form of generality and this, ultimately, makes his position unstable.

However, does this imply that it is also false? Does inexpressibility imply falsity? If this were not the case, then it is possible that the relativistic position is true but inexpressible, while the absolutist position is false but expressible. This does not seem a really comfortable scenario. However, as we saw at the end of our discussion of Williamson's argument and in the discussion on Grim's strategy to save his argument, the fact that relativism is not expressible is not enough to conclude for its falsity. This possibility has been deeply analyzed by Tim Button [2009]. It is in this form that we are going to look at it.

Button embraced the idea that restrictivism (how he calls relativism) is not coherently expressible. However, he believes the problem to be in stating *a positive restrictivist conclusion*. Rather, the restrictivist should not affirm a positive doctrine, but look at their own position as a *challenge* to the absolutist. Button calls such a restrictivist 'Dadaist' to keep her distinct from the 'doctrinal restrictivist', who interprets restrictivism as a positive doctrine. The idea being that when an absolutist claims to have a sentence that quantifies over everything, the restrictivist needs just to produce a sort of *ad hominem argument* (based on Russell's paradox) to show that the specific domain on which the absolutist was quantifying was not absolute after all. If the Dadaist succeeds, she should not draw from his victory any positive conclusion: ‹‹our Dadaist therefore thinks that *any putative doctrine whatsoever about 'unrestricted quantification' fails in its ambitions, whether that doctrine is generalist or restrictivist*›› (Button [2009], p. 395). That's why the Dadaist poses just a challenge to the absolutist[81].

Button considers two possible objections to Dadaism, the answers to them clarify what he has in mind. The first objection is as follows: the Dadaist poses her challenge because she thinks that she is always be able to show that a domain is not absolute. To do that, she exploits Russell's paradox to extend any given domain. However, in exploiting this reasoning she makes use of sentences with unrestricted quantifiers over everything. For example, it is likely that she will rely on what Button calls 'the extensibility principle': given any totality of objects, we can find some object which is not in that totality. But this is an absolute general claim, and in such case the Dadaist would affirm a positive truth in a not coherently way. This is exactly one of the objection we saw Plantiga moved against Grim. Grim's answer was to state the argument by using only particular sentences; but then Plantiga replied that if those particular sentences imply universal sentences, then Grim was back into troubles. Button's answer is different. The argument the Dadaist uses to show that a particular domain is not absolute is not a positive argument, rather it is a *reductio ad absurdum.* And in a *reductio* argument, we are not forced to be committed to the premises of the argument. The Dadaist can claim the extensibility principle in a *reductio*, without being committing

[81] This is idea is inspired by a certain interpretation of the Protagorean sentence 'there are no absolute truth'. The sentence is self-defeating; however, the Protagorean attitude should not take to be committed to a positive truth, rather it should be thought as a challenge to everybody who claims to have an absolute truth. The same for Fayerabend's irrationalism, which I take to be the true inspiration of Button's defense of Dadaism.

to the general truth it expresses (or, according to the Dadaist, without being committing to the general truth it seems to express).

But then comes the second objection: if the Dadaist's argument is a *reductio*, then its conclusion should be the negation of one of the premise. But this negation is an absolutely general claim (if the negated sentence is absolutely general). To this objection, Button replies by saying that strictly speaking the argument is *not* a proper case of a *reductio*. A *reductio* starts with a meaningful premise, it supposes that it is true, and it derives a contradiction that allows us to conclude the falsity of that premise. But for the Dadaist the premise of the argument (that there is an absolute domain of quantification) is *not meaningful*: ‹‹in fact, I start by pretending that some sentence *makes sense* (some sentence containing the phrase 'absolute generality' or 'absolutely everything'), and I then produce a series of sounds which might *seem*, to my opponent, like a logical argument towards a contradiction›› (Button, [2009], p. 395). If the argument is logically valid, since for the absolutist the premise makes sense, then for her the argument is an authentic *reductio* of the premise. However, this is not the case for the Dadaist, for whom every passage of the argument is, strictly speaking, a non-sense: the reason why the second objection is not a problem for the Dadaist is that pretending that the sentences are meaningful does not commit her to accept that those sentences actually make sense.

Is this position coherent? Generally speaking, it seems that the answer should be positive. If the Dadaist is not committed to any positive claim, then there is no possibility at all to show that it is committed to the negation of its claim. However, life as a Dadaist seems to be a really hard one. First of all, the Dadaist is only improperly a relativist. A relativist believes that no absolute general claim is possible. But the Dadaist would reply that as soon as you believe *this content*, then you have a positive claim that cannot coherently expressed (uttered, written, thought, or believed!). So, you could not coherently believe that no absolute general claim is possible. Moreover, this fact makes the challenge a quite weird challenge: usually, if I seriously challenge somebody on a certain matter is because I think I have all the means to win the challenge. But this cannot be the case for the Dadaist: if she believes that she shall win the challenge because she has some principle that allows her to expand every domain, then she is believing an absolutely general claim! A real Dadaist must just engage the challenge and see what happens. In other words, before the match, she cannot believe that she is going to win. All in all, a Dadaist may be happy with all this. But there is a consequence of this position that makes the Dadaist view a rather weak view: being not committed to any positive truths, Dadaism is not a denial of absolutism. Moreover, since it must proceed case by case, even if the Dadaist succeeds in showing that a certain domain is not absolute, there is no guarantee that she will always succeed. In other words, a Dadaist can never show the falsity of absolutism. So, it is not clear at all that Dadaism is a real threat for absolutism. Consequently, it is not clear at all that the absolutist should care about it.

A further problem for a Dadaist concerns the possibility of claiming to be a Dadaist. Suppose a Dadaist claims: I am a Dadaist. You could ask what a Dadaist is, and you will probably receive a reply. In this reply, there must appear somewhere the phrase ‘absolute generality’ and similar (for instance, in saying that Dadaism is a challenge to absolutism – as Button affirms). Now, all the occurrences of this phrase are meaningless for the Dadaist (maybe not for you that asked the question, which means that you can understand what the Dadaist is saying). Consequently, from the Dadaist point of view, every time she says to be a Dadaist she actually utters (or thought) a non-sense. A Dadaist cannot say of herself to be a Dadaist. And for the same reason, she cannot believe to be a Dadaist (she cannot think of being a Dadaist, and so on).

The last objection seems quite destructive for Dadaism. It seems that nobody can be a Dadaist. However, it is not clear that, even granted this last point, the fact that relativism is not coherently expressible and Dadaism cannot be supported is enough to say that absolutism is true. The standard absolutist must show that its actual domain is absolute, but as matter of fact she can always fail to show that. However, it must be noticed that the challenge of the Dadaist can be posed only if there is somebody who claims that a certain domain is absolute. But this is not the case with the ‘modal absolutist’, as we have seen above, who claims that absolute generality is possible, even if there is no general domain of quantification. From this point of view, the modal absolutist is untouched by the Dadaist’s challenge, exactly because she does not require the existence of an absolute domain. In addition, the modal absolutist is in a position to claim that there is no absolute domain (as the Dadaist would like, but cannot claim), without falling into contradiction.

CHAPTER 4

AGAINST SCHEMATISM (TO EXPRESS ABSOLUTE GENERALITY)

Abstract: In the debate on absolute generality, many authors have defended a relativistic position, namely that quantifiers are always restricted to a less than all-inclusive domain. Consequently, they hold that an unrestricted quantification over everything is not possible. One problem for such a view is the need to explain the apparent absolute generality of logical laws, like $\alpha = \alpha$ or $\sim(\alpha \wedge \sim\alpha)$. The standard response appeals to schemas. In this paper, I begin by examining the reasons why schematic generality has such a strong appeal in this debate, before raising an objection to show that schemas cannot be a good substitute for quantificational generality. What ultimately the paper shows is that to express absolute generality over an indefinitely extensible sequence, we need a form of generality that is both open-ended (as schematic generality) and express a proposition with a determined truth-value (as quantificational generality).

Key words: absolute generality, full schemas, relativism, indefinite extensibility.

1. Introduction: the relativist position and the appeal of schemas

One possible response to the set theoretic paradoxes is to deny that quantification over everything is possible. Notoriously, Russell was of this view, blaming impredicative definitions as the culprit of the paradoxes. His solution, the ramified type theory, bans impredicative definitions and does not permit a universal type, a type of all types. Ramified type theory is simple type theory with the supplementation of orders (sometimes called 'levels'). The universe is divided into infinitely many types: at type 0 there are individuals (things that are not classes); at type 1 there are classes of individuals (classes of type 0 object); at type 2 there are classes of classes of type 1, and so on. There is no universal type: each type has an immediately upper type. The type-stratification is not enough to ban impredicativity: for instance, at type 2 it is possible to quantify over all subclasses of type 1 (that is all subclasses determined by predicative or impredicative formulas). To avoid impredicativity, Russell adds 'orders', which introduce a stratification in the definitions of the classes. Apart from type 0 (where there are no classes), each type has infinitely many orders: at order 0 there are *predicative classes* (classes defined by means of individuals – if we are in type 1 – or by means of classes of an inferior type for each type >1); at order 1, there are classes defined by means of order 0 classes; at order 2, classes defined by means of order 1 classes, and so on. In this way impredicative definitions are prohibited: no class can be defined by means of the totality of classes to which it belongs.

In type theory, each quantifier is bound to a certain type, so that there cannot be a quantifier which ranges over all types. Nevertheless, we cannot avoid generalizing over any type: e.g. we may want to say that for each type there is an upper type. Russell and

Whitehead proposed to consider generalization over types as *typical ambiguous*. A formula such as $\vdash \varphi(x)$ expresses a determined proposition with a truth-value only when we make explicit the type of its variables. Until then the expression is ambiguous, in the sense that different substitutions give rise to different propositions of different types. In contemporary terms, a typically ambiguous expression is a *schema*, not a sentence. For Russell and Whitehead, their idea was that in asserting a formula such as $\vdash \varphi(x)$ we are not asserting a single statement, rather we are asserting *any* of its instances (Potter [2008], pp. 196-197).

A similar use of schemas can also be found in the contemporary debate on absolute generality. Let's call 'absolutism' the position according to which an absolutely unrestricted quantification is possible. An absolutist is therefore someone who believes there to be an all-inclusive domain of objects and that this domain is available for us to quantify over. Let's call 'relativism' the position according to which no absolutely unrestricted quantification is possible. A relativist will argue either that an all-inclusive domain does not exist, or that, if such domain does exist, it is not available for us to quantify over (Rayo & Uzquiano [2006], Introduction)[82].

Relativism concerning quantifiers must deal with a *prima facie* counterexample to their position: the generality of logical laws. How can we express the generality of logical laws if no unrestricted quantification is available? In a similar way to Russell's and Whitehead's proposal, the general strategy relativists have used to address this problem has been to appeal to schemas. Schemas are used extensively in logic; as such it is not surprising that many authors have thought of them as possible substitutes for unrestricted quantification. If we cannot express the generality of logical laws by means of unrestricted quantification, because there is no authentic unrestricted quantification, then we can use schemas to express such generality. Or at least that is the basic idea.

The structure of the paper is as follows: in §2 I define schematic generality and demonstrate how it differs from quantificational generality; in §3, I introduce the notion of a 'full schema', which is meant to be a particular kind of schema that can substitute unrestricted quantification, before considering some of the reasons relativists have given for affirming that schematic generality is not reducible to quantificational generality; §4 raises an objection against the use of schemas to express absolute generality, and argues that such a strategy fails; §5 concludes with some philosophical reflection on what the meaning of this failure is.

2. What is a schema?

[82] Throughout the paper I take quantification to be classical quantification and its semantics to be classical semantics, which means that the determinacy of the truth-value of a quantified sentence requires the specification of a domain of objects to act as the universe of discourse. Here and in the paper, I shall follow the standard use of the word 'domain', which does not indicate a set (or set-like collection) of objects, rather just *the objects* (or the plurality of objects, where the term 'plurality' is used as in plural logic) (Rayo and Uzquiano [2006] p. 2).

A schema is a system composed of a syntactic string of words or symbols, and placeholders (usually indicated by meta-variables, which are to be considered empty places), together with a *side condition* which explains how the placeholders (the meta-variables) must be substituted to obtain some instantiations of the schema (Corcoran & Hamid [2016], pp. 1-2).

Schemas are widely used in contemporary logic and mathematics, e.g. they are used to specify axioms and inference rules in a logical system or, more generally, to express logical laws as $A \vee \sim A$. The latter schema can be fulfilled in many ways, but the side condition prescribes us to substitute the propositional letter A with a well-formed sentence of English and to take the two logical symbols as disjunction and classical negation, respectively. Moreover, they are also widely used to formalize theories, as first order Peano Arithmetic (PA_1), which are not finitely axiomatizable[83].

A schema is not a sentence of English (or, more generally, a sentence of a language), because it has some empty places (indicated by meta-variables), which must be fulfilled if we want to obtain a meaningful sentence. The meta-variables of a schema do not range over a domain of objects, rather they are placeholders to be substituted with the variables of the object language. Consequently, a schema cannot be true or false (it is not a truth-bearer): only its instantiations can be true or false. It is fundamental to keep in mind that a formula with free variables is not a schema. I shall follow Quine [1945] in calling formulas with free variables 'matrices'. Once their variables are bound by quantifiers, matrices can occur as a part of statements, while this is not the case with schemas, which are just syntactic strings of symbols ("mere diagrams instrumental to the study of statements", as Quine [1945], p. 3 puts it).

Before proceeding, we will need to establish some definitions[84]: a schema is *closed* if it does not contain any free variable (note that I am speaking of variables, not of the schematic meta-variables); a schema is a *closure* of a free schema if it is obtained by a free schema with free variables x, y and z by prefixing to it the universal quantifiers $\forall x, \forall y$ and $\forall z$. An *instance* of a schema is a statement obtained by substituting the schematic meta-variables of the schema. A closed schema is said to be *valid* if all its instances are true. Otherwise, it is invalid. A commitment to a schema is a commitment to the claim that the schema is valid.

Since a schema does not have any truth-value, it cannot, strictly speaking,[85] be asserted. A commitment to a schema is not a commitment to the truth of the schema; rather it is a commitment to its validity. We are committed to the *instances* of the schema. In other words, we are saying that *all* instances of the schema are true

[83] Another typical use of schemas can be found in semantics, when it comes to specify the conditions for the definiteness of truth. Tarski's definition of truth employs a schema, the famous T-schema.

[84] These definitions are based on Quine's definition in [1945], p. 3.

[85] Lavine [2006], p. 118 notes that even though schemas are not truth-bearers, they are not totally neutral with regard to truth: "Acceptance of a full schema is certainly not neutral with respect to truth: it commits us to truths, namely its instances, and it blocks us from taking to be true sentences inconsistent with its instances [...]. I therefore take full schemas to be, *in an extended sense*, assertible" (emphasis added).

sentences. Notice that the statement 'all instances of the schema are true sentences' is naturally interpreted as a universally quantified sentence. Introducing the commitment to a schema in this way, we are introducing it by means of *quantificational generality*. From this point of view it seems that schematic generality cannot be considered a totally independent form of generality with regard to the quantificational one, precisely because it requires the latter to express the commitment to a schema. This means that this way of introducing schemas (which, by the way, is the standard way of introducing schemas) reduces them to quantificational generality. The notion of a full schema (to be introduced in §3) should avoid this reduction.

2.1 Schemas and negation

An important difference between schematic and quantificational generality concerns negation. A universally quantified sentence such as $\forall x P(x)$ is equivalent to the conjunction $Px_1 \wedge Px_2 \wedge Px_3 \wedge \ldots$; its negation is $\sim (Px_1 \wedge Px_2 \wedge Px_3 \wedge \ldots)$, which is $\sim Px_1 \vee \sim Px_2 \vee \sim Px_3 \vee \ldots$. The scope of the negation is the whole sentence, and this is possible because the quantified sentence expresses a determined proposition with a truth-value. On the contrary, the negation of a schema Ps is equivalent to $\sim Px_1 \wedge \sim Px_2 \wedge \sim Px_3 \wedge \ldots$, the reason being that since the schema does not express a determined proposition, its negation is equivalent to the negation of each single instance of the schema.

If we want to use schemas as a substitution for quantificational generality, this might raise a problem. How can we express schematically the negation of a quantified sentence? Consider the sentence 'no donkey talks': $\forall x(Dx \rightarrow \sim Tx)$. Its negation $\sim\forall x(Dx \rightarrow \sim Tx)$ means that there is at least one talking donkey. However, the negation of the schema $Ds \rightarrow \sim Ts$ is $\sim(Ds \rightarrow \sim Ts)$: 'anything is a talking donkey' (Williamson [2003], p. 438 and Lavine [2006], p. 139). In any case, it is easy to see how we can overcome this problem. The idea is to express the negation of a quantified sentence by means of a schema *in an indirect way*. If $\sim\forall x(Dx \rightarrow \sim Tx)$ is true, and so the schema $Ds \rightarrow \sim Ts$ is not valid, then we can add a new constant symbol c so that the sentence '$Dc \wedge Tc$' comes out true, and we can write the condition $(Ds \rightarrow \sim Ts) \vee (Dc \wedge Tc)$ to capture the idea that either the schema is valid or it is not. Consequently, we can express that there is at least one talking donkey, but we cannot express it just by adding a negation in front of a schema. This method has been developed by Lavine: "for any full schema $\phi(s)$, we introduce a new constant symbol c with axiom $\phi(s) \vee \sim \phi(c)$ and use $\sim \phi(c)$ to serve as the negation of $\phi(s)$" [9, p. 139]. This method seems to give the relativist the possibility of expressing negated quantified sentences by means of schemas. In addition, the relativist can exploit this to reply to another common objection against schemas: schemas are not apt to appear in the antecedent of a material conditional. The reason is that a material conditional $\alpha \rightarrow \beta$ is equivalent to $\sim\alpha \vee \beta$. If α is a quantified sentence and schemas cannot express the negation of a quantified sentence, then schemas cannot appear in the antecedent of a material conditional. But the method outlined above, offering a way of expressing the negation of a quantified sentence, shows that this objection fails. From this point of view, there seems to be no reason to think that schemas cannot express what quantification expresses.

2.2 Two interpretations of schemas

How do we interpret a schematic claim? In the literature, there are two different ways of interpreting schemas. The first is to interpret schemas as meaningless strings of symbols that can only give rise to meaningful sentences when their meta-variables are instantiated. For instance, Whitehead suggests this reading in a letter dated 27th January 1911 to Russell: “So far from that, my view is that our symbols remain mere unmeaning forms until the types of all the variables are determined”. In a letter dated 29th January 1911, he added: “According to me until all ambiguities are definitely settled there is simply a sequence of meaningless shapes” (for the quotation of Whitehead’s letter, see Potter [2008], p. 201). Whitehead took this radical view because he was worried of making typical ambiguity expressions collapse into quantificational generality. His idea seems to be that if a typical ambiguous sentence expressed some meaning, then it would be very difficult to distinguish it from a standard quantified sentence over all types.

The second way of reading schematic generality is less radical. According to this view, schemas express a meaning, e.g. the schema $\alpha = \alpha$ express a certain meaning – the concept of being self-identical; however, because of the placeholders, a schema cannot express a determined proposition with a truth-value. The schema is a sort of indefinite claim that commits us not to a single truth, but to the truth of each of its instances.

The latter interpretation is certainly the most widespread one within the contemporary debate on absolute generality, and it is the interpretation we will presuppose in the next paragraphs. In fact, the former interpretation does not seem to provide a valid substitution of quantification generality. For instance, consider a general sentence such as ‘Everything is self-identical’. This is certainly a meaningful sentence, which we can grasp. This means that if we read it as a schema according the first reading of schematic generality, our reading is completely inadequate to the task of translating such a sentence, because it would translate a meaningful sentence into a meaningless schema. In addition, as we shall see in more detail later, schemas have been used to express generality about a potential infinite[86]. But the problem with the first reading is, again, that schemas are not meaningful. Consequently, there would be no meaningful generalization over a potential infinite. The first reading is therefore completely unfit to be used as a substitution for quantificational generality, and for this reason we are going to focus our attention solely on the second interpretation.

However, one must notice that the second, less radical, interpretation of schemas (i.e. typical ambiguous sentence) is not available with the theory of types. The reason is simply that the theory of types required that we assigned a particular type to the

[86] See footnote 86 for a brief explanation of what I mean with ‘potential infinite’.

meanings of each expression of the object language, and not only to the syntactic expression. Therefore, schemas as $\alpha = \alpha$ cannot express a unique meaning; rather such an expression is assigned different meanings (different concepts) with different types. The second interpretation is possible for those who uses schemas with ontological hierarchies, i.e. people like Glanzberg or Lavine that argues for the non-existence of an absolute domain of quantification. They need schemas not because their language is typed (which in fact it is not), but because they believe there cannot be any unrestricted quantification over everything. Therefore, they can say that a schema has always the same meaning whatever domain we consider: no type restriction applies to them.

3. *Schemas as expressing absolute generality: full schemas*

3.1 Full schemas and open-endedness

As we have seen, the ordinary use in logic of schematic generality reduces it to quantificational generality. Of course, if schematic generality presupposes quantificational generality, then appealing to schemas cannot help the relativist in the absence of unrestricted quantification. However, some authors[87] have defended the existence of a different kind of schema that should not be reduced to quantification. The most articulate defence is offered by Lavine [2006], who writes:

> Fortunately, there is another form of generality more primitive than quantificational generality that will do the job: we can take the logical rules, for example, $\phi, \sim\phi \vdash \psi$, to be schemes used to declare that any instance is valid, where 'any' is to be sharply distinguished from 'every': the statement of a rule, though it does involve generality, does not involve quantification. In our examples, ϕ and ψ are [...] full schematic variables: 'full' is added to indicate that what counts as an acceptable substitution instance is open-ended and automatically expands as the language expands. ([9, p. 118]).

A full schema is thus a schema not reducible to quantification (which implies that schematic generality is a form of generality independent from the quantificational one), on account of its open-ended nature.

What exactly does 'open-endedness' mean? Following Glanzberg [2004], we can note that the truth of a quantified sentence such as $\forall x\ (x = x)$ does not depend on the domain of the quantifier: whatever domain we choose, that sentence will be true. This insensibility can be read as a form of domain-*independence.* On the contrary, the truth-value of a (standard) quantified sentence such as 'all the bottles are empty' – $\forall x(Bx \rightarrow Ex)$ – depends on the domain of the bound variable x (the sentence 'all the bottles are empty' may be true, if the quantifier ranges over the bottles in my home; but it is certainly false, if it rages over all existing bottles). Standard quantified sentences like the latter require the specification of a domain for the quantifiers to range over, i.e. they require a universe of discourse that specifies the objects the quantifiers range over. Glanzberg concludes that, because of this difference, a sentence such as $\forall x\ (x = x)$

[87] This view has been defined by several authors. See for example Glanzberg [2004], Hellman [2006], Lavine [2006] or Parson [2006].

should be translated into the schema $\alpha = \alpha$. Full schemas are open-ended because they are domain-independent.

This difference is mirrored by the epistemological status of the two kinds of generalization. The domain-independency of the former implies that we do not need to look at how the world is to know that everything is self-identical, rather this is known to be case *a priori.* On the contrary, since the latter kind of generalization is domain-dependent, we must look at how the world is (less grandiosely: we must look at what the domain of quantification is) to know whether the sentence is true or false. Therefore, this kind of generality expresses *a posteriori* truths.

Lavine [2006] gives the most detailed defence of full schemas. The idea is the same: the validity of a schema does not depend on a domain of objects, but as the domain or the language expands, so too the substitution instances of their symbols expand. This is mirrored in a difference in the *inferential role* between full schemas and quantification. A universally quantified sentence is true when all elements of the domain satisfy the formula; this is not the case with a schematic generalization: even if the substitution of every element of the domain in the schema only gives rise to true instances, this is *not enough* to declare the schema valid, because it may happen that in a expansion of the domain, we will uncover some counterexample to it. Another example of a difference in the inferential role can be seen by considering the derivation of $S0 \neq 0$ (with S indicating the successor function on natural numbers) from $Sn \neq 0$, where n is a schematic letter. This inference can always be done; however, if n were a quantificational variable, the inference would be valid just in case n does not occur free in one of the premises of the argument (Lavine [2006], p. 120).

A commitment to a *standard* schema such as $\alpha = \alpha$ is a specific commitment to a general sentence: 'all instances of the schema $\alpha = \alpha$ are truths'. *Full* schemas are different: thanks to their open-endedness, they cannot be reduced to a quantificational generality. In relation to full schemas, we can only say that *each* single instance of the schema is true, but not that all of its instances are true. Borrowing a well-known expression of Wittgenstein, we can *show* that, given an arbitrary instance for the meta-variables of the schema, the result of substituting the meta-variables with it gives rise to a true statement, but we cannot *say* that this is always the case. With an example: from the quantified sentence (whose intended domain is the set of natural numbers) $\forall x(\phi(x) \to \phi(Sx))$ together with $\phi(0)$, we can infer $\forall x\phi(x)$; but from the schema $\phi(n) \to \phi(Sn)$ and $\phi(0)$ we cannot infer the same general statement, because the schema does not make an assertion about all numbers, but merely provides a mechanism through which to make assertions about particular numbers (Lavine [2006], p. 121). As Fine [2006] underlines, the (full) schematic approach to absolute generality tries to split a general commitment to particular sentences (the instances), from a particular commitment to a general claim (the quantified sentence that should correspond to the schema).

3.2 Full schemas for indefinitely extensible sequences

Why is it not possible to derive the quantified sentence $\forall x(\phi(x) \to \phi(Sx))$ from the schema $\phi(n) \to \phi(Sn)$? In other words, from the schema $\phi(n) \to \phi(Sn)$ why can we not derive the correspondent matrix (the formula with free variables) $\phi(x) \to \phi(Sx)$ and then, by universal generalization, the quantified formula $\forall x(\phi(x) \to \phi(Sx))$? This latter universally quantified sentence would be the general truth we are committed to, when we commit ourselves to the schema $\phi(n) \to \phi(Sn)$. Lavine's answer focuses on the fact that, according to him, one must accept that we are working within an actual (infinite) domain in order for this reasoning to be valid. The quantified sentence requires the specification of a domain of objects, and if there are infinitely many objects, then the domain must be an actual infinite. However, if we are working with a potential infinite (with an indefinitely extensible sequence)[88], we cannot translate the schema into a formula with free variables, precisely because the translation would bring us from an open-ended schema to a formula that requires a fixed domain of objects to be interpreted. The translation would just delete the central feature of full schemas: their open-endedness.

Thanks to their open-ended nature, full schemas are useful for expressing generality over a potential infinite sequence, e.g. an indefinitely extensible sequence. If a quantified sentence needs a domain to be interpreted, indefinite extensibility shows that it is always possible to find elements that do not belong to that domain. The domain can always be extended. Their open-endedness, which allows them not to be bound by some particular domain, makes schemas suitable for generalizing over an indefinitely extensible sequence.

One may suppose that if unrestricted quantification is possible and, consequently, a universal domain, which contains every entity, is available, there cannot be any real difference between quantificational and schematic generality. Lavine argues that things are different: if it is possible that something exists but it does not actually exist, then full schemas would express a commitment that we would have if it existed, while simple quantification cannot express this commitment. Being bound to a certain domain of object, a universal quantified sentence cannot express the commitment to objects that could have belonged to its domain, but do not in fact belong to it. To express this commitment in quantificational terms, it is possible to adopt a modal framework – for

[88] Roughly speaking, a concept is indefinitely extensible if, for every definite totality of objects falling under it, it is always possible to find a more inclusive definite totality of such objects. It is clear that, to an indefinitely extensible concept, there corresponds an indefinitely extensible sequence of its extensions. This sequence can always be increased because, given an arbitrary extension of the concept, it is always possible to find a more inclusive extension of the same concept. In this sense, the sequence constitutes a potential infinite. Therefore, in what follows we shall use the expression 'indefinite extensibility' and 'potential infinite' as synonyms.

instance, by claiming that the sentence is necessarily true – but this shows that the full schemas are stronger than quantification generality[89].

3.3 'Any' and 'All'

Another typical way of arguing for a generality that is different from the quantificational one makes appeal to the distinction between 'all' and 'any'. Here is Russell [1908], §2:

> Given a statement containing a variable *x,* say '*x=x*', we may affirm that this holds in all instances, or we may affirm any one of the instances without deciding as to which instance we are affirming. The distinction is roughly the same as that between the general and particular enunciation in Euclid. The general tells us something about (say) all triangles, while the particular takes one triangle, and asserts the same thing of this one. But the triangle taken is *any* triangle, not some one special triangle; and thus although, throughout the proof, only one triangle is dealt with, yet the proof retains its generality.

Russell is here appealing to a typical way of proving general statements concerning objects of a particular kind: one has to consider an arbitrary object o and prove that Po is true. Since the object is arbitrary, the proof does not rely on any particular feature of the object, and therefore we are allowed to generalize the result and conclude that the general statement is true. In proving the result, we do not consider all the objects of a certain kind, but only one arbitrary object. The idea is that the determiner 'any' exactly captures this 'arbitrary' generality. An immediate objection is that once it is proved that Po is true, we can conclude that $\forall x Px$ is true. In this way, 'any' would not express a different form of generality. But the objection misses the point. Even if the latter conclusion is legitimate, it does not remove the difference between a proof that proceeds by considering a particular but arbitrary case, and a proof that considers all the cases.

A more recent defense of the difference between 'all' and 'any' can be found in Dieveny [2013, 2014]. However, we are not going to develop this point further, because it requires a deep linguistic analysis which exceeds the aims of this paper. Moreover, even in the case in which this analysis would in fact confirm that any-generality works differently from all-generality, this would not still be enough to show that schematic

[89] A different way of arguing for the irreducibility of schematic to quantificational generality can be found in Hellman [2006], who proposes to consider the generality of a schema such as $\alpha = \alpha$ in a rather Carnapian way. According to such proposal, these kinds of sentences are analytical and *a priori*, because they just constitute *stipulations* regarding how words like 'object', 'thing', 'entity' are to be used in our language. According to Hellman, when we say $\alpha = \alpha$ we are actually saying "anything that we ever recognize as an entity at all will be *assumed* to obey this" (Hellman [2006], p. 95, emphasis added). So we are dealing here with a stipulation, which tells us how to use the words in the language. On the contrary, the quantificational generality has nothing to do with stipulation (by saying 'all swans are white', we are not stipulating that the swans are white!), so the two kinds of generality must be different. I do not find this argumentative strategy particularly persuasive. How can a logical truth like $\alpha = \alpha$ have a stipulative meaning? When some people stipulate something, as in a contract, they have simply arrived at an agreement, a common decision. Implicit in this is that there was the possibility of a different decision being taken (or of no decision being taken, as when no contract is signed). But, in the case at hand, there are no possibilities for arranging things in a different way: we cannot coherently say 'let's stipulate that $\alpha = \alpha$ is not valid'. For this reason, speaking of a stipulation concerning logical laws such as $\alpha = \alpha$ can only have a metaphorical meaning and, consequently, it is better to avoid such use.

generality is irreducible to quantificational generality, the reason being that any-generality could be interpreted differently from schemas, as we suggest in §5. For this reason we prefer to put the problem of the difference between 'any' and 'all' to one side and concentrate on the difference we saw before regarding the open-endedness of full schemas. Therefore, in what follows we consider the authentic difference between universal quantification and schematic generality to rest upon the open-ended nature of the latter.

3.4 The semantics of schemas

Since schemas are not truth-bearers, when dealing with the semantics we are not directly dealing with truth, but rather with validity (which implies the truth of each instances of the schema). The problem consists exactly in expressing the validity of a schema. It is tempting to say that a schema is valid when all its instances are true, but we already know that this cannot be the case with full schemas. What we should do is express it by means of a meta-linguistic schema:

> One can, of course, formally specify the semantics of full schemes in a suitable metalanguage, but that isn't terribly helpful, since the metalanguage will also employ full schemes. Since the usual semantics for the quantifiers makes use of quantifiers in the metalanguage, I do not view the – fully parallel – situation for full schemes as in any way problematic. (Lavine [2006], p. 119).

Lavine is certainly right in claiming that the usual semantics for quantifiers employs the same quantifiers in the meta-language; in particular, meta-linguistic quantifiers are used to give the semantics of object language quantifiers, because quantifiers are considered primitive in a FO-language (at least one quantifier must be taken as primitive). The idea is that we grasp the truth-condition of the object language quantified sentence by means of the meta-language quantified sentence. Of course, this presupposes that we already have a previous grasp of the meaning of the quantifier in question[90]: by means of this knowledge, the standard semantics manages to convey the meaning of an object language sentence. The case of schemas is fully parallel to the quantificational case. We should grasp how a schema works by means of a meta-linguistic schema, and this is possible because schemas are meaningful and we certainly grasp what they express[91].

3.5 Some general remarks on full schemas

In this section, we have seen a number of differences between full schemas and quantification. We have also seen that the differences in the inferential role depend on

[90] For our argument, one need not take a position on the debate concerning the meaning of quantifiers: perhaps the meaning of quantifiers is given by their inferential role in natural deduction, or maybe we can only grasp the inferential role because we already grasp what generality is; in any case, for our purposes what matters is that standard semantics works by presupposing a grasp of the meaning of quantifiers.

[91] Notice that this would not be possible with the first, more radical, interpretation of schemas: according to that interpretation, schemas are meaningless strings of symbols, and consequently there would be nothing to grasp.

the fact that full schemas are open-ended, whereas quantification requires the specification of a fixed domain of objects to act as the universe of discourse.

The general picture that emerges from the considerations above is that the open-endedness of full schemas marks a true difference from quantificational generality. Since quantification requires the specification of the domain of discourse, it cannot be open-ended[92]. That is the reason why advocates of full schemas have proposed using them for generality over a potential infinite (an indefinite extensible sequence). *We could say that schemas are open-ended because they express something that does not depend on the objects in any domain*.

Their open-endedness means that full schemas are not reducible to quantificational generality. However, will this suffice to avoid the fact that a general commitment to particular truths implies a particular commitment to a general truth? That is the major question now posed. The need to avoid a particular commitment to a general truth stems from the fact that schemas are not truth-bearers: if it were not possible to avoid such commitment, this general truth would suggest the presence of a 'new' form of generality on which a full schema would depend. In §5 we will suggest that this is in fact the case. But beforehand, in the next paragraph, we will argue that the open-endedness of schemas does not depend on their lack of truth-values, and as such, open-endedness is not sufficient to avoid a commitment to a general truth.

4. An objection to the schematic approach

The objection concerns the potential infinite. Consider sentences such as 'Some sets are not members of themselves' or 'Each ordinal has a successor', and suppose that both the concepts of set and ordinal number are indefinitely extensible. Interpreted schematically, these sentences have a meaning, but not a truth-value. Above we underlined that the authentic difference between schematic and quantification generality rests upon the open-endedness of the former. Since the latter requires the specification of a domain of individuals, it is bound to the specified domain. So we need schemas, which are open-endedness, to express such general sentences. We already know Lavine's argument (paragraph 3.2 above) that the derivation of a quantified sentence such as '$\forall x\big(\phi(x) \rightarrow \phi(Sx)\big)$' from a schema '$\phi(n) \rightarrow \phi(Sn)$' presupposes that we are working within an actual (infinite) domain. Lavine is arguing that if we work within a fixed universe, then a general commitment to particular truths implies a particular commitment to a general truth, because within a fixed domain, schematic generality would collapse into quantificational generality. In fact, as soon as we have

[92] Of course, this must not be interpreted as if we were claiming that the inference rules for quantifiers were not open-ended. As a matter of fact, the inference rules for quantifiers (∀-introduction and ∀-elimination in Gentzen's natural deduction) do not determine the range of the quantifier (its extension). In first-order logic, this is clearly shown by the Löwenheim-Skolem theorem, which implies the existence of different models with different infinite sizes that make true exactly the same sentences of the languages. See Einheuser [2010], p. 240. Even though the inference rules for quantifiers are open-ended (they are domain-independent), quantification generality is not open-ended because its semantics is not open-ended.

fixed the domain, i.e. we do not consider the possibility of enlarging (or modifying) it, the open-endedness that characterizes schematic generality can no longer play any role. When we deal with a potential infinite, the open-endedness allows schemas to generalize over each extension of the sequence. Lavine's position thus suggests that open-endedness ought to be charged with the impossibility of shifting from general commitments to particular truths to a particular commitment to a general truth.

Lavine's reasoning can be summed up as follows:

1) A general truth always presupposes a fixed domain of quantification;
2) Sentences as 'Some sets are not members of themselves' or 'Each ordinal has a successor' do not have a fixed domain of quantification;
3) Therefore, such sentences do not express general truths.

If Lavine is right in thinking that a general truth always requires the specification of a domain of objects, then generality about a potential infinite is only expressible by means of schemas. Notice that this amounts to taking the standard semantics for the quantifiers very seriously: when a fixed domain of objects for the quantifiers is not available, then there can be no general sentence with a determined truth-value. But no argument is given to support the idea that a general truth always requires the specification of a domain of objects. In other words, sentence 1, which is the key-premise of the whole argument, is just assumed to be valid, not proved. Moreover, this approach towards quantification is dubious: one could in fact argue by *modus tollens* that since *there are* general truths about potential infinite, the standard account of quantification is simply wrong. For instance, Van Inwagen [2009.] p. 498 took this view:

> There are, I concede, philosophers who maintain that when one says 'Some sets are not members of themselves' or 'For every ordinal number there is a greater', what one says is meaningless unless in uttering these sentences one presupposes a domain of quantification—a particular set of sets, a particular set of ordinals. These philosophers are in the grip of a theory. They ought to reason by *Modus tollens*; they ought to reason that because it is true without qualification that there are sets that are not members of themselves and that for every ordinal there is a greater, that their theory about quantification is false.

Using Van Inwagen's expression, we may say that the defender of schematism is "in the grip of a theory": he considers the standard theory of quantification correct, and consequently it infers that some generalizations are illicit, while a supporter of Van Inwagen's line would take for granted that there are such generalizations and would conclude that the theory is wrong. I suspect that many people will agree with Van Inwagen's position, which seems more natural and more respectful of our linguistic practice. In any case, for what we have said both positions are legitimate.

However, some progress can be made by noticing that the presupposition for which a general truth always requires the specification of a domain of objects seems not only dubious, but simply mistaken. In fact there are cases of general truths that do not depend on the specification of a domain of quantification. These truths are domain-independent as schemas are, and consequently, they can be open-ended as schemas. I shall now provide two different examples of these truths. Their existence reveals that Lavine is wrong in thinking that the open-endedness is to be charged with the impossibility of going from a general commitment to particular truths to a commitment to a general truth. His mistake consists in taking for granted a feature of standard quantification theory. The existence of such examples is interesting because it shows that it is in principle possible to have a true generalization over a potential infinite, as we shall explain in §5.

The first example consists of what Kant called analytic judgments, sentences such as 'all bachelors are unmarried' or 'all bodies are extended". Such sentences are *not sensitive* to their single instances, because they express what we may call *conceptual truths*: they express propositions which are true just in virtue of the meanings of the words involved. We do not need to check every single bachelor to see whether or not he is married; we just need to know the definition of the word 'bachelor'. It is in virtue of the concept of bachelor that the previous sentence is true. Therefore, the truth-value of such generalizations does not depend on having previously specified a domain or a pool of candidates (a plurality of objects) as values for the quantified variable. No matter which pool of candidates we may consider, the truth-values of such sentences always remain the same.

That such sentences are not schemas but authentic general truths is shown by the fact that in uttering a sentence like 'all bachelors are unmarried', we utter something whose truth merely depends on how we have defined the concept of bachelor and being unmarried. In other words, we are in a position to exhibit the reason why such a sentence is to be regarded as true. It should be clear that we are committing ourselves to a general truth, and not to particular truths.

Other example sentences that do not depend on a previously specified domain are certain necessary *a posteriori* sentences such as 'all whales are mammals'. These are not *a priori* sentences, because it was an empirical discovery that whales are mammals, not fish: without looking at the world and at the animals that had been baptized[93] as whales, it would not have been possible to know if the sentence 'all whales are mammals' is true or false. But once the discovery was made, then the truth of the sentence is clearly not dependent on the domain of whales we may consider. If we discovered the existence of animals very similar to whales but which are not mammals, the normal reaction would be to say that those animals are not whales, and not that some whales are not mammals.

These two kinds of generalization are clearly domain-independent. This is not surprising, since both kinds of sentences concern necessary truths. Their domain-independence shows that we can have general truths about a potential infinite[94].

[93] The word 'baptism' is here used in the technical sense of Kripke-Putnam theory of reference.

[94] Domain-independence is a broader phenomenon than open-endedness; in particular, open-endedness implies domain-independence but not vice versa. For instance, the truth "all bachelors are not married" is

Suppose that the concept of ordinal number is indefinitely extensible and, consequently, that the ordinals form a potential infinite. The sentence 'every ordinal has an immediate successor' presents the same structure of the sentence 'all bachelor are not married'. It is true in virtue of the way in which ordinals are usually defined in set theory that each ordinal has an immediate successor. It is an *a priori* truth that does not require us to check each ordinal case by case. The sentence is therefore domain-independent: it is true in any domain of any model of set theory. Here we clearly have a general truth concerning a potential infinite.

It is clear that the schematist cannot try to reply to these two examples by arguing that what here we take to be general truths are in reality indefinite claims that do not express any proposition. The reason why this reply is not available is that the truth of such sentences does not depend on the specified objects of quantification, rather they depend on the definitions of the concepts involved (in the first case), and in the way reference works (in the second case). In both case we know *why* they are true. If they were schemas, we would find ourselves in the awkward situation of having to explain not only why they seem to express true propositions, but we should also explain why it seems that we know why they are true. There is no doubt that whilst dealing with such sentences, we are dealing with authentic propositions with a determined truth-value.

The existence of such generalizations casts doubt on the schematic approach for at least three reasons. First of all, their existence shows that the presupposition that a general truth requires the specification of a domain of objects is simply wrong: there are general true propositions which do not depend on any specific domain of quantification. Secondly, their existence shows that domain-independence and, in particular, open-endedness is not to be charged of the impossibility of shifting from a general commitment to particular truths to a particular commitment to a general truth. This means that open-endedness is compatible with the sentence expressing a true proposition. But as our exposition of the schematic approach has shown, open-endedness was the key feature to argue for the split between a general commitment to particular truths and a particular commitment to a general truth. Therefore, the schematist has lost the main argument that she had in support of his view. Thirdly, the similarity of generalizations like 'each ordinal has an immediate successor' with 'all bachelors are not married' strongly suggests that the former expresses a truth proposition as the latter.

These three points do not suggest that schematism is in itself incoherent, but they show that we have no reason at all to think that such generalizations are authentic schemas, i.e. indefinite claims. Since the most natural way of interpreting such generalizations considers them claims that express authentic propositions, it is in this way that we should interpret them. Of course, the open problem consists in

domain-independent in the sense that it does not depend on a previously fixed domain of bachelors, but it is hardly open-ended, because there is no problem in supposing that there is a finite set of all bachelors.

understanding how to interpret the logical form of such general sentences; since their open-endedness, they cannot be standard quantified sentences.

5. Conclusion

In the last paragraph we suggested that we should not interpret generalizations as 'Some sets are not members of themselves', or 'Each ordinal has a successor' as schemas, for mainly two reasons: first of all, the schematic argument is flaw, since it presupposes that a general truth requires a fixed domain of quantification, which we saw to be false; secondly, we argued that the similarity between 'all bachelor are unmarried' and 'each ordinal has a successor' strongly suggests that the latter sentence expresses a proposition with a determined truth-value as the former.

Once granted both of these points, there is no obstacle to extend this position to the case of logical laws as '$x = x$'. This extension amounts to the claim that logical laws are general, open-ended (domain-independent) truths.

However, with the friends of the schematic approach to absolute generality, we have argued that the open-endedness of schemas marks an authentic difference from quantificational generality. To deal with indefinite extensibility, quantificational generality is not adequate, because we need the open-endedness of full schemas. As such, it seems that we cannot express the validity of a full schema by means of quantificational generality: if this were possible, then it should be possible to quantify over all extensions of the languages, but if we are working with an indefinitely extensible sequence, there is nothing like 'all extensions'. So it seems that we have reached a stand-off: neither schematic nor quantificational generality are suitable to generalize over indefinitely extensible sequences.

This stand-off can be overcome by a form of generality that is both open-ended and that expresses a determined proposition, i.e. it is a truth-bearer. Above we gave two different examples of such generalizations. However, the problem consists in understanding how we can logically express these generalizations. At this point it should be clear that both standard quantification and the schematic approach do not work. The former does not express the domain-independence of those truths, the latter does not express a determined proposition, and therefore it ran into the troubles underlined above. In the literature there are (at least) two proposals that can fit our needs. The first proposes to interpret the quantifier as in intuitionistic logic. This is in fact the proposal advanced by Dummett [1991] in connection with indefinitely extensible concepts. For him, quantification over a potential infinite must behave intuitionistically. A different proposal consists in going modal, by using a primitive modal operator to combine with the quantifiers. This proposal has been put forward by Fine [2006] and further (and in a partially different way) developed by Linnebo (see Linnebo [2010]). What Linnebo calls a 'modalized quantifier' (combinations of modalities and quantifiers such as $\Box\forall$ or $\Diamond\exists$) represents an open-ended form of generality; however, as standard quantifiers (and differently from schemas), they can occur as part of meaningful statements.

Both such proposals share the advantage of making sense of open-ended generalizations without abandoning the idea that such a generalization lacks a determined truth-value. Moreover, both of them seems to be closer than the schematic approach with regard to our standard linguistic practise. In fact, in natural language, it is always possible to negate a sentence by prefixing to it the locution “it is not the case that...”. Both these proposals negate a sentence just by prefixing a negation to it. In this respect, they exactly mirror what happens in natural language. However, we already know that to express the negation of a general sentence by means of a schema, just prefixing the schema with a negation will not do, and that we need a more indirect method (see § 2.1). The reason is exactly that a schema does not express a unique proposition, but just an indefinite claim. This is a further suggestion that natural languages generalizations as ‘Each ordinal has a successor’ express unique propositions, and not indefinite claims, against what the schematic approach says.

CHAPTER 5

FROM FIRST-ORDER TO HIGHER-ORDER LOGIC

1. Quinean orthodoxy in logic: absolute generality in first-order logic

1.1 The model theoretic approach to semantics

For many years the orthodoxy in logic was detected by Quine's criticisms against the legitimacy of second order logic (SOL) and, more generally, higher order logics (HOL), as authentic logics. Quine raised at least two criticisms against SOL: first of all, since pure SOL allows to derive most of set theory and the latter leads to the antinomies, we should be very cautious in dealing with it; secondly, it seems that SOL commits us to the acceptance of classes or sets; but this is unacceptable because a logic should be applied to any domain of objects and this is possible only if it does not bring with itself any ontological commitment. In other words, Quine believed that SOL was nothing else than "set theory in sheep's clothing"[95].

The reason of this interpretation of higher-order logic was a certain conception of the semantic values of predicates. In *Words and Objects* (§19 and 20) Quine argues that predicates are plural terms, that is they refer plurally (i.e. they have a 'multiplicity of reference' or they have a 'divided reference'), which simply means that they refer to *the objects* they are true of[96]. Those objects make up the extension of the predicate. Thanks to the development of model theory, it has become standard to consider these extensions as sets: an extension is nothing more than a set of objects. However natural this view may seem, under his innocence a substantive thesis is hidden: not only do predicates have extensions, but their extensions constitute a semantic *relevant* feature of them (McGinn [2000], p. 53). What this means is that *the set of objects* that constitutes the extension of a predicate plays an essential role in the semantic contribution that the predicate brings to the meaning of the sentence in which it occurs. If a is an individual constant, P is a predicate, and $I(a) = d$ (d is the object of the domain of the language the name 'a' refers to, and I is an interpretational function, and $\in$ is the standard membership predicate), then this picture validates the following semantic clause for the sentence $P(a)$:

[95] Shapiro [1991]. There is a further reason to doubt of the logicality of SOL, which is its incompleteness. The completeness of FOL makes its syntax and its semantics on a par: they determine the same class of sentences. But the incompleteness of SOL implies that syntax and semantics do not coincide, which might lead somebody to doubt that we have a clear grasp of SO notions (for syntax is not enough anymore to grasp such notions). However, it is clear that the completeness of FOL derives from the limitation of its expressive power, while the incompleteness of SOL derives from its stronger resources. As a consequence, completeness might be looked at as an undesirable feature of a logical system, since it is due to its ideological weakness. As we are going to see in this chapter, absolute generality requires a quite strong expressive power, which sheds doubt on the possibility of accomplishing the task within the strict limits of FOL.

[96] ‹‹Semantically the distinction between singular and general terms is vaguely that a singular term names or purports to name just one object, though as complex or diffuse an object as you please, while the general term is true of each, severally, of any number of objects››. Quine [1960], pp. 90-91.

$P(a)$ is true if, and only if $d \in Ext(P)$

where $Ext(P)$, the extension of P, is the set of objects falling under P. Identifying the semantic values of predicates with sets has a straightforward consequence: when we generalize over predicates, i.e. when we quantify in predicate position, we are generalizing over sets. A second-order sentence as $\exists X\, X(a)$, which could be intuitively read as "there is a property that a has", should be explained in the following way:

$\exists X\, X(a)$ is true if, and only if there is (at least) a set x such that $a \in x$.

Therefore, from the thesis that the semantic values of predicates are sets of objects it immediately follows that generalizing over predicates is nothing more than generalizing over sets. Higher-order quantification ends up being just quantification over sets (or classes), which explain why HOL is just "set theory in sheep clothing". Moreover, since sets are (particular) objects, HO-quantification just collapses into FO-quantification. As it is made clear from the above clause, the higher-order quantifier $\exists X$ of the object language is translated with a FO-quantifier ('there is a set') in the meta-language.

Given this interpretation of HOL Quine draws the consequence that only FOL must be accounted as a legitimate logic, and therefore the only intelligible form of quantification is first-order quantification. Concerning semantics, this implies that the semantics of a (first-order) language must be given in a first-order meta-language. The natural way of doing this is by giving a tarskian model theoretic semantics (MT-semantics) based on set theory. Let's consider a first-order (toy) language L with the following syntax:

Countably many individual constants: $a, b, c, \ldots$
Countably many individual variables: $x, y, z, \ldots$
Countably many monadic[97] predicates: $P, Q, R, \ldots$
Logical connectives: $\sim, \rightarrow$ and the quantifier $\forall$.

We can specify the interpretation for such a language in a first-order meta-language with the same syntax of L plus the addition of a non-logical predicate '$\in$' for set membership in the following way: an MT-interpretation is a pair $< D, I >$, where D is a non empty set - the universe of discourse -, while I is an interpretation function such that $I(a) = d$, where $d \in D$; $I(P) = s$ where s is a first-order constant of the meta-language that refers to a subset of D. Let v be a variable assignment relative to a certain interpretation i such that $v_i(x) = d$, where $d \in D$. We can then recursively state the conditions according to which a formula is true under an interpretation i relative to a variable assignment v (for short: $true_{i,v}$):

$P(a)$ is $true_{i,v}$ iff $I(a) \in I(P)$
$\sim\phi$ is $true_{i,v}$ iff ϕ is not $true_{i,v}$
$\phi \rightarrow \psi$ is $true_{i,v}$ iff either ϕ is not $true_{i,v}$ or ψ is $true_{i,v}$

[97] I shall just deal with monadic predicates for matter of simplicity.

$\forall x\phi$ is $true_{i,v}$ iff for every d such that $d \in D$, ϕ is $true_{i,v[x/d]}$ (where $v[x/d]$ is the assignment that maps x to d whilst assigning the same values as the assignment v to the other variables).

MT-semantics is the semantics that fits best with Quine's idea concerning the semantic values of predicates. However, MT-semantics gets into troubles with the assumption that the first-order quantifiers range over absolutely everything. Let's call this assumption AG-∀:

(AG-∀) the (first-order) quantifiers range over everything.

The problem that MT-semantics faces if the language satisfies AG-∀ is that no model comprises everything as element of its own domain, because the domains of the models are sets and there is no universal set on pain of contradiction[98]. Therefore, if the object language manages to achieve absolute generality, MT-semantics is not suitable to capture the intended interpretation of a language with quantifiers ranging over everything.

1.2 Davidsonian style semantics

In the presence of AG-∀ a follower of Quine's approach may try to capture the intended interpretation by means of Davidson's quasi-homophonic semantics. The idea is that MT-semantics fails because it is based on set theory, which treats the same MT-interpretations as first-order objects (they are order pairs and, consequently, sets[99]). MT-semantics treats interpretations as objects. On the contrary Davison's semantic avoids reifying interpretations. A davidsonian semantics for L is as follows:

$P(a)$ is true iff $I(a)$ falls under the set of objects for which P is true
$\sim\phi$ is true iff ϕ is not true
$\phi \rightarrow \psi$ is true iff either ϕ is not true or ψ is true
$\forall x\phi$ is true iff everything d is such that ϕ is true$_{v[x/d]}$

It is worth noticing that a quasi-homophonic semantics - as the one just sketched here - is an *absolute* semantics, in the precise sense that the truth-predicate is not defined in relation to a certain model, as it happens with MT-semantics. In other words, truth is not defined as truth in an interpretation. In this semantics interpretations are not objects at all, which guarantees that there is no problem in validating AG-∀. Therefore, the quianean can claim to have a coherent picture in which a) just first-order quantification is taken to be intelligible; b) first-order quantifiers range over everything, and c) there is

[98] What about proper classes and non-well-founded set theory? To invoke proper classes will not do, because proper classes are objects that contain elements but they cannot be contained in any further collection. Therefore, no proper class contains itself. A model whose domain is based on a proper class will not contain its own domain, and so the proper class's solution fails to be totally absolutely general. Concerning the use of non-well-founded set theory see chapter 1, §4, where I dismiss the use of these theories in the absolute generality debate.
[99] Thanks to Kuratowsky, we can define an order pair $\langle a, b\rangle$ as $\{a, \{a, b\}\}$.

a straightforward way of giving a semantics for a first-order language in a first-order meta-language.

The problem with such a solution is that no quantification over interpretations is possible, because interpretations are not (first-order) objects. However, quantification over (all) interpretations is necessarily for many basic logical definitions, for example for the definition of logical consequence. Consider the following standard definition of (semantic) logical consequence in propositional logic (PL):

A wff ϕ is a PL-semantic consequence of a set of wffs Γ iff *for every PL-interpretation*, I, if $V_I(\gamma) = 1$ for each γ such that $\gamma \in \Gamma$, then $V_I(\phi) = 1$. (Sider 2010, p. 34; emphasis added).

How could a quinean who makes use of a davidsonian's style semantics reply? One possibility would be just to eat the bullet and declare such a kind of quantification impossible. But this does not seem a very promising path to take. If a logical system does not allow to define logical consequence – the central concept of logic! – then it should be considered inadequate. Moreover, one of the most appealing feature of a quasi-homophonic semantics is that it allows point c) above, that is that the quinean can give a semantics for a first-order language in a first-order meta-language. But the problem with the notion of logical consequence shows exactly that this apparent appealing feature is misleading, because it actually leads to the conclusion that it is not always possible to give a general semantics for a language (since there are semantic notions that cannot be defined). In other words, the first-orderist must give up what is usually called "Semantic Optimism" (Linnebo & Rayo 2012):

Sem-Opt: Given an arbitrary language L_1, it should be possible to articulate a generalized semantic theory for L_1 (based on L_1).

Of course, the quianian has the possibility of rejecting Sem-Opt; maybe it is just a brute fact that we cannot always give a general semantics for a language. There might be features of languages that are not investigable. However, Sem-Opt exercises a strong appeal. It is surely a desirable thing to be able to fully specify the semantics of a language. Moreover, it should be a principle of scientific enterprise not to consider anything to be beyond the limits of our understanding. What I mean by this is simply that, when facing a problem, we should try to find a solution, and we should not say that the problem is out of our reach. In this latter case, we would prevent ourselves from the possibility of finding out interesting, and maybe innovative, solutions. In any case, what happens if the first-orderist accepts Sem-Opt? Sem-Opt together with the fact that the only legitimate form of quantification is standard first-order quantification implies the All-in-One principle:

All-in-One Principle: quantifying over certain objects presupposes that these objects are collected in a set or a set-like object.

(Sem-Opt) implies that we can quantify over all interpretations (for example, in stating the definition of logical consequence). But since just first-order quantification is allowed, the first-order variable must range over an object (a set) that comprises all the interpretations. The truth of the All-in-One Principle brings the quinean back to MT-semantics: interpretations (models) are objects (ordered pairs), where the universe of the model must be a set. But now the quinean is back with the fact that no intended interpretation for the absolutely unrestricted quantification is possible in MT-semantics. If he accepts the truth of the All-in-One Principle he is forced to reject AG-∀, exactly because no universal set is available for an absolutely unrestricted quantification.

To sum up what we have seen so far, the quinean has two options on the table: he can choose between an MT-semantics and a davidsonian's style semantics. In the presence of AG-∀ an MT-semantics cannot express the intended interpretation of the quantifiers. If he chooses a davidsonian's style semantics, he can have AG-∀, but he cannot define basic notions as the one of logical consequence, which shows the inadequacy of such semantics for formal languages[100].

1.3 Adapting Kreisel's uniqueness argument

However, this last point can also be raised for MT-semantics. In fact, a model theoretic account of logical consequence requires quantification over all domains (of all models). But such quantification is not available, because no model is based on the universal set (since there is no universal set). Moreover, the davidsonian's style semantics seems not to be able to define the notion of logical consequence.

The standard reply to this objection would appeal to Kreisel's famous uniqueness argument (Kreisel [1967]). This strategy was used by Cartwright [1994] exactly to show that we do not need quantification over all domains to define the notion of logical consequence. Kreisel showed that we can give an *extensional* adequate characterization of logical consequence and truth just by quantifying over interpretations based on set-sized domains. Kreisel's argument is as follows[101] (we are always working within first-order logic):

- Suppose that ϕ is a logical consequence of a set of sentences Γ. Since every set-sized model of Γ is an interpretation of the language, ϕ is true in every set-sized model of Γ.
- Suppose that ϕ is true in every set-sized model of Γ. Since completeness of FOL, ϕ is derivable from Γ. Since the axiom of FOL are valid and its deductive rules preserves validity, ϕ is a logical consequence of Γ.

There has been a huge debate on the merits of such an argument. As McGee [1992] notices, if we add to the FO language a quantifier C such that *CxFx* is true just in case

[100] Davidson was interested in natural language, so the limitations stressed here do not imply that his kind of semantics is inadequate for his own aims.
[101] I took this reconstruction of Kreisel's argument from Rayo & Uzquiano [2006], p. 7.

there are more Fs than sets, the argument is immediately blocked. Another problem is that the argument works with sound and complete logics, so it cannot be generalized for HO languages. This last reason does not seem problematic for a quinean at all, since his rejection of HO logics. The former remark seems to be more problematic in the presence of AG-∀: if the quantifier of the object language ranges over everything, the introduction of a quantifier as Cx seems to be fully legitimate.

It is straightforward to adapt Kreisel's uniqueness argument to the case of absolute generality. The first-orderist will be happy to claim that a sentence as $\forall x(x = x)$ is an example of an absolute general claim. Now, $\forall x(x = x)$ is a theorem of FO predicate logic (just apply universal generalization to the tautology $x = x$). By soundness, $\forall x(x = x)$ is true in every (set-size) interpretation of the language. On the contrary, if $\forall x(x = x)$ is true in every interpretation of the language, then it is provable in FOL (thanks to completeness). At this point we only needs to notice that since every object has its own singleton, each object is in the domain of at least one model (interpretation) of the language (for an arbitrary object a, we can consider the model whose domain is $\{a\}$). This assures us that $\forall x(x = x)$ comes out true for every object whatsoever (no counterexample is possible), even if there is no interpretation based on the universal set.

This is certainly a clever response available to the quinian. There is no intended interpretation that makes true $\forall x(x = x)$; however, each object is contained in at least one set-size interpretations, and $\forall x(x = x)$ comes out true in each of these interpretations. The quantifier $\forall x$ in $\forall x(x = x)$ ranges over different sets of objects with regard to different interpretations, which means that the sentence does not express a unique proposition, rather it expresses different propositions with regard to different interpretations. For example, if I take the I_1 to be based on the domain $\{x: x\ is\ a\ natural\ naumber\}$ and I_2 to be based on the domain $\{x: x\ is\ a\ real\ number\}$, then the sentence $\forall x(x = x)$ with regard to I_1 will express the proposition that all natural numbers are identical with themselves; with regard to I_2 it will express the proposition that all real numbers are identical with themselves. Therefore, the sentence expresses different propositions with regard to different interpretations, but since there is no universal interpretation (an interpretation based on the universal set), the sentence does not express any proposition independently on any interpretation. In other words, $\forall x(x = x)$ turns out to be a valid schema. This reply thus commits the quinian to the use of schemas to express the absolute generality of logical laws. We have dealt with the schematic approach in chapter 4, where we raised some doubts about such an approach. In light of the discussion in chapter 4, I do not consider the schematic approach a promising approach for absolute generality. As a consequence, I do not consider this position very tenable.

All in all, the quinean position does not seem much stable. Either she must abandon absolute generality with all the problems we saw in the preceding chapters or she must choose between two options which present many restrictions on their expressive power. Maybe the moral we should draw form this situation is simply that FOL is a too weak system to be satisfied with. At this point the objection would be that we just considered two different styles of semantics, but we have not argued that in any case, with any possible semantics, the quinean will find herself in this poor situation. For everything we said in these pages, it is possible for the quinean to come up with a further semantics that allows her object language to have absolutely unrestricted quantifiers and allows a fully adequate semantics which can express generality over all interpretations. However, if both these elements are present we are going to face a contradiction, as §1.5 illustrates.

1.4 The Löwenheim-Skolem theorem and semantic indeterminacy

FO-logic is sound and complete, while SO-logic if sound cannot be complete. This has usually been seen as strong motivation to prefer FO-logic rather then SO-logic. However, it has for a long time been recognized that what appears to be a strength from a certain point of view, it may appear as a weakness from a different point of view. Working within standard model theory, a FO-theory T is sound and complete when the following holds: T is consistent if and only if T has a model (i.e. an interpretation that makes true all its axioms and all the sentences that can be derived by its axioms). From completeness, we can easily derive the Compactness theorem: every subset of T has a model if, and only if T has a model. The right-to-left direction is trivial. For the left-to-right, let us prove the contraposition: suppose that T does not have a model. By Completeness, T is inconsistent. So there must be a derivation from axioms of T and inferential rules of T that ends up with a contradiction. But each proof of a contradiction must be formed by a finite number of steps, which means that the contradiction can be derived from a finite subset of T. By completeness this subset has no model.

The compactness theorem can be exploited to show that a first order theory T has non-standard models. But it can also be exploited to prove the generalized version of the Löwenheim-Skolem's: if a theory T - formulated in a countable language - has an infinite model, then T has models of any infinite cardinality. This theorem is usually presented as the conjunction of two different theorems, the Upward Löwenheim-Skolem (ULS) and the Downward Löwenheim-Skolem (DLS).

ULS: if a theory T - based on a countable language - has an infinite model of cardinality α, then T has a model for any cardinality greater than α.

DLS: if a theory T - based on a countable language - has an infinite model, then T has an infinite *countable* model.

There can be situations in which a compact logic does not imply DLS. For example, consider the logic obtained by adding to FO-logic the axiom "there exist uncountable many objects". This logic is compact, but the generalized Löwenheim-Skolem does not hold, because DLS clearly does not hold[102].

The DLS was first exploited by Skolem [1922] to formulate what has been known as the Skolem's paradox: although set theory proves the existence of uncountable sets, there is a countable model of set theory. Skolem argued that the paradox can be easily explained by appealing to the fact that inside the countable model no bijective function can be found between a set and its Power-set; however, from a more comprehensive perspective (i.e. by working in a more comprehensive universe), it is easy to find out such a function. According to him, the paradox shows that the FO-axiomatization of set theory does not pin down the intuitive model of the set theoretic universe. Putnam [1980] gives a similar argument not only in connection with set theory, but with language in general. His aim is to show that reference is indeterminate. With small changes, that argument can be readapted in the present context so as to become an argument against the possibility of determinately quantifying over everything. The argument runs as follows: suppose you speak a countable[103] FO-language and you intend to quantify over everything. By completeness, there must be a model (an interpretation) for your language. The model is clearly infinite (e.g. since you intend to quantify over everything, you also intend to quantify – for instance - over all natural numbers). Let us call this intended model the 'absolute model'. But by the DLS there is a countable model – let us call it S - that makes true exactly the same sentences which are true in the absolute model. Since both are model based on your language, there is no way in looking for a sentence true in one model and false in the other. But the second model is not absolute, i.e. it does not contain everything. Therefore, the range of your quantifiers are intrinsically ambiguous: you may think of quantifying over everything, when in fact you may be quantifying just over a countable subset of everything.

The point is quite subtle: as Lavine [2006], p. 106 underlines, it is not merely that the argument shows that there is nothing in what we say that determines if we are quantifying over S or over everything; rather also the appeal to our intentions does not seem to be enough. If we claim that we do not merely *want* to quantify over S, but we intend to quantify over everything whatsoever, the argument shows that this same sentence comes out true also in the case in which we are quantifying over S. What the argument shows is that we can form and communicate such intentions even in the case in which we are quantifying over S[104].

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[102] Lindstrom (1969) showed that FO-logic is the strongest logic for which both compactness and the LS hold.

[103] This characteristic is necessary for the formulation of the argument; however, it is not a good idea to reply to the argument by considering a language with an uncountable vocabulary. Natural language's vocabulary is for sure countable, and in general each language people can speak is countable, which means that the argument can apply to each language people can speak.

[104] Lavine [2006] suggests that we can neither form nor communicate our intention of quantifying over everything, but this seems wrong: we can surely form the intention and communicate it to other people; the point is that this does not guarantee that our intentions obtain. The argument shows a sort of invisible restriction on the language which is compatible with everything we say, utter, think, etc.

The argument relies on a number of presuppositions that can be challenged. First of all, it can be applied only if the underlining language is first-order. Secondly, it presupposes that the semantics is the standard one: in particular, it presupposes that if *I* is an interpretation of the language, then it satisfies the following two constraints (Einheuser [2010], p. 238):

1. *I* satisfies all true sentences of the language[105];
2. *I* respects the meaning of the non-logical constants in so far as it is specified by the speakers of the language.

According to LS there is more than one interpretations that satisfy both 1 and 2, and thus it is undetermined which is the intended one.

Both these two presuppositions can be challenged; in particular Rayo and Williamson [2003] challenge the idea that English has a standard FO-semantics; however, I am not going to discuss their view since in the present setting we are interested in understanding if such an objection is problematic for first-orderism (which is usually combined with standard semantics). In any case, one could argue that that if one adopts a higher-order language the argument cannot take off the ground. However, Lavine [2006], p. 108 has suggested that this would be a too quick dismiss of the argument. He argues that going second-order blocks the objection only if we suppose that the logic is standard second-order logic, with the quantifiers ranging over all subsets of the first-order domain. This means that they are to be taken as fully unrestricted[106]. But the possibility of unrestricted quantification is exactly the point that the defender of the indeterminacy objection wants to challenge. For this reason, it cannot be assumed without begging the question.

If Lavine is right, the indeterminacy objection would be a problem not only for FO-approaches to absolute generality, but for all approaches (provided they meet the semantic conditions above). However, it is easy to see that Lavine's objection fails. If our higher-order language is totally unrestricted, then the indeterminacy objection cannot be formulated (there is no LS theorem for higher-order logic); if our higher-order language is not totally unrestricted, then the objection might apply. But in this latter case, we should already have a reason to think that our higher-order language is restricted. A defender of a higher-order approach who has no reason to think that its quantifiers are restricted will have no reason to think that the indeterminacy objection can apply to its language, and so the objection cannot have any appeal for her at all. Therefore, the objection already presupposes that we have a different argument for the restrictiveness of our quantifiers. In this case, the defender of absolute generality in a higher-order context should just focus on this argument and try to reply to it. If she manages to reply, then the indeterminacy objection does not apply; if she does not

[105] This constraint is what Lewis [1984] calls 'global descriptivism'.

[106] The reason why full SOL evades the Löwenheim-Skolem's theorem is that SO-quantifiers range over all subsets of a given domain, which are strictly more –in virtue of Cantor's theorem – of the objects in the domain.

manage, then her quantifiers will be restricted but not in virtue of the indeterminacy objection, rather in virtue of this other argument. Consequently, the indeterminacy objection has only an appeal in the case of FO-logic, contrary to what Lavine says.

In the literature there have been a number of responses to the indeterminacy argument. McGee [2000, 2006] develops two different responses; Rayo [2003] contains a further strategy; Einhauser [2010] suggests that the argument is less powerful than what appears at a first sight. We are going to expose each of these responses and argue that none of them succeed.

1.4.1 McGee's objections.

McGee's first interest is to understand if natural language can achieve absolute generality. Consequently, he suggests that we should look at the fact that language is constituted by "the rules […] and practice followed by its speakers" (McGee [2000]) to reply to the indeterminacy objection. The idea is that the objection relies on the fact that an adequate interpretation for a FO-language is one that just preserves the truth-values of sentences (i.e. an interpretation that satisfies conditions 1 and 2 listed above); however, this does not say anything concerning the practices and uses of natural language. This seems to be promising, because if the DLS shows that there is no difference in what one can say when speaking of S or of everything, it may be that looking at practices and usages of languages can give us something to appeal to find a difference between quantification over S and quantification over everything.

1.4.1.1 The objection from learnability

The first of McGee's objections deals with the possibility of learning S-quantification. The DLS shows that our quantifiers may be restricted to a S-domain; if this is actually the case, then S-quantification must be learnable (if we use it, we must have learnt to use it). But to learn S-quantification, we must be able to distinguish what objects are in S from non-S objects. Then the learner would be aware of a difference between S-quantification and quantification over everything: in the latter case, nothing is outside the range of the quantifier, which implies that as soon as she find something outside the domain of S, she will know that S does not comprise everything. But according to the DLS objection, there can be no difference between S-quantification and quantification over everything. So DLS implies that S-quantification is not learnable.

Reply: this objection is not very clear; in particular it seems at odds with some further considerations that McGee advances concerning our knowledge of the quantifiers. In particular, those considerations (rightly) allow to attack the premise according to which learning S-quantification requires to distinguish between S and non-S objects. However, for the time being we just notice that as soon as we find a sentence true in S but false in the all-inclusive domain (or vice versa), then the two domains cannot be both two interpretations of the language (conditions 1 would not be satisfied anymore). In other words, the DLS allows us to find a countable interpretation in which everything true in the absolute interpretation is also true in the S-interpretation. This means that *a priori* we know that there cannot be any sentence true in one and false in the other. If we have taken the right countable interpretation, then there cannot be any difference that can allow us to distinguish the two models. This shows two things: McGee's strategy of looking at the practice of a language is not enough to reply to the objection, because one can express those practices by means of true sentences; the strength of the indeterminacy objection: there is nothing you can say to distinguish the two interpretations.

1.4.1.2 The objection from uniqueness, naming and predication.

The second of McGee's objections is based on Belnap's uniqueness condition. Consider the universal quantifier. Its inferential role (in natural deduction) is governed by two rules of inference, the so-called introduction and elimination rules for ∀.

(Elim.- ∀) $\forall x\varphi(x) \vdash \varphi(t/x)$
(Intr.- ∀) $\varphi(t) \vdash \forall x\varphi(x)$, provided that the constant t does not appear free in φ or in some undischarged assumption of the proof of $\varphi(t)$.

Belnap's uniqueness conditions state that there are no two different logical operators that satisfy the two rules and fail to have the same inferential role. In other words, if two first-order quantifiers $\forall_1$and $\forall_2$ satisfy (Elim.- ∀) and (Intr.- ∀), then given two formulas $\varphi_1$and φ_2, whose only difference is that in $\varphi_1$there is the quantifier $\forall_1$, while in φ_2 there is the quantifier $\forall_2$, the two formulas are interderivable within classical first-order logic, which means that they are logically equivalent (this result is known as Harris' theorem). The role of the uniqueness condition in our present setting consists in showing that when a speaker learns how to use a quantifier, she learns a unique logical operation. Moreover, the inference rules are open-endedness: no matter what domain we take our quantifiers to range over, their inferential role is captured by the two rules above. In other words, understanding the inference rules does not require the specification of any domain of quantification.

At this point McGee argues that this unique logical operation is quantification over everything. The argument proceeds by showing that when we consider the whole apparatus of naming and predication in languages, then quantification over less than everything fails to satisfy the two inference rules above. For *reductio*, suppose that quantification in English is always over a countable subset S of everything. Consider a predicate P, whose extension lies within S. Suppose that $\forall xP(x)$ has been derived in a correct system, and therefore it is true. Let's now expand the language with a name c that refers to an object not contained in S. By (Elim.- ∀), from $\forall xP(x)$ we can infer $P(c)$ (this is due to the open-endedness of the quantifiers), which is false, given the assumption that the extension of P lies in S. If quantification is restricted, then we have a counterexample to the validity of (Elim.- ∀), but since (Elim.- ∀) is one of the rules that we learn when we learn quantification, this assures us that the quantifiers range over everything.

Reply: I follow Einhauser [2010] in distinguishing the *logical meaning* of quantifiers from their *extensions*. The former is the meaning the quantifiers have in virtue of their inferential role, and therefore it is fixed by the Introduction and Elimination rules of natural deduction. The latter is given by the objects over which they range. What Harris' theorem shows is that quantifiers have a unique logical meaning. McGee's argument exactly tries to exploit the uniqueness of the logical meaning to argue that the extension cannot be restricted to a subset S. But he clearly fails. In fact, what the DLS shows, is that the logical meanings of the quantifiers do not determine their extensions. The fact that a theory with an infinite model has different models with different infinite sizes shows that the extensions of the quantifiers are totally independent from their logical meaning. Two different models with different sizes present two different extensions with the same inferential relations between the sentences.

This point also gives reason to our previous claim about the oddity of the argument from learnability. To learn quantification is to learn the inference rules that governs it. However, these rules are independent from the extension of the quantifiers (they are open-ended), which means that it is not possible to learn S-quantification in the terms presupposed in the learnability objection. There McGee suggested that to learn S-quantification one must be aware that the subset S is not all-inclusive. But now, the open-endedness of the inferential rules suggests that to learn how to use a quantifier one must only learn its logical meaning. Extensions do not play any role in this process.

What we have just seen is the general reason why we cannot appeal to the logical meaning of quantifiers to argue something concerning their extension. But what about McGee's argument in particular? Einheuser [2010] gives an interesting analysis of the dialectical situation that emerges, which I have summarized in footnote[107]; however, we

[107] The dialectical situation is as follows: the defender of the indeterminacy objection could argue (in reply to this argument) that we should consider the limit language English* which is actual English with all the names the people will actually introduce (which is surely a countable set of names). The intended domain of English* is the domain of everything. But now we can use the DLS on the theory based on this language to show that there is a countable sub-domain S* that preserves points 1 and 2 above (S contains every object that speakers of English will ever name). What this shows is that the inference rules will never be violated, despite the fact that the quantifiers range over less than everything (Einheuser, [2010] p. 241). The natural counter-objection would be that the inference rules are meant to be validity-preserving not only in all actual extensions of English, but in all mathematical possible extensions of English. In this case, even though only the objects in English* will be named, every object in all extensions *could* be named: in the possible extensions that will never become actual, the objects could be named, but will never be named, which shows that in those extensions the rules will fail to be validity preserving. So the inference rules must hold throughout all possible extensions of English. But the skeptic could appeal again to the 'Skolemite' objection to argue that if the sentence "the inference rules must hold throughout all possible extension of English" is true, then there must be for the DLS a countable model in which it is true. At this point McGee could reply that this is not enough, because the constraints must be preserve in all the extensions. This seems to bring ourselves in a dialectical tangle: ‹‹if G [the sentence that says that the inference rules must be preserved in all mathematical possible extensions of English] was another constraint, alongside C1 and C2, on adequate interpretations of English – in which case global descriptivism would be false – then the DLST could not generate 'small' nonstandard interpretations. But if a speaker of English believed his language to be in the grip of the DLST, then being told, in *English*, that adequate interpretations conform to G will not alleviate the skepticism›› (Einheuser, [2010], p. 242).

shall focus on a different point which seems us to constitute a major problem with McGee's objection. His idea was that, once we add a new name for a new object, the open-endedness of the quantifiers' inferential rules will give us a false sentence $P(c)$, because P's extensions lies entirely within S. The problem is that we get $P(c)$ from $\forall xP(x)$, by $\forall$-Elimination. The fact that $P(c)$ turns out to be false depends on the fact that $\forall xP(x)$ is false in the new domain (because of the extension of P). But by hypothesis $\forall xP(x)$ has been derived by a correct system without any particular assumption on any object. What McGee suggests is that the open-endedness of quantifiers implies quantification over everything, otherwise we would face such a contradiction. But this is not the only possible path to take. In fact we could argue that the new object c either falls or does not fall under P: if it falls under P, then both $P(c)$ and $\forall xP(x)$ obtain; if it does not fall, then they are both false and we have the problem. Therefore, one can argue that, if $\forall xP(x)$ has been correctly derived by a correct logical system without any particular assumption on any object, then c falls under P. This means that also concepts as P must be taken as open-ended. In this case, we should deny that the extension of P lies entirely within S. In other words, McGee's *reductio* can be used to argue that concepts too are open-ended, and so one can deny the premise of the argument according to which the extension of P lies within S. In fact McGee's argument supposed that, once enlarged the domain, the extension of the concept P would not change.

Given the open-endedness of the rules of inference (for quantifiers), then also the concepts must be taken as open-ended. The defender of the indeterminacy argument could make appeal to the need of considering concepts to be open-ended to reply to McGee's objection. This shows that that objection is far away from showing that the open-endedness of the inference rules implies that quantifiers range over an all-inclusive domain.

1.4.2 Other objections against the Putnam's style argument

Other objections have been raised against this style of argument. Rayo [2003] tries to exploit Grice's conversational maxims to argue that, even if there is nothing our interlocutor could say that forces her quantifiers to range over everything, Grice's maxim of informativeness supports the view that if the interlocutor says to be quantifying over everything, we should believe her and take her to quantify over everything. However, this appeal to pragmatics and to the speaker's intentions are not enough: the intentions can be written down or uttered by means of a true sentence, and by the DLS there is a countable model that preserves the truth of that sentence.

Einheuser [2010] proposes a different reply to the argument. In a nutshell, her strategy is to notice that the inderterminacy argument leads to a contradiction: the conclusion of the argument that *there is* something beyond the range of our quantifiers is a sort of pragmatic contradiction. This is essentially the inexpressibility objection that we have already discussed; we sent the reader to that discussion for more details on the general aspects of the objection. Our conclusion there was that is a strong objection against relativism, but it is not a definitive objection. However, there are some peculiarities in the present context which are worth being discussed. Suppose that we are trying to convince Bill that the range of his quantifiers is indeterminate by means of the skeptic argument. In order the argument to apply, Bill's language must be a countable FO-language, with a complete set of deduction rules and all the machinery requires to prove the DLS. Let's follow Einheuer in calling such a deductive FO-theory T. If we convince Bill by means of the indeterminacy argument, he should be able to derive from T the following sentence:

D) There is an interpretation *I* of my language such that on that interpretation my quantifiers range over a countable collection and for all sentences φ of my language: φ is true according to *I* if and only if φ is true.

The problem is that, since T is a classical FO-theory, it does not contain his truth-predicate. So *D* cannot be derived from T, but only from T', which is obtained from T and the addition of the truth predicate for the sentences of T. This means that Bill cannot appreciate that his quantifiers are indeterminate, because the derivation of *D* for T is made in T'. In his new language, he can see that the quantifiers of its old language were restricted.

This is a situation that we already known from the chapter on the inexpressibility objection (see chapter 3, § 3.3). So our conclusion there applies here too: the fact that we cannot express the relativist position in a coherent way does not imply that the position is false.

At this point Einheuser suggests that to appreciate the indeterminacy argument it is enough to be able to derive *D-* from T:

D-) If there is an interpretation *I* of my language which makes all my beliefs (T) true, then there is an interpretation *I'* with a countable domain which makes all of my beliefs true.

This is weaker than *D* and it is derivable from T. The problem now rests upon the fact that since *D-* is provable from T, Bill can see that the countable model are non-standard (they omit the mappings that witness the countability of their domains), so he surely can appreciate that there are interpretations of his language which are not all-inclusive, but he is also able to recognize them as not intended, and therefore to reject them as inadequate: ‹‹Bill would be able to tell that such interpretations, too, were unintended, precisely because he can tell that they are countable. The very fact he needs to grasp to appreciate the skeptic's argument puts him in a position to distinguish the skeptic's proposed small-domain interpretation from the intended interpretation›› (Einheuser, [2010], p. 245).

Does this reply work against the indeterminacy argument (the skeptic's argument, in Einheuser's terminology)? The problem is that *D-* has been proved from T, which means that it is (by hypothesis) a true sentence. By the DLS there is at least a countable interpretation such that *D-* is true. So, how can Bill exclude that the interpretation of his language is countable? Einheuser's argument shows that if Bill can see that a model is countable, he recognizes it as non-standard and, therefore, inadequate; but what guarantees that he is able to see that the model of his own language is countable? The indeterminacy argument does not show that Bill's quantifiers are restricted; rather it shows that their range is undetermined, i.e. they *may be* restricted. So Bill does not know if his quantifiers are restricted or unrestricted. In each case, *D-* turns out to be true, which means that if he recognizes a model as countable, he is able to see that it is inadequate. So, Bill is able to see that, if the model of his language is countable, it is inadequate; if it is uncountable, it is adequate. But Bill is not in the position of seeing that – as a matter of fact – the quantifiers of his languages are S-restricted or unrestricted. The anti-skeptic may claim that, at this point, Bill will know that his model is uncountable, because the hypothesis that it is countable is inadequate; but, at the same time, the skeptic may argue that the domain may be countable, because *D-* turns also out to be true at a countable model. It seems that we have reached a stand-off from which no immediate resolution is recognizable. We therefore conclude that also this reply does not succeed in demolishing the indeterminacy argument.

1.4.3. Conclusion on the indeterminacy argument

The argument from the DLS is a strong argument for the indeterminacy of quantification. Its strength rests upon the fact that the DLS guarantees us the existence of a countable model where all true sentences are preserved. In other words, there is nothing we can say to distinguish this model from a model with an all-inclusive domain. McGee's and Rayo's reply are based on the idea that only pragmatics can help us distinguishes S-quantification from quantification over everything. But the problem is that any appeal to pragmatics or speaker's intention is going to fail for the simple reason that any intention can be expressed by a (true) sentence whose truth can be persevered in the countable model delivered by DLS.

All in all, it seems that this is a huge problem for a first-order defense of AG-∀. However, DLS depends on the weakness of FO-logic. So this kind of indeterminacy can be read as steaming from the weakness of FO-logic, rather than to be inscribed to language in general. The argument based on DLS is therefore a good reason to abandon FO for HO-logic. In this respect, it is interesting to look at what the defender or the enemies of this argument have said about the possibility of going higher-order. In fact, both presuppose that the right logic is FO-logic, otherwise the argument cannot be formulated. We have already dismissed Lavine's view on this topic; McGee [2000] only says that there remain many suspicions about SO-logic and therefore it is preferable to seek a FO-solution. This clearly begs the question against the aim of this chapter that wants to show different limitation of FO-logic when AG-∀ is admitted, motivating the choice of HO-logics. What Putnam said at the end of his 1980's paper is more interesting: to admit the legitimacy of second-order logic makes "necessary to attribute to the mind special powers of "grasping second-order notion"" (Putnam [1980], p. 481). I think Putnam is right in claiming that once we acknowledge the legitimacy of SO-logic (and its irreducibility to FO-logic), we must also argue that the mind grasps SO-notions; however, I think this is far more plausible as Putnam here is suggesting: grasping SO-notion is not grasping some transcendent meaning, but just grasping how quantification over predicates works. We are going to say more on this topic later on. For the time being, we conclude that the argument from DLS poses a real threat to AG-∀ in FO-logic.

1.5. A williamsonian version of Russell's paradox.

The last objection we shall raise against the possibility of having AG-∀ within first-order logic concerns the paradox. This is meant to be the strongest objection: form AG-∀ within FO-logic it is easy to derive a contradiction.

Let's consider a FO object language for which AG-∀ is true (let us call D its absolute domain), and consider a FO meta-language in which we are going to give the semantics of the object language. We suppose AG-∀ is true also for the meta-language[108]. Let us also suppose that Sem-Opt is true. So we can give a full semantics for the object language. Of course, the semantics cannot be a MT-semantics for the reasons we saw before. The reason why we take Sem-Opt as true is that we want to have enough expressive power to generalize over all interpretations of the object language, in order to define basic notions as the one of logical consequence. In addition, if interpretations are objects (as the first-orderist maintains), AG-∀ implies that we can quantify over all interpretations: were it not the case, then our quantifiers would not range over everything. Now, Williamson [2003] points out that for each meta-language predicate Φ that applies to some objects $\pi_1, \dots, \pi_n$ there is a legitimate interpretation of the object-language predicate μ such that μ applies exactly to those objects denoted by the meta-language terms ⌜ $d_1, \dots, d_n$ ⌝. Let's use i as a first-order meta-variable for interpretations, and let $\forall d$ be a quantifier which ranges over items d such that $d \in D$[109]. Williamson formalizes the condition in the following way:

GS- $\boldsymbol{\mu}$: Given a condition $\phi(d)$, there is an interpretation i under which μ applies to the item d if and only if $\phi(d)$: $\exists i \forall d(\mu - applies(d,i) \leftrightarrow \phi(d))$.

From (GS- μ) we can derive a contradiction:

1. $\exists i \forall d(\mu - applies(d,i) \leftrightarrow \phi(d))$ (GS- μ)
2. $\exists i \forall d(\mu - applies(d,i) \leftrightarrow \sim\mu - applies(d,d))$ by instantiating $\phi(d)$ with $\sim\mu - applies(d,d)$

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108 This is necessary, otherwise we could not give a proper semantics for the language. For more details see Williamson [2003], and our chapter 3, § 2.

109 I am following the presentation of the paradox given in Studd, forthcoming.

3. $\forall d(\mu - applies(d, a) \leftrightarrow \sim\mu - applies(d, d))$ by instantiating the existential quantifier

4. $\mu - applies(a, a) \leftrightarrow \sim\mu - applies(a, a)$ by instantiating the universal quantifier with a

This is nothing new: it is just a reformulation of Russell's paradox for the notion of interpretation. Suppose you consider the interpretation that applies to all and only interpretations that do not apply to themselves. Does such interpretation apply or not apply to itself?

We have motivated (GS-μ) by means of Sem-Opt. However, we could also have motivated it by means of a weaker principle which Studd (forthcoming, chapter 2) calls 'universe-based semantic optimism':

UBSem-Opt: Given an arbitrary language, it should be possible to articulate a generalized semantic theory for that language based on the universe D of that language.

The reason why one might prefer (UBSem-Opt) is that, if she is a relativist, she might claim that it is not possible to quantify over all interpretations. So a relativist would refuse (Sem-Opt). However, the relativist does not have any problem about quantifying over all interpretations of a given language, whose – for her restricted – domain is D, and therefore, she could be happy with (UBSem-Opt). But as the argument toward the paradox shows, even (UBSem-Opt) implies the contradiction.

Before proceeding, there are two objections worth being dealing with. The first one has been put forward by Peter Smith [2008], in his review of the book *Absolute Generality.* Smith argues that we are not forced to abandon unrestricted quantification to reply to Williamson's version of Russell's paradox, ‹‹instead that we shouldn't treat an interpretative 'true-of$_k$' relation defined in terms of R- as on a par with the true-of relation we started off with›› (Smith [2008], p. 400). The proposal is to introduce a hierarchy of 'true-of' predicates: ‹‹the moral could be that we have to ramify the truth-predicates, and recognize that given some such predicates, we can always 'diagonalize out' and define another new predicate which is not co-extensive with any of them››. Smith's solution is therefore to embrace an ideological hierarchy in the line of a tarskian hierarchy. We are going to deal with such a kind of solutions when we are dealing with Williamson's type theoretical proposal: there we are going to move some general objections which aim to show that any ideological hierarchy, despite being the standard way out from semantic paradoxes, is inadequate. Such criticisms suggest that we should look for a different kind of solution.

A further objection has been raised by Bennet & Karlsson [2008]. They propose to interpret Williamson's paradox like the Barber's paradox and not like Russell's paradox. The latter is an authentic paradox because the admittance of the set of all non self-membered sets leads to a contradiction, and at the same time, we have a principle - the naïve comprehension principle - which affirms the existence of that set. In the case of the

Barber there is no principle that forces us to accept that there is a Barber that shaves all and only the people who do not have themselves; so the contradiction we obtain by supposing that there is such a barber, can be used to argue – by *reductio* – that such a barber does not exist. According to Bennet & Karlsson this is also the situation with Williamson's paradox. Here we have a comprehension principle for interpretation, which is essentially (GS-μ). However, they hold that it is valid only for *contentful* predicates, and the predicate that gives rise to the paradox (being an interpretation that applies to all interpretations that do not apply to themselves) is not a contentful predicate, because it leads to paradox. So the comprehension principle does not force us to admit the existence of an interpretation that applies to all and only interpretations that do not apply to themselves.

The problem with such a position consists in understanding how one can decide whether a predicate is contentful or not. The comprehension principle should say us when there is a legitimate interpretation and it says that by linking interpretations with predicates. However, if it is restricted only to contentful predicates without giving a general law to discriminate between contentful and non-contentful predicates, it will be unable to tell us when a certain interpretation exists (it may be that the predicate we consider is not contentful). Moreover, if we apply the same strategy to the case of sets, we get that the naïve comprehension is restricted to conditions that do not lead to paradox, and so it does not commit ourselves to the Russell's set. But such a restriction is useless because we need a principle that says us which sets exists, and such a principle is not able to[110]. Moreover, the interpretation in question seems perfectly contentful. It is based on two ingredients that we surely understand: that there are things that do not satisfy an interpretation, and the notion of 'all'. It seems *ad hoc* to claim that such a predicate does not express any interpretation only because it leads to paradox. The conclusion we draw is that the williamsonian version of Russell's paradox constitutes an authentic problem for the first-orderist.

At this point we think that the quinean is forced to recognized that she cannot specify a fully adequate semantics for a first-order language. In the presence of AG-∀ her ideological resources bound her possibilities of maneuver. What the situation suggests is that we need higher-order ideological resources to accommodate AG-∀. We conclude this paragraph by claiming that we have strong reasons to think that AG-∀ forces us to abandon Quine's dogma that just FOL is a legitimate logic.

2. The plural approach to absolute generality

2.1 A general introduction to the plural approach

The plural approach to absolute generality steams from the development of a plural logic by George Boolos in the eighties (Boolos [1985a]). Boolos' general aim was to show

[110] There is a further reason why such a proposal does not work: as shown by Incurvati & Murzi (2017), there are multiple incompatible sets of mutually consistent instances of Naïve Comprehension, none of which is recursively axiomatizable.

that SOL was indeed a logic, against the criticism raised by Quine, according to whom SOL was nothing else than "set theory in sheep's clothing" (Shapiro [1991]). Boolos's project had to answer both the points we saw in §1, and this was possible by bringing into the picture the notion of "plural" (or plurality)[111] and to develop a plural logic which could avoid both the antinomies and the commitment to classes.

Why was Boolos interested in the logicality of SOL? There are at least two reasons connected with set theory that motivated Boolos towards his plural interpretation of SOL, and one more general reason concerning the formalization of natural languages' sentences. First of all, we need SOL to formalize talk about objects that cannot form a set as the non-self-membered sets or the ordinals. For instance:

a) Every ordinal has an immediate successor.

This sentence seems to be true. However, if it is formalized in standard FOL, the FO-quantifier needs the set of all ordinals to range over; but there cannot be such a set on pain of paradox. Secondly, SOL is needed to fully capture the meaning of axioms as Replacement and Separation, which are expressed by means of schemas in the FO-formalization of set theory[112]. Thirdly, there are natural language's sentences that cannot be formalized in FOL. The most famous example is the so-called Kaplan-Geach sentence[113]:

(K-G) Some critics admires only one another.

These are certainly three powerful reasons not to be happy with FOL alone.

What are plurals (or pluralities)? Plurals might be defined in comparison with sets. In standard set theory, a set is an object different from its elements. If we take some elements and apply the *set of* operation, what we get is a new object, the set whose elements are precisely the starting elements. For instance, if we start with the empty set $\emptyset$, the *set of* operation gives the singleton of $\emptyset$, that is $\{\emptyset\}$. It is exactly because the latter is a different object from the former that we can apply the *set of* operation (again) to $\emptyset$ and $\{\emptyset\}$ to get $\{\emptyset, \{\emptyset\}\}$. On the contrary, plurals are not different objects from their members, rather they are simply *their members considered at once.* So if the set of living dogs is a different object from the dogs (for instance it is a set and not a dog, it is an abstract object, and so on), the plurality of the living dogs is simply *the dogs*. Plurals are

[111] I will use the terms plural and plurality as synonyms. The reader must be aware of the fact that this is just loose talk, to be substituted by plural expressions: the plurality of the sets should be replaced with the plural expression 'the sets'. A plural expression is an expression that allows reference to several individuals at once.

[112] Boolos [1998], p. 65: ‹‹To declare it illegitimate to use second-order formulas in discourse about all sets deprives second-order logic of its utility in an area in which it might have been expected to be of considerable value. […] Whatever our reasons for adopting Zermelo-Fraenkel set theory in its usual [first-order] formulation may be, we accept this theory because we accept a stronger theory consisting of a finite number of principles, among them some for whose complete expression second-order formulas are required››.

[113] For the proof that the sentence cannot be formalized in FOL see Boolos [1985a] in Boolos [1998] p. 57.

nothing over and above their members, and in this sense, they are not (new) objects at all.

From this general difference, many other differences can be derived. While sets are abstract objects (even in the case that their elements are concrete), pluralities whose members are concrete individuals are concrete. Sets do not have any causal powers, while pluralities have as causal powers the sum of the causal powers of their members. Sets are not located either in time or in space, while pluralities are located where (and when) their members are located[114]. While there is an empty set (a set with no elements), it is usually assumed that there is no empty plurality (a plurality with no members at all)[115]; while the singleton of an object x is a different object form x, a plurality with only one member is identical with that member[116].

The membership relation – in symbol: ∈ - is the relation that an element bears to the set to which it belongs. The correspondent relation between pluralities and their members is the 'is one of'-relation, which we shall symbolized with '$\prec$': $u \prec xx$ must be read as u is one of the xx. The 'is one of'-relation is not the part-whole relation studied by mereology. For instance, suppose in a room there are five people. Each single person is one of the members of this plurality. However, the parts of each person (the legs, the heads, and so on) are not members of the plurality. On the contrary, if you consider the mereological sum of those people, then the parts of each of them will be parts of the sum. So the two relations are clearly distinct[117].

Natural languages show the existence of plurals in what has been called "collective predicates" (in the literature they are often called 'multigrade'[118] predicates: see Linnebo & Nicholas [2008]; Moltmann [2015]). An example of a collective predicate is 1:

1) There are 4 children in the garden (= the children in the garden are four).

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114 See Simon [2016], pp. 59-60. Of course, these features may be challenged. For instance, one might want to say that the singleton of a concrete thing –say Socrates – is located where Socrates is. I do not like such a view, but I am not interested now in criticizing it. For our purposes, what matter is the distinction between sets and pluralities.

115 There is no agreement in the literature about the lowest number of elements a plurality can have. Somebody thinks that a plurality has to have at least two members; others (for instance Linnebo [2003]) allow talk of pluralities with only one members; Oliver & Smiley [2013] allows talk of empty pluralities. I follow Linnebo in admitting one member pluralities, but not empty pluralities.

116 Simmons [2016], p. 60 argues that if we accept that Russell and Whitehead exist, then immediately we are accepting that the plurality whose members are exactly Russell and Whitehead (and nothing else) exists; but this is not the case with sets: we can accept that Russell and Whitehead exist, while at the same time we can deny that the set whose elements are exactly Russell and Whitehead exists. This is in fact the position of Simmons, who is a nominalist, and consequently think that no sets exist. However, one might deny this difference on the grounds that sets, if they exist, are abstract entities, which means that if they exist, they exist by necessity. If then one is committed to the idea that for any plurality of objects there can be a set whose elements are exactly those objects (see Linnebo [2010]), then one is committed to the view that, given Russell and Whitehead, we are immediately committed to the set whose elements are exactly Russell and Whitehead.

117 For more detail, one can see Oliver & Smily [2013], p. 34.

118 A multigrade predicate is a predicate that allows that more than one argument appear in a single argument place, as in 'Alice and Bob cooperate' (see Linnebo & Nicholas [2008]).

What is the semantic reference of the predicate "…are four"? It cannot be the set (or the collection) of the children, because the set is one and not four, but at the same time it cannot be each individual child, because each child is one and not four. The predicate "…are four" can only refer *to the children* (all the children in the garden) *at once*. The existence of such predicates seems to indicate the necessity of plurals in explaining the semantics of some sentences: plurals are the reference of certain terms. Therefore, the unity in which a plurality consists is a semantic unity, not an ontological one: from an ontological point of view, what exists are only the members (in our example, the children). It seems that a collective predicate requires a unity of the objects it refers, but this unity does not have to be an ontological one, rather only a semantic unity (a unity gained through an act of reference). In other words, the notion of plurality steams from the possibility of referring to many objects at once.

Standard FOL quantifies only over individual variables, while to speak of many things we need SOL, that is we need to quantify over predicates (and so we can say, for instance, "for all things that fall under a certain concept…"). But from a semantic perspective predicates are usually treated as sets or classes of objects. The notion of plural helps us speaking of many objects without committing us to the existence of the class of these objects. Therefore, a plural logic, which quantifies over plural variables, should allow us to get what SOL gives us without committing to classes, and it should show us a way out of the paradox. Indeed, Boolos developed such a logic and showed that we can use it to interpret monadic SOL[119]. This logic is first order, in the sense that the quantifiers only range over plural first-order variables. Consequently, it is usually called *Plural First Order Logic* (PFO)[120]. PFO is simply gained by adding to FOL plural quantification, i.e. expressions as $\forall xx$ or $\exists xx$, to be read respectively as "for all xs" and "there are some xs", and the two-place predicate 'is one of': '$\prec$'.

What actually Boolos showed is that it is possible to translate monadic SOL into PFO, i.e. any sentence of monadic SOL can be translated in a sentence of PFO. Here is a translation mapping (I took it from Linnebo [2003], p. 74):

$$Tr(X_j x_i) = x_i \prec xx_i$$

$$Tr(\sim\varphi) = \sim Tr(\varphi)$$

$$Tr(\varphi \wedge \phi) = Tr(\varphi) \wedge Tr(\phi)$$

$$Tr(\exists x_i \varphi) = \exists x_i Tr(\varphi)$$

[119] Boolos [1985a]. Linnebo & Nicholas [2008], §1: ‹‹Since plural quantifiers are logically just like second-order quantifiers over monadic (or one-place) concepts, plural logic has the expressive and deductive power of monadic second order logic. But unlike second-order logic with its vast ontology of concepts (or classes or other 'second-order entities'), plural logic does not seem to be committed to any new entities over and above the objects that its ordinary singular quantifiers range over››.

[120] Linnebo [2003]. The fact that we can interpret monadic SOL with PFO means that we can translate monadic SOL into PFO.

$Tr\big(\exists X_j\varphi\big) = \exists xx_i Tr(\varphi) \vee Tr(\varphi^*)$, where φ^* is the result of substituting $x_i \neq x_i$ everywhere for $X_j x_i$. (The second disjunct is needed to accommodate the case in which nothing falls under X_j).

Of course, to show that this translation does the work it is supposed to do, one must shows that each theorem of monadic SOL is mapped into a logical truth of PFO. To do so one must introduce a theory based on PFO, with a plural comprehension axiom and a deductive system. An example can be found in Linnebo [2003], pp. 74-75.

What is important to understand is the meaning of this translation. What exactly does it mean that monadic SOL is translatable into PFO? Does it mean that the two logics are equivalent? To answer this question, we must remind that the theorems of monadic SOL must be translated into *logical* truths of PFO, not into truths in general. This opens the possibility that the same sentence may express different propositions with different truth-values if it is formalized in SOL and in PFO. And it is in fact what happens if we take plurals as modally rigid. For example[121], the following sentence

(1) If anything could have been wet then it is wet

seems obviously false. My pullover could have been wet, but luckily it is not. A first-order formalization of it comes out false:

(2) $\forall x(\Diamond Wx \rightarrow Wx)$

We can now generalize (1) and (2) to obtain:

(1') If anything could have been X then it is X.

(2') $\exists X\forall x(\Diamond Xx \rightarrow Xx)$

Also these second-order versions seem to be false.

However, a first-order plural translation of both (1) and (2) turns out to be true:

(3) Any things are such that if anything could have been one of the wet things, then it is one of the wet things.
(4) Any things are such that if anything could have been one of the things such that X, then it is one of the things such that X.

Given the rigid reading of plurals, (3) and (4) seem true: for any thing and any things is not contingent whether the former is one of the latter (Williamson [2013]).

Why considering plurals as modally rigid? For sure plurals have an extensional nature in the sense they obey a plural version of the axiom of extensionality:

(Ext-P) $\forall xx\forall yy\ (xx = yy\ \leftrightarrow \forall u(u \prec xx \leftrightarrow u \prec yy))$.

[121] The example is taken from Williamson [2003]; see also Williamson [2013], pp. 241-242.

Therefore, coextensiveness is the *analogue*[122] of identity for pluralities: ‹‹if every one of these is one of those and every one of those is one of these, then these just *are* those›› (Williamson 2016, RTL). As a consequence, (Ext-P) is usually regarded as the criterion of identity for pluralities. The extensional nature of pluralities suggests that they modal profile is a rigid one, even if this latter aspect does not logically follow from the former[123].

In any case the most natural reading of plurals is the rigid one, and this is the reading we shall follow here. Moreover, it is the reading presupposed by proponents of plural logic to deal with the problem of unrestricted quantification. This marks a real difference between monadic SOL and PFO that makes the two logics non-equivalent[124].

2.2. Ontological innocence?

Another great debate, which we are not going to deal with, concerns the effectiveness of the claim that plurals are ontological innocent: Boolos was a strong defender of this idea, but others have expressed strong doubts about it. Boolos's example is well-known: when I eat the cheerios, I am not eating the set of the cheerios, rather I am eating THE CHEERIOS! Such simple examples are quite compelling; however, things become more difficult with cases of cross reference between pluralities and sub-pluralities. It has been suggested that the understanding of sub-pluralities requires some combinatorial and set theoretic notions (Parsons [1990], Linnebo [2003]). In any case, the fact that it is not clear if we can avoid reifying plurals when we deal with sub-pluralities or pluralities of pluralities does not constitute an ultimate reason against the innocence of plurals. It may be that we cannot help ourselves reifying pluralities (and so our comprehension of them requires some set theoretic notions); however, they still may be legitimated from a logical point of view. In particular we shall see below why the plural approach need a hierarchy of not reified plurals.

Of course, if plurals are not ontological neutral, but they commit us to the existence of set-like objects, then the plural approach towards the antinomies could not provide us with any solution to them. Therefore, in what follows, we will assume that the plural approach is ontologically neutral to see what it says about the paradoxes.

2.3 Plural logic, the antinomies and absolute generality

[122] One might be worried that the introduction of a sort of criterion of identity between pluralities implies treating pluralities as particular entities. However, this is not the case. A plurality is just many individuals considered at once. To say that the plurality of men is identical to the plurality of rational animals is just loose talk to be substituted with the claim that the plural term 'men' refers to the same objects of the plural term 'rational animals' or, which is the same, the two terms are coextensive.

[123] Linnebo [2016] gives a survey of different arguments that can close the gap between extensionality and modal rigidity. All these arguments assume further principles that, in combination with extensionality, give modal rigidity as a result.

[124] Of course, another clear difference between the two logics is that SOL quantifies into predicate position, while PFO quantifies into name position.

What is the verdict of the plural approach about the antinomies? Boolos's answer is simply that the antinomies show that there are pluralities of objects that cannot form a set. Considered the Russell set: since if we admit it we get a contradiction, it simply cannot exist (the paradox is therefore seen as a *reductio*). However, there are sets that do not belong to themselves. So the predicate "not belonging to themselves" defines a plurality of sets that, on pain of contradiction, cannot form a set. This diagnosis is possible because, by using plurals, we can rewrite the NCP as the conjunction of two different principles. Remember that NCP is the following principle[125]:

NCP: $\exists y \forall x (x \in y \leftrightarrow \phi(x))$

NCP can be seen as the conjunction of a *Plural Comprehension Principle* (Pl-CP) and *Collapse*[126].

Pl-CP: $\exists xx \forall u (u \prec xx \leftrightarrow \phi(u))$

where $u \prec xx$ is to be read as "u is one of the xs".

Collapse: $\forall xx \exists y Form(xx, y)$

where $Form(xx, y) =_{def} \forall u (u \prec xx \leftrightarrow u \in y)$. $Form(xx, y)$ must be read as "the plurality of xx forms the set y". By the transitivity of the conditional, we have that NCP is equivalent to their conjunction.

Pl-CP says that a predicate ϕ determines the objects that fall under it. From this perspective, it seems quite unproblematic. Therefore, Boolos can read the antinomies as *reductio* argument, because he proposes a new comprehension principle, which does not commit us with the existence of the problematic collections. On the contrary, Collapse says that every plurality forms a set: we do not need nothing more than its elements to apply the operation *set of* to get the corresponding set. Now, if we take the predicate "being a set", Pl-CP says that there are some sets that are all and only the sets; by applying Collapse to these sets, we find the set of all sets. Contradiction. What principle to get rid of? Since PL-CP seems unquestionable, the only alternative is Collapse. Consequently, the plural approach blames Collapse as the responsible of the raise of the contradiction.

Not only does the plural approach provide a solution to the paradoxes, but it also gives a way of interpreting unrestricted quantification over all sets. In standard logic quantification requires a domain of quantification and, in model theory, a domain is taken to be a set. Since there is no universal set, within model theory we cannot unrestrictedly quantify over the whole set theoretic universe. Now, the plural approach suggests that unrestricted quantification should be interpreted in a plural form, i.e. the domain of the quantifier should not be taken as a set, rather as a plurality: in our case

[125] For the sake of simplicity, we express here NCP in FOL, that is by means of an axiom schema.
[126] See Yablo [2004] and Linnebo [2010]. The formalization is taken from Linnebo [2010].

the universal quantifier would range over *all sets.* Therefore, what the plural approach rejects is what Richard Cartwright has called the "All-in-one's principle"[127]:

All-in-One: quantifying over certain objects presupposes that these objects are collected in a set or a set-like object.

The acceptance of this principle along with the nonexistence of the universal set imply the impossibility of quantifying over the whole set theoretic universe. But, according to Cartwright, the principle constitutes only an extrinsic feature of model theory and not a logically true statement[128]. In addition, he argues that the simple existence of *some things* allows them to be the values of a first-order variable. So Cartwright is taken a plural approach to quantification, which commits himself to the "All-in-Many principle":

All-in-Many: Quantifying over some objects satisfying a certain condition is to presuppose that there are *some objects* that are all and only those objects that satisfied that condition (Uzquiano [2009], p. 312).

Even if there is no universal set, the All-in-Many guarantees that we can quantify over all sets, because they constitute a plurality, not a set. That there are some objects that are all and only sets is, in turn, guaranteed by Pl-CP. The plural approach provides us with an appealing solution to the problem of unrestricted quantification, which consists in rejecting the All-in-One and embracing the All-in-Many and, consequently, in proposing to interpret unrestricted quantification as a plural quantification.

Let's now go back to Russell paradox. Consider the predicate "not belonging to itself". By PL-CP there are some sets that are all and only the sets which do not belong to themselves. But Collapse is not more valid, so we cannot conclude that there is a set that comprehend all and only those sets. So, not only did we block the derivation of the contradiction, but we block the argument in favor of the indefinite extensibility of the concept "not belonging to itself" (see chapters. 1 and 2). This argument requires the existence of the Russell set and therefore requires Collapse.

2.4 Semantics in a plural setting

We have just seen that the plural approach gives us an interpretation of Russell's paradox, a comprehension principle that avoids it, and the possibility of quantifying over everything. Now, we are going to introduce a formal language to study in more depth how the plural approach behaves with regards to semantic theorizing. Essentially, we are following Rayo [2006] approach, which exactly aims to study semantic theorizing in

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[127] Cartwright [1994]. Other defense of the plural approach to absolute generality can be found in Burgess [2004], Cartwright [2001], Oliver & Smiley [2013], Uzquinao [2003, 2009], van Inwagen [2009].

[128] Before we saw that the first-orderist finds herself committed to the All-in-One principle as soon as she accepts Sem-Opt. Sem-Opt plus the idea that the only legitimate form of quantification is standard (i.e. singular) first-order implies such a principle. However, the plural approach denies the second premise of this argument (allowing also plural first-order quantification) and therefore can reject the All-in-One.

a plural setting in the presence of absolute generality. What Rayo shows is that it is not possible to provide a general semantics for a certain language whose quantifiers are totally unrestricted in a language of the same type. The reason is a version of Russell's paradox that we saw before. The central point here is that, in denying that the non-self membered sets forms a set, but are only a plurality, the plural approach is forced to ascend from a language *n* where the quantifiers plurally range over these sets to a language *n+1* where the quantifiers can range over different pluralities (not sets!) of these sets. This is necessary not to treat pluralities as particular objects such as their individuals, which – in this case – would lead immediately to the paradox.

2.4.1 First-level expressions

A first-level predicate is a predicate that takes a singular term in each of its arguments. For instance, "… is an elephant" is a first-level predicate. Since the plural approach, such a predicate does not stand for the set of elephants, but for the plurality of elephant: "…is an elephant" stands for the elephants themselves:

$$\exists xx(\forall y(y \prec^{1,2} xx \leftrightarrow Elephant^1(y)) \wedge Ref^{1,2}('\ \dots is\ an\ elephant', xx))$$

Where '≺' is to be read as '…is one of…'; the upscripts indicate the level of the argument of a predicate (1 for individuals; 2 for plurality of individual, 3 for plurality of pluralities of individual and so on), and '$Ref^{1,2}$' is to be read as '… refers to…'. We can informally read it as follows: there are some things – the *xxs* – such that for every y, y is one of the *xxs* if, and only if y is an elephant and the predicate '…is an elephant' refers to the *xxs*.

In this setting, predicates refer plurally, and in the same way first-level plural terms. The plural term 'the elephant' does not stand for the set of the elephants, rather for the elephant themselves:

$$\exists xx(\forall y(y \prec^{1,2} xx \leftrightarrow Elephant^1(y)) \wedge Ref^{1,2}('the\ elephants', xx))$$

At this point Rayo introduces a *saturation operator* $'\sigma'$ such that, given a first-level predicate $'P(\dots)'$,$\sigma[P(\dots)]$ is a first-level term for which the following holds:

$$\forall xx(Ref^{1,2}('P^1(\dots)', xx) \leftrightarrow Ref^{1,2}('\sigma[P^1(\dots)]', xx))$$

For example, the application of the σ-operator to the first-level predicate '….is an elephant' gives rise to the first order term 'the elephant(s)'.

2.4.2 Second-level expressions

A second-level predicate is a predicate that takes a plural term (a first-level term) in (at least) one of its argument. The predicate '…are scattered on the table' is plausibly a genuine second-level predicate. It is of course possible to interpret this predicate as referring to all and only the *sets* whose members are scattered on the table; since sets are objects, this interpretation would make it a first-level predicate. However, in the plural interpretation such a predicate is intended to plurally refer to the *pluralities* whose members are scattered on the table.

Rayo characterizes the reference of such a predicate as follows:

$$\exists xxx\left(\forall yy\big(yy \prec^{2,3} xxx \leftrightarrow Scattered^{2}(yy)\big) \wedge Ref^{1,3}('\ldots are\ scattered', xxx)\right)$$

Where the treble variables are used for *super-plurals*, pluralities of pluralities. Quantification over super-plural is therefore called super-plural quantification. But what is it? As first-order plural quantification is quantification over several individuals at once, super-plural quantification (or second-order plural quantification) is quantification over several plurals at once. However, one should not interpret the latter as if it were quantification over particular objects, namely pluralities. Pluralities are not objects; what there are, are only the individuals: ‹‹super-plural quantification is not singular (first-order) quantification over a new kind of 'item' (super-plurality), nor is it plural quantification over a new kind of 'item' (plurality). Super-plural quantification is a new kind of quantification altogether›› (Rayo, 2006, p. 227)[129].

The need of interpreting super-plural quantification as a new kind of quantification (and not as an already accepted form of quantification over a new kind of item) steams from the paradox. If we reified pluralities, then we would immediately face a version of Russell's paradox.

We can now use the σ-operator to form second-level terms from second-level expressions by means of the following stipulation:

$$\forall xxx\big(Ref^{1,3}('P^{2}(\ldots)', xxx) \leftrightarrow Ref^{1,3}('\sigma[P^{2}(\ldots)]', xxx)\big).$$

2.4.3 Going beyond

We can reiterate the story to n-th level terms and predicates. So a third-level predicate is a predicate that takes a second-level term in one of its arguments (and no term of higher level). The reference of such predicates will be *super-duper-plurals,* pluralities of super-plurals. And so on until obtaining an infinite hierarchy of higher and higher levels.

The hierarchy we obtained is a hierarchy made of higher-level predicates, which must not be confused with the hierarchy of higher-order predicates (we shall discussed an example of the latter later on). A (monadic) *n+1-level* predicate takes a *n-level* term as an argument, while a (monadic) *n+1-order* predicate takes a *n-order* predicate as an argument. For example, whilst both first-level and order predicates refer to individuals

[129] The most straightforward way of arguing for the legitimacy of super-plurals and super-plurals quantification consists in showing that there are terms referring to super-pluralities in natural languages. The issue has not been settled yet; the interested reader should consult the following literature: Linnebo [2003] & McKay [2006] argues against the presence of super-plural in English; Linnebo & Nicholas [2008], Moltmann [2016], Simmons [2016] argue in favor of such a presence.

(the standard semantics would require sets, but it is certainly possible to interpret a first-order predicate as referring to some individuals), a second-level predicate refers to several pluralities of individuals, while a second-order predicate is a predicate of predicates. The two hierarchies must not be confused.

The plural approach thus leads to a type-theoretic approach when dealing with higher-level pluralities. Let us now have a look at the higher-level language that Rayo defines. What he defines is a Limit-ω language. From a type-theoretic point of view this is a ω-type language, where ω is the first transfinite type. This may seem weird (standard type-theory does not have any transfinite type, the reason being that transfinite type seems beyond our capabilities). However, as we shall see when discussing Williamson's proposal, type-theorist requires a transfinite language to quantify over all finite types (all finite languages). In particular, if we want to give the semantics for all the finite type, we need a transfinite type, as we are going to show below.

Rayo's limit-ω language consists of the following symbols:

1. the logical connectives $\sim$ and $\wedge$;
2. for $n \geq 0$ and $i \geq 1$, the placeholder $\ulcorner v_i^n \urcorner$;
3. for $i \geq 1$, the individual constant symbol $\ulcorner c_i^0 \urcorner$;
4. for s a finite sequence of positive integers and $i \geq 1$, the non-logical predicate-letter $\ulcorner P_i^s \urcorner$;
5. for $n \geq 2$, the logical predicate-letters $' =^{1,1'}$, $'\ulcorner \prec^{n-1,n} \urcorner'$, and $\ulcorner Ex^n \urcorner$;
6. for $n \geq 0$ and $i \geq 1$, the saturation symbol $\ulcorner \sigma_i^n \urcorner$;
7. the auxiliaries '(', ')', and '[', ']'.

Then he characterizes terms and formulas in the following way:

1. $\ulcorner c_i^0 \urcorner$ is a term of level 0;
2. $\ulcorner v_i^n \urcorner$ is a term of level n;
3. if s is the sequence $n_1, \ldots, n_m$ and $\ulcorner \mathrm{t}_1 \urcorner, \ldots, \ulcorner \mathrm{t}_\mathrm{m} \urcorner$ are terms of level $n_1 - 1, \ldots, n_m - 1$ (respectively), then $\ulcorner P_i^s(\mathrm{t}_1, \ldots, \mathrm{t}_\mathrm{n}) \urcorner$ is a formula[130];
4. if t_1 and t_2 are terms of level 0, then $\ulcorner \mathrm{t}_1 = \mathrm{t}_2 \urcorner$ is a formula;
5. if, for $n \geq 2$, $\ulcorner \mathrm{t}_1 \urcorner$ and $\ulcorner \mathrm{t}_2 \urcorner$ are terms of level $n-2$ and $n-1$ respectively, then $\ulcorner \mathrm{t}_1 \prec^{n-1,n} \mathrm{t}_2 \urcorner$ is a formula;
6. if for $n \geq 2$, $\ulcorner t \urcorner$ is a term of level $n-1$, then $\ulcorner Ex^n(\mathrm{t}) \urcorner$ is a formula;
7. if ϕ is a formula, then $\ulcorner \sigma_i^n[\phi] \urcorner$ is a term of level $n+1$;
8. if ϕ and ψ are formulas, $\ulcorner \sim \phi \urcorner$ and $\ulcorner \phi \wedge \psi \urcorner$ are formulas;

130 Notice that this is a non-cumulative type theory. A cumulative type theory is a type theory where a formula as *P(a)* is a wff in the case where the type of *P* is strictly greater than the type of *a*. A type theory is non-cumalative when a formula as *P(a)* is a wff just in case the type of *P* is exactly one type greater of the type of *a*, i.e. if *P* is of type *m*, *a* must be of type *m-1*. A cumulative hierarchy for super-plurals has been developed by Wagner E. [2015]. For the distinction between cumulative and non-cumulative type theory see Linnebo & Rayo [2012].

9. nothing else is a term or a formula.

A formula ϕ is a sentence if every occurrence of a place-holder ⌜ v_i^n ⌝ in ϕ is within a sub-formula of the form ⌜ $\sigma_i^n[\psi]$ ⌝ (this strange definition depends on the fact that there are no (primitive) quantifiers in the language).

The quantifiers are introduced with the following conventions:

$$\exists v_i^n(\varphi) =_{def} Ex^{n+2}(\sigma_i^n[\varphi])$$

$$\forall v_i^n(\varphi) =_{def} \sim\exists v_i^n(\sim\varphi)$$

Where $\exists v_i^0$ is meant to play the role of the singular quantifiers; $\exists v_i^1$ plays the role of the plural quantifiers; $\exists v_i^2$ plays the role of the super-plural quantifiers, and so forth. To give an example, $\exists v_1^0(Elephant^1(v_1^0) \equiv_{def} Ex^2(\sigma_1^0[Elephant^1(v_1^0)])$, which must be read as 'there is something that is an elephant if, and only if the elephants exist'.

At this point Rayo introduces a deductive system for such a language. I am not going to expose the system, which consists in a classical deductive system, together with a rule of saturation that governs the use of the saturation operation (I invite the interested reader to have a look at Rayo 2006).

2.4.4 The need for the hierarchy

Why does the plural approach need a hierarchy? The central argument Rayo provides is based – once again – on some version of Russell's paradox[131]. More specifically, Rayo argues that in the presence of absolute general quantification, it is not possible to provide a suitable general semantics for a given language in a language of the same logical type. This is the reason why the plural approach needs the hierarchy. The argument is based on the following result:

No Paraphrase: when an all-encompassing domain of discourse is allowed, it is not generally possible to paraphrase a basic second-order language as a first-order language.

The point is that if you paraphrase a second-order language with absolute quantification into a first-order language (for instance by nominalising predicates that refers to properties or by reifying pluralities as objects), you will run into a Russell's style paradox.

[131] Linnebo [2003] pp. 85-86 gives a further reason why the pluralist should need a hierarchy. In order to have a fully PFO we need also the impredicative instances of PL-CP. In turn, this means that we need to understand the notion of arbitrary sub-plurality, which implies that we understand the notion of plurality of (sub)-pluralities (higher-level pluralities). According to this view it is the need of impredicative instances of PL-CP that pushes us towards higher-level pluralities.

Say that a basic second-level language L^2 can be paraphrased as a first-order language only if there is a range of individuals such that, for any sentence in L^2, the following transformation preserves truth-values:

$(\exists v_i^0(\varphi))^{Tr} \longmapsto \exists x_i(\varphi^{Tr})$
$(\exists v_i^1(\varphi))^{Tr} \longmapsto \exists \alpha_i(\varphi^{Tr})$
$(v_i^0 \prec v_j^1)^{TR} \longmapsto x_i \in \alpha_j$
$(v_i^0 = v_j^0)^{TR} \longmapsto x_i = x_j$
$(P(v_{i\ 1}^0, \dots, v_{i\ n}^0)^{TR} \longmapsto P(x_{i_1}, \dots, x_{i_n})$
$(\phi \wedge \psi)^{TR} \longmapsto \phi^{TR} \wedge \psi^{TR}$
$(\sim \phi)^{TR} \longmapsto \sim (\phi^{TR})$

Where ⌜ x_i ⌝ ranges over individuals in the domain of discourse of L^2, ⌜ α_i ⌝ ranges over non-empty sub-collections (sub-sets) of these individuals, whilst '∈' is an appropriate membership relation.

Let us now see that No Paraphrase holds. Assume for *reductio* that it is generally possible to paraphrase a second-level language as a first-order language. The domain of discourse of L^2 contains absolutely everything, and let L^2 contain a predicate '*Member*', which is true of x and y just in case x is one of the elements of the collection y. Then we have:

1) $\forall v_1^1 \exists v_1^0 \forall v_2^0 (v_2^0 \prec v_1^1 \leftrightarrow Member(v_2^0, v_1^0))$.

By the translation we have:

2) $\forall \alpha_1 \exists x_1 \forall x_2 (x_2 \in \alpha_1 \leftrightarrow Member(x_2, x_1))$

Assuming that there are at least two objects, the last entails a contradiction. From 1 by **UE**[132] and **S**[133], we have:

3) $\exists v_2^0(\sim Member(v_2^0, v_2^0) \leftrightarrow \exists v_1^0 \forall v_2^0 (\sim Member(v_2^0, v_2^0) \leftrightarrow Member(v_2^0, v_1^0))$

It is now easy to prove that the antecedent of 3) is true. So we have:

4) $\exists v_1^0 \forall v_2^0 (\sim Member(v_2^0, v_2^0) \leftrightarrow Member(v_2^0, v_1^0))$

From which we can derive a contradiction by means of EE[134] and UE.

The problem can be looked at more directly by noticing that No Paraphrase entails that the following is a theorem of the deductive system exposed by Rayo:

[132] UE (Universal Elimination) is the following rule of Rayo's deductive system: $\exists v_j^n(\phi(v_j^n)) \rightarrow (\forall v_i^{n+1}(\phi(v_i^{n+1})) \rightarrow \phi(\sigma_j^n[\psi(v_j^n)]))$.
[133] S (Saturation operation) is the following rule: $\forall v_i^n(v_i^n \prec \sigma_j^n[\psi(v_j^n)] \leftrightarrow \phi(v_j^n))$.
[134] EE (Existential Elimination) is the following rule: if $\Gamma \vdash c = c \rightarrow (\phi(c) \rightarrow \psi)$, then $\Gamma \vdash \exists v_i^0(\phi(v_i^0) \rightarrow \psi$, where x does not occur free in Γ or ψ.

$$\exists v_1^0 \exists v_2^0 (\sim(v_1^0 = v_2^0)) \rightarrow \sim\forall v_1^1 \exists v_1^0 \forall \exists v_2^0 (v_2^0 \prec v_1^1 \leftrightarrow P_1^{1,1}(v_2^0, v_1^0))$$

The problem is that, from the perspective of the first-orderist, this sentence must be false when the $P_1^{1,1}$ expresses the membership relation appropriate for sub-collections of the domain of discourse (and the domain is absolutely not restricted), which in turn means that we cannot collapse down pluralities into objects as classes or sets.

This way of putting the argument is rigorous but a bit contorted. A much simpler, but still rigorous way of putting the argument is the following: consider the Pl-CP

Pl-CP: $\exists xx \forall u (u \prec xx \leftrightarrow \phi(u))$

This seems trivially true: given a condition ϕ, there are the objects that satisfy ϕ. Now first-orderism is the following principle: $\forall xx \exists y \forall u (u \prec xx \leftrightarrow u \in y)$ (this is exactly what we called before 'Collapse'). We can instantiate ϕ in Pl-CP with '$\notin$', which gives us the rr, those classes that are not members of themselves:

$$\forall u (u \prec rr \leftrightarrow u \notin u).$$

By Collapse we can infer: $\forall u (u \in r \leftrightarrow u \notin u)$.

By instantiating the quantifier with r, we obtain $r \in r \leftrightarrow r \notin r$, from which we can easily derived a contradiction.

No Paraphrase blames first-orderism (Collapse). In this setting, it corresponds to the translation: $(v_i^0 \prec v_j^1)^{TR} \longmapsto x_i \in \alpha_j$: the placeholder v_j^1 which can be substituted with a plurality of individuals is paraphrased as a single individual, i.e. the set that contains all individuals in the starting plurality. This is exactly the move we saw above: the pluralist denies that all pluralities form a set. In this more precise setting, we can see that this denial takes the form of the necessity of going higher-order. If the pluralist wants to be able to give a semantics for her own plural language and she wants to hold that her (plural) quantifiers range over everything, she is forced to adopt an ideological hierarchy as the one we have just described. Without going higher-order, she is forced to collapse down pluralities to sets, which is exactly what the plural approach refuses to do.

2.4.5 Rayo's conclusion

Rayo's general conclusion is that "when an all-encompassing domain is allowed, it is not generally possible to paraphrase a basic *(n+1)th* level language as a *nth*-level language" (Rayo [2006], p. 242).

No Paraphrase follows from Sem-Opt and AG-$\forall$. Once accepted these latter two principles and accepted the plural approach, we are forced to accept the hierarchy of languages. Of course, one could deny one of Sem-Opt[135] and AG-∀. However, Rayo does not intend to abandon either of them, and so he proposes the hierarchy as the least unattractive between the options on the table.

In any case, even if one accepts the hierarchical proposal there are still some considerations that must be dealt with.

2.4.6 Model theory

What is the effect of the plural approach to model theory? Model theory is the standard way of doing semantics (at least in logic). Can a pluralist follow a model theoretic approach in doing the semantics of a (plural) language? The answer is affirmative; however, she must partially modify the theory to avoid the following problem. A consequence of the generalization of Cantor's theorem to pluralities[136] is that there are more pluralities than individuals. Since in a language as the one defined by Rayo the reference of a monadic first-level predicate is a plurality, there are more ways of assigning reference to the predicates than there are individuals. Let us now define a model theory as 'strictly adequate' if each plurality gets assigned with a predicate. Therefore, a model theory for a full first-order language can only be adequate if it appeals to more models than there are individuals. However, this is not possible if models are based on sets (as in standard model theory). The reason is straightforward: the reference of predicates in standard model theory is given by sets, and sets are individuals.

The pluralist has an immediate answer: she just need to consider models based not on sets, rather on pluralities. Here is Rayo again:

> By taking a model to be a *plurality*, one can give a strictly adequate model-theory for first-level languages in a basic second-level language. (To fix ideas, think of a model m^1 as a plurality consisting of order-pairs of the form $<' \forall', x^0 >$ and order-pairs of the form $<' P_i^{1\prime}, x^0 >$ for 'P_i^1' a predicate in the language. Intuitively, $<' \forall', x^0 > \prec m^1$ just in case x^0 is in the domain of m^1, and $<' P_i^{1\prime}, x^0 > \prec m^1$ just in case x^0 is in the reference of P_i^1 according to m^1.) (Rayo, [2006], p. 243).

The pluralist can therefore give a semantics by using a plural version of model theory, which is something that we should have expected and that the pluralist will embrace happily.

Putting together this result with the previous one, we have Semantic Ascent:

Sem-Asc: it is impossible to give a strictly adequate model-theory for a limit-ω language in a limit-ω language.

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[135] To reject Sem-Opt means to embrace 'semantic-pessimism', the view that thinks impossible to give a semantics for some language built up from legitimate semantic categories. If this position is true, languages would have features that cannot be investigated because of the inner nature of language itself.
[136] For the generalization of Cantor's theorem to plurals see Bernays [1942], or Hawthorn & Uzquiano [2011].

This conclusion leads Rayo to embrace what he calls 'Open-Ended Optimism': the legitimate languages (the ones it is in principle possible to make sense of) form an open-ended hierarchy such that any language in the hierarchy can be given a strictly adequate model-theoretic semantics in some other language higher-up in the hierarchy.

To sum up, the plural approach thus gives a solution to the problem of absolute generality. Together with Sem-Opt, which we saw to be a compelling principle, the plural approach requires us to go higher-order. It is important to stress that the hierarchy is a purely ideological one: going up and up in the hierarchy the expressive resources of our languages enrich, but the ontology remains the same: the plurality of everything (over which the PFO-quantifiers range). The importance of not collapsing pluralities into sets (or other kind of objects) can also be seeing by means of the generalization of Cantor's theorem for pluralities, according to which there are more pluralities than individuals. If pluralities were a special kind of objects, then an unrestricted plural quantification over all individuals would not be absolutely general, because the number of pluralities of those individuals would be greater than the individuals themselves. If the PFO-quantifiers ranges are absolutely general, then pluralities cannot be any kind of object at all.

2.5 Problems for the plural approach

There are at least three big problems for this approach: the first one is a general problem regarding all ideological hierarchies; the second one concerns the idea that some pluralities cannot form sets; while the third one regards the concept of ordinal number.

2.5.1 The hierarchy of languages

There is a general worry regarding the introduction of a hierarchy of languages. In a nutshell: according to the theory it is not possible to generalize over all languages, because each sentence belongs to a certain language and, consequently, cannot generalize over upper languages. This objection can be formulated in a number of different ways, and, of course, defenders of hierarchies have given some replies. Since also the next proposal I am going to consider in §3 below is a hierarchical proposal, I am going to consider the objection in relation to that proposal; however, since the problem arises for the mere presence of an ideological hierarchy, the objection immediately applies to the plural approach. So I postpone the discussion in §3.

2.5.2 Limitation of size

The second aspect concerns the motivations of abandoning Collapse. The standard motivation makes appeal of the limitation of size's idea. This is an old idea, going back at least to Russell. Some pluralities cannot form a set, because they have too elements. Of course, the problem consists in making that "too" determined.

The idea of limitation of size is embedded in several aspects of standard set theory. The axioms of Separation and Replacement are an example of this. Separation prescribes to form a set from a given set by selecting some of its elements by means of a property; Replacement affirms that if we have a set and a function from its elements to the members of a plurality, then the plurality forms a set too. In both cases we need to start from a given set and this guarantees that the cardinality of the resulting set is not larger than the cardinality of the given set. In this way, we can never produce too large pluralities.

To make the idea of limitation of size work we need a further assumption: every two cardinalities must be comparable (*Cardinal Comparability*)[137]. Cardinal Comparability says that given two cardinals, *a* and *b,* tricotomy must hold. This is a consequence of the well-ordering of the set theoretic universe. Now, in turn this is implied by the axiom of choice. Therefore, cardinal comparability is a quite common assumption in set theory (at least it is as common as the adoption of the axiom of choice).

We know that the ordinals cannot form a set because of Burali-Forti paradox. Now, according to Cardinal Comparability we can compare whatever plurality with the ordinals. Limitation of size says that if there is an injection from the ordinals to a plurality, then the plurality has at least as many members as there are ordinals, and so it is too big to form a set. Examples of pluralities injectable with the ordinals are the pluralities of the cardinal numbers, the plurality of sets, and of non-self-membered sets. These would be examples of 'absolute pluralities'.

The defect of this strategy should be clear, in fact it is based on assuming the ordinals as a paradigm of a plurality that cannot form a set. An arbitrary plurality cannot form a set because it is injectable into the ordinals. And the ordinals? Clearly, we cannot give the same explanation for them, on pain of a vicious form of circularity. The only answer available for them seems to be that they cannot form a set because of the contradiction. But then the strategy reveals to be totally *ad hoc,* because no independent reason from the contradiction has been given for the claim that the ordinals cannot form a set.

Moreover, things do not improve if one tries to find a different threshold cardinality for the formation of sets. Since every set universe can be enlarged by adding to the underlined theory an axiom affirming the existence of an inaccessible cardinal of a certain rank, it seems that there is no reason to point out a particular cardinality as a threshold above which no plurality can form a set. Not only would every suggestion be arbitrary, rather it would contradict a fully legitimate set theoretic practice, the one of enlarging a set-universe by means of an axiom that affirms the existence of an inaccessible cardinal. We can certainly add a new axiom and, in this way, enlarge the universe; but this process cannot be carried out indefinitely, because at a certain point we will have too objects to be collected in a set (and so no new cardinal can be

[137] Linnebo [2010]. Linnebo develops this objection in detail. Here I just outline the main objection, and refer the interested reader to Linnebo's paper.

introduced). Of course the pluralist is not committed to the idea that if we go on adding new axioms of large cardinals, at a certain point we effectively reach a point where we must stop because there are too objects, as one who believes in the existence of the set of natural numbers is not committed to the idea that if we go on applying the successor function to finite numbers at a certain point we reach a number too big to be finite; however she is committed to the idea that the set universe is not indefinitely extensible and so there must be a threshold cardinality for set-formation.

In defense of Limitation of Size, one could try to argue that a plurality has too members if, and only if it has as many members as the objects in the whole universe. The threshold above which no plurality can form a set would not be arbitrary, being the elements of the whole universe.

There seems to be something intuitive about this reply: the plurality of everything cannot form a set, because there is nothing beyond it that can play the role of the set of everything, since there is nothing beyond everything. A set is a further object in relation to the plurality of its elements: if every object in the universe forms a plurality, then there cannot be a further object to play the part of the set of that plurality. The problem is that such conception of set for which a set is always a further object with regards to its elements is grounded in the iterative conception of set, where sets are formed in stages, starting from some urelements or the empty set, and by means of the reiteration of the 'set of'-operation.'[138]. The reason why there is no universal set is because given some sets in any stage of the hierarchy it is always possible to apply the 'set of'-operation to find the set that comprises exactly those members, and not because there are too members to form a set[139]. Moreover, there are other conceptions of sets that implies the existence of the set universe. For instance, the logical conception based on the naïve comprehension principle implies the existence of the universal set. This principle has often been regarded as very intuitive, which means that also the existence of the universal set should be quite intuitive (since it is a consequence of that principle). In addition, if there is a universal set, that set will be non-well-founded, which means that it is not true for it that it constitutes a *further* object with regards to its elements, because it is identical with one of its elements.

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[138] Formally, the hierarchy of sets is defined by transfinite recursion: $V_o = \emptyset$ or Urlements; $V_{\alpha+1} = V_\alpha \cup P(V_\alpha)$; $V_\lambda = \cup_{\alpha<\lambda} V_\alpha$ with λ a limit ordinal. Depth discussions of the iterative conception can be found in Boolos [1971, 1989] and Linnebo [2010, 2017].

[139] This does not mean that the iterative conception of set is incompatible with the plural approach: Boolos defended the validity of both this conception of set and the fact that the set universe is too big to form a set. This is a possible interpretation of the iterative conception. However, it is possible to interpret such a conception in a way for which given *any plurality of sets* it is always possible to apply the 'set of' operation. According to such an interpretation, there cannot be a plurality of set too big to form a set: the universe of set would be indefinitely extensible. Since both interpretations of the iterative conception are possible, it is not by appealing to this conception of set that one can justify the idea of limitation of size: either one makes appeal to a Boolos's style interpretation and so the limitation of size is already pushed inside the conception of set, which means that this conception cannot justify the limitation of Size's idea; or one makes appeal to indefinite extensibility that, if carefully defined (see chapter 2), is incompatible with Limitation of Size.

The intuitiveness of the idea that the threshold cardinality should corresponds the whole universe is therefore only apparent. It already presupposes a certain conception of set that excludes the possibility of the existence of the universal set. It seems that the Limitation of Size's idea cannot be justified by appealing to different considerations from the ones connected with the paradox, and from this point of view we find it totally *ad hoc* and unsatisfying.

2.5.3 How long is a well-order? An insidious problem for the plural approach

We saw that the plurality approach can stop both the derivation of the paradox and the argument toward indefinite extensibility. However, with ordinals this may be not enough. Let's consider Burali-Forti paradox, which is caused by the set of all ordinals. Since this is a well-ordered set, it has an order-type to which an ordinal, different from all ordinals that belong to itself, corresponds. Now, consider the following plural approach to it. The pluralist would argue that there is no set of all ordinals, because of the failure of Collapse. So we have *the ordinals*. However, they are well-ordered by their magnitude. Consequently, there is a different ordinal from all of them that corresponds to this well-ordered. We still have the paradox even in a plural context.

In order to understand better the issue, it is useful to cast an eye over a discussion between Boolos and Dummett on Basic Law V[140]. In that occasion Boolos charged Dummett of accepting the All-in-One principle:

> It would seem that he [Dummett] does think that there has to be a – what to call it – totality? collection? domain? containing all of the things we take ourselves at any one time to be talking about. […]. That is, he supposes that whenever we quantify, we quantify not over all the (ordinals or) sets that exist but only over some of them, and that, similarly, whatever sets we do on any occasion quantify over form a totality X which omits the item $\{x \in X : x \notin x\}$. Since $\{x \in X : x \notin x\}$ is a set (or a set-like item, an item "intuitively recognizable as a set"), we have not managed to quantify over all the sets there are.

What Boolos is saying is that if one accepts the All-in-One principle, then one is forced to accept the existence of indefinitely extensible concepts, because in this case the domain is thought to be an object (a set) and thus it can be added to its own objects, enlarging in this way the domain. Therefore, we cannot quantify over all sets, if one accepts the principle. The key point is the idea that indefinitely extensible concepts are a consequence of the All-in-One principle.

The reply of Dummett is straightforward, in fact he claims that one can accept the existence of indefinitely extensible concepts without accepting the All-in-One.

> Plainly, he [Boolos] takes this denial [the fact that Frege had no the idea of indefinitely extensible concepts] to follow from his repudiation of the view that the objects over which the individual variables of a mathematical theory range form a collection, super-class or what-do-you-call-it. But suppose that a platonistically inclined mathematician has formulated a theory whose variables ranges over ordinal numbers […]. Then the objects over

[140] Boolos & Cartwright [1993] and Dummett's reply [1994].

> which the variables range can be well-ordered by magnitude, and their order-type will satisfy the criterion for being an ordinal number, but cannot lie among the objects over which those variable range[141].

Dummett is simply saying that the concept "ordinal number" is indefinitely extensible even in the presence of the All-in-Many principle and not in the presence of the All-in-One. The reason depends on the way ordinals are defined, as order-type of well-orderings.

Notice that the argument does not presuppose that there are transfinite ordinals. Suppose you are a hard-core finitist, that is you are willing to admit only finite ordinals. But finite ordinals have a well-ordering and consequently, by the definition of ordinal, an ordinal should correspond to this well-ordering: this ordinal cannot be one of the finite ordinals. So we manage to extend the finite ordinals with a transfinite ordinal. This argument is particularly interesting because it shows exactly on what it is based:

1) The definition of ordinal numbers as order-type of well-orderings;
2) The All-in-Many principle, that is the possibility to consider the plurality of *all* finite numbers (without presupposing that this plurality forms a set).

Sentence 1) and 2) together imply that the ordinals are indefinitely extensible and therefore that we cannot quantify over all of them. A pluralist should reject one of these two premises in order to block the argument. Now, it seems hopeless to reject 2. We cannot do analysis if we cannot quantify over all the natural numbers. Moreover, rejecting 2 has the consequence of making unrestricted quantification impossible, against the pluralist's desiderata. Therefore, the only viable path to block the argument is by rejecting 1. But 1 is the standard definition of ordinal number, so it seems out of question to completely reject it. Maybe some restrictions may be imposed. Since the definition claims that all ordinals are order-type of well-orderings, the only restriction possible consists in claiming that there is at least one well-ordering to which no order-type (and therefore no ordinal) corresponds. This well-ordering must be the well-ordering of all ordinals. The idea is to claim that no ordinal corresponds to the well-ordering of all ordinals.

This idea is congenial to Boolos' approach to the problem. Against Dummett's argument above, Boolos could have argued that *all ordinals* are too much to form a set and, consequently, there is no order-type that corresponds to their well-ordering (since there is no set of all ordinals). The impossibility for the ordinals to form a set blocks the derivation from the claim that the ordinals are well-ordered to the claim that there exists an order-type (and therefore an ordinal) which corresponds to that particular well-order.

Does this trick work? The answer must be negative. Suppose that such a well-ordering (without a corresponding ordinal) in fact exists. However, from itself it is

[141] Dummett [1994], p. 248.

possible to define predicates corresponding to longer well-orderings[142]. If Ω is the well-ordering of all ordinals, then it is easy to show that it is well-ordered by the relation "less then". Now, if a and b are two ordinals, say that $a \prec_1 b$ if $a \neq 0$ and either $a < b$ or $b = 0$. What we have done is simply to add a 0 after Ω. What we get is a well-ordering longer than Ω. But we can also define well-orderings which are twice the length of Ω. Take $a \prec_2 b$ if either a is a limit ordinal and b is a successor ordinal, or they are both limit ordinals and $a < b$, or they are both successor and $a < b$. In this order the limit ordinals come before the successors, and both the limit ordinals and the successors are isomorphic to the ordinals.

The morale is that, given a well-ordering, we can define predicates corresponding to longer well-orderings, that is the notion of "well-ordering" is indefinitely extensible even if one supposes that there are well-orderings to which no ordinal number corresponds. These predicates are definable as soon as we can speak of a certain well-ordering; in the above example, as soon as we speak of the well-ordering of the ordinals. It is therefore clear that the plural approach fails, because it exactly allows the possibility of speaking of all ordinals and, consequently, it allows the definitions of further well-orderings. However, Boolos's previous reply could also be used here: the ordinals are too much to make any extension of their well-ordering possible. Here, he could argue as follows: it is true that the ordinals are well-ordered and that, given a certain well-ordering, we can define a strictly longer well-ordering. However, the ordinals are too much to form a set, which means that their well-ordering must be maximal. The fact that we can define strictly longer well-ordering does not prove that there actually are such well-orderings, as the fact that we can define what a round-square is, does not prove that there actually are round-squares. So, the idea of Limitation of Size can also be applied in this context. The problem with this answer is that it is not clear why facts about cardinality and size should matter in the definition of a well-ordering. The same extension of ordinals from the finite to the transfinite suggests that considerations of cardinality do not matter with the definition of ordinals at all. If we recognize the legitimacy of considering an object that lies after all members of a well-ordering (independently if the well-ordering is finite or infinite), and therefore if we recognize the legitimacy of ordinals as ω, we should recognize the legitimacy of 'placing an object' at the end of whatever well-ordering. In this sense, Boolos' reply seems to be very *ad hoc*, because in the case of natural numbers he would recognize as legitimate ω and the possibility of extending this well-ordering, while in the case of the ordinals, he would declare impossible to extend their well-ordering because they are too much, which is his explanation of the paradoxes. Ultimately, as recognized above, this explanation seems unclear and unintuitive, which means that the real reason why we should stop is to avoid the paradox. As Dummet

[142] I took these examples from Shapiro & Wright [2006], p. 288. They also notice that the argument works also against a strictly finitist: ‹‹notice that the constructions here are somewhat independent of how many ordinals one thinks there are. If one goes for a strict Aristotelian account, and maintains that all Definite totalities are finite, then Ω, the property, totality, whatever, of all ordinals will be what the set-theorist calls "ω". The above predicate characterizing $\prec_1$would thus define $\omega + 1$, which, for the strict Aristotelian, is longer than the ordinals››.

[1991, p. 231] wrote, just to point to the contradiction is "to wield the big stick, not to offer an explanation". What all this teaches us is that the indefinite extensibility solution is much more natural, intuitive and follows if we treat each case in the same way without introducing *ad hoc* elements. We may therefore conclude that there is no maximal well-ordering (and therefore no maximal ordinal number) and that the idea of well-orderings without an order-type is not enough to stop the process of finding new entities. The process is indefinitely extensible, which means that whatever well-ordering we can speak of, we can find a longer well-ordering.

2.6 An argument for indefinite extensibility (against the plural approach)

2.6.1 Preliminaries

We are now going to develop an argument whose aim is to provide support to indefinite extensibility, against the plural approach. Of course, since the plural approach is in itself consistent[143], this argument is not a purely logical one, in the sense that it does not point to a logical inconsistency within the plural approach. Rather it is a *philosophical* argument, based on a philosophical premise. In particular, the philosophical premise consists in a metaphysical thesis concerning the nature of mathematics. This premise, together with some well-established logical results concerning logical theories, will give us reasons to prefer the indefinitely extensible thesis in comparison to the plural approach.

First of all, I shall develop the argument in relation to set theory; after that I shall defend the philosophical thesis. Finally, I shall suggest that the argument not only speaks against the plural approach, but also to the so called 'linguistic approach' to indefinite extensibility defended by Williamson [1998] and Uzquiano [2015].

2.6.2 The argument in relation to ZFC-set theory

The argument has two premises:

1) Universal applicability, i.e. the idea that mathematics (in our case, ZFC) strives for generality;
2) Some limitative results, in this case the ZFC's theorem according to which there is no universal set.

Explanation of the premises:

1) This is the philosophical premise, and in our intention, it should describe an inner feature of mathematics (at least a feature of the foundational theories). What this thesis amounts to is simply that mathematics should be applicable to any objects whatsoever, without exceptions. In turn, this means that if we have a plurality of objects with some relations defined over them (I shall call objects with relations between them a 'system'), we should always be able to study them from a

[143] At least we do not have any reason to suppose that it is not consistent.

mathematical perspective. It is important to stress that universal applicability does not correspond to the idea that everything – in the world – has a mathematical nature (we are not defending a sort of Pythagorean view about the structure of reality). Rather the idea acknowledges that the mathematical point of view is only a point of view about reality (it is not the only one), and that there are other points of views that capture aspects of reality that mathematics cannot capture. However, as long as we have different systems of objects, universal applicability implies that we should be able to study them from the mathematical perspective. If universal applicability is true, then every object can be studied by mathematics (even if they may have particular aspects that cannot be captured by mathematics).

Universal applicability implies *self-reference*: if a theory can be applied universally, i.e. it can be applied to everything, then it can also be applied to itself (since it is one of the things that make up everything). Between the objects that mathematics studies there are also mathematical theories.

2) Premise two is just a well-known theorem of ZFC.

The argument runs as follows: according to premise 1, ZFC-set theory must be universally applicable, in the sense that everything should be modeled inside itself. In particular, the same universe of sets described by the theory must be studied. However, in virtue of premise 2, there is no universal set, which implies that ZFC cannot study the universe of sets, because it is a theory about sets, and the universe of sets is not a set. So, to be applicable to its same universe, we must expand ZFC, for instance by means of an axiom that affirms the existence of a great cardinal. In the universe of this new theory, which we shall call ZFC+, the universe of ZFC can be studied, because it is now regarded as a set. To echo Zermelo [1930], what from a certain point of view is an absolute totality, from a different point of view is an ordinary set[144]. Of course, premise 1 can also be applied to ZFC+, with the effect of expanding even its universe. By iterated applications of the argument, we can expand any universe described by any extension of ZFC.

In this picture, the expansion of the universe is driven by universal applicability: we expand the universe because we want to apply set theory to anything whatsoever, and in particular to the same universe of sets. It is clear that the picture we get is exactly the one described by Zermelo [1930].

It is important to notice that this picture is incompatible with a plural approach to set theory. Such an approach asserts that the whole universe of sets is a plurality that does not form a set, *and* it is not expansible. Ultimately, the pluralist approach denies universal applicability, because set theory is never applicable to its whole universe. Of

[144] ‹‹What appears as an ‘ultrafinite non- or super-set’ in one model is, in the succeeding model, a perfectly good, valid set with both a cardinal number and an ordinal type, and is itself a foundational stone for the construction of a new domain››. Zermelo, [1930], p. 1233. Zermelo proposes to extend any (domain or) model of the axioms of set theory to a richer (domain or) model. The result is an indefinitely extensible sequence of stronger and stronger theories with larger and larger models.

course, this does not mean that the plural approach is incompatible with the axioms that state the existence of inaccessible cardinals. In fact, a pluralist can accept anyone of these axioms. However, the pluralist is also committed to the idea that we cannot go on adding these axioms indefinitely. We must stop somewhere. The arbitrariness of such a stop is a point we have already criticized[145].

The counter-objection of the pluralist might be that the whole universe of sets is a plurality, which simply means that what there are, are all and only sets. But plural quantification do not commit ourselves to new objects apart from the members of the plurality. So the whole universe of set is not an object: as a consequence our previous claim that, within a pluralist approach, set theory is not universally applicable (because it is not applicable to its own universe) misses the point, because its own universe is not an object. Therefore, there is nothing to which set theory cannot be applied to.

This conclusion can also be motivated by means of a different strategy. Since for each object there exists its singleton, and set theory is about any of these singletons, then there is no object beyond the scope of set theory. The existence of the universal set is thus not necessary to study every object from a set theoretical point of view.

Of course, both these points are correct: from a pluralist point of view the universe of set is not an object (a plurality does not have an ontological unity), and for every object there exists its singleton. However, something still remains outside the scope of the theory: the totality of the relations between all sets. In other words, what the theory lacks is an unified perspective on its whole subject-matter. Each part of this subject-matter can be studied, but 'all sets' altogether cannot become the object of the theory.

What this amounts to is that the structure that all sets form is not a mathematical (a set theoretic) object. For sure all sets define a structure: in the iterative conception of set that lies behind ZFC set theory, the membership relation defines a well-order on them.

[145] A very similar argument can be run with regard to Category theory. Category theory aims to study general categories of mathematical objects, as the category of groups, of abelian groups, of sets, and so on. Each category is defined by means of some objects (the groups, or the sets, etc.) and some morphisms between them. Depending on the size of the objects that composed a category, we may define a category to be *small* if its objects form a set-size collection, while we define a category to be *non-small* if its objects are too many to form a set-size collection, as clearly happens with the category of all sets. Universal applicability (premise 1) implies that category theory should study every kind of mathematical objects, included categories themselves. Now, there is no problem in considering the category of all small categories. However, on pain of paradox (a re-adaptation of Russell's paradox), this category is non-small: the category of all small-categories is non-small. What about the category of all categories whatsoever? Again, not to fall into a Russell's style paradox, there cannot be a universal category. A pluralist may say that the categories do not form a universal category, and therefore category theory is not universally applicable, because it is not applicable to the whole universe of categories. However, if we want to stick with premise 1, the only move at our disposal is to claim that the notion of category is indefinitely extensible: taken all the *available* (small and non-small) categories, we can consider the category of all of them, which will be a non-small category but it won't be one of them. At this point we can consider all the previous available categories together with this new category to find a further category of all of them. The process can be iterated indefinitely.

Therefore, we clearly recognized a pattern in the set theoretic universe. It is precisely this pattern that cannot be studied by the plural interpretation of ZFC.

On the contrary, on the view we are defending, universal applicability forces us to extend any theory in order to treat its universe (which is a plurality that cannot form a set) as a perfectly standard set in the new expanded theory. The result is that with regard to any theory there will be pluralities too big to form sets, and so something to which set theory cannot be applied; however, this non-applicability is only *relative*: as soon as we consider an extension of that theory, that plurality is just a set and so an object that can be studied by a mathematical point of view.

2.6.3 Defending universal applicability

The argument just sketched is based on universal applicability, which is a controversial metaphysical thesis about mathematics. One could give several examples of thinkers that have considered universally applicability as a inner feature of mathematics. Maybe the most famous is Frege. It is well-known that Frege thought of logic and arithmetic to be universally applicable. In particular, he argued that the truths of arithmetic and logic govern "the widest domain of all" (Frege [1953], §14). The idea is that any kind of things, no matter their nature, can be counted, and therefore, arithmetic can be applied to them. Since for Frege arithmetic was just logic, and logic embedded (naive) set theory[146], also set theory is universally applicable[147]. Similar ideas can be found in Russell's view that logic does not have any specific content. This implies that logic is a formal discipline that can be applied to any object exactly in virtue of the lack of a proper content.

However, my strategy to argue for such a thesis does not consist in bringing different examples of well-known thinkers that were explicitly or implicitly committed to this idea. They all may turn out to have been wrong. My strategy will be far more general, relying on two aspects: the nature of the 'abstract method' of mathematics and the foundational role of set theory. Let us start with the first.

What I am going to argue is that it is the abstract method of mathematics that strives for universal applicability. According to Gowers [2002], p. 18 the abstract method can be summed up in the slogan 'a mathematical object *is* what it *does'*. The idea is that, from a mathematical point of view, it is not important the inner nature of a mathematical objects –let say the inner nature of the number 2- rather what really matters is the *role* an object plays inside a mathematical theory – in our example, the role of the number 2 in the system of natural numbers. No matter what the number 2 is: anything with the same relations with regard to the natural number system can play the role of the number 2. The abstract method arose in the nineteenth century, when the axiomatic

[146] See chapter 1, where we show how to derive the NCP from Frege's system.
[147] Of course, Frege's project was inconsistent, but this does not imply that the idea that logic and mathematics are universally applicable is itself incoherent. A similar view of the universality of mathematics can be found in Bernard Bolzano.

approach was largely adopted in any branch of mathematics (see Linnebo [2017], pp. 154-155). Before that, mathematical theories tended to have particular interpretations: arithmetic was about counting, geometry was about physical space, etc. As soon as the axiomatic approach got spread, mathematicians got less interested in the particular interpretations of their theories, while focusing more on the structural properties of their theories that could be interpreted in several different ways. Generally speaking, the abstract method consists in lying down axioms that define an abstract mathematical space, i.e. a general structure that can have multiple instantiations. The core idea is that the formal axiomatic approach 'abstracts away' from particular instantiations, and in this way it *can be applied to any object whatsoever*. It soon became clear that mathematics was interested in general structures, in common patterns between systems of objects, rather than in the particular systems. It is in this context that categoricity results (i.e. the proofs that all models of a certain formal theory are categorical) gained much of their importance. A first point to notice is that universal applicability becomes the thesis according to which there is no limit to the application of such a method. The reason is simply that, since the method abstracts away from particular features, there seems to be no particular objects that can "resist" it. It seems that, if something is to be recognized as an object, then it can be mathematically studied. Therefore, as it happens in Frege's view, it is the lack of a particular content that allows universal applicability. A second important point is that the abstract method can also be applied to structures as well. When we have a bunch of different structures, we may want to study what they have in common: this can clearly be seen in category theory, where mathematicians tend to be interested in more and more abstract categories. This is of course possible because structures and patterns can be treated as objects. This shows how wide is the range of the abstract method of mathematics: the abstract method allows the study of more and more abstract patterns; not only patterns in common between different systems of objects, but also patterns in common between different patterns.

At this point it should be clear why the abstract method is at odds with the plural approach to set theory. As we said above, we can surely recognize a common pattern defined by all sets. But if pluralism is true, then we cannot study this pattern, contrary to what we should expect from the abstract method. To be more precise, recognizing that the universe of set forms a pattern seems to provide us with all the necessary ingredients to mathematically study that pattern, i.e. we are in a position to lie down some axioms that defines that pattern as an object of the theory. In our present case, we can simply extend ZFC by means of a new axiom in order to study that pattern from a set theoretic point of view. What it is here fundamental to notice is that this practice is fully legitimated by the abstract method.

In any case, there is also a problem with the foundational aspirations of set theory. Historically, set theory was thought as a theory where all kinds of mathematical objects could be interpreted. If a certain pattern is possible, i.e. it is consistent, then it should find a place inside the set theoretic universe, but this is not the case with the pattern of the whole universe of sets in the plural approach to ZFC. Since the non-existence of the universal set shows that no theory of (well-founded) sets can comprehend all patterns, the foundational aspirations of set theory force us to indefinitely extend all set-theories so as to be sure that each pattern can be studied as a proper object in a certain extension of ZFC[148].

It is therefore the abstract method, with the possibility of studying more and more abstract structures, that justifies universal applicability. Every time we recognize a pattern, we can treat it as an object of a mathematical theory and so study it from a mathematical perspective. Moreover, the foundational aspirations of set theory give us further reasons to believe in universal applicability[149].

2.6.4 General conclusive remarks

If our argument is correct, then indefinite extensibility is a result of the inner nature of mathematics, steaming from universal applicability. It should be clear why this is an argument for indefinite extensibility, and against the plural approach. Boolos's plural approach to set theory makes set theory a theory which is not universally applicable. What it is important to notice is that the argument that we have just exposed is, at the same time, an argument against the Williamson-Uzquiano's interpretation of indefinite extensibility. According to that interpretation, indefinite extensibility is a linguistic phenomenon that consists in further and further reinterpretations of the set-theoretic vocabulary. The idea is that we can indefinitely reinterpret predicates as 'being a set', 'being an ordinal', etc.; however, all these interpretations are made inside an all-inclusive plurality (the plurality of everything). In this setting, the reinterpretation of such notions allows us to look at what (before the interpretation) were considered to be an *urelement* as a set (according to the new interpretation). This notion of indefinite extensibility is compatible with the failure of universal applicability for set theory. In fact, set theory is not universally applicable because it can never be applied to the all-inclusive plurality. If one regards, as we regard, universal applicability as a fundamental principle of mathematics, one cannot accept the Williamson-Uzquiano's interpretation of indefinite extensibility.

3. Williamson's predicativist interpretation of Higher-Order Logic

PFO-logic is a way of interpreting SOL in terms that remain – for some aspects – first-orderist. In particular PFO-quantification is (plural) quantification into name position (plural expressions as 'the dogs' are noun phrases), as FO-quantification is (singular) quantification into name position. On the contrary, SO-quantification is quantification into predicate position. As a consequence, a more natural interpretation of SOL, and HOL

[148] A somehow related line of thought has been developed by Hazen [1994].
[149] It is important not to confuse the abstract method with structuralism as a philosophical view concerning mathematics. It is true that structuralism gains its credibility from this method; however, structuralism is not only a method, but the thesis that the whole mathematics is the study of structures (see Shapiro [1997] and Linnebo [2008, 2017]).

in general, would directly make appeal to predicates and what they express: concepts. In this line, we find Williamson's predicativist interpretation of HOL.

3.1 Williamson's proposal

Williamson's starting point is his reformulation of Russell's paradox for interpretations (see chapter 5, §1.5 above). GS-μ was the comprehension principle for interpretations that allowed the existence of the interpretation – say a - which applies to *all* and only interpretations that do not apply to themselves. One of the key point in the derivation of the paradox at 1.5 is the passage from 3 to 4: the FO-universal quantifier 'for all' present in the definition of a is instantiated by the same interpretation a. This is possible only because we are treating interpretations, and in particular a, as first-order objects. We already know that dealing with interpretations of predicates as objects means treating the semantic values of them as sets; in this setting, generalization over the interpretations of predicates is just quantification over sets (FO-quantification), i.e. quantification into name position. But predicates are not noun phrases (at least from a *grammatical* point of view). It is certainly more natural to consider generalization over predicates' interpretations as quantification into predicate position, rather than quantification into name position. Following this path, the consequence is that interpretations of predicates cannot be treated as FO-objects. If so, the interpretation a in the derivation of the paradox above cannot be one of the values over which the FO-quantifier in 3 ranges, because it is not a FO-object at all. The paradox is thus blocked by disallowing the passage from 3 to 4.

Williamson's proposal consists in taking quantification into predicate position (SO-quantification) as irreducible to quantification into name position (FO-quantification). This means that the semantics of a FO-language must be developed in a SO-language, and more generally, the semantics of a *nth*-order language must be developed in a *nth+1* order language. The reason why semantics requires going higher-order is simply that if we give the semantics of a SO-language in a FO-language, then we are back with the paradox. In this way, we obtain an hierarchy of languages, each one irreducible to each other.

Interpretations cannot be considered as if they were higher-order objects. If they were special kind of objects, then there could not be any totally first-order unrestricted quantification, because there cannot be a quantification over all the orders of the hierarchy. Interpretations are not objects at all and speaking of higher-order concepts is just misleading: higher-order quantification does not bring with itself any *ontological* commitment (see also chapter 7, §2). In this way we can have AG-∀. The FO-quantifiers ranges over everything, and we are safe from paradox because interpretations are not objects of any kind[150].

[150] This is a contentious issue. Linnebo & Rayo [2012] and Kramer [2016] give reasons to challenge this view. In particular, Kramer stressed the fact that allowing – as Williamson allows – higher-order versions of the identity predicate is a strong clue that we are treating higher-order concepts as objects of some sort.

3.1.1 Going formal

Williamson's [2013] chapter 5 uses Gallin's type theory to generalize FO-logic to HOL. For this reason we shall call this kind of approach a 'type-theoretic' approach to absolute generality or, alternatively, high-orderism.

3.1.1.1 The type hierarchy

Base clause: e (type of terms for individual).

Induction clause: for any types $t_1, \dots, t_n$, there is the derived type $< t_1, \dots, t_n >$ of terms for relations between things of type $t_1, \dots, t_n$.

Exclusion clause: there are no other types.

Relations: a relation R between objects of type $t_1, \dots, t_n$ will be of type $< t_1, \dots, t_n >$.

Propositions are zero-place relations, and so their type is $<>$. A property of properties will be of type $<< p >>$.

3.1.1.2 The language of ML_p[151]

The language ML_p contains the following symbols:

1. *Constants* $\ulcorner c_i^t \urcorner$ $(i \geq 1)$, for each type t.
2. Variables $\ulcorner x_i^t \urcorner$ $(i \geq 1)$ for each type t.
3. The only typed logical constant symbol, i.e. the identity predicate $\ulcorner = \urcorner$ of type $< e, e >$;
4. Atomics: if $\ulcorner P, p_1, \dots, p_n \urcorner$ are of types $< p_1, \dots, p_n >, p_1, \dots, p_n$ respectively, then $\ulcorner P(p_1, \dots, p_n) \urcorner$ is an atomic formula (of type $<>$).
5. Negation: if A is a formula, then $\ulcorner \sim A \urcorner$ is a formula.
6. Conjunction: if A and B are formulas, then $\ulcorner A \wedge B \urcorner$ is a formula.
7. Generalization: if A is a formula and v is a variable of any type, then $\ulcorner \forall v A \urcorner$ is a formula.
8. Exclusion: there are no other typed expressions.

N.B.: 3. & 4. are the base case for the recursive definition of well-formed formulas.

3.1.1.3 Higher-Order Semantics

The basic idea of the higher-order semantics is nicely summarized by Williamson [2013] p. 236:

If this is the case, then it is not true anymore that the first-order quantifiers range over everything. Therefore, it is an essential part of Williamson's approach not to treat interpretations as higher-order objects.

[151] This exposition is based on Williamson [2013], chapter 5, where Williamson presents a typed modal language. We omit the modal clauses since we are here interested just in the technical developments of HOL and not in the modal part. Williamson's solution of the problem concerning absolute generality does not rely on modal notion at all.

A more faithful semantics [than the set theoretic one] should be formulated in a higher-order meta-language, with unrestricted first-order quantifiers and higher-order quantifiers irreducible to first-order quantifiers over sets.

This allows the higher-order quantifiers to range over the property of *self-identity,* despite the lack of the universal set. Moreover, the first-order quantifiers can range over everything (every first-order object). Here Williamson defines a type theory both for the object and the meta-language:

Object-languages types:

Base clause$_o$: e (type of terms for individual).

Induction clause$_o$: symbols of type $< t_1, \dots, t_n >$ applies to symbols of type $t_1, \dots, t_n$ respectively to form sentences.

Exclusion clause$_o$: there are no other types.

Meta-linguistic types:

Base clause$_m$: e (type of terms for individual).

Induction clause$_m$: for any natural number *n,* whenever the meta-language has symbols of type $t_1, \dots, t_n$, it also has symbols of types $< t_1, \dots, t_n >$.

Correspondence: each type t of the ML$_p$ corresponds to a type τt of the meta-language by the rule that τe is e and $\tau < t_1, \dots, t_n >$ is $< \tau t_1, \dots, \tau t_n >$.

The finite types of the meta-language are those that belong to the smallest set that contains e and whenever it contains $t_1, \dots, t_n$, it also contains the type $< t_1, \dots, t_n >$. At this point Williamson adds an *infinite type* λ to the meta-language: the expression of type λ are exactly those of any finite type. Thus, expressions of type λ also belong to some more specific type, while expression of type $< \lambda >$ do not[152]. Then we have the following definitions:

ASSIGN: let ASSIGN($a^{\langle e,\lambda\rangle}$) be the conjunction of the denumerably many conditions of this form for all symbols *s* of any type t in ML$_p$.

$$\exists x^{\tau t} \forall y^{\tau t} [a^{\langle e,\lambda\rangle}(\ulcorner s \urcorner^{e}, y^{\tau t}) \leftrightarrow (x^{\tau t} = y^{\tau t})]$$

VARIANT: for any symbol *s*, VARIANT($a^{\langle e,\lambda\rangle}, b^{\langle e,\lambda\rangle}, \ulcorner s \urcorner^{e}$) is the infinite conjunction of ASSIGN($b^{\langle e,\lambda\rangle}$) and conditions of this form for variables *y* of each specific subtype of λ:

$$\forall x^{e}((x^{e} \neq \ulcorner s \urcorner^{e}) \rightarrow \forall y^{\lambda}[a^{\langle e,\lambda\rangle}(x^{e}, y^{\lambda}) \leftrightarrow b^{\langle e,\lambda\rangle}(x^{e}, y^{\lambda})])$$

ASSIGN is the higher-order analogue of the first-order claim that a model assignment pair gives each variable a unique value of the appropriate type, and VARIANT is the

[152] λ-type corresponds to Rayo ω-type. Therefore, it is the smallest transfinite type.

higher-order analogue of the predicate *being a variant of an assignment with respect to a variable.*

At this point Williamson develops a higher-order theory of truth: for the symbols $S, s_1, \dots, s_n$, of $\mathrm{ML_p}$, let us introduce the predicate 'TRUE' for $\mathrm{ML_p}$:

Identity: $\mathrm{TRUE}(\ulcorner s_1 = s_2 \urcorner^e, a^{\langle e,\lambda\rangle}) \leftrightarrow \forall x^e \forall y^e[(a^{\langle e,\lambda\rangle}(\ulcorner s_1 \urcorner^e, x^e) \wedge a^{\langle e,\lambda\rangle}(\ulcorner s_2 \urcorner^e, y^e) \rightarrow (x^e = y^e)]$

Atomics: $\mathrm{TRUE}\big(\ulcorner S, s_1, \dots, s_n \urcorner^e, a^{\langle e,\lambda\rangle}\big) \leftrightarrow \forall X^{\langle \tau t_1,\dots,\tau t_n\rangle} \forall x_1^{\tau t_1} \dots \forall x_n^{\tau t_n}[\Big(a^{\langle e,\lambda\rangle}\big(\ulcorner S \urcorner^e, X^{\langle \tau t_1,\dots,\tau t_n\rangle}\big) \wedge a^{\langle e,\lambda\rangle}\big(\ulcorner s_1 \urcorner^e, x_1^{\tau t_1}\big) \wedge \dots \wedge a^{\langle e,\lambda\rangle}\big(\ulcorner s_n \urcorner^e, x_n^{\tau t_n}\big)\Big) \rightarrow X^{\langle \tau t_1,\dots,\tau t_n\rangle}\big(x_1^{\tau t_1} \dots x_n^{\tau t_n}\big)]$

Negation: $\mathrm{TRUE}(\ulcorner \sim A \urcorner^e, a^{\langle e,\lambda\rangle}) \leftrightarrow \sim\mathrm{TRUE}(\ulcorner A \urcorner^e, a^{\langle e,\lambda\rangle})$

Conjunction: $\mathrm{TRUE}(\ulcorner A \wedge B \urcorner^e, a^{\langle e,\lambda\rangle}) \leftrightarrow \mathrm{TRUE}\ (\ulcorner A \urcorner^e, a^{\langle e,\lambda\rangle}) \wedge \mathrm{TRUE}(\ulcorner B \urcorner^e, a^{\langle e,\lambda\rangle})$

Quantification: $\mathrm{TRUE}(\ulcorner \forall v A \urcorner^e, a^{\langle e,\lambda\rangle}) \leftrightarrow \forall\, b^{\langle e,\lambda\rangle}[\mathrm{VARIANT}(a^{\langle e,\lambda\rangle}, b^{\langle e,\lambda\rangle}, \ulcorner v \urcorner^e) \rightarrow \mathrm{TRUE}(\ulcorner A \urcorner^e, b^{\langle e,\lambda\rangle})]$.

Williamson concludes the passage with the following important remark:

> The main problem is philosophical, not technical. Informally, how are we to understand the higher-order quantifiers? Giving them a formal semantics in a still higher-order metalanguage does not answer that question, for how are we to understand its higher-order quantifiers in the metalanguage? (p. 239)

3.1.2 Interpreting higher-order quantification: the importance of the instantiation relation

We already know that HO-quantification must be taken as irreducible to FO-quantification. In other words, HO-quantification must be considered as *primitive*. This is not surprising, if we consider the fact that HOL gives us more expressive power than FOL:

> If introducing higher-order quantifiers of a given sort constitutes a genuine advance in expressive power, we cannot expect to explain them before introducing them. What we can hope to do is to account for them in retrospect, by using them to rewrite the inaccurate first-order explanation in more accurate higher-order terms [...] (p. 258).

Which are the arguments Williamson gives us to defend this irreducibility? First of all, there is the paradox. Going higher-order allows to block the paradox, while remaining first-order we face it. However, this could not be the only reason for high-orderism, otherwise it would be completely *ad hoc.* Williamson's strategy is thus wider, motivating the adoption of high-orderism by means of a bunch of different reasons. Not only is the paradox a good reason to go higher-order, but the expressive power one obtains by this step is worth taking it: ‹‹Metaphysically universal generalizations of logic are the structural core of metaphysics. We need the *best logic* we can get›› (emphasis added, p. 226). Logic is needed for different purposes, one of this is metaphysics. But metaphysics requires a strong logic, i.e. a logic with great expressive power, and HOL gives us the expressive power we need for this research program. Moreover, in his essay *Everything* Williamson provides a strong case in favor of absolutism, arguing that relativism is not coherently expressible (see chapter 3, §2). Since HOL gives us the means to be absolutist, this is a further reason in favor of it.

These are general reasons in favor of HOL, but only the one based on the paradox seems to provide a direct reason to claim that HOL is irreducible, and thus primitive, with regard to FOL. However, we met before a further reason Williamson gives to motivate this thesis: FO-quantification is quantification into name position, while HO-quantification is quantification into predicate position. This appears to be the only reason, independently from paradox, that suggests that we are dealing with two different kinds of quantification. Nouns refer to (first-order) objects, predicates do not refer to anything, rather they express meanings, concepts. For this reason Williamson's approach has been dubbed 'conceptualism' (for instance by Linnebo [2006]: this is not Williamson's terminology)[153].

With the latter terminology, we may say that quantification into predicate position is irreducible to quantification into name position, because concepts are irreducible to objects. The paradox of the notion of interpretation may be interpreted as an indirect proof of the fact that the semantic values of predicates cannot be objects as sets. Conceptualism requires us to abandon the nowadays standard set theoretic semantics, which is based on the notion of set or (which is in fact the same) on the ∈-predicate. If the aim of explaining the semantic of a sentence or of a language is to explain how the meaning of a complex sentence is the function of the meaning of its parts (at least this seems to be true for formal languages), then working within standard semantics, this explanation succeeds only if we understand the notion of set (or the meaning of the ∈-predicate). The same is true if the semantics is given by means of plural logic: we must previously understand the meaning of the logical predicate '$\prec$' (…is one of…). Conceptualism thus presupposes that we previously understand the instantiation relation 'η': $\eta(a, P) =_{def} P(a)$. More specifically, if a is an individual constant, P is a predicate, and $I(a) = d$ (d is the object of the domain of the language the name 'a' refers to according to the interpretational function I); $Ext(P)$, the extension of P, is the set of objects falling under P, and pp is a plural constant for the objects pps, then the sentence $P(a)$ is interpreted in the following way:

Set theoretic semantics: $P(a)$ is true if, and only if $d \in Ext(P)$

[153] From now on my interpretation will go beyond what Williamson effectively says about his view, and I shall present an interpretation which primarily aim is not to give a piece of exegesis of Williamsons' texts, rather it wants to understand where his view naturally leads. I do not bother whether the resulting interpretation fits with what Williamson's really thinks about his approach. The reader can evaluate by herself if my interpretation fits with the spirit of his view or not.

Plural semantics: $P(a)$ is true if, and only if $d \prec pp$

Conceptualist semantics: $P(a)$ is true if, and only if $P(d)$.

As one should expect, Conceptualism does not reduce the predicaticament relation to a different relation. This can be seen in the atomic clause of Williamson's theory of truth above (we report here the clause):

Atomics: $\mathrm{TRUE}(\ulcorner S, s_1, \dots, s_n \urcorner^e, a^{\langle e,\lambda\rangle}) \leftrightarrow \forall X^{\langle \tau t_1,\dots,\tau t_n\rangle} \forall x_1^{\tau t_1} \dots \forall x_n^{\tau t_n} [\left(a^{\langle e,\lambda\rangle}(\ulcorner S \urcorner^e, X^{\langle \tau t_1,\dots,\tau t_n\rangle}) \wedge a^{\langle e,\lambda\rangle}(\ulcorner s_1 \urcorner^e, x_1^{\tau t_1}) \wedge \dots \wedge a^{\langle e,\lambda\rangle}(\ulcorner s_n \urcorner^e, x_n^{\tau t_n})\right) \rightarrow \boldsymbol{X^{\langle \tau t_1,\dots,\tau t_n\rangle}(x_1^{\tau t_1} \dots x_n^{\tau t_n})}]$

Here I stressed the last part of the clause because it exactly shows that where the set theoretic semantics uses the $\in$-predicate and the plural semantics uses the $\prec$-predicate, the conceptualist semantics takes as primitive the *instantiation relation* between an object and a concept.

The first point to notice is that, no matter what kind of semantics one is affectionate on, in any case one has to take a certain notion as primitive. We explain something by means of something else with the consequence that if nothing could be considered as primitive (i.e. not explainable) then we find ourselves in a infinite regress. The problem is to understand which – between these three notions – should be taken as primitive. Is conceptualism, which takes the instantiation relation as primitive, plausible? I think it is. First of all, notice that the conceivability of set-sized collections and non-set-sized collections (pluralities too big to form a set) shows that the $\prec$-predicate is more general than the $\in$-predicate (if $a \in B$, then $a \prec bb$, but not vice versa). In the same way, the conceivability of indefinitely extensible concepts defined by means of plurals (as we defined them in chapter 2) shows that the instantiation predicate 'η' is more general than the $\prec$-relation. From an epistemological point of view, it is enough that indefinitely extensible concepts are conceivable; in other words, even if one believes that there are no indefinitely extensible concepts, if one believes that the same idea of indefinite extensibility is meaningful, then one must acknowledge that we understand the η-predicate, without recurring to the $\prec$-predicate, because in this example the latter does not apply.

However, as we underlined in chapter 2, the rival of indefinite extensibility usually challenges the coherence of the same notion, so she would probably deny that indefinite extensibility is a meaningful notion. Therefore, one can consider the general defense of indefinite extensibility as providing an argument to claim that the instantiation relation is more fundamental than the $\prec$-relation.

3.2 Problems for ideological hierarchies

In the last paragraph, we defended the legitimacy of considering the instantiation relation as primitive, and therefore irreducible to a more basic relation. In this regard, we completely agree with Williamson's view: in particular, we agree that the solution to the problem of absolute generality must be looked for in the irreducibility of concepts to the objects that they instantiated. However, the technical way in which Williamson develops his defense of AG-∀ is unsatisfactory. Here I shall raise four different concerns regarding this hierarchical proposal.

First of all, the type theoretic approach delivers us a very complicated and counter-intuitive picture of the logical structure of language. In fact, we must take the type-theoretic approach very seriously if we want to use it as a solution of the absolute generality problem, where 'very seriously' means that we are committed to the idea that type theory gives us the logical structure of language. From a certain point of view, type-theory regiments the (intuitive) difference between a noun, a predicate, a predicate of predicate, and so on, but by doing so it declares nominalization completely mistaken; more specifically, nominalization turns out to be only a grammatical feature of language, not a logical feature. This approach thus implies a strict distinction between grammar and logic, which is a view that the fathers of logic Frege and Russell took, but that many authors after them refused to take. The main problem is that there seems to be no clear method that tells us what elements belong to logic and what elements belong to grammar. It is not clear how to sharply distinguish the two.

Secondly, the type-theoretic approach multiplies without end (to infinity) the irreducibility of concepts to objects. In fact, not only concepts are irreducible to objects, but also concepts of concepts are irreducible to concepts, and concepts of concepts of concepts are irreducible to concepts of concepts. In general, *nth+1*-order concepts are irreducible to *nth*-order concepts. The point is that at each level with the term 'irreducibility' we mean the same thing: second-order concepts are irreducible to first-order objects *in the same sense* in which m+1-order concepts are irreducible to m-order concepts. This suggests that the irreducibility is a unique phenomenon that can be explained in a unique way and that the type-theoretic approach overcomplicates without necessity the picture by multiplying the same phenomenon[154].

Thirdly, type theory is too restricted because it rules out natural language sentences that are circular, but not paradoxical. Consider the sentence "All sentences on p.132 of this Dissertation are true". This is a sentence of English with a clear meaning, and, on the supposition that there is at least one false sentence on p. 132, the sentence is false. But the hierarchical approach implies that such a sentence is not well-formed[155]. What does such objection show? At a first sight, it simply shows that English is not hierarchical. But what about the claim that the hierarchical approach constitutes the logical structure of English? That English is not hierarchical can also be seen by the fact the it allows nominalization of predicates, but – by itself - this does not exclude that the type

[154] In the chapter about the theory of concepts I propose to identify this same phenomenon with the irreducibility of role, in a propositional function, between the argument and the predicate. This is – I think – the root of the irreducibility.
[155] See Priest [2006] p. 19 and Kripke [1975].

hierarchy constitutes its inner logical structure. However, this position is untenable. A necessary condition for a theory to be regarded as the logical structure of a language is that the theory does not change the truth-value of the sentences of a language; more specifically, in the case in question there are some sentences of English that turns out true or false with regards to the contingent circumstances of the world. Of course, this must be rendered possible by the logical structure of English: in other words, its logical structure must be compatible with this phenomenon. A theory that presents itself as capturing the logical structure of English should be compatible with it: but the type theoretic approach is not compatible with such a phenomenon, and for this reason it cannot be regarded as capturing the inner structure of English.

Fourth, there is a well-known objection against ideological hierarchy that it is now time to deal with. This objection underlines an expressive weakness of such an approach. I think that this objection alone is enough to motivate us to look for a different way of capturing the irreducibility of concepts to objects. Roughly speaking, the objection asserts that the theory of type cannot be stated (Fitch [1946, 1964]), because to state the theory we should use sentences that generalize over all types, but there is no universal type from which we can carry on these generalizations. For instance, while stating the theory, we may want to claim things like that:

1) It is not possible to make claim over all levels of the hierarchy of types;
2) For all types *n*, there is a further type *n+1*;
3) For all types *n*, a class of type *n* has only members of types *n-1* (if the theory is non-cumulative);
4) For all types *n,* a class of type *n* has elements of whatever type up to *n-1* (if the theory is cumulative);
5) For all propositional functions *f,* a propositional function that involves *f* cannot be an argument for *f*.

The problem with such sentences is not only that they are perfectly meaningful sentences that are declared not well-formed by the theory of types, rather the problem consists in the fact that one must assert them in order to state and explain the theory. But if the theory of types is correct, then these sentences are not well-formed, and so they cannot express any proposition, which means that they are certainly not true. Commenting on hierarchical approaches in general, Priest [2006], p. 20 writes:

> A final irony is that, even to explain what the hierarchy is, we must assert (among many other things) the existence, for each index *i* [in our case: for each type], of a truth predicate T_i, which is just what cannot be done on the hierarchy view. Hence any theory to the effect that the hierarchy is English is self-refuting (or inconsistent).

One must be very careful in declaring the theory of type inconsistent because of this problem. In fact, a theory is inconsistent if a contradiction can be derived from it. But this is not the case of the theory of type: for what I know, no contradiction has been derived (or can be derived) from it. Of course, if one of the sentences above is derivable from the theory, then the theory is inconsistent; however, all the sentences above belong to the meta-language in which we speak of the theory, and not to the object language of the theory (the reason why those sentences are not derivable form the theory is that they are built by means of quantifiers over all types, and there is no quantifier which ranges over all types in the object language of the theory of types). Thus, the objection does not say that the theory is inconsistent.

Here another way of seeing that the theory is consistent: suppose the theory holds just for some languages (or some theories), and not for all. Then, one could argue both that sentences 1-5 are true sentences about the theory of types, and that they belong to a language where the types' restriction does not hold. In this way, sentences 1-5 are well-formed simply because the theory of type does not apply to their formation rules. If the types' restriction regards only some restricted languages, then the objection above cannot take off the ground.

Unlucky this is not the case with the use of a hierarchy of languages in the absolute generality's debate. We have seen that to propose a type theoretic approach to allow AG-∀ implies taking the theory very seriously. In this context, 'very seriously' means that the theory of type must be regarded as universally applicable[156]. The reason is straightforward: if the theory were not universally applicable, then we could just consider a non-typed language strong enough to speak of everything. Then, for that language, we could derive the problems connected with unrestricted quantification seen above. So, to propose the theory as a solution of the problem of absolute generality implies that all theories whose expressive power is strong enough to allow unrestricted quantification must be typed. In particular, one cannot allow that the meta-language in which we speak of the theory of types is not typed, otherwise unrestricted quantification would give rise to paradox in that same meta-language. The type theoretic response to absolute generality falls under the objection just raised[157].

Fitch [1946], p. 71 considers the following counter-objection of the defender of the theory of types:

> One way of attempting to meet this objection to the ramified or simplified theory of types is to assert that a formulation of a theory of types is simply the formulation of a certain more or less arbitrary stipulations about the permitted ways of combining symbols.

This does not seem a viable path to take. In fact, Fitch replies:

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[156] Fitch [1946], p. 71: ‹‹Furthermore, such a ramified theory of types could not even be stated. Its sweeping restrictions against self-reference would apply to every theory, including itself, and so it would be self-referential in violation of its own edicts. A similar criticism could be made even against the more moderate simplified theory of types, if regarded as universally applicable››.

[157] One way of avoiding the problem is introducing a limit transfinite type as Rayo [2006] and Williamson [2013] do. This allows to speak of all finite types simultaneously. But then there are also infinite types, and so generalization over all (finite and infinite) types is impossible. On the legitimacy of introducing transfinite type see Linnebo & Rayo [2012, 2014]. On a critic against this introduction see Studd (forthcoming).

> This answer seems to be all right so long as one is restricting oneself to the realm of uninterpreted symbols, but as soon as one enters to the realm of semantical concepts it becomes necessary to apply distinction of "types" to *meanings* of symbols, as well as to symbols themselves, and self-reference reappears.

That the defender of a type-theoretic approach to absolute generality cannot be happy with the first, merely syntactic interpretation is shown by the fact that he is committing to the idea that the theory reveals the authentic logical structure of language. For him the theory is not only a way of labeling syntactic symbols, rather it is the logic of language, and so it must also deal with semantics (which is a fundamental part of logic). It is exactly because the meanings of words are typed, that the objection applies.

This last point, with his focus on the semantic meanings, may help us to settle the dispute between Linnebo [2006] and Kramer [2013]. Linnebo [2006] advances against Williamson's approach the same objection we have raised here, whilst Kramer is an attempt to reply to Linnebo (and therefore, indirectly, it is an attempt to reply to this paragraph). It may be helpful stating the way in which Linnebo develops the objection, which makes directly use of semantic notions. In particular, Linnebo argues that type-theorists are committed to the following claims, even though their theory prevents them to express those same claims:

Infinity: there are infinitely many types of semantic values;
Unique existence: every expression of every syntactic category has a unique semantic value, not only within a particular type, but across all types;
Compositionality: the semantic value of a complex expression is the function of the semantics values of its constituents.

As Linnebo notices, these are generalizations over all levels of the hierarchy, because what the sentences express should be true in all levels. That there are infinitely many types of semantic values is the corner stone of the theory; that every expression of every syntactic category has a unique semantic value must be true for all levels; the same for compositionality: the principle is valid for all levels of the hierarchy.

Kramer notices that if the three sentences above manage to express what they want to express, then the name and the predicates present in them must have, in each language of the hierarchy, the same semantic value, and therefore, they must refer to the same non-linguistic entity. With his words: ‹‹[Linnebo's] objection relies on the assumption that there is a general notion of the semantic contribution of an expression that picks out an extra-linguistic correlate of that expression››. Linnebo's objection would be thus based on the idea that in each level of the hierarchy there is a unique way in which a particular semantic value contributes to the meaning of the sentence in which it is embedded. Kraemer then goes on: ‹‹I have then tried to make a case that this assumption, and thus Linnebo's objection, can plausibly be resisted by higher-orderists. Specifically, I have suggested that to speak of the extra-linguistic correlates of expressions, higher-orderists may use a hierarchy of predicates of different orders, mirroring the hierarchy of syntactic categories in the object language››. The idea seems thus to be that there is not a unique notion of semantic contribution and, as a consequence, the three sentences above must be read as schematic sentences, which employ predicates and nouns of different orders to refer to their referents. Ultimately, the generality on all levels must be expressed by schemas.

The idea of using "a hierarchy of predicates of different orders, mirroring the hierarchy of syntactic categories in the object language" does not simply mean that the meta-language in which we speak of the semantic values must be typed: this would not reply to Linnebo's point. The reply is neither that semantic values are typed, which is something a type theorist is committed to. The reply is that the same notion of 'semantic contribution' must be typed. Linnebo's objection presupposes that there is a unique notion of semantic contribution across all types: for example, given two predicates M and N of types *m* and *n* respectively, they will express two concepts CON(M) and CON(N) of type *m* and *n* respectively. The fact that also the meanings of symbols are typed means that, not only the predicates M and N are typed, but also the concepts they express. Since they belong to different types, also the concepts they express belong to different types, and consequently they are different concepts. However, despite the difference in types, the semantic contribution of predicates is always the same across all types. So M and N have the same semantic contribution: both of them express concepts. The point is that the higher-orderist should claim – according to Kramer – that the same notion of semantic contribution is typed. So Linnebo's claims above would be just schemas that must be interpreted (by assigning the right types) in order to obtain sentences which express propositions with determined truth values. Kramer's is here appealing to the doctrine of typical ambiguity. However, as I explain below, typical ambiguity does not work in these cases. As I am going to argue in the next paragraph, with ideological hierarchy, typical ambiguity can be understood only in the sense that a typical ambiguous sentence is nothing more than a meaningless string of symbols, that must be interpreted in order to express a meaningful sentence. If generalization over all types are typical ambiguous, then there cannot be any meaningful generalization over all types: once interpreted, they give rise to a proposition of a certain type. The same for the notion of semantics contribution. If this is an ambiguous notion, there cannot be any meaningful sentence about all the different interpretations of 'semantic contribution'.

The point can be seen from a slightly different perspective. Since also meanings must be assigned a type, so with the meaning of the term 'semantic contribution'. But then the sentence 'The semantic contribution of a predicate of a type *n* is a concept of type *n*' is ambiguous, because we do not have specified the type of the term 'semantic contribution'. Suppose the type is *m*: only now we have a sentence that expresses a proposition: 'The [semantic contribution]$_m$ of a predicate of a type *n* is a concept of type *n*'. But this sentence can refer (at most, if the theory is cumulative) to all types inferior to type *m* in the hierarchy of types. Therefore, we cannot have a general sentence about the semantic contributions of different parts of sentences that generalize across all type. But we need such sentences to state the theory and to state how semantic contribution in all types works. For this reason Kramer's reply does not work.

3.3 Typical ambiguity: ideological vs ontological hierarchies

I have dealt with the doctrine of typical ambiguity in the chapter about schematism. I refer the reader to that chapter for more details on this doctrine. Here I will only motivate the assertion made in the previous paragraph: that in the case of ideological hierarchies (as the type theoretic ones) the theory must be interpreted in what I called – in the chapter about schemas – the radical way of understanding schemas. There I explained that there are two ways of interpreting schemas: the first and more radical way, which goes back to Whitehead, interprets schemas as meaningless strings of symbols that must be interpreted to give rise to sentences that express true or false propositions. I argued there that this radical way makes schemas unsuitable for the relativist's aims of expressing valid logical laws.

However, there is a less radical way that interprets a schema as something with a unique meaning (the schema $\alpha = \alpha$ would express the concept of self-identity), but requires interpreting the meta-variables to obtain true or false propositions. A schema has a meaning, but does not express a proposition. We saw that this way of interpreting schemas is the most spread in the contemporary literature on absolute generality, and it is not so easy to dismiss it.

The point I want to make is that this second, less radical, interpretation of schemas (i.e. typical ambiguous sentence) is not available with the theory of types and, more generally, with ideological hierarchies. The reason is simply that not only the syntactic symbols, but also the meanings of expressions must be assigned a particular typed, and therefore schemas as $\alpha = \alpha$ cannot express a unique meaning; rather such an expression is assigned different meanings with different types. The second interpretation is possible for those who use schemas with ontological hierarchies, i.e. people like Glanzberg or Lavine that argues for the non-existence of an absolute domain of quantification. They need schemas not because their language is typed (which in fact it is not), but because they believe there cannot be any unrestricted quantification over everything. Therefore, they can say that a schema has always the same meaning whatever domain we consider: no type restriction applies to them.

3.4 Nominalization

We have provided four reasons against a type theoretic approach to absolute generality. However, we also underlined some positive feature of Williamson's account that are worth being preserved. This would be the topic of the seventh chapter, where we are going to develop a theory of concepts that preserves the core idea of the irreducibility of concepts to objects, but avoiding the problem of taking the theory of types too seriously (we will use though some type theory, but in a not "too serious" way). Here it is time to reflect on the impossibility of considering the theory of types as expressing the logical features of languages. The direct consequence of this consists in a rehabilitation of nominalization.

Nominalization is a process in which a predicate or an adjective is transformed into a name. In the sentence "the rose smells good", 'x smells' is the predicate. We can now refer to the content of the predicate (the concept expressed by the predicate), for instance by saying "Smelling is a property that usually flowers have". The use of the gerund is a particular way in which we can nominalize a predicate. I will call 'property' the referent of a nominalized predicate. Of course, if we are interested in grammar or in logic, we may want to refer not only to the content of a predicate, but to the predicate itself, as when I utter "the predicate 'x smell' is a monadic predicate". The subject – 'the predicate 'x smell'' – is a name, so 'the predicate x' is another way of nominalizing predicates.

The reason why we nominalize predicates is clear: we want to make assertions not only about objects in the world, but also about predicates and concepts. It's the self-reference of language that requires nominalization to give the predicate a grammatical form when it is not used as a predicate.

The result of the last paragraph is a clear evidence in favor of the view that nominalization is something more than a superficial grammatical fact, being something we should regard as belonging to the logical structure of a language. Through nominalization we can make everything a variable of a first-order quantifier. If one abandons the idea of an ideological hierarchy of more and more expressive languages, then nominalization must be treated as a central logical phenomenon. It will in fact play a central role in our theory of concepts.

3.5 Difference between ideological hierarchy and ontological hierarchy.

There is a last remark to do concerning the difference between ideological and ontological hierarchies. One may think that the objection just raised against the type theoretic approach has a straightforward analogous in the case of ontological hierarchies. As it is not possible to quantify over all types, so it is not possible to quantify over all extensions of an indefinitely extensible sequence. A defender of the indefinite extensibility of a concept *C* would like to claim that for each extension of objects falling under *C* there is a more comprehensive extension of objects falling under *C*. But this is a universally FO-quantified sentence about all extensions and, in standard semantics, it requires a set to act as universe of discourse. But if *C* is indefinitely extensible no such a set is available. The defender of ontological hierarchies is in the same position of the defender of the ideological hierarchy, or so it seems.

However, the similarity is only apparent. The theory of types as a solution of absolute generality implies that meanings must be taken as *typed*: a predicate that seems to apply to all types is only an ambiguous expression that expresses different concepts with regard to different types. There can be no concept that applies to the whole hierarchy and that allows to generalize over it. The reason is simply that there is no universal language that can act as a meta-language for the whole hierarchy; in particular no logical device in any language can be about all the languages. If something is a language, then it has a specific position in the types' hierarchy. If the theory of types is taken seriously (as the problem of absolute generality requires), then this situation is inescapable.

An ontological hierarchy is different, because it is a hierarchy made of FO-objects, and therefore it does not require the introduction of higher-order quantifiers to range over it. In turn, this means that it is not necessary to introduce an apparatus where the meta-language has more ideological resources of the object-language (the paradox is dealt with by the fact that in this scenario the meta-language has a broader universe of discourse of the object language[158]). The point is that nothing prevents us to introduce some logical device to allow generalizations that come out true in any domain of any language. A language can express universal sentences whose truth does not depend on the universe of discourse of that language, and therefore allow absolute generality even in the lack of a universal set or a maximal plurality[159]. The general point to remind is that an ontological hierarchy does not preclude the existence of a device that allows generalization about any extension of an indefinitely extensible concept.

[158] See chapter 7 for more details on this.
[159] Again, I refer the reader to the chapter 7 for the development of such a view.

CHAPTER 6

THE DOMAIN PRINCIPLE AND INDEFINITE EXTENSIBILITY

Abstract: in this short note, I shall present three different accounts of the Domain Principle and I shall argue that one of them is compatible with the claim that there are indefinitely extensible concepts. This is interesting, because there is a well-known argument by Graham Priest according to which the Domain Principle implies the existence of "absolute totalities" (as the totality of all sets, ordinals, and so on), whose existence is denied by the defenders of indefinite extensibility. Moreover, I shall argue that this account explains why it is possible to have absolutely general claims concerning indefinitely extensible sequences of objects, which means that it is possible to have absolute generality without an all-comprehensive plurality of objects.

1. Introduction

The most well-known formulation of the Domain principle (DP from now on) is the cantorian formulation: "each potential infinite [...] presupposes an actual infinite"[160]. The general idea (to be explained in more details below) is that, if a variable has a range which can always be increased (a potential infinite), then the totality of its values must form an actual infinity.

In the contemporary debate, the DP is not in good shape. After being defended by Cantor, it is nowadays defended – as far as I know – only by Graham Priest (and maybe by some of his followers). The reason is straightforward to explain: the principle – as stated above - seems to commit ourselves to the existence of absolute totalities, whose existence is denied in the standard Zermelo-Frankel set theory. So, *prima faciae*, the principle seems to contradict an open-ended[161] picture of the set theoretic universe, or the claim that concepts as those of set, ordinal, cardinal etc. are indefinitely extensible (from now on, we shall use IE as an abbreviation for indefinite extensibility). Roughly speaking, a concept is indefinitely extensible if, for every definite totality of objects falling under it, it is always possible to find a more inclusive definite totality of such objects. A standard example is the concept of a non-self-membered class. In fact, R - the class of all classes that do not belong to themselves - cannot belong to itself (on pain of contradiction) and, consequently, the totality of all classes belonging to R together with

[160] Cantor *Nachlass*; see later for the quotation of the full passage. It must be noticed that the term 'Domain Principle' is not from Cantor, rather it is the name M. Hallett [1986] gave to a principle defended by Cantor.

[161] The concept of open-endedness indicates that in the set-theoretic universe, as described for instance by Zermelo-Fraenkel set theory, there is no universal set, which means that for each set, there is at least a more comprehensive set. This is very close to the concept of indefinite extensibility, but not quite the same. In particular, it is possible to interpret the latter in a way that it will correspond to the former, but there are also different interpretations of indefinite extensibility in which the two concepts are not the same anymore. We are going to specify in more details the concept of indefinite extensibility later on in the chapter.

R itself constitutes a more inclusive totality of objects falling under the concept 'being a non-self-membered class'.

Each definite totality of objects falling under an indefinitely extensible concept is *an* extension of that concept. It is clear that to an indefinitely extensible concept there corresponds an indefinitely extensible sequence of its extensions. This sequence can always be increased because, given an arbitrary extension of the concept, it is always possible to find a more inclusive extension of the same concept. In this sense, the sequence constitutes a potential infinite. Therefore, in what follows we shall use the expression 'indefinite extensibility' and 'potential infinite' as synonyms[162].

The DP thus seems to contradict the existence of indefinitely extensible concepts, because if an indefinitely extensible concept presupposes an actual infinite, then there should be a totality of its instantiations such that it is not possible to find new items, different from all items of this totality, that instantiate the concept (as IE would require). However, it is not clear at all what the Domain Principle really means and, therefore, there is hope that a clarification of its meaning can show that, at the end of the day, the principle is compatible with the existence of indefinitely extensible domains.

Here the structure of the paper: first of all, I shall introduce Cantor's view about the Principle and the simplification defended by Priest [1995, 2013]: it is in Priest's version that we are going to analyze it; secondly, we will distinguish three different accounts of the principle and we shall argue that only the first two accounts are incompatible with indefinite extensibility; in the end, we shall look in more depth at the third account. In particular, we shall focus our attention on three facts: firstly, the third account is more essential and simpler than the other two, while being enough to fulfill the role that the Domain Principle should play; secondly, we shall argue that this account explains how it is possible to have absolutely general claims even in the absence of an all-comprehensive totality of all objects; finally, we shall show that the account blocks Priest's argument for the existence of absolute totalities from the Domain Principle.

2. The Cantorian Domain Principle and Priest's simplification

Cantor's conception of the DP is well-known; however, it is worth quoting him once more:

> There is no doubt that we cannot do without *variable* quantities in the sense of the potential infinite; and from this can be demonstrated the necessity of the actual-infinite. *In order for there to be a variable quantity in some mathematical study, the 'domain' of its variability must strictly speaking be known beforehand through a definition.* However, this domain cannot be itself something variable, since otherwise each fixed support for the study

[162] We use the expression 'potential infinite' to indicate whatever well-ordered sequence with no maximal element, such that, given an arbitrary element in the sequence, it is possible to find a greater element (according to the well-ordering of the sequence). We do not impose any restriction on the nature of the elements of the sequence: if these elements are sets, they could be either finite or infinite. Consequently, the notion of potential infinite Aristotle had in mind is just a particular instance of this more general notion.

would collapse. Thus, this 'domain' is a definite, actually infinite set of values. Thus, each potential infinite, if it is rigorously applicable mathematically, presupposes an actual infinite. (Cantor's *Nachlass*, from Hallett [1984], p. 25, emphasis added).

The standard reading of this passage claims that Cantor is here introducing a principle concerning the potential infinite. However, as the italic sentence shows, Cantor is dealing with a more general principle, which he then applies to the case of potential infinite. This more general principle says that the domain of the values that a variable may assume cannot be something variable and must be known in advanced through a definition. In the particular case of the potential infinite, what Cantor states is that each potential infinite implies an actual infinite as its domain of quantification[163]. The cornerstone idea is that *to know* whatever infinite sequence, we must look at the values that its variables can assume; these values cannot be a new potential infinite, otherwise we will find ourselves in a vicious regress. Therefore, the values must form a determined totality, an actual infinite set.

The argument is straightforward and powerful: mathematics deals with potential infinities; to know them we must know the values their variables can assume; not to fall in a regress these values must form a complete and infinite set. To this idea Priest points out that, in order to work with potential infinite sequences, is not necessary *to know* the values a sequence can assume; it is sufficient that these values are determined (Priest [1995], p. 138). Priest does not explicitly distinguish between the general version of the principle and its application to the case of potential infinite, even if his analysis seems to suggest the distinction; in any case, thanks to his simplification, we can expose the two versions in the following way (where G stays for 'general'):

(GDP) whenever there is a variable quantity, the domain of its variability must be determined.

The particular case where the range of the variables constitutes a potential infinite is as follows:

(DP) whenever there is a variable quantity that can be unbounded increased (a potential infinity), the domain of its variability must be determined.

[163] It must be noticed that Cantor does not use the word 'transfinite' here, but 'actual infinite'. It is well-known that, in Cantor's philosophy, 'actual infinity' may denote either the transfinite or the absolute (God). In the passage above, the primary reading must identify 'actual' with the transfinite. However, Cantor is fully aware of the fact that if we apply the DP to the whole sequence of transfinite numbers, what we get cannot be a further transfinite number, otherwise we would not have applied the DP to the whole sequence. In some passages (see for instance Cantor [1988], in Cantor [1932], p. 405), he suggests that the transfinite hierarchy "with the abundance of its forms and figures" refers to the absolute, the "true infinity". In turn, this may suggest that he believes the DP should apply to the whole sequence of transfinite numbers, and in that case 'actual infinite' should be read as denoting the absolute. This could explain why in the passage above he formulated DP by speaking of 'actual infinite' and not of transfinite. However, in the essay *Mitteilungen zur Lehre vom Transfiniten* (in Cantor [1932], p. 399), he formulates the DP only in relation to the transfinite and not the actual infinite. In this stricter formulation, the DP is closer to the first account of the DP we are going to analyze below.

Why should we regard these principles as compelling? In other words, what reasons do we have to be committed to the (G)DP? Priest [1995, p. 138] gives a small example to clarify the strength of the principle: consider the claim "the root z of the equation $ax^2 + bx + c = 0$ has at least one value". This is true if z is complex, false if it is real (of course this is only valid for some appropriate coefficients a, b and c). So it is necessary that the domain of the variable x in the equation is determined in order the claim above to have a determined truth value. This is a powerful reason to support GDP, the general principle. The example shows that the determination of the domain of the variable is a necessary condition for the sentence to have a determined truth value. Different determinations may give us different truth-values.

What about DP? Since GDP enjoys plausibility, the same is valid for DP: as matter of fact, we distinguish different infinite sequences by the different values they can assume. If one objected that it is enough to have the laws of construction of the sequences to distinguish between several sequences, it must be replied that the laws are different because they allow us to find different values the sequences can assume. From this point of view, there seems to be good reasons to accept the DP.

This example shows why the DP is an appealing principle: in a way, it extends to the case of potential infinite sequences the general idea that a sentence (with quantifiers) expresses a determined proposition only if we are able to single out its domain of quantification. Below we are going to show that there is an account of the DP which is compatible with indefinitely extensible concepts. This has a really positive consequence: one can both argue that a sequence is indefinitely extensible and that it has, at the same time, a fixed domain of variation. So, when it comes to explain how quantification over an indefinitely extensible concept is possible, thanks to the DP one is *not* forced to argue that this quantification is a domain-free form of quantification.

We can thus summarize the two main motivations to adopt DP (which of course are also reasons to accept GDP): 1) the truth-value of a general sentence (a sentence with a quantifier) depends on the domain of the (bound) variables; 2) DP can be used to extend to the case of the potential infinite the standard practice of making explicit the domain of the bound variables in a sentence when giving its semantics.

Going back to Priest's analysis, his version of the DP is simpler than Cantor's, because it does not require that we know the domain of variability, rather it just requires the domain to be determined. In what follows, we shall work with the definition of the DP just given, so following Priest in claiming that it is not necessary to know the complete domain of the variable. However, in this version of the principle the problem consists in understanding what exactly the adjective "*determined*" means. Different interpretations are possible, which give different results. Understanding the meaning of "determined" in this context is fundamental, since Priest uses this principle to argue that the 'absolute totalities' exist. For instance, considering the lack of the universal set in ZF, he comments:

According to Cantor's Domain Principle, which we saw to be quite justified [...], any variable presupposes the existence of a domain of variation. Thus, since in ZF there are variables ranging over all sets, the theory presupposes the collection of all sets, V, even if this set cannot be shown to exist in the theory. Consistency has been purchased at the price of excluding from it *a set* whose existence it is forced to presuppose (Priest [1995], p. 175).

The passage clearly states that the existence of the universal set is a consequence of DP. If so, and if the reasons to accept the DP are strong enough, then ZF and each account that claims the legitimacy of IE are in danger.

3. Three accounts of the Domain Principle

How to interpret the claim that the values of a variable must be determined? Here are three different accounts.

Account 1: The first suggestion is to claim that these variables must constitute a *set.*

(GDP-1): whenever there is a variable quantity, the domain of its variability must be a set.

If so, the DP-1 would state that a potential infinite presupposes a set as a domain for its variables, which implies that the potential infinite would be extensible up to a certain limit ordinal[164]. In quantificational terms, this means that to quantify over an indefinitely extensible sequence, the whole sequence must constitute a set. The quantificational 'counterpart' of GDP-1 would be nothing more than the All-in-One principle[165] (and the quantificational counterpart of DP-1 would just be the restriction of the All-in-One principle to the case of potential infinite):

(All-in-One Principle): quantifying over certain objects presupposes that these objects are collected in a set or a set-like object.

With 'set' I mean a collection which is *distinct* from its elements (with regards to its elements, a set is always a further object) and which respects the axiom of extensionality[166]:

(Est-S) $\forall x \forall y\ (x = y\ \leftrightarrow\ \forall u\ (u \in x \leftrightarrow u \in y))$,

where x, y are first order variables for sets, and $\in$ is the membership predicate. Given extensionality, a (particular) set x is never extensible: if we enlarge x by adding even only one more element, by (Est-S) we obtain a new set x' such that $x \neq x'$.

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164 If a domain is extensible up to a certain limit ordinal, it is said to be bounded indefinitely extensible; if there is no such ordinal that constitutes the upper bound of its extensibility, then the domain is (unbounded) indefinitely extensible. See again Shapiro and Wright [2006], for an analysis of such distinction.

165 The All-in-One Principle was formulated for the first time by Cartwright [1994], who dismisses it as invalid.

166 I consider proper classes as an example of a set-like collection: they are usually considered to be collections too big to form a set; however, they are set-like, because they constitute *a further object* in relation to their elements.

DP-1 is incompatible with IE[167]. If we interpret the locution ‘definite totality’ in the characterization of IE given above as indicating a set of objects, IE turns out to say that for each set of objects falling under an indefinitely extensible concept, there is (at least) a more comprehensive set of objects falling under the concept. I shall call this interpretation of indefinite extensibility IE-1. IE-1 is clearly incompatible with DP-1, which implies that all extensions of an indefinite extensible concept form a set (which should be the maximal extension of the concept).

Moreover, on this reading of the principle, unrestricted quantification over all sets of ZF is not possible, if consistency must be preserved. Now, note two things: since a transfinite number is a set, this account is the right way of interpreting Cantor’s own idea concerning the DP (at least in the cases in which we do not deal with the whole sequence of transfinite numbers). In fact, in the quotation above he explicitly says that ‹‹this ‘domain’ is a definite, actually infinite set of values››. A potential infinite implies an infinite set (a transfinite) as its own domain of variation. Secondly, it is not so clear if this is Priest’s view concerning the principle: in stating it, he never says that to be determined means to be a member of a set (I think he will probably argue that these are two quite distinct things). However, there are points of his texts that suggest the opposite: in Priest [1995] he brings model theory as a modern defense of the DP, but the domain of a model is always taken to be a set[168]; when he argues for existence of absolute totalities from the DP, as reported above, it is arguing that these totalities exist and are *sets*. But this is possible only if he embraces this first account of the DP (this means that Priest’s argument above for the existence of absolute totalities from the DP presupposes this first account).

Account 2: according to the second account, that the variables are determined simply means that there are *some values* in the plural (or there is the plurality of the variable, where the “plurality” must be taken as in plural logic, that is as the objects referred to by a *plural expression*[169]). In this case, there is no need of asserting the existence of the set of the variables; just the variables are sufficient. GDP-2 amounts to the following principle (again DP-2 is the same principle restricted to the case of potential infinite):

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[167] I do not consider here the possibility of a relativist position. A relativist may argue for both the DP-1 and IE, and concludes that no absolutely unrestricted quantification is possible. An immediate problem for the relativist is to express the very idea that a concept is indefinitely extensible, which seems to require generalization over all extensions of the concept. But being committed to DP-1, the relativist thinks that no such generalization is available. For a deep discussion see Uzquiano & Rayo [2006], and Williamson [2003].

[168] In particular, in Priest [1995, pp. 138-139], we read: ‹‹Cantor’s argument can be put in contemporary form (bypassing the issue of what, exactly, a potential infinity is) by a consideration of modern semantics. [...] Now, for a sentence containing a variable to have a determinate meaning, the range of the quantifiers governing the variable (which may be implicit if the variable is free) must be a determinate totality, a definite set››. Priest is here speaking of contemporary model theory; however, as the quoting makes clear, he is using model theory as a contemporary example to support the same idea behind the DP. If so, he is claiming that the DP implies the set of the values of the variables of a potential infinity: the fact that he accosts the expression ‘a determinate totality’ with ‘a definite set’ is a clear clue that he is considering the two expressions as synonyms.

[169] A plural expression (i.e. the dogs, the students, and so on) allows reference to several individuals at once.

(GDP-2) whenever there is a variable quantity, the domain of its variability must constitute a plurality of objects.

GDP-2 is compatible with IE, if with the expression 'definite totality' we mean a set of objects as above. However, according to this interpretation IE would just be the thesis that, given a set of objects, there is a further and more comprehensive set of objects, which is trivially true by Cantor's theorem. In order to make IE an interesting thesis, we should interpret the locution 'definite totality' as denoting a plurality of objects. According to this interpretation, given an arbitrary plurality of objects falling under an indefinitely extensible concept, it is possible to find a more comprehensive plurality of objects falling under the concept. I shall call this latter formulation of indefinite extensibility IE-2. IE-2 is clearly incompatible with GDP-2 (and DP-2). In particular, DP-2 would claim that every potential infinite requires a definite plurality (not a set) as its own domain; in other words, the domain is to be identified with all values of these variables and these values must be given all at once (which means that it is not possible to find further values which are not one of those)[170].

The quantificational counterpart of this account is what is known (see Uzquiano [2009]) as the All-in-Many principle:

(All-in Many Principle): quantifying over some objects satisfying a certain condition is to presuppose that there are some objects that are all and only those objects that satisfy that condition.

As mention above, the expression 'a plurality' is just loose talk to be substituted by a plural expression. However, I shall speak of pluralities since this help to simplify the discourse. Therefore, a plurality is not a further object with regards to its members: the plurality of objects xx is simply *the xx.* Moreover, in the standard way of interpreting this notion (which I shall follow here), a plurality has an extensional nature, i.e. it must respect a plural version of the axiom of extensionality:

(Ext-P) $\forall xx \forall yy\ (xx = yy \ \leftrightarrow \forall u(u \prec xx \leftrightarrow u \prec yy))$,

where xx, yy are plural first order varibles for pluralities, and $u \prec xx$ is to be read as "u is one of the xx".

The standard defense of a plural approach to logic and set theory is Boolos [1984]. For our aims, it is important to remind that Boolos interpreted the set theoretic paradoxes simply as a *reduction* of the hypothesis that the correspondent sets exist. However, even without a universal set, Boolos believed the universe of all sets to be fully determined, because there is the plurality of all sets (a plurality that cannot form a set). Ultimately, according to him, what the set theoretic paradoxes reveal are the existence pluralities that are too numerous to form a set[171].

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[170] Notice that this formulation is still incompatible with GDP-1.

[171] See Boolos [1984, 1985] for a defense of plural logic and for a deep defense of these ideas.

Account 3: the third account constitutes a purely *intensional* way of interpreting the claim that the values must be determined. The idea is that a domain is expressed by means of a concept *C*, and, given an arbitrary objet *c*, the question 'does *c* fall or not fall under the concept *C*?' always admits an answer. In other words, the concepts used to specify a domain is fully determined with regards to each single instance. Here we presuppose neither that the domain forms a set, nor that it forms a plurality. Therefore, the domain might be indefinitely extensible (with regards to both IE-1 and IE-2).

We may formulate this third version of GDP as follows:

(GDP-3) whenever there is a variable quantity, it must be fully determined if an arbitrary object *x* is a value of the variable or not.

DP-3 is the following restriction of GDP-3:

(DP-3) whenever there is a variable quantity that can be unbounded increased (a potential infinity), it must be fully determined if an arbitrary object *x* is a value of the variable or not.

A simple example should clarify this account. Consider the sequence of the ordinals, and suppose they constitutes an indefinitely extensible sequence, according to IE-2. In what sense can they be said to be undetermined? The only sense is that there is no largest ordinal, and therefore it is possible to enlarge each extension of the concept of ordinal number without end. It is their extension to be always increased and, just in this sense, undetermined. But they have clear conditions of identity and applicability. From this point of view, their *intension* is completed determined and no doubt whatever can be raised about it. In turn, this means that the concept "being an ordinal number" is not vague at all. Therefore, given an arbitrary object it is fully determined if it is an ordinal or not; there are no borderline cases.

The fact that the domain may be indefinitely extensible shows that the locution 'an arbitrary object *c'* in the formulation above must be taken in an absolute sense, i.e. it must not be read as referring to an arbitrary object of a previously specified domain (set or plurality) of objects; rather, whilst dealing with indefinitely extensible sequences, the locution must be taken as denoting any object, however you can expand the starting domain (set or plurality). One obvious consequence of this is that a concept used to specify a domain cannot be introduced by means of a standard (or plural) quantification. In other words, we cannot introduce the concept C either by the sentence '$\forall x\ (Cx \vee \sim Cx)$' or by the sentence ''$\forall xx\ (Cxx \vee \sim Cxx)$'. In the first case, the sentence presupposes the existence of a set for the quantifier to range over, while in the second case it would presuppose the existence of a plurality. What we clearly need is a more *intensional* approach that can express the fact that the concept is determined however you can expand the starting domain (set or plurality). This may be expressed by a modal operator: $\Box\forall x\ (Cx \vee \sim Cx)$. The modality here in play must be taken as primitive in order not to presuppose a previously specified set or plurality of objects.

With the introduction of a primitive modal operator we can specify the quantificational counterpart of the GDP-3:

"Quantifying over any arbitrary object c satisfying a certain concept C is to presuppose that, necessarily (i.e. however you may expand the domain), either c falls under C or it is not the case that c falls under C".

The modality here invoked is very close to the modality invoked by Linnebo [2010, 2016] in his defense of a potentialist account of the iterative conception of set. I therefore refer the interested reader to Linnebo's paper for a full explanation of such an operator. Here I limit myself to few comments. Firstly, the primitive modal operator is only a technical means to capture the idea that concepts must be considered as purely intensional entities in order for the third account to take off the ground. A concept is specified by means of its conditions of identity and applicability, and not by means of the objects that, as a matter of fact, instantiate it. Secondly, the modal operator should be spelled out in terms of a process of individuation of new mathematical objects. The idea is simple: suppose you have fixed a language with a set or plurality of objects. If we are dealing with an indefinitely extensible concept C, then by exploiting the resources of the language (both its ideological and ontological resources) it is possible to individuate new objects, not present in the previously fixed ontology, which – however – respect the condition of application of C. In this way, we can expand the starting domain into a more comprehensive one. In this setting, the identification of new objects falling under C presupposes that C does not change while its extension expands. The modal operator exactly expresses this feature of C.

A close position to the third account of the DP can be found in Yablo [2004, pp. 151-152]. Yablo stresses that there are two ways in which a property (or a concept) can fail to determine the plurality of objects that instantiate it.

> The property P that (I say) fails to define a plurality can be a perfectly determinate one; for any object x, it is a determinate matter whether x has P or lacks it. How then can it fail to be a determinate matter what are all the things that have P? I see only one answer to this. Determinacy of the Ps follows from
> (i) determinacy of P in connection with particular candidates,
> (ii) determinacy of the pool of candidates.
> If the difficulty is not with (i), it must be with (ii). It is not the case that there are some things the Xs such that every candidate for being P is among them. If there were, one could go through the Xs one by one, asking of each whether it has P, thus arriving finally at the sought-after plurality of Ps[172].

In the case of the ordinals, the fact that the concept 'being an ordinal' is fully determined just means that (i) is satisfied; however, their IE-2 implies that (ii) is not satisfied, i.e. the pool of candidates is not determined, in the sense that given some ordinals, it is always possible to find a new ordinal which is not one of them. GDP-3 (and

[172] This idea has been further developed by Linnebo [2010].

consequently, DP-3) just requires that condition (i) is satisfied, while condition (ii) may fail.

The legitimacy of this third account relies on the fact that being determined is not synonym of being an element of a set (or being a member of a plurality). This point is also emphasized by Shapiro and Wright [2006; p. 286], who affirm that a relist towards the existence of the set universe described by ZF's axioms believes that every set in the cumulative hierarchy actually exists independently by the human thought, and still maintains that there is no set of all sets (or that there is no maximal plurality). In this view, to claim that the hierarchy is always extensible means simply to underline a feature of it. The independency of the sets from human thought and practice is a clear clue that for the realist every set is completely determined. Of course, this position adds something more to the DP as explained in account 3, insofar it adds a substantive ontological view concerning the existence of sets; however, it is worth reminding that, although quite implausible, the simple fact that this is a possible position, shows the legitimacy of distinguishing between "determined" and "being element of a set" (or "being a member of a plurality").

4. Defending the third account

The first two accounts of the DP are incompatible with indefinitely extensible domains (if you want to keep things consistent[173]). On the contrary, the third account allows the coexistence of the DP and indefinite extensible concepts. The third account is neutral to the fact that the objects in the domain form a set or a plurality, so it is more essential than the others: both the first and the second accounts can be built from the third by imposing more conditions on it. In particular, if we add to the third account the requirement that the extension of the concept forms a plurality, then we obtain the second account; while if we add to the third account the requirement that the extension of the concept forms a set, then we obtain the first account[174].

The simplicity of the third account is not by itself a sufficient reason to argue for the superiority of it with regards to the first two accounts. It may happen that for certain purposes the third account reveals to be inadequate, while one of the other accounts (or both) may turn out to be perfectly adequate. In this section we are going to argue that this is not case. We shall start with paragraphs 4.1 and 4.2 respectively, where we shall argue that the reasons we gave above that make the DP an appealing principle are met

173 This addition is necessary since [Priest, 2013] argued in favor of both indefinite extensibility and the first account of the DP. Of course, this is possible only by working with a paraconsistent logic and by embracing dialetehism: according to the first account of DP, there must be a maximal set that comprehends all the domains of an indefinitely extensible sequence, but since the sequence is indefinitely extensible, this maximal set, at the same time, is not maximal.

174 This seems to presuppose that there are pluralities that cannot form a set (maybe because they have too many elements), which is the standard view defended by the friends of plural logic (see Boolos [1985], and Cartwright [1994]). However, this presupposition can be challenged on the ground that every plurality *can* form a set (see Linnebo [2010]). I admit that I favor Linnebo's line; however, here we do not need to go into more details.

by GDP-3 and DP-3, and that those reasons suggest that we should prefer GDP-3 and DP-3 on their rivals; in addition, in paragraph 4.3 and 4.4 we shall argue that, in relation to absolutely general statements and Priest's argument above, the third account performs better than the other two.

4.1 The determinacy of sentences' truth-values

Above we motivated the appealing and the plausibility of the DP by means of Priest's example concerning the claim "the root z of the equation $ax^2 + bx + c = 0$ has at least one value". We argued that the truth value of the sentence depends on which is the domain of the values of the root z, and that this domain must be determined, if we also want the sentence's truth values to be determined. What it is now essential to notice is that to achieve this result, it is enough that the conditions that specify the domain are determined with regards to each single instance (GDP-3); we may believe that the complex numbers and the real numbers do not form any set or plurality, rather they are indefinitely extensible; however, as long as the conditions to specify them are clear and precise, we are able to discern if a certain number is real or complex and, consequently, we are able to make explicit the truth value of that sentence. For mathematical sentences which involved generalizations on some kind of number (natural, rational, real, complex, etc.) GDP-3 is enough to fulfill the role that DP should fulfill.

The fact that the third account is able to fulfill this role shows that the first and second accounts present some superfluous features that do not play any role in determining the truth-values of such kind of sentences. However, there are sentences were the third account seems not to be enough to guarantee the determination of their truth values. Consider, as an example, the sentence 'there is a measurable cardinal'. One could argue that the truth or the falsity of such sentence depends on the domain of the quantifier. If this domain is given by the universe described by ZFC, then the sentence will probably lack a truth-value, because ZFC neither prove nor disprove it. If the domain is given by the universe of sets described by ZFC plus the axiom that states that there are no measurable cardinals, then the sentence is (trivially) false. If the domain is given by the universe of sets described by ZFC plus the axiom that states that there are strongly compact cardinals, then the sentence is true (since the existence of strongly compact cardinals implies the existence of measurable cardinals).

In this example, the concept in play is the concept of set. GDP-3 applied to this specific case becomes something like that:

(GDP-3Set) whenever there is a variable quantity ranging on items that fall under the concept of set, it must be fully determined if an arbitrary object x is a value of the variable or not.

The problem with such formulation is that it is not clear if a measurable cardinal fall under or does not fall under the concept of set, because it is not clear if there are measurable cardinals at all. (GDP-3Set) seems to be compatible both with the existence and the non-existence of measurable cardinals. But is this a problem for the third account of DP? The reason why such a formulation fails to determine a truth-value for the above sentence is that the concept of set is *not enough determined* to settle whether measurable cardinals exist or do not exist. However, this seems to be a slightly different conception of determinacy with regard to the conception of determinacy at work in the GDP-3. In fact, it is clear that if measurable cardinals exist, then they are sets; if they do not exist, then they are not sets (simply because they do not exist). Once granted that there are measurable cardinals, it cannot happen that the question 'are measurable cardinals sets?' does not have a clear answer. So the concept of set is determined (according to GDP-3) with regards to each single of its (existing) instances, even if it cannot single out whether there are or not measurable cardinals.

In any case, it is interesting to have a look at how the other accounts perform with regards to this same case. The first account performs quite badly. It is easy to see that GDP-1 would imply the existence of the universal set, whose non-existence is a theorem of standard set theory. The second account performs better. In relation to the concept of set, GDP-2 tells us that *the sets* (the plurality of sets) are determined. Therefore, it is determined if a measurable cardinal is or is not a set and, consequently, the sentence 'there is at least a measurable cardinal' has a determined truth-value. *Prima faciae,* GDP-2 seems to perform better than GDP-3. However, the reason why GDP-2 performs well is that it requires us to think of the totality of sets as completely given, as completely determined independently of our knowledge of them. In other words, GDP-2 has a good performance only if it is committed to a certain amount of mathematical realism. But as we saw at the end of section 2 that also the third account is compatible with mathematical realism. In particular, one can take indefinite extensibility quite seriously, as a feature of an independent reality (the set universe) such that given *some sets* it is always possible to find more sets: in this way, all sets are never completely given. In this account, where GDP-3 is implemented with a (substantial) view in philosophy of mathematics, the sentence 'there is at least a measurable cardinal' will turn out to have a determined truth value. From this point of view, it seems we have no reason to prefer GDP-2 to GDP-3.

4.2 The second reason: extending the practice of making explicit the domain of the variables

The third account constitutes a purely intensional interpretation of the DP. While both sets and pluralities have an extensional nature (their conditions of identity are respectively the axiom of extensionality for sets and the axiom of extensionality for pluralities), the third account just deals with the *concept* used to define the domain of quantification, without imposing any limitation on its extension. The extension of a concept may constitute a set, a plurality or may be indefinitely extensible: all these cases are compatible with the third account of the DP. Moreover, it is this third purely intentional account that makes the DP very attractive: the upshot is that, no matter if we are dealing with sets, pluralities or indefinitely extensible sequences, in all such cases there is a fully determined domain of quantification. This account allows us to extend to the case of indefinite extensibility the idea that a sentence expresses a determined proposition if and only if it is well-determined which objects it is about (which means if and only if the domain of its variable is fully determined). This means that also the second reason is met by the third account: by allowing a purely intensional way of specifying a domain, we can extend the common practice of specifying the domain of the variable of a sentence also to the case where the values of the variable constitute a potential infinite. Of course, this is not the case with the other two accounts.

4.3. Absolutely general statements

There is another class of sentences where DP-3 performs very well. This is the class of absolute general sentences over an indefinitely extensible sequence of objects. Suppose the ordinals are indefinitely extensible (in the sense of IE-2: given an arbitrary *plurality* of ordinals, it is possible to find further ordinals which are not member of the considered plurality), and consider a sentence as 'every ordinal has an immediate successor'. The sentence seems to express a trivially true statement about any ordinal number; however, both for DP-1 and DP-2 the sentence should not have a determined truth-value. For DP-1 the sentence has a determined truth value only if the domain of its variables is a set, but there is no set of all ordinals; for DP-2 the sentence has a determined truth value only if the domain of its variables constitutes a plurality, but – by hypothesis - there is no plurality of all ordinals. Neither of them can explain why the sentence in question is true. However, according to DP-3 it is just enough that the conditions for being an ordinal are well-determined in the sense specified above. These are in fact enough to single out which objects are ordinals from which objects are not ordinals, and therefore they can guarantee that the subject-matter of the sentence above is constituted by each instance of the concept of ordinal numbers, and nothing else. From a logical point of view, such generalizations must be expressed by exploiting the primitive modal operator introduced above.

4.4 Blocking Priest's argument

There is another important consequence of the account: Priest's argument that the DP implies the existence of absolute totalities is blocked. We cannot longer argue that ZF implies the existence of the universal set (so a set which cannot exist in ZF), simply because the variables in the object language vary over all sets; according to the third account of the DP, this simply means that the set-conditions must be clearly defined. In other words, it must be clear what to account as a set in the theory, or what to account as an ordinal or as a cardinal. And this latter claim is compatible with the non-existence of the universal set[175].

[175] Of course, also the second account can block Priest's argument. Therefore, the fact that both the second and the third account block this argument speaks against the first account, rather than speaking in favor of only the third account. The superiority of the third account on the second derives from the fact that the former is simpler than the latter.

5. Conclusion

In this short note, we have defended an interpretation of the Domain Principle which makes it compatible with the existence of indefinitely extensible concepts. We argued that this intensional interpretation is more fundamental than the other two interpretations, in the sense that it imposes less conditions on the nature of the domain and, at the same time, is enough to determine the domain of the variables. We have also shown that this interpretation is enough to guarantee the truth of some (intuitively true) general statements over an indefinitely extensible sequence of objects and that it blocks Priest's argument for the claim that DP implies the existence of the universal collection in ZF.

However, this is not the end of the story. The main problem with indefinitely extensible domains concerns the possibility of quantifying over them. In standard semantics, a universal quantifier requires a domain of objects to range over. If this domain must be a set or a plurality, then the defender of indefinite extensibility will find himself into troubles. However, our suggestion is that what is really required is that the domain of quantification is determined, and the third account of DP is enough to guarantees this. So the intensional interpretation of DP might be used to give a more intensional interpretation of quantification. This is a big issue that we have only mentioned in §3, but that goes behind the limit of this chapter. Here, my only aim was to provide a defense of the third account of the DP, and to show there is no reason to believe the DP – so interpreted - to be inconsistent with indefinite extensibility.

CHAPTER 7: A THEORY OF CONCEPTS

1. What does the naive comprehension principle say about concepts?

The naïve comprehension principle (NCP) is the comprehension principle for sets responsible for the derivation of the set-theoretic antinomies, as Russell's paradox. In formula:

(NCP) $$\forall X \exists y \forall x (x \in y \leftrightarrow X(x))$$

(with y that does not appear free in X). From here the derivation of Russell's paradox is straightforward. Instantiate the second-order variable X with the predicate '$\notin$':

$$\exists y \forall x (x \in y \leftrightarrow x \notin x)$$

Let's call the set defined by this predicate r. We can instantiate the existential quantifier with r:

$$\forall x (x \in r \leftrightarrow x \notin x)$$

But now, since $\forall x$ is taken to be unrestricted, it also ranges over r, so we can instantiate the quantifier with r:

$$r \in r \leftrightarrow r \notin r$$

from which a contradiction can easily be derived.

There have been a number of responses to Russell's paradox; one of the earliest was Zermelo's axiomatization of set theory (the theory Z), which substitutes the NCP with the axiom schema of separation. The axiom of separation intuitively says that given a set x and a formula ϕ, there is a set y of all elements of x that satisfy ϕ: $\forall x \exists y \forall u (u \in y \leftrightarrow u \in x \wedge \phi(u))$, with y that does not appear free in ϕ. This axiom can be seen as a restriction of NCP to a set-sized domain: given a set, we can separate any of its subsets[176] by means of a formula. The main efforts were thus being made to rewrite set theory in a way that avoids the paradoxes: NCP was mainly interpreted as a (false) claim about sets. However, NCP can be read not only as a claim concerning a certain conception of set (what is known as the logical conception), but also as a claim concerning a certain conception of *concept*. If we think of the variable X in the NCP above as a variable for (nth-level[177]) concepts, then it is straightforward that NCP is

[176] In order to separate any arbitrary subset, it is usually allowed that some instances of the meta-variable ϕ are impredicative.

[177] A concept (or a relation) is of *first-level* if it applies to a (or more) *first-order* object(s). A concept that applies to a first-level concept is a second-level concept. This means that a first-level concept can be seen as a second-order object. In general, an nth-level concept applies to a nth-order object; a nth-level concept is a nth+1-order object. However, the reader should not take these distinctions too seriously; in particular, we shall see that the idea that a nth-level concept is a nth+1-order object may be challenged, by challenging the view that concepts can be interpreted as *sui generis* objects of any kind.

expressing an equivalence between sets and concepts. In this chapter, we shall look at NCP as expressing a (false) claim about concepts. In what follows, we shall call 'concept' what is expressed by the predicative expression of a propositional function $X(x)$, while we shall call 'object' the argument of such a propositional function. Moreover, when we say that a concept P is applicable to an object a, we mean that the proposition $P(a)$ is true; if P is not applicable to a, then $P(a)$ is false.

1.1. Non-equivalence of concepts and sets, and indefinite extensibility

The suggestion we are going to explore in the next pages is based on the idea that the logical antinomies reveal that concepts are not equivalent to sets of objects (that instantiate them). In turns, this implies that the membership relation $\in$ is not equivalent with the *instantiation* relation. To have a first intuitive grasp of this non-equivalence, we may notice that if concepts and sets were equivalent, then the correspondent comprehension schemas for concepts and sets respectively would be equivalent, which means that we could always substitute one schema for the other. Let NCP be the comprehension schema for sets and let $\forall X \exists Y \forall x(Yx \leftrightarrow Xx)$ be the one for concept (with Y not occurring free in X). In both cases take the first-order universal quantifier to be totally unrestricted. Instantiate now the condition X in both schemas with the condition of being self-identical. The comprehension principle for concepts delivers us the existence of the concept of being self-identical, while the NCP delivers us the existence of the universal set. While the former is considered to be a non-problematic concept and the proposition that everything is self-identical is trivially true, the non-existence of the universal set is a theorem of standard set theory (ZF). The two comprehension principles are therefore non-equivalent.

But how to interpret this non-equivalence? A well-known suggestion is simply that the paradoxes reveal the existence of indefinitely extensible concepts; concepts that can be identified neither with one of their instantiations nor with the totality of their instantiations. More specifically, an indefinitely extensible concept C is a concept associated with a principle of extension, i.e. a principle that, given some definite totality t of objects that fall under C, allows one to find a new object that falls under C but it is not one of the member of t. As a consequence, we can enlarge the starting definite totality t with the new object: what we obtain is a more comprehensive definite totality t' of objects that fall under C. But now the principle of extension allows us to find a further object that falls under C but which is not one of the members of t'. The upshot is that there is no definite totality of all the objects falling under an indefinitely extensible concept; to an indefinitely extensible concept, there corresponds an indefinitely extensible sequence of more and more comprehensive definite totalities of objects falling under it.

The standard example used to illustrate this phenomenon exploits Russell's paradox: let's consider a definite totality d of sets. Consider now the set r of all sets belonging to d such that each of them does not belong to itself, that is $r = \{x : x \in d \ \& \ x \notin x\}$. On pain of paradox r cannot belong to the definite totality d: if r belonged to d, r would belong to itself if, and only if it would not belong to itself. So r does not belong to d: but now if we take the union of d and r, we get a more comprehensive definite totality of sets to which we can apply again Russell's reasoning to find an even more comprehensive totality of sets.

1.2. *How to interpret indefinite extensibility*

It is notoriously problematic how to interpret the adjective 'definite' in the locution 'definite totality'. One natural suggestion consists in interpreting it as a set. Consequently, a definite totality would be a set of sets. In this case, the argument above is exactly the argument that Zermelo [1908] exploits to show that the domain of all objects that he uses as a starting point to his axiomatization of set theory is not a set, because if it were a set, by the Axiom of Separation we could separate the set r, and thus show that there is a set that does not belong to the domain (set) of all sets. However, identifying definite totalities with sets makes the indefinite extensibility thesis too weak: if 'definite totality' indicated sets, then the indefinite extensibility thesis would just mean that, given any set, there is a more comprehensive set. But this is trivial by Cantor's theorem. Moreover, in this setting indefinite extensibility turns out to be compatible with the existence of *all sets*; in other words, indefinite extensibility is compatible with the existence of the *plurality* of all sets[178]. Defenders of a plural approach to absolute generality as Boolos (1985) may welcome this reading; however, if indefinite extensibility is compatible with the existence of a maximal plurality of all sets, then we cannot draw from the set theoretic paradoxes the morale we drew above concerning the nature of concepts. To make indefinite extensibility an interesting thesis to defend, we must take it more seriously.

A better suggestion is to identify a definite totality of objects just with those objects, i.e. with the plurality of them. The reason for this identification relies on the fact that pluralities have an *extensional* nature, in the sense that they obey a plural version of the axiom of extensionality:

(Ext-P) $$\forall xx \forall yy\, (xx = yy \leftrightarrow \forall u(u \prec xx \leftrightarrow u \prec yy)).$$

Therefore, coextensiveness is the analogue of identity for pluralities: ‹‹if every one of these is one of those and every one of those is one of these, then these just *are* those›› (Williamson [2016])[179]. As a consequence, we consider (Ext-P) as the criterion of identity for pluralities. It is important to notice that considering definite totalities as pluralities has the important consequence that indefinite extensibility challenges a position like the one defended by George Boolos, according to which the set theoretic universe is determined because there is the plurality of all sets (even though there is no

[178] A plurality of objects is nothing over and above the objects, while a set is a further object with regard to its elements. I will follow the trend of speaking of pluralities of objects; however, this is just a loose talk, which should be substituted with plural locutions.
[179] See footnote 122 on §2.1 of chapter 5 for an important remark about identity between pluralities.

universal set). In this setting, if the concept of set is regarded as indefinitely extensible, there cannot be a maximal plurality of all sets. If we suppose there is such maximal plurality U, then we can run Russell's reasoning to show that there is (at least) one set r that is not one of the sets in U[180].

This interpretation of indefinite extensibility suggests that a concept cannot be identified with *the objects* that satisfy it because of the extensional character of pluralities. The idea is that the inner *intensional* nature of concepts is in no way reducible to extensional entities, on pain of paradox, as the NCP clearly shows. However, this *cannot* be read as the mere claim that a concept cannot be identified with the objects that *actually* instantiate it, because there could have been other objects that could have instantiated the concept, but that do not actually instantiate it. Let's illustrate this with an example. Consider the concept of set and the iterative hierarchy of sets. The sets in the hierarchy are formed in *stages*, starting with some Urelements (or with the empty set) and then, by application of the "set of" operation, one finds a new object – the set of all the previous elements. By going on applying the "set of" operation, it is possible to find more and more sets without an end[181]. The idea behind the iterative conception and the cumulative hierarchy is the open-endedness of the set universe: starting from a set it is always possible to find a more inclusive set by simply applying the set theoretical operations. The set universe is therefore open, i.e. there is no universal set. At this point, one could think that the intensional nature of concepts does not allow us to identify the concept of set with the sets present in a certain stage α, just because it is possible to find further stages with sets that are not present in α. However, this won't do, because this is still compatible with the plural approach to set theory: in this case, it would be fully legitimate to identify the concept of set with all sets in all the stages of the hierarchy. On the contrary, if you take seriously the idea of indefinite extensibility, this identification is not possible, since given any plurality, we can find sets that are not members of the plurality we considered[182]. In this context this implies that the concept of set cannot be identified with all sets, simply because there is no maximal plurality (or totality) of all sets. It is in this precise sense that we cannot identify an indefinitely

[180] Identifying definite totalities with pluralities has the positive consequence of making indefinite extensibility an interesting thesis, not trivially derivable from Cantor's theorem. However, it also brings the risk of making it an inconsistent thesis. The problem is that this identification requires us to work within a plural framework – at least in PFO (Plural First-order logic), and the existence of a maximal plurality is a theorem of PFO (this is just an instance of plural comprehension: $\exists xx\, \forall x\, (x \prec xx \leftrightarrow \varphi(x))$. Therefore, any PFO-formulation of indefinite extensibility turns out to be inconsistent. My solution to this problem is simply to go modal, that is I am going to give a modal characterization of indefinite extensibility. In a modal framework, PFO-comprehension just implies that in each domain there is a maximal plurality, but not that there is a plurality that comprehends all items from each domain. So it is compatible with the negation of this last assertion, which is what indefinite extensibility says.

[181] Through this operation we build the so called "cumulative hierarchy" of sets, which is formally defined by transfinite recursion: $V_o = \emptyset$ or Urlements; $V_{\alpha+1} = V_\alpha \cup P(V_\alpha)$; $V_\lambda = \cup_{\alpha<\lambda} V_\alpha$ with λ a limit ordinal.

[182] It must be noticed the modal vocabulary we used in relation to the hierarchy of sets. Of course, these modalities are not metaphysical modalities: sets are usually considered to be abstract objects, which means that if they exist, they necessarily exist. So, if sets exist, they exist in each possible world. If the concept of set is indefinitely extensible, then it is indefinitely extensible in each possible world. We are going to spell the meaning of these modalities later on.

extensible concept with the totality of its (possible) instantiations. In presence of our reading of indefinitely extensible concepts, the plural account fails because it is nothing more than an *extensional* account of concepts. On the contrary, indefinite extensibility requires that we take concepts very seriously, as *primitive and irreducible* to their extensions.

1.3 The nature of concepts: towards a modal approach to absolute generality

We could specify the intuitive notion of concepts with which we are working in the following way: a concept $P(x)$ is specified by means of some condition of application that tells us to what individual objects we can apply the concept. We shall express these conditions simply by means of a formula $\lambda x.\phi(x)$[183].

The term 'condition of application' is quite a general one. In some cases it could just denote other concepts: if you consider the concept of a human being, and you stick to the traditional definition of human being as a rational animal, then the conditions of application of the concept of human being is just the conjunction of the two concepts of being animal and being rational. The same happens with the concept of ordinal numbers: its conditions of applications are just those concepts used in its definition. However, this does not have to be always the case. For instance, the concepts of being warm or being painful may be associated with some physical feelings: every time we have a certain physical feeling, we may truly utter that it is warm or that it is painful. In these cases, the conditions of application are given by particular physical states, and not by other concepts. We are not interested in analyzing deeper what these conditions might be; for our aims, it is important to focus on the fact that their central feature lies on being *domain-independent* (with the word 'domain' I mean here a set or plurality of objects). This means that a condition of application tells us that if *some* individuals satisfy the condition, then the concept applies to them. No reference is needed to a previous domain of quantification: whatever individual *in any domain* that satisfies the condition falls under the concept[184]. Moreover, in this picture, concepts remain the same while their extension expands, which simply means that the same concept can be applied to new objects without being modified[185].

It goes without saying that we must not understand the locution 'some individuals' or 'in any domain' as if we were quantifying over all individuals or all domains (all extensions) of a concept. Such quantification is not allowed once we have embraced indefinite extensibility, which means that we have to find a different way of expressing generalized claims. Suppose we want to make a generalization such as the following: whatever satisfies the condition of application of the concept "being an ordinal" satisfies also the condition of application of the concepts "having an immediate successor". We

[183] Of course, not all formulas can give rise to a concept, which in turn means that not all predicates give rise to a concept. We do not discuss here which predicates give rise to a concept and which do not.
[184] Linnebo [2006] p. 157 defends a similar view concerning properties.
[185] Of course, this view can be challenged (and has been challenged). We are going to defend it from rival views later on.

may formalize it in the following way ($O =$ being an ordinal; $S =$ having an immediate successor):

$$\forall x\ (Ox \rightarrow Sx)$$

However, this will not do. In classical semantics, a simply universal sentence requires a set of objects for the quantifier to range over. Even if we interpret the sentence in a plural way:

$$\forall xx\ (Oxx \rightarrow Sxx)$$

the quantifier would range over some objects, and since the extensional nature of pluralities, this generalization does not work if the concepts involved is indefinitely extensible. So we express this independence by means of a *primitive* modal operator:

$$\Box\forall x\ (Ox \rightarrow Sx)$$

The modal operator just expresses the fact that the truth of the quantified sentence does not depend on the values of the bound variable[186]. The generalization will hold no matter what we take to be the values of the bound variable. I shall follow Linnebo [2010] in calling the combination of the modal operator and the quantifier 'modalized quantifier'.

With the modalized quantifier we can define when a concept is indefinitely extensible. Say that a concept P is stable if it satisfies the (necessitation of the universal closure of the) following axioms:

$P(x) \rightarrow \Box P(x)$
$\sim P(x) \rightarrow \Box \sim P(x)$
$U \leq U' \rightarrow D(U) \subseteq D(U')$

The first two conditions guarantee that a concept P does not change when its extension changes; while the latter guarantees that nothing gets lost in the passage from one extension to a more comprehensive one ($U \leq U'$ should be read as 'U has been expanded into U''). If a concept P is stable, then it is also indefinite extensible if it satisfies the following axiom: $\Box\forall xx\ \Diamond\exists u(P(u) \wedge u \nprec xx)$.

1.4. Two objections and two replies

[186] An immediate objection would be the following: if we consider a domain of just n ordinals (with n a finite natural number), then the sentence "each ordinal has an immediate successor" expresses a false proposition, because the *nth*-ordinal has no successor in the domain. But this is not a problem for our view. For instance, if we just consider finite ordinals, the sentence "each ordinal has an immediate successor" is a theorem of PA. Since PA has an axiom that states that for each natural number there is an immediate successor (just apply the successor function), no model of PA can be based on a finite set (or plurality) of natural numbers. On the contrary, if we also consider infinite ordinals, then the sentence "each ordinal has an immediate successor" is a theorem of ZFC: no model of ZFC concomitants just finitely many individuals (or no model of ZFC can contain – let's say – just $\omega + 17$ ordinals - another domain that would make the sentence false). It is therefore clear that when we take the sentence "each ordinal has an immediate successor" to be true, we take it to be true with regards to the models of a certain theory.

Before explaining the nature of this generalization, we would like to address a couple of objections that the reader may want to raise at this point. Firstly, dealing with the condition of application of concepts we said that a condition tells us that if *some* individuals satisfy the condition, then the concept applies to them. In stating the condition, we used a quantifier '*some'* that must be taken as absolutely general. If so, it seems that the introduction of the primitive modal operator is grounded on an absolutely general quantifier, which would make the operator totally useless. However, I think this objection is misleading. The expression 'some' is certainly a quantifier, i.e. an expression of generality, but there is no reason to interpret it according to standard logic. 'Some' is a natural language expression, and not a formalized one, which means that it is possible – at least in principle - to formalize it differently from the way it is usually formalized. Indeed, what we are suggesting is that we should formalized it by means of a modalized quantifier. The fact that we used (and we are likely to use again) natural language expressions that are normally read as standard quantifiers to explain what is going on with the modal operator does not mean that we are reducing this form of generality to standard quantification. If one has the feeling that the explanation actually reduces the modality to quantification is just because the acquaintance with the standard theory of quantification is so rooted that one immediately interprets expressions of generality in natural language as if they always behave as standard quantification.

In any case, we can make explicit the impossibility of reading those expressions as standard quantifiers by slightly modifying our previous characterization: a condition of application for a concept P tells us that, *necessarily*, if some individuals satisfy the condition, then the concept applies to them.

The second objection concerns a circularity worry. We are introducing the modal operator to capture the irreducibility of concepts to extensions, which suggests that once we grasp how concepts work we also grasp the meaning of the primitive modal operator. But to grasp how concepts work requires grasping some expressions of generality that must be formalized with the same modal operator, which makes the attempt of clarification plainly circular. Indeed, I think that here there is a circularity, but this circularity is not a vicious one. The reason is that we are dealing with a primitive notion that cannot be explained by means of other notions. If one would like to dismiss the whole account because of this circularity, then they should also dismiss the use of standard quantifiers, because standard quantifiers present the same circular pattern. For instance, consider the following sentence of a meta-language in which we state the truth-conditions of a first-order universal sentence:

$V_{M,g}(\forall\alpha\phi) = 1$ iff for every $u \in D$, $V_{M,g}(\phi) = 1$

The universal sentence of the object language has been assigned a truth-value in the meta-language by means of an expression 'for every u' composed by a quantifier whose range is the domain of the model. What does 'for every u' mean? If you formalize it, the only possibility you have in first order logic is to use the universal quantifier ∀ (or you can use an equivalent expression such as $\sim\exists\sim$). *So we have explained the truth-conditions of the universal quantifiers by means of the same universal quantifier*[187]. This circularity is inescapable: what it means is simply that a quantifier (in first-order logic) must be taken as something primitive, not reducible to other entities.

1.5 A new form of generality

Having dealt with these two objections, we can now explain in more details what kind of generality the modalized quantifier expresses.

A comparison with a different kind of generalization may help: suppose you are at the park and a group of white swans are swimming in from of you. At a certain point you utter the sentence "all swans are white". The proposition expressed by the sentence is true if you use the plural expression 'swans' to refer to the swans in front of you. The reason why the sentence is true is that each swan referred to by the plural expression is in fact white. However, uttered in a different circumstance, the sentence may express a false proposition: for instance, this is the case if the noun 'swans' refers to some swans between which there is at least one black swan. The fact that even one single black swan makes the sentence express a false proposition shows that this kind of generalization depends on *each* of its instances. Sentences that are sensitive to the objects over which the quantified variables range express different propositions with different truth values with regards to different values of their bound variables. This is the reason why – dealing with this kind of generalizations – we must specify in advanced the domain of quantification: different domains can give us different propositions. Therefore, standard quantification is the right way of formalizing this kind of generalizations.

However, sentences as "all ordinals have an immediate successor" or "all bachelors are not married" present a different kind of generalization. Such sentences are *not sensitive* to their single instances, because they express what we may call *conceptual truths* (we shall call 'conceptual generalization' a generalization that expresses a conceptual truth): they express true propositions just in virtue of the meaning of the words involved[188]. Another example of generalizations that are not sensitive to their single instances are generalization as 'All whales are mammals', which are necessary

[187] Of course, technically speaking the two quantifiers are not the same, since one belongs to the object-language, the other to the meta-language. However, it is clear that they are of the same kind, which is what we meant by saying that we have explained the truth-conditions of the universal quantifiers by means of the same quantifier.

[188] The reader may have noticed a close connection between what we called 'conceptual truth' with what Kant called 'analytical truth'. According to Kant's definition, a sentence is analytical if the meaning expressed by the predicate is contained in the meaning expressed by the subject of a sentence: if we analyze the meaning of the word 'bachelor', we are going to find the meaning of the predicate 'not being married'. Moreover, notice that one further reason not to identify these modalities with metaphysical modalities lies on the fact that it is not metaphysically necessary that the bachelors are not married. This depends on the way we defined the words 'bachelor' and 'married', but there is nothing necessary in these definitions: it could have been possible to define the concepts in different way such that the sentence "all bachelors are not married" would have expressed a false proposition.

and a posteriori truth[189]. We do not need to check every single ordinal or bachelor to see if it has an immediate successor or if he is married or not. It is in virtue of the concept of ordinal number and of bachelor that the previous sentences are true. Therefore, the truth-value of such generalizations does not depend on having previously specified a pool of candidates (a plurality of objects) as values for the quantified variable. No matter which pool of candidates we may consider, the truth-values of such sentences always remain the same. The only thing we must assure if we want the sentence to have a determined truth-value is that the concept involved in it is well-defined. A concept C is well-defined if, given an arbitrary object c, the question 'does c fall under C or not?' always have a determined answer. This request amounts to impose that the concepts in play are not ambiguous or vague, in the sense that it must be completely clear if an arbitrary object is or is not an instance of them. If there were borderline cases, we would have no assurance that the recognition of them as instances of the concept will not change the truth values of such generalization[190]. It is not necessary that the pool of candidate is determined in advanced, but it is necessary that the concepts involved are well-defined.

The fact that we do not need to determine in advanced the pool of candidates (that is the domain of the generalization) makes this form of generalization suitable for indefinite extensibility. If a concept is indefinitely extensible, then it is impossible to determine in advances the domain (set or plurality) of its instantiations. But if the concept is well-defined, we can easily make true generalizations concerning *any* of its instances. The primitive modal operator in a sentence as $\Box\forall x\ (Ox \rightarrow Sx)$ exactly expresses the fact that the truth-value of the sentence $\forall x\ (Ox \rightarrow Sx)$ does not depend on the particular pool of candidates we have considered. It should be clear that the need of a primitive modal operator is an immediate consequence of considering concepts as primitive and irreducible to extensions. The impossibility of interpreting the $\Box$-operator as a quantification over all domains of values that the bound variable can assume is clearly due to the fact that we are dealing with an indefinitely extensible concept[191].

The true difference between the generality expressed by means of the modalized quantifier and the standard quantificational generality lies on the fact that the truth-value of the latter depends on which objects there are in the domain of quantification, while the former is domain-independent. In other words, the modalized quantifier expresses a form of *open-ended* generality. The reason why standard quantification

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[189] See chapter 4, §4 for more details on these two kinds of generalizations.

[190] The situation I have in mind is the following: suppose that the items falling under a (complex) concept C have the features a, b, c and d, and suppose that there is an object x with the feature a, b and c, but without the feature d. Suppose further that x is a borderline case of the concept C. Consider the generalization 'All Cs are d'. This sentence will be true or false depending on how we are going to account for the borderline case: if we will recognize it as an item falling under C, then the sentence will be false; otherwise, it will be true. It is therefore fundamental that the concepts involved in this kind of generalization are well-defined.

[191] It is certainly possible to continue to speak of possible worlds as an intuitive way for grasping what is going on with the primitive modal operator. However, the speech must absolutely not be taken at face value

(both the classical one and the plural version) is not suitable to deal with indefinite extensibility is the fact that it is not open-ended.

The open-endedness is a typical feature of schematic generality[192]. A schema as $\alpha \vee \sim\alpha$ is a open-ended because, no matter how we instantiate the meta-variable α, we will always obtain a true sentence. The validity of the schema does not depend on the particular interpretations of the meta-variable, but just on its logical form. However, the schema in itself is neither true nor false, not expressing any determined proposition. On the contrary, the modalized quantifier expresses a generalization that has a truth-value: the truth-values of a conceptual generalization depend on the concept involved, not just on the syntactic structure of the sentence[193].

To sum up, the modalized quantifier expresses a form of generality that, on one hand, is neither reducible to standard quantification nor to schematic generality; but on the other hand, it is characterized by the two fundamental aspects that pertain, respectively, to quantification and schematic generality. As quantification, but differently from schematism, a modal quantified formula has a truth value, and therefore it expresses a proposition with a determined truth-value; as a schema, but differently from quantification, a modalized quantifiers is *open-ended*, that is it does not depend on a particular domain of objects.

2. Three different accounts of the irreducibility of concepts to objects.

What indefinite extensibility shows is the irreducibility of concepts to their instances, which means their irreducibility to their extensions. How then to interpret this irreducibility? In this paragraph, after looking at two well-known, but somehow problematic views in which this irreducibility might be understood, we propose a third view, which is the one we want to develop in this chapter.

2.1 Frege's distinction

The first interpretation of this irreducibility is the one we find in Frege's article *Über Begriff und Gegenstand* (Frege [1984]). Here Frege holds that there are things that cannot be predicated of something else: the objects. His argument to support this thesis is that, if $Y(x)$ is a predicative sentence, then it must be read as "x falls under the concept Y". Here, according to Frege, it is impossible to reverse the relation and say "Y falls under x", as happens with identity statements[194]. The irreversibility of the relation

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[192] See for instance Lavine [2006] and our chapter 4.

[193] In any case, we can use the modalized quantifier to express logical truths as the excluded middle: $\Box\forall p(p \vee\sim p)$, where p is a propositional variable (not a meta-variable). Also in this case, the modal operator is grounded in the concepts involved: in particular in what we take the logical constants to *mean.* When we say that a tautology is true and a contradiction is false because of their structures, we usually take for granted that the logical constants have their standard meanings, which means that it is in virtue of these meanings that we can say that a sentence with a certain structure is always true or false.

[194] The sentence "the morning star is the evening star" may be reversed in "the evening star is the morning star". This is possible because they are two identity statements, i.e. they simply say that the referent of the singular term "the morning star" is the same referent of the singular term "the evening star".

of predication shows that there are some things that can be object of predication, but cannot be predicated of something else (here Frege does not consider the possibility that an *nth*-level predicate can be the argument of an *nth+1*-level predicate. He will consider this possibility when dealing with the concept horse paradox. See Dummett [1973], chapters 7 and 8). All other things are called "concepts". So the fundamental distinction drawn here by Frege is between what cannot be predicated of something else (object) and what can be both predicated of something else and be an object of predication (concept). The distinction has to be taken as exclusive: the two categories do not overlap.

To fix this distinction Frege uses the adjective 'saturated' and 'unsaturated' to refer to objects and concepts respectively[195]. Objects are saturated entities, while concepts are unsaturated, in the sense that they must be fulfilled by an object (or more objects) to produce a proposition with a truth value. However, there is another thesis that Frege holds together with this one: in a logically perfect language as the one of his *Begriffschrift*[196], every singular term (a proper name or a definite description) always refers to an object[197]. Hence, we have here two theses:

a) An object is what cannot be predicated, while a concept can be both predicated and be object of predication: objects and concepts are exclusive;
b) A singular term always refers to an object[198].

But together a) and b) lead to a paradox: suppose you want to refer to a specific concept, let's say "the concept horse". This is a definite description (that is a singular term) so, according to b), it must refer to an object. Consequently, we have that the following sentence is true:

1) The concept horse is not a concept

And the following sentence is false

2) The concept horse is a concept.

It seems we cannot refer to concepts, because as soon as we try to refer to them we get a definite description (a singular term) and so we actually refer to an object.

The problem is more serious than may appear at a first sight. The adjectives 'saturated' and 'unsaturated' that Frege used to characterize objects and concepts belong to the same grammatical category, which suggests that objects and concepts are

[195] To be more precise, Frege called "unsaturated" all functions: a concept is just a function which, once saturated, gives rise to a proposition that denotes (*bedeutet*) a truth value, in other words a concept is a function that maps an argument to a truth value.
[196] A logically perfect language is a language in which the superficial grammar of each sentence perfectly mirrors the logical form of the sentence. According to Frege, in such languages each proper name refers to an individual. Frege considered the existence of empty names as a defect of natural languages.
[197] Frege used to call "proper name" all singular terms (what we nowadays call proper names and definite descriptions).
[198] This is what has been called the *logical notion* of object.

different entities of the *same* grammatical category. But the effect of this is of undermining the irreducibility of concepts to objects. If concepts and objects belonged to the same category, then the concepts would simply be a particular kind of object (higher-order objects). So Frege seems to be committed to the existence of an infinite hierarchy of objects. But as soon as we admit the legitimacy of a hierarchy of objects, we cannot allow nominalization anymore. When we refer to a concept by an expression as 'the concept of...' we are referring to it by means of a term that is the result of nominalising the correspondent predicate. The paradox stems from the tension between these two phenomena: on one side the linguistic phenomenon of nominalization, while on the other side the hierarchy of higher-order objects.

2.2. The hierarchy of languages

Frege's unpublished reply to the concept horse paradox was to embrace a hierarchy of levels and objects: at level 0 we have terms for objects, at level 1 predicates (that stands for concepts) that applies to terms for objects; at level 2 predicates of predicates of level 1, and so on[199]. In this way, he denied that nominalization is a logical feature of a logically perfect language.

A similar proposal to capture this irreducibility has been made by Williamson [2003], who proposes a type-theoretic view to deal with a reformulation of Russell's paradox for the notion of interpretation. We face this paradox as soon as we try to unrestrictedly quantify over everything, that is when the interpretation of the quantifiers is taken as totally unrestricted. Williamson argues that the paradox stems from treating interpretations themselves as first-order objects; but since interpretations are specified by means of predicates, it is more natural to interpret quantification over predicate position as irreducible to quantification into name position. Here irreducible means *primitive,* i.e. second-order quantification must not be interpreted by means of a first-order quantification. In this latter case, we would make the interpretation collapses to a first-order object, and the paradox would be reinstated. One important point to handle with care is that Williamson is not affirming that interpretations of *nth*-order quantifiers are *nth+1*-order objects (which means that he is not affirming that *nth*-level predicates are *nth+1*-order objects), otherwise the range of no *nth*-order variable would be absolutely unrestricted. Interpretations are not objects at all and speaking of higher-order concepts is just misleading: higher-order quantification does not bring with itself any *ontological* commitment. Commenting on Frege's distinction between saturated and unsaturated entities, Williamson writes:

> For the same reason, the attempt to contrast objects and concepts as saturated and unsaturated respectively is deeply misleading, for 'unsaturated' is the negation of 'saturated' and the two adjectives belong to the same grammatical category; but whereas 'is saturated' is a first-level predicate, we need a higher-level predicate in place of 'is saturated' to do the required work. *The distinction must remain one of grammar and not of ontology*, because one cannot use first-level and second-level expressions in the same grammatical context to

[199] See for instance Dummett [1973], chapters 7 and 8 for a discussion of this solution.

articulate an ontological distinction without violating constraints of well-formedness. (Williamson [2003], p. 71, emphasis added).

That the distinction must remain one of grammar and not of ontology means that moving higher-order we move to a more expressive language that allows a richer range of ways of reference to the same domain of individuals (the ontology does not change). According to this view, the first-order quantifiers already range over everything. However, one should not interpret higher-order quantification as a mere *ideological* apparatus. It is true that higher-order quantification does not have any ontological commitment, nevertheless, according to Williamson, they have a metaphysical commitment: ‹‹a higher-order commitment of the form $\exists X \phi X$ is typically neither ontological nor ideological›› (Williamson 2013, pp. 260). What he is suggesting is that the dichotomy between ontology and ideology is misleading, because not all metaphysical commitments are ontological commitment: ‹‹but not all metaphysical commitment is ontological commitment. The irreducibly higher-order metaphysical claim $\exists X \phi X$ may run just as great an epistemic risk of falsification by non-linguistic reality as the first-order metaphysical claim $\exists x Gx$›› (Williamson, [2013], p. 260).

This view is surely better than Frege's own, and allows one to have a strong logic with unrestricted quantification as a mean for one's theorizing. All in all, the advantages of this approach are several and quite promising. However, there is a problem which concerns any hierarchy of this kind: how is it possible to state the theory according to which there are infinitely many irreducible languages with infinitely many irreducible semantic values without quantifying over all such semantic values? According to such approaches, quantification over all semantic values of all languages is not possible, because no language can quantify over itself on pain of paradox; however, to state the theory we must quantify over all semantic values of all languages to say – for instance – that for every language, there is at least one language of higher-order. Therefore, this approach seems to be committed to the truth of sentences that are not expressible according to the theory itself, but that they are in fact expressed in stating the same theory. To be more precise, we can follow Linnebo [2006], where he argues that type-theorists are committed to the following claims, even though their theory prevents us to express them:

Infinity: there are infinitely many types of semantics values;

Unique existence: every expression of every syntactic category has a unique semantics value, not only within a particular type, but across all types;

Compositionality: the semantics value of a complex expression is the function of the semantics values of its constituents.

As Linnebo notices, these are generalizations over all levels of the hierarchy, because what the sentences express should be true in all levels. That there are infinitely many types of semantic values is the corner stone of the theory; that every expression of every syntactic category has an unique semantics value must be true for all levels; the same for compositionality: the principle is valid for all levels of the hierarchy.

I am not going to discuss here the possible replies that type-theorist could give[200]. Notice though that this problem arises no matter how you interpret the hierarchy of languages: both if you interpret it as a mere ideological hierarchy with no commitment at all and if you interpret it as bringing a metaphysical commitment but not an ontological one, the hierarchy prevents one from quantifying over all semantic values. Since this objection seems to me quite problematic given that general target of the discussion is absolute generality, I prefer to look for a different approach to interpret the irreducibility of concepts to their instances; a way that – hopefully – avoids the expressive limitation of the hierarchical approach. However, from the problem of the hierarchical approach we can draw some desiderata that our theory should respect: first of all, it must introduce no ideological hierarchy such that the meta-language is more ideologically expressive than the object language; secondly, as theories that preserve consistency without appealing to any hierarchy are either too weak or they need a quite drastic revision in the underground logic[201], we are open to accept some kind of hierarchy to avoid the paradox. Since this hierarchy can be neither ideological nor bringing just a metaphysical commitment, it must be ontological. Of course, this is not surprising given our defence of indefinite extensibility.

2.3. Irreducibility of role in a propositional function

The third view – the one I want to explore now – interprets this irreducibility as the irreducibility of role – in a propositional function – between the argument and the predicate. Spelt out in these terms, the difference between a concept and a (first-order) object is thus a difference in the *role* they respectively play in a propositional function. The object is the subject-matter, while the concept is what is ascribed to the subject-matter. This difference seems *irreducible*, in the sense that it is the corner stone of predication. Without it no predication would be possible.

Our hypothesis is thus that we can pin down the irreducibility of concepts to objects to the irreducibility of roles of argument and predicate in a sentence. In turn, this means that nominalization – the transformation of a predicate (or an adjective) into a noun that can play the part of an argument in a propositional function – will be taken seriously. More specifically, since the irreducibility is just an irreducibility of role, it is possible that what plays the part of a predicate in a sentence, in a different sentence plays the part of the argument (after being nominalized).

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[200] I have discussed at length this objection and Williamson's approach in chapter. 5, §3.

[201] There are several ways of preserving the consistency of a theory in front of a paradox. The standard one is to preserve full classical logic, and introduce an ideological hierarchy, as those just criticized here. An alternative way (which we shall develop here) is to preserve classical logic (with the addition of some further expressive resources as our primitive modal operator) and to introduce an ontological hierarchy. There are also ways of preserving consistency without going for any hierarchy. A first possibility is to ban the notions that make the paradox arises: however, the resulting theories are usually too weak, at least in the sense that they make meaningless some notion which is perfectly meaningful in natural language; a second possibility is to change logic. However, in this last case there needs to be quite a drastic revision, since not all non-classic logic avoids the ghost of hierarchies as Krikpe [1975] clearly shows.

We are going to sharpen this idea in the following way:

- We shall use a type-theory (Gallin's type theory with a small modification) to keep track of the irreducibility of concepts to objects. The object-language is therefore typed.
- In the meta-language (which has exactly the same *ideological,* but not ontological, resources as the object language, and so is a typed language), we allow that first-order variables range over all semantic values of any term of the object-language. In other words, from a meta-language point of view, we make all higher-order entities of the object-language collapse to first-order objects (this amounts to taking nominalization seriously). The aim of this is to avoid an ideological hierarchy with its expressive limitations.
- The effect of collapsing down any higher-order object of the object language to a first–order object of the meta-language is that the domain of the meta-language results more comprehensive than the domain of the object language. This is a result of Cantor's theorem according to which there are more concepts than objects in a language (each concept determines a subset of objects of the domain of the language: those objects that instantiate it)
- Of course, the basic logic is a modal logic, where the modal operator must be taken as primitive, as argued above. We shall see that the modal framework blocks the derivation of Russell's paradox for sets.
- Another important effect of taking nominalization seriously is to allow self-referential predicates. So the typed-theoretic response to Russell's paradox is not allowed to us. Of course, since our language is typed, a sentence such as R(R) is ill-formed. Consequently, we need to nominalize the predicate and apply the predicate to its nominalization. We shall argue that, if we take seriously the idea that the irreducibility of role should explain the irreducibility of concepts to objects, then this allows us to impose some constraints on the behavior of nominalization that, in turns, allows us to stop a form of Russell's paradox for concepts.
- In this framework, we are going to develop a theory of concepts that exploits a primitive modal operator to allow absolute generality in the absence of a definite absolute domain (set or plurality).

3. Towards a theory of concepts

3.1. The type hierarchy

Base clause: e (type of terms for individual).

ee (type of plural term). These are the only two underived type.

Induction clause: for any types $t_1, \dots, t_n$, there is the derived type $< t_1, \dots, t_n >$ of terms for relations between things of type $t_1, \dots, t_n$.

Exclusion clause: there are no other types.

Let's clarify the meaning of these definitions. Each name for an individual (a name for a first-order object) is of type e, while a name for a plurality of individuals (a name for a plurality of first-order objects) is of type ee. A monadic singular predicate is of type $< e >$ (it is a one place relation that can be instantiated by a singular term), while a monadic plural predicate is of type $< ee >$ (one place predicate to be instantiated by a plural term). Monadic singular and plural predicates express concepts. A monadic singular predicate of predicate is of type $<< e >>$. A monadic plural predicate of predicate is of type $<< ee >>$. A sentence is of type $<>$ (which means that a sentence is seen as a zero-place relation). From an intuitive point of view, a relation, whose term is of type $< t_1, \dots, t_n >$, can be seen as a function from a set of objects to a set of *n*-tuples (of those objects) of types $t_1, \dots, t_n$.

Why admit plural quantifiers and pluralities if we already admit standard second-order logic? Hasn't first-order plural logic been developed as an (alternative) interpretation of second-order logic? Certainly plural logic may be considered simply as an interpretation of higher-order logic alternative to Quine's interpretation of it as "set theory in sheep clothing", or alternative to the predicativist interpretation (with "predicativist interpretation" I mean Williamson's interpretation of higher-order logic as irreducible quantification into predicate position); however, there are a number of reasons that suggest considering plural interpretation not as an alternative, but as an independent way of understanding reference. First of all, natural language contains both singular and plural terms, and while plural quantification is quantification into plural term position, second-order singular quantification is quantification into predicate position: this grammatical distinction should motivate us to raise some doubts on the reducibility of one form of reference to the other. Secondly, the interation between pluralities, sets and concepts is an interesting one, which is worth studying on its own right: as argued above, we need to admit plurals if we do not want the indefinite extensibility thesis to collapse to the (trivial) claim that there is no universal set. Thirdly, and more importantly, plurals variables and predicates seems to have different modal profiles. The extensional nature of pluralities *suggests* that the natural reading of plurals variables is a rigid one: for any thing and any things, it is not contingent whether the former is one of the latter (Williamson [2013]). On the contrary, it seems simply false that a predicate necessarily applies to the objects to which it actually applies. For example[202], the following sentence

(5) If anything could have been wet then it is wet

seems obviously false. My pullover could have been wet, but luckily it is not. A first-order formalization of it comes out false:

(2)$\forall x(\Diamond Wx \rightarrow Wx)$

[202] The example is taken from Williamson [2003]; see also Williamson [2013], pp. 241-242.

We can now generalize (1) and (2) to obtain:

(1') If anything could have been X then it is X.

(2') $\exists X \forall x(\Diamond Xx \rightarrow Xx)$

Also these second-order versions seem to be false.

However, a first-order plural translation of both (1) and (2) turns out to be true:

(1'') Any things are such that if anything could have been one of the wet things, then it is one of the wet things.

(2'') Any things are such that if anything could have been one of the things such that X, then it is one of the things such that X.

Given the rigid reading of plurals, (1'') and (2'') seem true: for any thing and any things is not contingent whether the former is one of the latter (Williamson [2013]).

However, the rigidity of plurals may be put into question on the basis that it is not derivable from their extensional nature alone (the axiom of extensionality is not enough to derive the rigid behaviour of plurals). One may thus try to develop a theory in which plurals are non-rigid, which means that they will be intensional entities very similar to concepts. In particular, our interpretation of indefinite extensibility as a phenomenon that shows the irreducibility of intensions to extensions may pave the way for this approach: if the problem is the reduction of intensions to extensions, why not interpret plurals in a more intensional way to deal with the paradoxes? Even though this is technically possible, there are a number of reasons why it is better to avoid this path. First of all, as mentioned above, the natural reading of plurals is a rigid one, which suggests that a non-rigid reading will sound artificial or at least odd. Secondly, we already have intensional entities (i.e. concepts), which suggests that the work of non rigid plurals can be carried out by concepts. From this perspective, the non-rigidity of plurals does not seem necessary at all to capture our interpretation. Moreover, if we add that the rigid reading of plurals is the standard reading in the debate on absolute generality, not only would a non-rigid reading be unnecessary, but it may be the cause of confusion that it is better to avoid. For such reasons, we prefer to treat plurals as rigid[203].

In conclusion, there seem to be good reasons to view plural quantification as a distinct form of quantification from standard second-order quantification, and therefore to allow both kinds of reference in our theory.

3.2. The language $L^{\Diamond}$

The language $L^{\Diamond}$ contains the following symbols:

[203] See Linnebo [2016] for the analysis of three different arguments for the claim that pluralities must have a rigid modal profile.

9. *Connectives* $\sim$, $\wedge$ and the *quantifier* $\forall$ (the other connectives and the existential quantifier are defined as usual). There is also a *primitive modal operator* $\Box$ (and its dual: $\Diamond = \sim\Box\sim$);
10. *Constants* $\ulcorner c_i^t \urcorner$ $(i \geq 1)$, for each type t.
11. Singular variables $\ulcorner x_i^t \urcorner$ $(i \geq 1)$ for each type t.
12. Plural variables $\ulcorner xx_i^{ee} \urcorner$ $(i \geq 1)$ of type ee.
13. Non-logical predicate letter $\ulcorner P_i^{<t_1,\dots,t_n>} \urcorner$ $(i \geq 1)$ for each type $< t_1, \dots, t_n >$
14. The constant identity predicate $\ulcorner = \urcorner$ of type $< e, e >$; the predicate 'is one of' $\ulcorner \prec \urcorner$ of type $< e, ee >$; the membership predicate $\ulcorner \in \urcorner$ of type $< e, e >$, and the predicate '...is an instance of...' $\ulcorner \eta \urcorner$ of type $< e, e >$.
15. The lambda operator: if ϕ formula and x is a variable, then $\ulcorner \langle \lambda x. \phi \rangle^{<t_1,\dots,t_n>} \urcorner$ is a predicate abstract for each type $< t_1, \dots, t_n >$ (the free variable occurrence of $\langle \lambda x. \phi \rangle$ are the ones of ϕ except for the variable x, which is bound). Conversely, if $\langle \lambda x. \phi \rangle^{<t_1,\dots,t_n>}$ is a predicate abstract and t is a term, $\ulcorner \langle \lambda x. \phi \rangle(t) \urcorner$ denotes a formula (the free variables of $\langle \lambda x. \phi \rangle(t)$ are those of $\langle \lambda x. \phi \rangle$ together with all free variables of t).

3.2.1 Terms and formulas

1. Constants and variables are terms of type t.
2. Atomics: if $\ulcorner P, p_1, \dots, p_n \urcorner$ are of types $< p_1, \dots, p_n >, p_1, \dots, p_n$ respectively, then $\ulcorner P(p_1, \dots, p_n) \urcorner$ is an atomic formula (of type $<>$).
3. Negation: if A is a formula, then $\ulcorner \sim A \urcorner$ is a formula.
4. Implication: if A and B are formulas, then $\ulcorner A \rightarrow B \urcorner$ is a formula.
5. Generalization: if A is a formula and v is a variable of any type, then $\ulcorner \forall v A \urcorner$ is a formula.
6. Necessitation: if A is a formula, then $\ulcorner \Box A \urcorner$ is a formula.
7. Φ is a sentence iff every occurrence of a variable is bound.

3.3. A deductive system for language $L^{\Diamond}$:

The following is a deductive system based on the language $L^{\Diamond}$, consequently each axiom is typed:

1. Any instance of the following schemas of propositional calculus:

(PL1) $\varphi \rightarrow (\psi \rightarrow \varphi)$
(PL2) $(\varphi \rightarrow (\psi \rightarrow \chi)) \rightarrow ((\varphi \rightarrow \psi) \rightarrow (\varphi \rightarrow \chi))$
(PL3) $(\sim\varphi \rightarrow \psi) \rightarrow ((\sim\varphi \rightarrow \sim\psi) \rightarrow \varphi)$

2. Instances of the following schema for quantifiers (first-order, plural, and higher-order)[204]:

[204] Any variable must be taken as typed. I have not made the type of variables explicit in order to improve readability of formulas.

Universal Instantiation: $\forall x \phi \rightarrow \phi(y/x)$, provided that y is correctly substitutable[205] for x in ϕ;
Universal distributivity: $\forall x(\phi \rightarrow \psi) \rightarrow (\phi \rightarrow \forall x \psi)$, provided that x does not occur free in ϕ;

3. Comprehension axioms ($HO - Comp^{\Diamond}$) for higher-order quantifiers: $\exists X \Box \forall x(Xx \leftrightarrow A)$ with X not occurring free in A. This is restricted to predicative instances of A[206].
4. λ −conversion restricted to predicative instances : $\lambda x. P(x)(t) \leftrightarrow P(t)$
5. S4.2-axioms :

The K-axiom: $\Box(\phi \rightarrow \psi) \rightarrow (\Box\phi \rightarrow \Box\psi)$;
Reflexivity: $\Box\phi \rightarrow \phi$
Transitivity: $\Box\phi \rightarrow \Box\Box\phi$
Directedness (G): $\Diamond\Box\phi \rightarrow \Box\Diamond\phi$

6. Rules of inferences

Modus Ponens: $\varphi, \varphi \rightarrow \psi \vdash \psi$.
Universal Generalization: $\phi \vdash \forall x \phi$
Necessitation: $\phi \vdash \Box\phi$

7. Axioms for the constant predicates $\ulcorner = \urcorner, \ulcorner \prec \urcorner, \ulcorner \in \urcorner$, and $\ulcorner \prec \urcorner, \ulcorner \eta \urcorner$

(Ident.) $c_i^t = c_j^t \leftrightarrow \forall X(X(c_i^t) \leftrightarrow X(c_j^t))$

(Rgd $\prec$) $x \prec xx \rightarrow \Box(x \prec xx)$

$\sim (x \prec xx) \rightarrow \Box \sim (x \prec xx)$
$\forall u(u \prec xx \rightarrow \Box\Theta) \rightarrow \Box\forall u(u \prec xx \rightarrow \Theta)$ restriction of BF[207] to the predicate $\ulcorner \prec \urcorner$.

(Rgd $\in$) $x \in y \rightarrow \Box(x \in y)$

$\sim (x \in y) \rightarrow \Box \sim (x \in y)$

[205] The term y is correctly substitutable for x in ϕ if x does not occur free in any sub-formula of ϕ beginning with $\forall y$.
[206] Is not this restriction in contrast with what we said in the first chapter about impredicative definitions? There we said that we do not want to avoid the paradox by abandoning impredicative definitions. However, here there is no tension between this restriction and that desideratum because, as we are going to explain in §§4, 5 and 6, we have room for impredicative definitions when we give the semantics of the object-language. There we are going to allow nominalization (i.e. we allow that the first-order meta-linguistic variables range over all object-language's semantic values of any type), which has the effect of reintroducing in the theory impredicative definitions. See §§ 4, 5 and 6 for more details on this.
[207] BF stands for Barcan Formula: $\forall x \Box Fx \rightarrow \Box\forall x Fx$. What the clause means is simply that pluralities and sets are inextensible. If we extend a plurality or a set, what we obtain is a different plurality and a different set, respectively.

$\forall u(u \in y \to \Box\Theta) \to \Box\forall u(u \in y \to \Theta)$ restriction of BF to the predicate ⌜ $\in$ ⌝

(Instan.) $\eta(x, p) \leftrightarrow P(x)$ (to be read as 'x is an instance of property p if, and only if the concept P applies to x)[208], where P is the concept expressed by the predicate ⌜ P ⌝, while p is the property to which the nominalization of ⌜ P ⌝ refers to.

8. Proper axioms of the theory of concepts

($HO - Comp^{\Diamond}$) $\exists X\Box\forall x(Xx \leftrightarrow A)$ with X not occurring free in A (where A is restricted to predicative instances).
($P - Comp$) $\exists xx\forall u(u \prec xx \leftrightarrow A)$ with xx not occurring free in A (here we do not need the modal operator in front of the universal quantifier since the rigidity of plurals)[209].
(Collapse$^{\Diamond}$) $\forall xx\Diamond\exists y\forall u Form(xx, y)$ where $Form(xx, y) =_{def} \forall u(u \prec xx \leftrightarrow u \in y)$.
(Identity for concepts) $\lambda x. A = \lambda x. B \leftrightarrow \Box\forall u(A(u) \leftrightarrow B(u))$

3.4 Some brief comments

a) The fact that we have only the non-modal version of Plural Comprehension is due to the fact that concepts are intensional entities, while pluralities are not only extensional entities, rather they are also modally rigid. With the word of Linnebo & Shapiro [2017], the position we are developing here is a sort of *liberalist potentialist*, which is characterized by two claims: firstly, the individuation of objects falling under an indefinitely extensible concept is indefinitely extensible, i.e. we can go on individuating new objects indefinitely; secondly, there are definite truths about these objects. These truths are those which depends on how the concepts have been characterized.
b) The axiom that states the identity conditions for concept is a modalized version of Basic Law V. We are going to say more on modalized version of abstraction principle later on. For the time being, we just notice that modalized abstraction principles play a central role in our theory, in particular with regard to how we should understand the central notion of abstraction.
c) Why S4.2? S4 is the most appropriate modal logic to deal with indefinite extensibility, because the relation between extensions of the same indefinitely extensible concept is reflexive (any set or plurality is a subset or a sub-plurality of themselves) and transitive. The G-axioms captures the further idea that given two different extensions A and B of an IE-concept, if it happens that it is not the

[208] We are going to explain in details the relation between a concept and its correspondent property when we are dealing with nominalization. The idea is simply that when we nominalize a predicate that expresses a concept, we obtain a noun that refers to a property, which is thus a first-order object.
[209] There are two important restrictions to Pl-CP. We need plural resources just to state our definition of indefinite extensibility, which means that we need only pluralities of objects, and not higher-order pluralities. In other words, we only need pluralities of type *ee.* Therefore, Pl-CP is restricted to type *ee*. There is no a version of Pl-CP for each type of the hierarchy. The second restriction is motivated by the fact that, as explained in chapter 5, we are not willing to accept empty pluralities: so the predicate A must be restricted to predicates with at least one instance (i.e. empty predicates are excluded). However, we admit impredicative instances of Pl-CP.

case that $A \subseteq B$ or that $B \subseteq A$, then there is an extension C such that $A \subseteq C$ and $B \subseteq C$.

3.5 A theorem about modalized formulas

The fact that our underground logic is S4.2 makes available to us a theorem (proved by Linnebo 2010) concerning the behavior of modalized quantifiers. Let us call $TC^{\Diamond}$ the theory based on $L^{\Diamond}$ and the deductive system of § 3.3. In order to prove the theorem, we must restrict the theory to assure that every atomic predicate is stable. This is straightforward from the axioms of point 7 above for the constant predicates $\ulcorner = \urcorner$, $\ulcorner \prec \urcorner$, $\ulcorner \in \urcorner$, and $\ulcorner \prec \urcorner$. For the constant predicate $\ulcorner \eta \urcorner$ we must assure that the predicate P in the axiom (Instan.) $\eta(x, p) \leftrightarrow P(x)$ is stable. But this is not a problem, since the theorem regards modalized quantifiers, which are introduced to allow generality over indefinitely extensible concepts. But, according to our definition of indefinite extensibility, every indefinite extensible concept is stable. Therefore, the theorem can be read with regard to indefinitely extensible concepts only. Let $\vdash_{TC^{\Diamond}}$ be the relation of provability in $TC^{\Diamond}$, and $\vdash_{TC}$ be the relation of provability restricted to the non-modal fragment of $TC^{\Diamond}$. If ϕ is a formula, we call $\phi^{\Diamond}$ the result of substituting every non modalized quantifier in ϕ with a modalized quantifier ($\forall$ will be replaced by $\Box\forall$, while $\exists$ will be replaced by $\Diamond\exists$). A formula is fully modalized if and only if all its quantifiers are modalized. Then we have the following:

Theorem (Linnebo 2013): Let $\phi_1, \dots, \phi_n$ and ψ be non-modal formulas in the higher-order language $L^{\Diamond}$. Then we have

$$\phi_1, \dots, \phi_n \vdash_{TC^{\Diamond}} \psi \quad \text{iff} \quad {\phi^{\Diamond}}_1, \dots, {\phi^{\Diamond}}_n \vdash_{TC^{\Diamond}} \psi^{\Diamond}$$

Proof: by induction on the length of formulas (see Linnebo [2013])[210].

What the theorem guarantees is that the modalized quantifiers behave, from a proof-theoretic point of view, as the standard quantifiers. This is a positive result since they were introduced to allow generality in the absence of an all-inclusive domain, and therefore the theorem assures us that they depart from standard quantifiers just as much as we need to guarantee absolute generality. More specifically, this means that the modalized quantifiers respect all the laws of classical logic. They diverge from standard quantifiers only with regards to classical semantics. This clearly shows that indefinite extensibility challenges the way classical semantics has been developed and not the laws

[210] Linnebo develops the theorem for first-order quantifiers. However, it is straightforward to extend it also to HO-quantifiers, if formulated in terms of free HO-logic (see Linnebo & Shapiro [2017], §7).

of classical logic[211]. But the theorem also allows us to interpret standard quantifiers in many ordinary theories as implicitly modalized[212].

4. A glimpse on semantics

4.1. A translation from higher-order to a first-order language

We argued above that to avoid the expressive problems of the ideological hierarchies, the meta-language must have the same ideological resources as the object language, which means it is a typed higher-order modal language. Moreover, we do not have to take types very seriously; in other words, we allow the first-order variables of the meta-language to vary over *all* semantic values of any expression of the object-language, no matter the type.

The effect of allowing the first-order variables of the meta-language to range over all semantic values of the object-language is that the domain of the meta-language must be more comprehensive than the domain of the object-language. Let's see this with a toy example. For a sake of simplicity, let's consider the non-modal monadic fragment of $L^{\Diamond}$ (that is, the non-modal fragment of $L^{\Diamond}$ with just monadic predicates) and let's give the semantics for it.

Let's call D_{OL} the domain of the object-language (i.e. the domain of the monadic non-modal fragment of $L^{\Diamond}$). This domain is made of everything referred to by terms of type e of the object-language. Therefore in D_{OL} there will only be individuals (first-order object). Let's call D_{ML} the domain of the meta-language. Since we allow the first-order meta-variable to range over all semantic values of any type of the object-language, we shall suppose that in D_{ML} there are only first-order objects (i.e. objects referred to by terms of the meta-language type e). In order to be able to state the semantics we start by imposing that $D_{OL} \subseteq D_{ML}$ [213]. We are going to allow the first-order variables of the meta-language to range over all semantic values of the object-language in the following way:

- The semantic values of terms for pluralities of objects in D_{OL} will be sets in D_{ML};
- The semantic values of predicates for concepts whose extensions are defined in D_{OL} will be properties in D_{ML}.

For the sake of simplicity let's suppose that in D_{OL} there are no sets and properties, but just individuals (what we may call urelements: in this sense, a urelement is neither a

[211] This thesis will clearly emerge in the Appendix which deals with to Dummett's reading of indefinite extensibility.

[212] The theorem clearly requires that the atomic formulas are stable. Stability corresponds to the formula $\phi \rightarrow \Box\phi$ which is well-known to produce the collapse of the modalities. In fact, to prove the theorem one has to prove a previous lemma, that states that, given the conditions specified above for the theorem, the formulas $\phi, \Box\phi$ and $\Diamond\phi$ are equivalent.

[213] This seems to be a minimal requirement to be able to give the intended semantics of a language. See Williamson [2003].

set nor a property)[214]. Since the conditions imposed in D_{ML} there will be three kinds of (first-order, i.e. type e) objects, i.e. individuals, sets, and properties. These three categories are exclusive (they do not overlap).

Let's now introduce an operator σ defined in relation to plurals and concepts as follows: given any term c of type ee (that is a plural term), $\sigma(c)$ refers to the set of all and only objects that are members of the plurality to which the plural term refers; given any term of type $< t >$ (a predicate), $\sigma(c)$ refers to a property p such that p applies to all and only objects to which the concept expressed by the predicate of type $< t >$ applies. Let's now define a model for this fragment of the language.

A model is an ordered pair $< D_{OL}, I >$, where D_{OL} is the domain of the object language, and I is a function that maps each non logical constant c of type t to $I(c) \in D_{ML}$ in the following way:

- If c is of type e, $I(c) = d$, with $d \in D_{ML}$ (d is a an individual);
- If c is of type ee, $I(c) = \sigma(c)$, with $\sigma(c) \in D_{ML}$ ($\sigma(c)$ is a set);
- If c is of type $< e >$, $I(c) = \sigma(c)$, with $\sigma(c) \in D_{ML}$ ($\sigma(c)$ is a property);
- If c is of type $<< e >>$, $I(c) = \sigma(c)$, with $\sigma(c) \in D_{ML}$ ($\sigma(c)$ is a property of properties);
- …

Remind that sets, properties, properties of properties and so on are all first-order object in D_{ML}. Therefore, the domain of the meta-language is just type e domain.

Variable assignment: a is a variable assignment mapping each variable v of type t to $a(v) \in D_{ML}$. In particular we have:

- If v is of type e, $a(v) = d$
- If v is of type ee, $a(v) = \sigma(v)$ (with $\sigma(v)$ being a set)
- If v is of type $< t >$, $a(v) = \sigma(v)$ (with $\sigma(v)$ being a property)

Constant interpretation: $in_{M,a}(c) = I(c)$

Variable interpretation: $in_{M,a}(v) = a(v)$

We can now state the conditions under which sentences are true:

Identity: $M, a \vDash c_i^e = c_j^e$ is true iff $in_{M,a}(c_i^e) = in_{M,a}(c_j^e)$

Plurals: $M, a \vDash c^e \prec c^{ee}$ is true iff $in_{M,a}(c^e) \in in_{M,a}(c^{ee})$

Atomic: $M, a \vDash P^{<t_1,\dots,t_n>}(p_1, \dots, {p_n}^{t_1,\dots,t_n})$ is true iff $< in_{M,a}(p_1), \dots, in_{M,a}(p_n) > \eta(in_{M,a}(P))$

[214] Of course, this is not necessary: the object language may speak of sets and properties. But it is useful to keep the object language as simple as possible in order to grasp in details the consequences of this approach.

Conjunction: $M, a \vDash \varphi \wedge \phi$ is true iff $M, a \vDash \varphi$ and $M, a \vDash \phi$

Negation: $M, a \vDash \sim\varphi$ is true iff $M, a \vDash \varphi$ is false

Implication: $M, a \vDash \varphi \rightarrow \phi$ is true iff either $M, a \vDash \phi$ is true or $M, a \vDash \varphi$ is false

Generalization: $M, a \vDash \forall v^{t_1,\ldots,t_n} A$ is true iff $M, a[v/k] \vDash A$ for all k, such that $k \in D_{ML}$

Once given the semantics for this fragment of $L^{\Diamond}$, we can prove the following fundamental result.

Theorem: if the cardinality of the domain D_{OL} is α (with α either finite or infinite), then the cardinality of the domain of D_{ML} is at least $2^{\alpha} - 1$ elements.

Proof: suppose the cardinality of D_{OL} is α. By the generalization of Cantor's theorem[215], if the cardinality of D_{ML} is α, then the cardinality of the subpluralities of D_{OL} is $2^{\alpha} - 1$, where the sign ⌜ – ⌝ denotes subtraction defined on both finite and transfinite (cardinal) numbers[216]. By the plural clause $M, a \vDash c^{e} \prec c^{ee}$ is true iff $in_{M,a}(c^{e}) \in in_{M,a}(c^{ee})$, which means that there must be an injective function from pluralities of objects of D_{OL} and sets in the domain D_{ML} (just consider the function that maps some objects xx_i to the set whose elements are exactly the xx_i[217]). This concludes the proof.

From a linguistic point of view, what happens with the translation is that we nominalize predicates and plural terms that respectively denote concepts and pluralities in the object-language, and transform them in nouns that respectively denote properties and sets. Properties and sets are therefore first-order objects that can be seen as the *reification* of concepts and pluralities. With reification I just mean that we come to regard an item as if it were a first-order object (see Linnebo [2017b]). What we have illustrated before just amounts to the idea that it is the self-referential structure of language that is responsible for reification. It is when we reflect on our own language that we treat the semantic values of linguistic expressions as they were first-order objects. This fact is fundamental to understand the nature of indefinite extensibility. Both predicates and plural expressions play a central role in our practice of referring to objects. A plural expression is just an expression that allows reference to several individuals at once. Also a predicative expression can be seen as taking part in the way we refer to objects. In a sentence as "the table is brown" we manage to refer to the table in virtue of the meaning of the noun 'table'; however, the predicative expression 'is

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215 This theorem can also be proved with regard to the collapse of concepts into properties, and in this case it requires Cantor's theorem, not its generalization.

216 Subtraction on finite number is defined as usual: $x - 1 = y$ if and only if $x = s(y)$, where $s(x)$ denotes the successor of x. Subtraction on transfinite cardinal number is defined as follows: for every transfinite cardinal number a, $a - 1 = a$. The reason why the generalization of Cantor's theorem gives us $2^{\alpha} - 1$ as a result is due to the absence of an empty plurality. For the generalization of Cantor's theorem see Hawthorn & Uzquiano [2011].

217 Strictly speaking we cannot define a function from pluralities to sets, because pluralities are not objects. What we need to fix the problem is just an encoding procedure. For more details see Uzquiano [2015].

brown' characterizes the perspective of reference: we intend the table in so far as it is brown. In general, the role of *meaning* is that of singling out and characterize the object of reference. As Frege claimed, it is the *Sinn* (intension) that determines the *Bedeutung* (the object of reference). What we are proposing is therefore that as soon as we treat the meanings of different expressions as first-order objects, we produce an expansion of the domain of the language. As soon as we reflect about the language, that is when we reflect about the structures that make reference possible, it is natural to treat these structures as if they were objects. In other words, it is the self-referential structure of language which is responsible for the expansion of the domain. According to this view, indefinite extensibility is rooted in a semantic phenomenon: what makes the universe expand is just the fact that semantic values are treated as objects (values of first-order variable) in the meta-language.

Admittedly, these ideas are just sketched. In § 7, we shall try to make them more rigorous by using modalized versions of abstraction principles.

4.2. Self-reference and expansion

The toy model above shows quite nicely that the expansion of the domain is rooted in the self-referential structure of the language. By reflecting on (a piece of) language, we treat the semantic values of expressions as they were (first-order) objects, and in this way "we expand" the domain of the object language. Letting the first-order variables of the meta-language varying over all semantic values of the object language (where the object and the meta-language share the same ideological resources) has the aim of mimic the self-referential structure of natural languages. What happens with natural languages is that we reflect on a (piece of) language within the language itself. Tarsky himself acknowledges that natural languages as English are semantic closed, which means that they contain the T-schema (truth-schema) for each sentence of the language. There is no a more powerful meta-language from which to state the truth conditions for sentences of a natural language. In this sense, natural languages are essentially self-referential. What our toy model shows is that, to preserve consistency, this feature of languages implies an expansion on the domain of objects of the language. It goes without saying that the same happen when we give the semantics of the meta-language in a meta-meta-language, and so on. What we obtain is a sequence of languages, each of them with the same ideological resources, whose domains are more and more comprehensive:

$$D_{OL} \subseteq D_{ML} \subseteq D_{MML} \subseteq \cdots \subseteq D_{MM\ldots L} \subseteq \cdots$$

In this setting, the quantifiers are considered to be unrestricted inside the domain of each language, so that they can range over all objects present in those domains. But since each domain can be expanded, no quantifier is absolutely general, that is no quantifier simultaneously ranges over all semantics values of any languages (or, which is the same, no quantifier ranges over all objects in any domain). However, in the toy model we worked within a non-modal fragment of the language $L^{\diamond}$. Of course, adding the modal operator does not change the fact that allowing the first-order variables of the meta-language to range over all semantics values of the object-language produces an expansion of the domain of objects of the language. But the modal operator gives us the resources of rescuing absolute general claims in the absence of an absolute general domain. Thanks to the open-endedness of the modalized quantifiers (which means – I remind the reader – that the truth of a sentence with the modalized quantifier is domain-independent, in the sense that it does not depend on which objects are considered to be in the range of the quantifiers), we can express within a particular language sentences which expresses true propositions in relation to any another language. If in the first language (the one based on D_{OL}) I utter the sentence $\Box\forall x(x = x)$, even thought the variable x just ranges over the objects in D_{OL}, I managed to express an absolute general claim, because I know that its truth does not depend on which objects there are in D_{OL}. The modalized quantifier gives us the resources of asserting – inside a language - absolute general claims that are true however you expand the domain.

4.3. Nominalization and impredicativity

Given the role of nominalization in our theory, we must define some axioms that describe the behavior of nominalization. We shall start with an axiom that defines when a noun is the nominalization of a predicate:

$Nom(P, x) =_{def} \ulcorner p \urcorner$, where $\Box\forall y(Py \leftrightarrow y\,\eta\,p)$

$Nom(P, x)$ can be read as 'x is the nominalization of the predicate P': x is the name that refers to the first-order property p, which is instantiated by exactly those objects y for which Py is true. It must be notice that $Nom(P, x)$ is a formula of the meta-language, while the formula $\Box\forall y(Py \leftrightarrow y\,\eta\,p)$ belongs to the object language. How does nominalization behave? A natural suggestion is to think that the following inheritance principle should turn out to be true:

$$Nom\,(P, x) \rightarrow \Box Nom\,(P, x)$$

If x is the nominalization of P, then necessarily x is the nominalization of P (with the loose talk of possible worlds, we could say that if x is the nominalization of P in a world, then x is the nominalization of P in any world). However, as I shall argue below, there are cases where such a principle fails, and therefore we cannot assume it as a further axiom. The failure of this principle is fundamental for our theory of concepts, since our response to the revenge paradox is based on it. In other words, assuming $Nom\,(P, x) \rightarrow \Box Nom\,(P, x)$ as a further axiom would have made our theory inconsistent[218].

The plausible principle that must be abandoned in order to stop the paradox is therefore the inheritance principle. Since its plausibility, one should also give some reasons why this principle seems plausible despite it fails. The reason is

[218] If the inheritance principle fails for a certain concept P, then $Nom(P, x)$ is not a stable concept. However, this does not imply that P is not stable. As we argue below, the Russell's concept R is stable (it is indefinite extensible), but its nominalization is not stable, because the inheritance principle fails in this case.

straightforward: in the majority of cases, the principle in fact holds. It only fails in relation to particular cases, where an impredicative definition is in play. Consequently, it is natural to consider the principle a plausible one.

Let us explain why the inheritance principle above may fail. In order to do that, let's begin with two examples in which the inheritance principle above is valid: the concept of being a dog and the concept of being married. Let's start with the former. If we nominalize the predicate 'being a dog', what we obtain is a noun that refers to the property of being a dog. This property does not change by changing the domain in which the nominalization is carried out. So, for such concept, we have that $Nom\ (P, x) \rightarrow \Box Nom\ (P, x)$. This is due to the fact that concepts remain the same no matter in relation of which objects they are defined (concepts are domain-independent). Let's now consider the latter example. The concept of being married requires a quantifier to be defined: being married means that *there is* someone to whom one is married. Despite the presence of a quantifier, also in this case it seems straightforward that the nominalization of the predicate 'being married' gives us a noun that refer to a property (the property of being married) which does not change if it is defined in relation to different objects. No matter what objects the quantifier in the definition of 'being married' ranges over, the property remains the same. Again $Nom\ (P, x) \rightarrow \Box Nom\ (P, x)$.

However, things are different with the concept R defined as the concept such that $\forall x\{Rx \leftrightarrow \exists F(Nom(F, x) \wedge \sim Fx)\}$. As in the examples above, also this concept remains the same no matter what objects the quantifiers present in its definition ranges over. Therefore, we could rewrite the concept in this way: $\Box\forall x\{Rx \leftrightarrow \exists F(Nom(F, x) \wedge \sim Fx)\}$[219]. As explained above, the $\Box$-opertor indicates that the truth value of the formula that follows the operator does not change if the range of the quantifier changes, and this is grounded on the fact that concepts are domain-independent. But there is a *second* aspect at work in this case. The *concept is defined by means of a totality of objects* (it is the concept that applies to the totality of *all* non-self-applying concepts). This totality is the range of the universal quantifier present in the definition of R. Since we have now two different aspects in play, we must check both of them when we carry out the process of nominalization. Let's suppose we nominalize the predicate with regard to two different sets of values over which the variables range. Let's call these two different sets w_a and w_b. What we shall obtain is the property of all non-self-applying properties in w_a, and the property of all non-self-applying properties in w_b. Since we have supposed $w_a \neq w_b$ the two properties are different. The reason is that the universal quantifier in the definition of the concept R binds the nominalization of the predicate that expresses R to the totality of non-self-applying properties over which it ranges. If it ranges over different non-self-applying properties, then the resulting nominalization will be different. Therefore, the inheritance principle $Nom\ (P, x) \rightarrow \Box Nom\ (P, x)$ fails.

[219] Later, in a more formal setting, we shall derive this formulation of the concept from our comprehension principle for higher-order predicates.

A comparison with Kaplan's distinction between *character* and *content* may help understand what is going on in such cases. Kaplan introduces the distinction by dealing with *indexicals*. According to Kaplan, indexicals have two sorts of meaning (see Braun [2016]). Suppose both Mary and John utter the sentence 'I am a philosopher'. In both cases the indexical 'I' is used by the speaker to refer to themselves: this is the *character* of the indexical, which does not change by the change of the context of the utterance. However, Mary's and John's claims express two different propositions: the first is the proposition 'Mary is a philosopher', the second the proposition 'John is a philosopher'. The *content* of the indexical 'I' changes if the context changes: in the first case 'I' refers to Mary, in the second to John.

What I am proposing is that something similar happens with concepts as R. Such concepts have a *character* that does not change and which is simply captured by their definition. However, the second aspect of the concept – the fact that it is defined by means of a totality of objects – implies that if the same character works with different totalities, it will express different *contents.*

As the example of the concept of being married shows, the failure of the inheritance principle $Nom\ (P, x) \rightarrow \Box Nom\ (P, x)$ cannot be ascribed to the presence of a quantifier in the definition of a concept. At a first (and superficial) sight, one might have thought that the presence of a quantifier was enough, because quantifiers require some objects to range over. If these objects were different, then maybe the result of nominalization should have been different. But this is not the case. The failure of the inheritance principle seems to be grounded in something deeper than just the presence of a quantifier. My suggestion consists in identifying the culprit with the *impredicativity* of concepts such as R. Roughly speaking, a concept is impredicative if it is defined by means of the totality of objects to which it belongs. R, which is a non-self-applying concept is impredicative because it is defined by means of all non-self-applying concepts. Now, with such a concept there are two different aspects in play: the first is simply the character of the concept; the second is the range of values which are taken to form the totality of non-self-applying concepts. While the former remains always the same, the latter may change producing different contents. The upshot is that, while the concept always remains the same, the result of nominalization differs according to which values constitute the range of the quantifier.

One might think that the failure of the inheritance principle $Nom\ (P, x) \rightarrow \Box Nom\ (P, x)$ is incompatible with the definition of nominalization that we gave above, according to which p is the nominalization of the predicate P when $\Box\forall y(Py \leftrightarrow y\ \eta\ p)$. In particular, one might argue that, since the $\Box$-operator allows generalization over all domains, the definition forbids the evaluation of the nominalization (of a predicate) with regards to *a specific domain*, which is, on the contrary, required by the failure of the inheritance principle. In other words, when nominalizing we must always look at the totality of all domains.

This is, of course, the wrong way of interpreting the $\Box$-operator. Since we are working with indefinitely extensible concepts, there is nothing as the totality of all domains. The same fact that the $\Box$-operator is primitive, i.e. it is not reducible to a universal quantifier over all domains, is required by the fact that there can be no maximal domain that comprehends all extensions of an indefinitely extensible concepts. To believe that the $\Box$-operator gives us the possibility of considering all domains from a sort of external perspective is simple false. Rather the $\Box$-operator allows trans-domain generality by remaining inside a particular domain. Consequently, a sentence as $\Box\forall x(x = x)$ should not be read as generalizing simultaneously over all objects in all domains; on the contrary, what it expresses is that the sentence $\forall x(x = x)$ is true no matter what domain you consider (no matter how you expand the present domain).

Under this reading the supposed incompatibility vanishes. If we are working within a domain D_1, the quantifier in $\Box\forall y(Py \leftrightarrow y\,\eta\,p)$ ranges over the member of D_1. The $\Box$-operator is an intensional operator, not a quantifier, and it simply tells us that nothing depends on working in D_1 instead of working in D_2, or D_3, etc. In this sense, the definition of nominalization does not imply that $Nom\,(P, x)$ remains always the same. On the contrary, when there is a concept which is defined by reference to a totality of objects, as in the case of impredicative concepts, it is natural to suppose that, by changing the totality when nominalizing the predicate that expresses it, also the results will be different.

Let's now go back to the axioms that govern nominalization. It is natural to require that if x and y are the nominalization of the same predicate in the same domain, then they express the same concept:

(ID-N) $\qquad \forall F\ \Box[\forall x\forall y(Nom(F, x) \wedge Nom(F, y) \rightarrow x = y]$

Of course, it seems also natural to require that it is possible that every predicate has its own nominalization, which means that we can speak of any concept; in fact, saying that we cannot speak of a certain concept implies speaking of it:

(Ex-N) $\qquad \forall F\Diamond\exists x\ Nom(F, x)$

5. Avoiding revenge!

How does our theory avoid paradox? Starting with the idea of indefinite extensibility we have factorized NCP in two different principles, and we have argued for a (particular) modal version of one of them. The two principles are the following:

$(P - Comp)$ $\exists xx\forall u(u \prec xx \leftrightarrow A)$ with xx not occurring free in A.

(Collapse$^{\Diamond}$) $\forall xx\Diamond\exists y Form(xx, y)$ where $Form(xx, y) =_{def} \forall u(u \prec xx \leftrightarrow x \in y)$.

Suppose we instantiate A in $(P - Comp)$ with the predicate '$\notin$':

$$\exists xx\forall u(u \prec xx \leftrightarrow u \notin u)$$

The plurality xx is the plurality of all objects (sets) that do not belong to themselves. The principle says that there is a plurality of all and only sets that do not belong to themselves.

(Collapse$^{\Diamond}$) says that for every plurality, possibly there is a set whose elements are exactly the members of the plurality in question. From (P-Comp) and (Collapse$^{\Diamond}$) we derive

$$\Diamond \exists y \forall u (u \in y \leftrightarrow u \notin u).$$

Let's call the set defined by this predicate r:

$$\Diamond \exists r \forall u (u \in r \leftrightarrow u \notin u).$$

To get Russell's paradox we must instantiate the universal quantifier with r. However, since we have $\Diamond \exists r$ and not $\exists r$, we have no reason to think that r is in the range of the universal quantifier $\forall u$. In fact, we can show that this is not case. Suppose, for reduction, that r is in the range of the universal quantifier $\forall u$. Then we can instantiate the quantifier with r:

$$r \in r \leftrightarrow r \notin r.$$

With easy passages we can derive a contradiction. This show that r is *not* in the range of the universal quantifier $\forall u$: we can take the union of all the sets u together with the set r to obtain a more comprehensive totality of non-self-membered sets[220].

Russell's paradox is avoided by means of our modal framework: in each domain we can consider all the non-self-membered set, then (Collapse$^{\Diamond}$) implies that possibly there is a set that contains all those non-self-membered sets: the derivation of the contradiction is then exploited to diagonalize out of the starting domain. The set exists but in a different domain.

But the most threatening paradox is a version of Russell's paradox for concepts. Our comprehension principle for concepts is $\exists X \Box \forall x (Xx \leftrightarrow A)$. Since the language is typed, we cannot have a formula as R(R). However, nominalization allows the self-reference in the case of concepts: a concept can be applied to the nominalization of the predicate that expresses it[221]. Let's define R in the following way:

$R =$ the concept that applies to all and only objects x, such that $Nom\,(X, x)$ (X is a variable for predicates that express concepts that do not apply to their nominalization).

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[220] This way of avoiding Russell's paradox is essentially the same of Linnebo [2010]. This way of avoiding the paradox shows why we can admit impredicative instances of Pl-CP. For the same reason we can admit the instance obtained by substituting A with $x = x$: $\exists xx \forall u (u \prec xx \leftrightarrow u = u)$. Since plural resources does not give us absolute generality, this instance gives us the plurality of all self-identical objects that are in the domain of the quantifiers $\exists xx$ and $\forall u$. But since each plurality can be extended, this instance can never give us the maximal plurality.

[221] This is the way in which impredicative defintions are alloed in our theroy: they emerge while speaking in the meta-language of the object-language.

The existence of such a concept is a consequence of HO-Comp. We just need to consider the condition A as the condition of being a concept that does not apply to its nominalization: $\exists F(\ Nom(F,x) \wedge \sim Fx)$. So we get:

$$\exists X \Box \forall x \{Xx \leftrightarrow \exists F(\ Nom(F,x) \wedge \sim Fx)\}$$

Since we have called this concept R, we can instantiate the existential quantifier with R:

$$\Box \forall x \{Rx \leftrightarrow \exists F(\ Nom(F,x) \wedge \sim Fx)\}$$

Before looking at the way we can block the paradox, we should just warn the reader that in what follows we have used possible worlds indexes (that is, we have considered formulas at different worlds). This must not be taken seriously as if we were reducing the primitive modal operator to quantification over worlds. The talk of worlds has the only function of making things easier to state and to be intuitively grasped[222].

Assume $w_1 \Vdash Nom(R,r)$

Case 1:

$w_1 \Vdash\ Rr$ by supposition;
$w_1 \Vdash\ \exists F(Nom(F,r) \wedge \sim Fr)$, by definition of R
$w_1 \Vdash\ Nom(R,r) \wedge \sim Rr$, by $\exists$ instantiation.

Case 2:

$w_1 \Vdash\ \sim Rr$ by supposition;
$w_1 \Vdash\ \forall F(Nom(F,r) \rightarrow Fr)$ by definition of R
$w_1 \Vdash\ Nom(R,r) \rightarrow Rr$, by instantiation of $\forall$ with R
$w_1 \Vdash\ Rr$ by modus ponens.

We have reached a contradiction: however, this was possible because we assumed that $\mathrm{w}_1 \Vdash Nom(R,r)$. Therefore, we can interpret the contradiction as a *reductio* of that assumption: r is not in world w_1. At this point we can just enlarge the domain of w_1 by adding to it r. Then we can rerun the argument above, assuming that in the new domain w_2 it is true that $Nom(R,r)$ and diagonalize out of w_2. Of course this is possible because of the failure of the inheritance principle $Nom\ (P,x) \rightarrow \Box Nom\ (P,x)$, which allows us saying that the nominalization of R in w_1 is different from the nominalization of R in w_2. The reason is simply that R is impredicative and therefore r quantifies over[223] the domain of w_1, while r' quantifies over the domain of w_2. The difference between the two is the difference between the property that applies to all and only the non self-applying properties of w_1 and the property that applies to all and only the non self-applying properties of w_2 (since $w_1 \neq w_2$, they are clearly different properties).

[222] It may be useful not to think of these worlds as ways as the actual worlds could be, but just as domains (pluralities) of objects.
[223] More precisely: in the definition of r there is a quantifier that quantifies over w_1.

What is important to notice is that the expansion of the starting domain is given by the nominalization r of the predicate R. It is by nominalizing predicates, i.e. by treating concepts as first-order objects that the domain gets expand. This is exactly what happens when we let the first-order variable of the meta-language range over all semantic values of any formula of the object-language: also in that case the expansion of the domain was caused by the reification of higher-order entities into first-order objects.

In the case of Russell's paradox for sets, the standard version of Collapse was replaced by (Collapse$^{\Diamond}$), which affirms that, necessarily for any pluralities of objects, possibly there is the set that contains it. (Collapse$^{\Diamond}$) does not affirm the existence of such a set, but just its potential existence. What if we try to reformulate Russell's paradox for concepts by means of a claim of potential existence rather than a claim of existence? In other words, what if we take the condition A to be $\Diamond\exists F(Nom(F,x) \wedge \sim Fx)$? In this case HO-Comp gives us the following concept:

$$\Box\forall x\{Rx \leftrightarrow \Diamond\exists F(Nom(F,x) \wedge \sim Fx)\}^{224}.$$

We shall now see two different ways of avoiding this revenge paradox.

First solution: Assume $w_1 \Vdash Nom(R,r)$

Case 1:

$w_1 \Vdash Rr$ by supposition;
$w_1 \Vdash \Diamond\exists F(Nom(F,r) \wedge \sim Fr)$, by definition of R
There is a $w_2 \geq w_1$ such that $w_2 \Vdash \exists F(Nom(F,r) \wedge \sim Fr)$
$w_2 \Vdash Nom(F,r) \wedge \sim Fr$, by $\exists$ instantiation.

But now we do not have any reason to think that $Nom(R,r)$ is equal to $Nom(F,r)$ (this is possible because we have rejected the inheritance principle: $Nom\ (P,x) \rightarrow \Box Nom\ (P,x)$). So no contradiction arises from the supposition above.

Case 2:

$w_1 \Vdash \sim Rr$ by supposition;
$w_1 \Vdash \Box\forall F(Nom(F,r) \rightarrow Fr)$ by definition of R
$w_1 \Vdash \forall F(Nom(F,r) \rightarrow Fr)$, by reflexivity of the accessibility relation
$w_1 \Vdash Nom(R,r) \rightarrow Rr$, by instantiation of $\forall$ with R
$w_1 \Vdash Rr$ by modus ponens.

In this case we have derived the negation of our supposition. Therefore, the first solution blocks the paradox by blocking the first case: supposing that Rr does not lead to any contradiction. Of course, this is possible only if r (the nominalization of R) is not

[224] One might consider other options for the revenge phenomenon: for instance, R defined by means of $\Box\forall x\{Rx \leftrightarrow \Box\exists F(Nom(F,x) \wedge \sim Fx)\}$ (so by means of two box operators). I am not going to spell out the details, but it is easy to see that what we would obtain is one of the two solutions available for the standard case we are considering. In neither case we get a revenge phenomenon.

true in w_2. Had it been true in w_2, then in w_2 we could have derived $\sim Rr$, which brings us to Rr by means of Case 2. At this point, case 1 would have brought us to affirm $\sim Rr$ in w_3 and so on without an end. In this scenario, a *regress* would have started with the consequence of not being able to establish Rr or $\sim Rr$ in any world (domain). But there is no reason to suppose that r is present in w_2 and, consequently, the regress cannot arise.

Second solution: Assume $w_1 \Vdash Nom(R, r)$

Case 1 : (as above, but now we argue that r belongs to w_2).

$w_1 \Vdash Rr$ by supposition;
$w_1 \Vdash \Diamond\exists F(Nom(F, r) \wedge \sim Fr)$, by definition of R
There is a $w_2 \geq w_1$ such that $w_2 \Vdash \exists F(Nom(F, r) \wedge \sim Fr)$
$w_2 \Vdash Nom(F, r) \wedge \sim Fr$, by $\exists$ instantiation.

We now argue that r belongs to w_2. The argument is given by the following interpretation of Case 2. In this way we can conclude $w_2 \Vdash \sim Rr$ (which gives us the regress above).

Case 2: (the derivation is the same as above)

$w_1 \Vdash \sim Rr$ by supposition;
$w_1 \Vdash \Box\forall F(Nom(F, r) \rightarrow Fr)$; by definition of R
$w_1 \Vdash \forall F(Nom(F, r) \rightarrow Fr)$; by reflexivity of the accessibility relation;
$w_1 \Vdash Nom(R, r) \rightarrow Rr$ by instantiation of $\forall$ with R
$w_1 \Vdash Rr$ by modus ponens.

But in this case we have room for maneuver. In fact, since we started with the supposition that $\sim Rr$, it may be that r does not fall under R in w_1, because r does not belong to the domain of w_1[225]. If so, we can enlarge $w_1$by adding r to it. We thus obtain a world w_2, such that $w_2 \geq w_1$ and r belongs to w_2. This interpretation makes problematic Case 1. But now, we cannot derive any contradiction from Case 2. In fact, r is the nominalization of R with regards to w_1 and it belongs to w_2. Let us now repeat the same argument in w_2. Suppose $w_2 \Vdash Nom\ (R, r')$ with r' being the nominalization of R with regard to w_2 , then by means of Case 2, we can conclude that r' belongs to w_3. And so on. Since r with regards to $w_1$is the property that applies to all and only properties of w_1, we can say that the property that applies to all and only properties of w_1 belongs to w_2 and does not apply to itself; the property that applies to all and only properties of w_2 (which is r') belongs to w_3 and do not apply to itself, and so on. In this case, we have a clear indefinite extensibility phenomenon. Of course, also this solution is made possible because the inheritance principle above fails.

225 As we shall explain below when we are going to expose the semantics of the theory, if the term *a* of an atomic formula *Pa* lacks a referent, then we consider the formula *Pa* simply false. Consequently, in this second solution, when we claim that r does not belong to w_1, we have as a result that $\sim Rr$ is simply false.

The two solutions are both available to us, but they are incompatible each other. The reason of their incompatibility is that the second solution implies that r (the nominalization of R at w_1) belongs to w_2. If so, in the Case 1 of the first solution, we can derive the regress above. In fact, that case started by supposing $w_1 \Vdash Rr$, and ended by claiming $w_2 \Vdash Nom(F,r) \wedge \sim Fr$. The regress we spoke above was avoided because we did not have any reason to suppose that r belongs to w_2 and therefore we could not conclude $w_2 \Vdash \sim Rr$. But Case 2 of the second solution exactly claims that r belongs to w_2, and consequently makes Case 1 of the first solution problematic. Therefore, we must choose. One can be tempted by the second one, because it provides the same way out as the solution of Russell's paradox for sets; however, this would be a too quick reason; in fact, one could argue that the reason why the first solution does not produce an indefinite extensibility phenomenon can be explained by noticing that the modal formulation already provided a framework where the expansion of the universe is taken into account. So we do not need indefinite extensibility to expand the universe.

In any case, our preference goes to the second solution, because it seems to us that only this solution may explain why the nominalization of R at w_1 (r) is different from the nominalization of R at w_2 (r'). The reason is simply that r quantifies over the domain of w_1, while r' quantifies over the domain of w_2. The difference between the two is the difference between the property that applies to all and only the non-self-applying properties of w_1 and the property that applies to all and only the non self-applying properties of w_2. It is therefore clear while in this case $Nom\ (P,x) \nrightarrow \Box Nom\ (P,x)$ must be the case.

6. Defending the conception of concept

In this paragraph we would like to defend the intuitive notion of concept which we have worked with from some general objections that can be raised against it. First of all, we recall the general conception: a concept $P(x)$ is specified by means of some condition of application $-\lambda x.\phi(x)$ -, which tells us to what individual objects we can apply the concept. More accurately, a condition of application tells us that, necessarily, if some individual t satisfies the condition, then the concept applies to it: $P(t)$.

Linnebo [2006], p. 157 expresses a close idea about the nature of concepts in the following way (Linnebo actually speaks of properties, but you can simply substitute any occurrence of 'property' with an occurrence of 'concept'): ‹‹*it is not essential to this property that it applies to precisely those objects to which it in fact applies. Rather, it is essential to the property that it applies to all and only such objects as satisfy the condition associated to the property*››. The idea is that concepts are determined not by the elements that as a matter of fact instantiate them, but by the fact that in different possible situations they apply to all and only objects that satisfy the condition associated to it. This means to consider concepts as purely intensional entities.

One of the consequences of this notion of concept is that, despite the fact that the extension of a concept may change, the intension does not change. We can go on finding more and more ordinals, but the concept 'being an ordinal' remains always the same.

This might seem to be wrong. One could claim – following some ides of the latter Wittgenstein - that the application of a concept to a new instance necessarily brings some modification in the intension of the concept. As a matter of fact, meanings of the words change over time, and to account for this phenomenon it seems plausible to hypnotize that application of a predicate to a new item brings with itself some small modification in the intension of the concept expressed by that predicate. We are now going to look at two different ways of understanding this idea, the first one more radical (and easier to criticize), while the second one less radical, and better motivated. Finally, we shall argue that our account is able to explain the changes in the meanings of words.

The first interpretation considers the idea that each application changes the intension of a concept in a literary way. If a single application implies the modification of the intension, then the intension is defined by means of the objects that, as a matter of fact, fall under the concept. In turn, this means that something like the axiom of extensionality should be regarded to capture the nature of intensions: to each extension, there corresponds a different intension. However, such a radical view seems to be very implausible, since it brings absurd consequences. The first problem is that this account is dangerously close to the NCP, which states an equivalence between sets and concepts. Secondly, in this scenario a concept would just be a set or a plurality of elements, and as soon as we add or subtract even one single individual from this set or plurality, what we obtain is a different set or plurality, which in this context means a different concept. The straightforward implication is that it would be impossible to learn concepts. To learn a concept would require learning each of its instantiations (otherwise we would simple learn a different concept). If we did not know all the instances, then, according to the axiom of extensionality (the condition of identity of sets and pluralities), we would have learned a different concept. It is clear that something has gone wrong. Learning a concept is certainly not a problem for our view, since it just requires to grasp its meaning, that is its intension, and not all its instances.

The radical reduction of concepts to their instances is very implausible; however, this is not the only interpretation of the idea that application of concepts modifies their intensions. In particular, Williamson [1998] proposed a different interpretation of indefinitely extensible concepts. Mainly working with semantic paradoxes, Williamson writes:

> We start with one set of correlative meanings for "say", "true" and "false"; we use them to construct a sentence that says nothing in that sense of "say"; but reflection on that sentence causes normal speakers to give "say", "true", and "false" a new set of correlative meanings, much likes the previous ones except that the sentence in question says something in the new sense of "say"; the process can be repeated indefinitely. Normal speakers are not aware of the change, just as they are not aware of many ordinary processes of gradual change. They feel themselves to be going on I the same way, but they are not.

What Williamson is here arguing is that reflection on semantic paradoxes produces a shift in the meaning of the semantic notions involved in these paradoxes. Then he proposes to interpret the set theoretic paradoxes in the same way. What indefinitely extensibility shows is not that to the notions of set or ordinal there corresponds an indefinite number of extensions, rather that we can reinterpret in indefinitely many ways the meanings of such notions. Each reinterpretation results in a more comprehensive notion of set or ordinal, which embraces more instances than the former interpretations:

> For given any reasonable assignment of meaning to the word 'set' we can assign a more inclusive meaning while feeling that we are going in the same way [...] The inconsistency is not in any one meaning [...] it is in the attempt to combine all the different meanings that we could reasonably assign it into a super-meaning.

Suppose we take a bunch of sets such that we believe to be all the sets there are. Then we consider all non-self-membered sets and the set R that comprises exactly all of them. By Russell's paradox, that set is not one of the starting sets. Reflecting on this reasoning, we have assigned to the word 'set', which remains the same, a new and more comprehensive meaning which accounts for the new set R. In this way, indefinitely extensibility is not accounted as an ontological, but just as a linguistic phenomenon: we have liberalized our language, by liberalizing the meaning of the word 'set'. The new interpretation recognizes as a set what before was not a set (and therefore was an urelement from the point of view of the former meaning of 'set'). There is no super-meaning, that is a meaning that remains the same no matter the expansion of the extension: the feeling that there is a common meaning is explained both by the fact that the word 'set' remains the same and by the fact that the changes are gradual, which make speakers not aware of them.

Notice that this account can explain how we acquire concepts: even though each new application modifies the meaning of the word 'set', the modifications are so thin that the speaker is not usually aware of them. Consequently, it is still possible to learn a concept just by grasping it, without knowing all of its instances.

Is such an account of concepts compelling? If we are just dealing with semantic notions, as 'true', 'false' and so on, the account has a certain appeal. Moreover, also with many predicates that are usually considered to be vague or ambiguous the account seems to explain how it is possible that their meaning changes. In those cases, the reason why the account is appealing is that we do not have clear definitions of such notions. However, to extend it to the mathematical case, where most notions – as the one of the ordinals – are sharply defined seems simply wrong. The condition of application of such notions are well-defined and this excludes that the recognition of a new item can modify the concept in play. Given the standard way the ordinals are introduced (0 is an ordinal; every object that is obtainable from 0 by a finite number of applications of the successor function is an ordinal; the limit of a sequence of ordinals is an ordinal), it is fully determined if an arbitrary object j falls under the concept of being an ordinal or not. Here the adjective 'determined' means that the question "does j fall under the concept of being an ordinal?" has a unique and fixed answer. In such cases the idea that application modifies the same concept seems to be simply false.

However, there is a more serious worry for an account that holds that each application of a concept to a new item forces a reinterpretation of the notion in play. Let consider again the case of Russell's paradox. We start with a bunch of sets that, according to our current interpretation $I_1$of the word 'set', are all sets there are. Let's call these sets sets-I_1.We now consider all the non-self-membered sets. Each of them is a set-I_1. We further consider the set R that comprehends all and only the non self-memebered sets-I_1. On pain of paradox it is not a set-I_1: let's call it a set-I_2, that is a set according to a more comprehensive meaning of the word 'set'. The problem I want to raise can be summed up in a question: how is it possible to consider R as a set? An account as the one defended by Williamson would say that it is by reflecting on Russell's paradox that we become aware of the shift of meaning from set-$I_1$to set-I_2, which allows to consider R as a set. The problem with such an explanation is that *it does not clarify why we should reinterpret the notion of set to account R as a set.* According to I_1, R is an urelement: why do we need to reinterpret the notion of set with regard to R and not – say – with regards to other arbitrary urelements? The only reason seems to be that R is in fact a set, but not one of the set-I_1. If so, *we have recognized it as a set before the shift in interpretation from I_1 to I_2,* which is possible just in case R satisfies the condition of application of the concept of set-I_1. Is this the case, then it is simply false that recognizing R as a set produces a shift in the meaning of the word. The recognition is based on the condition of application of the concept of set, which in turn implies that the extension of the word 'set' does change, not the intension.

In general, the problem is that when we consider the set R of all the non-self-membered sets-I_1(before performing and reflecting on Russell's paradox), this object must be treated as an object of the same nature as the other sets. In order to perform Russell's reasoning, we must regard it as a set. What legitimates this fact is just that R shares some structural properties of sets (for instance, that it makes sense to ask if it has elements that belong to it, and so on). These properties are preserved after reflecting on the paradox. Therefore, we consider it as a set before and after reflecting on the paradox, which is possible only if there are common properties between the two purported conceptions set-I_1 and set-I_2. If we recognize that there is something in common, then we can consider these feature as capturing the intension of the notion of set, while considering the paradox to show that it is always possible to enlarge the extension of such an intension. Moreover, if it were just a matter of reinterpretation of the meaning 'set', why do not simply conclude that the paradox shows that R is not a set after all, but just an urelement? Evidently if the conclusion is that R is not one of the set-I_1, but it is in any case a set, then it seems that what we have done is just to have kept fixed some notion of set (which cannot be identified with set-I_1) and to have enlarge its extension. To sum up, this alternative interpretation seems to require that in each reinterpretation of the word 'set' something remains the same: if so, we are back to our view such that there is an intensional aspect which does not change while its extension changes.

There remains one last thing to consider: from indefinite extensibility, we have derived a general picture of what concepts are. Concepts are primitive intensional entities that can be applied to any object that satisfies their condition of applicability. New applications enlarge the extension of concepts, but they do not change their intension. However, can this conception account for the change of the meaning of the words? If the reply were negative, it would be a good reason to dismiss the whole account as simply wrong. But it seems to me that our account could easily explain why meanings of words change. We should not look for the reason in the applicability of concepts, rather in the *nature* of the conditions of application. If the condition of application is vague or ambiguous, if it allows for borderline cases - in other words, if it is not fully determined -, then it may happen that new application changes the meaning of words. However, this is not due to the application, rather it is due to the nature of the condition of application. The mathematical case of indefinite extensibility exactly shows that application *per se* is not responsible for this semantic phenomenon.

7. Towards a theory of dynamic abstraction

In § 4.1 we gave the semantics of a non-modal fragment of the language $L^{\Diamond}$. According to that semantics, the semantic values of pluralities of objects of the domain of the object language was given by sets in the domain of the meta-language, and the semantic values of nth-level concepts of the object-language was given by nth-level properties of the meta-languages. Since both sets and properties are first-order objects, the result is that the meta-language domain is more comprehensive than the object-language domain (see theorem §4.1).

What this kind of semantics tries to mimic is the natural language's process of *nominalization,* i.e. the transformation of an adjective or a predicate into a name. In particular, what happens is that we nominalize predicates and plural nouns that respectively denotes concepts and pluralities of objects in the object-language into nouns that respectively denote properties and sets in the meta-language. In this way, from the meta-language's point of view we treat concepts and pluralities as if they were (first-order) objects. In addition, §4.2 has explained the role of the self-referential structure of language in this picture: it is this feature of language that forces the expansion of the first-order domain of the language. In the present paragraph we want to explain in more details how nominalization is meant to work, which means that we would like to explain how the collapse of higher-order entities as concepts into properties and the collapse of pluralities into sets works.

Nominalization can be seen as a linguistic counter-part of the process of abstraction. Informally speaking, when we abstract something from something else, we focus our attention on a particular aspect of the thing, without paying attention to other aspects, and we consider this particular aspect as if it were an independent object. I do not intend to go here inside the riddle of questions and problems that surrender abstraction; here I just restrict myself to few considerations. First of all, since the result of nominalization is to introduce new objects in the domain of the language, we should be able to distinguish these objects from other objects, and we should also be able to recognize these objects after further expansion of the domain. But to do that we need some criteria of identity for those objects. As Frege rightly notices:

> If for us the symbol *a* is to denote an object, then we must have a criterion which determines in every case whether *b* is the same as *a*, even if it is not always within our power to apply this criterion (Frege 1965, §62)[226].

In other words, when introducing a new object we must introduce it together a criterion of identity that allows us to re-identify the object no matter how we expand the starting domain.

However, a criterion of identity is not enough: we must also be able to say when the collapse is possible, i.e. when we can collapse concepts to properties and pluralities into set. Here the answer is: always. The modal approach is meant to allow this collapse every time.

Let's start with the collapse of pluralities into sets. We must provide a criterion of identity and a criterion of existence. The following is the natural criterion of identity:

(ID-P): $\Box[\forall xx \forall yy \forall u(u \prec xx \leftrightarrow u \prec yy) \rightarrow \{u|u \prec xx\} = \{u|u \prec yy\}]$

Which simply says that two pluralities determine the same set if they really are the same plurality.

We already know the criterion of existence, which simply is Collapse$^{\Diamond}$:

$$\forall xx \Diamond \exists y Form(xx, y)$$

where $Form(xx, y) =_{def} \forall u(u \prec xx \leftrightarrow x \in y)$.

We can put together in a unique law these two principles. What we get is the following plural modal version of Basic Law V:

$(Pl - BLV^{\Diamond})$ $\Diamond \exists x \Diamond \exists y(x = y) \leftrightarrow \Box \forall u(u \prec xx \leftrightarrow u \prec yy)$

Where $x = \{u|u \prec xx\}$ and $y = \{u|u \prec yy\}$.

Of course, the inconsistency of the non-modal BLV is avoided by the modal operator. What this version of BLV[227] says is that, if you consider an arbitrary language L_0 with a domain D_0, then you can collapse each plurality of objects in D_0 into sets that, because of Cantor's theorem, cannot all belong to D_0. So the law allows us to expand D_0 into a more

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[226] See Linnebo [2012] for a similar approach towards reference by abstraction.

[227] Notice the fundamental difference between this and the following version of BLV, and the one we presented while exposing the formal system. The latter formulation was inside the object-language, and therefore it was restricted to predicative concepts, in order to preserve consistency in the object-language. The former formulations are meant to connect the meta-language and the object-language by describing how the collapse of higher-order resources of the object-language into first-order objects of the meta-language happens. As a consequence, they admit impredictive instances, and consistency must be preserved in a different way, as it is explained in the main text.

comprehensive domain D_1, on which we could re-perform the abstraction. This is the meaning of the $\Diamond$-operator: the existence of those sets is only potential with regard to the existence of the correspondent pluralities. On the contrary, the meaning of the $\Box$-operator is just to say that this process can be carried out for any domain you may consider: no matter how far you can expand the D_0 in more comprehensive domains, you can always perform the abstraction. The operation can be reiterated indefinitely.

Let us now see the case of concepts and properties. Again, we already know the criterion of identity and existence of a property for the simple reason that a property is what the nominalization of a predicate refers to. So here we have:

(ID-N) $\quad \Box\forall x(Fx \leftrightarrow Gx) \rightarrow Nom(F,x) = Nom(G,x)$

Which says that if, however the domain you consider, two concepts are always instantiated by the same objects, then they determine the same properties (or: their nominalization is the same, which means that it denotes the same property). Then we have:

(Ex-N) $\quad \forall F \Diamond \exists x\, Nom(F,x)$

Which says that for each concept, possibly there is a property (a nominalization that denotes the property in question).

Also in this case we can put together the two principles in a unique law:

$(BLV^{\Diamond})$ $\quad \Diamond\exists x\,(Nom(F,x) = Nom(G,x)) \leftrightarrow \Box\forall x(Fx \leftrightarrow Gx)$

This is a modal version of Basic Law V. Of course the inconsistency of the non-modal BLV is – once again - avoided by the modal setting. What this version of BLV says is that, if you consider an arbitrary language L_0 with a domain D_0, then you can collapse each concept of L_0 into (first-order) properties (which will be denoted by the nominalization of the predicate that expresses the concept). Because of Cantor's theorem, these properties cannot all belong to the domain of D_0. In this way, we have expanded D_0 into a more comprehensive domain D_1, on which we could re-perform the abstraction. This is the meaning of the $\Diamond$-operator: the existence of those properties is only potential with regard to the existence of the correspondent concepts. On the contrary, the meaning of the $\Box$-operator is just to say that this process can be carried out however domain you may consider: no matter how wide is the domain you may consider, you can always perform the abstraction. The operation can be reiterated indefinitely.

However, in this specific case, the modal setting alone is not enough to guarantee consistency. If the inheritance principle $Nom\,(P,x) \rightarrow \Box Nom\,(P,x)$ had not failed, the formulation above of modal BLV would have been inconsistent (the situation is analogous to the one of §5). This marks an important difference between the current proposal and other two proposals concerning dynamic abstraction principles. Respectively they are Linnebo [2009, 2012] and Studd [2016].

Concerning Linnebo's proposal, my account shares with his account the idea that concepts are individuated by means of their defining conditions. When a concept X is defined by means of a condition ϕ with regard to a domain of objects D, then the objects that fall under X, however you expand the domain D, are exactly those that satisfy ϕ in the expanded domain. No matter how you expand a domain, concepts remain the same, while their extensions change. The individuation of a concept in a certain domain D is 'forward-looking': such concept *can* be applied to yet not individuated objects, which will fall under the concepts in some further extension of D. However, Linnebo does not notice the failure of a principle as $Nom\,(P, x) \rightarrow \Box Nom\,(P, x)$, which forces him to impose a groundedness restriction on the individuation of concepts. This restriction makes his proposal very close to predicativist restriction of (modal) BLV. On the contrary, by discharging $Nom\,(P, x) \rightarrow \Box Nom\,(P, x)$ we may allow any impredicative instances of modal BLV.

To avoid some version for Russell's paradox, and not to impose predicativist restrictions on modal BLV, Studd [2016] diverges on how he considers concepts. In his account, concepts are *extensional entities*. He writes:

> On the account developed here, under each interpretation I_α, second-order quantifiers express full, impredicative second-order quantification over the extensional Concepts under which zero or more members of M_α fall. For any condition ϕ, there is an extensional Concept X under which precisely the zero or more members of M_α that satisfy ϕ under I_α fall. But the condition ϕ serves to fix the extension of the Concept rather than to give its meaning. Under any subsequent interpretation I_β, the same zero or more objects fall under X even if they no longer satisfy the condition ϕ under I_β.

The idea is clear enough: what allows Studd to have full impredicative second order quantification is the extensional nature of concepts. Moreover, it seems that his concepts are also modally rigid: if something falls under a concept X, then necessarily it falls under the concept X. I think this deliver us a counter-intuitive conception of concepts (see §6 for what I take to be a natural conception of concept). This counter-intuitiveness can be appreciated by the fact that, as Studd acknowledges, the condition ϕ does not give us the meaning of a concept. The reason is that the condition ϕ has a purely intensional nature, while in his account a concept is something extensional. The fact that ϕ has an intensional nature can be appreciated by the last line of the above quotation: 'Under any subsequent interpretation I_β, the same zero or more objects fall under X even if they no longer satisfy the condition ϕ under I_β'. Studd is therefore forced to acknowledge an intensional element; however, since his concepts are extensional, this intensional element must be confined in the condition we use to specify a concept. In §6 we considered the possibility of interpreting concepts in an extensional way, and we dismiss it. However, if Studd tried to reply by appealing to the intensional aspect of the condition of specification, one should reply that he is using such conditions in a way very tied to how people normally understand concepts.

Our solution lies in a certain way in the middle between Linnebo and Studd. We share with Linnebo the same view about concepts. However, the failure of the inheritance principle makes nominalization, and therefore the properties for which that failure obtains, more extensional in nature (those properties differ from domain to domain), and it is this extensional feature that allows us to avoid a revenge paradox. Properties for which the inheritance principle fails are very close to Studd's concepts.

Appendix 1

INDEFINITE EXTENSIBILITY WITHOUT INTUITIONISM

On Dummett's argument for intuitionism from indefinitely extensible concepts

Abstract

The aim of this paper is to examine Dummett's argument for intuitionistic logic in mathematics from the existence of indefinitely extensible concepts. After presenting the argument in detail, we will demonstrate that indefinite extensibility alone does not suffice to establish the conclusion and that the argument requires more and not trivial assumptions to work. We will suggest that Dummett smuggles some constructivist ideas into his interpretation of indefinite extensibility, which have the effect of preventing the argument from being a *new* case for constructivism in philosophy of mathematics.

1. Introduction

The aim of this paper is to discuss a famous argument, posited by Michael Dummett, according to which indefinite extensibility implies intuitionist logic for set theory. A concept is indefinitely extensible if, for every *definite* totality of objects falling under it, it is always possible to find a more inclusive definite totality of such objects. The concept "being a class that does not belong to itself" is an indefinitely extensible concept, because the class *R* of all classes that do not belong to themselves cannot belong to itself (on pain of contradiction) and, consequently, the totality of all classes belonging to *R* together with *R* itself is a more inclusive totality of objects having the property of "not belonging to themselves". More generally, an indefinite extensible concept C is a concept associated with a principle of extension, i.e. a principle according to which, given some definite totality *t* of objects that fall under C, allows the discovery of a new object that falls under C but is not a member of *t.* As a consequence, we can enlarge the starting definite totality *t* with the new object: what we obtain is a more comprehensive definite totality *t'* of objects that fall under C. But now the principle of extension allows us to find a further object that falls under C but which is not one of the members of *t'*. The upshot is that there is no definite totality of objects falling under an indefinitely extensible concept; to an indefinitely extensible concept there corresponds an indefinitely extensible sequence of more and more comprehensive definite totalities of objects falling under it. In what follows, when we speak of an "indefinitely extensible domain" we mean these indefinitely extensible sequences of definite totalities of objects falling under an indefinitely extensible concept.

Michael Dummett, who interpreted the set-theoretic paradoxes as showing that concepts such as "being a set" or "being an ordinal" are indefinitely extensible, argued that this view implies an intuitionist logic for set-theory (Dummett 1991, pp. 307–21). More precisely, Dummett argues for the following conditional: *if* there are indefinitely extensible concepts, *then* also the fundamental mathematical domains (the domains of natural and real numbers) are indefinitely extensible and quantification over them behaves intuitionistically[228]. Dummett's intention was to provide a new and independent argument from his well-known meaning-theoretic argument; the latter was a *global* argument to be applied to any meaningful sentence, and was based on the idea that truth cannot outstrip verifiability. The meaning-theoretic argument thus supports a broadly anti-realist view, whose upshot is the validation of intuitionistic logic. On the contrary, the argument from indefinite extensibility was meant to be a *local* argument for intuitionism, that is, an argument that appeals only to some features of some mathematical concepts. In this way, the argument was not meant to be grounded in Dummett's anti-realist view; rather, it should provide a strong case for anti-realism in mathematics. Therefore, the general idea is that indefinite extensibility represents a new form of indeterminacy, which does not depend on a constructivist assumption. Richard Heck puts the point nicely:

> I should, however, emphasize that this is a *new* argument for intuitionism, quite different in character from the meaning-theoretic arguments for which Dummett is well known. It is a local argument for anti-realism about mathematics, one which depends upon considerations peculiarly mathematical in character; it therefore has not the propensity to generalize which the meaning theoretic arguments have (Heck 1993, p. 233).

In the analysis of the argument below, I shall follow Dummett and Heck in considering this argument independently from the meaning-theoretic one. However, I will claim that the argument does not achieve its goal: this means that indefinite extensibility alone is not sufficient to establish an intuitionistic logic as suitable for set theory. In particular, our analysis will show that Dummett smuggles his constructivist view into mathematics when interpreting indefinite extensibility, and as such his argument – contrary to his intention – cannot provide us with any new reasons for anti-realism in mathematics, because it *presupposes* an anti-realist interpretation of indefinite extensibility.

If our analysis of Dummett's argument turns out to be correct, the consequence is that this argument can only work together with the meaning-theoretic argument, which would be a disappointing result in light of Dummett's promise of providing a new and independent argument for intuitionistic logic.

[228] Dummett speaks of fundamental mathematical domains in the plural, meaning both the domain of natural and the domain of real numbers. Dummett is clear that what he counts as fundamental are the domains of natural and real numbers on the grounds that we derive our conception of different infinite cardinalities from them. See Dummett (1991, pp. 317–18). However, the argument we are going to analyze works *only for real numbers*, as should become clear from the proceeding analysis. Dummett's view on natural numbers is slightly more complicated: in Dummett (1963) he takes the domain of natural numbers to be a fully determined domain over which quantification behaves classically; however, in Dummett (1991 and 1993) he changes his mind and expresses deep doubts over the possibility that our conception of the natural numbers can offer a fully determined domain of them. Later, in Dummett (2007) he seems to return to a position more reminiscent of that defended in 1963. For our purposes, we will set aside the status of quantification over natural numbers, and we will read Dummett's argument as working only for real numbers. For more details on Dummett's conception of natural numbers, see also Rumfitt (2015, pp. 265–6).

2. Dummett's argument for intuitionistic logic from indefinite extensibility

Dummett's argument for the adoption of an intuitionistic logic is based on the fact that we do not have a definite conception of the elements that belong to the fundamental mathematical domains:

> My argument was for an intuitionistic rather than a classical understanding of the quantifiers, and hence for an intuitionistic logic in general, within fundamental mathematical theories, essentially on the ground that we do not have a sufficiently definite conception of what elements belong to the domain of such theories (Dummett 2004, p. 791).

When we quantify over a domain which is not completely definite, our quantification behaves intuitionistically. And the fundamental mathematical domains (the domains of natural and real numbers) are not definite because they are indefinitely extensible. The argument can be summarized as follows:

1. The indefinite extensibility of the set universe implies the non-completely definite nature of the fundamental mathematical domains. In other words, if the set-theoretic universe is indefinitely extensible, then the fundamental mathematical domains are also indefinitely extensible.
2. An indefinitely extensible domain is a not completely definite domain which requires quantification to be intuitionistic.

Conclusion:

3. The quantification over a fundamental mathematical domain must be intuitionistic.

At the heart of the argument lies the notion of a completely definite domain. What does it mean, in Dummett's argument, for a domain to be completely definite? We will now answer this question by separately analyzing the two premises.

2.1 Analysis of premise 1

The first premise is justified by two further theses[229]:

a) Mathematical truth cannot outrun what is already implicit in our concept of number (that is, in our concept of the fundamental mathematical domain).
b) Our concept of number is not sufficiently determined for us to be able to determinately fix the truth or falsity of every arithmetical statement.

The two theses are different, but intrinsically connected. However, before analyzing them, we should ask ourselves another question: what does Dummett mean by the expression "concept of number"? Clearly he is speaking of an intuitive idea of these domains, which cannot be reduced to a formalistic position:

[229] These are exactly the first and second theses of Sullivan's reconstruction of this argument. See Sullivan (2007, p. 757).

> the possession of such a conception of a mathematical system – of an intuitive model for the theory that relates to it – is without question essential for us to have a mathematical theory at all, rather than a mere piece of formalism; and it is this which tempts us to speak of mathematical intuition (Dummett 1991, p. 311).

Keeping this characterization in mind, the first thesis represents Dummett's acceptance of logicism as the view that mathematics is a body of analytical truths (Sullivan 2004, p. 757). Mathematical truths are analytical because they are derivable by deductive reasoning from our concepts of fundamental domains, which are partially captured by the axioms of our theories. Only this view can explain the central role of the proofs in establishing new mathematical knowledge. If mathematical truth could go beyond what we set with our conception of number (the fundamental domain), then this truth could never be proved (because no deductive reason can bring us to it from the concept of number) and, hence, we would not be able to justify its necessity. This is the same argument that lies behind Dummett's criticism of the notion of intuition (of mathematical entities) conceived as analogous to sensitive perception. If intuition is a sort of mind-perception of abstract objects, like sensitive perception, it can only tell us what exists and not that what exists *must* exist. In this way, the necessity of mathematical objects can never be justified[230].

The justification of the second thesis is based on an important distinction in Dummett's reasoning: the distinction between generalizing over an empirical concept – e.g. the concept of a star – and generalizing over an abstract concept – i.e. natural numbers, ordinal numbers, and so on. Regarding the empirical case, we need only have precise and clear conditions of identity and applicability in order to generalize a concept: then the spatio-temporal reality will determine its domain of application. This is possible because the placing of an object in space and time allows a plurality of perspectives of reference: "their spatiotemporal location accounts for their having different aspects in terms of which they may be referred to in different ways (from different perspectives). In so doing it fixes the domain of material objects" (Dummett 2004, p. 796). That the reality is determined (and therefore the domain of quantification is fully determined) means that for every quantified sentence we can make, the reality will always establish its truth value. Therefore, quantification can be thought of as classical. Obviously this does not mean that we know the truth value of every single sentence we can ever utter; rather, that the sentences are in themselves either true or false. However, this is not the case with abstract objects: the identity and applicability conditions are no longer enough, because here there is *not* a reality which can fix *a priori* the domain of quantification. We know what conditions an object should satisfy in order to be acknowledged as a natural number, but we do not know *a priori* which objects are natural numbers: we have to check this on a case by case basis.

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[230] Of course, intuition in mathematics can be (and perhaps should be) thought of in a different way from sense perception. For a strong defense of intuition as a way of knowing "quasi-concrete" mathematical objects as numerals and geometric shapes, see Parsons (1979).

The crucial difference between the empirical and the mathematical case relies on the fact that the presence of the reality in the former case fixes the range of ways in which an object can be given or specified, while this range remains undetermined in the latter case. Even if we specify the criteria of identity and application for a mathematical object – say a real number – this is not enough to fix the domain of real numbers, because there could be other ways of specifying a real number that are not settled by such criteria of identity and application. Concerning these criteria, Dummett writes:

> [The criteria are] quite adequate to explain what is required of a specified mathematical entity for us to recognize it as a real number; but it does not suffice as a means of circumscribing a domain of quantification, when such quantification is to yield statements with determinate truth-values. It does not do so, because it fails to determine the limits of acceptable specification of something to be acknowledged as a real number: we still need a means of saying which real numbers the domain comprises. (Dummett 1991, p. 315)

Therefore, the fact that our conception of number is not able to determinately fix the truth or the falsity of every arithmetical statements means that it does not determine the range of ways in which numbers can be specified. If we now put together theses a) and b) we have it that, since mathematical truth cannot outrun what is already implicit in our concept of number (thesis a)) and this concept is not able to determinately fix the truth or the falsity of every arithmetical statements (thesis b)), then there are statements that are neither true nor false, and consequently quantification over the mathematical domain must behave intuitionistically[231].

At this point of the reconstruction, a couple of remarks are in order with regard to the justification of premise 1. First of all, it is clear that the crucial premise is thesis b) (as explicitly recognized by Dummett)[232]. However, thesis b), as it is stated, is a general claim concerning any mathematical domain whatsoever, not just indefinitely extensible domains. If so, intuitionism would not follow from the presence of indefinitely extensible domains, but rather from a general feature of each mathematical domain. If correct, the argument would not be an argument for intuitionism from the existence of indefinitely extensible domains. Secondly, this general feature is simply the fact that there is not a mathematical reality that can fix the truth value of each mathematical statement. But Dummett does not provide any argument in favor of such view, whilst he simply says that such a strong realism would be an 'heroic' position to defend[233]. In this context,

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[231] Actually, what follows is the weaker claim that quantification does not behave classically, which does not immediately mean that it behaves intuitionistically. There could be other logics – for instance, some trivalent logic or a classical modal logic – that can accommodate it. However, in this context, Dummett does not consider these (or other) alternatives, and simply concludes that the logic must be intuitionistic.

[232] In replying to Sullivan, Dummett writes: "the analysis of my argument that the fundamental mathematical theories admit only an intuitionistic, not classical, logic, with which Sullivan opens the second section of his essay, is very well set out. The first premiss [thesis a)] is certainly one that I endorse, and on which my argument is based. The second premiss [thesis b)] is *the crucial one* from which, in *FPM,* my conclusion was derived. *It certainly applies to the real numbers in my view. Whether or not it applies to the natural numbers is more questionable*" (Dummett 2007, p. 788; emphasis added).

[233] Commenting on this point, Rumfitt (2015, p. 265) writes: "Dummett takes it to be obvious that mathematics does not describe a realm wholly independent of human thought. No one, he opines, could be

however, this is rather problematic, because he is trying to provide an argument against the realist's use of classical logic and, consequently, taking this for granted seems to beg the question against the realist. To say that, for mathematical entities, conditions of identity and applicability are not enough to fix what objects there are (and hence to fix the truth conditions of sentences about them), because the mathematical reality is not in itself determined, means to have already excluded a realist position.

However, these two problems may be overcome by considering Dummett's grappling with the idea that we have a clear grasp of the totality of natural numbers, and by comparing this grappling with his firm belief that we do *not* have any grasp of the totality of the real numbers. Concerning natural numbers (in Dummett 1991, p. 318), he argues that we do *not* have such a grasp because we can only grasp the principle of extension that brings us from one natural number to its immediate successor[234]; however, later he expresses some doubts about this position, and he explicitly states that if we have a clear grasp of the totality of natural numbers, then quantification over it must be classical:

> If what I maintained in *FPM* [*Frege's Philosophy of Mathematics*] is wrong, then, as far as the argument of that book goes, we should have to allow the use of classical logic in elementary (first-order) number theory. (Dummett 2004, p. 790)

What this struggle suggests is that we need to weaken thesis b) above: thesis b) does not apply to those mathematical domains of which we are able to grasp the totality of their elements. If such grasp is beyond our possibility, then thesis b) applies and the domain is not fully determined. In this way, the two problems above fade away: the argument is no longer question-begging, because even though Dummett excludes the plausibility of a strong form of realism, he acknowledges the possibility of having classical logic in some mathematical fields; furthermore, to argue for intuitionism we need to make a further step, i.e. we have to argue that we cannot grasp the totality of objects of a domain. Indefinite extensibility thus seems a promising candidate to take this step.

However, set theorists and mathematicians in general usually consider the real numbers as forming a legitimate set, which seems to show that we actually grasp the totality of the real numbers, contrary to what Dummett held. What is, then, the

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so much of a realist as to hold that it did (Dummett 1993, p. 75). Many mathematicians and philosophers have in fact taken the position that Dummett rules out *ab initio* […]".

[234] Here is the passage from Dummett (1991, p. 318): "we have a strong conviction that we *do* have a clear grasp of the totality of natural numbers; but what we actually grasp with such clarity is the principle of extension by which, given any natural number, we can immediately cite one greater than it by 1". In Dummett (2007, p. 790), he raises some doubts about this same passage: "I confess to feeling quite dubious about this question. When I am asked to envisage what, for us, are simply enormous natural numbers – say a number N whose representation in decimal notation would fill as many and as large volumes as the *Encyclopaedia Brittanica,* or, larger yet, the number N^N – I think what I said in *FPM* is right, especially when I reflect that N and N^N, like all natural numbers, are small in the sense that most natural numbers are greater than that. […] On the other hand, when I contrast the totality of natural numbers with the totality of the real numbers, the sense that we have a perfectly firm conception of the former begins to grip me once more".

difference between natural and real numbers? The answer must be sought in the existence of indefinitely extensible domains. In fact, if the universe of sets is indefinitely extensible, then so too is the notion of 'arbitrary subset'. This latter notion plays a crucial role in the classical Power Set Axiom, and therefore in the use of this axiom in showing that there is no maximal cardinal number. However, the notion of 'arbitrary subset' also has a crucial role in the definition of the real numbers (for example, consider Dedekind's construction of the real numbers from the rational numbers by means of cuts: Dedekind considered *arbitrary* cuts of rational numbers, where each cut is just a set of rational numbers. Dedekind then shows that for each rational number, there is the corresponding cut, but not vice versa. As a consequence, we can extend the rational numbers with the real numbers such that for each cut there is exactly one real number and, vice versa, for each real number there is exactly one cut. Since each cut is a set of rational numbers, i.e. an arbitrary subset of the set of all rational numbers, there are as many real numbers as subsets of rational numbers. Moreover, as there is a one-to-one correspondence between the rational and the natural number, we can conclude that there are as many real numbers as arbitrary subsets of the natural numbers). Since the notion of real number requires the notion of arbitrary subsets, if the latter is indefinitely extensible (as the paradoxes seem to indicate), then the way of specifying the real numbers is also indefinitely extensible, precisely in the sense that the range of ways in which a real number could be specified is not determinately fixed, as thesis b) states.

To sum up, the idea that emerges from these considerations is that quantification over abstract objects as numbers behaves classically only when we can previously fix the totality of objects belonging to the domain of the quantifiers. The sentence 'there are natural numbers' is true only if we have previously determined the domain of natural numbers. Therefore, the determination of the domain of quantification, with regard to quantified sentences over abstract objects, plays the same role that reality plays with regard to a quantified sentence over concrete objects: it provides *a fully determined subject matter* that allows us to fix the truth values of every well-formed quantified sentence. When such determination is not possible, as in the case of real numbers, the domain is undetermined and quantification over it cannot behave classically.

2.2 Analysis of premise 2

So far, so good. The end of the previous section set out the role of the indefinitely extensible domain in Dummett's argument, particularly in respect of its strict connection with thesis b). It is now time to examine more carefully what indefinite extensibility amounts to, and therefore how premise 2 must be interpreted. We will argue that indefinite extensibility alone is not enough to play the role that Dummett's argument demands.

Premise 2 says that an indefinitely extensible domain is not a complete definite (determined) domain. From a certain point of view this is a triviality: if one can keep on finding new elements of a certain kind, it is clear that the domain is never fully complete.

But what we need to understand is why quantification over an indefinitely extensible domain cannot provide determined truth values. Here things become quite complex. If quantification always provides determined truth value, then it is classical. So the question above becomes the following: why cannot quantification over an indefinitely extensible domain be classical? The answer to this question must lie in the nature of classical quantification (Shapiro and Wright 2006, pp. 294–6). Classical quantification is usually thought of as a function (a product in the case of existential quantification and a sum in the case of universal quantification) that gives a truth value as a result. Every function needs a domain and domains are (usually taken to be) sets. Hence if we have a statement as $\forall x \varphi x$ and a domain D and, for every $d \in D, \varphi(c_d)$ is true (with c_d a constant term which denotes d; if there are no such terms in the language we can simply add one fresh constant), then the quantified statement is true as well. Whenever the domain of a quantified statement is determined, then the statement will have determined truth values. However, if the domain is indefinitely extensible, no quantification can embrace all instances of the domain, because the same act of embracing all instances produces a new and not yet embraced instance. Ever more new elements can be found and they can generate new truth values. This seems to be the reason Dummett has in mind as to why quantification over an indefinitely extensible domain cannot always produce a determined truth value. In relation to this point, Shapiro and Wright say:

> The crucial thought is thus that a function requires a *stable* range of arguments if it is to take a determinate value. [...] The operation of classical quantification on indefinitely extensible totalities is frustrated not because it is vague what the arguments are, but because any attempt to specify them subserves the construction of a new case, potentially generating a new value. The reason why [for Dummett] quantification, classically conceived, requires a domain – a definite totality – to operate over is just that. (Shapiro and Wright 2006, p. 296)

However, it is imperative to note that this should not be read as Dummett accepting the so-called "All-in-One Principle". The latter consists of the thesis that quantification always requires a set (or a set-like object) as its own domain (Cartwright 1994). Not only could the discussion above convey the misleading idea that Dummett accepts this principle, but one could also refer to all those passages where he explicitly requires – for objectual quantification – a previous specification of the domain. An example can be found in Dummett (1991, pp. 307–21) where he insists on the necessity (but, at the same time, on the problem) of previously characterizing the fundamental mathematical domains[235].

That we need a set-like object to quantify is quite controversial. Cartwright has objected to this point by underlining that it is only a feature of modern semantics based on model theory that requires a set as a domain of interpretation of a formal language. This is right. The kind of logical laws quantification must follow cannot depend on extrinsic features of a discipline. What indefinite extensibility certainly challenges is the

[235] For a critical discussion of this point, see Sullivan (2007).

way classical semantics has been developed (Shapiro and Wright 2006, p. 297). In fact, suppose the All-in-One Principle is right. In that case, to quantify over all sets, they need to be collected in a domain, that is, a new set different from all previous ones (this is obviously true if we are working on a set theory with the axiom of foundation). Hence it would be out of the range of the quantifier. If we also tried to quantify over it, it would become one of the elements over which our variables range: but the All-in-One Principle requires that all these members are collected in a new domain. And again, this new domain would lie outside the range of quantification. So, if we want to quantify over every set, we have to drop the All-in-One Principle and, consequently, the way classical semantics has been developed.

Boolos had accused Dummett of founding his interpretation of the fundamental mathematical domains as indefinitely extensible on the All-in-One Principle (Boolos 1993). In his response, Dummett (1994) denies that he embraces this Principle. He emphasizes that the fact that quantified statements always produce determined truth values only if the domain of quantification is fully determined does not mean that the domain forms a collection or a set:

> But the claim that, for quantified sentences of a mathematical theory to be capable of being construed as making statements with determined truth values, *true* and *false*, there must be a means of determining over just which objects the variables of quantification range has nothing to do with any question whether those objects form a collection or super-class (Dummett 1994, p. 248).

Dummett does not presuppose the validity of the All-in-One Principle either to challenge classical quantification or to defend his thesis about the indefinite extensibility of the fundamental mathematical domains. Rather, indefinite extensibility and, consequently, the challenge for the development of classical semantics arise as soon as we recognize that domains like the ordinals are well-ordered (by their magnitude). Namely, this means that there is order-type that corresponds to the sequence of ordinals: by definition, there is a new ordinal, different from every ordinal of the sequence, which corresponds to this order-type. In this way, without presupposing the validity of the All-in-One Principle, we have discovered that the domain of the ordinals is indefinitely extensible and therefore quantification over it cannot be thought of as in classical semantics.

We have just seen that indefinite extensibility challenges how classical semantics has been construed. But can this fact also have consequences for the determinacy of the truth values of quantified sentences? Notice that, at this point, we cannot give a positive answer on the basis of Dummett's argument above, since that argument exploits the existence of indefinite extensibility to argue that we do not have a grasp of the totality of all real numbers. Such an answer would be plainly circular in this context. However, the right answer seems simply to be negative, in the sense that, from indefinite extensibility alone, we cannot derive the indeterminacy of quantification over it: making this deduction would beg the question against the realist. As noted by Shapiro and Wright

(2006, p. 286), the realist can think of the cumulative hierarchy of sets as fully determined in itself and independent from human thought and practice, and still affirm its indefinite extensibility[236]. A realist would affirm the actual existence (outside space and time) of every transfinite set: for him, indefinite extensibility only means that there is no transfinite set that encompasses all other sets. It is clear that this position can be challenged in a number of ways, but at this point of the discussion, this account is fully legitimate. To see exactly why Dummett's argument fails, we need to take one more step and ask ourselves on what does the legitimacy of the realistic account depend: the answer is that this legitimacy depends on the difference, underlined by Dummett himself in the last quotation above, that the full determinacy of the domain is something distinct from its constituting a set. In other words, it rests upon the fact that the determination of the objects over which we quantify is independent from the All-in-One Principle. Precisely because they are two different things, the Platonist can affirm the former and deny the latter. This shows that it is logically possible to interpret indefinite extensibility in a way that preserves a quantification over it that always gives rise to determined truth value[237]. Therefore, the reasons Dummett gives to claim that indefinite extensibility implies intuitionistic logic do not seem enough to justify his claim. Premise 2 is thus the weak premise of the argument.

2.3. General assessment of the argument

We have just argued that Dummett's argument for premise 2 does not work, because indefinite extensibility challenges only the All-in-One Principle, which has nothing to do with the determination of the objects of quantification. At the end of our analysis of premise 1, we saw that the determination of a domain of quantification should provide, for Dummett, a fully determined subject matter for quantified sentences. But we have just seen that it is *in principle* possible to interpret an indefinitely extensible domain such that each definite domain in the sequence is fully determined and, consequently, the full sequence turns out to be fully determined. From the analysis of the first premise, we know that Dummett tries to exploit indefinite extensibility together with thesis b) to argue that the range of possible specifications of the real number is not determinately fixed. But at this point it should be clear that this is not adequate to support Dummett's conclusion. Indeed, to reach that conclusion we need to take a further step. One possibility would just be to already interpret indefinitely extensibility in a *constructive* way. This can be shown as follows: consider, for example, the classical Power Set Axiom. Since its impredicativity, to specify the Power set of a given set G, we have to specify what sets in the whole universe of sets are subsets of G: so we must quantify over all sets in the universe. But this has the consequence that because we need to quantify over all

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[236] This was the view of Zermelo.

[237] Indeed, recent works by Fine (2006) and Linnebo (2010; 2013; and 2016) propose to deal with indefinite extensibility in a *classical* modal framework: the modality they invoke, which must be taken as primitive, are required precisely because indefinite extensibility challenges classical semantics. However, the modal operator allows the truth values of general claims over an indefinitely extensible sequence to be always determined.

sets in order to apply the Power set axiom, and our set universe – from a *constructive* perspective – is expanding, with the expansion of the universe the power set of G will also expand. We can always find new sets that belong to it. With regard to the real numbers, the expansion of the universe has the consequence that the notion of 'arbitrary subset' also expands, meaning that there are more and more real numbers that were not previously there. In other words, if our quantifiers over real numbers were classical, the expansion of the universe gives us new real numbers that are not in the range of our classical quantifiers. So quantification over all real numbers cannot be classical. However, a non-constructivist reading of indefinite extensibility blocks the argument. According to the latter reading, indefinite extensibility just amounts to the thesis that, given a set, there is always a more comprehensive set. From this view, when we go up and up in the hierarchy of sets, our vocabulary is enriched, allowing us to define new sets of P(G). The expansion of the universe does not produce new sets, but rather gives us a richer language by means of which we can define new sets that were already there, but that we could not define before – which is exactly what happens in the Constructible Universe (ZF + V=L). So our quantifiers over real numbers can behave classically: they range over all real numbers, even those we cannot currently define.

To reach Dummett's conclusion, it seems that we must previously interpret indefinite extensibility in a constructivist way. The outcome is that indefinite extensibility alone is not enough to have intuitionistic logic. Therefore, Dummett's new case against classical mathematics does not seem capable of holding on its own; instead, it already requires a constructivist reading of some mathematical phenomena. This is quite disappointing, since Dummett's promise is to offer a genuinely new and independent argument for intuitionism and, more generally, for constructivism in mathematics.

3. Rejecting a further interpretation of indefinite extensibility

If our previous analysis is right, then Dummett's conclusion presupposes a constructivist view of indefinite extensibility. The upshot is that, contrary to his aim, Dummett fails to provide an argument for intuitionism from indefinite extensibility: indefinite extensibility alone is not enough to argue for an intuitionistic rather than a classical logic. But there has been some interpretation of indefinitely extensible concepts as vague concepts that could be used to require a different quantification from that of the classical. Some authors have proposed interpreting the indefinite extensibility thesis as if it would imply that the indefinitely extensible domains are *vague* domains (hazy totalities). This interpretation relies on some of Dummett's own statements, in which he suggests that we see these domains as having a hazy length of increasing sequences "which vanishes in the indiscernible distance" (Dummett 1991, p. 317). The expression "*vanishes in the indiscernible distance*" has been read as saying that these kinds of domains are vague. If a domain is in itself vague, it is certainly undetermined (not definite), having borderline cases. Roughly speaking, a borderline case for a concept, C, is an object (in the general sense of the term) about which we do not know if it has to be considered as falling under concept C or not. Now, as we have seen, Dummett states that in an indefinitely extensible domain, it is undetermined what sets there are, but this does not mean that the domain is vague in this sense of vagueness (if not in this sense, then in what sense?). Even though there cannot be a fully comprehensive totality of all ordinal numbers, there are not borderline cases, because – as Dummett explicitly acknowledges – the identity and applicability conditions are well-determined. For every possible object we could consider, it is well-determined – without any shadow of a doubt – whether or not it is a set or an ordinal number[238]. All in all, this argument does not seem very compelling, because the determinacy of the identity conditions does not allow space for borderline cases[239].

4. Going beyond Dummett: a proposal for a characterization of indefinite extensibility

Dummett's discussion with Boolos suggests a different way of defining the notion of indefinite extensibility. There, Dummett stressed the fact that he does not presuppose – in order to argue for the indefinite extensibility of the ordinals – that the ordinals form a set or a set-like collection. Indefinite extensibility follows from the fact that *the ordinals* are well-ordered, and thus there is a further ordinal that corresponds to their order-type. If the ordinals do not form a set or a set-like collection, what do they form? At this point, it seems natural to suggest that what the argument for indefinite extensibility needs is just *the ordinals* (notice the plural form). In other words, we should interpret the locution "definite totality" in Dummett's characterization of indefinite extensibility as denoting a plurality of objects (where the term "plurality" is used as in plural logic, i.e. as the objects referred to by a plural expression[240]). We thus obtain the following characterization:

(IE_{def}) A concept is indefinitely extensible if, for every *plurality* of objects falling under it, it is always possible to find a more inclusive *plurality* of such objects.

Before discussing this definition, it should be made clear that this is not Dummett's view, and that he would not be happy with it. The problem for him would rely on the use of plural logic, which he saw as an unauthentic logic, and which – on the contrary – must be taken seriously once the definition above has been accepted[241].

[238] The only case which could be seen as vague is that of the totality of all ordinals: by construction this totality does not belong to itself; but at the same time it must belong to itself because it is an ordinal number. This is the only case we can think of as a possible borderline case. But as soon as we accept that the paradoxes show the indefinite extensibility of some concepts, this vagueness disappears and the class we believed to be the totality of all ordinal numbers becomes an ordinal number among others.

[239] More critics against interpreting Dummett's argument as suggesting the vagueness of this kind of domain can be found in Shapiro and Wright (2006, pp. 294–6).

[240] A plural expression (i.e. the dogs, the students, the ordinals, etc.) allows reference to several individuals at once. A plurality is therefore nothing over and above its members: a plurality is its members.

[241] Dummett rejected the legitimacy of the notion of plurality. For instance, in Dummett (1991, p. 93), he writes: "There is no such thing as a 'plurality', which is the misbegotten invention of a faulty logic: it is only as referring to a concept that a plural phrase can be understood, because only a concept-word admits a plural".

Going back to (IE_{def}), it has at least two great merits: first, it makes the concept of indefinite extensibility non-trivial; and second, it allows us to develop a philosophical position that is incompatible with Boolos' plural approach to unrestricted quantification over all sets. If we defined indefinite extensibility as the claim that, given *a set* of objects, it is always possible to find more comprehensive sets of objects[242], this claim would be trivially true in virtue of Cantor's theorem. Moreover, this claim is compatible with Boolos' approach: there is no universal set (because of Cantor's theorem), but the set universe is fully determined because there are all the sets, i.e. the set universe is nothing more than the maximal plurality of all sets.

On the contrary, the definition that exploits the notion of plurality is not a direct consequence of Cantor's theorem (which is a theorem about sets, not pluralities). This makes it an interesting philosophical thesis to defend, and not merely the consequence of a mathematical theorem that must be accepted if one works within Zermelo-Frankael set theory; in addition, it is incompatible with the plural approach to unrestricted quantification, because for the latter there is a maximal plurality of everything, while (IE_{def}) implies that, given an arbitrary plurality, it is always possible to find a more comprehensive plurality of objects (and so there can be no maximal plurality).

Given the proposed definition above, an indefinitely extensible concept P is a concept for which the following holds:

$$\forall xx\ \exists u(u \nprec xx \wedge P(x))$$

where "$\nprec$" must be read as "is not one of" and $\forall xx$ is a plural quantifier[243]. However, the problem this formula brings is that it is formulated in a plural first-order logic (PFO), and it is theorem of such a logic that there is a plurality that comprehends every object (this is just an instance of plural comprehension: $\exists xx\ (x \prec xx \leftrightarrow \varphi(x))$ where $\varphi(x)$ has been replaced by the predicate $x = x$). Therefore, the PFO-formulation above is inconsistent.

At this point, one might try to rescue the intuitionistic approach to sort out the problem. However, our discussion of Dummett's argument above should have made clear that this is only a possibility, not a necessity. There may be other ways to proceed. One such way has been developed by Linnebo (2010), which defends a modal formulation of the principle above:

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242 This means to interpret the locution "definite totality" as denoting a set of objects: a totality of objects is definite if it is a set.

243 Of course, this formula just expresses a necessary condition that a concept must satisfy to be indefinitely extensible, not a sufficient one. The concept must also be "stable", in the sense that the following formulas must be true of it: $P(x) \to \Box P(x)$; $\sim P(x) \to \Box\sim P(x)$, and $\forall x(P(x) \to \Box\Theta) \to \Box\forall x(P(x) \to \Theta)$ - restriction of the Barcan Formula to the concept P. For more detail, see Linnebo (2013).

$$\Box\forall xx \Diamond\exists u(u \not\prec xx \wedge P(x))$$ [244].

Linnebo develops his potentialistic account inside a classical framework (the logic is first-order classical modal logic), where sets are treated as necessarily existing (if they exist at all), and therefore the hierarchy of sets is fully determined even if there is neither a universal set nor a maximal plurality of sets. The indefinite extensibility of the set hierarchy is considered to be a feature of the same hierarchy[245]. For this reason, each general sentence will have a determined truth value. Linnebo's proposal can be seen as a non-constructivist way of interpreting indefinite extensibility. Here is not the place to go deeper into such a proposal; those interested should consult his texts. It is important to stress that the legitimacy of such a proposal shows once again that indefinite extensibility challenges the way classical semantics has been developed, not classical logic. We are not forced to abandon any classical law, such as the law of the excluded middle, to deal with indefinite extensibility.

5. Conclusion

In this paper, we have examined Dummett's argument for intuitionistic logic from the existence of indefinitely extensible domains. We have argued that the argument fails, because indefinite extensibility alone is not enough to argue for intuitionistic logic. We have also suggested that Dummett may have already smuggled in a constructivist view whilst interpreting indefinite extensibility. However, a more realistic view of the phenomenon is also legitimate and, on this view, the argument is blocked. The conclusion is that the argument requires more and not trivial assumptions to work.

The fact that intuitionistic logic does not follow directly from indefinite extensibility does not mean that we can accommodate it into classical logic without any problems. Contra Dummett, we argued that it is possible to interpret quantification over the real numbers classically. However, in discussing premise 2, it emerged that what indefinite extensibility challenges is the way classical semantics has been considered (based on models with sets as domains of interpretation). In other words, if one accepts the possibility of unrestricted quantification over all sets, indefinite extensibility challenges the validity of the All-in-One Principle. By committing to this principle, the only two solutions one can adopt are the following: either to reject the legitimacy of unrestricted quantification about all sets, or to adopt a paraconsistent quantification (which has the cost of implying contradiction). But, as we have seen, the All-in-One Principle is far from obvious. However, even dismissing it does not suffice to solve all our problems: if we quantify over all sets (or all ordinals), we have to recognize them as well-ordered: they

[244] It must be noted that this formulation is consistent with plural comprehension. The fact that plural comprehension has as an instance the existence of a plurality which comprehends every object is accommodated, in this modal logic, simply by the fact that, in each world (domain), plural comprehension turns out to be (trivially) true.

[245] Of course, this means that the modality invoked by Linnebo (which is a primitive modality) is not metaphysical; rather, it is a modality grounded on a certain picture of the iterative conception of set. I refer the interested reader to Linnebo's work for a full characterization of his modality and his proposal in general.

have an order-type[246]. To this order-type corresponds an ordinal that, for construction, will be different from all the sets (or ordinals) that we were quantifying. This is the real problem that indefinite extensibility posed to unrestricted quantification. In the literature, there are a number of attempts to solve this puzzle, and an intuitionist approach may be one possible solution. However, as shown by the previous discussion, it is by no means the only possible approach.

[246] We are dealing here only with well-founded frameworks: the reason is that in a not well-founded set theory, we can have the universal set and so we no longer have open-endedness.

CONCLUSION

1. *What has been done*

In this dissertation, we have defended the existence of a 'new' form of generality and provided support to the thesis that there are indefinitely extensible concepts. The adjective 'new' merely indicates that this form of generality is not captured by the standard theory of quantification, and not that this form did not exist before we introduced it. In chapters 4, 6 and 7 we gave reasons to think that standard quantification fails to express the domain-independence of two different kinds of necessary truths – conceptual truths, which depend only on the meanings of the terms used to express them, and necessary *a posteriori* truths (chapter 4, §4). These examples clearly show the existence of a domain-independent, and so open-ended form of generalization. We implemented standard quantification with a primitive modal operator to express this particular form of generality. It is the open-endedness of this form of generality that makes it compatible with the existence of indefinitely extensible concepts.

The defense of indefinite extensibility has been developed in different stages. Firstly, in chapter 1, we argued that the indefinite extensibility thesis is the more natural approach to the set theoretic antinomies: in a way it takes at face value every element present in the derivation of the paradoxes, while it just requires to abandon the implicit supposition that we were working in a fixed universe. This move seems to be far less revisionary than many of its alternatives. On this regard, the second part of chapter 2 shows that indefinite extensibility is compatible with the presence of impredicative definitions, which are essential in mathematics. Chapter 5, §2 argues for indefinite extensibility in a indirect way, by showing the problems of the plural alternative, and the *ad hoc* solutions it is forced to take to keep things consistent. Finally, § 2.6 develops a direct argument based on universal applicability, which I consider an appealing feature to require for foundational mathematical theories. All in all, the basic idea of indefinite extensibility is the following: there are certain situations in which, once fixed a theory based on a language and a domain of objects, we are in a position to identify new objects – structurally similar to those in the domain – but which are distinct (and so different) from any of them. What we should do – we have suggested – is just to acknowledge the existence of those objects, and so to acknowledge the expansion of the starting domain.

Defining indefinite extensibility is a big issue. We argued that a proper definition must already use the tools that allow absolute generality over an indefinitely extensible sequence. The key feature of our definition is the clause $(IE-2)^{\Diamond}$: $\Box\forall xx\ \Diamond\exists u(u \nprec xx \wedge$

$P(u)$). This is where we can find a common ground with the denier of indefinite extensibility: of course, she will not understand the modal formulation of such a clause, but she will perfectly understand the non-modal counterpart of it: $\forall xx\ \exists u(u \nprec xx \wedge P(u))$. The *common ground* is that both the plural absolutist and the friend of indefinite extensibility will recognize that this clause is, in some way, related to indefinite extensibility and both of them will recognize it as false. Of course, they will diverge on their diagnosis: for the plural absolutist, the sentence is false because there are no indefinitely extensible concepts (which is incorporated in her logic that admits, as a theorem, the existence of a maximal plurality), while for the friend of indefinite extensibility, the sentence is false because it is formulated within a logic that excludes the existence of such concepts. For her the problem is the logic, not the concepts. The importance of having a common ground (although it may appear quite restricted) lies on the fact that it is a necessary condition to assure that the plural absolutist and the friend of indefinite extensibility actually discuss the same phenomenon when the former denies what the latter affirms.

The fact that we argued for the compatibility between absolute generality and the existence of indefinite extensible domains undermines one of the main argument for relativism. The argument in question is the one which claims that absolute generality is not possible because there is no absolute domain (set or plurality) comprehending everything, since each domain can be enlarged. Our 'new' form of generality is meant to show that this is false. However, there is still a version of this argument that works: no standard unrestricted quantification can be absolutely general, because there is no absolute domain. In this new setting, this is not an argument for relativism *tout court*, rather it is an argument for relativism concerning standard quantification.

As we saw in chapter 4, this new form of generality undermines another relativist's standpoint, i.e. the use of schemas to capture the generality of the law of logic. Generally speaking, relativism does not seem 'in fit': chapter 3 discussed the well-known inexpressibility problem for relativism, concluding that it constitutes a big objection against any form of relativism. What was interesting of that objection is that the modal form of absolutism we defended behaves much better in relation to the relativist's challenge than the traditional standard form of absolutism (see chapter 3, §4).

The full justification of the new form of generality is given by the theory of concepts developed in chapter 7. The key idea is that quantifiers generalize the concepts or the properties they work with, and in this way generality inherits the features of concepts. Indefinite extensibility is exploited to argue that concepts are domain-independent in a very radical sense of the term. From a certain point of view, this vindicates Dummett's claim (see Dummett 1991, chapter. 17) that the failure of Frege's logicist program depended on not having any suspicious of the existence of indefinitely extensible concepts. The identification of concepts with extensions (and so with sets) that is encoded in Basic Law V is possible only if there are no indefinitely extensible concepts. However, we did not follow Dummett in his constructive approach to mathematics, and in particular in the adoption of intuitionistic logic to deal with indefinite extensibility. Our theory of concepts has strong connections with Linnebo's work (especially in the technical developments), but whereas Linnebo mainly works with set theory (and with the iterative conception of set), we have developed the other side of the medal: concepts. In general, while Linnebo's work is mainly devoted to show how a proper dynamic theory of abstraction can be used to ground mathematics, our main interest in this dissertation was to see if similar ideas could explain how concepts and abstraction work in general.

Since this general aim, it was of great importance not to declare some perfectly understandable notions of natural language as meaningless, or ill-formed. This lies at the ground of the attempt of chapter 7 §4, where we develop a semantics for a non-modal fragment of the language of our theory of concepts. What that paragraph wanted to model is the phenomenon of self-reference, which is for sure one of the key aspect of natural languages. This was one of the motivation not to accept a type-theoretic approach to the problem, which makes self-reference impossible. Similarly, our response to a revenge phenomenon was not based on declaring some concepts as illegitimate, as happens also with Linnebo's proposal. In fact, Linnebo has to impose a well-founded constraint on the individuation of concepts (i.e. concepts must be individuated through a well-founded process; therefore, if a concept cannot be individuated in this way, it is not a legitimate concept); on the contrary, we did not impose any constraints on what concepts are allowed: all concepts allowed in natural language are also allowed in our theory. Despite this lack of constraints, we manage to avoid the revenge paradox by noticing the particular behavior of nominalization with regard to impredicative concepts (see chapter 7 §5). I think that the particular behavior of nominalization of impredicative concepts is well justified, since it depends on the fact that impredicative definitions require the specification of a totality (set or plurality) of objects. It is fully natural to think that the same concept (the same intension) nominalized with regard to different totalities will give rise to different properties.

The last point we shall mention here concerns chapter 5, which sums up different arguments to the claim that FOL is not enough in the presence of absolute generality. Since one of the aims of the dissertation as a whole was to show that we cannot live without absolute generality, chapters 5 provides strong reasons to abandon Quine's prohibition of not going beyond FOL. HOL is as legitimate as FOL. Of course, as we argued, just going higher-order is not enough to solve the problem of absolute generality, but it is a necessary step to take. Absolute generality requires strong ideological resources, which a FOL simply cannot provide.

2. *What must be done*

There is still a lot of work to be done. First of all, there is some technical work to do in connection with the theory of concepts in chapter 7. Maybe the first thing to do is to try to prove consistency for the theory we introduced; secondly, the part on the dynamic theory of abstraction is just sketched, and many developments are required to make it a proper theory of abstraction. What we did was just to give an idea of how we intend to use abstraction principles to formally describe how abstraction work.

The theory of concepts only assumes that concepts are what predicates express (whereas a property is the correspondent first-order object). On this base the whole theory is an attempt to describe the behavior of concepts and their collapse into firt-order objects, namely properties. However, nothing has been said about the ontology of concepts and properties. Since concepts are open-ended and they do not change while changing their extensions, concepts can be said to be *universals*. Does our theory implies the existence of universals? If so, are they transcendent universals (*ante rem*) or are they immanent universals (*in re*) in the physical things? Or is our theory compatible with a more nominalistic view concerning universals, such as – for examples – a view that affirms that the physical things are made up of particular attributes (tropes) and universality (and so generality) is a matter of thought and language? While an *ante rem* position concerning universals seems to me to be quite implausible, I think that the theory of concepts of chapter 7 is compatible both with an *in re* theory of universals (as the one developed by David Armstrong), and with a tropes' theory that affirms that physical reality is made up of particular attributes, but recognizes the necessity of generality and universal concepts present in language (essentially it should recognizes that meaning is universal, while the language - considered as a physical phenomenon - can be recognized as made up of tropes). Much work should be done to understand the different developments these two views have for our theory of concepts.

But more work should be done also in set theory. Linnebo's justification of the modal approach is based on a certain metaphysical relation between sets and their elements: the elements are *prior* to the sets. This metaphysical relation excludes the possibility of giving a structuralist interpretation of set theory (see Linnebo [2008]). Sets in the hierarchy of sets only depend on their elements (consequently, a set of rank α depends only on the elements present in the ranks strictly less than α) and not on the whole hierarchy, as a structuralist account would imply. In fact, according to this latter view, a set would gain its identity only in relation to the totality of sets, and therefore to the whole hierarchy. I think that our insistence in a theory of concepts can suggest a way of justifying the modal approach to absolute generality here presented in a more structuralist fashion. The idea is simply to identify a concept with an implicit definition: for instance, the concept of ordinal number is implicitly defined by means of the set theoretic axioms of ZF_2. Of course, we know that ZF_2 has different models, i.e. interpretations that make true its axioms and whatever can be proved from these axioms. In this setting, letting the concept of ordinal to be implicitly defined by means of the axioms implies that the concept captures the structural properties of ordinals, i.e. the properties that remain the same in any model of the theory. At this point one can make appeal to the semi-categoricity theorem for ZF_2 proved by Zermelo [1930]. The theorem claims that two models of ZF_2 are either isomorphic to each other or one is isomorphic to a initial segment of the other, which mean that, if they differ, they differ only on the height of the model. This implies that how many ordinals there are is not a structural feature of ordinals, or - in other words -, the length of a well-ordering is not a structural feature of well-orderings. This allows to indefinitely extend both ordinals and well-orderings. As Zermelo [1930] explained, you can extend ZF_2 so as to have an indefinitely extensible sequence of models for stronger and stronger theories based on ZF_2. Each one of these models represents an initial segment of the whole hierarchy of set. They are exactly alike except for the height. From the point of view of a certain model, every smaller model of the theory are simply sets, which means that they have an upper bound that allows to give a direct structuralist interpretation of their elements: one can say, for example, that each set of a given model depends on the whole model, and not only on the sets present in the previous stages. However, the whole hierarchy is unbounded, and so this direct structuralist interpretation cannot be given, because there is no maxiamal model, namely a model for the whole hierarchy. It is at this point that the modal account enters into play: the necessity operator can be used to express the fact that a structural feature always remains the same however you extend the universe of sets; the possibility operator just says that you can extend a universe so as to make true a certain sentence (i.e. it says that the truth of a certain sentence is compatible with the structural features of the theory). In this way, one obtains a structuralist justification of the modal approach: since there is no absolute domain and no absolute model, we need the modality to capture the fact that there are structural properties that do not change while enlarging the domain of the model, and so that are valid however model you may consider.

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