Arbitrary Terms with No Arbitrary Object

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written by

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Abstract

This thesis investigates the topic of arbitrary reasoning: for example a reasoning starting with a stipulation such as “let n be any natural number”. We shall consider which construal of arbitrary terms, such as indeed ‘n’, is the most appropriate, in particular how much one can argue for a referential picture of the meaning of such terms. We will start with an Introduction (Chapter 0), where an informal introduction to the topic is given and where it is argued against a view which regards such terms as purely meaningless. Then we shall delve into the two most mainstream types of referential accounts, respectively in Chapter 1 and 2, concluding how they all seem unsatisfactory. Finally, in Chapter 3, an original proposal is suggested, which is referred to as the quasi-referential account: first by introducing it informally and philosophically, then by providing a possible formalization for it.

Chapter 0
Introduction

Mathematical reasoning strikes whoever approaches it because of its generality and abstractness. Typically, mathematics requires us to suspend our judgment on a plurality of aspects of the subject matter under investigation, restricting our attention to certain features, made as precise as possible. When studying the property of a triangle in an euclidean space, we do not care whether the triangle is made of rubber or concrete, or even whether it is made of any matter at all: rather then being refuted, those aspects are simply regarded as irrelevant, and thus the mathematician refrains from considering them. The assumption seems to be that whatever the triangle is made of, what we established in the geometrical study will still be true.

Such informal way of reasoning is actually crucial in all of mathematical domains. As mathematicians, we want to aim at general or universal statements: they are the most informative and powerful statements we have... but also the most difficult to prove. Indeed, how can we establish that all members of a certain collection of entities have a certain property? In some cases it may be possible to scan through each member and check that they have a certain property. For example, I want to find whether all numbers between 15 and 25 are less than 1000 if multiplied by 5: I do not need anything particularly smart, I can simply count and see. Clearly, this can be done for rather small collections. Nowadays, computer science is extending our possibilities, we may be able to individually check each member of rather large collections, but the domain of infinity, which is so present in contemporary mathematics, resists every finitary procedure. It simply is not possible to check each member of an infinite collection individually, does not matter the technology. In mathematics, however, it is full of universal claims regarding an infinite collection of entities: how are they established?

The key reasoning strategy used to achieve generality is the topic of this thesis: arbitrary reasoning. The idea is roughly the same I informally stated above in the talk about the triangle. When the collection can be grouped as those objects which satisfy a certain property F, then we can simply consider an object δ that we suppose has the property F – and we refrain from any other assumption regarding it: what can be deduced by only using the assumption that δ is an F can then be claimed about all elements of the collection. We shall call δ an arbitrary term (A-term); the collection of the Fs its *range* and the property F its stipulation-property[1]; a proof involving the use of arbitrary terms will be referred to as an A-proof. Let's consider an example from order theory applied to the real line to get a feeling of the different uses of arbitrary terms.

> Let $A \subseteq \mathbb{R}$ be non-empty and bounded below. Let $B := \{b \in \mathbb{R} | b \leq a, \text{ for all } a \in A\}$. We want to prove that $sup(B) = inf(A)$.
> Now, by definition of *supremum* and *infimum* and by definition of A and B, $inf(A) \in B$, thus $inf(A) \leq sup(B)$. Moreover, $b \leq sup(B)$ for all $b \in B$; since each $b \in B$ is a lower bound of A, $b \leq inf(A)$: then $inf(A)$ is an upper bound of B, hence $sup(B) \leq inf(A)$.
> Therefore we can conclude that $inf(A) = sup(B)$, for any non-empty and bounded below subset A of $\mathbb{R}$, where B is the set of lower bounds of A.

This proof may seem a little bit elaborate as an example, but it shows almost every use of arbitrary reasoning we will be interested in. The first arbitrary term which is introduced is A, whose range are all non-empty subsets of $\mathbb{R}$ which are bounded below; B is then introduced as an arbitrary term somewhat dependent on A: it is the set of lower bounds of A, but A was an arbitrary term: so the range of B should be looked as the collection of all sets of lower bounds of some non-empty and bounded below subset of $\mathbb{R}$. Or better, what can be said about B is what can be said of any set of lower bounds of some non-empty and bounded below subset of $\mathbb{R}$. Similarly, $sup(B)$ is dependent on B in a similar fashion: it seems like we can operate on the arbitrary terms with all those functions and descriptions which can be applied to objects satisfying the stipulation-property.

All the constructions made in the proof started from a single assumption on a newly introduced term A: therefore we must recognize a schematic nature to the proof. If we substitute any actual non-empty and lower bounded subset of $\mathbb{R}$ and we follow the constructions of the proof above (to obtain correct substitutes for the dependent terms), we will obtain the same conclusion. For example, let A be the set of all positive integers. Then B will consequently be $= \{r \in \mathbb{R} | r \leq 0\}$; and it is easy to see that $inf(A) = sup(B) = 0$: every sentence or formula occurring in the proof will still hold. Technically, even if we picked for A an empty or not-lower bounded subset, the proof would not be negated: it would simply be vacuous, as a conditional with a false antecedent.

So the basic idea for the justification of the validity of the generalization step in A-proofs seems to be that, by refraining from further assumptions, we can obtain a proof-schema in which we can substitute (for the arbitrary terms) constants standing for objects which satisfy the stipulation-property. Put differently, since in the proof no further assumption (other than "δ is an F") is

[1] Maybe two words on the fact that some people regard it as a description not a stipulation, for it describes the arbitrary object which extsts already

made, then the proof can be applied to anything which is an F, and it will yield the desired conclusion. We will call this explanation the minimal justification (MJ).

I believe (MJ) is reasonable and there should be general agreement on it. It should not be controversial that A-proofs play the role of a schema. This is indeed the way in which the matter is typically presented in mathematical textbooks. However even the way in which I referred to this explanation (i.e. the "minimal" one) should betray my suspicion that this is not all of the story. By adopting (MJ) as a sufficient explanation, we are pushed towards a view of A-terms as mere placeholders. The reason being that, as we take an A-term to refer to some object, it is not clear anymore why refraining from certain assumptions would allow the generality-step: the worry is that we are still reasoning about a single object, and aspects of this object can infect the generality of the proof even if not explicitly stated in assumptions (we shall see an instance of this in Chapter 1, when presenting an argument against using ignorance about the referent as a further justification for the generality-step). On the other hand, if the term is only a placeholder, there is no implicit content that can damage the proof. This is I believe the most mainstream view on arbitrary terms: we look at them as variables that we implicitly quantify over.

But can we reasonably maintain a position under which A-terms are just placeholders? The intuitive concern is that the feeling you get as you work with arbitrary terms, is not that of a mere sign. But of course, this is not enough to many, so I shall propose a better argument.

The goal of this argumentation is to give reasons to believe that schematism (the view that arbitrary terms are mere placeholders) cannot be regarded as an exhaustive explanation of how A-proofs work. Let's consider a stipulation: let δ be an F; and let's suppose that δ is a mere placeholder. What is the role of saying that δ is an F? that is, what is the role of the stipulating-property? It seems like we have two options. One is to regard also the predication as meaningless: we are not really interested in what the property means, on how it is semantically interpreted: the whole proof is just a scheme. Under this view, the operations we perform on the terms and on the formulas in general during the proof, must then be regarded as pure syntactic manipulations, applications of a derivation system. I believe that this option is too hardcore: people do not use arbitrary reasoning as a purely syntactic operation. One thing is to simply state that arbitrary *terms* are mere placeholders... and it may be plausible in this case to argue that the "intuitive experience" is not relevant; but by reducing the entirety of an A-proofs to a bunch of syntactic operations, the distance between the way in which mathematicians actually work and the proposed philosophical picture is perhaps too much. I shall not consider this option further.

On the other hand, we may want to regard the predication as meaningful: the term is a placeholder but the stipulated-property and the other facts we obtain are meaningful, not purely syntactic. The problem with this view is, I believe, that by attributing some referential meaning to the stipulated-property, then we must also attribute *some* referential meaning to the A-term: at least we must say that δ refers to some F. Typically, schematists answer that δ

does not need to be an F in order for the proof to still be valid (as we said above, by plugging a non-F in, it would became vacuous); however it must be admitted that it is utterly ridiculous to pretend that while working with δ a mathematician is not restricting the scope of her focus on the Fs. In the proof above, if you tried following the passages, I doubt you ever considered A as a triangle and vacuously followed the steps. The truth value of the steps would stay the same, but it would be simply impossible to *produce* a proof reasoning this way. In sum, if we concede a semantical role to the predication "being an F" in the scope of the A-proof, it is not plausible not to also concede that the term δ, by being described as an F, does not become partly meaningful too.

The schematist would probably try to explain away this meaningfulness of the placeholder by saying that it is a dedicated variable, that we implicitly quantify over in the context. This is for example what Pettigrew (2008) argues for, despite in a different debate than the one we are considering now. So it is meaningful, the predication tells us something *about* it, but this happens only thanks to the hidden quantification. The problem with this is that it does not seem that we are implicitly quantifying: the fact that one can find a quantified statement which is true in exactly the same conditions as the statement involving the arbitrary term does not mean that the meaning of the latter can be reduced to that of the former. Indeed, one can perfectly well claim the quantificational statement to be unclear and explain it using a statement involving arbitrary terms. There is surely a connection between arbitrary terms and quantification, the former are ways in which we investigate generalities, and general truths help us investigating arbitrary representatives of our concepts, but given the fundamentality of both type of statement, trying to reduce one to the other always looks like trying to reduce the chicken to the egg (or perhaps the egg to the chicken).

Therefore, I believe that the wiser approach is not to rush but rather attribute, as we intuitively perceive, some meaningfulness to the arbitrary term; this work will try to analyze how much we can regard this meaning as referential. I will not argue anymore against the schematic view, and I shall just consider the so called referential accounts of arbitrary terms. However, as it will be clear reading the first two chapters of the work, I will not defend a purely referential account either: rather I shall call my proposal quasi-referential and the idea will be to interpret the effect on reference that the predication has as some sort of *directioner*, which points the reference relation towards the Fs, but not deeply enough to determinately pick out a referent. But we shall see this afterwards. Now we shall conclude this introduction with a brief discussion on the origins of arbitrary terms in modern times with Locke's and Berkeley's debate, than some of the more contemporary concerns regarding the postulation of arbitrary objects. Finally we shall consider the contents of the thesis.

A very brief history

Despite the intuitive fact that, as we work with an arbitrary term, we do not refrain from attributing any meaning to it, the schematic approach has been the mainstream option for a long time. At least until the Eighties, with Fine's pioneering work *Reasoning with Arbitrary Objects* and Horsten's *Metaphysics and Mathematics of Arbitrary Objects* in 2019.

Although the use of arbitrary terms to reason in generalities probably dates back to the origins of deductive reasoning and mathematics, a conscious study into how they work and what they stand for is more recent. It is probably even possible to find traces of it in the debate around universals that crossed all medieval philosophy, but here – in this introduction – I will only consider Locke and Berkeley's famous debate around general ideas. I am no historian of modern philosophy, and the purpose of this presentation is not to enter an hermeneutic debate around the right interpretation of Locke's or Berkeley's position. I just want to broadly characterize their view and see where they disagree, simply because it is useful to also make the contemporary debate more clear.

Roughly speaking, Locke can be seen as a reificationist ante-litteram, while Berkeley a critic of such position. For Locke, we basically have an ability to abstract from all the individual ideas we perceive and obtain what he calls *general ideas*, which indeed refrain from any particular property of the individuals of the collection we abstracted from. As we can read from the following passage picturing how a child comes to form the idea of man:

> "Afterwards, when time and a larger Acquaintance has made them observe, that there are a great many other Things in the World, that in some common agreements of Shape, and several other Qualities, resemble their Father and Mother, and those Persons they have been used to, they frame an Idea, which they find those many Particulars do partake in; and to that they give, with others, the name Man, for Example."[2]

Therefore, such *general* (or if you prefer generic) ideas will correspond to an arbitrary bearer of the represented property. In the case in the passage, the idea of man is formed, by constructing a model of man, which is constructed by retaining the properties that are found common throughout all the people that were met, while those qualities which are possessed by some but not by others are withdrew.

On the other hand there is Berkeley. I am hesitant to characterize him as a pure schematist, for he does not regard a general name as meaningless. His focus is more on rejecting the existence of general ideas as the representation of a generic bearer of a certain property. That is, every time we have an idea of man, we picture a particular man, with eyes of a certain color and so on; nowhere in the mind can be found this general idea. Nothing as a generic man is ever entertained in the intellect. The generality of general names, such as indeed

[2] [18], p. 411

"man", does not consists in them being a sign for the generic man, but rather in the fact that they can suggest several particular ideas (several representations of human, one with long hair, one with blue eyes and so on):

> "But it seems that a Word becomes general by being made the Sign, not of an abstract general Idea but, of several particular Ideas, any one of which it indifferently suggests to the Mind."[3]

If we remember that Berkeley has a very idealistic metaphysical view, we can probably translate this for our purpose as saying that an arbitrary term does not achieve generality because it refers to a generic object of some sort, which refrains from particular aspects, but rather by the fact that any particular object (any particular object which is relevant – if we are talking about an arbitrary triangle, the stray dog eating from my garbage can does not apply) can be indifferently associated with that term. What Berkeley wants to strongly reject is that something as a general idea or object can exist: everything that exist is particular and thus fully determined. The "indeterminacy" comes from the name being able to select a referent indifferently (we would probably now say arbitrarily). He proposes a form of argument in Introduction X, which became very famous in the debate concerning arbitrary objects. I shall propose it here in the form which became mainstream, which regards an arbitrary natural number. The argument challenges the idea of a number which only has those properties which are common to all numbers.

> Let n be an arbitrary natural number.
> Every natural number is either even or odd, thus n is either even or odd.
> Not every number is even, thus n is not even;
> Not every number is odd, thus n is not odd.
> Thus n is either even or odd; but neither even, nor odd: a contradiction.

There are different ways to deal with this argument, to which in what follows we shall refer to as Berkeley's paradox. For example, Horsten argues that the arbitrary number n is actually not a natural number; while Fine defines a mathematically precise and elegant way to have an arbitrary object satisfy a disjunction without satisfying any of the disjuncts. We can even find some echo of it in this famous passage from Frege, often quoted as an example of a case against arbitrary objects

> "The author [Cuzber] obviously distinguishes two classes of numbers: the determinate and the indeterminate. We may then ask, say, to which of these classes the primes belong, or whether maybe some primes are determinate numbers and other indeterminate. We may ask further whether in the case of indeterminate numbers we must

[3] [2], Introduction XI, p. 15

> distinguish between the rational and the irrational, or whether this distinction can only be applied to determinate numbers. How many indeterminate numbers are there? How are they distinguished from one another? Can you add two indeterminate numbers, and if so, how? How do you find the number that is to be regarded as their sum? The same questions arise for adding a determinate number to an indeterminate one. To which class does the sum belong? Or maybe it belongs to a third?"[4]

The concern again is the same: if we admit generic objects, how can we characterize them in a consistent way? The issue is surely not trivial, but contemporary defenders of arbitrary objects have found plausible ways to do so.

Of course, there are also authors today that share the worry for arbitrary objects, and prefer to attribute, on the same line of Berkeley, the arbitrariness to the act of reference rather than the object denoted. Even Frege's own position on the matter can be seen as sharing this last idea:

> "Of course we may speak of indefiniteness here; but here the word 'indefinite' is not an adjective of 'number', but ['indefinitely'] is an adverb, e.g., of the verb 'to indicate'. We cannot say that n designates an indefinite number, but we can say that it indicates numbers indefinitely. And so it is always when letters are used in arithmetic, except for the few cases (π, e, i) where they occur as proper names; but they designate definite, invariable numbers."[5]

Among the authors that we shall consider embracing this stance there are Martino and Breckeridge & Magidor. Also the position that I shall propose follows the same lead, but differs quite radically from the declination that these authors give to it.

Above we saw a form of argument that we referred to as Berkeley's paradox. Before concluding this introduction, I shall present an other, more technical, paradox, which was probably invented by the polish logician Lesniewski, and which again attacks the reification of arbitrary terms into arbitrary objects. The paradox runs as follows. We let $P(x)$ be some property in our language and we introduce an operator $\delta x.-$, which basically applies to formulas with one free-variable, denoting an arbitrary object satisfying that formula. Then we proceed as follows.

Let $\delta x.P(x)$ be the arbitrary $P(x)$ and let $Q(y) \equiv y = \delta x.P(x)$. We said that an arbitrary object has exactly the properties which are common to all of its range, thus:

$$Q(\delta x.P(x)) \quad \rightarrow \quad \forall x(P(x) \rightarrow Q(x))$$

But then this means:

$$(\delta x.P(x) = \delta x.P(x)) \quad \rightarrow \quad \forall x(P(x) \rightarrow (x = \delta x.P(x)))$$

[4] [10], p. 160

[5] [9], p. 109

Since, it seems, we must admit that $\delta x.P(x)$ is equal to itself, as all Ps are, then we obtain the absurd result that everything with the property P is identical with the arbitrary P. We will see how one can avoid these paradoxes.

Concluding Remarks and Thesis Structure

Summing up what we established so far, the least we can say is that what we can prove about the arbitrary term is what follows (under the inference rules and theories which are accepted in the mathematical context of the proof) *only* from the assumption that δ is an F. However, if one wants to recognize some referential aspect of δ, the natural concern is: what are we actually referring to? which one of the members of the collection is it? is it something distinct from the collection? In the literature we essentially find two semantical options to construe the meaning of an arbitrary term. Suppose we say: let δ be any F. Then, either δ is considered as one of the specific Fs, the individuals we normally predicate F about: if for example F is the property of being a natural number, then δ will refer to 0 or 1 or 2 or 1258693 and so on. Typically, since no further information is given in the stipulation of δ but that it is an F (or perhaps a natural number), it is impossible to determine which of the specific numbers we are actually referring to; hence such accounts usually opt for an epistemicist position, where the referent is unknown by any mathematical agents. They will be considered in detail in Chapter 1: *Arbitrary Terms as denoting Specific Objects*. The other standard option we can find in the literature is also the most mainstream one among metaphysicians and philosophers: the arbitrary term does not refer to one of the specific objects, in the example considered, it does not refer to any of the natural numbers: rather it refers to some other higher type of entity, an arbitrary *object*. Such a position must then explain and formalize the connection between the collection of specific objects and the arbitrary object which ranges on it. We will see such positions in Chapter 2: *Arbitrary Terms as denoting Arbitrary Objects*.

The referential accounts encounter a new problem that we shall consider carefully and which was not present for the schematist. The schematist view and the minimal explanation are tightly related: if δ is not a mere placeholder, what grants us that (MJ) is true? if the term genuinely refers, it is not clear why there should not be properties which may be true about δ, but should not follow only from the assumption that $F(\delta)$ (or viceversa). More informally: how is it then that we can pass from a discourse on a single object to a general statement?

What stops us from implicitly using further assumptions on δ? The types of account analyzed in Chapter 1 use the fact that we ignore the referent as an explanation. The accounts of Chapter 2, design the arbitrary object as having only certain desirable properties. We shall see how both are not satisfactory. After this *pars destruens*, in Chapter 3 I will then move on to present a philosophical construal of the matter which aims at avoiding the problems of both positions. I shall denote this proposal as the *quasi-referential* account, and its core idea is to apply the line of thought which we use in arbitrary reasoning to the meta-description of the reference relation. By doing so, I will argue that we are able to maintain that the referent is a specific object (as for the accounts of Chapter 1), but that none of the specific objects can be pointed out as referent (as in the accounts of Chapter 2). At the start of the chapter I will list some desiderata for an account of arbitrary term and I will then argue why my proposal satisfies them, contrary to the accounts considered in the first two chapters. Finally I will present a formal construal of the matter, which will allow us to represent our reasoning of arbitrary objects in a satisfactory way.

Chapter 1
Arbitrary Terms as Specific Objects

In this chapter we will consider the first type of referential account present in the literature. The authors considered here typically strongly reject the idea of an arbitrary or generic object, while also rejecting a purely schematic or syntactic view of arbitrary terms. They want to maintain that such terms genuinely refer, but not to any sort of ambiguous, generic or indetermined object: it simply refer to one of the specific objects in its range. Let δ be an arbitrary F. For sake of clarity in the following discussion, let's define:

(A) it is determined that 'δ' refers to an F

(B) it is determined which specific F

Clearly, (A) is the uncontroversial side of the statement that 'δ' refers to an F,[6] while (B) is the controversial one, for which the accounts here must provide more explanations. For if I consider an arbitrary natural number n, unless I am a schematist, I will be fine conceding that 'n' refers to a natural: but why should it refer to 2 instead than 362? As we have also seen in the introduction, these group of authors wants to treat the act of reference as arbitrary, thus indicating indifferently a range of objects. However, they also want to assert (B): how can one maintain both? on one hand, the term refers to each number arbitrarily; on the other, there is one among the other numbers which is actually denoted? Typically these authors make an epistemic move, by saying that such true referent is determined, but *radically unknown*: we are completely ignorant about it, hence we can regard the term as denoting arbitrarily. It is the act of reference fixing which, due to its epistemic inaccessibility, makes the choice random or arbitrary. That is, A-terms are, semantically speaking, just proper names (or numerals if we are in an arithmetic context), the difference lying in what we do and can know about the object they denote.

[6]Of course, as with all philosophical debates, there are no purely uncontroversial positions. For example, Horsten, that will be discussed in Chapter 2, would not admit it.

The linguistic identification between A-terms and constants is confirmed by what Breckeridge & Magidor say in their definition of the notion of arbitrary reference:

> "It is possible to fix the reference of an expression arbitrarily. When we do so, the expression receives its ordinary kind of semantic-value, though we do not and cannot know which value in particular it receives."[7]

The natural question which then arises is what determines the referent, if it is radically unknown, hence unknowable? As we will see, authors differ quite radically on this point.

The validity of the generalization is then justified and explained by appealing to this same unknowability of the referent: it is because we do not know what the referent is, that we can keep the proof at the right level of generality. Martino explains the point nicely:

> "...the ignorance of which number one is referring to has the desired effect to grant generality to the reasoning: what is provable for a completely unknown number holds necessarily for all numbers"[8]

Moreover, the approach seems to construe the relevant statements occurring in A-proofs at face-value: when reasoning on δ, we are actually working with some object and this object is an F in the most standard sense. Not everything that shines, however, is gold. As we will see there are serious problems both with this type of proposals on a general level, and with the specific positions of the authors that will be analyzed. I will now present four general objections to the framework. Afterwords, I shall consider two accounts which instantiate the general approach, namely those presented respectively by Breckeridge & Magidor and Martino. Finally I will consider an account which was proposed more recently by Breckeridge in response to some objection which was raised against the position he outlined with Magidor.

Suppose we are working with an arbitrary natural n and we prove that $F(n)$; then (since generalization on arbitrary terms is valid), we can conclude that $\forall x F(x)$. Then if n is referring to a specific natural number in the sense explained above, this means that one of the inferences

$$F(0) \vdash \forall x F(x); F(1) \vdash \forall x F(x), ..$$

must be valid, which is absurd[9]. This holds if we accept that, when two terms have the same referent, then they satisfy the same formulas – which should be unproblematic.

Secondly, since the justification of the validity of A-proofs in these accounts depends upon the fact that we do not know which the referent is, then they

[7] [3], p. 1
[8] [19], p. 65
[9] This argument was suggested by Luca Incurvati

must assert that such referent is not contingently unknowable, but rather absolutely unknowable: it is not a matter of mathematical progress whether we will discovery or not what the referent of the arbitrary term n is, we simply will not – and getting to know it, paradoxically, would ruin the validity of the generalization step. The consequence of this is that we posit a dimension of meaning which is epistemically and absolutely inaccessible (on pain of the validity of the proof); I find problematic and counterintuitive that something can be part of the meaning of a term even if no possible speaker can deal with it. This critique may be seen as perhaps too general, for it does not simply apply to such theories of reference, but in general to epistemicist positions. This is a point I must concede, however notice that the critique applies to a certain subclass of epistemicist positions, where the radically unknowable aspect is not only not accessible, but where it is also impossible to make any progress towards the meaning-theoretic content which is postulated.

Thirdly, notice that their type of explanation for the validity of the generalization will be also available in any account which does not accept (B), for if some linguistic fact is not determined, then it is also not knowable. Moreover, the explanation undergenerates: we could know what the referent of the term is, but still refrain from considering aspects of it which do not follow only from it being an F. That is, this type of explanation leaves open the question: why do we use arbitrary terms? what is the point of them if, as we saw above, they are linguistically identical with constants, and the difference which is posited with respect to the latter (ignorance on the identity of the referent) is not necessary to account for the validity of the A-proof (for in this scenario I could simply refer explicitly to the number 2, while refraining from the specific information). Brekeceridge & Magidor comment on this and say that the latter case would be more prone to errors, while, since it is not possible to know the referent of the arbitrary term, ignorance would protect as better from error. I shall argue in the next section that ignorance does not seem enough to save us from mistakes in the first place. Moreover it is plain that by accepting this explanation, if the only advantage is to be less prone to errors, the reason why we us arbitrary terms becomes quite contingent and accidental.

Now, what could be the reason why these authors want to assume this further fact (namely (B)), which seems to play such a small (perhaps non-existent) role? Frege analyzing the issue of arbitrary objects, says (considering an arbitrary number n):

> "We cannot say that 'n' designates an indefinite number, but we can say that it indicates numbers indefinitely."[10]

Both Breckeridge & Magidor and Martino seem to partially agree on that, for they want to avoid having to deal with ambiguous objects. However, they do not fully embrace the thesis on the indeterminacy of the act of reference: the ambiguity of arbitrary reference is not due to the act of reference itself (which well-determinately fixes a referent in the sense of (B)), but to our epistemic

[10][8], p. 658

limitations. The reason why they avoid saying that the act of reference itself presents elements of indeterminacy (or refrainment more in general) seems to be the assumptions that when using an arbitrary term '*a*' in a proof, it "indicates the same object in all its occurrences in the proof" [11]. As we can read farther from the same source:

> "Consider, e.g., the talk:" Take an arbitrary real number. . . . Suppose it is irrational. . . ." Which number does the pronoun "it" refer to? Of course the correct answer is not "to any real number", but "to the number under consideration". In this answer the definite article is used just for referring to the number introduced by the indefinite article."[12]

The point to make is whether we need to be determined that the arbitrary term refers to some specific object, or to be determined which object. I claim that again, the latter seems to be not necessary and that it even cause problems on its own. If (A) holds, then there is space to determine the referent further with other suppositions such as in the case in the quote, without incurring in any problem: the range of the term gets simply restricted. It does not seem to me that an argument regarding arbitrary terms requires the referent to be stable in the sense of keeping denoting the same specific object, what it requires is that it keeps falling in the scope of the information which is supposed in the course of the proof. What this means, considering for example the quote above, is not that we can take as an arbitrary real number $1/2$ and then, as we suppose it is irrational, we change it to π. Simply, no referent is given in the standard sense and no choice is made. Now, if on the other hand we introduce "let r be a real" and r refers to $1/2$, then it is very difficult to see how the further supposition, "let r be also irrational" has to be taken seriously. This raises the fourth general problem: if r selects a specific object as referent and it is somehow semantically determined which, then what tells us that a further supposition we make will not exclude this actual referent of r, making the proof fundamentally vacuous? So the problem of continuity of reference, rather then giving reasons to believe in the determinacy of the specific referent, undermines it.

I will now consider two particular accounts which I consider close to the framework I just delineated and attacked, namely the one presented by Martino and Breckeridge & Magidor . Finally I shall consider a further account that Breckeridge proposed recently and which constitutes a nice transition towards the accounts of arbitrary objects.

Breckeridge & Magidor

Breckeridge & Magidor position, if I understand correctly follows quite precisely my characterization. They argue that the stipulation itself fixes the referent among the specific objects in the range, but that we cannot know this fact. We

[11] [19], p. 66
[12] [19], p. 66

saw before in the presentation to this general framework, that these authors at this point need to explain what determines the referent, if no one knows it. They are aware of it and indeed consider an argument, that we may rephrase as follows. Let n be an arbitrary natural number and suppose that this expression fixes the value of n at 2: then that n refers to 2 is a semantical fact which does not seem to be determined by any use fact[13], despite being commonly held that semantical facts superveine on use facts. Breckeridge & Magidor agree that the semantical fact in question is not determined by any use fact, indeed they say:

> "...we propose that nothing determines which Frenchman is referred to, nothing, that is, other than the semantic fact that we have referred to the particular Frenchman in question."[14]

Indeed they conclude that

(*) semantical facts do not superveine on use facts.

Now, I do not think it is necessary to go into the question of whether (*) should be accepted or not. The problem with their position is not that no use fact determines that 2 is the referent of 'n', but that seemingly no semantical fact does so either. Even admitting (*), one can argue that the not-superveining determined semantical fact is only that 'n' refers to a natural, not which natural in particular[15]. We can agree that in some case, especially in ordinary language, the context may allow the stipulation to select a specific referent; however it is not so in general, especially in the context of a mathematical proof. Moreover, (*) would not imply that semantical facts can be determined without the determination having any type of role whatsoever in discourse and linguistic practice. The fact that 'n' refers to 2 cannot play any role in any proof, for we would lose generality. Breckeridge & Magidor are not simply getting (*), but also that there are semantical facts which exist without having any consequence on linguistic practice: (B) ends up being an isolated fact, not just without causes, but more importantly without consequences.

Breckeridge & Magidor then deal with the following concern: if 'n' refers to 2 or whatever, how can it be possible to validly generalize to all numbers? This is connected with our first objection. The core of their justification is by appealing to the referent being unknown, here we shall see it in more detail. They make a distinction between what they call proper and improper inferences. If I understand correctly, a proof $P \vdash C$ is proper if for any model M, if $M \models P$, then $M \models C$; while it is improper if, if for all models M $M \models P$, then for all models M $M \models C$. Their move then is to say that (UG) is an improper inference, but not a proper one (notice that all proper inferences are also improper in classical reasoning): we just need to accept the inference if

[13]What they mean by use facts is not exactly clear, however it is something including "facts about one's environment or facts about which properties are most natural", but excluding facts such as: the name 'Pierre' refers to my neighbor.

[14][3], p. 3

[15]This is the position in Assadian & Sbardolini(2023)

$F(n)$ is valid, that is (given soundness/completeness results) if we have a proof for it, for then it does not matter what model we consider (hence which referent we pick for 'n'); however we cannot pass from the truth of $F(n)$ in a model to the truth of $\forall x Fx$ in that same model.[16] Now what they claim is that thanks to the ignorance, we manage to provide a valid proof for $F(n)$, instead of a proof of the truth of it with respect to a specific model which sets its reference: ignorance provides a justification of (UG) as an improper rule of inference:

> "All we know is that Jane is some person or other. Thus in demonstrating that Jane has a certain property, the only properties of Jane that we can appeal to are ones that Jane shares with all other persons."[17]

Put differently, the proof we have of $F(n)$ cannot use any property which is not common to all numbers, because we do not know which of all numbers n is. There are different problems with this explanation.

First of all, the first objection still applies: if (B) is given (and say 2 is the referent), than from $F(n) \vdash \forall x F(x)$ being valid (properly or improperly) we will obtain $F(2) \vdash \forall x F(x)$; simply because '2' and 'n' denote the same object, and so when something is true about the former, it will be true about the latter – and so we obtain the universal statement. Moreover, everybody will agree that (UG) is improper, the issue is – given this account – why is it not proper? given (B), we should be able to make the proper step (at least in those models that are correctly interpreting 'n').

Secondly, simple ignorance around the denotation of a term does not imply that the assumptions used in the proof do not rely on some of the unknown (but well-determined) facts. Suppose for example that I was a mathematician and I was reasoning about $5 + 3$; unfortunately I am not very smart and I cannot see that the addition corresponds to 8, I only know that it is included in the interval $(1, 10)$ over the naturals. Despite my limited knowledge on addition, I know how to distribute multiplication over additions and also how to multiply, and so I prove that $(5 + 3) * 2 = 5 * 2 + 3 * 2 = 10 + 6$. Now, this is clearly not a good ground to conclude that all numbers between 1 and 10 are equal to $10 + 6$ if multiplied by 2. The general lesson is that lacking knowledge does not equate with the ability to refrain from the unknown information in the proof, and it thus fail to provide the justification of (UG) as an improper inference. Again, they would have to talk about absolute ignorance or something of this sort, to avoid counterexamples similar to mine, but this as we have already seen is problematic.

Finally, once we accept the inference as improper, it is even more difficult than before to see the role of (B). For if the statement $F(n)$ has to be considered across all models (indeed we need to check whether it is valid or not), then this means that whatever is the real referent of n as given by (B), this will be on

[16] We can also have the distinction (at least in classical logic) by saying that $P_1..P_n \vdash C$ is proper if $\vdash (P_1 \wedge .. \wedge P_n) \rightarrow C$. This does not account for infinitely many premises of course, but it gives the idea.

[17] [3], p. 22

the exact same plane as all the other possible referents. And if there is some privileged model, that gets the right referent, we cannot privilege it in the proof.

Martino

Martino's position is slightly different than Breckeridge & Magidor's, but I believe it still falls under the general characterization. In a passage he says:

> "To consider an arbitrary number means to imagine that a number has been fixed. Imagination is all is required for this kind of reference. Arbitrary reference rests on our ability of imagining that an object of the universe of discourse has been fixed."[18]

By relying on imagination, one may suppose that his positions may be different than Breckeridge & Magidor's, for if we only require to imagine that a referent has been fixed, than what we need is only (A) and not (B). However, it seems that what Martino means is that this act of imagination also fix a determined referent in the sense of (B). Indeed later in the essay we find:

> "Referring to an arbitrary object a amounts to supposing that "a" designates an unknown, though well-determined, object."[19]

I struggle to make this compatible with a rejection of (B), for if the specific identity of the referent is not determined, then there would be no reason to appeal to the epistemic dimension of it. Most of the objections I proposed for the general framework and for Breckeridge & Magidor will thus still apply; let's see however where the originality of this account lies.

In the (idealized) picture that Martino proposes, "arbitrary reference is a sort of *direct reference* based on an imaginary *choice act*"[20]. We must imagine to have direct access to an imaginary and ideal mathematical agent, which has direct access on mathematical objects and is capable of choosing in a completely arbitrary way any of them. Interestingly, when picking an arbitrary F, the ideal agent is not restricting the scope of choice to the Fs, but simply picking an object a randomly and supposing that $F(a)$. Accordingly the following principle is stated and assumed:

(CAP): *Every object of the universe of discourse is capable of being chosen by the ideal agent.*

Now, the generality of the proof obtained from the assumption is given by our ignorance about the choice of the ideal agent.

I believe that a first type of objection specific for this account could question relying on an ideal agent. It seems like such an ideal agent would always be an explanatory device, rather than saying anything literally true about our mathematical practice: indeed Martino uses it to answer "how is it possible for

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[18] [19], p. 66
[19] [19], p. 67
[20] [19], p. 69

an object a to get fixed?". I already argued above that I do not believe necessary for an object to be fixed in the sense of (B) throughout an A-proof and that fixing it on the other hand creates problems with successive suppositions we make on the object.

Moreover the very idea of an ideal agent complicates things on its own. Let's consider the following dilemma for example. If the ideal agent chooses an object arbitrarily and knows the choice – and if ignorance is what provides justification – then the proof will be invalid for the ideal agent. If on the other hand the agent can still recognize the proof as valid, then ignorance is not the right justification for the validity. If finally we want to argue that the agent does not know the choice she makes, then I would question the effective role that this agent has, for what would be the difference from a random choice of a normal mathematician (which as we saw in Breckeridge & Magidor does not seem to work either).

Thirdly, Martino argues that when picking a arbitrary F, the agent does not restrict the scope of the choice to the Fs, but rather picks anything from the whole universe of discourse and then suppose that it is an F. We shall see more about this in next section, but so far let's notice that if the agent does not restrict the scope of the choice, then when picking an arbitrary even it could also pick an odd number, say 3, and suppose it was even. But then the proof gets vacuous, in a similar way as in the case of nested suppositions in the fourth objection to the general framework.

The reasons that seem to bring Martino into the ideal agent lore and in general into the assumption of (CAP) is that the following type of reasoning can be said to be correct *only* under the hidden assumption that every object in the domain of discourse is capable of being chosen:

> "*...let a be a member of* α; by the pairing axiom there is a set β whose only member is a."[21]

I personally do not see how such an instance of arbitrary reasoning would need every object of discourse to be capable of being chosen. Especially I do not see the difference with other instances of arbitrary reasoning. If Martino's position is that any arbitrary stipulation needs (CAP) to be accounted for, then we could argue against it by using similar arguments to that for the "stability" of the referent – in general the fact that a choice does not seem to take place. If on the other hand this type of proof has something special that I am missing *mea culpa*.

Martino, especially with Carrara in [4], actually moves on and consider these ideas in their application to a huge variety of topics, ranging from Boolos' plural quantification, to discussions of the ontological innocence of mereology. I will not consider such applications here, neither the fruitfulness of the account of arbitrary terms proposed with respect to those topics.

[21] [19], p. 70

Breckeridge renewed

Breckeridge in a manuscript called *Existential Instantiation, Arbitrary Reference and Supposition* (2018?) argues for a different position than the one proposed with Magidor considered above. He mentions different problems that a position of this sort encounters, but one especially got particular attention. This was proposed by Haze in [12] and runs as follows. Suppose there are unreferred-to objects and let σ be any of them. If σ refers to a specific unreferred-to thing as it is argued in [3], then it seems we are getting an absurdity. Moreover neither it seem the case that we can dismiss the supposition that there is some unreferred-to object, hence we cannot treat it as a case where the description fails to be satisfied by any object.

I find an argument like this paradoxical, probably following a scheme related to Berry's paradox, as Gutierrez argues in [11]. She notices that no contradiction seems to follow anymore if we adopt a stratification of language (as it happens in Berry's paradoxes) and that the predicate in question seems to entail some impredicativity. We shall come to the paradoxical aspect of the argument in a moment, let's first see what is the philosophical implication that Haze must draw (and actually draws) if the argument is successful.

It seems that if we reason as Haze, thus concluding that 'σ' is not referring to any unreferred-to object, then a legitimate consequence (if we assume that 'σ' is referential) seems to be that $Unref(\sigma)$ is false (despite being our starting assumption). The way in which Haze answers in [13] is something along the line of schematism: we should construe 'σ' as a mere placeholder, not referring to anything at all: in this way we block the inference from 'σ' not referring to an unreferred-to object, to σ not being an unreferred-to object. Indeed this is the type of option which asserts that when doing an A-proof starting say with $Let(\delta, F)$, a statement like $F(\delta)$ is not genuinely meaningful unless an hidden quantification is supposed to take place: an A-proof is merely the skeleton of a proof, which can be dressed with the specific objects which F's. The limits of such a position were already discussed in the introduction.

Now I shall show that regardless of the theory of reference (or not-reference) that we are adopting, we can still obtain a contradiction from the stipulation "Let σ be such that $Unref(\sigma)$" and in general the assumption that there are unreferred-to objects. Moreover it will be clear that the strategy of stratification of language still applies, indeed suggesting that the real problem is with the predicate $Unref(x)$ and not the theory of reference adopted for 'σ'.

Suppose $\exists x Unref(x)$ and let σ be any unreferred-to object. Let 'δ' be a new term and set 'δ' to refer to σ: then σ is not unreferred-to anymore. Thus we can conclude that $\exists x Unref(x) \to \bot$.

I do not see any easy way out for Haze. It seems that in order to reject the paradoxality of his argument in [12], he also needs to reject the paradoxality of the one above. Moreover, even on its own, the argument strongly points toward some intrinsic problem with the notion of being never referred-to. Notice that Haze needs a rather strong construal of such notion: if for example we take it to mean that we never referred to it *so far*, then a referential account does not have problems since we can take the arbitrary term introduced to be new. It seems that we need unreferred-to to mean that it is never-referred-to, maybe even that it is impossible to refer to: but such strong construal end up contradicting the fact that we can always pick a new term and make it refer to *any* unreferred-to object. Moreover the argument I just proposed does not use any theory of reference and the contradiction obtained cannot be explained out in terms of it. If Haze wanted to say that the stipulation "let 'δ' be a new term and set 'δ' to refer to σ" is not legitimate (or presupposes some theory of reference), this does not seem to be doable without a *petitio principii*: it seems indeed that the following is perfectly legitimate: let n be a natural, let 'w' be a new term and let w refer to n. Normally, w is just a redundant term, that basically plays the role of the sign 'n' – or anyway does not cause contradictions (neither presupposes any theory of reference, at least no theory of reference which is not suggested already by ordinary arbitrary stipulations).

This opens the related issue: how to account for cases of arbitrary stipulation when there are no F-things? Under Breckeridge & Magidor, it seems that if δ refers to a F-thing, but there are no F-things, then we must say that δ simply fails in achieving its meaning. However, the concern goes, we can use such terms in proofs, such as a *reductio ad absurdum.* Before getting into Breckeridge's solution, let's see how this problematic is actually not concerning for a position which accepts (A), but refrains from (B).
There are two cases: either we have a proof of the fact that there are no Fs; or we do not know. The former case is not problematic: if we proved that there are no Fs, we would not consider legitimate to pick an arbitrary F. In the latter case, before legitimately introducing the arbitrary term, we first need to suppose that there are some Fs. If we prove that $\bot(\delta)$, then what gets negated is the existential supposition, not that what 'δ' refers to is an F (that is the meta-theoretical statement (A) for 'δ'): 'δ' still points towards the Fs – only there is nothing there.

We now come to Breckeridge new position as sketched in the manuscript. We should keep in mind that he wants to keep a properly referential account: so 'a' must refer determinately to something.

To avoid the objections, the strategy is to say that in a stipulation $Let(\delta, F)$, 'δ' refers to a *supposedly* F-thing. The idea is that every time we use such a stipulation, we are doing it under the supposition (or proof) that there are some F-things. The jump that Breckeridge requires from us is to accept that

(*) If we suppose that there are some x such that $F(x)$, then there are also some x that supposedly have F.

Let's consider the following argument (classically valid):

(1) $\exists x Fx$

(2) $\forall x(Fx \to Qx)$

(3) Fa

(4) Qa

(C) $\exists x Qx$

The problem that Breckeridge finds with the most natural and standard explanation, regards the step from (1) to (3). Breckeridge argues that we cannot maintain that (3) follows from (1), for in that case whatever 'a' refers to, it must be the case that necessarily, if there are Fs, then a has F. Now there are some properties F such that there is no thing which necessarily have F whenever there are some Fs. Therefore it seems like we cannot suppose that (3) simply follows from (1), if we want to maintain a referential account of 'a'.

However, we can reply, (3) follows from (1), in at least two senses. A proof-theoretic and a model-theoretic one: the former says that given a proof of (1) we have a proof of (3); the latter that for any model $\mathcal{M}$, if $\mathcal{M} \models (1)$, then $\mathcal{M} \models (3)$. In both cases it seems that nowhere it is implicit in the assumption of the validity of the inference from (1) to (3) that what 'a' denotes is something which necessarily has F whenever something has F. Now, if what Breckeridge is arguing for is that 'a' has a specific referent even abstracting away from any model or class of model, I would regard it as simply absurd and not supported by any mathematical practice. If instead we assume that 'a' has a referent which depends on the choice of a model or a class of models (even in the sense of Breckeridge & Magidor), then we can encompass the validity of $(1)\vdash(3)$ in both senses without having to assume any necessity[22]: in the proof theoretic case it will pick a specific object in a specific model between the ones under consideration in the proof[23]; in the semantical case, when we are working in a specific model, the referent will be picked in that model. Crucially, if 'a' is a new term, then we can also pick a specific referent to be given in the sense of (B), while keeping the inference valid: for by (2) that F will also have Q and so it will be enough to prove that $\exists x Qx$.[24]

Of course I am only considering the context of mathematics and it is possible that some concern may be more interesting in other applications of arbitrary reasoning. However this shows that to accept the validity of that inference in mathematical proofs, we do not need anything fancy. So let's say that for a theory of arbitrary reference as applied in mathematics, there does not seem to be a need in the first place for a supposedly F-thing: for the inference can be accounted for by accepting $(1)\vdash$ (3), which is not problematic as Breckeridge argues; not even for a referential account. Anyway, let's consider how his new account would work, and then other independent objections.

[22]Of course as we saw in the precedent sections, other problems would arise if we take this to mean that (B) is given.

[23]We could also talk about purely syntactical arguments; in those cases it seems however reasonable to assume some class of models as a semantic counterpart of the proof, just enough to give a meaning to the terms and predicates used.

[24]We thus notice an important difference with the dual rule (UG) considered before: for in that case, picking a specific referent in the sense of (B) seemed to be problematic in order to preserve validity. Here this is not the case.

Since he is not going to accept that (3) follows from (1), Breckeridge introduces other intermediate premises to the argument, so to make its validity clear under this construal. If I am understanding correctly, we obtain something similar to the following argument:

(1) $\exists x Fx$

(2) $\forall x(Fx \rightarrow Qx)$

(1*) $[\exists x Fx]$

(1**) $\exists x : [Fx]$ (by (*))

(3*) $[Fa]$

(4) Qa

(C) $\exists x Qx$

The idea is then that (3) (or (3*) here) is true not because it follows from (1), but because we fixed the referent of a to a supposedly F-thing. But for this step to be valid, we need to accept (*). The rest of the argument is justified in the standard way. Then, Breckeridge concludes:

> "...this is how lines 3 and 4 help us to see that the original argument is valid – they help us to see that C is true on the supposition that 1 and 2 are both true."

Unfortunately, this is not what is happening. Under this construal, to see the validity we need to also accept that once we suppose that there are things which are F, then there are things which supposedly have F: the validity of the original inference is not clarified, we are simply adding a (controversial) premise.

Moreover, one could reasonably ask why in this case the same type of argument proposed against (3) following from (1) does not apply in the case of (3*) following from (1**). For again whatever 'a' refers to, it must be something such that necessarily, if there is something which supposedly have F, then a supposedly has F. But there seem to be no such object: I can always suppose that a has $\neg F$, or at least it does not seem impossible to do so.

Let's consider the philosophical advantages that this position is supposed to have with respect to objections to $B\&M$. The core advantage is that now whatever property we consider, even if there are no things with that property, we can assume there are and thus we obtain things which supposedly have the property. The interesting case is what happens when a has $\neg P$, while we suppose that it has P: for it seems that the proof would become vacuous in a case of this sort, similarly to other cases considered in Martino's and Breckeridge & Magidor's sections. Breckeridge distinguishes between inside and outside of the supposition: so in the proof we only work in the scope of the supposition and

we do not consider that $\neg P(a)$ is true outside of it. I claim that this cannot work to save us from vacuousness.
Firstly, it seems that we fall in a similar problem as before: the referent of 'a' does not have a role in mathematical proofs. Secondly, symmetrically as it was for the criterion of unknowability, distinguishing between an outside and inside of the supposition does not help us in seeing why the not-supposed property of a does not play a role: usually, we can use theorems from outside the supposition to draw inferences in the supposition[25] and as we saw ignorance is not enough to avoid using pieces of information in a proof. Similar objections to the ones I already proposed still apply in a natural way, because we still have an object which is determinately the referent of the arbitrary term, but again we cannot know which one (this time, we cannot even know whether it actually has the property or not). An objection which does not apply straightforwardly anymore is the fourth one: if we have nested suppositions, it seems that this account gives a way to keep the referent stable, while also possibly stipulating descriptions which are not satisfied by it. But of course we doge an arrow to get a bullet: it is the first supposition (that is the stipulation $Let(\delta, F)$) which already risks to make the proof vacuous, as seen above.

If on the other hand we want to say that the thing which supposedly have F but does not have F outside of the supposition also does not satisfy $\neg F$ outside of the supposition, then we are committing to not fully-determined objects, and thus we may as well go into a position which treats the referent of 'a' as an arbitrary object. We shall turn to a consideration of such positions in the next chapter.

[25] This is particularly interesting with respect to the argument that B gives to counter Haze's argument. The answer relies on a being never-referred-to inside the supposition, but referred-to outside: this is how we avoid the absurdity of referring to a never-referred-to object. But then we why can't we use that a is referred to inside the supposition, thus concluding that no never-referred-to object can exist?

Chapter 2
Arbitrary terms as denoting Arbitrary Objects

In this chapter we shall consider three authors, Fine, Horsten and Santambrogio. What these authors have in common is that they treat arbitrary terms as referring to a well-determined object. They want to take the face-value of the term seriously. The authors call these objects with names like variable, arbitrary or generic object, but the core idea stays more or less the same: they are some sort of higher entity with a certain range of individuals (or more specific objects) associated. What the precise relationship between the arbitrary object and the range of individuals is changes between the authors and hopefully it will be clear by reading the relative sections. Particular interesting for these authors become stipulations such as: "let a, b be naturals" or "let a, b be naturals such that $a \neq b$". In the previous sections, the accounts we considered did not have any particular problem with stipulations of this sort: for example, in Breckeridge & Magidor's account, a and b refer to two specific natural numbers, but it is unknown which (hence the possibility for them to be equal is open); same happens in the second stipulation, only this time a and b will need to be distinct objects. On the other hand, we will see that as soon as one takes arbitrary terms to refer to some sort of higher-level object, accounting for such stipulations becomes incredibly difficult.

Fine

Fine proposed what became the mainstream model of arbitrary objects. This is because, despite the philosophical construal being – in my opinion – highly questionable, the theory captures what is generally thought to be the right theory of arbitrary objects. Fine makes precise the idea that an arbitrary object possesses all and exactly those properties which are common throughout all individuals that it stands for. And this is I believe also the intuitive understanding that mathematicians have: what can we say, in principle, about an arbitrary natural number n? well, if some number does not have a property, then we cannot say that n has that property either; if all numbers have a property, then n will have that property too.

But then, what about a property such as being a specific object? or being an arbitrary object? Such properties do not seem to follow the same principle: if n corresponds to an arbitrary object, then it should satisfy the formula saying that it is an arbitrary object... however everything that it ranges on (the natural numbers) are specific objects, hence it must be a specific object. To solve this impasse, Fine distinguishes between two types of properties, or two sides of properties. There are some properties that pertain to the arbitrary object as a representant of its range: this are called generic properties. On the other hand, there are properties that describe the arbitrary object as an object on its own: these are called classical properties. Notice that most properties that are true under the classical construal will be false under the generic one and viceversa. Many attack this distinction saying that it is not clear or imprecise: personally, I do not think so: in the end Fine can appeal to his own model-theoretic framework and assert that what is true in the generic sense is what is true in the models; on the other hand, Fine could argue, the classical construal will be the object of a metaphysical investigation.

Notice that Fine himself does not seem to believe in arbitrary objects in any strong sense, rather he considers them on par with other abstract objects, such as indeed numbers or concepts:

> "If now I am asked whether there are arbitrary objects, I will answer according to the intended sense of 'there are'. If it is the ontologically significant sense, then I am happy to agree with my opponent and say 'no'. I have a sufficiently robust sense of reality not to want to people my world with arbitrary numbers or arbitrary men. Indeed, I may be sufficiently robust not even to want individual numbers or individual men in my world. But if the intended sense is ontologically neutral, then my answer is a decided 'yes'. I have, it seems to me, as much reason to affirm that there are arbitrary objects in this sense as the nominalist has to affirm that there are numbers."[26]

In our philosophical investigation, we are allowed to an intermediate theorizing, where we take the statement under analysis (in this case, those involving arbitrary terms) at face-value: and this in Fine's view means treating them as denoting some sort of object, that on one hand we recognize as an object in its own right, while presenting a description (i.e. the theory of the proposed models) of it which must agree with what we would say in mathematical practice about the term. Now, this is surely an interesting piece of philosophical methodology, to which I would reply that the intermediate theorizing should not be regarded as always justified: if the problems that the "face-value" interpretation presents are too many, then we should maybe theorize at an other level. Moreover, notice that the comparison with nominalism is perhaps a provocation: for if we are working towards a characterization of what is denoted by arbitrary

[26] [7], p. 57

terms, and we claim that this is an object distinct from the set of individuals, perhaps that the correct way to reason about them is as further entities beyond the specific objects, then the burden seems to be slightly heavier than the burden that the nominalist has when saying that numbers can be said to exist within certain rules and conventions of mathematical practice. Sometimes metaphysicians think they are magicians, they throw the stone and then you cannot find the hand anymore. Fine is, unquestionably, making an ontological claim, perhaps only a conditional one: he does not expose himself on the ontological status of abstract entities, but whatever those entities are, beside specific natural numbers, we find arbitrary numbers as *further* abstract objects in their own right.

In what follows I will present the formal account, which is based on this intermediate theorizing idea, by treating arbitrary objects as new elements of the domain. Apart from this, the account is I believe correct (at least when it comes to classical contexts)... but there is a catch: by assuming this intermediate theorizing, by generating the classical aspect of arbitrary terms (i.e. treating them as objects in their own right) Fine also makes the philosophical construal of the generic aspect hardly tenable. Perhaps it is possible to provide a theory of reference which is still close to our intuitions, yields a correct theory, but does not consider arbitrary terms as objects on par with their range. Without further spoilers, I shall now present the formal account and then my critiques at the interface between generic aspects and the so called classical ones.

We are going first to consider the account in its most elementary formulation, and then we shall consider what happens by introducing the relation of dependence among A-objects. Given an L-model $\mathcal{M}$, we can extend it by adding a set A of new elements (namely the arbitrary objects) together with a collection of functions V taking elements of A as input and of M as output, yielding the generic construal of L-formulas: the idea is that an arbitrary object $a \in A$ will satisfy those properties which are satisfied by all objects $m \in M$ such that, for some $v \in V$, $v(a) = m$. Let's suppose, for example, that our underlying classical model is $\mathbb{N}$: the arbitrary even number will be any element e of our arbitrary domain such that: $\exists v \in V$ such that $v(e) = n$ if and only if n is even. We will denote the so extended model $\mathcal{M}^+$. More formally, we extend our language L with a collection of A-terms, which we shall denote AT: we thus obtain the language L^+.

Def. 1 *Let $\mathcal{M} = (M, \{R_i\}_{i \in I})$ be an L-model. Then we let:*

(i) A be a set of objects, disjoint from M

(ii) V be a non-empty set of partial functions from A onto M

(iii) d a denotation-function, from AT onto A

Then we define the L^+-model $\mathcal{M}^+$ as a tuple $(M \uplus A, \{R_i\}_{i \in I}, V, d)$.

As we introduce this class of models we will introduce certain axioms that they must satisfy in order to behave in the desired way, the first being that the set V must be closed under restrictions of its domain:

(A1) (Restriction) For any $v \in V$ and $B \subseteq A$, then $v \upharpoonright B \in V$ (where $v \upharpoonright B$ is the restriction of v to B as usual).

Formula satisfaction will then have the following definition, whose aim is to capture the generic construal we talked about in the introduction:

Def. 2 $\mathcal{M}^+ \models \varphi(\overline{a})$ *iff for all* $v \in V$*:* $\overline{a} \in Dm(v)$*:* $\mathcal{M} \models \varphi(v(\overline{a}))$

Given an arbitrary object a, the set of individuals to which it refers will be called its range, or $rg(a)$, which is defined as:

Def. 3 $rg(a) := \{m \in M | \exists v \in V : v(a) = m\}$.

Two words are also needed for the set V of valuations. What does it mean that a certain partial function $v : A \to M$ belongs to V? It means that all $a \in Dm(v)$ can receive their value *simultaneously.* Suppose, for example, that we are working in the standard $\mathbb{N}$ and we pick an arbitrary natural a and its square a^2. In the approach, an A-model suitably modeling this situation would not admit a valuation v such that $Dm(v) = \{a, a^2\}$ and $v(a) = 2$ while $v(a^2) = 5$.

However, there seem to be more to take into account. For suppose I wanted to consider an arbitrary k such that $k \leq n$, where n was already an arbitrary natural number: how are we going to evaluate k? and what would its range be? An instinctive answer to the second question could be to define $rg(k)$ as the set of all numbers x that are less or equal than some natural number. Therefore, the range of k simply seem to be all natural numbers. If we tried however to evaluate a statement about k naively using generic attribution as we did above, we may encounter some problem. For example, if we consider the statement $S =$ "$k \leq n$", it seems that whatever is the value assumed by n, there are values for k that are not less or equal to n: however being less or equal than n was the stipulation-property of k, thus it must be true! In general, we can say that there would be no problem in applying generic attribution to a formula where no other arbitrary term occur: but when other terms occur, especially terms used to define k, then things may go wrong.

The key aspect to this relationship between arbitrary terms seems to be what I shall call *value-dependence.* Considering again the example above, the value-dependence between k and n is such that the value k takes is based upon the value that n has took: e.g. if n has value 1, then k must have value 0. We saw how for standard arbitrary objects, as in the case of n, there is a collection of objects (which we called the range) and we can straightforwardly construe them as satisfying exactly those properties which are invariant over the collection. In the case of k, finding the range seems more problematic, or, if we take the most intuitive notion of range we cannot apply generic attribution to formulas where other terms occur. If evaluation of n can be compared to the quantification over a collection of individuals, evaluation of k seems to require something more: we need to check all possible paths that we can follow to satisfy S, and this means choosing a value for n, say $v(n)$ and then ranging on the collection of values that satisfy $x \leq v(n)$. It is as if every choice has its own range: the range of k is a collection of indexed ranges (that is indexed by valuations for n).

I believe this value-dependence to be the essential aspect for the relation of dependence in order to account for arbitrary reasoning. Now, there are different ways to construe it both philosophically and mathematically speaking, however, using the explanation in the last paragraph, we can give what I think to be an uncontroversial construal of it: a is value-dependent on b if choosing a definite value for a requires having a definite value for b. This is the aspect that I will be interested in retaining when proposing my mathematical account of arbitrary terms. Fine, on the other hand, has all the interest in proceeding by postulating a primitive dependence relation: we shall now see how the class of models that we started to describe above can be refined by introducing a dependence relation $\prec$ and certain axioms on its behavior and relationship with valuations.

We thus want to introduce an order on the collection of arbitrary objects: indeed a relation of dependence $\prec \subseteq A \times A$. We shall call A-models $\mathcal{M}^*$ any model $(\mathcal{M}^+, \prec)$. We will read $a \prec b$ as a is dependent on b. The relation must satisfy the conditions of being transitive and conversely well-founded:

(A2) (Transitivity) If $a \prec b$ and $b \prec c$, then $a \prec c$.

(A3) (Foundation) There is no infinite sequence $a_0, a_1, a_2, ...$ of A-objects such that $a_0 \prec a_1 \prec a_2 \prec ....$

Now, how can we connect the valuations and the relation of dependence? First let's define some intermediate terms that we shall need for the following discussion. Following Fine's notation, for $B \subseteq A$, V_B will denote the set of valuations v such that $Dm(v) = B$; moreover the closure of B under $\prec$ will be denoted as $[B]$, while $|B|$ will denote the set $[B] - B$. For $a \in A$ we define the value-dependence function $VD(a, B)$ to be a function from V_B into M (that is, the individuals), such that

$$VD(a, B)(v) = \{m \in M | v \cup (a, m) \in V\}$$

That is, it outputs the individuals that can be values of a, given a certain assignment of values to the A-objects in B. If $B = |a|$, then we shall denote $VD(a, |a|)$ simply as VD_a. I think it is plain how this value-dependence function captures the idea I expressed above about value-dependence: if a is dependent on other A-objects say $b_1 ... b_n$, then the range in this account becomes a collection of ranges, indexed by valuations for $b_1 ... b_n$. In a moment we will see how Fine finally connects this notion of value-dependence (and the relation $\prec$ on which it is based) to talk about formulas realizations and satisfaction. First we are going to introduce the last two axioms:

(A4) (Partial Extendibility) Let $v \in V$. Then $\exists v^+ \in V$ such that $v^+ \supseteq v$ and $Dm(v^+) = [Dm(v)]$.

(A5) (Piecing) Let $\{v_i | i \in I\}$ $(I \neq \emptyset)$ be an indexed subset of V such that: (a) $Dm(v_i)$ is closed under $\prec$, for any $i \in I$; (b) $v = \bigcup_{i \in I} v_i$ is a function. Then $v \in V$.

In what follows, we are going to refer to the class of models just defined as F-models.

We now come to the notion of realization of a definitional system. The main application that Fine has in mind for this account, at least under a formal point of view, is to the definitions of arbitrary terms. So we want to study the relation between a definition and models realizing it. A definition is simply a couple $(a, \Delta(x))$, where a is an A-term and $\Delta(x)$ (sometimes simply referred to as Δ) a set of formulas with only the free-variable x occurring. We shall denote, given a set A of A-objects, A_Δ as the set of given terms in Δ. Given a valuation v, $v(\Delta)$ will mean the simultaneous application of v to all terms occurring in Δ – and all formulas.

We now set the notion of realizability as follows:

Def. 4 *Let $\mathcal{M}^* = (\mathcal{M}, A, \prec, V, d)$ be an A-model. $\mathcal{M}^*$ realizes the definition (a, Δ) if:*

(a) For all $c \in A$, $a \prec c$ iff $c = b$ or $b \prec c$ for some b occurring in Δ;

(b) For any $u \in V_{|a|}$, $VD_a(u) = \{m \in M | \mathcal{M} \models u(\Delta(m))\}$

A system of definitions G will simply consist of a collection of definitions. Now, we are going to call G *unequivocal* if it does not contain two different definitions (a, Δ) and (a, Γ) of the same term a. It is *well-founded* if there is no infinite chain $a_0, a_1, a_2, ..$ of defined terms such each a_i is defined in terms of a_{i+1}. Finally it is *complete* if every given term of G is a defined term of G.

Last but not least, we wish to define a notion of equivalence or isomorphism between such models. We will call such equivalence with the name of *generic equivalence*, and we shall denote it as $\cong_g$ or $\cong$ if it does not raise confusion.

Def. 5 *Let $\mathcal{M}^1 = (\mathcal{M}, A_1, V_1, \prec_1)$ and $\mathcal{M}^2 = (\mathcal{M}, A_2, V_2, \prec_2)$ be two F-models with same underlying model $\mathcal{M}$ and same language. Then $\mathcal{M}^1 \cong_g \mathcal{M}^2$ if there is a one-one map $f : A_1 \to A_2$ such that:*

(i) $a \prec_1 b$ iff $f(a) \prec_2 f(b)$, for all $a, b \in A_1$

(ii) $v \in V_1$ iff $\{(f(a), m) | (a, m) \in v\} \in V_2$

Fine can now establishes this very interesting result, which connects the generic structures of arbitrary objects and their (possible) stipulations:

Thm. 1 *Let G be a definitional system which is unequivocal, well-founded and complete. Let $\mathcal{M}_1$ and $\mathcal{M}_2$ be two A-models with same underlying L-model, say $\mathcal{M}$. Then let $\mathcal{M}_1 \restriction A_G$ and $\mathcal{M}_2 \restriction A_G$ be the restrictions of the two models to the terms occurring in G. Then:*

$$\mathcal{M}_1 \restriction A_G \cong_g \mathcal{M}_2 \restriction A_G$$

We now gave a fairly comprehensive introduction to Fine's account, both philosophically and mathematically, and we shall pass to the critiques.

Critiques

The main critique I have against Fine regards the claim he makes of taking the mathematical statements at face-value, in particular, I shall show that as we use this referential account to construe mathematical statements as occurring in ordinary A-proofs, there seem to be no way to give justice to the natural interpretation of such statements. Suppose we start an A-proof with the stipulation $Let(\delta, F)$. What is the right construal of the predicate F? Is it the generic or the classical one? Remember we are dealing with a referential account, thus 'δ' refers to an arbitrary object and then δ does not possess the property F under the classical construal: therefore adopting this interpretation we would have to conclude that the statement mathematicians use in their practice is strictly speaking false. I do not see any plausibility in such a position and I do not think this is acceptable for Fine either. Henceforth, it seems that the right interpretation of the stipulation as occurring in a proof is the generic one. I claim that this option is not ideal, too. The first reason is that mathematicians are not considering the predicate F in its generic reading, but in its classical one: no one, I can assure, when saying "let n be an arbitrary natural number", is considering the set $\mathbb{N}$ augmented with other objects than the specific natural numbers. Therefore the claim Fine (and also Horsten) makes that his account can preserve the face-value meaning of arbitrary terms is misleading: while (supposedly) preserving the face-value meaning of the term δ we completely lose the face-value of the predication we use to introduce the term: which means we lose the face-value of the term too. Secondly, this approach seems to require, at least for some A-proofs, a switch from the generic to the classical interpretation. Suppose we say: let a, b, c be such that F:... and we prove that either $a = c$ or $b = c$, thus concluding (C) there are at most two elements which are such that F. If F is taken in its generic reading, (C) may well be false. Suppose F has exactly two elements in its classical reading: then a, b and c will cause F to have more than two elements in the generic reading, of course. Therefore, while the reading needs to be generic in the stipulation, it needs to be classical in the conclusion (C): but why should the interpretation change? and where is this interpretation switch required other than in this ad hoc theory? again, there seem to be nothing of this sort in the ordinary understanding by mathematicians.

A further layer is created, which is never assumed in mathematical practice and thus makes it impossible to properly account for the meaning of the mathematical statements involved in A-proofs. An other example of this is if when we explicitly refer to an arbitrary object. Even supposing that arbitrary objects are there, when I say "let a be an arbitrary object" (meaning that I am ranging on the arbitrary objects), under this account a is actually referring to an *arbitrary* arbitrary object, which augments the domain of arbitrary objects.

A second and less serious concern regards the notion of identity. The metaphysical criterion of identity that Fine proposes is (IC): for two arbitrary objects a, b, $a = b$ iff $rg(a) = rg(b)$. There are two kind of problems in the account with respect to this criterion. On one hand, we have cases in which two objects should be different under (IC) but are actually indistinguishable by the account.

On the other, (IC) makes equal objects which should not be so (and which the account actually distinguishes).

As an instance of the first problem, notice that if a range S has cardinality bigger than 1, then by picking a, b such that S, no model proposed by Fine will be able to satisfy $a = b$. Even if we consider a, b as an arbitrary pair, the pair will range on $S \times S$, thus containing cases where the values of (a, b) are different. As a further instance of this same problem, consider the case of higher-order arbitrary objects. We have one arbitrary object $\alpha \in M^{++}$ ranging on the arbitrary objects $a_1..a_n$, ranging respectively on $X_1..X_n$; and let $\beta \in M^+$ be the arbitrary object ranging on $\bigcup_i X_i$. The theory of $\mathcal{M}^{++}$ will satisfy the same formulas for α and β. However, since we are treating them as objects in their own right – and given the identity criterion –, it is not clear why this should be so and the fact that the theory cannot account for the difference should be regarded as a weakness.

As an instance of the second problem, consider a stipulation such as "let n_1, n_2 be F such that $n_1 \neq n_2$": they seem to have the same range but are assumed to be different. Either we say that we are making a vacuous assumption, or we need something else. But a solution is not so easy to find: one may try to consider them as an ordered pair, thus being an arbitrary pair of distinct elements, but this does not account for cases in which I first introduce n_1 and after a while introduce n_2: it seems strange to say that the real interpretation of n_1 is only given after we also introduce n_2, especially if we conceive them as objects.

We can posit similar arguments concerning the relation of identity and other properties. The point is always that while we posit a further layer of objects, the account considers them simply as things which range on their specific objects, that is only with respect to the generic properties. The classical properties are always ignored and not really expressible in the account and the distinction between the generic and the classical interpretation of properties arises as necessary only after we move into the *intermediate theorizing* that Fine proposes. As we treat arbitrary terms as denoting an object, we break the nature of the term in two: one that regards the "object" as a device to reason on the specific objects in the range; one that regards such object as an object in its own right. One must admit that classical properties are properties regarding the metaphysics of the entities we supposed for the intermediate theorizing, which have nothing to do with the range of such entities, thus with the use we make of the names for such entities outside of metaphysical discourse. The point I am making is that the classical side of arbitrary object seems useless to account for the use of arbitrary terms – even Fine's own theory cannot express such aspects of the arbitrary objects (not even in those cases where the construal of the matter seems to make it compelling, such as in the higher-order case) – and causes problems on its own as highlighted above: it strongly points towards a divorce from the natural understanding of predications on arbitrary terms; it creates new metaphysical problems, regarding in particular the notion of identity involved. So the natural question is: can we obtain a theory which agrees with Fine's object theory, without having to pay this price? The answer seems to be yes, and it will be investigated in the next section. Before that we shall consider one last concern.

As we saw, Fine proposes a very interesting way to deal with value-dependence. However one may regard it as perhaps too general. Effectively, $a \prec b$ means a certain connection between the respective valuations: the connection can be summarized in the following notion of *suitability*:

Def. 6 *Let $\mathcal{M}^F$ be an F-model. We are going to call $\mathcal{M}^{\mathcal{F}}$ a suitable F-model if, for all $a, b \in A$ such that $a \prec b$: $\forall v \in V$: if $a \in Dm(v)$, then either $[a] \subseteq Dm(v)$ or there is some $v^+ \in V$ and $B \subseteq A$, such that $v = v^+ \restriction B$ and $[a] \subseteq Dm(v^+)$.*

This is, as far as I understand, the effect that the primitive dependence relation has on the valuations of arbitrary objects, as it is given by the axioms. Indeed we can easily prove that all F-models are suitable. Suppose $a \prec b$, $a \in Dm(v)$ and that $\exists b \in [a] : b \notin Dm(v)$. By Partial Extension there is some $v^+ \in V$ such that $[a] \subseteq Dm(v^+)$ and $v \subseteq v^+$: thus we can look at v as a restriction on v^+. What this intuitively means is that each way in which a can be evaluated always agrees with the value given by some valuation also giving a value to the full closure of a.

The problem is that this misses out on cases where the values of the dependent term are actually not influenced by those of the dependees. For example, we could posit any two terms as dependent, with any two ranges, without the values being affected at all: that is, a could be dependent on b, in the sense that $(a, b) \in \prec$, but choosing a value for b does not actually restrict the scope of values for a. How can we represent this formally here? For $a \prec b$, we can say that the values of a are not affected by the values of b if

$$\forall v \in V : b \in Dm(v) => VD(a, b)(v) = rg(a)$$

This is how we can characterize it using Fine's resources. Then, if we wanted to rule out cases of "fake"-dependency, we have the possibility to posit the further axiom

(A6) If $a \prec b$, then for some $v \in V$ such that $b \in Dm(v)$: $VD(a, b)(v) \subset rg(a)$

What this axiom says is that whenever a depends on b then there must be some value of b, such that having b assume that value restricts the scope of total admissible assignments for a. I will not investigate further whether this move can make us obtain a perfect relation of dependence. But still it would make Fine's account respect more our intuitions on value-dependence, and I believe it to be a necessary refinement of the account.

Supervaluationist cure for Fine

In this section we provide a variation on Fine's approach. The goal is to keep such approach as close as possible to Fine's formulation, with the only difference that arbitrary objects are not postulated, but we will only work with arbitrary terms. The possibility of such a formulation undermines Fine's philosophical construal. We saw how Fine was in the middle of a tension between proposing an ontologically neutral intermediate theorizing, and on the other hand construing A-terms as objects in their own right (i.e. the classical side of them). As we already noticed, the account yields a very plausible object theory, but achieves so by paying a high price in terms of the intuitive and philosophical construal, all based in the end on reifying A-terms into objects. The most serious concerns that were raised in the precedent section indeed arose from the postulation of A-objects; and other than to account for the "face-value" of the term, such postulation is considered necessary mainly to account for the relation of value-dependence. Indeed, while Fine is discussing some possible applications of his theory to other fields and its affinity with other approaches, he considers supervaluationism and asserts that despite generic truth and supervaluational truth could be equated:

> "[...] there would appear to be no counterpart, within the supervaluational approach, to the relation of dependence."[27]

We shall show how, in a supervaluational spirit (thus defining valuations starting immediately from arbitrary terms rather than objects in the domain) a representation of the dependence relation is actually possible. Since no further layer of objects is postulated, there is no need anymore for a distinction between generic and classical construal of properties: one can simply argue that what is true about the arbitrary term is just the generic truth, there is no further ontological dimension to account for. However, this account is still general and not strictly related to syntactic stipulations. In the formal account that will be proposed at the end of Chapter 3, on the other hand, we will be mostly interested in A-terms which are definable through a stipulation. Fine's account, on the other hand, wants to abstract away from the syntactic counterpart, thus this present formalization will also try to achieve it.

We start with a classical L-model $\mathcal{M}$. We want to introduce a collection of new constants At and then a stipulation operator $|$: for $a \in At$ and $S \subseteq M$, $a|_S$ will represent an arbitrary term ranging on S. In this account, an arbitrary term will always occur with the stipulation operator and a certain subset range. We let $X \subseteq \mathcal{P}(M)$ and we pick

$$\mathcal{A} \subseteq \{a|_S \quad | \quad a \in At \wedge S \in X\}$$

satisfying that $a|_S \in \mathcal{A}$ implies $a|_T \notin \mathcal{A}$, for $S \neq T$. We let $\prec \subseteq \mathcal{A} \times \mathcal{A}$ satisfy well-foundness and transitivity. We now construct the associated valuation set V by using a construction which shall be useful again in Chapter 3 when I shall consider transformation from the class of models I will define and the class of F-models. The idea is to progressively extend the valuations, first setting the stage for all independent terms, and then by defining valuations for dependent terms only as extensions of valuations for their dependees. We will then close the result under (Restriction) and (Piecing) (and then (Restriction) again).

[27] [5], p. 45

(base) Let $\{c_i|_{S_i}\}_{i\in I}$ be all independent terms and we shall suppose they have been ordered in a linear and well-founded way, to simplify our construction. We will start with $c_0|_{S_0}$ and start defining a set of partial valuation from $\{c_0\}$ to S_0: for each $m \in S_0$, we construct a unique v such that $v(c_0) = m$. Now supposing we defined valuations for c_j for all $j \leq i$, we are going to extend each v defined so far so to also evaluate c_{i+1}: for each $m \in S_{i+1}$, we add the extension v^+ of v such that

(i) $Dm(v^+) = Dm(v) \cup \{c_{i+1}\}$

(ii) $(c_{i+1}, m) \in v^+$

The idea is that since these terms are all independent, we are going to interpret them as ranging freely on their range (as indicated by the stipulation operator) in such a way that any combination of their values is admissible.

(step) Now we come to the definition of dependent terms. Suppose we defined valuations for all $|a|$, where a is stipulated as $a|_S$. What we wish to do is allow only for extensions of the valuations for $|a|$ which send a in S. However, contrary to the case of independent terms, where we made sure to cover all of the stipulated range, here for each valuation v such that $|a| \subseteq Dm(v)$, we only require that

$$\{v^+(a)|v^+ \supseteq v\} \subseteq S$$

That is, we extend v in a variety of ways so to also evaluate a, but admissible valuations for a need to be directed towards S: possibly, for given values of the dependees, the admissible values are restricted. Of course, we could also want to satisfy (A6), thus avoiding cases of fake-dependency, and so require that for some valuation v interpreting $|a|$, values *must* be restricted, hence:

$$\{v^+(a)|v^+ \supseteq v\} \subset S$$

Once this whole process is finished and we defined valuations for all terms constructed, say obtaining the set V^*, then we close V^* under Restriction, then Piecing, and then Restriction again, thus obtaining our V.

We shall now prove that these models satisfy all axioms of F-models, indeed yielding an equivalent formulation. It is clear that (A1) holds since it is the last closure; (A2) and (A3) hold by definition of $\prec$; we only need to check whether (A4) and (A5) hold or not. As for (A6), we have seen how we can decide, as it was with Fine, whether to add it or not.

Given the set V^* obtained by respecting the construction above, we shall denote $R(V^*)$ its closure under restrictions, $P(R(V^*))$ the subsequent closure under piecing and V the final result, i.e. the second closure on restrictions.

(A4) Let $v \in V$. We want to show that there is some $v^+ \in V$ such that $v^+ \supseteq v$ and $Dm(v^+) = [Dm(v)]$.

(case 1) $v \in V^*$: then if $a \in Dm(v)$, $[a] \subseteq Dm(v)$, by construction: every A-term is added to the domain of a valuation only after that valuation already evaluates all of its dependees, i.e. its closure. Therefore $Dm(v) = [Dm(v)]$.

(case 2) If v was restricted from some valuation $v^+ \in V^*$ we would be already done; if v was obtained by restricting some valuation from $P(R(V^*))$ we would again be done, since pieced valuations need to be closed. If v was pieced from elements of $R(V^*)$, then again we would be done for the same reason.

(A5) Let $\{u_i | i \in I\}$ be all closed under $\prec$ and let

$$v = \bigcup_{i \in I} u_i$$

be a function. We want to show that $v \in V$. We may assume that for some $j \in I$, $u_j \notin P(R(V^*))$, otherwise we would be immediately done: then $u_j = w \upharpoonright B$, where w was obtained by piecing $\{w_k | k \in K\}$ and $B \subseteq \mathcal{A}$. By hypothesis, $w \upharpoonright B$ is closed on $\prec$, therefore, for any $k \in K$, so is $w_k \upharpoonright B$. Now let

$$\mu = \bigcup_{k \in K} w_k \upharpoonright B$$

If w is a function, so must be μ. Therefore, since for each k, $w_k \upharpoonright B \in P(R(V^*))$, we can conclude that $\mu \in P(R(V^*))$. But clearly, $\mu = w \upharpoonright B = u_j$.

Since j as arbitrary, $v \in P(R(V^*))$, hence $v \in V$.

This concludes our proof. It would be nice to define ways to transform F-models into such supervaluational models and viceversa, in a way which preserves all the relevant structure, but this is left to future work.

Horsten

When presenting Fine's account, I talked quite extensively about the intermediate theorizing and the distinction between generic and classical properties. I believe that it would be fair to characterize Horsten's account (as far as it is presented in *The Metaphysics and Mathematics of Arbitrary Objects*) as a theory of the classical aspect only. He is not primarily interested with the relation arbitrary objects have with their range, neither with the way in which arbitrary objects "receive" the properties from their range, but only in the metaphysics of arbitrary objects – as objects in their own right. A desideratum which is reasonable to require from such a metaphysical theory is that, even if one is not interested in the aspects of the arbitrary objects that mathematicians care about (i.e. those aspects that in mathematics we regard as being true about the arbitrary term, when using it in a proof), the metaphysical theory should be compatible with such aspects, it should not make them not plausible or even inconsistent. After having presented the account, I shall show that this is precisely where Horsten account is at fault.

The idea behind Horsten account is to represent arbitrary objects as entities that assume certain values depending on the state in which they are. Intuitively, *an arbitrary natural number will be anything which can be in the state of being a natural number but that cannot be in any other state.* Horsten models this by taking arbitrary objects to be functions from a set of states to a collection of objects. If we are considering an arbitrary natural number n, then we need for every natural number k a state s where $n(s) = k$; moreover, for no state s, $n(s) \notin \mathbb{N}$. Informally, this means that an arbitrary object must cover all and only the objects in its range. A good way to picture arbitrary objects in this account is by considering a matrix with entries of the form (s_i, m_j), where s_i is a state and m_j an individual, such as:

$$\begin{array}{cccc} (s_1, m_1) & (s_1, m_2) & (s_1, m_3) & \cdots \\ \\ (s_2, m_1) & (s_2, m_2) & (s_2, m_3) & \cdots \\ \\ (s_3, m_1) & (s_3, m_2) & (s_3, m_3) & \cdots \\ \\ \cdots & \cdots & \cdots & \cdots \end{array}$$

and then consider threads along these matrix: that is, any collection of entries such that for every state s, there is an entry (s, m), for some m. So for example, we may have the arbitrary object corresponding to the function: $s_i \mapsto m_i$, which would correspond to the thread:

$$\begin{array}{cccc} \boxed{(s_1, m_1)} & (s_1, m_2) & (s_1, m_3) & \cdots \\ (s_2, m_1) & \boxed{(s_2, m_2)} & (s_2, m_3) & \cdots \\ (s_3, m_1) & (s_3, m_2) & \boxed{(s_3, m_3)} & \cdots \\ \cdots & \cdots & \cdots & \cdots \end{array}$$

More formally, given a certain kind F of individuals, we can define the *complete arbitrary object space associated with* F as

Def. 7

$$A(F) = \{f | Dm(f) = F \wedge CoDm(f) \subseteq F\}$$

The domains of the functions play the role of the state-spaces: we can identify the state-space with the collection of individuals themselves. Indeed, this concept of "modality" arising by treating arbitrary objects as something that can be in this or that state, and which Horsten calls *afthairetic*, is different from the usual modalities, for an arbitrary object never actually takes a value: that is, there is no possible state where the arbitrary object is *this* value.

However, by appealing to states Horsten can propose interesting new definitions for the notion of identity and dependence between arbitrary objects. Indeed we can now distinguish between two arbitrary objects that have same range, as in a stipulation like "let $a, b \in \mathbb{N}$". Since here arbitrary objects are functions, two functions are not identical just because they have same codomain, but rather when they assign some outputs to the same inputs. We thus adopt the following identity criterion: for a, b arbitrary objects

$$a = b \text{ iff } rg(a) = rg(b) \text{ and for all states } s,\ a(s) = b(s)$$

The concept of dependence also has an original treatment, which can get rid of fake-dependencies. He takes as primitive a notion of *independence*, from which we can derive a notion of dependence. This is, a, b are independent if

> For all values l, k that a and b can respectively take, there is a state s such that $a(s) = l$ and $b(s) = k$.

Horsten then shows how to define an arbitrary object space and other interesting constructions from this notion; he even follows a lead from Kripke, and uses Carnapian first-order modal models to represent systems of arbitrary objects and then delve into structuralism...but we will not consider them here. I am sure that an analysis of these modelizations would be interesting and fruitful, but they're main focus – providing a framework where one can reason about structuralism by using arbitrary objects – falls out of the scope of this work.

A first remark we can make on the plausibility of this way of representing arbitrary objects, regards the appeal to states. I do not see why the value of an arbitrary object should depend on states or situations, if this states are not simply valuations. I share the concern Fine has:

> "In developing a theory of A-objects, we should dispense with this peculiar modality and talk explicitly about the values that x and y take in different possible situations. Thus in describing the behavior of x and y within such a theory, we should say, not that necessarily y takes twice the value of x, but that in any possible situation in which both are defined the value of y will be twice the value of x. Moreover, these possible situations should themselves simply be taken to be the 'admissible' assignments from A-objects to values. Thus what it all comes down to is that, in any admissible assignment a on which both x and y are defined, a(y) = 2.a(x)."[28]

[28] [6], pp. 604-5

Especially since Horsten himself, as we saw in the presentation of the account above, is ready to identify states with the individuals. It really seems like states are not playing a role in determining which value is taken by the arbitrary object; rather they are only used to create a layer of distinction between arbitrary objects with same range. It would be as if Fine introduced an ordering on the valuations and decided to distinguish between arbitrary objects that have same range but a different ordering of the relevant valuations. I will not discuss here whether this could be fruitful or not for Fine's account or else; I just wanted to point out how the appeal to states and to modality appears misplaced, and it is rather just the introduction of a further layer of distinctions (an ordering, or an indexing), that allows to create more identity-classes of arbitrary objects.

Secondly, if we consider the example of arbitrary numbers, there are 2^ω arbitrary numbers, and all of them are good candidates as referents in a stipulation "let $n \in \mathbb{N}$". Thus it seems that the arbitrarity of reference is not really solved or even tackled, rather the situation gets worse: at least before we only had ω candidates as referents of an arbitrary term for a natural number, now 2^ω. As we have seen, Fine's account does not encounter this problem, since for each range there is a unique arbitrary object, despite this solution being problematic for other reasons.

Horsten is aware of this and makes what I believe to be a very odd move. For the reason why one usually feels compelled to adopt arbitrary objects is to account for the arbitrarity of, indeed, A-terms; surprisingly, Horsten adopts the view famously espoused by Pettigrew in [20]:

> "So there is something deeply right about the theory described in Pettigrew (2008), and the way in which my position concerning the semantics of these expressions differs from Pettigrew's position is somewhat subtle. We agree that they function as dedicated variables, and disagree with Fine who takes them to function as names. But Pettigrew holds that these designated variables range over specific entities, whereas I hold that it is an equally good hypothesis that they range over arbitrary entities." [29]

All mathematical terms are dedicated variables we implicitly quantify over, only the standard view is that the quantifiers range on the specific objects, but Horsten wants to argue that they range on both specific and arbitrary objects, or at least that this is a way to construe the matter. So, on a more superficial construal, arbitrary terms are regarded as constants, only for higher-order entities; however, the deeper underlying picture is that we can never refer in a constant-like way to mathematical entities, and all constants (both specific and arbitrary) are actually dedicated variables we implicitly quantify over.

I really struggle to see how one could accept Pettigrew's view on mathematical terms, while also feeling compelled to introduce a further layer of arbitrary objects. Once we regard all constants as dedicated variables, there is no need anymore for arbitrary objects. I believe Horsten has some more compelling

[29][15], p. 161

argument for the introduction of arbitrary objects in the discussion around structuralism for example and other metaphysical considerations. However, if we want to account for the use we make of arbitrary terms, this combination of positions is odd to say the least. I shall therefore investigate (despite this maybe not being Horsten's intent) whether the arbitrary objects he proposes could be seen as plausible referents for arbitrary terms.

Now comes the most serious concern. We shall notice that what Horsten does is presenting a system of standard mathematical entities and using them to study metaphysically the notion of an arbitrary object. While in Fine case, we had a class of models which could provide a theory in accordance with what we can and should say about arbitrary terms in mathematical practice, which at the same time also treated arbitrary terms as denoting objects in their own right; in Horsten case, we only have the latter, but no connection with the use of arbitrary terms is presented. Not only so, I believe Horsten's account makes it impossible to account for reasoning with arbitrary terms: that is why, although I do not want to express any opinion on the metaphysical value of the theory (that is, its value with respect to tackling purely metaphysical problems on the nature of mathematical objects, or things such as modality or grounding), I shall argue that the entities presented by Horsten theory are not plausible candidate as referents of arbitrary terms, even assuming that arbitrary terms refer as proper names.

The reason why I say so is rather general and primitive for me. If we want to talk about what is denoted by terms used in mathematics, we must listen to mathematical practice. I find utterly absurd to assert that some statement, which mathematicians take to be true (whatever this means in mathematics), can be said to be false on a purely metaphysical ground. This is precisely what happens in Horsten theory. For consider the stipulation "let n be a natural number". Horsten says that the term 'n' refers to an arbitrary natural number; on the other hand, arbitrary objects are not among the natural numbers, they are distinct from them. Notice that this is precisely how Horsten solves Berkeley's paradox: there is no inconsistency simply beacuse $n \notin \mathbb{N}$. Moreover, while answering a Frege concern he says:

> "The prime numbers are not arbitrary numbers: they are specific natural numbers. It may be that a concept of prime number can be generalised to the space arbitrary natural numbers, just as it can be generalised to various algebraic structures, but that is another matter." [30]
>
> "Being rational or irrational is a concept governing the specific real numbers. It may be that it can be generalised to the space of the arbitrary real numbers, but that is another matter."[31]

So the situation does not seem to be as it was with Fine, where arbitrary objects presented two sides and so where there was a sense (maybe surriptious

[30] [15]p. 67
[31] [15]p. 67

or unclear) under which an arbitrary natural number was a natural number[32]. Here, an arbitrary object is simply not a specific object; it is just in the state of being a specific object, whatever this means. The properties which it has are the properties of indeed functions from states to the range: e.g. an arbitrary natural number will in general have the properties that a function from states to ω has. So it really seems that Horsten must say that the premises mathematicians use in A-proofs (that is a the stipulation with which we start, such as "let $n \in \mathbb{N}$"), are actually wrong. The unavoidable conclusion is then that A-proofs are vacuous.

Santambrogio

Santambrogio works in the same direction as Fine: he believes we should take our talk about arbitrary terms at face-value, which means construing them as referring to some sort of generic object; and then searches for a formal way to represent systems of such objects and their connection with the individuals they stand for. Despite having a similar philosophical spirit, the theory appeals to different aspects of generic objects and uses different mathematical resources, yielding a very different result.

Santambrogio's account represents systems of arbitrary objects using complete Heyting algebras. The partial order in the algebra corresponds to a relation of specification: so $a \leq b$ informally means that a is more specified than b (for example, a could correspond to the arbitrary mammal, while b to an arbitrary animal). The algebras under consideration are also assumed to have atoms (or point-like elements[33], which informally correspond to the traditional *specie infimae*, some sort of approximations of individuals: for the purpose of this work, we can either straightforwardly equate them with individuals, or with maximally consistent sets of conceptual determinations. So formally speaking, here we are dealing with structures $(A, \leq)$ satisfying the following four conditions:

(i) $\leq$ is a partial order on A

(ii) for all $X \subseteq A$, $\bigvee X \in A$

(iii) $\bigvee$ and $\bigwedge$ distribute

(iv) $\forall a \in A, a \neq \bot_A$: for every maximal downward chain starting from a, there is at least one point-like element belonging to the chain.

This defines what we may call a frame, the type of structure we may use to represent generic objects.. but this still says nothing about formula satisfaction. In order to define suitable semantical clauses – so to properly make such frames

[32] Indeed Fine solves Berkeley's paradox by saying that the arbitrary natural number n is such that Even(n)$\vee$Odd(n) holds, while neither Even(n) nor Odd(n) do: we avoid a contradiction because we reject some classical inference

[33] Given a partially ordered set A closed under joins and an element a, we say that a is point-like if (for $X \subseteq A$) whenever $a \leq \bigvee X$, there is some $b \in X$ such that $a \leq b$.

models – Santambrogio gets strongly inspired by Beth's semantics for intuitionistic logic, with the difference that evaluation is relative to a *shift* instead of a node.

Def. 8 *Given an algebra A, a shift S is function from A to A s.t.:*

(i) $S(a) \leq a$

(ii) $S(\bigvee X) = \bigvee_{x \in X} S(x)$

We can define an order $\leq^*$ on shifts:

$$S \leq^* T \text{ iff } \forall a : S(a) \leq T(a).$$

When no confusion shall arise, we will denote the order on shifts simply with $\leq$, as the order defined on the algebra. Notice then that this is well defined because there is a minimal shift M: that which sends everything to the bottom of the algebra.

Now, we let $\mathcal{S}$ be the set of all shifts. We need one last notion to be defined before being able to provide our semantical clusters, and that is the notion of *bar for a shift*:

Def. 9 *$\mathcal{T} \subseteq \mathcal{S}$ is a bar for the shift S if every maximal descending chain in $\mathcal{S}$ containing S also contains some shift T such that $T \leq S$, $T \in \mathcal{S}$ and $T \neq M$.*

Last but not least, the clauses for formulas satisfication:

$S \models P(a)$ iff $S(a) \in P^A$

$S \models \neg\varphi$ iff for all $T \leq S$, $T \not\models \varphi$

$S \models \varphi \vee \psi$ iff there exists a bar $\mathcal{T}$ such for every $T \in \mathcal{T}$: either $T \models \varphi$ or $T \models \psi$

$S \models \varphi \wedge \psi$ iff $S \models \varphi$ and $S \models \psi$

$S \models \varphi \rightarrow \psi$ iff for all $T \leq S$, $T \models \varphi$ implies $T \models \psi$

$S \models \exists x\psi$ iff there exists a bar $\mathcal{T}$ such for every $T \in \mathcal{T}$: $T \models \psi(x/a)$ for some $a \in A$

$S \models \forall x\psi$ iff $S \models \psi(x/a)$ for all $a \in A$.

But still we do not have everything to represent arbitrary objects. What the picture is lacking so far is the relationship with individual objects. We already saw how the point-like elements of the algebra could be approximated as individuals[34], however so far these are only elements of the algebra as much as the others. What we want is to establish an heredity condition, which allows us to pass the properties of more generic objects to the less generic ones, i.e. a variant of the principle of generic attribution.

Our goal is for a principle of this sort to hold, which we shall call (GP):

[34] Actually Santambrogio goes further and then identifies individuals with any maximal directed subset of $A - \{\bot_A\}$ containing at least (hence exactly) one specie infima.

> Let $\varphi(x)$ be a formula not containing terms for arbitrary objects. Than a satisfies $\varphi(x)$ if and only if for every shift $S \neq I$, $S(a)$ satisfies $\varphi(x)$.

To achieve it we need to assume the following as a base-case. Let A be an algebra of generic objects and let v be a valuation that for every n-ary predicate R of L, assigns a set $R^v \subseteq A^n$.

(b1) Let S be any shift and $a_1..a_n \in A$. If $(a_1..a_n) \in R^v$, then $(S(a_1)..S(a_n)) \in R^v$.

(b2) Let $\mathcal{T}$ be a bar of the shift S. If, for all $T \in \mathcal{T}$, $(T(a_1)..T(a_n) \in R^v$, then $(S(a_1)..S(a_n)) \in R^v$.

From these to base assumptions, we can prove (GP) quite easily. What we obtain is that an arbitrary object satisfies exactly those formulas which are satisfied by all specific objects it ranges on *and also all more specific arbitrary objects*. This implies a remarkable difference with Fine and Horsten approach, for it is not in general true that if all specific objects in the range satisfy a formula, then the arbitrary object satisfy it. This makes justice to the intuition that even if all the coins in my pocket are worth ten cents, an arbitrary coin in my pocket should not be worth ten cents. But when we go into examples from the concrete world, things get more complex: for example, one may argue that the specific coins to consider to talk about the arbitrary coin also include coins that were or will be in my pocket; coins that are possibly in my pocket and so on. In mathematics, it seems that, at least where the context is classical, there is no such ambiguity on the range of an arbitrary object; moreover it seems there are no properties, possessed by all specific objects but not by the arbitrary object. To conclude the presentation of the account, let's consider the syntactic operator $\delta x.-$. For any formula $\varphi(x)$, we can consider $\delta x.\varphi(x)$, which intuitively picks the arbitrary $\varphi(x)$. In this class of structures, its semantical interpretation is given by the following definition:

Def. 10

$$[\delta x.\varphi(x)]_A = \bigvee\{a \in A | A \models \varphi(a)\}$$

We now come to the critiques of the account. The account presents a very interesting way to deal with arbitrary objects. It has a little problem however...it is inconsistent! We will now look at the argument for its inconsistency. Then we will reformulate the account so to avoid the contradiction, while keeping its interesting and peculiar aspects, so that we will be able to make a meaningful discussion and comparison.

Suppose $a, b \in A$ and let v be a valuation on A such that $a \in P^v$, while for all $c \leq b$, $c \notin P^v$ (or equivalently, for all shifts S, $S(b) \notin P^v$). Let's consider the minimal shift M, which sends everything to the bottom $\perp$ of the algebra. By condition (b1), $M(a) = \perp \in P^v$; but clearly by assumption, $M(b) = \perp \notin P^v$.

This short argument shows that whenever we assume in those models that there are two objects a, b with a satisfying P and b satisfying $\neg P$, then, under the assumption of (b1), we obtain an inconsistency. Notice that this inconsistency does not simply regard which properties we predicate about $\bot$, but most importantly the extension of predicates: the issue is not that $\bot$ has both the property of being a P and of being a $\neg P$; rather, that it has the property of being a P, while not having the property of being a P. The contradiction is in the meta-theory of this class of models. Since it seems not possible to simply rule out the assumptions we made about a, b (why shouldn't there be arbitrary objects with different properties?), either we reject (or modify) (GP), or we modify the models. I would like to point out that the former way is clearly less promising: in any case we want some equivalent of (GP) if we are treating arbitrary objects. So one idea could be to exclude $\bot$ from the scope of application of the principle: so basically we say that properties are inherited by more specific elements, but that this does not hold for $\bot$. This seems to entail then that $\bot$ is an object with no property. I am not sure whether this would work: firstly, the idea of $\bot$ was that of an overly-specified object: thus it should at most satisfy every property, rather than none. But even if we accept this, and we say that for every valuation v and every predicate, $\bot \notin P^v$, than by the clause on negation we get that $\neg P(\bot)$ will be always true. Then to actually achieve $\bot$ to satisfy no formula at all we would have to posit it as a matter of definition and actually exclude it from the scope of application of the semantical clauses. I think it is plain that this path is not ideal.

I shall propose an other option, where we modify the models eliminating $\bot$ completely. Doing so we will obtain a model which really seems consistent and which we shall prove maintains the interesting aspects of Santambrogio's models. This will allow us to actually consider the weaknesses and strengths of the idea behind the account, which were previously obscured by the contradiction.

The idea behind this reformulation is to keep the same models while eliminating the bottom of the order. So, let $(A, \leq)$ be a partial order; we shall assume that the order is well-founded: every descending chain must reach a bottom in finitely many steps: the idea is to identify these last points as individuals. Moreover we want the following conditions to hold:

(i) there is a unique $x \in A$ such that, for all $a \in A$: $x \leq a$. Let it be denoted by $\top_A$.

(ii) for all $X \subseteq A$; $\bigvee X \in A$.

Since we do not have a bottom, we do not obtain closure under the mean $\bigwedge$ operation in general: before we could say that a, b were incompatible if $a \wedge b = \bot$; now we will say that a, b are incompatible if $a \wedge b$ does not exist.
Then, we define the notion of shift exactly as before; the same goes for the semantical clusters and the base cases for (GP), conditions (b1) and (b2). We are able to prove (GP) at this point, without incurring in the inconsistency I highlighted before.

Before moving into the discussion, I would like to make a digression. Samtambrogio points out the similarity between his models and intuitionistic ones. This reformulation makes the two even more similar I believe. The idea is that nodes in Beth's intuitionistic models and shifts in Santambrogio models have exactly the same role, and indeed the principle (GP) on the latter is an equivalent of the heredity condition on the formers. The old class of models (if we forget about it being inconsistent) was clearly a proper class of intuitionistic models, for the former always had a minimal shift, while the latter does not always have a maximal node: the reformulation does not have a minimal shift in general (but it may have, and it may have more than one, as it is for maximal nodes in intuitionistic models), since there is no minimal element. I will not pursue this connection farther, but it is surely an interesting topic.

Now, coming back to our discussion, the reformulation allows us to keep (GP), while preserving basically the same structure as before; in what follows, when talking about Santambrogio's models (MS-models), I will refer to the corrected version of them. I will consider various problems with the account, in particular comparing it with Fine's account. I shall argue that even assuming arbitrary objects, the type of structure Santambrogio proposes is not likely to be appropriate.

A first matter which is not completely clear to me is how to interpret n-ary predicates: what type of arbitrary object (or objects) does it correspond to? I shall consider three options.

Let's not take shifts into account yet and let's just work with predicates and valuations v. In this account, the denotation of the arbitrary term $\delta.P(x)$ under a valuation v is given by:

$$\bigvee\{a \in A | A \models_v P(a)\}$$

Now, how should we interpret an arbitrary term $\delta.R(x_1..x_n)$? A first idea could be:

$$\bigvee\{i_1..i_n \in A | A \models_v R(i_1..i_n)\}$$

The problem is that the arbitrary term then is a single object, which does not capture the fact that what satisfies R should be an n-tuple. The arbitrary R under this construal is equivalent to the arbitrary x such that for some $1 \leq j \leq n$, $x = i_j$ and $R(i_1..i_n)$, which seems quite far from our intuition on the matter. More informally, an arbitrary R so represented would simply be an object which can be plugged in some spot in the formula $R(x_1..x_n)$. For example, if we consider the property "x loves y", i.e. $L(x, y)$, the corresponding arbitrary object would be an arbitrary z which either loves someone or is loved by someone. Therefore it seems we need something else.

An other natural option is to construe it as:

$$\bigvee\{(i_1..i_n) \in A^n | A \models_v R(i_1..i_n)\}$$

Here we solve the problem we had before, since now it seems we do not have a single object, rather something like an arbitrary n-tuple. However, for $\bigvee$ to have sense, we need to consider it in the algebra A^n. I think it is maybe possible, but uselessly complex to adopt this as a construal of n-ary predicates: for each arity we have to consider the respective product and how to define an appropriate order in general is all but clear. It seems that the risk to lose harmony with the initial algebra and its order is high. I then propose the following solution, which can solve the issue of the first one and avoid the problems of the second: we will not treat the arbitrary term to denote an arbitrary n-tuple, but just an n-tuple of arbitrary terms. First, we need to define the following sets:

$$R_1 := \{i \in I | A \models_v R(i, x_2..x_n), \text{ for some } x_2..x_n \in I\}$$

.

.

$$R_n := \{i \in I | A \models_v R(x_1..x_{n-1}, i), \text{ for some } x_1..x_{n-1} \in I\}$$

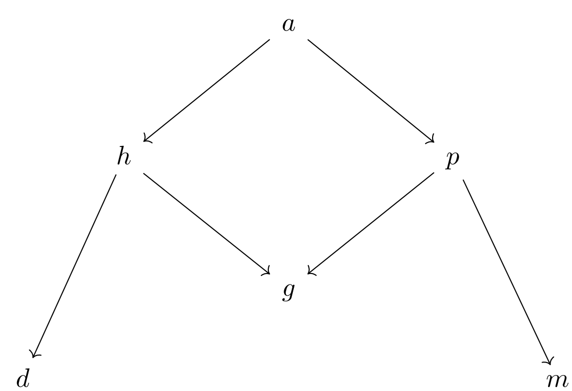
Informally, R_i will contain all and exactly those individual objects which can be in the $i-th$ position in $R(x_1..x_n)$ with the formula satisfied. Then, we interpret $\delta.R(x_1..x_n)$ as:

$$(\bigvee R_1, .., \bigvee R_n)$$

So for example, if we considered the predicate $L(x, y)$, an arbitrary representative of it will be a pair consisting of an arbitrary x such that x loves someone; and an arbitrary y such that y is loved by someone.

Doing this we can take care of n-ary predicates, and also n-ary formulas, at least where the variables are all independent. But the cases where the arbitrary terms depend on each other is more complex, and it is not clear how Santambrogio's account could express the desired value-dependency, neither he really faces the question. If for example we consider a stipulation like "let $n \in \mathbb{N}$ and $k \leq n$", k and n have exactly the same range and taking the join of such ranges will bring us to the same object both for n and for k. But there would be no value-dependence between the two. Similar problems arise when we consider a stipulation like "let $a, b \in \mathbb{N}$": in a case like this, we want a, b to be possibly the same object, but possibly different too. However, if we treat a statement involving them like $a = b$, by using the strategy used for 2-ary predicates, we would have that it is true: a and b are assigned the same object. In general I do not see any way to properly represent the matter of dependence in this account. Indeed, this suspect is made even stronger by the way in which generic attribution (GP) is formulated and Lesniewski's paradox is thus avoided: it was by forbidding the application of the principle to formulas which already contained arbitrary terms... but then how can one represent the dependency between two terms? and how can one connect this dependence to formula satisfaction?

And here comes the final problem. The structures that we considered are such that for each subset there is a least upper bound, also called a join. However, arbitrary objects may present genuine examples where there is a subset with no join, despite there being an upper bound. For example, let a be an arbitrary animal, h an arbitrary hairy animal, p an arbitrary primate, g an arbitrary gorilla, d an arbitrary dog and m an arbitrary man. Then their relations of specification should be as follows in the graph:



Then the set $\{d, g, m\}$ does not have a join. A case like this seems to suggest that using an algebraic structure whose order is the relation of specification, and closing it under the join operation, may not be the right path.

Chapter 3
A quasi-referential account

Desiderata for a theory of arbitrary terms

In the course of the discussion and the critiques to the various considered accounts, certain desiderata upon a theory of arbitrary terms arose as natural. As also stated in the introduction, my primary concern is to account for the use of arbitrary terms in mathematical practice. Since we are dealing with a semantical issue, staying as close as possible to practice translates as providing an account which construes the arbitrary term, at least in its most basic features, *simpliciter* as mathematicians do when working with them in A-proofs. More in particular, the uncontroversial and most basic feature for an arbitrary term seems to be its stipulation property. As we saw, Horsten lacked to meet this requirement, and indeed that was the main critique to his account. However, we have also seen that it is possible to give the impression of respecting the requirement, while actually surreptitiously betraying it: for we could posit a theory where the meaning of the predicates gets redefined and, under this redefinition, it seems that the arbitrary term satisfies its stipulation property, hence that we meet the requirement; but under the meaning *which is actually used* by mathematicians working with A-proofs, it does not. This is what happened in the case of Fine, with the generic/classical distinction. This gives rise to our first desiderata:

Desideratum 1 ***(Stipulation-Clarity)*** *Let δ be an arbitrary term stipulated as an F. Then, under our theory, δ must possess the property F, in the same sense as the property is standardly possessed by the relevant collection of objects.*

Of course, what possessing a property means may change from context to context. But supposing, as all the accounts we considered so far, a partially extensional approach, where thus properties are also characterized by their extension, and supposing we are working in a classical model $\mathcal{M}$, then our theory must assert that: $\delta \in F^{\mathcal{M}}$.

This links quite strongly with an other requirement which was often discussed, which is that of taking the meaning of the arbitrary term at *face-value*.

It is often not easy to understand what this face-value exactly is, and there is often disagreement between authors on what is genuine face-value or not. Even the first desiderata we just introduced could be seen as saying that the predication through which we stipulate or define the A-term should be taken at face-value. Now if we restrict our attention to the term, what pertains to its face-value meaning? Well, it seem uncontroversial that we get the feeling of working with an object and that for example, as discussed in the Introduction when arguing against schematism, we are not simply working with an empty-sign waiting to be quantified over. However, I believe authors often tend to overestimate the "amount of objectuality" which is suggested by practice. The least we want to require is that our theory of arbitrary terms should assert that the term refers to an object. However the further consequence that this object should be given (in the sense of an arbitrary/generic/variable object, as in Horsten, Santambrogio or Fine, or in the sense of one of the specific objects determinately being the referent, as in Martino and Breckeridge& Magidor) is extra information. Extra information that may be justified in principle, but I believe I proposed enough arguments against all these positions to make an inquiry into some other option at least desirable: there may be better ways, more economic in terms of assumed information, which can still be able to account for the minimal requirement:

Desideratum 2 ***(Term-Clarity)*** *Let δ be an arbitrary term. Our theory should assert that: δ refers to some object.*

A third aspect which was recurrent in our evaluation of positions was the measure in which it was possible to account for the principle of generic attribution and/or universal generalization: in general, does the theory of arbitrary terms give a plausible explanation as to why we can pass from a formula being true of the arbitrary term to that same formula being true of every object satisfying the stipulation property of the term in question? As was said in the Introduction, there is a minimal justification (MJ) for this generalization ... As we have seen this syntactical explanation stops being enough if we construe arbitrary terms as objects. If they are specific objects, and thus we take the epistemicist position, I argued that ignorance cannot be used as an appropriate base for it. If they are construed as arbitrary/generic objects, then it is not clear anymore why a proof made on them should be regarded as schematic. Of course in this latter case it is perfectly possible to provide further explanations as to why this is so: for example by expliciting a certain relationship regarding formula satisfaction between arbitrary objects and specific objects which entail the generic attribution, as it happens with Fine and Santambrogio. So it seems that on one hand the schematist has the easiest life accounting for the validity of the generalization step; however as argued we want to recognize some meaningfulness to the arbitrary term, precisely as much as we need to meet the first two desiderata; and at this point we want to justify the generalization step, but by staying as close as possible to the minimal justification:

Desideratum 3 ***(Gen Justification)*** *The theory should give a plausible explanation as to why generalization is allowed, while staying as close as possible* *to the purely syntactical explanation (MJ).*

We now come to the fourth desiderata for our theory. We saw how arbitrary terms do not simply involve cases like an arbitrary natural number n or an arbitrary even e, it is also possible to construct or stipulate terms by using other arbitrary terms in their stipulation. For example I may consider n^2 or "any b such that $b \leq e, n$". Understanding how these terms are used and reasoned with requires a different machinery than that that would be required to simply deal with the first type of terms. In the course of the discussion we referred to the relationship between, say n above and n^2 or b, as *value-dependency.* This formulation expresses the idea that, considering the example above, the set of admissible values for b is relative to the admissible values for n: for each value n can take, there are certain admissible values for b. As we have seen, dealing properly with such dependency is not trivial, and most accounts struggle in doing so. For example, under Breckeridge&Magidor account seems not only difficult, but impossible: consider the arbitrary natural number n and suppose that the true but unknown referent of n is in this case 0: now it becomes impossible to pick a dependent term k such that $k < n$. It is an argument with the same flavor as the fourth general objection I proposed in the first chapter: by having a well-determined referent among the specific numbers, what grants us that further assumptions or stipulations will not be inconsistent with that true value?
Moreover, if the theory was accompanied by a certain modelization, then this modelization should provide a way to formally represent the dependence in such a way that formulas where dependent terms occur are correctly evaluated. Fine, to my understanding, is the only author we analyzed that achieved this result; but even in his formulation, there was some problem, namely that of fake-deopendency: we would need to introduce a further axiom to sule out such cases, whereas Horsten, at least from a philosophical standpoint, cared to distinguish fake-dependence from genuine dependence. Santambrogio could not extend his semantical clusters to deal with dependent terms and in general stipulations $\delta x.\varphi(x)$, where $\varphi(x)$ already contained arbitrary terms.
Let's state our fourth desiderata then:

Desideratum 4 ***(Value-Dependency)*** *The theory should give a way, firstly, to intuitively understand the construction of dependent terms, without making it implausible; secondly, if some formal account is proposed, this should correctly capture the value-dependency, and distinguisg genuine dependence from fake dependence.*

An other issue is that of identity. When should we regard two terms equal, when distinct? The question is particularly pressing for the reificationist accounts; and as discussed in Chapter 2, none of those account can really provide a uniform and satisfactory identity criterion for arbitrary objects. The less ontology we assume, the easier it gets to deal with identity with arbitrary terms, but still it needs to be clear. In particular, we want to distinguish cases such as in "let a, b be naturals", from cases of the form "let a, b be naturals and such that $a \neq b$".

Notice that the less we assume, the more we can stick to the easy answer: they are equal when we can prove they are equal, distinct if we can prove they are distinct, neither otherwise.

Desideratum 5 ***(Identity)*** *If arbitrary objects are postulated, present identity conditions for them; if objects are not postulated, give a suitable notion to distinguish or identify terms.*

Finally, in the introduction we presented two paradoxes: Berkeley's and Lesniewski's. It is desirable that, whatever is the referential stance we want to take with respect to arbitrary terms, we have a way to construe them which allows us to avoid deriving the contradictions these two paradoxes make us infer. For example, with respect to Berkeley's paradox, Horsten asserts that the arbitrary n is not a number; Fine and Santambrogio adopt non-classical semantical clauses; Breckeridge & Magidor can say that n is (though unknowingly) either even or odd. Lesniewski's paradox on the other hand is more tricky and requires a formal background to be dealt with. We can surely say that Fine's account can encompass it with no problems, while Santambrogio must take a more awkward route, by forbidding generic attribution applied to terms containing other terms, as we saw in the discussion on dependence.

Desideratum 6 ***(Paradoxes of Arbitrary Reasoning)*** *The theory should solve the most common paradoxes, in a non ad hoc way, i.e. without randomly forbidding certain arbitrary terms or by simply declaring some premise of the paradoxical arguments false.*

With this we conclude our list of desiderata, that will now guide my argumentation in favor of my proposal. We will proceed as follows. First I will give a generic introduction to the position, while arguing how the first two desiderata are met. I will then argue for the other desiderata, where possible, informally. Then I will introduce my formal theory – actually, two slightly different options, and discuss the formal aspects of the listed requirements.

The proposal

The heuristics behind my proposal is to assume the least as possible about what an arbitrary term 'δ' refers to. To get a good first grasp I think it is useful to consider the group of positions from the first chapter. Such positions want to assert that (A) what 'δ' refers to is one of the specific objects in the range and that, farther, (B) the identity of this specific object is also determined. Indeed, if we accept that 'δ' refers to some object in the range, how is it possible not to accept the further fact that indeed one of such objects is the referent? We are used to reason this way. This is the same difficulty that Fine encountered when having to defend arbitrary objects against the Berkeley paradox: the common and shared opinion was that if we assert that n is either even or odd, then either we can assert that n is even or we can assert that n is odd. As we have seen, Fine on the other hand wanted to argue that, if n is the arbitrary natural number, then it is true that "$Odd(n) \vee Even(n)$", while both "$Odd(n)$" and "$Even(n)$" are neither true nor false. What happens if we drop this type of closure for our theory of reference? This is, in my opinion, what *properly* regarding arbitrarity as a characteristic of the way we refer (rather than what we are referring to) is all about. The way in which we will achieve this is basically by keeping the statement (A), but refraining from any statement that would yield a determination of (B).

Let's consider then an arbitrary term 'δ', whose stipulation property is F. What we shall assume regarding the referent of 'δ' is *only* that it is an F. What being an F precisely means is given by the mathematical context and in this context "being an F" will be connected with other properties and facts, e.g. saying that all Fs are Gs. In this case, of course, we could claim that the referent of 'δ' is a G. But if we want to keep the treatment as general as possible, I shall consider the least set of assumptions regarding the reference of 'δ' that we shall need to work in a proof with the term and obtain the desired results: this least set is simply that what 'δ' denotes satisfies the stipulation-property. In general, if the extension of F is larger than a singleton, it will be neither true nor false that 'δ' refers to k, for any of the specific k bearing F. Let's consider an arbitrary natural n as an example: 'n' refers either to 0, or to 1,... or to 1756 and so on. However, it is not the case that it refers to 0, as it is not the case that it refers to 1, ... and so on.

This may sound odd to some. We are used to have vagueness and indeterminacy contained at the object-level of our theories. We look suspiciously to a position which regards reference itself as a phenomenon which requires a description inherently involving vagueness. The concern goes: if 'δ' refers to either a, b or c, then either of the following statements must be true

'δ' refers to a – 'δ' refers to b – 'δ' refers to c.

However, I want to maintain that neither of the statements above is true (neither it is false) – while the statement 'δ refers to either a, b or c' is true. How is it possible to make sense of it? The key, as briefly said above, is that arbitrary reasoning itself provides us the way. As we say that the arbitrary natural number is either even or odd, but neither it is even nor it is odd, we can apply the same principle to our meta-theory on reference.

The consequence of this view is that while, as a matter of fact, 'δ' refers to some F; there is no matter of fact as to which F. A similar view can be found in the recent literature: in Assadian & Sbardolini we can find the following passage:

> "We have said that Tom refers to a British soldier, and we agree with Breckenridge and Magidor (2012, p. 377) that "we do not and cannot know" who it is. If one asks Which British soldier is Tom?, cases in which Tom is used to refer arbitrarily are cases in which there is no answer However, we disagree with Breckenridge and Magidor's claim that there is a (brute) fact of the matter about

> who Tom is, which is unknowable to us. Rather, we think there is no fact of the matter."[35]

The difference between this quasi-referential account and Assadian & Sbardolini's position is that in the latter the indeterminacy of the reference of Tom is only intersubjective, while subjectively there is no vagueness, the choice of referent is well-determined. That is, there is no fact which determines the referent of 'Tom' in general, but each speaker will set their choice to some specific individual. Such a claim, despite probably tenable for ordinary discourse, is not plausible in mathematics. For as we saw the lack of information about the specific referent is what grants generality to the proof, hence if the single mathematicians had to choose one number or the other, the proof would be impaired: mathematicians cannot use such a choice in the proof.

There is an other aspect from Assadian & Sbardolini which inspired the quasi-referential account, which is the will to escape from the constant-variable dichotomy:

> "On the one hand, Pettigrew (2008) defends the "Skolemite" view that parameters are variables. On the other hand, Breckenridge and Magidor (2012) defend the view that parameters are individual constants, except that we can't know what they refer to. We think that the constants-or-variables question is a false dilemma, and that we don't need to fall hard on one or the other side of it."[36]

Arbitrary terms, indeed, really seem to lie at the interface between a constant and a variable (or placeholder), and the quasi-referential account can be seen as a tentative to keep together these two aspects. As we saw in the critique to schematism in the introduction, there were two corns in the dilemma: either going for a purely syntactic view on A-proofs, which was ruled out as too extreme; or regarding the A-term as a mere placeholder, but the stipulation property as meaningful. In this latter case I argued that one must recognize that the predication and the term are not isolated and that the former has some influence on the construal of the term, too, in such a way that, despite not becoming fully referential as a standard constant would, it has a certain direction we may say: it directs indifferently towards a certain range. In this sense, the quasi-referential account shares the feeling that the constant/variable one is a false dilemma, for it looks at arbitrary terms, some of the most frequent and used terms in all of mathematics, as sharing features both from full constants and full variables. It would not surprise me if it was reveled that all constants and variables are actually just different occurrences of arbitrary terms, just lying on a spectrum with more or less accent on their referentiality or their variability.

This is, I believe, the best way to account for the arbitrarity of reference: that is, attributing the generic feature of arbitrary terms to the way we refer, rather than the object which is referred to: this, I claim is what it means to refer arbitrarily. Or at least, this is a way in which we can describe it: if it

[35] [1], p. 57
[36] [1], p. 56

is the act of reference which is arbitrary, a (meta-)theory which describes the act of reference should not leave us surprised or suspicious when it does not respect the semantical closure of disjunction. At least, it should not leave us more surprised or suspicious than the case of the arbitrary natural number n. To make the point clearer let's consider an analogy: I am typing the following sentence on my computer: "De_r Olivia, h_w are y_u?" (perhaps interpret the underscores as blank spaces): we have two levels at which we can explain what's happening: one says that the sentence is not completely determined for there are blank spaces; the other says that there are blank spaces because I skipped certain letters while typing. The latter is the level of explanation I am trying to elaborate. Adopting an option à la Fine, we do not have to talk about an indeterminate act of reference, but we still have to assume a domain of discourse where arbitrarity is "at loose", which implies assuming in some measure an extravagant ontology. My option would on the other hand be able to account in principle for the arbitrarity also in the object-level without having to assume any fancy object. I shall now show how the quasi-referential account avoids those objections and meets the desiderata we outlined in the course of the discussion and listed at the start of this chapter.

The first and the second desiderata are easily satisfied. The quasi-referential account is sewed on the face-value of the stipulation which introduces the arbitrary term. The primitives we take are precisely that 'δ' refers to an F, where the meaning of "being an F" is that given by mathematicians in the context of the proof in which 'δ' occurs: we can therefore say that δ has the property that it should have, without any stretch of the construal of what bearing that property is, and we can say that δ is an object too, i.e. that it refers to an object. This last point is subtle and it is the same point I made above with the example of a, b, c. The arbitrary term is referring to some object: this aspect is completely determined; however we must refrain from further specifications of the act of reference, hence of the object denoted. In this way, by applying arbitrary reasoning to the study of arbitrary reference, we can get that an arbitrary term refers to an object, in a way which, within certain limits, can be equated with the semantical role of a constant; without having to change the construal of the properties we predicate of the arbitrary term (as it was in Fine's and also Santambrogio's approach), and without having to go down the path of B&M or Martino, having to postulate one of the objects as the unknown referent.

How are we meeting the (Gen Justification)? How is the rule of universal generalization philosophically justified, explained, made more natural? This account fundamentally adopts the minimal justification (MJ) we saw in the introduction. However, the quasi-referential account gives us further reasons to believe in it. How is it that we do not make further assumptions on δ? As we saw in the first chapter, the fact that we simply do not know which the referent is, does not give us any certainty that we are not implicitly using specific information about the referent in the course of the proof. At least if there is a referent in the sense of Martino and Breckeridge & Magidor. One way to dodge the issue is the schematists' justification: 'δ' is a mere placeholder, therefore by refraining from further assumptions we are sure we will be able to apply the proof-scheme so obtained to any object: there is no implicit information we may be using, there are just syntactic parts being manipulated. Now, if, like me, you are not happy with the schematic picture, there is also Fine's answer[37], under which we can refrain from further information because the object which is denoted satisfies exactly those properties which are relevant and which are not too specific (i.e. the properties which are invariant over the range). Also this view seems to have its own problems and contradicts other desiderata (the second in particular) as we already saw. In the quasi-referential account we take the easiest way possible to account for the reason why we can refrain from the further assumptions (that is, the easiest way possible there is if we regard the term as meaningful): it is because the semantical information is not given, not determined, beyond the piece of information saying that δ refers to an object which is an F. Then this meta-theoretical fact is *projected* into the object-level, which depends on the mathematical context and which will yield what "being an F" means: from that we will be able to obtain our conclusions, following the logical consequence which is accepted in the mathematical context. This is the picture that the quasi-referential account offers: we start from a meta-theoretical fact, regarding what the arbitrary term refers to; then this fact is projected in the object level, following the conventions and construal of terms, predicates and logical consequence that the relevant mathematical community has. One may ask: how do you refrain from the further meta-referential information? You questioned how can one be sure to refrain from object-level information, but why should the situation be different for the meta-level? Of course, we still have to take something as primitive: however I believe the primitive facts which we are taking are much less demanding. We stipulate[38] a new term by means of a certain description: the meta-theoretical claim that 'δ' refers to an F is the least thing we have to assume: we are even adding a reference to the context, just to make sure we are making the least assumptions on what we should say in the proof about "what is denoted". Moreover, it is easier to see how we can refrain from further information in such a simple context, where we only care what the term refers too, and do not have to delve into the complexity of the object-level. It is arguable that this move from the object-level to a meta-level is just an idealization: in practice mathematicians never consider such aspect, but work directly within the object-level. Of course, I agree with this, but idealizations are not always bad: I believe that the advantages that this position has in accounting for the use we make of arbitrary terms makes this idealization similar in role to that of a physicist which considers the movement of a body without considering the friction of the air: despite every body moving on earth will be subject to some friction by the air, we can still learn a lot about its movement by making the idealization, and it is rather easy to plug in again the ignored variables – in our case, by considering what "being an F" means in the particular context.

[37] Better: an answer available by accepting Fine's account, I do not know if Fine would agree with it – but I think so

[38] There is a discussion regarding whether it is a stipulation or a definition or a description

With respect to this third desiderata there is also a formal aspect: in the class of structures to be proposed, we need some version of the principle of generic attribution to be satisfied: properties satisfied by the arbitrary term need to be satisfied by the objects in the range too. We shall see this later, as we present the formal account, but it will be fairly obvious, actually by a matter of definition, that properties predicated of an arbitrary term can be generalized to all objects in its range.

How would we represent dependence between arbitrary terms in this account? Here we will not discuss in detail about how to achieve the desired value-dependence in a formal account, this will be done later in the dedicated section. We shall show that it is possible, even without A-objects to either extract a dependence relation which "does the job" similarly enough to Fine's dependence relation; and, if one wishes to, we also have a way to copy the relation completely. Here however we shall discuss how to philosophically construe the phenomenon of dependence given the quasi-referential account. I believe that the first and most straightforward way to look at dependence is first of all to look at it as a syntactic relation regarding the stipulation properties of the terms in question. Saying that a depends on b in the end means that to stipulate a we needed to make reference to b, as when we say "let b be a natural and let $a \geq b$". As was discussed in the second chapter when we dealt with the issue of dependence in the case of Fine, one way in which we can construe the dependence between two terms is by looking, instead then simply at their range, at the paths of value-choices that we have to make: in our example, choosing a value for a means choosing a value for b, say $v(b)$, and then a value $v(a) \geq v(b)$: hence we have all the possible choices for b, and for each of this choices a collection of choices for a. So the way we construe this in the quasi-referential account is straightforward: a refers to something such that $b \leq a$; of course what this condition means also includes that b refers to a natural, and that whatever a refers to, this must be less than whatever a refers to. We will see later the formal counterpart.

We now come at the fifth desiderata to be considered. Now, since the quasi-referential account does not introduce any generic object, there is not an identity criterion to propose and we can stick to the most natural construal of identity, which, in Fine's terminology, is the generic one: a, b will be considered identical if and only if $\forall m, n \in rg(a) \times rg(b) : m = n$. Or put differently, if in the A-proof we can prove that the two arbitrary terms are equal. And, accordingly, they will be distinct simply when for all choices of values, the two values are distinct. Of course, there will be many cases where the two A-terms will be neither equal nor distinct.

Let's consider a couple example of particularly problematic cases. As we have seen, by adopting Fine's criterion the first of the cases below becomes difficult to account for

> "Let a, b be in $\mathbb{N}$". In this case, we can as couples of values we have both $(1, 1)$ and $(2, 1)$ so they are neither equal nor distinct.

> "Let a, b be in $\mathbb{N}$ and not: $a = b$". In this case, we use the apparatus from dependence. We are going to regard b (but we can also switch a and b here, it is indifferent) as dependent upon a, dependent in the sense that for every value that a takes, the corresponding value of b will be different. Thus we obtain that a and b will be distinct.

For what regard the paradoxes, once the formal account is presented, it will be clear that they are dealt with in a desirable way, namely as Fine does. We can however briefly present the idea behind the solutions. In both cases we follow the same lead as Fine does, indeed our formal machinery will define a set of valuations yielding the construal of A-terms and a notion of super-truth or generic truth, under which certain closures holding for classical valuations will not hold. In particular the disjunctive closure will not, and thus we take care of Berkeley's paradox. For what regards Lesniewski, when considering whether an arbitrary term a is equal to itself, we are going to pick a valuation for it: clearly, any such valuation will result in $v(a) = v(a)$; but no weird conclusion will be obtained: it will simply be indeterminate whether a specific x in the range of a is equal to a or not.

We now come to the exposition of the formal account. There are some remarks to make before starting. The quasi-referential account involves interpreting reference as a non-classical phenomenon. Now, if we wanted to be completely loyal to this approach, we would need to present a formal account with a non-classical meta-theory, which allows the models to assign the interpretation of a term to some element, but at the same time where no element can be picked out as the referent. At the present moment, I would not know how to develop such an account in a precise way – it will be maybe the goal of future research. I believe however that it is possible to get good approximations of the idea even from within a classical meta-context. The way which I decided to adopt is to represent the indeterminacy of the act of reference by considering many well-determined references, i.e. a collection of valuations, say C_V. Then we could regard the reference relation of the arbitrary term, that is the reference described under the quasi-referential account, as represented by an arbitrary valuation in C_V. In the formal account we are not going to consider arbitrary valuations as a further layer of valuations, of course, or we would end up in a *regressus ad infinitum*; however this is the intuitive reading I intend, where we consider the plurality of valuations as a way to achieve the arbitrarity of the reference relation, analogously to what we do when considering an arbitrary number and the range of natural numbers.

Defining the reference of an arbitrary term using a collection of valuations going into its intended range can be done in two ways: either like Fine, where we have a collection of partial valuations defined on the collection of terms (objects in the case of Fine, but here we can equate them), and then to evaluate a certain term we look at all and only the valuations that have that term in their domain; or, for each term, we define its set of valuations. In what follows, I shall often call these two approaches *global* and *local* respectively. As you may have guessed, I am going to follow the local path in what follows. However, notice that one may also consider as a formal counterpart of the quasi-referential account the approach we defined as a "supervaluationist cure" for Fine's, where the reference relation for an arbitrary term a is given by the collection of valuations $\{v \in V | a \in Dm(v)\}$.

A formal account

We are now going to introduce an alternative account of reasoning with A-terms, which is supervaluationally inspired, but still allows us to talk about a dependence relation among the A-terms. As briefly said before, in Fine's approach valuations can be said to act *globally* upon the formula to evaluate: if we want to evaluate $\varphi(a_1...a_n)$, we are going to consider any valuation that has all of $a_1, .., a_n$ in its domain. Suppose these valuations are indexed by $\{v_i\}_{i \in I}$: then for each $i \in I$, we are going to check whether $\varphi(v_i(a_1)...v_i(a_n))$ holds or not. On the other hand, the SV^+-approach wants to evaluate each arbitrary term *locally*, and then consider all global combinations of such local valuations. So, for a case like the one above, we will define suitable notion of combination or simultaneization of the local valuations for each a_j, $1 \leq j \leq n$. I will now give an informal introduction to it, then present the formal machinery.

The idea is to introduce a collection of arbitrary terms At, as we did for F-models. For each $a \in At$ and formula $\varphi(x) \in L \cup At$, we introduce a binary operator $|$, which we shall call the stipulation-operator or $|$-operator, which yields the construction

$$a|_{\varphi(x)}$$

and whose aim is to mimic the construction of an arbitrary φ, as when we say "let a be anything such that $\varphi(a)$". In this account an arbitrary term a never occurs without an associated description or stipulation: the idea is that this description is what tells us which class of local valuations is to consider to evaluate sentences where a occurs. Of course, we are going to assume some reasonable constraint, for example we do not want to allow the same arbitrary term to receive different stipulations.

If we now consider the example above, the formula $\varphi(a_1...a_n)$ in this account is rendered as $\varphi(a_1|_{\psi_1}...a_n|_{\psi_n})$. For each $1 \leq j \leq n$, a class of valuations is defined and denoted as $V_{\psi_j}(a_j)$ – we will refer to such sets as val-sets and we will see shortly how they are constructed. Then the global evaluation for φ will be given by all sets $\{v_1, ..v_n\}$, where $v_j \in V_{\psi_j}(a_j)$ and on which, depending on the syntactical relations between those terms (e.g. if one depends on an other), we shall put certain constraints.

Of course, doing so we only consider arbitrary terms stipulated by a finite set of formulas (or, which is equivalent, by a single formula). We could generalize the idea of having local valuations and their connection with the stipulation-operator for arbitrary sets of formulas Δ. We will show how to do this in the last part of this section. Doing so has some advantages and one disadvantage. On one hand, it yields a more powerful account, where we can represent more of mathematical practice, and it also provides us with a framework where it is easier to establish a connection with Fine's account. On the other hand however, now our val-sets could contain infinite sets, making working with such terms surely more difficult.

We shall now spend some words about the general justification of this account. Each arbitrary term will be considered always relatively to a certain stipulation; semantically this translates in each term having an associated and unique valuation space, which we shall call val-set: roughly with valuations making it range on what satisfies the stipulation in the underlying classical model. Treating each arbitrary term with its own val-set has three main advantages if considered in connection with the philosophical remarks made since the beginning of this chapter.

The first one is that it provides a more direct formal counterpart to the quasi-referential account, with respect to Fine's global valuations. Also Fine's approach (in its super-valuationist version of course) can provide a formal counterpart, but it is inevitably more distant from the syntactic aspect of the arbitrary term.

Secondly it allows us to formulate the nicer notion of dependence we discussed in Chapter 2. As already pointed out there, Fine's relation of dependence allows for the representation of cases where two A-objects a, b are such that a depends on b, but choices of values for b does not actually restrict the scope of values for a. In our context, cases like this are analogous to cases where b occurs in the stipulation of a, but it does not bring any information to the table. An example can be a a stipulation like "let a be such that $P(a) \wedge b = b$", where b say was stipulated as a number: stipulating a like this is completely equivalent as stipulating it simply as a P. We have seen how Fine could rule cases like this out by adding a new axiom; but still the approach is blind with respect to the particular relation of dependence holding between the two terms. The formulation I shall propose can introduce this distinction by also expressing the syntactical construction that connects the two dependent terms.

Thirdly, suppose you were presented with a case where we consider two arbitrary terms $a|_\varphi$ and $b|_\psi$. We do not know it, but they actually have same range. Fine's account cannot distinguish between the different syntactic constructions that they allow, given the knoweledge of the mathematician. It is true that once we discover that their range is the same, we will feel legitimated in co-extending all syntactic constructions, however this is not the case if this fact is unknown. Fine would present the two corresponding objects as indistinguishable but domain-wise distinct entities, without no way to consider the specific poset of arbitrary terms which we can construct for each term, given the epistemic position of the mathematician. Our account on the other hand can encompass this differences, while also providing a semantical base, where these terms indeed satisfy the same formulas.

SV^+-models

Let L be a first-order language and $\mathcal{M}$ an L-model. Let At be a countable collection of new constants. We let

$$Stip(L) = \{a|_{\varphi(x)} | a \in At \wedge \varphi(x) \in L\}$$

We can now define the language L^* recursively:

Def. 11 *Let L be a first-order language*

$L_0 = L$

$L_{n+1} = L_n \cup Stip(L_n)$

Let $L^* = \bigcup_{n\in\omega} L_n$.

We will use italic letters such as $v, u, w, ...$ as variables for functions from A_t to M, while greek letters like $\gamma, \mu, \nu, \vartheta, ..$ to denote either functions from At to M or sets of such functions, or sets of sets, and so on... We are now going to define the val-sets recursively. The idea is that valuations for a term will assign a value to the term and to all the terms occurring in its stipulation. We will then define a way to combine the val-sets together. Let $a \in At$.

(base) Let $\varphi(x) \in L$:

$$V_{\varphi(x)}(a) := \{v \subseteq \{a\} \times [|\varphi(x)|]^{\mathcal{M}} |\ v \text{ is a function}\}$$

(step) Let $\varphi(x) \in L_{n+1}$ and let $b_1|_{\psi_1}, .., b_n|_{\psi_n}$ be all the terms in $Stip(L_n)$ occurring in $\varphi(x)$ (that is we can rewrite $\varphi(x)$ as $\varphi(b_1|_{\psi_1}, .., b_n|_{\psi_n}, x)$). By recursive hypothesis, we may suppose $V_{\psi_1}(b_1), .., V_{\psi_n}(b_n)$ are already constructed (by a matter of simplicity, let's refer to them simply as $V(b_1), .., V(b_n)$). Let's define:

$$V_{\varphi(x)}(a) = \{u : \{b_1, .., b_n, a\} \to M | \forall i : \exists v_i \in V(b_i) : v_i(b_i) = u(b_i) \wedge u(a) \in [|u(\varphi(x))|]^{\mathcal{M}}\}$$

The writing is more tedious than the idea: the idea is that the set $V_{\varphi(x)}(a)$ gives us the val-set for the arbitrary term $a|_{\varphi(x)}$. If the formula φ does not include any other arbitrary term, then we are simply considering all possible assignments of a on objects of the classical model satisfying $\varphi(x)$. If other terms occur, we first evaluate the bottom of such terms, i.e. terms which are described by an L-formula; and then we inductively construct the higher-terms. The construction above allows us to retain the appropriate value-sets for the lower terms, while constructing a new one.

We now have everything to characterize the class of SV+-models, which roughly speaking will be all models constructible in the way explained above, using as arbitrary terms the elements of some $\mathcal{A} \subseteq Stip(L^*)$. Not any subset of $Stip(L^*)$ is suitable though. We shall call an A-term $a|_{\varphi(x)}$ *syntactically dependent on* $b|_{\psi(y)}$ if $b|_{\psi(y)}$ occurs in $\varphi(x)$; in symbols we shall write:

$$a|_{\varphi(x)} \prec_S b|_{\psi(y)}$$

If two terms are not only syntactically independent, but moreover there is also no term on which they both depend, then we shall call them *fully syntactically independent*. Then we are going to impose on $\mathcal{A}$ the following closure, which we shall call *syntactic closure*:

$$a|_{\varphi(x)} \in \mathcal{A} \quad \& \quad a|_{\varphi(x)} \prec_S b|_{\psi(y)} ==> b|_{\psi(y)} \in \mathcal{A}$$

That is, if b occurs in the stipulation-formula of $a \in \mathcal{A}$, then $b \in \mathcal{A}$.

Secondly, we want $\mathcal{A}$ to be unambiguous, which means if $a|_{\varphi(x)} \in \mathcal{A}$, then, for any $\psi(x) \neq \varphi(x)$, $a|_{\psi(x)} \notin \mathcal{A}$.

For $\mathcal{A} \subseteq Stip(L^*)$ satisfying syntactic closure and unambiguous, we let

$$\mathbb{V}_{\mathcal{A}} = \{V_{\varphi(x)}(a) \mid a|_{\varphi(x)} \in \mathcal{A}\}$$

(Syntactic closure is what grants that we can construct each val-set as showed in the procedure above).

Def. 12 *Let L be a first-order language, $\mathcal{M}$ an L-model and $\mathcal{A} \subseteq Stip(L^*)$ satisfy syntactic closure. Then we define the correspondent SV+-model as:*

$$\mathcal{M}^*_{\mathcal{A}} := (\mathcal{M}, \mathbb{V}_{\mathcal{A}})$$

We can now come to the semantical clauses for the satisfaction of formulas involving arbitrary terms. For $a|_{\varphi(x)} \in \mathcal{A}$:

Def. 13 $\mathcal{M}^*_{\mathcal{A}} \models \psi(a|_{\varphi(x)})$ *iff for all* $u \in V_{\varphi(x)}(a)$*:* $\mathcal{M} \models \psi(u(a|_{\varphi(x)}))$

Now how can we evaluate an n-tuples of arbitrary terms $a_1|_{\varphi_1}, .., a_n|_{\varphi_n}$? We shall define a notion of simultaneous/global val-set for $V(a_1), .., V(a_n)$, to be denoted as:

$$\bigwedge_{i=1}^{n} V(a_i)$$

If all such terms are fully syntactically independent, then we want the global val-set to simply consist in all combinations $\{v_1, .., v_n\}$ of valuations from $V(a_1), .., V(a_n)$. If there is some dependence relation, we need to make sure to combine the valuations in the right way: for example, if we have $a_1|_{x \leq b}$, $a_2|_{y=b^2}$ and $b|_{\psi}$: we cannot combine valuations v_1, v_2 such that $v_1(a_1) = 3$, $v_1(b) = 2$, $v_2(a_2) = 25$ and $v_2(b) = 5$: despite there not being any local problem, those values are not globally admissible. We achieve both these requirements with the following definition:

$$\bigwedge_{i=1}^{n} V(a_i) = \{\{v_1, .., v_n\} | v_i \in V(a_i) \wedge \forall b \forall j, k : a_j, a_k \prec_s b => v_j(b) = v_k(b)\}$$

Indeed, by the construction we defined above of val-sets, we know that each dependence relation will be satisfied appropriately if considered on its own, i.e. locally. Thus we only need to be sure that by combining the terms, we do not consider cases where one valuation of some term implies the falsification of the stipulation for some other term. The definition above also implies that when two terms, $a \prec_S b$, are evaluated simultaneously, the values considered will coincide with the values that valuations for a consider as admissible.

We can now define the semantical clause for n-tuples of terms as:

Def. 14 $\mathcal{M}^*_{\mathcal{A}} \models \psi(a_1|_{\varphi_1}, .., a_n|_{\varphi_n})$ *if*

$$\forall\{u_1, .., u_n\} \in \bigwedge_{i=1}^{n} V(a_i) : \mathcal{M} \models \psi(u_1(a_1|_{\varphi_1}), .., u_n(a_n|_{\varphi_n})$$

Before moving to the definition of a notion of dependence for this account, I want to informally clarify how a valuation for an arbitrary term would look like in this account. Let's consider an arbitrary term $a|_{\varphi(x)}$, where $\varphi(x) \equiv R(x, b|_{\psi(y)})$ and where $\psi(y) \equiv S(y, c|_{P(z)})$: so we basically have a's stipulation involving b, while b's stipulation involves c, and c is stipulated through an L-formula. In this case, we will have valuations for c as all possible assignments in $[|P(z)|]^{\mathcal{M}}$: valuations for b will assign values to c "copying" the val-set for c, and will extend those valuations with values satisfying b's stipulation; and similarly for a.

A new dependence relation

At this point we can obtain a notion of dependence relation which is satisfactory enough. The intuitive idea is that every time an arbitrary term a depends on an other term b, then this happened through a stipulation: a is anything which satisfies some formula $\varphi(x, b, \overline{c})$, for some A-term $\overline{c}$. However, we cannot naively identify value-dependence with syntactic dependence, for it is perfectly possible to construct a term which includes an other term in its stipulation but which is then independent. For example, we may say, "let a be such that $P(x) \wedge b = b$", where b satisfies $Q(y)$. In this case, there does not seem to be any value-dependence between a and b: whatever value we choose for b, this will not affect the choice of a value for a. So we want to characterize cases of genuine dependence as a proper subclass of cases of syntactic dependence, where the information regarding b in the stipulation actually affects the val-set for a. This is achieved formally by the following definition.

Def. 15 $a|_{\varphi} \prec^* b|_{\psi}$ *if*

(i) $a|_{\varphi} \prec_S b|_{\psi}$

(ii) There are no $\xi_1, \xi_2 \in Sfor(\varphi)$ *such that* ξ_1 *contains all occurrences of* x *and no occurrence of* b *(and viceversa for* ξ_2*) and*

$$\{v(a)|v \in V_{\xi_1}(a)\} = \{u(a)|u \in V_{\varphi}(a)\}$$

What this means is that either we cannot partition φ in two subformulas, one which only regards x, the other only regarding b (as when we stipulate a as $R(x,b)$); or, if we can, then constructing the val-set for $a|_\varphi$ does not yield the same result as taking the val-set for $a|_{\xi_1}$. For example, in the case above of $P(x) \wedge b = b$, for simplicity let $V_{P(x)\wedge b|_{Q(y)}=b|_{Q(y)}}(a) = V(a)$ and $V_{Q(y)}(b) = V(b)$ Then:

$$V(a) = \{u : \{b,a\} \to M | \exists v \in V(b) : u(b) = v(b) \wedge u(a) \in [|P(x) \wedge u(b) = u(b)|]^{\mathcal{M}}\}$$

But clearly, for any valuation $v \in V(b)$ will be such that $v(b) = v(b)$, therefore whenever $u \in V(a)$ then there will be some $v \in V_{P(x)}(a)$ such that $u(a) = v(a)$, and viceversa. In general this criterion entails that we lack dependency when $\xi_2(y)$ holds generically for $b|_{\psi(x)}$, that is when for all $\nu \in V_{\psi(x)}(b)$: $\mathcal{M} \models \xi_2(\nu(b))$: that is when the information regarding b in the stipulating formula of a cannot restrict the scope of admissible valuations for a.

This criterion defines a notion of immediate dependence, from which we can easily obtain a standard notion of dependence, by taking its transitive closure. I believe this to be a reasonable criterion, which captures the idea of value-dependence. By considering local valuations, instead than global ones (as in the supervaluationist approach that Fine probably has in mind), we get an account which is refined enough to distinguish cases where b's occurrence in the stipulating formula affects the values of a or not. We thus obtain a derived notion of dependence, which is based upon the stipulations used and the valuation of such stipulation in the underlying model. We saw already in Chapter 2 how the ranges of dependent terms should be seen as ranges of ranges, indexed by valuations. An alternative way to construe the matter is by saying that, while an independent term is comparable to a quantification over a subset, dependent terms to quantification over paths: we need to assign a value to all dependees (following the appropriate order, from independent ones and up in the hierarchy), then we can assign a value to the dependent term. The approach here allows us to account for this intuition.

Notice that in Fine's account, despite the relation being well-founded, we could still have a term dependent on infinitely many other terms. This would correspond to say "let a be such that all the following formulas are satisfied", and then proceed to give infinitely many formulas. For example, we may have an infinite collection of arbitrary terms and then stipulate an arbitrary term which is greater than all such terms. But how do we account for a case like that? The account can be generalized quite naturally. We will apply the same construction, with same closure on stipulations; only this time stipulations are (possibly infinite) sets of formulas.

For a generic language L, we define

$$Stip^+(L) = \{a|_\Delta | a \in At \wedge \Delta \subseteq L\}$$

So $Stip^+$ constructs terms described by possibly infinite set of formulas. Again we recursively define L^* as before, but we substitute $Stip$ with $Stip^+$:

Def. 16 *Let L be a first-order language:*

$L_0 = L$

$L_{n+1} = L_n \cup Stip^+(L_n)$

In general, for a collection of formulas Φ, let

$$[|\Phi|]^{\mathcal{M}} = \bigcap_{\varphi(x)\in\Phi} [|\varphi(x)|]^{\mathcal{M}}$$

Now, for $\Delta \subseteq L$, that is such that Δ only contains unary formulas from L, we define the val-set as:

$$V_\Delta(a) = \{v \subseteq \{a\} \to [|\Delta|]^{\mathcal{M}} |\text{ v is a function}\}$$

And supposing we have defined the val-sets for $\{b_i|_{\Psi_i}\}_{i\in I}$, the val-set for a term $a|_\Delta$ in whose stipulation such bs are all and the only A-terms occurring, is defined as follows. Let $V(b_i)$ denote $V_{\Psi_i(y)}(b_i)$. Then:

$$V_\Delta(a) = \{v \subseteq \{b_i\}_{i\in I} \cup \{a\} \to M | \forall i \in I : \exists v \in V(b_i) : v(b_i) = u(b_i) \wedge u(a) \in [|u(\Delta)|]^{\mathcal{M}}\}$$

Again we are going to consider a subset $\mathcal{A}$ of $Stip^+(L^*)$, a generalized notion of syntactic closure, as before: $a|_\Delta \prec_S b|_\Gamma$, if $b|_\Gamma$ occurs in Δ. And analogously for the syntactic closure on $\mathcal{A}$.

We can then proceed and define the class of models and the relevant notion of global valuations and formula satisfaction in the exact same way as before. The dependence relation needs a slightly different definition, but the essence is the same: rather than considering a partition in two subformulas of the stipulation of a, we consider a partition in two disjoint subset of $\{\psi(x)|\psi(x) \in \Delta \vee \psi(x) \in Sfor(\varphi), \varphi \in \Delta\}$. However to establish the connection with Fine's account we do not need this definition: we can also just keep it general, and simply copy the value-dependencies, without excluding those cases where the stipulation has no effect in restricting the scope of values. However, I still believe this latter to be a better notion of dependence.

Transformations

In this last section I shall investigate how one could transform a model from the class just defined so to obtain an equivalent F-model. And viceversa, under which conditions we can pass from an F-model to an SV^+-model. The precise characterization of this connection is left for future work; here we shall just present an intuitive construction to go back and forth between the two classes of models.

From SV^+ to F

First, we want to show how one can pass from an SV^+-model to an F-model copying all of its information (or enough of it at least). To do so our construction will have to convert local valuations in global valuations. We will follow a construction which resembles the one proposed in Chapter 2 for the supervaluationist approach.

Let $\mathcal{M}^*_{\mathcal{A}}$ be an SV^+-model. We construct an F-model in the following way. We first take all syntactically independent terms of $\mathcal{A}$ and order them linearly $\{c_i|_{X_i(x)}\}_{i \in I}$ (we shall also suppose the ordering is well-founded). We start with $A = \{c_i | i \in I\}$ and $V = \emptyset$ and $\prec = \emptyset$. For c_0, we simply copy the valuations in $V(c_0)$ and start putting them in V. Now we want to extend those valuations so to cover all combinations of simultaneous assignment of values covered by the product of all val-sets of all independent terms. Suppose we have defined valuations for all $j \leq i$. We can thus consider any $v \in V$: this will assign values to all $\langle c_j \rangle_{j \leq i}$. Suppose $\kappa = |rg(c_{i+1})|$ (where $rg(c_{i+1}) = [|X_{i+1}(x)|]^{\mathcal{M}}$): than we are going to construct κ-many extensions of v, one for each element of the range of c_{i+1}. And we are going to do this with all the valuations we obtained thus far.

We now come to the construction of dependent objects. Let $b|_\Gamma$ be a term and let $\{d_j|_{Y_j}\}_{j \in J}$ be all the terms occurring in Γ. Suppose by induction hypothesis that there are already in V valuations covering all ds, and all terms they syntactically depend upon. Then let $v \in V$ be such a valuation. Then we are going to extend v with the pair (b, m) iff $\exists u \in V_\Gamma(b)$ such that for all $j \in J$: $u(d_j) = v(d_j)$ and $u(b) = m$. Moreover, set

$$\bigcup_{j \in J} (b, d_j) \subseteq \prec$$

(That is, extend $\prec$ so that b gets dependent on all ds). Now, we let V be the set of valuations obtained by iterating this construction until we covered all terms in $\mathcal{A}$. Then we close V under Restriction and Piecing, and Restriction again.

We shall now check whether the model so obtained is indeed an F-model. All conditions hold trivially by construction, apart from (Partial Extension) and (Piecing), for which the proof is utterly analogous to the one provided in Chapter 2.

From F to SV^+

Let G be a definitional system which is unequivocal, well-founded and complete. Let $\mathcal{M}^F = (\mathcal{M}, A, \prec)$ be an F-model which realizes G in such a way that the following condition (C) is satisfied:

$$\forall (a, \Delta) \in G : \forall b \in A \text{ such that } \exists c \in A_\Delta \cup \{a\} : c \prec b\text{: then } \exists \psi \in \Delta \text{ such that both } c \text{ and } b \text{ occur in } \psi$$

(Informally this means that ψ defines the dependency between c and b (and possibly other objects)). These conditions need to ensure that when an F-model realizes a definition (a, Δ), the whole dependency tree $[a]$ of the realizing element is defined: i.e. all independent objects are defined, and all steps in the dependency chains are defined, too.

Given such a setting, we have a way to construct an SV^+-model preserving all the information we need. In particular, my conjecture is that if we apply on such SV^+-model the transformation defined above to obtain an F-model again, we will find an F-model which is *generically equivalent* with respect to the starting one, realizing G.

Let $(a, \Delta) \in G$. We first consider all independent objects $\{c_i\}_{i \in I}$ each with their respective definitions $X_i(c_i) \subseteq \Delta$. We thus constructs the arbitrary terms $\{c_i|_{X_i(x)}\}$, with their respective val-sets.
By condition (C) and completeness of G we know that every $b \in [a]$ is a defined term of (a, Δ). Now, supposing we have constructed the respective terms and val-sets for all objects that b depends on, let them be $\{d_j | j \in J\}$, and letting $Y(y)$ to be the definition of b in Δ, we can construct the val-set of b as

$$V(b) = \{v \subseteq [b] \to M | \forall j \in J : \exists v \in V(d_j) : v(d_j) = u(d_j) \wedge u(b) \in [|u(Y(y))|]^{\mathcal{M}}\}$$

So this procedure allows us to construct a model which copies the dependency tree of a in the F-model, by following the way in which dependencies are defined in the definitional system. We can then consider suitable combinations of the so defined local valuations as we did before.

Concluding Remarks and future applications

Let's summarize what was so far achieved. We investigated into a referential construal of the meaning-theoretic aspect of arbitrary terms. We saw that, for different reasons, all the mainstream options were unsatisfying, and we thus searched for an other option. This option, the quasi-referential account, seems to meet the desiderata that arose during the discussion, at least avoiding the critiques that we posited, and I believe it stands as a plausible alternative to such accounts. On the formal point of view, two main approaches were provided. One as an alternative to Fine's approach which does not take A-terms to denote objects other than the standard elements of the model under consideration. The other, whose aim is to stay closer to the intuitions used in the philosophical explanation of the quasi-referential account and which yields an interpretation of A-terms closer to their syntactical aspect.

As it was pointed out many times during this work, a first interesting direction for future work can surely be to to establish a precise relationship between the different classes of models that were considered, and between those classes and F-models as they were proposed by Fine. Can we regard the different classes of models to be equivalent modulo their theory, or is there something radically different, also on a mathematical point of view? A conjecture which comes to my mind is the following: let's call the first transformation, from SV^+-models to F-models, π; while the latter shall be denoted by τ. We wish to show that, if $\mathcal{M}^F$ realizes G (respecting all the constraints we listed in the relevant section), then $\pi(\tau(\mathcal{M}^F)) \cong_g \mathcal{M}^F$. This show that, under suitable restrictions, namely if, as it would happen in an everyday mathematical context, we explicitly stipulate all definitions of the arbitrary terms involved, and all dependencies between them, then the SV^+-approach can preserve all relevant information from the F-model. Similarly, we could define a notion of equivalence for SV^+-models and show that applying τ and then π preserves this notion.

A second direction of work regards the application of the quasi-referential account to the debate around structuralism. Horsten (2019) started using arbitrary objects as counterparts for the concept of structure, and I believe a connection can be established also in our case. Indeed, one may regard terms standing for structures as arbitrary terms, and in general the reference we fix to structures (such as $\mathbb{N}$) as instances of arbitrary reference: what would be the consequences of construing reference to structure as arbitrary reference, and arbitrary reference using the quasi-referential account for the debate around structuralism? I believe that an original concept of structure – as indeed what is denoted quasi-referentially – could then arise and provide an interesting and fruitful alternative to the standard dichotomy between eliminativist and non-eliminativist.

Moreover, it would surely be interesting to explore other possible mathematical formulations compatible with the quasi-referential account. As we have seen, what this approach suggests is that our meta-theory on reference should be regarded as non-classically closed: is it possible to do so formally? hence constructing a class of models where reference behaves in this way? The approaches we developed always use a classical referential apparatus for the models: what we did (as also Fine did) was rather use a collection of valuations/reference relations to represent the arbitrarity of reference. But is it possible to obtain some more direct approach? This would require much preliminary work, for what we need is a non-classical meta-theory, which is no trivial issue.

This connects to the issue of generalization. In all formalizations, and also in all of the discussion, we assumed that the underlying range of arbitrary terms was fixed, a classical set; and we basically defined what was true of the arbitrary term in terms of such set. However, we use arbitrary reasoning also outside of the context of classical mathematics, and also outside of the context of mathematics: how could we generalize (some of) the ideas proposed so to yield an apparently sound treatment of arbitrary reasoning? For example, how could we define arbitrary terms if the underlying model is intuitionistic, or para-consistent? how could we treat arbitrary terms if no underlying model is identifiable, maybe in a case of ordinary day-to-day reasoning?

Finally, the use we make of arbitrary terms, even within the context of classical mathematics, sometimes plays out in ignorance of the range, e.g. in proofs of uniqueness. Moreover, we never establish that an arbitrary representative has a property because we check every element in its range, rather the arbitrary term is a tool we use to avoid checking all single instances to establish a general fact. The role of instances, on a very practical and proof-theoretic aspect, seem to be purely negative: we can check instances in the range only to establish that something does not hold of the arbitrary term, i.e. when there is a falsifying instance; but when we assert that something does hold, it seems we must pass from a different route: which exactly? So as a further line of research, one could try to develop an account which stays closer to practice, aiming at a constructive representation of the way in which knowledge around arbitrary terms is actually built.

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