Metaontological Skepticism

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Contents

1. Introduction

1.1 Worrying Questions

How can it be that philosophers haven't made any progress in ontology for 2500 years? Why do we still not know whether there are abstract objects? Why is it that no rational consensus has been secured over any substantial proposition about what there is?

Metaontological skepticism yields an answer to these questions. The answer is pessimistic. Ontology is not a legitimate philosophical discipline. Therefore it is no wonder that we haven't made any progress in ontology for such a long time and that no rational consensus has ever been secured over any substantial, foundational proposition about what there is. When we are engaging in ontology, we are engaging in an illegitimate discipline.

But what are the arguments for this claim? Is ontology really an intellectually worthless endeavour?

In this dissertation I will try to answer these questions by outlining, criticizing and assessing the prospects for metaontological skepticism. I will do this by categorizing and laying out the motivations and arguments for various skeptical metaontological positions to show where their strengths and weaknesses lie. Moreover, I will draw some conclusions for disciplines like the philosophy of language, that follow from my work on metaontological skepticism. And I suggest the outlines of an own account of ontology. But before we will get started with this project, I want to flesh out in greater detail, what metaontological skepticism is.

Metaontological skepticism is a skeptical metaontological position. So, first, what is metaontology?

1.2 Metaontology

Metaontology can be characterized as the philosophical study of the foundations, presuppositions and limits of ontology. A study that includes a clarification of the basic concepts and the legitimacy and the cognitive status of ontology, as well as a formulation and investigation of the task and the genuine methods of its subject matter discipline.

You should note however, that the word "metaontology" involves an ambiguity. The ambiguity is of the same kind as the ambiguity in the word "ontology". Let's illustrate the ambiguity by means of the – possibly more familiar – word "ontology". On the one hand, "ontology" denotes a philosophical discipline. Namely, that discipline which is concerned with what there is. The characterization of metaontology sketched out above matches this use of ontology. On the other hand, "ontology" denotes the entities that a theory, a discourse, or a person, assumes or is committed to. For instance, a discourse about whether the electrons under a dielectric in a MOS transistor function as charge carriers assumes that there are electrons under the dielectric of a MOS transistor. A sincere Hawaiian *kahuna* in the 17th century reporting his encounters with the ghosts of deceased clansmen assumes that there are ghosts which he can talk to. By the same token, the word "metaontology" can be used to stand for the theory of the nature of ontology which one adopts; it can stand for *ones* metaontology. In this sense, my metaontology is possibly different from yours. Of course, this cannot be the case, when the first characterization mentioned is adopted.[1] In the following, I will use "metaontology" first and foremost in the sense of the first characterization mentioned.

[1] You should note that the subject matter of metaphysics cannot be derived from the prefix „meta-" that obviously. After all, the subject matter of metaphysics is not physics and not any other scientific discipline. Rather, the name "metaphysics" dates back to Andronicus of Rhodes, who edited the Aristotelian works, at around 70 B.C. For the loose body of Aristotelian works which he chose to class after the "physics", he reserved the name „τὰ μετὰ τὰ φυσικά" which means „the books following the books about the nature". (It is often said that from a systematic point of view, however, one of the following two suggestions would probably have been more appropriate: 'ἡ περὶ τῶν πρώτων θεωρία' [„the theory of which is first"; (Theophrast)], 'περὶ πρώτης φιλοσοφίας' [„about the first philosophy"].) Eventually, in Neo-Platonism this name became the systematic title for philosophical investigations and questions, we would be inclined to call "metaphysical" today.

In contemporary philosophy, metaontology enjoys the outstanding status of being – as it is put by the Leeds Gang Ross Cameron and Jason Turner – "the new black". A 1998 publication by Peter van Inwagen[2], bearing the very name "metaontology", both introduced the name "metaontology" into the jargon of the philosophical community and revived the interest in questions which had already been heavily discussed from the 1940's to the 1970's under the lead of Quine and Carnap but somehow had lost its attraction to philosophers during the 1980's and 1990's. Some paradigmatic examples for metaontological questions are, "Is being an activity?", "Is being the same as existence?", or "Does the existential quantifier adequately capture the single sense of existence?".

Obviously, Van Inwagen introduced the name "metaontology" to stress its character as a metadiscipline. Metaontology is about ontology. It is a discipline about a discipline. Insofar, it is like metaethics, metaphilosophy, or metamathematics. But - like metaethics and very much unlike metaphilosophy and metamathematics - it is no part of its subject-matter. Metamathematics is obviously still a part of mathematics, metaphilosophy still a part of philosophy.[3] So much about metaontology. What about skepticism?

1.3 Skepticism

Philosophical skepticism stirs up most dust in epistemology. Most notable are the skepticisms about knowledge and justification. However, skepticism is not solely restricted to epistemology. There is skepticism about meaning, and reference. About the past, and the future. About the existence of time and so on and so forth. In short, one can be a skeptic about almost everything which can be seriously discussed and thought about. Since so much attention, though, is brought to skepticism in epistemology, the extensive research done in this philosophical area about this topic is very helpful for illustrating what makes up philosophical skepticism in general and metaontological skepticism in particular.[4]

Skepticism in epistemology would not be very interesting if the subject matter of its doubt was the proposition that we know exactly that the visitors of Munich's tourist magnet "Wiesn" will have sunny weather on September 28th seven years from now. No one believes

[2] Van Inwagen (1998)

[3] Cameron (2008), Turner (2008d).

[4] In my illustration, I heavily draw on the excellent exposition of Klein (2005).

that we know this. No one expects it. In fact, everyone doubts it.[5] But skepticism in epistemology goes further. The subject matter of its doubt is not a solitary proposition like the one just mentioned. Rather, it extends to whole kinds of propositions - following Klein (2005) I will call them *epistemologically interesting propositions* or *EI-type propositions* - which contain tokens, many of which are generally thought to be known, given what we ordinarily take knowledge to be. It concerns, for instance, knowledge about the future, the present or the past, knowledge about other people's minds, or about the "external world." Everyone believes that we have such knowledge. Everyone expects it and no one doubts it. So if the skeptic was right and we would be wrong this would be a surprising result.

Now, consider some proposition p. Basically, there are only three possible attitudes one can have towards p's truth when considering whether p is true. I say "basically", since even though there are - of course - multifarious attitudes one could have towards p all these attitudes towards a proposition can be reduced to the following three basic types of attitudes:

1. One can assent to p.
2. One can dissent to p.
3. One can remain agnostic as to whether p or non-p.

You might be happy or sad that p. Or which is much more common, you might simply be uninterested as to whether p. But these attitudes are parasitic on one of the three basic types. When we are happy or sad that p we are happy or sad that p is true, when we assent to p. Being happy or sad results from our assenting to p or non-p. When we are uninterested in p we are *not* considering whether *p* is true and we remain agnostic as to whether p or non-p (at least in some cases).

The proposition, *We can have knowledge of epistemologically interesting propositions* is about the very scope of our knowledge. Given what we have said in the last paragraph we can have the following three attitudes towards this proposition:

(1) One can assent that we can have knowledge of EI-type propositions.
(2) One can dissent that we can have knowledge of EI-type propositions.
(3) One can remain agnostic as to whether we can or cannot have knowledge of EI-type propositions.

[5] Note that there are also exceptions. A reader commented: "An meinem Geburtstag gibt's jedes Jahr gutes Wetter auf der Wiesn!"

I will call adherents of (1) *Epistemists*. Epistemism is the typical non-skeptical position shared by almost all epistemologists today. This position stands in contrast to the skeptical positions expressed by (2) and (3). For the adherents of (2), I will reserve the name *Cartesian Skeptics* or simply *Cartesians* although one can also find different names like *Academic Skeptics* (and correspondingly *Academics*) or *possible /switched world skeptics* circulating in the literature.[6] It is not hard to see that the name "Cartesians" dates back to René Descartes (1596 – 1650) who can be seen as the inventor of modern skepticism in the mid-17th century.[7] Whilst the name "Academics" was first used by Sextus Empiricus (around 200 A.C.) to refer to the leaders of the Academy (founded by Plato) during the 3rd to 1st century B.C. Of course it is a controversial question among historians of philosophy whether they really doubted knowledge of EI-type propositions. Switched world skepticism or possible world skepticism typically involve imagining oneself to be in some possible world that is both vastly different from the actual world and at the same time absolutely indistinguishable (at least by us) from the actual world.[8] What underlies this form of skepticism is assent to the proposition that we cannot know EI-type propositions because our evidence is inadequate.

Adherents of (3) will be called *Pyrrhonian Skeptics* after Pyrrho (ca 365 – ca 275 B.C). The main part of what we know about Pyrrhonian Skepticism is due to the writings of Sextus Empiricus (at around the end of the 2nd century AD). The Pyrrhonians' aim in remaining agnostic as to whether we can or cannot have knowledge of EI-type propositions, is to being able to withhold assent to all propositions about which genuine epistemological dispute was possible. Thus they did not fall prey to the dogmatism of the Epistemists and Cartesians. According to them, both Epistemists and Cartesians were likewise dogmatic in assenting to the - by pure arguments - unjustifiable and never establishable proposition that we can have

[6] Of course, there is this other notorious form of "possible world skepcitism", namely Quine's, according to which there are no such things as possible worlds.

[7] Cf. Popkin (2003). To term Descartes "the inventor of skepticism" would be a bit unfair. After all, his famous development of skepticism culminating in the famous *Cogito*-argument is a product of the work of a huge load of other philosophers who had worked on this topic in the two centuries before Descartes. But Descartes was also the one who radicalized skepticism in such an genuinely new, influential and brilliant way that no one afterwards working on skepticism could have failed to learn from his work. Given that, I think it not completely unfair to call him the "inventor of *modern* skepticism".

[8] An interesting version of switched world skepticism is developed in Williamson (2000), where the skeptic is characterized as someone holding that we can never rule out the skeptical scenario since we are never able to tell, whether we are in a state of mind that is different but entirely indistinguishable from the one we are in when we are not in a skeptical scenario.

knowledge, or in assenting to the denial of that claim, respectively. The Pyrrhonian accuses the Cartesian Skeptic of holding an inconsistent and self-defeating position by uncritically relying on the ability of reason in his arguments concerning epistemological propositions about EI-type propositions. This problem is famously called the "Cartesian circle" and used to be one of the big problems in Middle Ages philosophy. To be more precise, as you certainly remember, in his First Meditation Descartes, starts out as follows:

> "Several years have now elapsed since I first became aware that I had accepted, even from my youth, many false opinions for true, and that consequently what I afterward based on such principles was highly doubtful; and from that time I was convinced of the necessity of undertaking once in my life to rid myself of all the opinions I had adopted, and of commencing anew the work of building from the foundation, if I desired to establish a firm and abiding superstructure in the sciences." (Descartes 1641: 11).

He then sallies out to demolish any confidence in the epistemic reliability of our faculties by raising the heavy armory of skeptical doubts. However, as the Pyrrhonian points out, Descartes has never ceased to rely on his power of reasoning during this process. There is thus a circularity in the Cartesian's argument. According to the Pyrrhonian the only way out of this predicament is the dogmatism culminating in Luther's famous: Here I stand, relying on the abilities of my reason, and as such I can do no other. But besides this dogmatism there is not a single argument for the Cartesian's conclusion. Rather the Pyrrhonian suggested that remaining agnostic as to whether EI-type propositions were knowable or not, was the only appropriate epistemic attitude to have. This having said, I turn to the combination of metaontology and skepticism: metaontological skepticism, the topic of this dissertation.

1.4 Metaontological Skepticism

In metaontology, a corresponding distinction to the just mentioned trichotomy between Epistemists, Cartesians and Pyrrhonians can be applied to describe the various positions toward the question, "Is ontology a legitimate philosophical discipline?".

First, there are the ones who hold that ontology is a legitimate philosophical discipline. These are all the nominalists and realists, all the ones who seriously engage into ontology. Call them *Ontists*. Second, there are the ones who deny that ontology is a legitimate philosophical discipline, which I will call *metaontological Cartesians*. And, third, there are the ones who want to remain agnostic as to whether ontology is a meaningless discipline. These will be called *metaontological Pyrrhonians*.

Let's have a look into which category the different skeptical metaontological theories discussed in this work fall.
The theory of quantifier variance, the first skeptical metaontological theory I will discuss in this dissertation, holds that ontological disputes are nothing over and above trivial disputes about how we should use our language. Insofar ontological disputes are on the same level as disputes about whether we should use "mile" for a nautical mile or for a statute mile. The quantifier variantist even thinks that for any ontological view there is a language such that the ontological view comes out true in this language. Given that quantifier variantists say that ontological sentences are trivial, ontology is not a legitimate discipline. Thus quantifier variantism is a Cartesian metaontological position.

The second skeptical theory that I will discuss - Carnap's 1950 account of metaontology - is on a first-order ontological level, a Phyrronian discipline, and thus a kind of ontological agnosticism. Carnap and his fellows withhold both assent and dissent as to whether abstract objects exist. But on a second-order level when it comes down to having to decide which stance to take towards ontology, it is a genuine Cartesian skeptical account, for they deny that ontological existential sentences are cognitively significant, which causes them to say that ontology is not a legitimate philosophical discipline. By the way, the skeptical metaontological account which Carnap developed in his (1950) is - together with the theory of quantifier variantism - the most influential skeptical metaontological account in contemporary philosophy.

Jonathan Schaffer, one of the most influential (some even say *the* most influential) contemporary philosopher(s), developed another Cartesian skeptical metaontological account. This is the third skeptical theory that I will discuss in this dissertation. According to his metaontological account traditional metaphysics *without* ontology is as informative as it is *with* ontology. Schaffer thinks that the thesis that (almost) everything exists, a thesis, which Schaffer calls *permissivism*, holds only for some special kind of existence question. Given that the traditional existence questions (as for instance "Are there numbers?") are in his opinion of this special kind, permissivism holds for the traditional existence claims. But it does so if and only if ontology does not add any informative content to metaphysics. So, ontology does not add any informative content to metaphysics. Schaffer concludes that ontology is an illegitimate philosophical discipline. Given his permissivism ("everything exists") Schaffer is a first-order Ontist. But on a higher-order, on a metaontological level he is a genuine Cartesian.

It is important to note that all the mentioned skeptical metaontological positions are motivated by linguistic considerations. They all develop their skepticism out of the assumption that ontological sentences are either trivial, or meaningless, or both, in short, that they are without cognitive significance.

The quantifier variantist argues that ontological disputes are nothing over and above trivial considerations about which language or which conventions in a language to choose. Carnap argues that ontological sentences are trivial within a framework and meaningless outside of such a framework. Schaffer argued that ontological sentences are trivial, since there trivially exists anything.

The overall reasoning of these skeptics can be captured by the following argument.

LINGUISTICALLY-MOTIVATED ONTOLOGICAL SKEPTICISM:

(P1) Ontological sentences are without cognitive significance.
(P2) If the sentences of a supposed philosophical discipline are without cognitive significance, the discipline is illegitimate/valueless.
(C) Ontology is illegitimate/valueless.

Even though other forms of metaontological skepticism are conceivable, all currently available skeptical metaontological theories are linguistically-motivated in such a way.
Thus, for the most part of this dissertation, I will be concerned with this kind of metaontological skepticism. Only in the end I will sketch other non-linguistically motivated forms of metaontological skepticism. *Note that when I speak of metaontological skepticism in this dissertation, I mean the linguistically-motivated metaontological skepticism, if I don't say otherwise.* Finally, here's an overview of the dissertation.

1.5 Overview

In the second chapter of this dissertation, I will be concerned with the theory of quantifier variance. I will argue that the theory of quantifier variance is not a genuinely skeptical metaontological position. Rather, quantifier variance is an ontic metaontological position in disguise. Furthermore, I show that the quantifier variantist has problems in accounting the tight connection between language and beliefs.

The third chapter contains a discussion of Carnap's sceptical metaontological account. I will show that a proper understanding of Carnap's work from 1950 cannot be gained without a prior understanding of its intellectual roots in the philosophy of the logical positivists, the Vienna Circle and other writings of Carnap's from the post-Vienna period. On this base I will give a thorough reconstruction of Carnap's 1950 account and the involved notion of a *linguistic framework*. Then I will rebut the currently widespread move to identify Carnap's theory with the theory of quantifier variance.

In the fourth chapter, I give an argument against Carnap's account in particular and against some forms of linguistically-motivated metaontological skepticism in general. I will argue against these forms of metaontological skepticism. They are self-defeating. Even though metaontological skepticism is not self-defeater instable *per se*, the most promising and widely adopted ways of developing it are.

In the fifth chapter, I am concerned with Jonathan Schaffer's sceptical metaontology. I will argue that he is wrong in assuming, first, that not all traditional existence questions are trivial and second, that all traditional existence questions belong to a special kind of existence

question, existence questions *per se*. In these arguments, I adopt methods and theories of contemporary philosophy of language. This paves the way for the discussion in the next chapter.

In that chapter, the sixth chapter, I show that our ignorance about what numbers are, leads to a hard problem for the standard theory of reference, the theory of direct reference. Direct reference theorists hold that the meaning of singular terms is completely exhausted by their referents. In the case of numerals then, the meaning of numerals is completely exhausted by the numbers they refer to. I will argue that this assumption leads to severe problems in accounting for how we can understand mathematical sentences.

The seventh chapter contains some concluding remarks, but also the outlines of a new approach. Even though ontology may not be able to tell us what exists, this does not render ontology illegitimate or valueless. Rather, the (or a) task of ontology is to build good models that can serve as frames for the theories of other philosophical or non-philosophical disciplines.

Skeptical metaontological positions often involve triviality claims. But so far no attempts have been made to explicate this notion. I will close this gap in the literature in the Appendix. In my explication, I will exploit the fact that triviality is tightly connected to lack of information.

2. Quantifier Variance

This chapter contains my first discussion of a position commonly running under the label "skeptical metaontological". This position is the theory of quantifier variance. The theory of quantifier variance is explicitly defended in Hirsch (2002), (2004) and Turner (2008a) and (2008b) but often also ascribed to Putnam (1987a) and (1987b) and Rudolf Carnap (1950). In recent years, the theory of quantifier variance has gained a lot of attention in metaontology. Examples are Sider (2001), (2004), (2006), (2007) and (2008), Hawthorne (2006), and (2008), Eklund (2007), (2008a), and (2008b), Bennett (2008), Chalmers (2008), Hale and Wright (2008). I will first state the theory, both in an informal and a formal way, and, secondly work out three arguments against the theory of quantifier variance. The upshot will be that the theory of quantifier variance is not a genuinely skeptical metaontological position.

2.1 Overview

For Hirsch ontological disputes are nothing over and above trivial disputes about how we should use certain expressions. Hirsch holds that for any ontological position a language exists whose ontological vocabulary (as for instance the existential quantifier) possesses such a meaning that the claims of this ontological position come out true in this language.

Ontological dispute is as trivial as the dispute about whether one should use the expression "mile" synonymous with the expression "nautical mile" or "statute mile."

As an example, consider the following situation. A British and a French sailor talk about whether one should use the car to get from village A to village B or whether one can also use the bike. It is common knowledge among the two conversational partners that the distance between the two villages amounts to 1852 metres.
The French sailor says that it is well possible to use the bikes since the distance between A and B amounts only to a mile. And, after all, it is well possible to cover a distance of one mile by bike. The British sailor heavily disagrees. The distance is not a mile at all. 1852m does not make up a mile. But the French sailor still disagrees with British sailor. Finally, they come to blows.

What do the two sailors argue about? They certainly do not argue about whether the distance between A and B amounts to 1852m. This is common knowledge among them. Rather, they only argue about whether "mile" should be used synonymous with "nautical mile" or with "statute mile". While the French sailor argues that "mile" should be used with "nautical mile," the British sailor heavily disagrees. For him the word "mile" should be reserved as being synonymous with "statute mile". Thus they only argue about how words should be used and about which conventions for a word should be adopted. In other words, their dispute was *merely verbal*. Should we use the convention that "mile" denotes a distance of 1852m or that a mile denotes a distance of about 1609m? Our normal reaction would be to claim that we couldn't care less which expression to use. After all it is pretty pointless to say that one of the options fares generally better than the other. They are on a par.

According to Hirsch, this is exactly what happens when two ontologists argue about what exists. He writes:

> "What the doctrine does imply is that our linguistic decisions determine the meaning of the expression "there exists something"; hence, they determine the meaning of the sentence "There exists something composed of Clinton's nose and the Eiffel Tower". Hence, the truth or falsity of this sentence depends in part on our linguistic decisions. It is merely a use-mention confusion to conclude that whether or not there exists something composed of Clinton and the Eiffel Tower depends on our linguistic decisions" (Hirsch 2002: 52).

Take two ontologists, say a nominalist and a platonist. Ordinarily, one would be inclined to think that the nominalist and the platonist argue about whether there are abstract objects. While the nominalist denies that there are abstract objects, the platonist affirms that there are abstract objects. However, this is not what Hirsch maintains. According to Hirsch, the nominalist and the platonist do not argue about whether abstract objects belong to what reality is made up by, rather they argue about which meanings we should give to our words. They argue only about whether the ontological vocabulary should be used as in the nominalist's language or whether it should be used as in the platonist's language. Both languages are on a par. No language is intrinsically better than the other. But in the nominalist's language "there

are no abstract objects" comes out true, while in the platonist's language, "there are abstract objects" comes out true.

Another example is the debate over the ontology of material objects. This debate centers around the question "When are given material objects part of some further composite object?". A mereological universalist, Lewis (1991) or Link (1998) for instance, wants to say that given material objects always form some further composite object. Thus, Kurt Gödel's pen and Alfred Tarski's wife form a composite object. Others, like Dorr (2005), say that this is never the case. There are no composite material objects. Some, like Van Inwagen (1987), say that composite objects sometimes, but not always, exist. Only if objects stand in the appropriate relation to each other like being appropriately glued together, there exists a further material object that they compose. Here Hirsch thinks, that the different factions do not argue about whether reality is made up by composite material objects, but rather about which meanings we should give to our words. Their dispute is again merely verbal. They only argue about whether the ontological vocabulary should be used as in the mereological universalist's, the mereological nihilist's or the mereological moderatist's language. All languages are on a par. No language is intrinsically better than any other. Hirsch writes:

> "There are many possible perspectives on 'the existence of objects', which are all adequate for describing the same facts, the 'same way the world is'" (2004: 231).

So, according to Hirsch, a quantifier variantist will "…address a typical question of ontology either by shrugging it off with Carnapian tolerance for many different answers, or by insisting with Austanian glee that the answer is laughably trivial" (2002: 67).

To put it in a nutshell, the theory of quantifier variance is the conjunction of the following two theses:

First. For each ontological position, there is a language, such that the quantificational expressions can be interpreted such that the theory comes out true.

Second. None of these interpretations and languages is any "better" than the others.

So far so good. But the theory of quantifier variance is not the clearest theory. What are these meanings and interpretations of quantificational expressions? Do existential quantifiers

express other things than the second-order property of being non-empty? And why should there be any such languages or interpretations? We do yet not have answers to these questions. We need a sharper formulation of the theory of quantifier variance.

2.2 An Attempt to Specify the Theory[9]

An explication of the theory of quantifier variance must satisfy firstly an appropriate account of what these quantifier meanings are and secondly of what is understood under "better". There are two natural suggestions to answer the first question. These suggestions are based on the fact that a quantifier Q is normally understood as ascribing to its domain D a property P. Thus changing the meaning of the quantifier Q either involves changing domain D or property P.

The first natural suggestion (made by Øystein Linnebo in p.c.) to answer the first question does not work. The suggestion is that the quantifier meanings are just the restrictions of the quantifiers. This idea has it that the mereological universalist and the mereological nihilist only differ by restricting their quantifiers in unequal ways. The mereological nihilist's quantifier for example is restricted to all atoms, while the ontological pluralist's quantifier is not restricted to atoms only, but also quantifies over the composite material objects made up by these atoms. The problem with this suggestion is that if a quantifier is restricted, one just ignores some of the things which one excludes by the restriction. But the things are still there. According to this suggestion, the mereological nihilist would never be a real nihilist but rather a mereological universalist who just ignores for a certain while some of the things a universalist believes in.

The second suggestion to answer the first question does not work either. Normally the existential quantifier is understood as expressing that its domain is non-empty. Or in other words, the existential quantifier expresses the second-order property of sets of being non-empty or, again in slightly different terms, the second-order property of properties of being instantiated at least once. This is the *overlap property* of the existential quantifier. So, if the meanings of the quantifiers vary with the languages, one could argue that the existential quantifiers in these languages express other second-order properties like the property of being

[9] In this section I draw heavily on Sider (2007).

instantiated at least twice (or being at least two-membered), or being instantiated at least three times (or being at least three membered), etc. However, this suggestion doesn't hold, since the inference rules for the existential quantifier would have to be changed accordingly in the different languages. But the quantifier variantist does not want to change the inference rules. Thus, the second suggestion can not be what the quantifier variantist means.

This leaves it pretty mysterious what the quantifier variantist's claim about the different meanings of the existential quantifiers boils down to. In fact, I think that the theory of quantifier variance is already dead at this point. But let's see whether we can make allowances to the quantifier variantist. In face of the quantifier variantist's enigmatic quantifier meanings, the most reasonable way out of this lacuna is giving up the task of trying to understand what these quantifier meanings are supposed to *be*. Rather, I will follow the lead of Sider (2007), who has done as much as no other to give a better formulation of the theory of quantifier, in only trying to understand what the quantifier meanings are supposed to *do*.

The second question –what does "better" mean – is not easy to answer either. Sider tries to answer this question by extending Lewis's (1983) eligibility theory of meaning to logical expressions. According to Lewis (1983), the meaning of a term is at least in part dependent on the eligibility of the entities that are the possible meanings of the term. Meaning is not simply supervenient on use. Lewis thought this amendment to the meaning-as-use theory necessary in face of some worries brought up by Putnam (1980) and Kripke (1982). As an example, consider Kripke's worries. Kripke presents a skeptic who claims that there is nothing in a speaker's use of a particular expression that determines what meaning the expression has. "+", for instance, could be used by an ordinary speaker, such that it does not express the addition function, but the "quaddition function". The quaddition function yields the same numbers as the addition function, when the arguments taken by the function are less than 10^{10} but yields 125, when either of the arguments is greater than 10^{10}. The paradox is that even though there is nothing in the use of "+" which determines that an ordinary speaker means addition rather than quaddition when he uses "+", yet "+" means addition rather than quaddition. As I already mentioned, Lewis thinks that there is something which makes the addition function a more natural referent of "+" than the quaddition function since "it carves nature better at its joints".

Let's turn back to the existential quantifier. If one accepts the results of Kripke's thought experiment, there might be uncountable different meanings for the existential quantifier. If one accepts Lewis's general outlook, these different meanings for the quantifier might differ from each other in their degree of naturalness as meanings of the quantifier. As Sider (2003: 144) says:

> "existence is the one and only highly eligible meaning that fits our use of (unrestricted) quantificational expressions".

For the sake of simplicity, I will join Sider in fleshing out the notion of "being a better meaning of a quantifier" with the notion of "being a more natural meaning (in the sense just discussed) of a quantifier". However, I don't think that one is forced to do that. Other ways are always open, as an axiomatic treatment of the "better than"-relation.

Above, we concluded that it is wholly mysterious what the quantifier meanings are. Therefore, we gave up the attempt to say what they are and confined us to the task of saying what they are supposed to do. Three demands have to be met by quantifier meanings. First it must be possible to ascribe the notion of naturalness to them. Second they have to play a part in determining the truth-values for quantified sentences. Third it must be possible to say that there are more or less "expansive" quantifier meanings, where this is not the same as varying domain restrictions.

Sider (2007: 11) satisfies these three demands by introducing three undefined relations for a meaning-context pair <m,c>. (Contexts are necessary as an explanation of contextual variation of quantifier domains). The relations are as follows.

(1) Meaning m is *at least as natural as* meaning m'.
(2) Model M *depicts* meaning-context pair <m,c>.
(3) Context c *belongs to* meaning m.[10]

[10] Sider doesn't define "meaning", "naturalness", and so on. One could do this by giving axioms which govern the relations. So, for instance, the relation "at least as natural as" could be governed by the following axioms where the quantifiers range over meanings, where "N" represents the relation "is at least as natural as":
(N1) $\forall x \forall y \forall z\ (Nxy \wedge Nyz \rightarrow Nxz)$
(N2) $\forall x(Nxx)$

The first relation is supposed to help answer the first constraint, the second one the second constraint, and the third one the third constraint. If Q is a meaning context pair <m,c>, such that (2) is satisfied, then Q is called a *quantifier*.

By means of clause (2), a definition of truth for a sentence relative to a given meaning can be given. The definition is as follows:

DEFINITION 1 Sentence ϕ is true^{c}_{m} if and only if ϕ is true in some model that depicts <m,c>.

Moreover, let the following be true:

(a) For each context, there is exactly one meaning to which it belongs.
(b) No model depicts anything other than a quantifier.
(c) Each quantifier is depicted by some model.
(d) The same sentences are true in any two models that depict the same quantifier.

So far we have shown how the first two constraints can be satisfied. But what about the third constraint? According to the third constraint, it must be possible to say that there are more or less "expansive" quantifier meanings, where this is not identical with varying domain restrictions. So how can we model these expansions, that are independent of domain restrictions? A possibility is to adopt the following definition:

(N3) $\neg\forall x\forall y\,(Nxy\wedge Nyx\rightarrow x=y)$

With the relation "is at least as natural as", it is simple to define the relation "is as natural as" thus:

$N^{=}xy :\leftrightarrow Nxy\wedge Nyx$,

and the relation "is more natural than" thus:

$N^{+}xy :\leftrightarrow Nxy\wedge\neg N^{=}xy$.

Antisymmetry, which is the negation of (N3) doesn't hold. That is, it doesn't hold that:

(N4) $\forall x\forall y(N^{=}xy\rightarrow x=y)$. Why is this? (N4) says that if two meanings are exactly alike in naturalness, then they are identical. In such a case, the theory of quantifier variance would be trivially wrong. After all, it assumes that the candidate meanings of the existential quantifier are equally natural but not identical. Obviously, one can also define "is at least as natural as" by means of the above defined relations "is as natural as" and "is more natural as". Of course the axioms are trivial. The real challenge for Sider, however, is to give *non-trivial* axioms.

DEFINITION 2 Quantifer Q *expands* quantifier Q' if and only if every model that depicts Q' has a supermodel that depicts Q. Q *properly expands* Q' if and only if in addition, Q'does not expand Q.

This definition captures the idea that a mere restriction changes the context but retains the same meaning, whereas the distinctive kind of expansion changes the meaning as well as the context. This is achieved as follows: We can expand a quantifier in another way than by manipulating its domain that is blowing up or downsizing it. Proper expansion collapses into ordinary restriction, if <m,c> properly expands <m',c'> and m=m', because then only the contexts c and c' differ. After all, the restrictions on the domains are dependent on contexts. A quantifier determines together with a context a restriction of the domain.
But here we have the possibility of some other kind of expansion, if m≠m'.

Now we are able to state the first versions of the thesis of the quantifier variantist. Let M be a class of models. The models in M are regarded as "quantifier worlds". This is intended to capture the idea that the models describe what the world would be like given various quantifier meanings. After all, as Harold Hodes (1991a) once elegantly put it: Truth in a model is a model of truth. If E is a set of meanings, let Q(E)={<m,c> | c belongs to m and m∈E} be the set of quantifiers that are grounded in E. Q(E) is made up by the multiple candidate quantifier meanings in which the quantifier variantist believes. Any such claim will say roughly that each quantifier world in M depicts some quantifier in Q(E).

How do quantifier worlds and quantifiers correspond to each other? Here are four suggestions:

THESIS 1 Weak M/E-quantifier variance

Every member of M depicts some member of Q(E)

But Weak M/E-quantifier variance allows that all the quantifiers in Q(E) are restrictions on a single maximal quantifier.

So we need something stronger. Here are some suggestions:

THESIS 2 Moderate $\mathbb{M}$/E-quantifier variance
Weak $\mathbb{M}$/E-quantifier variance + some member of $\mathbb{M}$ outruns some member of E,

where model M *outruns* meaning m if and only if for no c does M depict <m,c>. This version avoids the pitfalls of THESIS 1 by prohibiting that every quantifier world is covered by only one meaning.

THESIS 3 Strong $\mathbb{M}$/E-quantifier variance
Weak $\mathbb{M}$/E-quantifier variance + every $M \in \mathbb{M}$ outruns some member of E (provided M is a proper extension of some member of $\mathbb{M}$).

THESIS 4 Unrestricted $\mathbb{M}$/E-quantifier variance
Every member of $\mathbb{M}$ depicts some unrestricted member of Q(E).

Strong quantifier variance goes further than both THESIS 1 and THESIS 2 by claiming that each quantifier world is beyond the reach of some meaning (except when the world is a "minimal" member of $\mathbb{M}$). Unrestricted quantifier variance goes even further than THESIS 3 by claiming that each quantifier world depicts some unrestricted (!) quantifier.

What is the content of these four theses? What is the range of quantifier worlds?

UPWARD CLOSURE
Any supermodel of a member of $\mathbb{M}$ is itself a member of $\mathbb{M}$.

The next definition is as follows:

DOWNWARD E-CLOSURE
If M is a supermodel of M' and M depicts some member of Q(E), then M' outruns some member of E.

Downward closure allows that there are no restrictions on how small quantifiers can get. Note that this minimization is not possible for restriction. Restriction is bounded by the empty set. (Mind you that quantifiers are explicitly defined over the empty set, which does, however, obviously not mean that such quantifiers ranging over the empty set do not have any range at all!) That is, if we are speaking one language, whose quantifier is depicted by some model,

and we choose a reduct of that model, then there is some other language we can speak in which we will not reach the chosen reduct. Moreover, reaching this reduct is independent of quantifier restriction. All the same, Sider (2007: 15) thinks that the quantifier variantist may perhaps not want downward closure. After all, some sentences could be atomic, in that no meaning treats them as false except because of quantifier restriction. Sider (2007: 15) thinks that the sentence 'There exist electrons' might be an example. Atomicity can be defined as follows:

DEFINITION 3 Sentence ϕ is E-atomic if and only if for every $m \in E$, there exists a c such that i) ϕ is true^{c}_{m}, and ii) for any c', if ϕ is not $\text{true}^{c'}_{m}$ then <m,c'> is a restriction of <m,c>.

I doubt that the quantifier variantist really wants to avoid this claim. An idealist could at last reject the sentence "There exist electrons". In ontology there are no atomic sentences in the sense that every ontologist would accept them. If there were, some of the most important ontological positions could be ruled out *a priori*.

Nonlogical expressions can undergo expansion, in extensions of models either if new non-logical expressions not interpreted in the reduct are interpreted in the extensions of the models, or if the extensions of the non-logical expressions of the reduct are enlarged in the extensions of these models. This is captured in the following definition.

DEFINITION 4 M is a (proper) <K,L>-supermodel of M' if and only if i) M is a (proper) supermodel of M', ii) any nonlogical expressions that are *newly* interpreted (i.e., interpreted by M but not by M') are in K, and iii) any nonlogical expressions that are *altered* (i.e., have different extensions in M and M') are in L.

Different versions of <K,L>-quantifier variance can be derived from this definition. If one allows, for example, models that leave the extensions of the old nonlogical expressions unaltered but introduce new nonlogical expressions for the features of "the newly introduced entities", one has one version of quantifier expansion. If one allows models that change the extensions of the old nonlogical expressions but do not introduce further predicates, one has another form of quantifier expansion. NeoCarnapians, for instance, may want the extension of different mereological predicates like "part of", "overlaps", etc. to change, but may not need any new nonlogical expressions to describe what is going on in disputes between

mereological universalists and nihilists. NeoCarnapian quantifier variance claims may therefore have the form <∅,{'part of', 'overlaps', . . . }>, where the empty set as the first member of the pair indicates that no new non-logical expressions were introduced.

In the face of all this, we could state the theory of quantifier variance, for instance, as follows:

NeoCarnapian quantifier variance There is a nonempty class of models, M, and a class of meanings, E, such that:
i) M obeys upward <∅,{'part of', 'material object' . . . }> - closure
ii) every member of E is as natural as every other, and no meaning not in E is as natural as any meaning in E
iii) strong M/E-<∅,{'part of', 'material object', . . . }>-quantifier variance is true.

With these formal remarks in mind, we can turn to the next part of the chapter. This part contains some criticisms of the theory of quantifier variance.

2.3 The Metatheoretical Commitment to Unique Quantifier Meanings

As we have seen, we can informally characterize a quantifier variantist as someone who believes that there are multiple meanings for the existential quantifier such that all these meanings are on a par. No one of these meanings is somehow better or worse than the others. All are equally natural meanings. In our strict formal definition of quantifier variance this informal idea was characterized as follows:

> NeoCarnapian quantifier variance There is a nonempty class of models, M, and a class of meanings, E, such that:
> i) M obeys upward <∅,{'part of', 'material object' . . . }> - closure
> ii) every member of E is as natural as every other, and no meaning not in E is as natural as any meaning in E
> iii) strong M /E-<∅,{'part of', 'material object', . . . }>-quantifier variance is true.

But these two characterizations pose a devastating threat for the quantifier variantist: a self-defeater is lurking. If the quantifier variantist is someone who thinks that *there are* multiple meanings for the existential quantifier, then he uses an existential quantifier in the formulation of his claim. In the formal definition he is committed to the claim *that there is* both a nonempty class of models and a class of meanings. But if he uses an existential quantifier in his claim, his claim is susceptible to his own thesis. This implies that there are numerous ways of how to interpret his thesis which does not express something substantially about the world, but only about which conventions we should accept, to govern the use of our language. Quantifier variantism becomes a triviality. But this is not what the quantifier variantist wants. He wants to make a substantial claim about ontology.
It is hard to see how the quantifier variantist can counter this objection. After all, it is he, who introduced the thesis. Maybe he could find a paraphrase which avoids the commitment to abstract entities. However, I doubt that there is such a paraphrase which leaves the language-dependence of the quantifier theorist's thesis as strong as in the above (formal) definition. The onus of proof is on the quantifier variantist's side.

2.4 The Collapse of Quantifier Variance in Ordinary Ontological Positions

As we have seen, the quantifier variantist believes that ontological debates are shallow. When two ontologists argue about whether there are Fs, they are not engaging in any genuine ontological debate. They are not arguing about what stuff reality is made up of. They are only talking past each other. In fact, they argue about which linguistic conventions we should use. Thus, their disagreement is merely verbal. They are not different from the two sailors from above arguing as to whether 1852m make up a mile or not. The two sailors are just arguing about whether one should use "mile" as synonymous with "nautical mile" or "statute mile".

What happens if the two sailors have arranged a quarrel and convened upon how to use our language? Say the two sailors have convened upon using "mile" for a distance of 1852m. In the above stated example they then agree that the distance is one mile and that they can use the bike to get to village B. With this convention in mind, they agree that it would be wrong to call a distance of 1609m a "mile".

Likewise, assume that we have answered the question about which ontological language we should use. Nominalists and Platonists, Mereological Universalists and Nihilists have all agreed upon which conventions they should use to govern the use of our ontological vocabulary. What happens if this "better stadium of the philosophical world" has arrived? According to the quantifier variantist, it will turn out, that all ontological questions can be settled trivially. If we have convened upon which language to use, we have agreed upon which language to use. Ontological sentences are still true in such a case. But this seems to mean that ontological questions can still be legitimately raised. There still exists the question as to whether there are Fs or not. So far the quantifier variantist.

However, the analogy which the quantifier variantist persues does not carry over to the ontology case. The dispute between the two sailors, as to whether 1852m constitute a mile is certainly trivial, when they have settled upon how to use the word "mile". This is due to the fact, that it is trivial for the first sailor that 1852m is a mile and trivial to the second sailor that 1852m is not a mile. After they have settled their use, they just agree on one way of understanding "mile". This doesn't lead to a change in triviality since it has been a trivial matter on both counts. But, it is not trivial, whether there are abstract entities, even when one has settled upon how to use his ontological vocabulary. After all, philosophers need hard arguments to establish the claim that there are abstract objects or that there are no abstract objects. Almost no one thinks that it is trivial whether there are abstract objects or not.[11] We don't know whether there are abstract objects or not. And we need arguments to dispel our ignorance. But this means that there is also no triviality which can carry over to the question as to whether there are abstract entities. The analogy between "mile" and ontological vocabulary has reached its limits. Thus, the quantifier variantist is clearly in need of an account which explains why ontological disputes are nevertheless trivial. The onus of proof lies on his side. Therefore, it cannot be that ontological disputes are trivial. Therefore, quantifier realism is not a genuinely skeptical metaontological position.

The theory of quantifier variance furthermore poses some urging epistemological problems. The quantifier variantist does not make any normative claim about how ontology should be done. Rather, quantifier variantism is a descriptive metatheory of ontology. The quantifier variantist claims that his thesis provides an adequate description of what happens when people engage in the philosophical business of metaontology. Still almost no ontologist thinks that

[11] Exceptions are Schaffer (2008) and Schiffer (2003).

the quantifier variantist is right and that he provides a correct description of what he does. Why is this?

The quantifier variantist holds a view according to which our ignorance about our own language is extreme. The quantifier variantist maintains that we do not know anything about the meanings of the ontological vocabulary of our languages. Given that we have to use ontological vocabulary all the time in daily life, our semantic competence is minimal.

But now consider an ontologist called *Quiner*, who starts out his philosophical career as a nominalist and ends it as a platonist. Normally one would say that the positions he defended over the course of his life are contradictory. First he defended a view according to which there are no abstract entities, then he defended a view according to which there are abstract entities. But according to the quantifier variantist, such a philosopher just changed the languages in which he speaks over the course of his life. The quantifier variantist says that he did not realize that he changed the languages in which he spoke. In fact, he thinks that he still speaks the same language. So the positions he defended over the course of his life are not contradictory. So far the quantifier theorist has no problems in explaining what is going on in such a case. But didn't Quiner also preserve his beliefs? The answer is: no. In the early stages of his career Quiner believes that there are no numbers. In the later stages of his career Quiner believes that there are numbers. This is due to Quiner's extreme ignorance about the meanings of the ontological vocabulary: After all, he doesn't know that the positions he defends are not contradictory. Nevertheless, the beliefs Quiner held over the course of his career are contradictory. This makes it very hard to see how the tight connection between our language and our thought can be preserved by the quantifier variantist. Moreover, it makes it very hard to explain, how communication should be possible. If we do change our beliefs over time, but cannot express this change in language, how can we ever confer our beliefs to someone else? Given this, I reject the theory of quantifier variance.

2.5 Conclusion

In this chapter, I first tried to motivate the theory of quantifier variance and stated it both informally and formally. The formal statement of the theory took place in an algebraic setting. The advantage of this setting was that we were not forced to specify what the *prima facie* mysterious quantifier meanings are, but could confine us to specifying their functions. This

setting formed the basis on which two new criticisms against the theory of quantifier variance were developed. The first critical point was that the theory of quantifier variance is self-defeating, as its skeptical thesis can be applied to itself. This was due to the occurrence of an existential quantifier in the formulation of the quantifier variantist's thesis. The second critical point was that the theory of quantifier variance cannot explain why ontology is a trivial business. Thus, it is not a real skeptical metaontological position. I ended by noticing that the quantifier variantist does not have a good account of how our language and our thought is connected.

3. Carnap's Skepticism

> *The first attempts to cast the ship of logic off from the* terra firma *of the classical forms were certainly bold ones, considered from the historical point of view. But they were hampered by the striving after 'Correctness.' Now, however, that impediment has been overcome, and before us lies the boundless ocean of unlimitied possibilities.*
>
> Carnap (1934), *The Logical Syntax of Language*

The subject matter of this chapter is the skeptical metaontological position developed in Carnap (1950).[12] This account is generally conceived of as the father of all the skeptical metaontological accounts in the 20th century.

3.1 Overview

The skeptical metaontological account which Carnap developed in his (1950) is the most influential skeptical metaontological account in contemporary philosophy. There is no introductory class in metaphysics which fails to discuss this seminal piece of work and no discussion of metaontological skepticism which misses to mention it.

In the following chapter I will discuss this account. I will show that the fruits of this latter opus can already be seen in its roots in earlier work; a proper understanding of the account can only be achieved by reviewing its intellectual roots in the philosophy of the logical positivists, the Vienna Circle and other writings of Carnap's from the post-Vienna period. This will be done in sections 2 – 4. Sections 5 and 6 contain a presentation of Carnap's 1950 account in general and his notion of *linguistic framework* in particular.

[12] Thanks to Hannes Leitgeb and Øystein Linnebo

The work done in sections 5 and 6 will form the basis for my rebuttal of a widespread misconception of Carnap's position in contemporary metaontology. This misconception is the tendency to assimilate Carnap's metaontological skepticism to the theory of quantifier variance which was discussed in the former chapter. The rebuttal will take place in section 7. Section 8 sums up.

3.2 The History of Carnap's Skepticism

In his (1950), Rudolf Carnap famously argued that the existential statements which philosophers are interested in (as e.g. "are there abstract objects?") are meaningless. This skepticism towards ontological matters does not come out of the blue. Rather, it can be traced back to a more comprehensive skepticism towards metaphysics in general. Carnap shared this attitude towards metaphysics with the other logical positivists of the Vienna Circle.[13] As is well known, the Vienna Circle was a group of German and Austrian philosophers who regularly met to discuss various philosophical problems in Vienna of the 1920's and early 1930's. Basically, the group included Otto Neurath, Rudolf Carnap, Hans Hahn, Moritz Schlick, Kurt Gödel (at least partly), Friedrich Waismann, Karl Menger, Philipp Frank, Gustav Bergman, and Edgar Zilsel.

The skeptical metaontological outlook of the Vienna Circle is one of the few plump pillars logical positivism is often reduced to.[14] Other pillars are the verification principle of meaning and a naïve reductionism which was allegedly defended by the members of the Vienna

[13] An exception is, of course, Gödel, who defended a radical version of platonism. To some readers, reckoning Gödel to the Circle may seem dubitable. After all, he didn't attend every meeting and didn't share many of the logical positivists' beliefs. I agree with this. There is another sense in which the reckoning is pretty uncontroversial. The Vienna Circle can be regarded as a mutual intellectual influence community. Gödel had a big influence on the other members of Circle as had the other members of the Circle a big influence on Gödel (cf. Goldfarb (2005)) . In his (1934), Carnap, for instance, thanks Gödel in the Preface for having read a draft of the book and already astonishingly exploits the Gödelian method of arithmetization in the book. (Carnap reports in his (1963b: 53): "In August 1930 he [Gödel] explained to me his new method of correlating numbers with signs and expressions. Thus a theory of the forms of expressions could be formulated with the help of the concepts of arithmetic.") It is in this latter sense when I reckon Gödel to the members of the Circle and not in the former.

[14] In my assessment of the philosophical reception and presentation I agree with Friedman (1999). One paradigmatic example of oversimplified presentation of logical positivism is for instance (Scriven 1969).

Circle.[15] However, it is important to note that the metaontological skepticism of the logical positivists was very sophisticated and complex and cannot be transferred easily to contemporary metametaphysical skepticism.

In fact, the philosophical bogeyman as which logical positivism is willingly presented in introductory classes, is seldom substituted for the serious and sophisticated philosophical position that it has in reality been. The complexity of the demands that the logical positivitst had to satisfy haven't often been recognized. Many people think what Michael Scrivens (1969: 195) once polemically wrote:

> "The Vienna Circle or *Wiener Kreis* was a band of cutthroats that went after the fat burghers of Continental metaphysics who had become intolerably inbred and pompously verbose."

Fundamentally different influences generated the tensions that logical positivism had and wanted to master. What were these influences?

First. The philosophy in Germany from 1910 – 1925 was strongly shaped by the reception, interpretation and further development of Kant's philosophy. Various different neo-Kantian schools set out to give different solutions to problems of the Kantian philosophy. The two most influential schools were the so-called *Marburg School* and the *Southwest German School*. These two schools stood in direct rivalry. Carnap, for instance, was educated by Bruno Bauch, a then famous adherent of the Marburg School. The leading question which was of interest to the logical positivists was whether there was any *a priori* knowledge.[16]

Second. The Logical Positivists were strongly influenced by the German mathematical

[15] Michael Friedman correctly dismisses the idea of reductionism: "The special sciences – more specifically, the "exact sciences" – are simply taken for granted as paradigmatic of knowledge and certainty. Far from being in a position somehow to justify these sciences from some higher vantage point, it is rather philosophy itself that is inevitably in question. Philosophy, that is, must follow the evolution of the special sciences so as to test itself and, it need be, to reorient itself with respect to the far more certain and secure results of these sciences" (Friedman 1991: 508).

[16] Cf. Friedman (2000)

tradition[17] and the German tradition in the natural sciences. Schlick, for instance, graduated under Max Planck and was befriend to Einstein. Beside these personal acquaintances there were huge influences from the revolutionary developments in the sciences. To give some examples: Einstein's theory of relativity, which made it obvious that Kant's analysis of space and time couldn't be true (Carnap tried to reconcile the Kantian theory of space and time with the consequences of Einsteinian theory in his dissertation "Der Raum" from 1920), discoveries which led to the development of quantum theory[18] and Gödel's incompleteness theorems.

Third. The positivists were strongly influenced by Russell, Frege and Wittgenstein.[19] As you certainly know, Carnap was a student of Frege. Frege's influence was especially strong in the positivist's philosophy of language. His conception of analytic truth showed the positivists a way of how to retain a radical empiricism in the spirit of Mill but at the same time avoiding the weakness of traditional empiricism in being able to give a faithful account of mathematics. (As you may remember Frege argued against the Kantian assumption that mathematics is synthetic a priori. Frege tried to show that arithmetic is independent on our spatiotemporal intuition but is built into the general conditions of thought itself.) Even in his later work (1947), Carnap retained the Fregean idea of assigning different semantic dimensions to expressions. All members were acquainted with the writings of Russell on sense data and his and Withehead's *Principia Mathematica* and tried to extend the realm of what is mathematically tractable to philosophy. Carnap, for instance, employed this device in the

[17] Friedman (1991: 510) thinks that the influence of some of German mathematics was the main impetus on the logical positivists: „The initial impetus for their [the Logical Positivists; WS] philosophizing came rather from late nineteenth-century work on the foundations of geometry by Riemann, Helmholtz, Lie, Klein, and Hilbert …"

[18] Hahn (1933: 248 – 49) has the following story: "Vor vielen Jahren, auf einem mit einem Freunde unternommenen Spaziergange im Walde, machten wir, indem wir dem Treiben in einem Ameisenhaufen zusahen, die scherzhafte Bemerkung, die Zoologie könne doch gar nicht davon sprechen, wie sich Ameisen verhalten, sie könne nur davon sprechen, wie sich Ameisen verhalten, wenn Menschen ihnen zusehen; das war ein Scherz, aber es liegt viel Ernst in diesem Scherze: jeder Vorgang wird irgendwie dadurch gestört, dass man ihn beobachtet; die Physik aber spricht vom ungestörten Vorgange – dass das nicht eine zu vernachlässingende Spitzfindigkeit, sondern von prinzipieller Bedeutung ist, wird durch die neueste Entwicklung in der Physik in klares Licht gerückt."

[19] In his (1963b) Carnap ascribes the greatest influence on his philosophizing to these three philosophers. "For me personally, Wittgenstein was perhaps the philosopher who, besides Russell and Frege, had the greatest influence on my thinking."

theory in his *Aufbau*. As you certainly know, too, there were some meetings with Wittgenstein, parts of which were protocolled by Friedrich Waismann. Wittgenstein inspired the positivists to treat the mathematical sentences as tautologies and a priori.

Fourth. There were social and political aspects. The logical positivists were Marxists (Neurath was even a minister of economics in the short-lived Bavarian *Räteregierung* under Ebert). Beside that, they felt a tight connection to the movement of the *Neue Sachlichkeit* and the *Bauhaus* in the arts. With this influence they were direct opponents to national-conservative philosophers like Martin Heidegger.[20]

A nice quote by Michael Friedman sums up this situation:

> "In the European context of the 1920's, logical positivism arose and developed as a powerful revolutionary force, deeply intertwined with the other revolutionary trends (in the sciences, in the arts, in politics, and in society) that made up what we now know as Weimar culture. The logical positivists aimed at nothing less than a total refashioning of philosophy as a whole, that would definitely end the fruitless, and endless, controversies of traditional metaphysics on behalf of a new "scientific" enterprise in which continuous and cooperative progress could be made solving fundamentally technical problems. And they took their inspiration and their models for such a radical disciplinary refashioning from the breathtaking revolutionary developments simultaneously taking place in mathematical physics and the foundations of mathematics. Although the positivists were, of course, also well aware that there were powerful opposing forces, particularly within German philosophy, working in a quite contrary direction, these developments in the sciences themselves still inspired them, in the words of Carnap's *Aufbau*, in "the faith that this [scientific-philosophical] orientation belongs to the future" (Friedman 1999: xiii).

The skeptical metaphysical outlook of the positivists has its place in this attempt to satisfy all of the above influences. Again Friedman aptly writes:

[20] Cf. Friedman (2000)

> "It is by no means surprising, therefore, that the logical positivist movement was very actively engaged with the other vocal philosophical movements of the time as well – with neo-Kantianism, with Husserlian phenomenology, even with the "existential-hermeneutical" variant of phenomenology then being initiated by Martin Heidegger. For all of these movements took it upon themselves to venture a radical reform of the German philosophy in which it would renew and reinvigorate itself in a "scientific" spirit, much as the sciences themselves had recently done" (Friedman 1999: xi – xii).

The logical positivists wanted to establish what they called scientific philosophy. Carnap, for instance, writes:

> "In our "Vienna Circle", as well as in kindred groups (in Poland, France, England, USA, and, amongst individuals, even in Germany) the conviction has grown, and is steadily increasing, that metaphysics can make no claim to possessing a scientific character. That part of the work of philosophers which may be held to be scientific in its nature – excluding the empiricial questions which can be referred to empirical science - consists of logical analysis. The aim of logical syntax is to provide a system of concepts, a language, by the help of which the results of logical analysis will be exactly formulable. *Philosophy is to be replaced by the logic of science* – that is to say, by the logical analysis of the concepts and sentences of the sciences, for *the logical of science is nothing other than the logical syntax of the language of science*" (Carnap 1934: xiii).

They wanted to do philosophy in a way which was not inferior to methods which are as precise and formal as in the sciences. Basically, they wanted to achieve this goal by the then newly developed formal devices of Frege and Russell. But still they saw themselves in a tradition of the synthetic a priori inaugurated by Kant. For the positivists metaphysics was a paradigmatic example which violated these demands.
Carnap writes:

> "Dieser Begriff [*metaphysics*; WS] wird in dieser Arbeit wie in Europa üblich als Bezeichnung für den Bereich angeblichen Wissens über das Wesen der Dinge gebraucht, der sich der empirisch begründeten induktiven Wissenschaft entzieht. Metaphysik in diesem Sinne umfasst Systeme wie die von Fichte, Schelling, Hegel, Bergson, und Heidegger, jedoch nicht Ansätze, die auf eine Synthese und Verallgemeinerung der Ergebnisse der verschiedenen Wissenschaften zielen" (Carnap 1932: 108).

So what then are the logical positivist's arguments against metaphysics?

3.3 Examples of Skeptical Metametaphysics

The main direction of impact of the logical positivists' attack on metaphysics underwent a change over the years. The epistemologically oriented criticism from the first years of the Vienna Circle turned towards the logico-linguistic one of the late years.

Moritz Schlick, for instance, argues in his (1926) that there is no legitimate conception of how information can be gained in metaphysics.[21] The suggestions are either entirely meaningless, or they redound to metaphysics collapsing into science.
In particular, Schlick argues as follows. There is a fundamental difference between phenomenal experience (*Erleben*) and knowledge (*Erkenntnis*). Knowledge can be gained by induction. Knowledge is propositional. Propositional structure is expressible by sentences. Sentences have logical forms and stand in logical relations to other sentences. So knowledge is directed at "pure forms" [22] and logical relations. Not so phenomenal experience. Phenomenal experience is neither directed at logical forms nor at logical relations but at "content".[23] It is the humanities and the arts whose first and foremost task it is to animate this experience. Animate and not express it; experience is outright inexpressible. Knowledge transcends experience.

[21] See also his (1934)
[22] „Erkenntnis ist also ihrem Wesen nach Erkenntnis von Formen, Beziehungen, und nichts anderes" (op. cit.: 176).
[23] „Erlebnis ist Inhalt, das Erkennen geht seiner Natur nach auf die reine Form" (op. cit.: 175).

If metaphysics is the science of the transcendental, any science is metaphysics. After all, by definition, what can be said is transcendental.

This inexpressibility stands also in the way of philosophers who think that metaphysics stands out of all the other sciences due to some special kind of knowledge – intuition – which is utilized in it. Intuition allows the objects of our experience to enter in our consciousness. It allows us thus, to identify the objects of our intuition with parts of our consciousness. Characterizing idealism in this way as a product of metaphysical methodology makes it vulnerable to the objection of inexpressibility. After all, the object of our special knowledge faculty intuition, which is what is experienced, cannot be told.

Rudolf Carnap develops his arguments in his (1928a) and (1928b) from his more general outlook that he developed in his (1928a). (Actually, his (1928b) is just a précis of his (1928a).) In his *Aufbau* Carnap develops a constitutional theory ("*Konstitutionstheorie*"). Carnap presents a constitution system in which all scientific concepts are defined on the basis of a few fundamental concepts. The domain of this particular logical construction is an autopsychological basis in the private sense experience of an individual. On this basis he constructs the physical world and finally even other individuals with their private sense experiences out of this domain. The constitutional system determines a scientific concept of reality for the objects. This concept is called *empirical reality*. Depending on which stage the object is formed different criteria for being empirically real have to be met. For instance, a physical object is real only if it is constituted as a class of space-time points. So if a physical object is not constituted in this way, it does not exist in the constitution system. Since science is determined by constitutional systems, it is necessary to determine the two systems. But in contrast, there is also a metaphysical concept of reality. The question whether something is real in this latter sense is independent of any constitutional system. It is extra-constitutional. Carnap then shows that some concepts are extra-constitutional.

> "Wenn zwei Geographen, ein Realist und ein Idealist, ausgeschickt werden, um die Frage zu entscheiden, ob ein an einer bestimmten Stelle in Afrika vermuteter Berg nur legendär sei oder wirklich existiere, so kommen sie beide zu dem gleichen (positiven oder negativen) Ergebnis. Denn für den Begriff der Wirklichkeit in diesem Sinn – wir wollen ihn als *„empirische Wirklichkeit"* bezeichnen – liegen in Physik und Geographie

> bestimmte Kriterien vor, die unabhängig von dem philosophischen Standpunkt des Forschers eindeutig zu einem bestimmten Ergebnis führen… Die Wahl des philosophischen Standpunktes hat also keinen inhaltlichen Einfluss auf die Naturwissenschaft… Der Gegensatz zwischen den beiden Forschern tritt erst auf, wenn sie die übereinstimmend gefundenen, empirischen Ergebnisse philosophisch interpretieren“ (Carnap (1928b: 36).

Even though Carnap shifts the emphasis of his arguments against metaphysics away from epistemological considerations to an emphasis on logical considerations, his epistemological arguments already bear the core idea of the criticisms of metaphysics and ontology in his later career.

In contrast, Hans Hahn presents a logico-linguistic argument against metaphysics. His argument runs as follows. Only observation can give us knowledge of facts. But science is full of sentences which are not and - what's worse - cannot even principally be confirmed by observation. Not only are there sentences which say something about why something cannot be confirmed by experience but science also contains a lot of universal statements. He asks,

> “Wie sollte jemals durch Beobachtung festgestellt werden, dass wirklich alle Körper sich durch Erwärmung ausdehnen? Wie sollte durch Beobachtung auch nur festgestellt werden, dass alle Amseln schwarz sind? Denn wären durch Zufall selbst alle Amseln in der Welt auf ihre Farbe beobachtet worden, so könnten wir doch nicht behaupten, alle Amseln seien schwarz, weil wir nie wissen könnten, dass die beobachteten Amseln alle Amseln sind“ (Hahn 1933: 251).

These concepts are not constituted. Constitution is the same as Carnap's sense of constitution in his (1928a).

As we saw, constitutability is not a necessary condition for an expression being meaningful. Expressions can be meaningful even if they are not constitutable. So what else is needed for an expression to be meaningful? Hahn gives an interesting answer. Every expression has a

rule which tells us how to use this expression.[24] Most notably, these rules tell us something about the inferential relations of these expressions to other expressions.[25] We are not allowed to make any inferential move with every sentence, since we are not allowed to buy any inference ticket for any sentence. If we violate these rules as for instance when we utter a sentence of the form „p∧¬p“, we have the following situation:

> “Der Mann hat *Renonce gemacht*: er hat sich vergangen an die Regeln, nach denen wir sprechen wollen, und ich werde mich weigern mit ihm zu sprechen. Es ist ganz so, wie wenn jemand bei Tarockspielen versuchen wollte, mir den Skieß mit dem Mond zu stechen; auch da werde ich gar nichts nachprüfen, ich werde meine Ansichten über das Verhalten von Gegenständen nicht abändern, sondern ich werde mich weigern, mit ihm weiter Tarock zu spielen“ (op. cit.: 239).

A vital feature of these rules is that they have to give rise to sentences, the sole constituents of which are constitutable. This requirement ensures that even though a sentence may be not constitutable, it is nevertheless connected with observation. Hahn gives us the example of a universal sentence which contains an atomic matrix like the sentence “For all x, x is green,” where “x is green” is an atomic matrix.

So, being constitutable or being governed by special rules is a necessary condition for an expression having meaning. In a nutshell, the following bi-conditional holds:
x is meaningful/has meaning if and only if x is constitutable or there is a y (and a system S) such that y is a rule (in S) which governs the behavior of x (in S) and y allows us to infer from x sentences the sole constituents of which are constitutable.
(For the truth of “x is constituable”, it is required that there is a system in which x is constituted. However, I used “system”, since this is already ensured by the modal component expressed by the suffix “-able” in “constitutable”)

[24] “Jedesmal, wenn man unkonstituierbare Terme in die Wissenschaft einführt, muss man ihnen eine Gebrauchsanweisung mitgeben, man muss Regeln angeben, wie mit ihnen zu operieren ist, wie Sätze, in denen sie vorkommen, in andere Sätze transformiert werden sollen” (op. cit.: 252).

[25] This seems to be an egregiously modern conception of the meaning of sentences. Cf. Brandom (1994).

Metaphysical sentences do not fulfil this condition. They are neither constitutable, nor do they allow the derivation of sentences which are rule-governed in the way Hahn demands.

Hahn (op. cit.: 253) has again an entertaining flowery comparison:

> "Die legitimen Sätze der Wissenschaft mit unkonstituierbaren Termen sind wohlgedecktem Papiergeld vergleichbar, das bei der Nationalbank jederzeit in Gold umgewechselt werden kann – die metaphysischen Sätze gleichen ungedecktem Papiergeld, für das niemand Gold oder Waren gibt."

Another and related example for the latter kind of criticism is contained in Carnap (1930). Carnap argues in this work on the base of the conception of the tautological character of logic which he takes over from Wittgenstein (1918). You can never gain any information through inference which is not yet included in the premises. According to Carnap, metaphysicians violate this assumption: metaphysicians want to infer from experience to something transcendental, that is to something which lies beyond any experience, as for instance, what they call "the thing of itself", "the absolute", or the "nature" and "sense". Since this cannot be done through inference, the argumentations of the metaphysicians contain unbridgeable gaps. These gaps render the introduced metaphysician's concepts irreducible to the given or the physical. But if the concept is neither reducible to the given or the physical or is not contained in the tautologies of logic and mathematics, it is empty and void of cognitive content. Thus, the metaphysician's concepts are empty and void of cognitive content.

A further criticism from philosophy of language is contained in Carnap (1932). Like Hahn, Carnap assumes that it is an important condition of an expression being meaningful as to how its relations to different sentences are. Carnap then shows that expressions used in metaphysics do not satisfy this condition.

There are also other conditions for the meaningfulness of a sentence: the sentences have to be formed according to certain rules of syntax.

Carnap says that lexems denote concepts. Lexems which do not denote concepts are meaningless. Which criteria have to be fulfilled so that a lexem denotes a concept? First the

"syntax of the word" has to be fixed. The syntax of a lexem is fixed if and only if it is fixed, how it can occur in elementary sentences ("*Elementarsätze*"). The atomic formula "x is a stone" is the elementary sentence for "Stone." Beside this purely syntactical condition, the following conditions have to be fulfilled by the elementary sentence in which the lexem can occur.

1. The empirical characteristics for the constituent expressions of the sentence in question have to be clarified.
2. It has to be fixed from which protocol sentences the sentence in question can be derived.
3. The truth-conditions of the sentence in question have to be fixed.
4. It has to be known how to verify the sentence in question.

Constitutability demands that the protocol sentences of lexems which are constitutable can be derived from the protocol sentences of the lexems by which they can be constituted.
After having stated the conditions, Carnap gives some examples of metaphysical lexems which do not fulfil these conditions. One of his examples is "God".
Carnap gives us the raw empirical thesis that there was a time when all lexems in natural language had a meaning. Today some lexems lack meaning as a result of a change in meaning. Words can undergo changes in their meanings. Words even change their reference. Just consider Gareth Evans's (1973) infamous Madagascar case. "Madagascar" first referred to a main portion of Africa but now refers to an island off the coast.
But sometimes it is forgotten that we do not fill up a sense again. Carnap says that "God" has a mythical meaning. In earlier ages it was used to refer to real-life persons, which have special abilities, live on high mountains, or are very old and wise. However, with the time people no more referred to real-life persons with flesh and blood but only to souls. In this moment "God" became without meaning.
In contrast to the syntax of a logical language, the syntax of the natural language does not, however, prohibit forming sentences which lack any meaning. Sentences like "Julius Caesar is a prime number" are perfectly fine both from a syntactic and a logical point of view. Nevertheless, they are without any cognitive content. But metaphysicians use sentences which may be perfectly fine from a syntactical, but not from a logical point of view. Sentences like "Das Nichts nichtet" are examples. Carnap argues that these sentences cannot at all be formed in a predicate-logical language. The negation is neither a term nor a predicate.

> „Die Bezugnahme auf die *Principia* hatte also für Carnap eine doppelte Funktion: Auf der negativ-kritischen Seite verfallen die philosophischen Thesen, die nicht im Rahmen der *Principia* rekonstruiert werden können, der Ablehnung, auf der anderen Seite erscheinen Differenzen, die sich in der Sprache der *Principia* nicht ausdrücken lassen, philosophisch belanglos und sind dem Belieben des Einzelnen anheimgestellt" (Mormann 2004: XX).

As was already said, the period in which these criticisms were developed was the period when the logical positivists were confronted with competing schools in the philosophy of the first three decades of the 20th century. As is well known, the logical positivism of the German speaking countries which we just discussed, found its abrupt end when the Nazi regime came to power. Almost all logical positivists emigrated into Anglo-American countries and logical positivism was finally given a different shade by Anglo-American pragmatism which it was combined with.

However, the logical positivists and especially Carnap never lost their skeptical stance toward everything metaphysical. In this connection Friedman has again a nice passage which is worth being quoted in full length:

> "But these radical intellectual ambitions [of the logical positivists; WS] which extended far beyond the boundaries of philosophy as a discipline, could not be transplanted easily onto American soil. The revolutionary context and rhetoric of radical philosophical transformation, especially in light of its explicitly Marxist overtones, had to be forgotten quickly as the erstwhile "scientific philosophers" from Central Europe were embraced by more down-to-earth and pragmatically minded thinkers such a Charles Morris, W.V. Quine, Nelson Goodman, and Ernest Nagel. In addition, the revolutions in the sciences from which the positivists had taken their philosophical inspiration had now run their course as well. Einsteinian relativity was a well-established part of the mainstream physics; the "foundation crisis" in modern mathematics had reached a denouement, of sorts, with Gödel's incompleteness theorems. Finally, and most importantly, the positivists were no longer confronted with their

> philosophical competitors and rivals within the European philosophical tradition, the competing radical reform movements of Marburg neo-Kantianism, Husserl phenomenology, and the "existential hermeneutics" of Heidegger: In the more comfortable postwar years following the global defeat of National Socialism, and in the more comfortable climate of American pragmatism and commonsense empiricism, the positivists lost much of their revolutionary fervor. No longer militantly crusading for a form of philosophy as a whole, for a new type of scientific philosophy, they instead became respectable (and domesticated) practitioners of the new subdiscipline of philosophy of science" (Friedman 1999: xiii – xiv).

3.4 "Logical Syntax" and "Testability and Meaning"

In his (1934) Carnap, presents a different logico-linguist argument against metaphysics. However, this argument bears an important similarity to the epistemological arguments of Carnap (1928a) and (1928b). In fact, it is exactly this similarity which makes up the core of Carnap's metametaphysical position. In his (1928a) and (1928b), Carnap relativizes existential sentences to constitutitional systems. This relativization is manifested by introducing a distinction between questions and sentences internal to a constitutional system and questions and sentences external to such a system. Sentences internal to a constitutional system can be answered by scientific means. Questions external to such a system cannot. Thus they are the meaningless pseudo-questions of the philosophers. In his (1934) Carnap preserves the idea of a relativization. To see how this is done, we have to take a look at the project Carnap undertakes in his (1934). Carnap wants to inaugurate a new scientific discipline in his (1934): scientific philosophy. As was already mentioned above, scientific philosophy is what is left of traditional philosophy after we have cleansed it of metaphysics and allocated the scientific questions philosophers are interested in back to the sciences. What is left is just the questions "of the logical analysis of science, of its sentences, terms, concepts, theories, etc." (Carnap 1934: 279). Scientific philosophy scrutinizes the logical syntax of the languages of science. Already on the first page of his (1934), Carnap gives a characterization of what he understands under "logical syntax":

> "By the *logical syntax* of a language, we mean the formal theory of the linguistic forms of that language – the systematic statement of the formal rules which govern it together with the development of the consequences which follow from these rules. A theory, a rule, a definition, or the like is to be called *formal* when no reference is made in it either to the meaning of the symbols (for example, the words) or to the sense of the expressions (e.g. the sentences), but simply and solely to the kinds and order of the symbols from which the expressions are constructed" (Carnap 1934: 1).

A logical syntax is a *calculus* for Carnap. However, what Carnap calls calculi are not what we understand under calculi today. What we understand under calculi are so-called proof calculi (*Beweiskalkül*). Though proof calculi come in a variety of guises (axiomatic calculi, calculi of natural deduction, tableaux calculi) they basically contain only inference rules (and possibly axioms). In contrast to this, Carnap's calculi contain not only inference rules (which he calls *transformation rules*) but also syntactical formation rules (which he calls *formation rules*) (and possibly axioms).[26]

A Carnapian calculus of a language $\mathcal{L}$ can be represented by a quadruple <A, F, T, Φ>. A is a set of logical and non-logical expressions, F is a set of syntactical rules (for terms and formulas) of $\mathcal{L}$, Φ is the set of formulas of $\mathcal{L}$ and T is a set of inference or transformation rules of $\mathcal{L}$. The syntactical rules govern how complex expressions from $\mathcal{L}$ are built up from the logical and non-logical expressions in A. Φ is the set of logical axioms. The inference rules in T tell us something about the inferential relationships of the formulas that are built up according to F. (Note that languages can be considered in basically three different ways. First, as a set of non-logical relation and function symbols. Second, as a set of formulas built up on a vocabulary made up by a set of non-logical relation and function symbols. Third, as an ordered pair of a set of non-logical relation and function symbols and a set of syntactical rules (for terms and formulas). Languages in this third sense are closed under the usual set-theories like ZFC. I started out with such a conception of language here.)

[26] Carnap's language I and II actually do contain axioms. For the sake of simplicity I leave them out. Calculi are also called "systems which contain formation and transformation rules" by Carnap.

In his (1934) Carnap shows how many philosophical disputes can be incorporated into the picture that he painted. For Carnap the only remaining philosophical questions are questions which concern the logical syntax of language. He (1934: 47) says:

> "… [Q]uestions will no longer be put in the form: "What is this or that like?" but instead we shall ask: "How do we wish to arrange this or that in the language to be constructed?" or, from a theoretical standpoint: "What consequences will ensue if we construct a language in this or that way?"

An example is the crisis in the foundations of mathematics. According to Carnap, this problem cannot be solved if it is taken at face value. There is no one correct way of how to build up secure foundations of mathematics. The only thing which can be done is a development and comparison of different language forms in which the various constraints of the different approaches to the foundational crisis can be formulated.

As Friedman once has put it:

> "Moreover, [Carnap] is once again attempting to neutralize the philosophical disputes arising in connection with these developments by showing how all parties involved are in possession of *part* of the truth; the remaining part that appears to be in dispute is then argued not to be subject to rational debate at all. More specifically, the dispute is declared to be a matter of convention in precisely Poincaré's sense: There is simply no fact of the matter concerning which party is "correct," and thus the choice between them is merely pragmatic" (Friedman 1991: 516).

Carnap's language I, which is primitive recursive arithmetic (Carnap allows only definite number properties ("*definite Zahleigenschaft*")), can be discussed as an example. This language serves constructionistic, intuitionistic, and finitistic needs. The intuitionist for instance demands that an existential sentence can only be asserted if there is a concrete witness or at least a procedure by means of which a witness can be constructed. Rejecting the law of excluded middle arises from this demand. Carnap shows a way of how the intuitionistic demands can be satisfied without rejecting the law of excluded middle by

excluding unrestricted operators from his system. In his system, unrestricted talk is achieved not via operators but via free variables like in "Fx."

So philosophy is only concerned with the kinds of expressions and the order of symbols from which the expressions are constructed of the languages of science and their meta-languages.[27] Thus only the formal mode of speaking can be adopted in philosophy. There is no need to adopt the material mode of speech. The questions which are raised inside of the logical syntax are meaningful. The questions which are raised outside of the logical syntax are meaningless philosophical pseudo-questions.

It is important to note that it is not principally impossible to speak meaningful in the material mode, or as he puts it: "The material mode of speech is not in itself erroneous" (1934: 312). If we lay down suitable rules and definitions, there are no more obstacles in meaningfully adopting the material mode of speech. In fact, this is what Carnap does in his (1947). But the material mode is dangerous since it invites us to falsely infer some meaningless pseudo-sentences about the world from it. According to Carnap, speaking in the material mode then is the main source of asserting meaningless pseudo-sentences. So it is best to avoid the material mode of speaking. If we use the material mode of speech we should translate it into the formal mode of speech. To speak like Wittgenstein: Everything that can be said can be said in a formal mode.

In his (1936) it is exactly this kind of argument which Carnap uses in his attack on the kind of metaphysics which philosophers in the US and the UK had engaged in or as he puts it "which is customary in other philosophical movements, especially in America and England" (Carnap 1936: 428). He argues that speaking in the "material mode" in contrast to the "formal mode" is the cause of all the pseudo sentences.[28] So his first arguments against the metaphysicians in the Anglo-American philosophical community are based on some considerations of his in the *Logische Syntax*.

> „Zu Abschnitt 6 „Realismus" und „Idealismus": Dass sowohl negative wie positive Thesen betreffend die Realität der Aussenwelt Scheinaussagen

[27] And maybe so on. However, Carnap uses the method of arithmetization to formulate the language and its metalanguage which Carnap is concerned with (his Language I and his Language II) in itself.

[28] Actually, Carnap uses the expressions "assertion" which denote speech acts according to today's speech acts.

> sind, habe ich in *Scheinprobleme* zu zeigen versucht. Dass entsprechendes auch für ontologische Thesen über die Wirklichkeit oder Nicht-Wirklichkeit abstrakter Entitäten wie Eigenschaften, Beziehungen oder Propositionen gilt, wird diskutiert in [Carnap 1950]“ (Carnap 1932: 109).

After all these historical and systematical investigations we are eventually in a position to consider Carnap’s skeptical metaontological position from 1950.

3.5 “Empiricism, Semantics and Ontology”

The term “ontology” was re-introduced into the philosophical discourse by Quine in the 1940s.[29] Ontology was one of Quine’s philosophical favourite children. Given that Quine and Carnap had a huge influence on each other and that Quine had also a huge influence on the philosophical community in the US at this time, Carnap gives an explicit account of why he thinks that the branch of metaphysics called “ontology” is meaningless. The result is his seminal (1950).

According to Carnap, existential statements, which are something like tokens of existential sentences, can be classified into two kinds of questions: internal and external questions. Internal questions are questions which are raised inside of a framework. External questions are raised outside of a framework. External questions are further distinguished in theoretical-external and pragmatic-external questions. Internal and theoretical-external questions are distinguished by which sense of *being real* they express. Internal questions express the scientific sense of being real. Something is real in the scientific sense if and only if it is an element of the system. So when we ask whether something is real, we want to know if we can allocate it in the system of entities in question. It is only possible to find out what is real in the scientific sense, if one has a *linguistic framework* in which one can talk about the relevant entities. Pragmatic-external questions are distinguished from theoretical-external and internal questions since they do not express any sense of being real. Pragmatic-external sentences are

[29] Actually Quine and Carnap quarrel with each other about whether it is good to re-introduce this name into the debate. Quine introduced it to establish an intuitive connection to the debates about universals in the Middle Ages. Carnap thinks that this intuitive connection is infortunate. Quine (1951: 203) answers that meaningless words, like “ontology”, are “precisely the words which I feel happiest to specify meanings for”.

just pragmatic questions about which framework or frameworks serve our purposes in a particular situation best. It is not impermissible that they may nevertheless be influenced by theoretical knowledge. Pragmatic-external and theoretical-external questions are distinguished by internal questions, since they concern linguistic frameworks as a whole. Theoretical-external questions then ask something which cannot even be expressed in the framework, namely the existence of the framework as a whole. But something like this cannot be done inside of the framework. Thus, it is no internal and thereby also no theoretical question. With this in mind, finally, we have the last distinction: Theoretical-external questions are distinguished from internal and pragmatic-external questions since they are meaningless philosophical pseudo-questions. It can be clearly seen that Carnap preserves the idea of relativization in his (1950) from his early work (1928a), (1928b) and (1934) in his skeptical metaontological arguments. Questions are relativized to frameworks. A question makes only sense if it is posed within a framework. His ontological arguments are just a corollary of his general skeptical metametaphysical argument. Given that ontology is just a subdiscipline of metaphysics this makes perfect sense. Now I want to turn to the question of what frameworks are.

3.6 Frameworks

As was already mentioned, in his (1934) Carnap outlines a program of what scientific philosophy as logic of science is. According to Carnap, this program is basically interested in a critical appraisal of calculi. Note that the calculi Carnap talks of are not to be confused with what we understand under calculi today, namely proof calculi (*Beweiskalkül*). The latter kind of calculi only involve inferential rules. Carnap's calculi, however, do also involve formation rules, that is rules which tell us how to built up complex expressions like sentences from simple expressions. These rules are hence what we would call "syntactical rules" in a proper sense today.
A system of a language contains the formation and inference rules of a language.

Speaking in the formal mode is only about the syntactical structure and logical form of expressions, but not about the meaning of sentences.
So everything which is internal to such a calculus is in the formal mode. Everything which is external to such a calculus is external to a framework.

As we saw, calculi are nothing over and above ordered quadruples like <A, F, T, Φ>, where A is the logical and non-logical vocabulary of $\mathcal{L}$, F a set of syntactical rules (for terms and formulas) of $\mathcal{L}$, Φ a set of formulas of $\mathcal{L}$ and T a set of inference or transformation rules of $\mathcal{L}$. The syntactical rules govern how complex expressions (terms or formulas) from $\mathcal{L}$ are built up from the logical and non-logical expressions in A. Φ is the set of logical axioms and possibly also meaning postulates.[30] The inference rules in T tell us something about the inferential relationships of the formulas that are built up according to F. The ordered pair <A, F> can be considered a language here.
Carnap gives the following example of such a framework.

> "As an example of a system which is of a logical rather than a factual nature let us take the system of natural numbers. The framework for this system is constructed by introducing into the language new expressions with suitable rules: (1) numerals like "five" and sentence forms like "there are five books on the table"; (2) the general term "number" for the new entities, and sentence forms like "five is a number"; (3) expressions for properties of numbers (e.g., "odd", "prime"), relations (e.g., "greater than"), and functions (e.g. "plus"), and sentence forms like "two plus three is five"; (4) numerical variables ("m), "n", etc.) and quantifiers for universal sentences ("for every n, …") and existential sentences ("there is an n such that …") with the customary deductive rules" (1950: 208).

So frameworks are very near to calculi. But now we have only the special case, where frameworks are "logical" but not "factual." That is, we only have frameworks which are analytic. Mind you that Carnap belongs to a tradition that classifies sentences according to their alethic status. The period in which this tradition had prevailed stretches from Kant's critiques of the 1780's to the twentieth-century analytic movement which ended broadly speaking in the 1950's as a result of Quine's work.[31] Sentences are either analytically or synthetically true.

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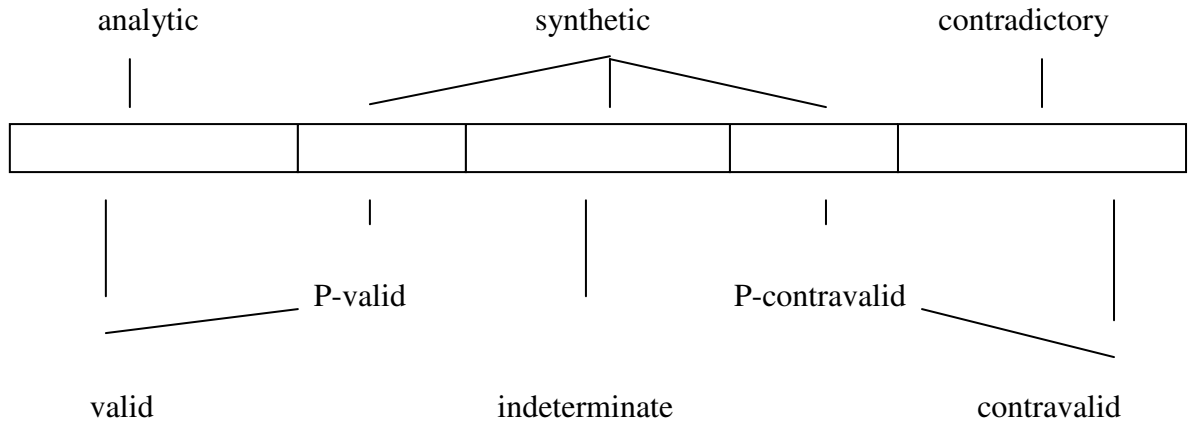
[30] cf. Carnap (1952)

[31] Of course, it was only the classification which was adopted by the logical positivists. But this does not mean that they agreed with everything that Kant did with it. In fact, the logical empiricists denied two of Kant's influential thesis. First, they denied that the possibility of knowledge requires that there be synthetic a priori

The following diagram which Carnap regularly incorporates in the multitude of writings over his whole career to illustrate the distinction between the alethic status of sentences is certainly familiar to the reader.



If a sentence S can be deduced from Φ with the rules from T alone, it is an L-consequence of Φ. If S can be shown to be true on the basis of the rules in T alone, it is L-valid. If S can be shown to be false on the same basis it is L-contravalid or contradictory. If S is neither valid nor contravalid it is indeterminate. If S is neither analytic nor contradictory, that is if its truth cannot be determined by logic alone S, is called synthetic. So, for instance:

> „‘There is an n, such that n is a number’. This statement follows from the analytic statement ‘five is a number’…” (Carnap 1950: 209).

But we also want frameworks which are not analytic, but synthetic like the framework of material things. How do we introduce such frameworks? Carnap writes the following:

> “The thing language contains words only like “red”, “hard”, “stone”, “house”, etc., which are used for describing what things are like. Now we may introduce new variables, say “f”, “g”, etc., for which those words are substitutable and furthermore the general term “property”. New rules are laid down which admit sentences like “Red is a property”, “Red is a

truths and, second, that there are such truths. According to the logical positivists the sentences of mathematics are a priori and are devoid of any information.

> color", "These two pieces of paper have at least one color in common" (i.e., "There is an f such that f is a color and …")" (1950: 211f.).

In his (1936) Carnap departs from the more radical principles of verifiability that he defended in his earlier writings. Roughly, the more radical principle holds that a statement must be (at least in principle) verifiable to be meaningful. Carnap backs out of this principle for the reasons that universal sentences cannot be meaningful and that science contains a lot of sentence which are not verifiable (Compare this with what Hahn says in the earlier quotes). However, he doesn't entirely depart from the principle of verifiability of meaning. He only weakens it. According to Carnap (1936), an existential sentence must be testable to be meaningful. Carnap also alludes to this principle in his (1950) when he writes:

> "To accept the thing world means nothing more than to accept a certain form of language, in other words, to accept rules for forming statements and for testing, accepting, or rejecting them" (1950: 208).

We can incorporate this demand by including a set C of confirmation rules into the ordered tuples, representing linguistic frameworks. Where C is a confirmation rule, "C(A,B)" says that evidence A confirms B. Note that the confirmation rules also hold for analytic sentences. But analytic sentences are trivially confirmed since they do not contain any factual content.

So we have the following suggestion:

(1) <A, F, T, Φ, C>

But this still won't be sufficient. It won't be sufficient since there are cases in which the languages are minimally different. Consider the following example which draws on a point made by Niebergall (2000: 29) in the context of reducibility of theories.

ZF is normally formulated in the first-order language $\mathcal{L}$[ZF] with '$\in$' as its sole non-logical predicate. Let the language $\mathcal{L}$[ZF] be the ordered triple <A, F> with the conventions from above. Now the set A consists of only logical vocabulary - say a set of first-order variables x, y,... and '$\perp$', '$\rightarrow$', '$\forall$' and '(' and ')' - and the sole non-logical predicate "$\in$". A ZF

framework, FR[ZF], is then an ordered pair <A, T, F, Φ, C> where Φ is a set of the axioms of ZF.[32] Assume that $\mathcal{L}$[ZFη] results from $\mathcal{L}$[ZF] by interchanging the expression "∈" through "η". Thus $\mathcal{L}$[ZFη] differs from $\mathcal{L}$[ZF] only in its non-logical vocabulary. Let A' be the set of logical and non-logical vocabulary of $\mathcal{L}$[ZF] and $\mathcal{L}$[ZF] be the ordered 4-tuple <A', F, T, C>. So we have <A', T>≠<A, T>. Let FR[ZFη] be the framework resulting from this substitution. Then it trivially holds that FR[ZF] ≠ FR[ZFη]. But this is clearly counterintuitive. FR[ZF] and FR[ZFη] should be identical.

This intuitive difference is supported by Carnap's defence of the method of *Ramsification*. Carnap (1966) once tried to give an explication of the analytic-synthetic distinction by means of Ramsey sentences.
But let me first sketch Ramsey's account. In his (1929) Ramsey suggested the sentences that bear his name as a solution to the problem of how to treat theoretical terms. Theoretical terms pose a number of difficulties. For instance, it is unclear how they get their meaning, and how we can find out the empirical meaning of theoretical terms. His ingenious solution consisted in an elimination of the theoretical terms.

Let T be a theory and let the postulates of the theory be T[F_1,…,F_n], where F_1,…,F_n are the theoretical terms of T. If the theoretical terms are replaced by variables X_1,…,X_n we get a formula which Lewis (1970b: 81) calls the *realization formula* of T. Every n-tuple of entities that satisfies this formula is a realization of T. If T is realized, we get the Ramsey-sentence of T which is as follows: ∃X_1,…,X_n[X_1,…,X_n]. Ramsey finally suggested that we can substitute this sentence for the original theory without any loss of content. Carnap exploited Ramsey's method for an explication of the notion of analyticity in a theoretical language by merging some analytical sentence A which is supposed to be an analytical postulate for the theoretical terms of a theory with a sentence S which is supposed to contain the whole observational content of the theory. According to Carnap S is the Ramsey sentence of the theory. A and S together imply T. The easiest sentence which together with S implies T is the conditional sentence S→T. Or if we adopt the way of putting things from above, we have:

[32] Don't forget the inference rules. They are important, since we can derive different sentences from the axioms if we use the inference rules from minimal or intuitionistic logic (cf. Aczel and Rathjen (2000/2001)). After all, theories are deductively closed sets.

$$\exists X_1,\ldots,X_n[X_1,\ldots,X_n] \rightarrow T[F_1,\ldots,F_n]$$

This sentence which is called the *Ramsey-Carnap sentence* is then our sentence A. According to Carnap this sentence does not contain any factual content. It just says that if there is a n-tuple of entities that satisfies the formula in the antecendens, then the theory is true. By means of this sentence, analytic and synthetic sentences in the theoretical language can be distinguished from each other pretty easily. Analytic sentences are simply the sentences which are logically implied by the Ramsey-Carnap sentence.

If we treat the elementhood relation as a theoretical term and formulate the Ramsey sentence of ZF, we get the sentence "$\exists X.ZF[X]$". If we formulate the Ramsey sentence of ZFη under the same assumption, we get the same sentence. The Ramsey sentence of ZF is the same as the Ramsey sentence of ZFη. If Φ in the ordered tuple

(2') $<A, T, F, \Phi>$

making up the framework FR[ZF] is the Ramsey sentence of the axioms of ZF, then FR[ZF] = FR[ZFη].

So (2') is not yet sufficient. But it is not difficult to improve (2'). To avoid the problems we need to take the equivalence relation of the vocabulary [A].

(2) $<[A], T, F, \Phi, C>$

3.7 Carnap and Quantifier Variance

It seems to be a widespread assumption under contemporary metaontologists that Carnap was a quantifier variantist.[33] As we saw in the last chapter, the quantifier variantist claims that there is a class C containing equally and maximally natural candidate meanings for quantifier expressions in that, firstly, no member of C is more natural than any other member of C, and, secondly, no candidate meaning for quantifier expressions that is not in C is as natural as any member of C. For each ontological theory there is a language, such that the claims of the ontological position come out true in this language. However, I don't think that this is true. In this last section of the chapter, I will rebut the arguments for this claim. The arguments for this claim mainly stem from work by Ted Sider. Consider the following passage:

> „On this view [the no-conflict view; WS], the stuff-ontologist as well as many different thing-ontologists have different 'frameworks', which employ different meaning-rules governing language use. Within any of these frameworks there are answers to questions about what there is, but any question about which framework is the right framework is 'metaphysical' in the pejorative sense of being a pseudo-question. The nihilist and I think we disagree. I affirm while the nihilist denies the sentence 'keyboards exist'. But our claims are not contradictory, for we mean different things by this sentence. The meaning of such a sentence is only determined relative to a linguistic framework, that is, a set of linguistic rules; and the nihilist and I employ different linguistic rules" (Sider 2001: xix).

I think for the following reasons that Sider's argumentation is not valid.

First. Sider claims that two parties arguing about ontology (in his example the mereological nihilist and the mereological universalist) use different frameworks. This is wrong. According to Carnap, disagreeing ontological parties do not use a framework at all. After all, philosophical and *ipso facto* ontological questions are external questions. And external questions are by definition not questions occurring within a framework. Given this, the ontologist's questions cannot even be properly formulated. Carnap writes:

[33] See for instance Sider (2004), (2006), and (2008), Eklund (2008a), and (2008b).

> "The formulation of this statement ['"Five" designates a number'; WS] presupposes that our language L contains the forms of expressions which we have called the framework of numbers, in particular, numerical variables and the general term 'number'" (1950: 217).

Second. Sider ascribes to Carnap the quantifier theorist's view that ontological dispute is not genuine. That is that ontological disputes are merely about language. But this is wrong again. If the dispute had been purely verbal, then it would have been a dispute about which framework we should use. So it would have been a pragmatic-external question. Carnap concedes that such a question makes perfect sense, but denies that it is this kind of question philosophers are interested in.

Third. The ontological pluralist says that ontological existential statements have an objective and determined truth-value, if we have agreed upon how to use the existential quantifier despite the fact that they may be trivial. But according to Carnap, ontological statements do not have any objective and determined truth-value at all. Ontological pluralism is still an ontological realist position. But Carnap's position is a variant of an ontological anti-realism in the sense that it doesn't hold that ontological sentences have any truth-value.

3.8 Conclusion

Throughout his long career Carnap was suspicious of metaphysics and ontology. The central tenets of the account developed in his (1950) can already be located in earlier work like his (1928a) and (1928b) and especially his (1934). Indeed, even though the target of his first criticism against metaphysics was not identical with the target of his 1950's attack against ontology the path of his attack has never changed. Carnap always accepted some framework as a control of existential sentences. So far I have concentrated on just giving an explicit presentation of Carnap's views for this hasn't been done in the literature so far. In the next chapter, however, I will as well give a general argument against the linguistically-motivated accounts of metaontological skepticism which thus concerns Carnap's position.

4. The Self-Defeater Argument[34]

Is there a possibility to reject metaontological skepticism *tout court*? Is there any general argument against metaontological skeptics? To show that skeptical positions are self-defeating is the best way to argue their rejection. In the following chapter I will exploit this possibility for a general argument. The result will be, that even though metaontological skepticism is not self-defeater instable *per se*, the most promising and widely adopted ways of developing it are. From a strategic point of view the argument presented in this chapter can be seen as an overall argument to rebut skeptical metaontological attempts in addition to the special arguments against the particular skeptical positions developed in the last chapters of this work.

4.1 Overview

Philosophical skepticism comes in a variety of guises (for instance skepticism about knowledge and justification, reference and the scientific value of ontology). But this variety is no more than a variation of a common theme. A theme that is made up by a bunch of interesting extrinsic and intrinsic characteristics that help to distinguish skeptical positions from other philosophical doctrines.

One characteristic is the *intellectual-value characteristic* of skepticism. For instance, epistemological skepticism is widely held to be wrong.[35,36] Epistemologist take it that

[34] Thanks to Alexander Oldemeier for helpful discussions.

[35] An exception is Stroud (1984: 1): "My aim is not to solve the problem [of skepticism] but to understand it. I believe that the problem has no solution; or rather that the only answer to the question as it is meant to be understood is that we can know nothing about the world around us". However, even Stroud gave up his sceptical stance by now.

[36] Actually, the sentence needs the qualification "today" for this hasn't always been thought. In ancient times both Academic and Phyrronian skepticism was thought to be true. In the medieval ages it was largely thought that skepticism is true. In fact, the examination of philosophical skepticism had a big impcat on the political reality in Europe.

skepticism is tantamount to *intellectual suicide*[37] and that Moorean considerations about our inclination of deliberately making knowledge ascriptions in daily life reveal the skeptic's outright craziness.[38] But this is not to deny the intellectual value or scientific significance of epistemological skepticism. The failure of skepticism it is claimed enables us to gain deeper insight into the nature of knowledge.
Similarly a failure of metaontological skepticism enables us to gain a deeper understanding into the nature of ontological questions and into what we are doing when we engage in ontology.

Another characteristic of skepticism is that skeptics are not in need of justifying their premises.[39]

But skeptical positions also suffer from a particular disease: they are always in the threat of becoming self-defeating. Crispin Wright (1991: 89) for instance invites us to imagine the "mythical glass-chinned sceptic who claims that there is no reason to believe anything at all." It is not hard to see that this position is self-defeating and "easy to confound such an opponent in debate."

Other delightful examples are brought forward by the history of philosophy. After the re-discovery of Sextus Empiricus's works in the 16th century, skeptical positions were used to attack the Catholic Church and the criterion of truth. Martin Luther, for instance, was one of the first to launch such a skeptical attack on the Church. But it didn't take a very long time until the defenders of the Church used skeptical arguments to show that the skeptics who attacked the Church were wrong.
Popkins nicely writes in his seminal monograph about the skepticism in the sixteenth century:

> "With each side trying to sap the foundations of the other, and each trying to show that the other was faced with an insoluble form of the classical skeptical problem of the criterion, each side also made claims of absolute certainty for its own views" (2003: 14).

[37] Williamson (2000)

[38] Hawthorne (2004)

[39] Cf. Wright (1991)

The question is: Does metaontological skepticism suffer from the same disease?

The subject-matter of the doubt of the metaontological skeptics is the cognitive significance of ontology. Roughly, metaontological skeptics deny that ontology is a cognitively significant discipline. Metaontological skepticism thus becomes self-defeating if and only if it is an ontological discipline by itself. In this case, it says of itself that it lacks cognitive significance. And if it lacks cognitive significance, their claims about ontology are out of place because of their lack of cognitive significance.

In the following chapter, I will give a negative answer to a general rebuttal of skeptical positions. *Per se*, metaontological skepticism is not self-defeater instable. Nevertheless I will argue that a particular form of metaontological skepticism – in fact, the most widely adopted and most defensible and plausible form – is indeed self-defeating. The chapter is structured as follows: Section 2 contains an argument for the thesis that metaontological skepticism is self-defeating. In section 3 it will be shown that the argument is defective. An insight from this rejection is exploited in Section 4 where a self-defeater argument against the most defensible formulations of metaontological skepticism is formulated and defended. Section 5 sums up.

4.2 A First Argument

First one should note that if we want to show that the metaontological skeptic falls prey to his own skepticism and construe such a dilemma, it is critical to establish a genuine problem that it be shown that the various judgements of correctness should hold under an unequivocal reading of the premises. For it might be that there is a formulation of the premise in which it is correct to say that the skeptic falls prey to its own skepticism; and it might also be that there is a reading of the premise in which it is incorrect to say that metaontological skepticism is self-defeating.

It could be argued that skeptical metaontological positions are self-defeating on the basis of the following argument:

(I)

(P1) Metaontological skepticism involves existential sentences.

(P2) Every existential sentence is an ontological sentence.

(C1) Hence, metaontological skepticism involves ontological sentences.

(P3) If metaontological skepticism involves ontological sentences, it can be applied to itself.

(P4) If metaontological skepticism can be applied to itself, it is self-defeating.

(C2) Thus, metaontological skepticism is self-defeating.

If a kind of Cartesian skeptical position can be applied to itself, of course, it becomes self-defeating. The subject matter of the scepticism of the Cartesian is after all that some proposition p holds. The form of the Cartesian's thesis is thereby ¬p. So, if the skeptic thesis of the Cartesian can be applied to itself, namely ¬p, then the skeptic has to bite the bullet and must accept ¬¬p and so p, which is the denial of his own thesis. Metaontological skepticism falls prey to such a self-defeating argument if it is an ontological thesis.

According to (P3), if metaontological skepticism involves ontological sentences, it can be applied to itself. Someone could object to this premise, that if the subject matter of metaontological skepticism is the cognitive significance of ontological sentences, that is, if metaontological skeptics deny that ontological sentences are cognitively meaningless, it does not follow that the whole skeptical position is to be rejected. It only follows that the position contains some sentences, namely the ontological sentences, which are to be rejected for the reason the skeptic says. So even though there may be some sentences of the skeptic which are without cognitive significance, it does not mean that the whole position has to be rejected.
This is true. But if one of the sentences which are to be rejected is the core thesis of the metaontological skeptic, then the whole position has to be rejected. For example it is certainly the case that if the skeptical metaontological position says p on the basis of some considerations c, and p is an existential sentence, then the skeptical position has to be rejected on the grounds of being self-defeating no matter whether any sentence in c is an existential sentence.

Here is why one can assume this. As was already mentioned, skeptical positions are commonly construed as denying some proposition p. Suppose the proposition is not existential, then this proposition can be transformed into a proposition which is existential. For instance, if someone denies that Wagner overtures are beautiful, he denies that there are

beautiful Wagner overtures. So a skeptical denial always implies some denial of the existence of something.

Note that someone who believes that the denial of a skeptic always implies a denial of the existence of something, does not have to believe that it is the other way round.
To better illustrate these vague remarks consider epistemological Cartesian skepticism. One can deny that there is knowledge but assume that knowledge ascriptions are true. Michael Williams (1996) for instance, holds a view according to which there is no knowledge (of the external world) but which grants that knowledge ascriptions like "S knows that p" are still true. He says that he agrees with the skeptic about the former, but characterizes the skeptic as someone who maintains that knowledge ascriptions are wrong and that there is no knowledge. Therefor he can maintain a non-skeptical position. I agree with Williams's characterization. So even though one claims that a Cartesian skeptic trusts that if no knowledge ascription is true, there is no such thing as knowledge. One does not need to claim that it is the other way round. Thus every kind of skeptical position crucially involves some ontological assumptions.

In fact there is a bunch of skeptical positions which contain such negative existential sentences. A good example provides *moral skepticism*. According to moral skepticism there are no objective values. Ergo it is a decidedly ontological position. In this spirit J.L. Mackie writes in his (1977: 90f.):

> "Firstly, what I have called moral scepticism is a negative doctrine, not a positive one: it say what there isn't, not what there is. It says that there do not exist entities or relations of a certain kind, objective values or requirements, which many people have believed to exist…. Secondly, what I have called moral scepticism is an ontological thesis, not a linguistic or conceptual one."[40]

Another good example provides skepticism about meaning. This kind of skepticism was defended by such distinguished writers as Wittgenstein (1953), Quine (1960), Kripke (1982), Schiffer (1987), Davidson (2001), and Gauker (2003).

[40] Note that Mackie identifies thesis and position.

Can metaontological skepticism be transformed into such a thesis? Metaontological skepticism denies that ontology is a philosophically valuable and legitimate discipline. So it holds the following thesis which I call *(MOS1)* (where "MOS" is short for "metaontological skepticism)":

> **(MOS1)** Ontology is not a philosophically valuable, relevant or legitimate discipline.

If we can transform (MOS1) into an existential sentence, we get the following sentence:

> **(MOS2)** There is no philosophically valuable, relevant or legitimate discipline of ontology.

So as it turns out, the core thesis of the metaontological skeptic can be transformed into an existential sentence. (P3) says that the core thesis of metaontological skepticism is (MOS2) Then metaontological skepticism can be applied to itself. Since then, metaontological skepticism is an ontological theory. Note that this argument does also concern the skepticism of Carnap and his fellows. Although these theorists are Pyrrhonian skeptics, they are concerned with first-order ontological questions as for instance whether abstract objects exist. They are Cartesian skeptics only when they are concerned with second-order ontological questions. Carnap and his fellows remain agnostic and withhold both assent and dissent when they are asked first-order questions, but they clearly take a dissenting stance towards questions about the philosophical discipline of ontology. In particular, they deny that ontology is a meaningful, legitimate and valuable philosophical discipline. So even though their *theoretical outlook* is Pyrrhonian, their *metatheoretical outlook* is Cartesian.
Does this show that metaontological skepticism is self-defeating?

4.3 A Revealing Response

Even though I think that the argument points into the right direction, it is not valid. The reason for this is as follows:
(P2) and (P3) lead to some kind of collapse. If every existential sentence is an ontological sentence, then every position which contains existential sentences contains ontological

sentences. And if every position which contains existential sentences is an ontological position, then almost every position is an ontological position. According to such a view, utilitarianism for instance, would be an ontological theory. Since Einstein's theory of special relativity involves existential sentences, it would be an ontological theory. But all these theories are very far away from what we would intuitively classify as ontological positions. So (P2) and (P3) render the concept of ontology empty. So the argument (I) as it stands is too simple-minded.

This leaves two possibilities to avoid these strange consequences. Reject (P2), or reject (P3). As we will see, metaontological skeptics reject (P2).
The metaontological skeptics didn't make the naïve claim that every existential sentence is an ontological sentence. According to them the class of existential statements must be further divided into a class of existential statements which are ontological and a class of existential statements which are not ontological.

Carnap's skeptical metaontological position includes a paradigm example for such a distinction. As you may remember from Chapter 3, Carnap distinguished between existential statements inside and outside of a framework. The existential statements outside of a framework are the so-called *external (existence) statements* (which are the answers to the so-called *external (existence) questions*). The existential statements inside of a framework are the so-called *internal (existence) statements* (which are the answers to the so-called *internal (existence) questions*). External statements are either pragmatic statements about which framework serves our current purposes best or meaningless statements. Internal statements are either trivial or scientific statements without any relevance for philosophy. Given that external and internal statements do not differ from their syntactic structure, that is that they are from the same type, it is better to distinguish between tokens of existential sentences. I think that Carnap's use of the word "sentence" is rather congenial to this view. So according to Carnap, what makes an existential sentence an ontological sentence is it's occurrence in a particular context. If it occurs within a framework it is internal, if it occurs outside of the framework it is external.

Consider for example, the existential sentence "There are numbers". If a statement of the form "There are numbers" occurs within a framework, this context determines that it is an internal

existential sentence and no ontological sentence. If it occurs outside of a framework, it is an external existential and ontological sentence.

In his (1963a), Carnap gives us a nice illustration of this distinction when he compares the demarcation between science and metaphysics of Popper and the Vienna Circle.

I. *Scientific statements* Boundary *A* (Popper)
II. *Pseudo-Scientific statements* Astrology, magical beliefs, myths. Boundary *B* (Vienna Circle)
III. *Pseudo-statements* Declarative sentences devoid of cognitive meaning.

In the following paragraph Carnap describes his distinction between three kinds of declarative statements (and *ipso facto* of existential sentences):

> "Let kind I comprise genuine scientific statements, i.e. those under which, in view of their form, would be regarded by scientists as of sound, scientific, empirical character, irrespective of whether the available evidence is sufficient for their acceptance or rejection. Under kind II we shall classify those statements which we might call, with Popper, "pseudo-scientific". Statements of astrology, myths, ancient magic, and popular superstitions are examples of the second kind. Such statements are comprehensible and concern empirical matters, but they cannot be taken seriously from a scientific point of view. To kind III we delegate what we called "pseudo-statements" in Vienna, i.e., declarative sentences which are devoid of cognitive meaning" (Carnap 1963a: 878).

It is taken for granted by Carnap, that science contains existential sentences. These existential sentences belong to the first kind of statements.[41] In contrast to that, the sentences of metaphysics and *ipso facto* the existential sentences of metaphysics belong to the third kind of sentences, the pseudo-statements.[42] Neither the internal nor the external statements are of any philosophical significance or value.

As will be seen from the discussion of Jonathan Schaffer's skeptical metaontological view in the next chapter, he also distinguishes between philosophically interesting existential sentences and philosophically not interesting existential sentences. Schaffer says:

> "My first answer to this third reply is that the existence questions this reply invokes are not the ones the Quinean considers. On this reply, there is still no question of whether such things as properties, meanings, and numbers exist. There is only a question of whether such beasts as 'substantial universals,' 'fundamental meanings,' and 'transcendent numbers' exist" (Schaffer 2008: 14).

[41] "… I regard sentences with any number of universal and existential quantifiers as significant" (Carnap 1963a: 881).

[42] „Popper classifies metaphysics under kind II, whereas we classify it under III… It seems to me that the books which are customarily called „metaphysical" contain statements of both kinds II and III. To us, those of kind III seem to be especially characteristic of metaphysics; therefore, in a generalizing way, we often called all statements of the kind III "metaphysical", even when they belonged not to the field usually called "metaphysics", but to epistemology or to the philosophy of science" (Carnap 1963a: 879).

Schaffer also distinguishes between classical philosophical, ontologically interesting existential statements (which are the answers to classical existence questions) and existence questions which are philosophically not interesting (which are the answer to existence questions which are philosophically not interesting). He calls the latter kind of existential statements "beasts".[43] So he clearly accepts a distinction between the two kinds of existential sentences. The questions can be divided into philosophically relevant and philosophically non-relevant questions.

Given that the most skeptics deny premise (P2) it cannot be shown that metaontological skepticism is inherent instable. The reason for why this couldn't have been shown was a distinction between existential sentences that is made by metaontological skeptics.

4.4 A Better Argument

So, metaontological skeptics commonly assume that not all existential sentences are ontological sentences. Metaontological skepticism is not concerned with both kinds of existential sentences. It is concerned with the philosophically interesting, ontological existential statements only. But according to the skeptic, all existential sentences lack philosophical relevance and value. So we have the thesis:

(MOS3*) All ontological sentences lack philosophical relevance and value.

But if we translate this sentence into an equivalent sentence we get a contradictory sentence:

(MOS3) There is no ontological sentence of philosophical value and relevance.

(MOS3) itself is an ontological sentence of high philosophical relevance and value. After all, (MOS3) is what we are after in the debate we are leading with the metaontological skeptic. If the thesis is true, we have to concede the victory to the metaontological skeptic. If it is false

[43] Schaffer does not say outright that the beasts are not philosophically interesting. But from his general outlook it becomes clear that he thinks so. He thinks that all legitimate existence questions can be answered affirmatively.

metaontological skeptics have to concede the victory to the adherents of the thesis of a philosophically legitimate and valuable philosophical discipline of ontology.
But (MOS3) constitutes a grave problem for the metaontological skeptic. It makes his position self-contradictory. If (MOS3) is false, then metaontological skepticism is false. After all, it is the core thesis of the metaontological skeptic. But even if one denies MOS3, it is implied by metaontological skepticism under the weakest readings. If MOS3 is true, then there is at least one existential sentence of philosophical relevance. But then MOS3 is false, since it denies exactly this. So, MOS3 is false. Since metaontological skepticism implies this claim, it is false.

How can the skeptic answer this argument? As you certainly see, he could reject the distinction between the two kinds of existential sentences. But in this case, all the advantages for drawing such a distinction would no longer be available to him. The distinction between internal and external questions would have to be given up. That makes him vulnerable to much simpler self-defeater arguments.

But given that this was almost the only assumption which leads to this argument, the metaontological skeptic does have to bite the bullet. Of course he can argue that one is not allowed to make the step from (MOS3*) to (MOS3) since the first is not contradictory in contrast to the second. If (MOS3*) is false, metaontological skeptics would have to retreat. If it is true, then one can not make the self-defeating inference, since it is not an existential sentence itself. So it is not concerned with its own content. That the sentences are not equivalent is shown by the fact that the first kind of sentence is self-contradictory, while the second sentence is not.

This is a point. But if the skeptic argues like that, he has to adopt a new logic and must be much more precise about it. So I don't think that there is any reason to accept this move by the skeptic.

4.5 Conclusion

I argued that metaontological skepticism is not tenable. This argument was based on the observation that skeptical positions involve negative existence claims and qualify thereby as ontological theories.

5. Schaffer's Stroke[44]

What's in a name? that which we call a rose
By any other name would smell as sweet;
So Romeo would, were he not Romeo call'd,
Retain that dear perfection which he owes
Without that title.
Shakespeare, *Romeo and Juliet*

Nenn es dann, wie du willst,
Nenn's Glück! Herz! Liebe! Gott!
Ich habe keinen Namen
Dafür! Gefühl ist alles;
Name ist Schall und Rauch, Umnebelnd Himmelsglut.
Goethe, *Faust I*

Jonathan Schaffer has recently made the radical metaontologically sceptical claim that metaphysics *without* ontology is as informative as traditional metaphysics *with* ontology. This chapter will criticize Schaffer's argument for this claim. His argument runs as follows. The thesis that (almost) everything exists, which Schaffer calls *permissivism*, holds only for some special kind of existence question. The traditional existence questions (as for instance "Are there numbers?") are of this special kind. So permissivism holds for the traditional existence claims. But it does so if and only if ontology does not add any informative content to metaphysics. So, ontology does not add any informative content to metaphysics. In this chapter I show that Schaffer's argument fails. In particular, I argue that he is wrong in assuming, first, that not all existence questions of this special kind are trivial and, second, that all traditional existence questions belong to this special kind.

[44] Thanks to Anthony Everett, Thomas Hofweber, Godehard Link, Østein Linnebo, Carlos Ulisses Moulines, Karl-Georg Niebergall and Alexander Oldemeier for helpful discussions and comments on earlier drafts of this chapter.

5.1 Overview

Ontology hasn't always enjoyed as great popularity as it now does. Rather, scepticism towards ontology was widespread until the middle of the 1950's. This sort of scepticism seeks to undermine the legitimacy of ontology as a philosophical discipline by denying the cognitive significance[45] of ontological statements. Undermining the legitimacy of ontology by means of such a semantical claim is possible. For according to a reasonable metaphilosophical assumption, a philosophical discipline can not be valuable and thereby not legitimate if it does not contain any cognitive significant statements. One well-known example of such metaontological scepticism is logical positivism. The logical positivists claimed that the statements of metaphysics and *ipso facto* the statements of ontology (as for example "There are properties", and "There are numbers") are not verifiable.[46] If a statement φ is not verifiable it lacks cognitive content. So the statements of ontology lack cognitive content and are without cognitive significance. Valuable philosophical disciplines should contain cognitively significant sentences. But since ontology doesn't contain any cognitively significant sentences, it is not a valuable philosophical discipline.

Nowadays, logical positivism is widely regarded as dead and buried. Metaontological scepticism, however, is, though just asleep, still as fit as a fiddle and as threatening as ever. Jonathan Schaffer, for instance, aims a sceptical but non-positivistic stroke against ontology in his recently developed metatheory of metaphysics.[47] In this picture ontology contains only trivial sentences. But if a sentence φ is trivially true or trivially false, it is without cognitive significance. So, ontology contains only cognitive insignificant sentences. From this it follows (what I will henceforth call the *Triviality Claim* (TC)), that nothing informative and cognitively significant is added to metaphysics by ontology. We simply don't change the substantial and informative content of metaphysics if we omit ontology from it. Ontology is on this view not a valuable philosophical discipline. If Schaffer's metaontological position is

[45] An expression α is not cognitive significant if and only if α is uninformative, meaningless, or nonsensical (see Hempel (1950)). This includes a Fregean as well as a stricter logical positivist's understanding of cognitive significance. On the Fregean understanding it is being informative (see Frege (1892a)). On the stricter logical positivist's understanding it is being meaningless or nonsensical.

[46] Of course, „There are numbers" is understood as what the late Carnap would call an "external-theoretical" statement. The verifiability requirement, however, does not hold for so-called „internal statements". An internal statement of „There are numbers" is for example trivially verifiable in the „natural number framework".

[47] See his (2003) and (2008).

right, a new form of metaontological scepticism would have been established and much work done in the post-logical-positivist metaphysics rendered moot.[48]

This chapter will criticise Schaffer's defence of his metaontological position.
Section 2 presents Schaffer's general metaontological position and his argument for TC. Section 3 presents a problem with this argument and Schaffer's proposed solution to it. Sections 4 and 5 contain two arguments against Schaffer's solution to this problem. Section 6 sums up.

[48] Since Schaffer is pretty hard to interpret, Thomas Hofweber suggested that I should say something more explicit about why I interpreted Schaffer's position in the above way. Ok, I did a lot of this Lewisian (1990) "translation" thing. So, I agree that it is a terminological dispute. But how could one avoid having to translate Schaffer's eulogies into his own words, when he writes things like:
"On the now dominant Quinean view, metaphysics is about what there is. Metaphysics so conceived is concerned with such questions as whether properties exist, whether meanings exist, and whether numbers exist."
If I say, "Arithmetic is about the natural numbers.", I want to say that arithmetic is about the natural numbers and only about the natural numbers, and not for instance about the rational or real numbers. I want to say this since I have the expression (in German) "unter anderem" to make the narrower claim. But if Schaffer intended such a reading he has an unreasonable narrow conception of metaphysics, leaving out issues like causation and persistence. It is not really fair to presume that such a conception of metaphysics is the now dominant view. Furthermore, I doubt that it is historically correct and that Quine thought something like that. In fact, Quine was careful to make the "distinction" between metaphysics and ontology (he actually reintroduced it) and the logical positivists were so, as well, as soon as they were aware of it, as is testified by Carnap's famous "Nachwort" to his "Überwindung der Metaphysik". So does he want to talk about ontology when he uses the word "metaphysics"? To be honest, I cannot see any good reason to depart so far from our ordinary language use. So with a little Gricean reasoning and the maxim of quantity one is almost forced to adopt my interpretation.

5.2 Permissivism and Two Kinds of Existence Questions

5.2.1 Permissivism and Grounding

Schaffer thinks that ontology embodies only trivial sentences. This is due to the triviality of the answers of existence questions. For instance, consider the question about whether there are numbers. The affirmative answer can be trivially established by the following argument:

A

(P1) There are prime numbers between 10 and 20.

(C1) So there are numbers.

Almost every existence question can be *affirmatively* answered with this kind of argument.[49] From this it follows not only that ontology contains only trivial sentences. It also follows that almost everything exists. This thesis is called *permissivism* by Schaffer.[50]

So instead of worrying about what exists, metaphysicians should rather turn to the much more interesting and fundamental question of what grounds on what. This "Aristotelian conception of metaphysics"[51] asks questions like "Which entities are the fundamental entities that ground all other entities?", or "Which ones are derivative and grounded on other entities?".

Consider the following two diagrams (Fig. 1 and Fig. 2).

[49] Those arguments have been called "Cheap Arguments". See Carnap (1950), Schiffer (1996), Yablo (1998), Schiffer (2003), Szabó (2003), Hofweber (2005) and Hofweber (2007). Schaffer's arguments differ from the arguments normally given only insofar as he uses "adjective drop inferences". Those inferences peculiarly exploit the upward monotonicity of the existential quantifier (see Barwise and Copper (1981), Westerståhl (1989) and Peters and Westerståhl (2006)). Thereby he can use his subversive two line arguments.

[50] One may be inclined to think that permissivism can be established trivially as well. However, it is far from clear that this is legitimate. This step is possibly based on some kind of S4-principle for triviality. According to this principle the following inference is valid:

$T\varphi \Rightarrow TT\varphi$, where "$\varphi$" ranges over sentences and "T" represents the sentence operator "it is trivial". This principle is wrong. Consider mathematics: It is wrong that everything which is trivial is trivially trivial. Sometimes it can be highly non-trivial to show that something is trivial. For more on this issue see the Appendix where a "logic of triviality" is sketched. Thanks to Øystein Linnebo for helpful discussion.

[51] Schaffer (2008: 1)

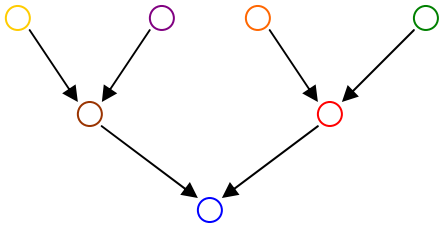


Fig. 1: A's Ontology

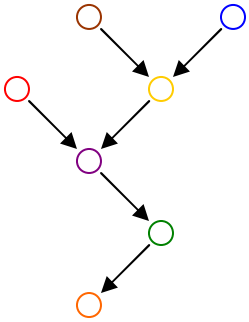


Fig. 2: B's Ontology

The diagrams represent the structure which two Aristotelian metaphysicians, "A" and "B", ascribe to reality. The blue, brown, red, yellow, purple, orange and green circles represent the entities in A's and B's ontologies. A and B do not disagree over their ontologies. A's and B's conceptions of the structure of reality differ in the grounding relations, which they ascribe to the entities in their ontologies.[52] In the diagram, these grounding relations are represented by the arrows.[53] In A's ontology for instance, the entity represented by the red circle is grounded on the entity represented by the blue circle. But it grounds the entities represented by the orange and the green circles. In B's ontology the entity represented by the red circle is grounded on the entity represented by the purple circle while it grounds no entity. In both ontologies the entity represented by the red circle is grounded on some other entity. That means that it is a "derivative" entity.[54] If an entity is not derivative it is fundamental. So the only fundamental entity in A's ontology is the entity represented by the blue circle.

[52] Such that every permutation of the circles in one diagram already yields a different conception.

[53] This is implicit in Schaffer's conception: „Grounding is then irreflexive, asymmetric, and transitive. It thus induces a partial ordering over the entities (*the great chain of being*), with minimal elements (what is fundamental, the substances, the foundation post for the great chain of being). Formally this may be modeled by a directed acylic graph, for which every path has a starting point" (Schaffer 2008: 22). (Note that Schaffer is sloppy here: It neither *follows* that the induced partial ordering has minimal elements, nor that it is directed. This job could be done by asking for well-foundedness. This may be a bit too strong though.)

[54] Schaffer (2008: 20)

5.2.2 Schaffer's Diagnosis

The Schafferian claims about the triviality of existence assertions are obviously a result of his assessment of Cheap Arguments. This assessment consists in claiming that Cheap Arguments are perfectly sound and establish permissivism straightforwardly. However, such an assessment of Cheap Arguments is highly controversial. There are many worries about those arguments. I want to pick out a particular one, which is contained in the question: Why do able philosophers still argue about ontological matters if they are resolved that easily?

Schaffer's answer to this objection is simple: Able philosophers do not argue about ontological matters. They argue about grounding. Consider for instance, the debate about whether numbers exist. According to Schaffer, no one doubts that there are numbers. Rather, the debate is about "how"[55] numbers exist. A defender of a particular version of intuitionism for example will only deny that numbers are mind-independent while a realist will deny that they are mind-dependent. According to Schaffer the basic question is "How does reality make true the claim that there are numbers?" While our intuitionist thinks that they are true thanks to an arrangement of various ideas in human minds, the realist says that they are true thanks to some mind-independent entity.[56]

The central and hotly debated metaphysical questions are not ontological questions. The central and hotly debated metaphysical questions are grounding questions. Someone who claims the opposite misses the point. But Schaffer's response to this objection faces a serious worry.

5.3 The Problem

It is certainly true that if an intuitionist argues that numbers are mind-dependent and a realist argues that numbers are not mind-dependent, they agree that there are numbers. But could the intuitionist still not deny that there are mind-independent numbers? And could the realist still not deny that there are mind-dependent numbers? Wouldn't this mean that we have some substantial ontological dispute? Could we not reformulate the grounding debate between the intuitionist and the realist as an ontological debate about whether there are mind-dependent numbers and mind-independent numbers? If so, the debate about those existence questions

[55] Schaffer (2008: 5)

[56] Compare Turner (2008a), (2008b), and (2008c)

would be non-trivial, for we are just paraphrasing the non-trivial grounding debate with existence question vocabulary.

Schaffer answers this worry by drawing a distinction between two kinds of existence question. First, there are existence questions which do not contain any grounding information in the description of the candidate entity. Second, there are existence questions which do. He calls the first kind of question "existence questions *per se*". According to Schaffer, the questions over whether there are mind-dependent and independent numbers are not *per se*. They contain information about what the numbers - that is the candidate entities - are grounded on. In the intuitionist's case for example the mind, and in the realist's case, not the mind. In contrast to that, the question over whether there are numbers is an existence question *per se*. Schaffer agrees that non-existence questions *per se* do not have trivial answers and that permissivism does not concern them:

> "I certainly do not mean to suggest that every candidate entity should count as an existent. For instance, if a candidate entity is described in such a way as to entail grounding information (e.g. "a Platonic number", understood as a transcendent substance)… one need not be permissive" (Schaffer 2008: 10).[57]

But he claims that traditional existence questions are existence questions *per se*.[58] Existence questions *per se* do have trivial answers and are concerned by permissivism. So traditional existence questions do have trivial answers and are in accordance with permissivism.

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[57] This leaves us with the question, what it means to say that permissivism holds that (almost) all entities exist. There are certainly *a lot* of entities which can only be described by means of grounding information. But permissivism does not hold for such objects. So, permissivism does not hold that (almost) all entities exist. Does this constitute evidence against the thesis that permissivism is some kind of *deflationary maximalism* (see Eklund (2006) and Eklund (forthcoming))? Roughly, maximalism says all possible objects exist (see Hawley (2007), Sider (2007) and Hale/Wright (2008)). A certain kind of maximalism gets deflationary if it comes out trivially true in some philosophical view (see Eklund 2006: 326). But does it come out true in permissivism? I would be inclined to say "no", since permissivism doesn't say anything about the entities only describable by grounding information. Permissivism is probably more aptly described as a form of Lewisian "allism", which doesn't collapse into meinongianism.

[58] "My first answer to this reply is that the existence questions this reply invokes are not the ones the Quinean considers" (Schaffer 2008: 14).

In the next section, I argue that there is at least one traditional existence question, which is *per se* but does not have a trivial answer.

5.4 A Non-trivial Classical Existence Question *Per Se*

Schaffer's permissivism trivializes the answer to every traditional ontological question. It is this generality that justifies him in undermining the fruitfulness of ontology. Obviously he looses this justification when he looses generality. That is, if it can be found that there is an existence question, which has a non-trivial answer. I give such an example in what follows.

Consider the question "Are there abstract objects?". Undoubtedly, a paradigm example of a classical ontological question. Moreover, it is a question which was brought to fame by Quine[59] and hence a question, which every Quinean considers.[60] Beside this it does not contain any grounding information. It leaves entirely open whether and how abstract objects are grounded in other objects.

The Schafferian permissivist has to claim that the answers to this question are trivial. As we saw above, the permissivist establishes his triviality claims by means of cheap arguments. That is, an answer to a classical existence question is trivial if and only if there is a cheap argument for this conclusion. So, if there is no such cheap argument, he is forced to admit that there is a classical existence question without any trivial answer. So, the Schafferian permissivist has to claim that there is a cheap argument establishing that the answer to the question "Are there abstract objects?" is trivial. So, is there any such argument? Consider the following two candidates:

B

(P2) There are prime numbers between 10 and 20.

(C2) There are numbers.

(C3) There are abstract objects.

[59] Before Quine it was the universal/particular distinction which was of interest to ontologists. See for instance Russell (1911), (1912), and (1940).

[60] This is important since Schaffer dismisses some counterexamples to his thesis as questions which no Quinean considers.

C

(P3) There are properties that Bob and Carol and Ted and Alice share.

(C4) There are properties.

(C5) There are abstract objects.

It seems that those arguments establish that there are abstract objects, as trivially as they establish the claim that there are numbers and properties, respectively.[61] But this is not true. Even though there may well be nominalists who agree that there are numbers, there are hardly any nominalists who agree that numbers are abstract objects. A nominalist is almost by definition someone who denies that there are abstract objects.[62] It may be irrational to deny that primes are numbers if one understands the sentence "Primes are numbers". Perhaps as irrational as to deny that bachelors are married, if one understands the sentence "Bachelors are married". But it is certainly not irrational to deny that numbers are abstract as it is certainly not irrational to take a sceptical or outright eliminativist stance towards any of these putative abstract entities, even if it is in the end incorrect to do so. Moreover, the strength of the cheap arguments derives from their being entirely devoid of any philosophical baggage. For example, it seems that *arithmetic* shows that there are numbers. This licenses the inference from the premise to the first conclusion. But the step from the first conclusion to the second conclusion does not possess this kind of philosophical neutrality. *Arithmetic* certainly does not show that there are abstract entities since it can be interpreted in such a way that no commitment to abstract objects is made.[63]

This shows that cheap arguments are applicable to the smaller special domains, as for instance the domains of numbers, properties and propositions. But they are not applicable to the greater, sortal domain of abstract objects. And they cannot be extended to the greater, sortal domain, either. Thus, it cannot be established by means of cheap arguments that the existence question *per se* "Are there abstract objects?" has a trivial answer. So we have an example of an existence question *per se* which is not trivial. Maybe it will turn out that there is another

[61] This conclusion is among others also very naively accepted in Künne (2006).

[62] At least in the sense which is of interest here. Sometimes the nominalist is almost defined as someone who is denying the existence of universals. A nominalist only accepts particulars, whereas a platonist accepts both universals and particulars.

[63] See Lewis (1991).

way of establishing the triviality of the answer to the question "Are there abstract objects?" but the route via cheap arguments is blocked.[64]

[64] There is a further worry with the arguments A, B and C. Schaffer states them such that they beg the question. What is to be shown, namely that there are numbers and properties, is already assumed in the premises. So they are not valid. Schiffer (1996) and (2003) and Hofweber (2005) and (2007) are more careful. They state the argument without begging the question like Hofweber (2005: 258):

(P1*) Fido is a dog.
(C1*) Thus, Fido has the property of being a dog.
(C2*) Thus, there is a property that Fido has, namely being a dog.
(C3*) Thus, there are properties, among them being a dog.

But there is a possibility in which one could make sense of Schaffer's arguments in particular but also of cheap arguments in general by transforming them into different arguments. Such arguments could be based on the truth of the following type of premise:
(P1**) There are contexts in which the truth of "There are prime numbers between 10 and 20" is beyond any serious doubt.
To see that this premise is true consider the following situation: When I try to solve an exercise sheet in a mathematics course, I am heavily relying on the truth of the claim that there are primes between 10 and 20. Indeed, it is of great practical relevance for me that the sentence is true. So (P1**) is true. But there are also contexts as for example when I am in an ontology seminar, where the truth of (P1**) is questioned. (There is a certain similarity to the varying truth-value of a knowledge ascription K in different contexts. But should this mean that "existence" is an indexical? What does this mean? Does this mean that the quantifier is restricted to different sets in different contexts? This would sound very much like the theory of quantifier variance. But then it is hard to see how we could avoid platonism. After all, the quantifier variantist allows that the sentence "There exist properties" is true if the quantifier ranges over certain domains. And what is character supposed to be?)

5.5 Traditional Existence Questions ≠ Existence Questions *Per Se*

Here is a truism: If one wants to know what was asked with a question like "Are there Fs?" one has to know what "F" means.[65] A simple result of a compositionality constraint on questions.[66,67] According to this constraint the meaning of a complex expression depends on the meaning of its parts, its structure and on nothing else. For instance, if one wants to know what was asked with the question "Are there socks in the fridge?" one has to know what "socks" means. If one changes the meaning of "socks" one changes the meaning of the whole question. Questions contain propositions.[68] Yes-no questions can be represented as ordered triples (E, P(X), D), where X, a set of finitely many propositional variables occurring in P(X), and thus D, the domain, is all empty and E stands for the yes-no interrogative attitude, and P stands for a propositional content presented in that attitude. Questions of other interrogative types can be understood along the same lines. Yes-no problems are thus a special case of questions of other interrogative types.[69]

Insofar this situation is analogous to the one which Andrew Arana (2008) is concerned with in his attempt to specify the nature of a pure proof. Arana cites the following example. We want to know whether $\sqrt{2}$ is a rational number (or in slightly other words whether there is a rational

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[65] "What was asked" is analogous to "what was said" in the case of non-interrogative sentences. So I am concerned with semantics. Note that I am not saying anything about the pragmatic content of questions here.

[66] Here it is assumed that questions are bearers of propositional content. But this seems to be beyond any reasonable doubt. The compositionality constraint results from the assumption that productivity holds for questions. We are principally able to understand a large collection of questions which we have never encountered before.

[67] Note, however, that it is not entirely clear of how to treat questions type-logically. This unclarity stems from the fact that the normal interpretation of the interrogative pronoun "does" in polar questions like "Does Anna read?" is not available for existence questions like "Do Fs exist?". Normally, "does" is interpreted as follows:

[[does]] = $\lambda x.\lambda X.Xx$: $s_y/(np\backslash s)/np$, (where s_y could stand for a distinct syntactical category for polar questions).

But this definition requires that "exists" has to be treated as a predicate (with type $<e, t>$). That's not what philosophers commonly want to do (even though the cross-linguistic data do not point into this direction unequivocally). Philosophers want to treat "existence" as a quantifier. This suggests the following treatment:

[[does]] = $\lambda X.\exists xXx$: $s_y/(np\backslash s)$.

However, this would mean that we would have to raise the type of predicates. So if we want to give a unified semantical treatment of the interrogative pronoun "does", we are in the middle of a dilemma. Either one treats "existence" as a predicate or one raises the type of predicates to the type of quantifiers. Both options are unsatisfying.

[68] See Searle (1969)

[69] Cf. Arana (2008: 12) for this treatment of questions.

number a such that $a^2 = 2$). We answer this question by giving a *reductio*-proof to the conclusion that there is no such rational number a.[70] This proof looks as follows:

1. Suppose that $a \in \Theta$ and that $a^2 = 2$.
2. Then, there exist $p, q \in Z$ $(q \neq 0)$ such that (a) $(p/q)^2=2$ and (b) p and q are coprime.
3. Then, $p^2=2$
4. Then, there is a $z \in Z$ such that $z = 2p$
5. Then, $4z^2 = p^2 = 2q^2$
6. Then, $2z^2 = q^2$
7. q is an even number, which contradicts assumption (b) in line 2.
8. Thus, there is no rational number a such that $a^2=2$. Q.E.D.

The crucial point here is that if one rejects one of the steps in the proof, one changes the problem or the question. Arana wonders why this is so. There is an easy answer. If one rejects one of the steps in the proof, one denies that the notions in this proof mean what we ordinarily understand under these notions. For example, one can deny that "rational number" and "squaring" means what we normally think. Since squaring is just an iteration of the multiplication function and the root function is just the inverse of the quadratic function, the meaning of the root function crucially depends on the meaning of the multiplication sign. Given the compositionality principle one denies that the meaning of $\sqrt{2}$ is as we commonly think. And thus one changes the proposition contained in the question and thereby the question. Why does the question change? Simply because we have changed the meaning of expressions on which the meaning of expressions occurring as constituents in the question depend. But contrary to Arana, I don't think that the old problem disappeard. After all we could introduce a function, call it *schmultiplication*, which means exactly what was understood under multiplication before the change in meaning occurred.

Now we face two cases. An *epistemological-descriptivist* and a *metaphysical* case. The cases differ in the metaphysical stability of the existence question's content. In the epistemological/descriptivist case the content of the existence question is basically unstable. It depends crucially on what a philosopher understands under "Fs" in "Are there Fs?" or better on what descriptions he associates with "Fs". In the metaphysical case the content is stable. It

[70] Actually, Arana uses "problem". But I think given what Arana says, it is more appropriate to talk of questions. This was conceded by Arana in p.c.

doesn't depend on what descriptions a speaker associates with "Fs". The two cases are in important ways different. But in spite of their importance they have never been noticed. I will begin by laying out the epistemological-descriptivist case.

5.5.1 The Epistemological-Descriptivist Case

This case is based on the fact that a question is a certain kind of speech act. If we ask a question, we have a certain intention which is expressed by the question. So if we ask a question of the form "Are there Fs?" we have a particular thing in mind, of which we want to know whether it exists. According to this option, if a philosopher asks the question "Are there numbers?", he has a particular thing in mind. For example a certain kind of set, from which he wants to know whether it exists. The philosopher somehow stipulates what numbers are by specifying this entity via some definite descriptions. Seen this way, he says: "Numbers are those entities, which have such-and-such properties". If he asks the existence question, he asks whether there is an entity which fulfils those descriptions. *This situation is widespread in metaphysics, since we often don't know what the entities - as for instance numbers, propositions, or what have you - of which we want to know whether they exist, are.* If we have a different conception of what numbers are, we will not report what he asks, by saying, "He wants to know whether there are numbers", but by saying, "He wants to know whether there is something which fulfils those descriptions". Anything to the contrary would mean to confuse use and mention.
Thus writes David Lewis:

> "Sometimes it is wrong to take a philosopher at his word, when he tells us what he believes to exist. For if we differ with the philosopher at some point of semantics, then we must make allowance for that difference if we want to report his position in our words, in indirect quotation. Example, if someone seemingly tells us that God exists, and then goes on to tell us that 'God' denotes a fictional character, and if we ourselves think that this fictional character is far from deserving the name he gives it, then we should count him an atheist. We may report that he says the words 'God exists', but we would be wrong to say that he says *that* God exists. He believes in something that he thinks deserves the name 'God'. But if we are right and he is wrong about what

> it takes to deserve the name, then he does not believe in anything that would in fact deserve that name, and we would be wrong to otherwise" (1990: 23f.).

Lewis stresses that one's answer to questions about the nature of an entity (which can be different from someone else's answer) influences one's answer to existence questions. For instance, if someone thinks that all the Fs are all the Gs and he knows that there are Gs, he will affirmatively answer the question whether there are Fs. If the Fs are Ks and someone knows that there are no Ks, then the answer to the concerned question is "no". So one's answer to the question "Are there Fs?" depends crucially on what one takes the Fs to be. Likewise Lewis writes in his (1979: 134):

> "The general agreement that the objects of the attitudes are propositions is to some extent phony. Not everyone means the same thing by the word "proposition.""

A paradigmatic example of this first case in metaphysics is when we try to answer the traditional existence question "Is there a God?". This question is normally understood as already containing a built-in answer to the question of what "God" means, an answer to the question of what it is to be God. If we ask "Is there a God?", we normally ask whether a particular entity exists. Or to speak more explicitly in a Fregean manner: *we ask whether something falls under a particular concept (of God) - a concept which involves for instance, properties and powers like being almighty or omniscient.* Schaffer relies upon this very fact in his defence of combining atheism and permissivism:

> "While I obviously cannot speak to every contemporary existence debate here, perhaps it will suffice to speak to one other debate that may stand in as paradigm of metaphysical existence questions, namely the question of whether God exists. I think even this is a trivial *yes* (and I am an atheist). *The atheistic view is that God is a fictional character*. The atheist need not be committed to the view that there are no fictional characters! (To put this point another way, if the theism debate were about the existence of God, then the following would count as a defense of theism: (i) God is a fictional character, and (ii) fictional characters exist, hence (ii) God exists. But obviously that is no

defense of theism! Hence the theism debate is *not* about existence)" (Schaffer 2008: 10).

When Schaffer defends atheism he defends the thesis that there is nothing which falls under the above mentioned culturally coined concept of God. For him, God is a fictional character and fictional characters simply lack the powers and properties which we ordinarily ascribe to God. (Isn't it odd to say that a fictional character takes corrective action in our world?). Since Schaffer's God does not have the relevant properties and powers as our traditional God, Schaffer's God is different from the traditional God. And it is exactly the exploitation of this prior understanding of our concept of God, on which Schaffer bases his argument. (*Thereby*, he is not committed to the view that fictional characters do not exist.)

To sharpen this point: define the concept A-GOD as a conjunction of concepts like BEING A SUBSTANCE, BEING OMNISCIENT, BEING ALLMIGHTY, etc. Define the concept B-GOD as the concept A PARTICULAR FICTIONAL CHARACTER SUCH THAT IT IS GROUNDED ON etc. In the debate between Schaffer and the theist it was discussed whether something falls under one of those concepts. The original question "Is there a God?" comes down to two questions in this debate:

(1) Is there an A-God?

(2) Is there a B-God?

As was said, Schaffer's atheism consists in giving a negative answer to the first question. The base for this assertion forms the following claim: A-God≠God=B-God. Furthermore he claims that there is a B-God. So, he does not reject the following thesis:

Atheism$_1$: There is no God at all.

He just rejects the following thesis:

Atheism$_2$: There exists an A-God.

In contrast to that, Schaffer's opponent, call him "Mustache Freddie", claims that B-God≠God=A-God and that there is no such things as a B-God. Mustache Freddie defends **Atheism$_1$** and **Atheism$_2$**.

According to Schaffer such questions as (1) and (2) are not *per se* for they contain grounding information. But it is this information which is necessary to determine what it is to be an A-God or a B-God.[71] In our examples A-God and B-God were results of ways in which people

[71] To determine what it is to be an A-God or a B-God it is necessary to determine the category of the concerned entities. Grounding is tightly connected to being in a category. Schaffer describes this as follows: "Thus a sorting structure presupposes a prior dependence ordering over the entities. Categories are places in the ordering

understand what it is to be God. So it seems that the question "Is there a God?" cannot be understood in any other way than as some kind of grounding question. If people argue about whether there is a God, they argue about the existence of entities which can only be described by means of grounding information.

Schaffer could respond to this by pointing out that both people still agree that there is a God. They just disagree about whether this God is an A-God or a B-God. They disagree about whether God is grounded like Mustache Freddie's God or like Schaffer's God.

This reply doesn't work.

For Schaffer it holds that God=B-God; for Mustache Freddie it holds that God=A-God. A-God and B-God are grounded on different entities. By Leibniz's Law it follows that A-God≠B-God. Then somewhere down the road it turns out - very much to the regrets of Mustache Freddie - that Schaffer is right: God=B-God. But then it follows that God≠A-God. We can see that Mustache Freddie and Schaffer did not agree about ontology. Mustache Freddie did not believe that there is a God (since he did not believe that there is B-God). So, existence questions *per se* do not work in this first case. Since we use grounding information when specifying what is meant by "God".

Another example. Take the question over whether there are numbers. In this case, *prima facie* it seems that Schaffer's thesis is *straightforwardly* false: there are nominalists who deny that there are any numbers at all.[72] But let's give the necessary credit to Schaffer. Suppose that every nominalist does accept that there are numbers. Such a nominalist holds that numbers are concrete inscriptions like strokes on a piece of paper or a board.[73] This nominalist would claim that numbers are grounded on concrete particles in the physical world. He only denies that there are the numbers which the realist or the intuitionist believes to exist. Numbers which are abstract objects like certain sets[74] and which are grounded mind-independently or –dependently (see Sec. 2.2). Call such numbers B-numbers and C-numbers, respectively. So, according to Schaffer, the nominalist rejects the following thesis:

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structure. Substance itself serves as both root node and focal category" (Schaffer 2008: 7). Being in a certain category has impacts on the essences of entities. Thus grounding information is necessary to say what it is to be an A-God or a B-God what is the essence of A-Gods and B-Gods. Quite generally, is it hard to say what grounding is. Personally, I think that it is best understood as a form of ontological reduction. Thus grounding debates are debates about ontological reduction. It seems that Schaffer gave us old wine in new bottles.

[72] Maybe this is why Schaffer originally illustrated his view on this question by means of the intuitionist and the realist.

[73] See Goodman and Leonard (1940), Goodman and Quine (1947), Goodman (1950)

[74] For instance B numbers could be von Neumann numbers.

Number$_1$: There are no numbers at all.

But only the following one:

Number$_2$: There are B-numbers or C-numbers.

The nominalist thinks that numbers are C-numbers and denies that numbers are A- and B-numbers. In contrast to that the realist thinks that numbers are B-numbers and denies that numbers are A- and C-numbers. And, finally, the intuitionist thinks that numbers are A-numbers and denies that numbers are B- and C-numbers. A-numbers, B-numbers are each grounded on different entities. By Leibniz's Law it follows that A-numbers≠B-numbers≠C-numbers≠A-numbers. Then somewhere down the road it turns out - very much to the regrets of the nominalist and the intuitionist – that the realist is right: numbers=B-numbers. But then it follows that numbers≠A-numbers and that numbers≠C-numbers. We can see that the nominalist, the realist and the intuitionist disagree about ontology. The nominalist did not believe that there are numbers (since he did not believe that there are B-numbers).

5.5.2 The Metaphysical Case

In the metaphysical case the content of the question remains stable. In this case, "Fs" in "Are there Fs?" is not interpreted in such a way that the meaning of "Fs" is determined by what we take the Fs to be. In contrast in the epistemological case, if we utter "Are there numbers?" and think that numbers are A-numbers, we could substitute "numbers" in "Are there numbers?" by "A-numbers". As I said, this is often done in metaphysics, since we don't know what numbers are. But Kripke (1972) and Putnam (1973), (1975) have taught us that there is another case in which we want to have the option to ask, "Are there Fs?", without any preconception of what the Fs are. If this option wasn't available to us, we would have severe problems with the continuity of mathematics and incommensurability of mathematical discourse over the centuries.

The problem of incommensurability was first introduced by Kuhn in his seminal (1962). Kuhn was concerned with the development of science and the change of scientific theories. According to him, science does not proceed continuously by additions of new truths to the stock of already established truths or the increasing approximation to truth but rather as a

sequence of alternating phases. These phases are called *normal*, and *revolutionary* and they differ fundamentally and qualitatively from each other. While normal science phases are phases of puzzle solving and accumulating information in the established - what he called - *paradigm*, revolutionary phases are not cumulative but involve revisions of already existing scientific beliefs, methods, and practices. Since Kuhn does not employ a unique use of the notion of paradigm, it is hard to say what it is, but as a thorough characterization, one could understand the notion of paradigm as a conglomerate of such disparate things as theories, methods, instruments, values and metaphysical assumptions that are shared among the members of a community of puzzle solvers. Revolutionary phases are caused by accumulations of anomalies in the particular paradigm employed in the normal phase in question. In such revolutionary phases different paradigms are competing with each other. Eventually one of the different paradigms takes hold and forms the base for a new phase of normal science. When we assess the quality of a theory, according to Kuhn, we do not apply any rules of method to the theory and the evidence, but compare it to the competing paradigms. The standards of assessment are for Kuhn not theory independent rules but highly theory-dependent criteria like relations of similarity which vary with the change of the scientific theory. For example, when Newton's account of gravitation, involving action at a distance with no underlying explanation, had become accepted and the paradigm relative to which latter theories were judged, the lack of an underlying mechanism for a fundamental force was regarded to be in favour of Coulomb's law of electrostatic attraction. But before Newton's theory had become the paradigm, it was considered a weakness by many in the seventeenth century when compared to Ptolemy's explanation of the motion of the planets in terms of contiguous crystalline spheres or to Descartes' explanation in terms of vortices. Given that the standards of evaluation are themselves subject to change, Kuhn concluded that the theories do not share a common measure. This phenomenon was called *incommensurability* by Kuhn. In his writings three kinds of incommensurability can be distinguished, methodological, perceptual/observational and semantic incommensurability. I am going to be concerned with semantic incommensurability only. According to this type of incommensurability, comparison of scientific theories is aggravated by the fact that the meanings of key terms in the languages in which the theories are formulated have changed as a consequence of a scientific revolution. So the key terms of theories from different periods of normal science may not be substitutable. For instance, Kuhn says:

> "… the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name.

> (Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.)" (1962: 102).

So if Newton used the expression "mass" we must not assume that he meant the same thing as Einstein, when he used this expression. By the same pattern one could argue that, for instance, when the Pythagoreans used the numeral **5** they referred to a different entity than Fermat in the 17th century and we today. And when Fermat used **5**, he referred to a different entity than we. After all, neither the Pythagoreans nor Fermat had a precise idea about the Dedekind-Peano axioms. Given this, their theories must have been different from our contemporary theories. But the meanings of numerals like **1** or **5** are at least partially determined by the Dedekind-Peano axioms in that they exclude which entities do not count as natural numbers as, for instance, real or complex numbers. As a result the meanings of mathematical expressions like numerals changed their meaning over time. Furthermore, neither the Pythagoreans nor Fermat knew anything about modern axiomatic set-theory and *ipso facto* didn't they have the slightest idea of the reducibility of arithmetic to set-theory. McGee (1997: 40) therefore writes:

> "When Fermat used the numeral **7**, there wasn't anything he did, said, or thought that fixed any particular set as the referent of his symbol; set theory hadn't even been invented yet when Fermat wrote. What Fermat referred to, assuming his numeral had a determinate referent, wasn't a set, whereas what we refer to, after we have carried out the reduction, is a set. There is some part of reality -namely, the referents of Fermat's numerals- that our theory either denies or ignores. If so, ontological reduction is surely a bad bargain. To retreat to a theory that denies or ignores mathematical entities whose mathematical properties were already well understood in the seventeenth century is too steep a price to pay for mere notational convenience."

According to such a Kuhnian view, mathematics is not continuous. We need not assume that contemporary mathematics is concerned with the same subject matter like the mathematics in the past centuries even though this seems to be the case. A mathematician who is writing about number theory today is not concerned with the same subject matter as a mathematician who wrote about number theory 200 years ago even though it seemed that they are both interested in natural numbers. And what mathematicians in the past found out about natural

numbers is not at all commensurable with what mathematicians find out today. This view is mad. Referential continuity has to be preserved. A widespread suggestion, of how this continuity can be preserved is by claiming that numerals are rigid designators.
This establishes that numerals and expressions referring to sets refer to the same numbers and sets, respectively, in all possible worlds in which they exist. If the Dedekind-Peano axioms are true, numerals refer to the same entities in all possible worlds, since the Dedekind-Peano axioms are necessarily true. Sets are rigid designators since in every possible world the following transworld identity condition holds:

$$x \in \{x\} \rightarrow \quad (x \in \{x\})$$

Every directly referential expression is a rigid designator: its associated semantic rules determine the actual referent of the expression (in a context) and when evaluating what is said by the sentence containing the expression (in that context) in other possible circumstances, this same referent is always relevant. To illustrate, if I utter

(3) 7 is odd

at the present time and we want to evaluate whether what I said by means of that utterance is true or false in other possible circumstances, it is the properties of 7 in those other circumstances that are relevant. Thus, '7' is rigid.

It is a question of considerable debate whether general terms are rigid designators.[75] I think that an argument can be made that at least some general terms are rigid. One such general term is the bare plural noun phrase "numbers", another is "sets". The argument is simple. It is commonly taken to be the case that numerals are rigid designators[76]. But the word "numbers" just collects these expressions. Thus, "numbers" is itself rigid. An analogous argument is available for "sets" being a rigid designator.

So my argument runs as follows:
(P1) Compositionality holds for questions of the form, "Are there Fs?".

[75] See Soames (2002)

[76] However, see the next chapter of this work!

(P2) The meaning of "F" is determined by what it is to be an F, by its essence.[77]

(P3) F's essence is determined by the category in which F falls.

(P4) Category information is grounding information.

(C1) So, if a and b belong to different categories, they are grounded on different entities.

(P5) So if people say that Fs are grounded on different entities, they ascribe different essential properties to those entities.

(P6) If a differs from b in its essential properties a is not identical to b.

(P7) So, people ask different questions.

(C2) So, even though everybody gives an affirmative answer to the question "Are there Fs?", not everybody claims that Fs exist.

(C3) So, people didn't agree about ontology.

Grounding information is indispensable to say what it is to be an F, what the essence of the Fs is. So it is impossible to know what was asked with a question "Are there Fs?" without using grounding information. In this sense there are no such questions as existence questions *per se*. It is nonsense to assume that one can answer an existence question without relying on grounding information. We simply do not know what we are talking about when we do not have the relevant information. If we want to know what we are talking about when we say "Fs exist", we have to know what "Fs" means. And if we want to know what "Fs" means, we have to fall back on grounding information. So, if we want to know what we are talking about when we say "Fs exist", we have to fall back on grounding information. If we don't have the grounding information we don't know this.

A consequence of this view is that grounding relations tell us something about the nature of an entity. Let me illustrate this with an example.

It does not come out true that the diagram in Fig. 3 yields a different conception of the metaphysical structure of the world than the diagram in Fig. 1. I do not think that *every* permutation of the circles yields a distinct conception of the metaphysical structure of the world.[78]

[77] See Fine (1994)

[78] Even though I think that some do.

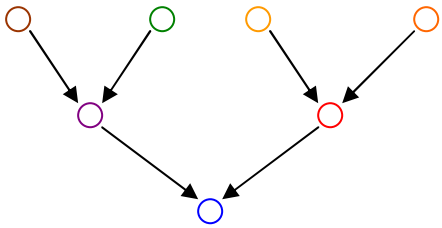

Fig. 3

Compare Fig. 1 with Fig. 3. On Schaffer's view the metaphysical conception of reality in Fig.3 is entirely different from the one sketched in Fig. 1. The difference accrues from some circles (as e.g. the violet ones) taking over different places in the structures. According to Schaffer's view, it is sufficient for the metaphysical conceptions to differ from each other if the same circles take over different places in the same structure. But according to my view, the conceptions of reality are identical. This is due to the fact that the structure alone has the potential to determine the nature of entities. As a corollary it is true that, if the same circle (as e.g. the violet one) takes up different places in the same structure, the entities represented by this circle in the two structures cannot be identical. The entity represented by the violet circle in Fig. 3 is identical with the entity represented by the brown circle in Fig. 1. As you probably may have noticed, I am then retaining some almost self-evident assumption that give rise to highly non-self-evident and non-trivial structuralistic accounts of mathematics. Structuralistic accounts are widespread in mathematics and philosophy of science. These accounts assign to some entities a crucial role in determining the nature of some other entities. According to structuralistic accounts of mathematics what individuates entities is their place in a particular structure and the relations it has to other objects, etc. Along those lines, the number 4 is nothing more than its place in the ω-sequence. Stewart Shapiro has a beautiful statement of this view in his (1997: 72):

> "The Platonist view may be that one can state the *essence* of each number without referring to the other numbers. The essence of 2 does not invoke 6 for any other number (except perhaps 0 and 1). If this notion of independence could be made out, we structuralists would reject it. The essence of a natural number is its *relations* to other natural numbers. The subject matter of arithmetic is a single abstract structure, the pattern common to any infinite collection of objects that has a successor relation with a unique initial object and satisfies the (second-order) induction principle. The number 2, for example, is no more and no less than the second-order position in the natural

> umber structure; 6 is the sixth position. Neither of them has any independence from the structure in which they are positions, and as places in this structure, neither number is independent of the other. The essence of 2 is to be the successor of the successor of 0, the predecessor of 3, the first prime, and so on."

With this in mind, I want to look at Schaffer's two replies to the problem sketched above (Sect. 3). Schaffer's first reply is twofold. First, he says the following:

> "On this reply, there is still no question of whether such things as properties, meanings, and numbers exist. There is only a question of whether such beasts as 'substantial universals,' 'fundamental meanings,' and 'transcendent numbers' exist" (Schaffer 2008: 14).

It is deeply misguided to think that it is possible to speak about entities *per se*. Entities like properties, meanings and numbers simply are such beasts as 'substantial universals' and 'transcendent numbers'. The concepts which pick out those entities always contain built-in grounding information.
Schaffer's second reply is as follows:

> „My second answer is that metaphysics is still not about existence questions *per se*. The most this second reply can show is that metaphysics can be framed as concerning *existence questions of a specific sort*, namely *those that pack grounding information into the description of the entity in question*. To answer such questions one still needs to determine what grounds what" (ibd.).

Right, ontology is not about existence questions *per se. Ontology is about non-existence questions* per se. An existence question *per se* does not make any sense. It is as if we were asking a question like "Are there miffies?" where "miffies" doesn't mean anything. Right, to answer those questions one needs to determine what grounds what. Grounding determines (at least partially) what it is to be an F. This is necessary to be able to answer the question of what it is to be an F. But on this count ontology is about interesting and non-trivial non-existence questions *per se*. Grounding questions may be prior to existence questions, in that

questions like “What are Fs?” have to be answered before questions like “Are there Fs?” can be answered. However this does not mean that existence questions are trivial.

Traditional existence questions contain grounding information. But this means by definition that they are not *per se*. But existence questions which contain grounding information are non-trivial and interesting. So the traditional existence questions are non-trivial and interesting. So it seems that ontology does not only consist of trivialities. TC is false: Metaphysics with ontology is richer in content than metaphysics without ontology. Schaffer’s metaontological scepticism is not successful. Ontology makes a difference.

5.6 Conclusion

Rarely do new metatheories of metaphysics involve arguments for metaontological scepticism. Jonathan Schaffer has attempted to provide just such an argument. We saw that this argument fails. The result of our investigation was that traditional existence questions are not *per se* but contain grounding information. Schaffer’s crusade against ontology failed. It was not a worthless enterprise though. He has reminded us that people give different answers to questions like “What is it to be a number?”, or “What is it to be a property?” and that these answers influence answers to questions like “Are there numbers?”, or “Are there properties?” in crucial ways. Metaphysical and ontological debates are strongly knit together. Philosophers were fully aware of this when they were interested in ontological reduction. But somewhere down the road, philosophers forgot ontological reduction. And with it they forgot that we cannot answer ontological questions without having made progress in metaphysics. The boundaries of our progress in ontology are settled by the progress we make in metaphysics. We shouldn’t forget this again.

6. On Combining Platonism with the Theory of Direct Reference

In the last chapter I used the assumption that we don't know what numbers are. For those of you acquainted with Frege's famous Julius Caesar problem and Benacerraf's concerns about Ernie and Johnny and the reduction of arithmetic to set-theory this isn't particularly great news. But as you may remember, I argued that our ignorance is not only to be deplored for its own sake, but also because it creates an obstacle to any serious progress in ontology. Answers to ontological questions depend more on answers to questions about the nature of entities than is commonly assumed. In this chapter I want to make another case for the importance of answers to ontological questions for the nature of entities. If the answers to ontological questions are important for many other philosophical questions and disciplines, this casts further doubt on the metaontological skeptic's thesis that ontology is illegitimate and valueless. I try to show that the fact that our ignorance about what numbers are leads to a tough problem for our standard theory of reference, the theory of direct reference (henceforth: **TDR**).

6.1 Overview

Mathematical Platonism is the doctrine that mathematical statements are either determinately true or determinately false, that our mathematical axioms are principally right, that mathematical objects really exist, and that they are abstract. A realistic understanding of analysis has it that when the sentences of analysis are understood in their standard meaning, each sentence has a determinate truth-value such that there is a fact of the matter whether for instance, every convergent sequence is a Cauchy-sequence.

TDR is a doctrine about the contribution of certain kinds of lexemes to the proposition expressed by a sentence that contains such a lexeme as a constituent. The lexemes TDR is concerned with, semantically function as singular terms and are thus capable of referring. If an expression is directly referential it *directly* (without mediation of something like for instance, a Fregean *Sinn*) contributes its referent and *nothing else* over and above its referent

(like for instance a Fregean *Sinn*) to the proposition expressed by a sentence which contains this expression as a constituent in singular term position. Obviously, this semantical theory is concerned with the meanings of singular terms. Since numerals are commonly treated as singular terms that refer to numbers, numerals *directly* contribute numbers and *nothing over and above* numbers to mathematical propositions expressed by mathematical sentences. Along these lines the constituent numerals **1** and **2** of a mathematical sentence like "1+1=2" contribute the numbers 1 and 2 themselves to the proposition expressed by this sentence.
Platonism and the theory of direct reference are plagued by numerous philosophical problems some of which are quite penetrating. But here I do not want to defend the theory of direct reference or Platonism. Rather I presume it and discuss a problem which arises within a combination of these two theories.

The internal problem is this: No one, and especially not the platonist, knows what numbers are. We only know that they are the referents of our numerals. But this is not sufficient to ensure an understanding of mathematical statements. For such an understanding presupposes knowledge of the referents. Stating the problem more precisely gives us the following argument:

(P1) The meaning of a singular term is its referent.
(C1) Hence, to know the meaning of a singular term is to know its referent. (From (P1))
(P2) Numerals are singular terms. (Standard View)
(C2) Hence, to know the meaning of a numeral is to know its referent. (From (P2))
(P3) The referents of numerals are numbers. (Standard View)
(C3) Hence, to know the meanings of numerals is to know (what) numbers (are). (From (C2) and (P3))
(P4) We don't know (what) numbers (are). (Argument in the previous chapter)
(C4) Hence, we don't know the meanings of numerals. (From (C2) and (P4))
(P5) To understand an expression is to know its meaning. (Alleged Truism)
(C5) We don't understand numerals. (From (C4) and (P5))
(P6) To understand a complex expression is to know its meaning and its structure (and nothing else). (Compositionality)
(C6) Hence, we don't understand complex expressions which contain numbers as constituents. (From (P6) and (C5)).

(P7) But we do understand complex expression, whose constituents contain numbers. (Fact)
(C7) So, something went wrong.

This problem is internal to the mentioned combination. It won't arise if you combine nominalism[79] with the theory of direct reference or if you combine Platonism with a non-direct-reference theory. But, for the combiner, it is a particularly urgent problem, since as long as the alleged process of how we can understand mathematical sentences and terms remains clouded in mystery, the realist can reasonably be accused of dabbling in the occult. The problem here is not the traditional epistemic problem: "how do we know that our mathematical beliefs are true?" It is a question that arises earlier: How do we even have mathematical beliefs? For to believe an axiom, we have to understand what proposition the axiom expresses because the proposition is the object of our belief. But we simply cannot answer the question how we understand the proposition the axiom expresses.

The chapter is organized as follows. It is divided into three main parts. The first part which consists of section 2 contains a more detailed presentation of TDR and the structural propositions accounts in which it is normally shaped. This presentation serves to illustrate and establish premise (P1) in the above argument. The second part which contains sections 3, 4 and 5 is concerned with a defence of some of the crucial premises in the above argument. In particular: Section 3 defends (P5). Section 4 contains a defence of compositionality and thus (P6). Section 5 discusses (P2). The third part, which subsumes section 6 encompasses my argument from some related problems of Paul Benacerraf's, Harold Hodes's and Van McGee's and explains where and how it differs from these arguments. Section 7 sums up.

6.2 Direct Reference, Structured Propositions and Rigid Designation

TDR yields a simple answer to the following question:

(A) What is the semantic contribution of a singular term to the proposition expressed by the sentence that contains this singular term as a constituent?

[79] One could also think about adding intuitionism to this list. However, as Godehard Link has pointed out to me, it is far from clear what this boils down to.

The simple answer is: The entity the singular term refers to. In the late 1960's this simple answer constituted an attack to the then dominant approach to the semantics[80] of proper names championed by Gottlob Frege, and worked out further by Rudolf Carnap and Alonzo Church. This attack of the then young and eager philosophers Keith Donnellan, Saul Kripke, Hilary Putnam and David Kaplan was so successful that it is not exaggerated to call this theory today's *paradigm of reference*. In fact the theory is so deeply entrenched in our current thinking about reference, that one makes himself intellectually suspicious when one is rejecting it. From a sciento-sociological point of view a remarkable situation for and in philosophy.

Frege (1892a) famously postulated two different semantic dimensions of meaning with natural language expressions: *Sinn* and *Bedeutung*. The *Bedeutung* of an expression, that is what I will henceforth call its *reference*, is the object the expression refers to; the *Sinn* of an expression, that is what I will henceforth call its *meaning*, is a mode of presentation of the referent of the expression. For instance, a sentence like "Gödel walks" expresses the sense or, as Frege would have said the *Thought*, that Gödel walks and refers to a truth value, the True or the False, depending on whether Gödel walks or not. The referent of the sentence is computed by the 1-place function, or in Frege's terminology the *concept*, WALKS which takes the individual Gödel as its argument and maps it onto the True or the False depending on whether Gödel walks or not.[81] The thought that Gödel walks is build up from the senses of the constituents "Gödel" and "walks" of the sentence "Gödel walks". Then only items which play a role in the determination of the senses of complex expressions are themselves senses of constituent expressions of this complex expression. And the items which play a role in the determination of the reference of complex expressions are themselves referents of constituent expressions of this complex expression. There is then a clear separation between the two semantic dimensions. No items from the sense dimension of meaning play a role in the determination of reference of a complex expression and no items from the reference dimension of meaning play a role in the determination of sense of a complex expression.[82]
The DR theorist grants that the Fregean distinction between two differing kinds of semantical dimensions of meaning (namely what is commonly called its *intension* and its *extension*) is

[80] Salmon (1986)

[81] Frege endorses a principle of compositionality on the level of reference. This principle is stated in more thorough form below.

[82] At least in indirect contexts, i.e. what Frege calls *gerades Vorkommnis eines Ausdrucks*.

true of many kinds of expression, but denies that proper names are of any such kind. Rather his claim is that the meaning of a term is exhausted by its referent.[83] According to this view a proper name lacks sense, but has only a referent. In the case of proper names, meaning and reference collapse, the extension and the intension of proper names are identical.

So the answer of TDR to our question (A), "What is the semantic contribution of a singular term to the proposition expressed by a sentence that contains this singular term as a constituent?" is the object the singular term refers to.

According to TDR, singular terms (or to speak more syntactically proper noun lexems) like proper names, numerals, demonstrative pronouns, and indexicals are, what David Kaplan (1989) called, *devices of direct reference*. As was already mentioned, this means that proper noun lexems contribute the individual to the proposition expressed and nothing else. But what lies behind this metaphorical talk of individuals being contributed to propositions as constituents? This picture is painted on the grounds of a special metaphysical account of propositions: *structured propositions*. [84] Without adopting structured propositions the metaphor of a singular term contributing an individual to the proposition expressed by a sentence is running out of steam. [85] Almost everyone working on the metaphysics of propositions defends such a structuralistic account – among them Frege (1892a), (1892b), Russell (1903), Lewis (1970a), Fine (1977), Cresswell (1985), Kaplan (1989), Crimmins (1992), Bealer (1982), Larson and Ludlow (1993), Soames (1985), (1987), Salmon (1986), Zalta (1983), (1988), Menzel (1986), King (1995), (1996), (2007) and in a certain sense (with his notion of intentional isomorphism) Carnap (1947).[86] Kaplan nicely illustrates this idea as follows:

> "If I may wax metaphysical in order to fix an image, let us think of the vehicles of evaluation -- the what-is-said in a given context -- as propositions. Don't think of propositions as sets of possible worlds, but

[83] There are attempts to extend the theory of direct reference to all terms.

[84] I draw on King's excellent (2001).

[85] See Kaplan (1989)

[86] The big competitor to this account is the possible worlds account of propositions which one can find for example in Stalnaker (1984) and Lewis (1979), (1986), and (1990). However it is not necessarily inconsistent to hold a view which combines propositions and properties. Indeed most defenders of a structured propositions account combine a possible worlds account with a structuralistic account. See for instance Lewis (1970a), (Fine 1977), and Kaplan (1989).

> rather as structured entities looking something like the sentences which express them. For each occurrence of a singular term in a sentence there will be a corresponding constituent in the proposition expressed. The constituent of the proposition determines, for each circumstance of evaluation, the object relevant to evaluating the proposition in that circumstance. In general, the constituent of the proposition will be some sort of logical complex, constructed from various attributes by logical composition. But in the case of a singular term which is directly referential, the constituent of the proposition is just the object itself. Thus it is that it does not just *turn out* that the constituent determines the same object in every circumstance, the constituent (corresponding to a rigid designator) just *is* the object. *There is no determining to do at all*" (Kaplan 1989: 494f.).

The claim that propositions are *structured* is a claim about the nature of propositions. Roughly, propositions are conceived as complex entities, which have parts or constituents that are bound together in a peculiar way. Different accounts of structured propositions can (and in fact do) differ in at least two manners: 1) they can differ as to what kinds of thing are the constituents of structured propositions; and 2) they can differ as to what functions as metaphysical "glue" of these constituents and binds them together in a proposition. The view that propositions are structured is purely a metaphysical thesis about what propositions are and what they are like. First and foremost it entails nothing about the relation between a sentence and the proposition it expresses. But it is needless to say that defenders of a view of structured propositions do have something to say about the relation between a sentence and the proposition it expresses.

Intuitively, given that a sentence expresses a structured proposition, the proposition will have parts or constituents that are the semantic values of words or subsentential complex linguistic expressions occurring in the sentence; and the proposition will have a structure similar to the structure of the sentence. For example, suppose that the semantic value of a transitive verb is a relation and that the semantic value of a name is its referent, then a structured proposition theorist will claim that the sentence

(1) Gödel loves Adele

expresses a proposition consisting of Gödel, the loving relation and Adele. Those constituents are bound together in some peculiar way into a unity. Letting '*g*' stand for Gödel, '*a*' for

Adele and '*L*' for the loving relation, we can *represent* the proposition in question as the following ordered pair:

(1a) <*g*,<*L*,<*a*>>>[87]

Thus (1a)'s structure is very close to the syntactical structure of (1); and (1a) has as constituents the semantic values of the words occurring in (1). Indeed, in the case of (1) and (1a) all and only semantic values of words in the sentence are constituents of the proposition. But a given account of structured propositions may not believe that this is the case in general for any one of at least three reasons. First, one might defend the view that certain words as they occur in phrases in sentences do not contribute *their* semantic values to the propositions expressed by those sentences, because instead the semantic values of these words partially determine the semantic values of the *phrases* in which they occur, where these latter semantic values are contributed to the proposition. For example, one might think that in the sentence

(2) Tarski loves all beautiful women.

Letting 't' stand for Tarski, 'L' for the loving relation and 'ALL' for the second-order property "all beautiful women" and 'W' for the property "beautiful women", we can represent the proposition in question as the following ordered pair:

(2a) <*t, L,* <*ALL,* <*W*>>>>

[87] The proposition can only be *represented* by an ordered pair not identified. If one wanted to identify propositions with ordered pairs, one would face the following three big worries. First, a Benacerraf (1965) style worry: There is no good reason to identify the proposition expressed by (1) with (1a). There are a number of further candidates all of which seem to be equally good candidates to be identified with this proposition. Consider the following examples.

(1b) <L, <g, a>>

(1c) <L, g, a>

(1d) <a, L, g>

Moreover, we have just implicitly used the Wiener-Kuratwoski way of defining ordered pairs. But there are a bunch of other ways to do it, so there are still more candidates of ordered pairs which could be identified with meanings all of which are equally eligible to be propositions.

Third, propositions are normally viewed as having truth-conditions. But if propositions are ordered pairs it is mysterious how they can have truth-conditions.

Compare Lewis (1970a) and King (2007).

The phrase ‘loves all beautiful women’ contributes to the proposition expressed by (2) the property of loving all women, which is its semantic value. Though the semantic value of the word ‘beautiful’ partly determines the semantic value of the phrase ‘loves all beautiful women’, the proposition expressed by (2a) contains no constituent that is the semantic value of the word ‘beautiful’ alone.

Second, one might hold that a sentence may express a proposition (in a context), where the proposition has constituents not contributed by *any* syntactic constituent of the sentence, let alone any word in the sentence. For example, Mark Crimmins (1992) claims that an utterance of the sentence

(3) It is raining

expresses a proposition to the effect that it is raining at a particular time and place. The present tense manages to somehow contribute the time of utterance to the proposition. But no syntactic constituent of the sentence contributes the place to the proposition, though Crimmins claims it is a constituent of the proposition expressed.

Third, one might think that certain words simply have no semantic values, and so make no contribution to propositions. The so-called pleonastic ‘ne’ in French is thought to be an example of this.

But even though it may be implausible to defend a view according to which a sentence does not express a proposition whose constituents are the semantic values of all and only the *words* in the sentence, it is reasonable for a structured propositions theorist to claim that sentences express propositions, where *many* (and perhaps *most*) words or *phrases* occurring in the sentence contribute their semantic values to the proposition. As in the case of (1) and (1a), the constituents of the proposition in (1a) are precisely the semantic values of the words in (1). In the case of (2) and (2a) (given the assumptions made above), the constituents of (2a) are precisely the semantic values of the name ‘Tarski’ and the verb phrase ‘loves all beautiful women’. And in the case of (3), the proposition it expresses has three constituents, two of which are contributed by ‘raining’ and the present tense construction.

Ignoring the qualifications just made, the position of the structured propositions theorist (henceforth: SPAP) can be summed up as follows[88]:

> **SPAP:** *Sentences express propositions that are complex entities, (most of) whose constituents are the semantic values of expressions occurring in the sentence. These constituents are bound together by some structure-inducing glue that renders the structure of the proposition similar to the structure of the sentence expressing it.*

Given that structured propositions have as parts the semantic values of expressions in the sentences expressing them, the semantic values of those expressions are *recoverable* from the semantic values of the sentences (i.e. the propositions). This important feature of structured proposition accounts is highlighted by SPAP and distinguishes them from their main competitors, which hold that propositions are sets of possible worlds.

As we saw, the direct reference theorist claims that singular terms are devices of direct reference. David Kaplan argued in the late 1960's and early 1970's that paradigm examples of such devices are *indexicals* like personal pronouns, as for example 'I', demonstratives, as for example 'that', temporal pronouns, as for example 'yesterday', and modal pronouns, as for example 'actually'.

Defending TDR Kaplan saw himself, however, in a tradition inaugurated by Saul Kripke's seminal (1972). However, it is important to become straight about the fact that Kripke neither introduced nor adopted the direct reference rhetoric. His claim in *Naming and Necessity* was rather that proper names (among other expression like natural kind terms) are rigid designators. According to Kipke's intended use in his (1972) a rigid designator refers to the same individual *a* in all worlds in which *a* exists.[89, 90] He thought that when we consider a sentence containing an ordinary proper name, such as

(4) Gödel is a great logician

[88] Compare also King (2001). I draw on this article in my presentation.

[89] David Kaplan has spotted an ambiguity in the notion of rigid designation in Kripke (1972). Kripke has responded to this in his preface to the printed version.

[90] I write „the intended use", since Kripke's notion of rigid designator is ambiguous in "Naming and Necessity".

and ask whether it would have been true or false in various counterfactual circumstances, it is the properties of the very same man, Gödel, in those circumstances that are relevant to the truth or falsity of the sentence. So, 'Gödel' designates the same man in these various counterfactual circumstances; it is a rigid designator.[91]
Powered by a strong technical apparatus – possible world semantics – Kripke confronted the then widely adopted theory of reference - the description theory[92] - with some erratic arguments.

But what precisely is the relation between direct referential expressions and rigid designators?

Every directly referential expression is a rigid designator: its associated semantic rules determine the actual referent of the expression (in a context) and when evaluating what is said by the sentence containing the expression (in that context) in other possible circumstances, this same referent is always relevant. To illustrate, if I utter

(5) I write

at the present time and we want to evaluate whether what I said by means of that utterance is true or false in other possible circumstances, it is *my* properties in those other circumstances that are relevant. Thus, 'I' is rigid: when evaluating the truth or falsity of what is said by an utterance of a sentence containing 'I' in counterfactual circumstances, it is the properties of the person whom 'I' referred to in the utterance (the actual utterer) that are relevant.

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[91] Actually, the situation is more tricky. Kripke could have said that: 'Suppose Gödel was not in fact the author of this theorem etc.'. So a certain counterfactual scenario is being picked out in Gödel terms. Thus there's just no interesting question about who the name 'Gödel' refers to in this scenario. Trivially, the name 'Gödel' refers to Gödel, and I've told you who Gödel is in the scenario. Everyone has to agree with me on that, whether they're Millian or descriptivist. The real question is whether the scenario is metaphysically possible in the first place - the rhetorical strategy is to distract you with the (in fact trivial) question of how the names refer so that you forget to object to the very possibility - and then your tacit acceptance of the possibility reveals the Millianism which underwrites your own referential practice.

[92] This theory is a *version* of the Fregean theory of reference. In fact, Kripke's criticisms were so influential that almost no one agrees with this version of the Fregean theory anymore today. But it is important to note that there is *another* version of the Fregean theory which does *not* fall prey to Kripke's criticisms, namely the version that treats Fregean senses as modes of presentation. Insofar, Kripke has not succeeded in conclusively rejecting a Fregean semantics of proper names.

But the relation between directly referential expressions and rigid designators is not identity. It would be if all rigid designators were directly referential. Not so. Some rigid designators are not directly referential.
Consider the following example of David Kaplan's (1989: 495):

(6) The n[(snow is slight & n^2=9) $\vee$(~snow is slight & 2^2=n+1)]

This description determines the same object, namely 3, in all possible circumstances. Therefore it determines 3 in possible circumstances all in which it exists. If I utter

(7) The n[(snow is slight & n^2=9) $\vee$(~snow is slight & 2^2=n+1)] is odd

at the present time and we want to evaluate whether what I said by means of that utterance is true or false in other possible circumstances, it is the properties of the number 3 in those other circumstances that are relevant. Thus (6) is rigid.

But it is not directly referential: It doesn't contribute an individual to some proposition expressed by a sentence which contains this description in an appropriate syntactical position. It contributes a property. More precisely, a second-order property.[93] This second-order property says of the (slightly unperspicuously statable) property of *being* n^2=*9 while it is the case that snow is slight or being* 2^2=n+*1 while it is the case that snow is not slight* that it is instantiated exactly one time, namely by 3. So even though a proposition expressed by a sentence of the form "The n[(snow is slight & n^2=9) $\vee$(~snow is slight & 2^2=n+1)] is F" is about 3, it does not contain the number 3 as a direct or indirect constituent. A proposition expressed by a sentence of the form "Q is F", where Q is a determiner phrase, and F a predicate is called *general* if the proposition is not about any *particular* entity. A proposition expressed by a sentence of the form "Q is F" is called *particularized* if the proposition is about a particular entity but does not contain this entity as a constituent. Thus, "The n[(snow

[93] This means that the contributions of definite descriptions are on a par with the ones of quantifiers. And it means categorizing definite descriptions as quantificational and not referential. Thus, accepting that not all rigid designators are directly referential is to prescribing to a particular view of definite descriptions, namely the famous view initiated by Russell (1905). This has seldom been noted in the discussions of Kaplan's example. If one doesn't accept this particular view of the semantics of definite descriptions, one doesn't have to accept Kaplan's example. This doesn't have to imply, however, that there is no difference between rigid designation and direct reference.

is slight & n^2=9) ∨(~snow is slight & 2^2=n+1)] is F" is a particularized and not a singular or general proposition. Other (and much easier statable) examples of definite descriptions which are rigid designators are for instance "the successor of 1", "the predecessor of 3", "the product of 2 and 1". They all designate the same individual, namely the number 2 in all possible worlds.

To sum up: Kaplan intended to contrast directly referential expressions with expressions such as definite descriptions, which, though designating particular individuals, do so by means of descriptive conditions being expressed by the description and satisfied by the designated individual. Thus Kaplan (1989: 483) wrote that directly referential expressions "refer directly without the mediation of Fregean *Sinn* as meaning". The designation of definite descriptions *is* mediated by something like a Fregean sense (i.e. their associated descriptive conditions). Thus Kaplan writes:

> "For me, the intuitive idea is not that of an expression which *turns* out to designate the same object in all possible circumstances, but an expression whose semantical *rules* provide *directly* that the referent in all possible circumstances is fixed to be the actual referent. In typical cases the semantical rules will do this only implicitly, by providing a way of determining the *actual* referent and no way of determining any other propositional component" (1989: 493).

In possible worlds semantics, linguistic expressions are associated with intensions, functions from possible worlds to appropriate extensions. In the case of expressions designating individuals, these intensions will be functions from possible worlds to individuals. Note that *all* rigid designators (whether directly referential or not) will have intensions that are constant functions: they will be functions that map all possible worlds to the same individual. Thus possible worlds semantics tends to blur the distinction between directly referential expressions and rigid non-directly referential expressions (e.g. rigid definite descriptions). To make the distinction between directly referential expressions and rigid non-directly referential expressions more vivid, Kaplan invoked the notion of structured propositions as can be seen from the last but one of Kaplan's quotes above.

In the light of the foregoing discussion, Kripke's arguments for the thesis, that proper names are rigid designators and for what is generally called *Millianism* - the thesis that the meaning of proper names is exhausted by its referent - can be recast as arguments for the thesis that

proper names are directly referential. Take for instance Kripke's modal argument (1972). With this argument he wanted to show that the view, that proper names like 'Gödel' are synonymous with ordinary definite descriptions like 'the logician who proved the Incompleteness Theorems', is wrong. David Kaplan (1989: 512 -513) used the argument to conclude that demonstratives are directly referential expressions and hence are used to express singular propositions. An argument for this conclusion can be presented as follows:

Suppose that Carnap is standing at a chalkboard with two men; Gödel on his right and Tarski on his left. Tarski lives in Warsaw and Gödel lives in Vienna. Carnap points to the person on his right and utters sentence (9) below at time *t*.

(8) He lives in Vienna.

Carnap has expressed a proposition, call it "PROP" that is about Gödel. For the sake of reductio assume that PROP is not a singular proposition. Then PROP does not have Gödel as a direct or indirect constituent, but is in a different way about the particular individual Gödel. We then need to find some particularized proposition with which we can identify PROP. Consider the propositions expressed by the following sentences.

(9) The person on the right side of Carnap (at *t*) lives in Vienna.
(10) The person Carnap pointed to at *t* lives in Vienna.

Both of these propositions are particularized propositions about Gödel. Since they are directly about him, by means of the (contingent) properties he has (namely, being on the right side of Carnap at *t* and being pointed to at *t* by Carnap, respectively). Now consider the following counterfactual circumstance. Tarski has switched place with Gödel but everything else about them remains the same. Especially it is still the case that Gödel lives in Vienna and Tarski in Warsaw. In such a case, intuitively, PROP would be true while the propositions expressed by (9) and (10) would be false, in which case the former is distinct from either of the latter.

It is important to be clear that we are asking what the truth value of the proposition expressed by Carnap's utterance of (8) (i.e., PROP) and the propositions expressed by (9) and (10) *would be* in the described counterfactual circumstance. We are *not* asking what proposition Carnap would have expressed by uttering (8) in the counterfactual circumstance. It is true that different propositions would have been expressed by (8) in different circumstances of utterance or different *contexts*. Gödel can utter (8) pointing to Tarski or Tarski can utter (8)

pointing to Carnap. In these different contexts different propositions will be expressed, which may well have different truth values. Our claim is that the proposition expressed by Carnap's actual utterance of (8) has, in the described counterfactual circumstance, a different truth value than the propositions expressed by (9) and (10).

Since PROP differs in truth value in the described counterfactual circumstance from the propositions expressed by (9) and (10), it follows that PROP is distinct from the propositions expressed by (9) and (10). This is because it is an axiom of propositional theory that if PROP = PROP*, then PROP and PROP* have the same truth value in all counterfactual circumstances, as propositions are the objects that are true or false in counterfactual circumstances and so one and the same proposition cannot both be true and not true in a given counterfactual circumstance. These considerations suggest that the proposition expressed by Carnap's utterance of (8) is about Gödel directly and hence that PROP is a singular proposition. This is because any proposition that is about Gödel indirectly, in virtue of qualities that he contingently instantiates, will give rise to a similar argument.

Let me close the first part of this chapter by pointing at some other problems for direct reference theories.
An old problem which already plagued Frege and Russell is the following:

Compare the following two inferences:
(I)
John owns a book by Alfred Tarski.
Alfred Tarski = Alfred Teitelbaum.
Hence, John owns a book by Alfred Teitelbaum.

(II)
John believes he owns a book by Alfred Tarski.
Alfred Tarski is Alfred Teitelbaum.
Hence, John believes he owns a book by Alfred Teitelbaum.

Since co-referential expressions, that is expressions which refer to the same object, can be intersubstituted *salva vertitate* in extensional contexts, like the one generated be the first sentence, (I) clearly constitutes a valid inference.

In opposite to this, the latter inference in (II) isn't valid. After all, John can assent to the first premise while denying the conclusion. The question now is this:
How can it be that the first inference is valid, while the second is not valid?
The Fregean has an easy answer to this problem. His explanation is that even though the proper names "Alfred Tarski" and "Alfred Teitelbaum" have the same reference, they differ in sense. Since sense is what matters in propositional attitude ascriptions, it is no wonder that the inference in the first example is valid while it is invalid in the second.

I am now turning to the second part of this chapter. In this part I want to investigate whether the other premises of my argument can be defended by a direct reference theorist. I will start with whether there are any reasons to deny that knowing the meaning of an expression is to understand it.

6.3 Knowing Meaning and Semantic Externalism

Here is a platitude: To understand an expression *e* is to know its meaning. This platitude is supposed to establish a necessary connection between meaning and use.[94] It is not an informative analysis of what knowledge of meaning consists in – a question of considerable debate.
The platitude is supported by the following consideration.[95] If one does not employ the platitude, it is hard to see how and why competent speakers are able to understand complex expressions they never heard before. The only reasonable explanation is that they know something on the basis of which they can figure out, without any additional information, what the complex expressions mean. If this is so, something they already know must determine what *e* means. And this knowledge cannot plausibly be anything but knowledge of the structure of *e* and knowledge of the meanings of the simple constituents of *e*. I take it that this argument knocks down any attempts to reject the platitude.
This argument also establishes that the platitude may not however establish a sufficient condition between meaning and use. Knowledge of meaning is not sufficient for understanding. Suppose that someone has written down a sentence of Sanskrit on the board in room 6EG in Ludwigstrasse 31. A reliable informant tells me now that this sentence means

[94] Cf. Williamson (2000: 110)

[95] Cf. Schiffer (2003)

that Gödel was a great logician. Since the informant was a reliable source I came to know the meaning of the sentence. Nevertheless, I may not understand it as I don't know anything about the structure and the meanings of the constituents of the sentence.

Michael Dummett (1975), (1976), for example, uses this platitude in his arguments against Donald Davidson's (1984) truth-conditional semantics. According to Davidson's outlook, one understands a sentence if one knows the truth-conditions of this sentence. These truth conditions are biconditionals of the form "'s' is true if and only if P". But if one knows such truth-conditions, one has also to understand the biconditional. But if one also has to understand the biconditional, one has to know the truth-conditions of the biconditional, etc. Infinite regress. Furthermore, since the realist assumes that sentences can have truth-conditions, even though there is no speaker of the language which can tell and recognize whether they hold, Dummett concludes that the realist has no substantial explanation of what knowledge of truth-conditions consists in.

I don't want to discuss the consequences Dummett draws from this supposed failure of truth-conditional semantics, but just show that the platitude has always been regarded as a substantial and important condition which theories of meaning have to explain.

In fact, even Neo-Fregeans like Christopher Peacocke (1992), who defend decisively epistemological accounts of understanding, have made it an essential ingredient of their theories that to understand an expression, that is to possess the concept which is expressed by this expression, is to know its semantic value.
Peacocke's identification of understanding with knowledge of semantic values can be seen as a reaction to some arguments that shaped the discussion on *semantic externalism* from the 1970's until today. Semantic externalism is the negation of *semantic internalism*. Semantic internalism is the thesis, that the meaning and reference of some of the words we use are solely determined by the ideas we associate with them or by our internal physical state. Semantic internalism was questioned by three great attacks. First, some of Kripke's arguments in his (1972) to the conclusion that the reference of proper names and natural kinds is partly determined by external causal and historical factors cast forcefully doubt on semantic internalism. Second, Hilary Putnam (1975) and (1973) invites us in his famous "Twin Earth" thought experiment, to imagine the remote planet, Twin Earth in 1750, which is exactly like Earth except that instead of water, H_2O, it had a different substance, twin-water, the name of whose chemical composition is abbreviated by XYZ. Given that XYZ looks like water and

tastes like water, that it could be found in the rivers and oceans on Twin Earth, etc. the macro properties of XYZ are just identical with the macro properties of H_2O. But, since chemistry hadn't been invented that far, neither Earthlings nor Twin Earthlings were in a position to tell water from XYZ. According to Putnam, the crucial point in this thought experiment now is that an Earthling in 1750 who used the word "water" would have been referring to H_2O and not to XYZ. And a Twin Earthling in 1750 who used the word "water" would have been referring to XYZ and not to H_2O. Neither did the Earthling knew that water is H_2O and not XYZ nor did the twin earthling know that his "water" is XYZ and not H_2O. Putnam claims that it is absurd to hold that this ignorance created an obstacle to the Earthling referring to H_2O and the Twin Earthling referring to XYZ when using the term "water". If the Twin Earthling had pointed to a sample of XYZ and said "That's water," he would have said something true. But if the Earthling had pointed to the same sample he would have said something false.

As was said, Putnam intended his thought experiment to be an attack on *semantic* internalism. But McGinn (1977) showed that the thought experiment can generally be extended to reject internalism about mental contents. Internalism about mental contents is the thesis that mental states who can trigger mental contents, for instance propositional attitudes like belief, desire or regret, do not supervene on intrinsic facts about individuals alone. Thus, consider an Earthling before 1750 who uttered the sentence "water quenches thirst". By means of the utterance the Earthling expresses his belief that water quenches thirst. This belief is true if and only if H_2O quenches thirst. Now suppose that on Twin Earth there is a physically identical counterpart of our Earthling. This counterpart had only encountered twin-water and had never encountered samples of water or heard about water from other people since there is no H_2O on Twin Earth. The semantic externalist takes this to show that our Twin Earthling does not believe that water quenches thirst. For by means of the utterance "water quenches thirst" the Twin Earthling is expressing his belief that twin-water quenches thirst. This belief is true if and only if XYZ quenches thirst. Thus the proposition which he stands to in the belief relation has different truth-conditions. The semantic externalist takes this thought experiment to establish that the Earthling and the Twin Earthling had different beliefs even though being intrinsically identical. Thus there are beliefs which do not supervene on intrinsic facts alone. Given this denial of internalism about mental contents, externalists argue that their view about mental content is correct.

In the Twin Earth thought experiment Putnam is only concerned with natural kind terms. He tried to show that their semantic values depend on our environment and the identity of certain physical substances in this environment. A different version of externalism is defended by Tyler Burge in his (1979) and (1986). This is the third argument against semantic internalism. Burge shows that not only the environment and the identity of certain physical substances in this environment fix the semantic values of some expressions and as a consequence also the contents of our beliefs and thoughts, but that social institutions also play a crucial role in fixing the semantic values of some of our expressions and as a consequence, the contents of some beliefs and thoughts.

Burge invites us to imagine an English-speaking individual, which he calls "Jane" and who suspects she has arthritis on the basis of her observation of having an ailment in her thigh. But Jane is entirely on the wrong path. She is not a doctor. She does not know that arthritis is a condition of the joints only. So when she expresses her belief by means of the utterance "I have arthritis in my thigh" she is expressing a false belief, since her belief is true if and only if she has arthritis in her thigh. Now Burge stipulates a counterfactual situation where the internal states and history of Jane are identical with the ones in the actual world with one exception. The exception is that she grew up in a community where the word "arthritis" is used to apply to a different disease, say tharthritis. Thartritis includes rheumatoid ailments of not just the joints but also the thighs. If this counterfactual scenario is really possible, Jane lacks the belief that she has arthritis in her thigh, or any other beliefs about arthritis. The reason for this is that nobody in her linguistic community possesses the concept of arthritis. But by the belief she is expressing by means of the utterance "I have arthritis in my thigh", she is instead expressing the true belief that she has tharthritis in her thigh. In this counterfactual scenario her belief is true if and only if she has thartritis in her thigh. Thus the proposition which she stands to in the belief relation has different truth-conditions. The semantic externalist takes this thought experiment to establish that Jane had different beliefs in the actual world and the counterfactual scenario even though Jane in the actual world is intrinsically identical to Jane in the counterfactual scenario. Thus there are beliefs which do not supervene on intrinsic facts alone. Given this denial of internalism about mental contents, externalism about mental content is correct. Moreover, the thought experiment seems to show that semantic values and mental contents are partly determined by communal linguistic practice. After all, the two situations differ only in the linguistic usage of the community.

Given the above arguments, he concludes that the semantic values of singular concepts - that is concepts which are expressed by singular terms - cannot be entirely determined by the internal physical states of a subject but have to be grounded in the external world. He satisfies this constraint by identifying the semantic values of singular concepts with their referents. So only when one knows what the semantic referent of a singular concept is, one possesses this concept and understands a singular term which expresses this concept.

What about compositionality? Should a platonist who defends DRT reject compositionality to save his position from the commitment to the claim that we don't understand mathematical statements? In the following section, I want to argue that he shouldn't.

6.4 Compositionality

It is not an exaggeration to call the principle of compositionality one of *the* great paradigms in the philosophy of language, the philosophy of mind, and in modern linguistics. The principle of compositionality is commonly stated as follows[96]:

> **COMP1:** The meaning of a complex expression is a function of the meanings of its constituents and its structure and nothing else.

Compositionality is the semantical version of the widespread idea of a recursive process by which an infinite number of sentences can be generated from a finite vocabulary and a finite set of rules. Such a recursive process is important not only in semantics but also in syntax, morphology and even phonology. It is said that recursive processes are unavoidable as explanations of the productivity and systematicity of (natural) language and a solution to the problem of linguistic creativity.

But the formulation as it stands is not yet completely satisfying. First, we should relativize COMP1 to a particular language $\mathcal{L}$ such that questions about the structure of the sentences and about the meanings of simple expressions are settled by the grammar of $\mathcal{L}$ and the lexicon of $\mathcal{L}$, respectively. Second, since the formulation leaves out a determination of the

[96] Cf. Szabó (2000), (2004), and (2008). This section draws on this work.

range of the function in question, we should reformulate the principle (ontologically more neutrally) as a determination claim.

> **COMP2:** For every complex expression *e* in $\mathcal{L}$, the meaning of *e* in $\mathcal{L}$ is *determined* by the structure of *e* in $\mathcal{L}$ and the meanings of the constituents of *e* in $\mathcal{L}$.

Thus formulated the principle of compositionality is a global principle. We can discriminate this global version from a local version, which is formulated as follows.

> **COMP3:** For every complex expression *e* in *L*, the meaning of *e* in *L* is determined by the immediate structure of *e* in *L* and the meanings of the immediate constituents of *e* in *L*.

The immediate structure of an expression is the syntactic mode its immediate constituents are combined. *e* is an immediate constituent of *e′* if and only if *e* is a constituent of *e′* and *e′* has no constituent of which *e* is a constituent.

One weakness of COMP2 and COMP3 is that it does not completely rule out a language which contains a pair of non-synonymous complex expressions with identical structure and pairwise synonymous constituents. After all, such a pair of complex expressions could exist in *distinct* languages. But if the principle of compositionality is supposed to capture the idea of meaning determination it has to rule out this possibility.

Here is an illustration from Szabó (2000b): Suppose English is compositional. Take two of its non-synonymous sentences—say, 'Elephants are grey' and 'Julius Caesar was murdered on the ides of March'—and define Crypto-English as the language with the same expressions, the same syntax and almost the same semantics as English. The *only* difference is that if a sentence is synonymous in English with one of the two designated sentences, then it is synonymous with the other in Crypto-English. We assumed English is compositional and hence that there is no pair of non-synonymous complex expressions in English with identical structure and pairwise synonymous constituents. Trivially, the same must hold for Crypto-English as well. But intuitively, Crypto-English is *not* compositional. The structure and the meanings of constituents of the Crypto-English sentence 'Elephants are grey' cannot determine what this sentence means in Crypto-English—if they did then the structure and the

meanings of constituents of the English sentence 'Elephants are grey' would have to determine what 'Julius Caesar was murdered on the ides of March' means in English.
Thus one could enumerate the following formulation:

> **COMP4:** For every complex expression *e* in *L*, the meaning of *e* in *L* is functionally determined through a single function for all possible human languages by the structure of *e* in *L* and the meanings of the constituents of *e* in *L*.[97]

Formally, the principle of compositionality can be captured by an ingenious suggestion of Richard Montague's (1970). The key idea is to postulate the existence of a *homomorphism* between the expressions of a language and the meanings of those expressions.[98]

According to Montague (1970) a language can be seen as a Universal algebra, that is a set of (simple and complex) expressions on which a number of operations are defined.[99] The operations are to be interpreted as syntactic rules. The syntactic rules (some of which are free to be left undefined) are required to apply to a fixed number of expressions to yield a single expression. Then let *syntactic algebra* be a (possibly) partial algebra $\mathsf{A} = \langle |\mathsf{A}|, (F_\gamma)_{\gamma \in \Gamma} \rangle$, where $|\mathsf{A}|$ is the set of (simple and complex) expressions and every F_γ is a possibly (partial) syntactic operation on $|\mathsf{A}|$ with a fixed arity. The syntactic algebra is interpreted through a meaning-assignment β, that is a function from $|\mathsf{A}|$ to $|\mathsf{M}|$, the set of available meanings for the expressions of A.

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[97] A further and highly sophisticated formulation of the principle of compositionality is defended in Szabó (2000). This version reads as follows:

> For all possible languages , for any meaning property M and any complex expression e in $\mathcal{L}$, if e has M in $\mathcal{L}$, then there is a constitution property C such that e has C in $\mathcal{L}$, and for any possible language $\mathcal{L}_0$ if any complex expression e_0 in $\mathcal{L}_0$ has C in $\mathcal{L}_0$ then e_0 has M in $\mathcal{L}_0$.

Since nothing in my argument hinges on it, I won't comment on this version and Szabó's reasons to endorse it in the rest of this work.

[98] Note that I follow Szabó (2004) in my discussion.

[99] In the literature, it is often overseen that Montague's algebra is not simply Boolean but Universal (an example is Szabó's work), which leads to a generalization of classical Tarski semantics. For more on this issue see Link's *locus classicus* (1979).

A k-ary syntactic operation m on $|\mathbf{A}|$ is F-compositional just in case there is a k-ary (partial) function G on $|\mathbf{M}|$ such that whenever $F(e_1,\ldots,e_k)$ is defined,

$\beta(F(e_1,\ldots,e_k)) = G(\beta(e_1),\ldots,\beta(e_k))$.

This means that there is a (partial) function from the meanings of $e_1,\ldots,e_k$ to the meaning of the expression built from expressions $e_1,\ldots,e_k$ to which the syntactic rule F was applied.)

Let m be compositional *simpliciter* only if for each syntactic operation in $\mathbf{A}$, m is F-compositional. Whenever m is compositional, it induces the semantic algebra $\mathbf{M} = \langle |\mathbf{M}|, (G_\gamma)_{\gamma\in\Gamma}\rangle$ on $|\mathbf{M}|$, and it is a homomorphism between $\mathbf{A}$ and $\mathbf{M}$.

In the above, the formal statement of compositionality places no restrictions on what β assigns to members of $|\mathbf{A}|$ we are able to capture not only compositionality meaning, but could also compositionality of reference. Such a principle of compositionality is endorsed, for instance, by Frege (1892a). It can be stated as follows:

> **COMP5:** For every complex expression e in L, the reference of e in L is determined by the structure of e in L and the references of the constituents of e in L.

The principle captures only local language-bound compositionality. This is due to the fact that it requires that each application of each syntactic rule within a language has to be matched by an application of an appropriate semantic function. We can extend this principle to capture cross-linguistic compositionality by claiming that the expressions within $|\mathbf{A}|$ are the expressions of all possible human languages.

As Szabó (2004) points out, global compositionality can be formally captured as follows. If the same syntactic operation is applied to lists of expressions such that the corresponding members of the list are synonymous, expressions e and e' are called *local equivalents*. (More formally: $\exists n\in\mathbf{N}\ \exists$ n-ary $F \in \mathbf{A}$, $\exists e_1,\ldots,\exists e_n$, $\exists e_1',\ldots,\exists e_n' \in |\mathbf{A}|$. $e = F(e_1,\ldots,e_n)$. $(e' = F(e_1',\ldots,e_n') \wedge \forall i\ (1\leq i \leq n \rightarrow \beta(e_i) = \beta(e_i')))$. m is locally compositional just in case locally equivalent pairs of expressions are all synonyms. The expressions e and e' are said to be *global equivalents* only if they resulted from applying the same syntactic operation to lists of expressions such that corresponding members of the lists are either (i) simple and synonymous or (ii) complex and globally equivalent. (Szabó gives the following elegant

recursive definition: Let us say that the expressions e and e' are *1-global equivalents* just in case they are synonymous simple expressions. Let us say that the expressions e and e' are *n-global equivalents* just in case for some natural number k there is a k-ary $F \in \mathsf{A}$, and there are some expressions $e_1,\ldots,e_k, e_1',\ldots,e_k' \in |\mathsf{A}|$, such that $e = F(e_1,\ldots,e_k)$, $e' = F(e_1',\ldots,e_k')$, and for every $1 \leq i \leq k$ there is a $1 \leq j < n$ such that e_i and e_i' are j-global equivalents. Then the expressions e and e' are *global equivalents* only if there is some $n \in \mathrm{N}$ such that they are n-global equivalents.) I follow Szabó (2004) in calling an expression globally compositional just in case globally equivalent pairs of expressions are all synonyms.

The motivation and the strength of structured propositional accounts lies in the fact that they are in a position to rigorously adopt compositionality. I say "rigorously adopt compositionality" for their account grants the tight correspondence between syntax and semantics which is intuitively associated with compositionality. If one thinks that the possible worlds account of propositions is true, the meaning of a sentence is the set of possible worlds where this sentence is true. According to this account tautologies are synonymous. They hold in all possible worlds. But it is first highly unintuitive that sentences like "All bachelors are bachelors" and "War is War" are synonymous. Especially, since sentences resulting from embedding tautologies under intensional operators are not true in the same worlds. Consider the sentences "Gödel believes that all bachelors are bachelors" and "Gödel believes that war is war". The first sentence can be true at a world, while the second is false and vice versa. Given that the sentences "All bachelors are bachelors" and "War is war" have a different syntactic structure – the first contains for instance a determiner phrase, while the second doesn't – and different constituent expressions, they express the same proposition. This seems to be an unacceptable violation of the principle of compositionality. A structured propositions theorist is not committed to violate the principle of compositionality in this way. He explicitly takes into account how a sentence is syntactically structured and which constituents it contains, when he considers which proposition is expressed by a particular sentence.

Since tautologies might differ structurally or in the meaning of their constituents, structured propositions theorists are in a position to avoid the claim that all tautologies are synonymous. In addition to that this difference in structure and constituents enables them to explain how the application of operators generating intensional contexts to tautologies can yield non-synonymous sentences.

A very early example of a structured propositions account which exploits this fact is Lewis's 1970 theory. Lewis argues that a proposition is something which cannot be identified with intensions alone. If propositions could be identified with intensions, tautologies like "All bachelors are bachelors" and "Peter = Peter" would express the same propositions and thus be synonymous. But Lewis takes it that there are differences in meaning between those sentences. This leads him to the claim that differences in intension supply us only with coarse differences in meaning. How do we get the fine differences in meaning then? According to Lewis, we have to investigate how a compound can be analysed into constituents and what intensions the constituents have.

For instance, the sentences "Grass is green" and "Snow is white" have the same intension. Nevertheless, the two sentences vary in what constituent expressions they contain and in what intensions the constituent expressions have. Lewis thereby identifies meanings with semantically interpreted phrase markers minus their terminal nodes: finite ordered trees having at each node a category and an appropriate intension.[100]

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Another example is the theory developed by Jeff King in his (2007). Interested in circumventing worries about the ontological status and innocence of propositions, King suggests that propositions are nothing else than facts. Since, according to King, it is no mystery that there are facts and that facts contain constituents, those facts contain the object which we talk about in the sentence as a constituent. For example consider the following sentence:

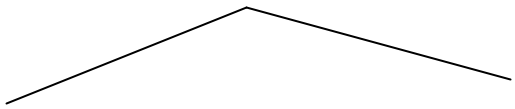
(11) Gödel swims.

(11) is true if and only if Gödel swims. Gödel swims if and only if Gödel instantiates the property of swimming. And Gödel instantiates the property of swimming is true if and only if there is a fact of Gödel instantiating the property of swimming. This fact makes the proposition expressed by (11) true but it is not identical with the proposition. Since if Gödel had failed to possess the property of swimming, there would have been no fact consisting of him possessing the property and as a consequence, the proposition that Gödel swims would

[100] Lewis thinks that the problem is that his theory may interpret meanings too finely, since a sentence of the form "Non Snow is not white" and "Snow is white" differ in meanings, since they differ in constituent expressions and the intensions of the constituent expressions. But in the end, he says that this does not matter very much, since what he identifies with meanings is sufficiently near to what the real meanings are

have been false. The difference between the fact which is the proposition expressed by (11) and the fact which makes this proposition true is that the former contains constituents which the latter lacks. Since it is highly sophisticated what these further constituents are, I will build up King's picture step by step. King exploits the fact already mentioned above, that syntactic representations form the basis on which any sort of semantics must be defined over. These syntactic representations tell us in which relations the different words of a complex expression stand to each other by binding them together and imposing structure on them. The semantics must consist, first, of rules that map these syntactic representations to structured propositions and, second, of a definition of truth for structured propositions. The constituents of complex expressions and the structure which are to be assigned meanings and which are mentioned in COMP 1 – 5 are thus determined by syntax alone. For example the syntactic representation of sentence (11) looks as follows:

(12)



Gödel swims

The question is:

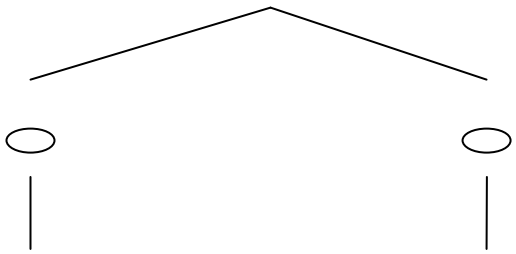
(13) What binds together Gödel and the property of swimming in the proposition expressed by (11)?[101]

King thinks that Gödel is the semantic value of an expression of a language that occurs in subject position in the sentential relation, call it "R", which is represented by the branching line in (12) and of an expression of the language at the terminal node of the right branch of R

[101] King (2007). He has the following example: If propositions are just mereological fusions of their constituents, there need be nothing that binds the constituents of propositions together. Normally a defender of such a mereological approach demands that mereological composition or fusion is unrestricted and that there is no unique fusion of given parts (in order that one has distinct propositions with the same parts). King asks which fusions are propositions and why have the fusions that are propositions truth-conditions?

that has as its semantic value the property of swimming. We know this relation simply in virtue of the sentence existing and the words "Gödel" and "swims" having the semantic values they have. King represents this relation as follows, where the branching line stands for R, Gödel* and swims* stand for the semantic values of "Gödel" and "swims", respectively, that is for Gödel and the property of swimming, the vertical lines above Gödel* and swims* stand for the semantic relations between "Gödel" and Gödel, and between "swims" and swims* and the small ovals between the vertical lines and R for the relation between properties of *joint instantiation*, that is the property of referring to Gödel and occurring at the right terminal node and expressing the property of swimming are jointly instantiated.

(14)



Gödel* swims*

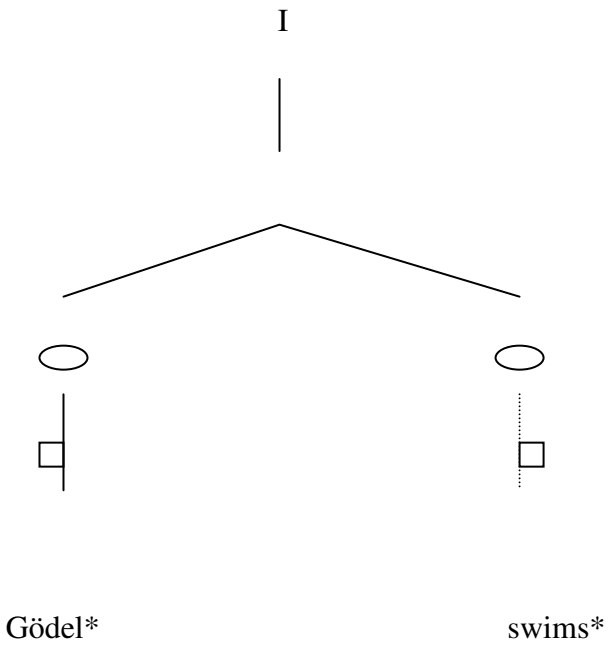
But this suggestion isn't completely satisfying yet. First, we don't have the guarantee that the syntactic concatenation function R encodes the instantiation function., call it "f". There could be languages where the syntactical concatenation function does not encode this function. Thus King (2007: 36) says:

> "… not only is it a contigent fact that the semantic significance of the syntactic concatenation function in [(12)] involves the instantiation function f …, but it is also a contigent fact that that syntactic concatenation *instructs* that f is to be *applied* to the semantic values of the expressions at the left and right terminal nodes (and a world) to determine the truth value of the sentence (at the world)."

In the face of this, King builds into the proposition, that the syntactic concatenation function encodes the instantiation function.

Moreover, we have only said something about expressions which are not context-sensitive. To include such expressions we need to allow semantic relations between expressions and their semantic values that are relativized to contexts to bind constituents into propositions. After adding these further constraints, propositions can be represented by the following tree, where the vertical line from R to the instantiation function I is represented as a solid line and the small squares represent properties of contexts of utterance:

(14)



So the answer to our question in (13) is: The relation binding Gödel* and swims* together in the proposition is the relation between a person (x) and a property (y): there is some context c and some words a and b of some language L such that a has x as its semantic value relative to c and occurs at the left terminal node of the sentential relation R that in L encodes the instantiation function and b occurs at the right terminal node and has as its semantic value in c y.

So, both Lewis and King, rely heavily on assumptions about the principle of compositionality, when they are concerned with meanings.

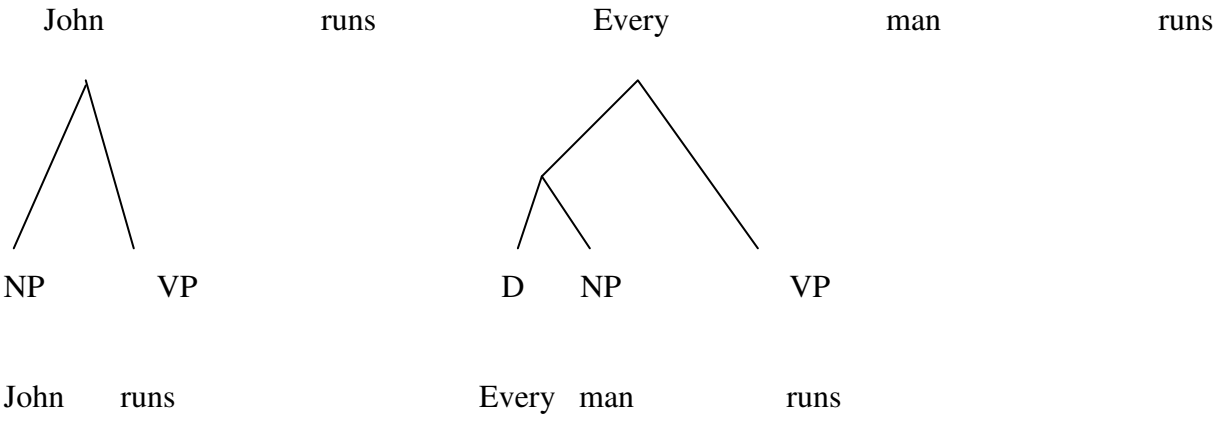
To put it in a nutshell: There is a strong dependence of TDR on the principle of compositionality. This dependence of TDR on the principle of compositionality is carried over by the dependence of structured proposition accounts on the principle of compositionality. And as was argued in the above section, entitled "Direct Reference, Structured Propositions, and Direct Reference", structured proposition views are congenial to TDR. Only structured proposition accounts supply TDR with a metaphysical grounding. Given this chain of dependences it is a bad idea for a direct reference theorist to reject compositionality and thereby premise (P6) - to understand a complex expression is to know its meaning and its structure (and nothing else) - in the above argument. In the next section, I want to investigate whether there is a way of rejecting premise (P5) - the claim that to understand an expression is to know its meaning.

6.5 Numerals as Quantifiers?

Should we treat numerals really as quantifiers? We can do that and in fact this also has been done: For example in generalized quantifier theory.

Montague and Lewis raised the type of proper noun phrases to the type of quantifiers.[102] On this construal a proper name denotes a set of subsets of the domain, namely this set of subsets which contain the individual John as an element. More formally, this can be stated as the set $\{S \subseteq |M| : j \in S\}$. Montague treated proper names as quantifier phrases since he wanted to evade the objection that he assigns two semantical types to a syntactical category thus making himself liable of giving up the homomorphism between syntax and semantics. Consider the following trees:

[102] Partee (2005: 11) reports the interesting story, that it cannot be clearly said, if the idea of raising the types of proper names to the one of quantifiers is due to Lewis or to Montague.



We can see that the phrases "John" and "Every man" are of the same syntactical category. But originally Montague assigned to the two phrases two different semantical values. Namely, the basic type e in the case of "John" and the complex type <<e,t>, <e,t>, t> in the case of the Noun Phrase "Every man". Syntacticians were not content. But by raising the type of proper noun phrases like "John", Montague was ingeniously able provide a unified semantical treatment of NPs.

But this treatment of NPs raised the question whether all NPs are really on a par. Are proper names really quantifiers? Or are proper names not really something else than quantifiers?
The DRT Platonist does not have to believe that all proper nouns are quantifiers. It is still open to him to claim that only a special kind of proper noun, namely numerals, are quantifiers. Numerals would then contribute second-order properties to the propositions sentences express with this relation.

Harold Hodes (1990), (1991a), and (1991b) has made a precise suggestion of how it is possible to treat numerals as non-referring expressions in this sense. According to Hodes numerals are a special kind of encoders which encode higher-order cardinality quantifiers in first-order garments. Hodes shows this by constructing two model-theoretic semantics the first of which represents Platonism, which he calls the *mathematical object picture* of mathematics, while the latter represents his own alternative theory of the "semantic underpinnings of mathematical discourse". However, his construal of singular terms won't help the Platonist. Even though one motivation of Hodes is to avoid the theory of direct reference[103], this theory is not applicable for the Platonist. After all, Hodes's theory is not a platonistic theory. Hodes construes a theory in which sets or numbers, are not genuine objects.

[103] Hodes (1984)

But this is exactly what the platonist claims. According to Hodes, there simply are no such things as numbers. And this leads to having to give up on Platonism.

6.6 Some Related problems

Our problem for someone combining the theory of direct reference and platonism is to answer how we can understand mathematical statements if we don't determinately know what numbers are.
This problem is different from the question of how our thoughts and practices can fix the meanings of mathematical terms with sufficient precision to ensure that each sentence has a determinate truth value. And it is different from the problem of how we can refer to abstract entities if there are no causal-historical connections between humans and numbers. And it is different from the problem of whether and if so how numerals have determinate referents. To give a more workable overview, there are four problems which concern the Platonist's answers to the following four questions (which I will henceforth call *problems (A), (B),* and *(C) and (D)*):

(A) What is the semantic contribution of a lexem to the proposition expressed by the sentence?
(B) How is reference fixed? (By the satisfaction of a causal-historical chain or a description?)
(C) Have numerals determinate referents?
(D) Is there anything in our thoughts and practices which more determinately supplies referents of numerals than up to isomorphism?

As I said in the beginning of this chapter, I am concerned with question (A). In this section I am going to give a thorough exposition of the problems (B) – (D). I start with problem (B).

Problem (B) goes back to Harold Hodes (1984). This problem is a semantical variant of Benacerraf's (1965) famous epistemological problem. Benacerraf's epistemological problem is how can we have knowledge of numbers, given their abstract nature. If numbers are abstract, they are not located in space and time, and causally inert. But if they are not located in space and time, and are causally inert, there is nothing that enables us humans as physically embodied creatures to keep track of these entities. In his presentation of the problem

Benacerraf relied on the causal theory of knowledge developed by Goldman (1967) amongst others. But Field (1989) has shown that this problem is independent on which theory of knowledge one uses to state the problem. If one adopts reliabilism, one has to answer as to how we can achieve reliable beliefs about knowledge.

According to Kripke's 1972 theory of reference, reference of singular terms and some general terms, like natural kind terms, is fixed by causal-historical chains between humans and the world of concreta. Those causal-historical connections determine that our words refer to physical entities like individuals and natural kinds. But the platonist cannot use this reference fixation in his account of reference to numbers since numbers are abstract entities. This means that they are not located in space and time and are causally inert. So it trivially holds, that there is no causal-historical connection between numerals and numbers. How can there be a causal chain between numerals and numbers, if numbers are causally inert? How can there be a baptism ceremony, if we don't have any access to what we are supposed to baptize? Given that the standard theory of reference does not decide how our numerals refer to numbers and that it does not give an informative account of the 'microstructure of reference' of mathematical discourse, the platonist who adopts the causal-historical theory of reference owes us a plausible account of how we are able to refer to them. According to Hodes this situation is deeply unsatisfying since we have to give an informative account about the microstructure of reference of mathematical discourse which binds the distinct phenomena of speaker's reference, semantic reference and our referential abilities themselves together. He allows however that this account does not have to state necessary and sufficient conditions for this microstructure.

> "The challenge to the mathematical-object theorist is: Tell us about the microstructure of reference to, e.g. cardinal numbers. In what does our ability to refer to such objects consist? What are the facts about our linguistic practice by virtue of which expressions in our language designate such objects and the concepts under which they fall or fail to fall? If no plausible answers are available, we should reject the theory that invites these questions. We should conclude that in asking these questions we are overstepping the positive analogy that sustains a model, applying to a picture a weight that only a theory could bear" (Hodes 1984: 126 – 127).

It is obvious that Hodes's problem already contains problems (C) and (D).

Problem (C) is a variant of Paul Benacerraf's famous argument developed by McGee (1997). As is the case with Hodes argument for problem (B), it makes it highly questionable, that problem (C) is soluble at all. Problem (C) is a problem for everyone who, like the Bourbaki for instance, pursues a reductive and foundationalist goal by trying to reduce classical mathematics to set theory. Benacerraf asks the following questions:
What does the numeral **5** refer to? What determines what set, if any, the numeral **5** refers to?

Various suggestions to answer these questions have been made. The numeral **5** refers to the set {4}, as Zermelo suggested in his plan for reducing number theory to set theory. The numeral **5** refers to the set {0,1,2,3,4}, as von Neumann suggested. The numeral **5** refers to neither the set {4} nor the set {0,1,2,3,4} but to something else which doesn't have to be a set at all. Benacerraf's problem seems to show that neither the truth conditions of mathematical sentences nor our practices in using the numbers in counting and ordering answer this question. "Any of countlessly many isomorphic structures will serve equally well as the referent of the phrase 'natural number system'" (Mc Gee 1997: 37).

Even though the problem is normally understood as depending on questions about ontological reduction, McGee (op. cit.) shows that this problem is not dependent on those questions. Rather it can be extended to a general argument for the conclusion that there is nothing in our mathematical thoughts and practices that supplies a more definite determination of the referents of numerals than "up to isomorphism" that is problem (C). Since only our mathematical thoughts and practices can supply such a more definite determination, this means that numerals lack determinate referents.

McGee goes through several options of how a platonist could respond to the problem but rebuts all these options. First, it is possible to stipulate that the numeral **5** refers to the von Neumann ordinal {0,1,2,3,4}. But McGee argues that this way out of the difficulty just postpones the problem. Even though such stipulations would give the platonist a definite determination of reference for numerals which are more than only up to isomorphism, he still needs a definite determination of reference for set-theoretic terms. But here the same problem as with numbers shows up. Nothing in our thoughts and practices fixes the referents of set-theoretic terms more determinately than up to isomorphism. If we identify sets with abstract

objects, the reference stays underdetermined for the well-known reasons. If we identify sets with concrete objects for as McGee (op.cit.: 37) elegantly puts it:

> "We cannot take the terms of the language of set theory to refer to a system in which the von Neumann number '7' is identified with the dog Fido, since if we did so and, heaven forbid, Fido were run over by a truck, there would no longer be a prime number between 5 and 11. So we may rule out concrete objects as the referents of the von Neumann numeral '7'."

McGee argues that this problem is a variant of the more general phenomenon of inscrutability of reference. The inscrutability of reference is the thesis that there is no fact of the matter what our terms refer to. Quine (1960) is concerned with a field linguist who is investigating a foreign language of a tribe. If a member of this tribe utters "gavagai" and points to a rabbit, the field linguist cannot precisely determine what the word "gavagai" refers to. There are multiple theories which the field linguist can adopt but which contain competing claims about what "gavagai" refers to. According to a first theory '*gavagai*' refers to rabbits, according to a second theory, it refers to undetached rabbit parts, according to a third it refers to particular time-slices of rabbits. All of these options are equivalently confirmed by the body of evidence available to the field linguist. And worse, there is not even a *primus inter pares*. Quine invites us to imagine what happens when we apply this scenario to our own language. If a speaker uses the word 'rabbit,' he is still not in a position to rule out any of the above mentioned theories. 'Rabbit' can refer to rabbits, to undetached rabbit parts, or to particular time-slices of rabbits or to anything else. All of these options are equivalently confirmed by the body of evidence available to the field linguist. And again there is not even a *primus inter pares*. Quine concludes that there is no such thing as to what our words refer to. Davidson (1984), and (2001) radicalizes this thesis by saying that the notion of reference is of no use at all.

These arguments seem to establish that no matter which singular term we have, numeral or not, referring to abstract objects or not, the referent of this term is fixed no more precisely than up to isomorphism. This thesis has proven highly controversial, however. After all, there are contingencies of occasion, place, and world, as well as causal connections between word and object that may succeed in penetrating the inscrutability to concrete objects. But this move won't work in the case of numerals. As was already mentioned above there are no causal connections to abstract objects and *ipso facto* the platonist's mathematical objects and

there are also no contingencies. Hence there is nothing which can avoid the inscrutability of reference of numerals. Mc Gee (op.cit.: 39) takes this to arise from the fact "that our thoughts and practices in using mathematical vocabulary are unable to discern a preference among isomorphic copies of a mathematical structure".

McGee (op. cit.: 39) makes an important point when he notes that inscrutability of reference need not be an obstacle to determinateness of truth values:

> "If our thoughts and practices in using the vocabulary distinguish an isomorphism class of equally good candidates for what the terms refer to, this will be enough to establish a determinate truth value for each of the sentences, even though it doesn't pin down the referent of any term. Inscrutability of reference does not imply inscrutability of truth conditions. Once we see that our commitment to determinate truth values does not entail a commitment to a determinate reference relation, questions like what set, if any, the number 7 is equal to are no longer a matter of urgent concern. It matters vitally that the natural number system is isomorphic to an initial segment of the von Neumann ordinals. It doesn't matter at all whether the natural number system is identical to the initial segment. Once we give up realism about reference, precise ontological reduction is no longer necessary."

Only giving up the doctrine of referential realism allows an unproblematic use of ontological reduction.

McGee argues that we adopt a convention that assigns truth values to sentences like '$5 \in 7$', to which Fermat's usage, together with the mathematical facts, assigned no values, when we "identify" the natural numbers with the von Neumann numbers. This practice is on a par with assigning truth values to borderline attributions of vague terms by adopting conventions making the vague terms more precise. If it suits our purposes to do this, we are entirely free to do this. I outright agree with McGee (op. cit.: 40) when he says that "Identifying the numbers with the sets is not a matter of pruning our ontology, but rather a matter of refining our usage. What there is, is not a question of notational convenience, but how we use our symbols is, in large measure, a matter of convenience."

McGee points out that even though pure and applied set theory has a lot to say about the internal structure of the universe of sets, I has not much to say about which entities the word 'set' refers to. It could be compatible with our current practices to improve on our current usage of set-theoretic notation by employing the word 'set' in such a way that every abstract entity is regarded as a set. According to such a broad usage of ‘set’, a Platonist would believe that everything there is, is either a concrete individual or a set.

McGee is mainly concerned with how we learn a mathematical language. According to him this question is hard to answer. Since the straightforward answer won’t do. The straightforward answer concludes, that we learn a mathematical language by counting matchbox cars, measuring pieces of matchbox cars and grouping together matchbox cars. But these practices of counting, measuring and grouping cannot ensure that mathematical sentences have determinate truth values, since they do not specify the meanings of mathematical terms with enough precision but leave open a number of nonstandard models. If knowing how to use mathematical terms in practical problem solving is not enough, what then must be added to this practice such that all the nonstandard models are excluded?
McGee (op. cit: 40) thinks that it is learning of some body of mathematical theory. Since our practices and thoughts do not satisfiably specify the meaning of mathematical terms, since our practical uses of those terms do not determinately and precisely specify the truth-conditions of mathematical sentences, we have to look for our theoretical uses of the terms.

> “The axioms that serve as meaning postulates for the mathematical vocabulary are not propositions but sentences, sentences we use in the same ways we use sentences that express propositions we regard as true. We assert and assent to the axioms. We assert and assent to sentences derived from the axioms by the rules of logic. If ‘φ’ is a sentence not containing the new vocabulary that we have derived from the axioms by the rules of logic, we accept the proposition ‘φ’ expresses. The rules of language governing the use of mathematical vocabulary permit us to use the axioms the same way we use sentences that express propositions we regard as true. These rules succeed, as we shall see, in fixing a determinate truth value for the mathematical sentence, since, up to isomorphism, there is one and only one way to assign referents to the terms so as to make the axioms come out true. The meaning postulates for the language of arithmetic single out an

> isomorphism class of candidates for the referent of the phrase 'natural number system'. Assuming that the class is nonempty, an arithmetical sentence is true if and only if it comes out true (according to a Tarskian definition of truth) when the arithmetical terms are assigned referents from a member of the class."

McGee solves this problem by suggesting a new axiom for set theory which is not supposed to be contained in the implicit concepts of ordinary speakers. He claims that this axiom can ensure that the axioms of set-theory pick out unique (up to isomorphism) referents. He thinks that the sentences of the language of set-theory do not have determinate truth values before this axiom was accepted as a new meaning postulate for the language of set-theory.

6.7 Conclusion

Mathematical platonists often uncritically adopt the theory of direct reference and direct reference theorists often uncritically adopt mathematical platonism. I showed that such a combination is faulty. The two views cannot be held simultaneously. If one does, one is committed to the claim that we can neither have mathematical beliefs nor an understanding of mathematical sentences. This is absurd. Obviously, we do understand mathematical sentences and do have mathematical beliefs. I showed that rejecting any of the premises of my argument leads to highly unwanted consequences. Some of those consequences are rejecting the principle of compositionality or rejecting the truism that understanding consists in knowledge of meaning. So a combiner does either have to bite the bullet and reject any of these highly plausible premises or he has to give up either mathematical platonism or the theory of direct reference.

7. Ontology and Models

Karlstadt: Jetzt mach mer die „Dichter und Bauer“-Overtüre.
Valentin: Die kömmer nicht machen. Weil der … weil der Pauker… der Paukist nicht da is.
Karlstadt: Jaja. Da brauch ich sie nicht dazu. Das seh’ ich selbst, dass der nicht da is!
Valentin: Nein, der ist nicht da!
Karlstadt: Joa, is ja Recht. Das SEH ich ja, dass er nicht da is!
Valentin: Wenn er nicht DA is! Der IS NICH DA heut!
Karlstadt: Ja, hern denn sie schlecht?! Ich sagte doch ebn, „das seh ich selbst, dass er nicht da ist“!
Valentin: Ja, wie kann man denn ein’n sehn, wenn er nicht da ist?
Karlstadt: Ja, wer sieht’n denn eigentlich?
Valentin: Sie.
Karlstadt: Ich hab ausdrücklich gesagt, ich seh, dass er NICHT da is! Ich kann ean doch net sehn, wenn er nicht da is.
Valentin: Des mein doch ich.
Karlstadt: Na also… Oder sehn’n sie?!
Valentin: Ah! (*Winkt ab.*) Wenn ea net da is, kann i’n net sehn. Wenn er… Ich seh ihn erst wenn er käme.
Karstadt: Wenn ea käme. Ja, der kumt nicht heut. Der… der hat heut Ausgang. Darum müssen Sie heut trommeln.

Karl Valentin *Orchesterprobe*

This last chapter takes stock of what has been accomplished in this dissertation and gives some further outlook. The first section sums up. The second section presents the outlines of a new metatheory of ontology.

7.1 Taking Stock

This dissertation has come to an end. So, it's time to conclude. In the introduction, I began with the questions, "How can it be that philosophers haven't made any progress in ontology in 2500 years?" and "Why is it that no rational consensus has been secured over any substantial proposition about what there is?". As mentioned, metaontological skepticism yields an explanation to these questions. According to the metaontological skeptics, we are engaging in a valueless discipline when we engage in ontology; we are hunting an intellectual chimera. The linguistically-motivated metaontological sceptics argue that the reason for it is the lack of cognitive significance of ontological sentences. Ontological sentences are either trivial or meaningless. But if the arguments in this dissertation are correct, any available linguistically-oriented skeptical metaontological position is seriously flawed.

The theory of quantifier variance seems to be self-defeating. Its skeptical thesis - based on the rejection of unique existential quantifier meanings - can be applied to itself, since an existential quantifier occurs in the formulation of the quantifier variantist's thesis. Thus quantifier variance is not a real skeptical metaontological position. Rather, it becomes an ontological position itself. Furthermore, the quantifier variantist cannot provide a good story of how our language and thought are connected.

Carnap's theory of metaontology is not better off. His theory falls prey to a principal problem of metaontological skepticism. All formulations of the metaontological skeptic's thesis contain negative existence claims and according to some reasonable standards those theories thereby qualify as ontological theories. Thus the theories are self-defeating.

According to Jonathan Schaffer's theory of metaontology, traditional existence questions are existence questions *per se*, which means that they do not contain any grounding information. I argued that this wrong. Traditional existence questions contain grounding information and are thus not *per se*. Furthermore, I disagreed with Schaffer that it can be established that anything exists by means of cheap arguments. Cheap arguments do not show that there are abstract objects.

7.2 Sketches of a New Account

Even though any available skeptical metaontological theory was found wanting, metaontological skepticism is not a completely unreasonable philosophical position. For instance, it is the only metaontological position that can deal with the important metaontological problem, that I started out with in the introduction: "Why is it that no rational consensus has been secured about any substantial propositions about what there is?".

Of course one could argue that this is a feature of any genuine philosophical problem. Philosophical problems are characterized by the very fact that no rational consensus has been secured about any substantial proposition about these problems. One could tend to think that philosophy doesn't make any progress. But this is not true. Counterexamples (from philosophy of language, the area which I know most about) are Gottlob Frege's treatment of statements of generality by means of quantification, the formal taming of context-sensitive phenomena in natural language inaugurated by work of David Kaplan's that legitimates us in assuming that a truth-conditional semantics of natural language might work after all, Hans Kamp's suggestions of how to understand discourse structures in natural language and Paul Grice's discovery of how to build a pragmatic dimension into the then too rigid conception of language as a purely semantical phenomenon.

As you certainly remember, in the introduction I said that the common contemporary skeptical metaontological approaches are linguistically motivated. These approaches focus on ontological sentences. As said, the main line of reasoning behind these approaches is as follows:

TRADITIONAL LINGUISTICALLY -MOTIVATED METAONTOLOGICAL SKEPTICISM

(P1) Ontological sentences are without cognitive value.
(P2) If the sentences of a supposed philosophical discipline are without cognitive value, the discipline is illegitimate/valueless.
(C) Thus, ontology is illegitimate/valueless.

But in this dissertation, I showed that there is absolutely no reason to suppose that such a linguistic thesis is true. All linguistically motivated accounts were defective in one way or another.

But is this the only possibility to construe a skeptical metaontological account? No. As I already said in the introduction other options are thinkable even though this way of construing metaontological skepticism is currently the most common way. One possibility, for instance, is an epistemologically motivated skepticism. Here is a suggestion for how the argument for such a skepticism could be developed.

EPISTEMOLOGICALLY MOTIVATED METAONTOLOGICAL SKEPTICISM

(P1') Ontology transcends evidence.
(P2') If a philosophical discipline is evidence transcendent, the discipline is illegitimate/valueless.
(C) Thus, ontology is illegitimate/valueless.

This epistemological argument for metaontological skepticism is based on the idea of ontology as an evidence-transcendent discipline. What is evidence? That of course is a difficult philosophical question. One natural suggestion is, that evidence is what justifies our beliefs. Using the concept of supervenience, we could then say that what we are justified in believing supervenes on our evidence, so that only a change in evidence can lead to a change in what we are justified in believing.
If ontology transcends evidence, this would mean that there is nothing which justifies our beliefs about what there is - an unbearable situation for a scientific discipline.
But as it stands the argument is at best only an outline of an epistemological skeptical metaontological argument. To make this argument work, one would still need to put a lot of labour into it. First, one would need to flesh out the notion of evidence much more explicitly than I did. Second, one would need to show that a philosophical argument does not supply sufficient grounds to justify beliefs into ontological positions. After all, in philosophy our beliefs are (at least most of the time[104]) justified by arguments. And then one would need to

[104] Sometimes it is perhaps justified by mathematical proofs or scientific investigations.

argue that ontology is the only philosophical discipline which is concerned by this failure of argument and explain *why* ontology is the only discipline which is concerned by it. I mean, why should other philosophical disciplines be better off?

Let me now turn to my own approach to metaontology.

In ontology we encounter a deeply dissatisfying situation. This is what I call the *argumentative stand-off* in ontology. It is not a question of argument whether we believe in abstract objects. It is a question of ideology. Arguments do neither convince any nominalist nor any platonist that his position is false. Consider Benacerraf's (1965) worries about the epistemological access to abstract objects. I don't know of any platonist who has been convinced by this argument that his position is wrong and that nominalism is true. On the other side, think of the problems that the nominalist faces in supplying us with a story about how mathematics (without sets) or how our language (without meanings) works. Nominalism borders to intellectual suicide when it comes down to giving minimally faithful accounts of these and other phenomena. No nominalist (except for Quine maybe) has been convinced of the difficulties that nominalism is wrong and platonism right. Therefore we have reached the argumentative stand-off in ontology. Borrowing a phrase from above, we could say that changes in arguments about what there is, do not lead to changes in beliefs about what there is. David and Stephanie Lewis give a nice illustration of the course of ontological disputes in their (1970). Argle, a die-hard nominalist and materialist, and Bargle, a die-hard Platonist and anti-materialist, argue about whether holes can be incorporated into a materialistic and nominalistic framework. Argle thinks, yes, Bargle, no. But after both having pulled out all the argumentative stops in their long "haggling" (1970: 212), they remain faithful to the position they started out with. The goal of ontology is to state what there is. As it seems, this goal is not achieved. Given this, one could make the following argument for metaontological skepticism:

BEST EXPLANATION ARGUMENT FOR METAONTOLOGICAL SKEPTICISM

(P1'') There is no rational consensus about any substantial proposition about what there is.
(P2'') The best explanation for there being no rational consensus about any substantial proposition about what there is, is that ontology is not able to provide such a consensus.

(P3'') If ontology is unable to provide such a consensus, it is illegitimate/valueless.
(C) Thus, ontology is illegitimate/valueless.

I don't agree with this argument, however. Even if we make the strong assumption that ontology is unable to provide such a consensus and that it is unable to find out the truth about what exists, it does not follow, that ontology is therefore illegitimate or worthless. The reason for this is as follows.
Science is striving for truth. But not always. Sometimes it is (only) building models. Building models is a not negligible, important and invaluable aspect of science and engineering. After all, the models are of the utmost importance to engineers and scientists and engineers spend in fact a lot of their time in building these models. Building the real devices, which we are using in our every day lives, is often impossible without previously having a model of these devices. For instance, engineers and scientists often give a mathematical model to specify and try to understand a hypothetical structure. Or think of the electrical engineer who has to test his circuit designs by running a computer simulation in programmes like VHDL or ABEL before he can realize them in a real circuit. Another example are toy models of ships that are used to assess the approximate displacement of the real ships. Moreover, engineers make up their minds about ideal wires with zero resistance, batteries without any internal resistance, lumped circuits with zero internal resistance, etc. They do this to be able to built real circuits, which do not contain any ideal wires, ideal batteries or zero magnetic flux per time. After all, neither ideal wires, nor ideal batteries or lumped circuits do exist physically.

Note that I am not using the term "model" in the sense of model-theory. In model theory, a model is a set-theoretical object, a set with a certain structure. In this sense a model is an ordered pair, consisting of a non-empty set and some operations defined on this set. I am using the term in a more imprecise and wider sense. According to my use, a "model is an imagined or hypothetical structure that we describe and investigate in the hope of using it to understand some more complex real-world target system or domain".[105, 106, 107]

[105] Godfrey-Smith (2009: 4). See also Giere (1988). I am very close to Godfrey-Smith.

[106] Note that this definition provokes the objection that there has to be something that is represented by the ontologist's models. After all, according to the definition, there must be a real-world target system or domain. Note, however, that the Nullator and the Norator in Circuit Theory do not represent anything in the real world as well, nontheless they are still models. Nullator and Norator are both linear and time-invariant one ports, where the "Nullator" is defined as having zero voltage and zero current across its terminals, such that its characteristic

There are different types of models used in engineering and science[108]: Some of these models are model-descriptions or collections of them, some of them are model-systems or collections of them. Here are some examples:

First. Engineers and scientists are developing thought experiments. Einstein's thought experiments are a particularly famous example. But there are also a lot of much less exciting thought experiments in the daily life of engineers, like assumptions about infinitely small conductances etc.
Second. As already mentioned, engineers often use computer simulations.
Third. The type of model engineers are interested in are real systems. It can be that one real system is built to analyse another real system. A good example is the construction of the Olympiastadion in Munich. In face of their inability to predict the static of Otto Frei's famous bubble roof, the engineers built a model of the metal skeleton of the roof's steel carriers and placed it into soap sud where they made a mould of the soapbubbles that formed themselves around the metal skeleton. It was only by means of this mould that they were able to built the roof. Another example is the 1/50 model of parts of the New Orleans canal system that the US Army Corps of Engineers built after the hurricane Katrina, to assess its stability.[109]

In ontology the same is true. Building models is a valuable and legitimate goal. So, even when ontology is principally unable to find out what exists, it is still legitimate and valuable to engage into ontology when we are building models. In fact, this makes the situation of ontology look much less dissatisfying and way more attractive. When we do not have to find out what exists, we are not seeking a rational consensus about what there is. We are building models of what exists and of the relations that the entities which exists bear to each other.

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in the u-i-plane comes down to the origin, and the "Norator" is defined as having arbitrary but non-zero voltage and current across its terminals, that its characteristic is the whole u-i-plane except the origin. Both circuit elements do not actually exist. However, there are many constraints which govern their use in actual circuit design. For instance, they can only appear together in the form of the two-port Nullor whose input is the Nullator and its output the Norator. It cannot happen that only one of them appears in a circuit or that the input-output role is switched.

[107] Note that this definition does not carry any ontological commitment to abstract entities in the form of hypothetical structures since we are free to perceive concrete senteces as such structures.

[108] Cf. Godfrey-Smith (2009: 5).

[109] The example is from Godfrey-Smith (2009: 4).

The models built by ontologists are supposed to serve as frameworks for other philosophical disciplines. The theories of other disciplines should be embedded and built into these models. As in the case of engineers, ontologists have to build good models, since much depends on these models in other disciplines. For instance, Lewisian realism about possible worlds (1986) provides a good model about how fictional discourse or about attitudes *de se*. Platonists who believe in possible worlds allow for a good model to develop a great deal of formal semantics in it.

This approach to ontology does not belittle ontology, since building good models is not at all a trivial and unimportant matter. In fact, it is pretty difficult and an engineer, who builds bad models, is seriously threatened by loosing his job. A good ontological model, for instance, could be a model which allows to develop many theories in it, or which other models do not allow, etc. So what are the models of ontology supposed to be? I think that the most natural suggestion is that they are some kind of theories.[110]

Note that I am not denying there is a true ontological theory. (Actually, I take it that there is one true ontological theory, since I think that there either are abstract objects or not. Hence, either nominalism or platonism is true. And if the platonist is right, there are certainly wrong platonistic theories about what there is. For instance, a platonistic theory postulating merely sets may be true, while a platonistic theory postulating only universals that are no sets may be wrong. And if the nominalist is right, there are certainly wrong nominalistic theories about what there is. For instance, a Goodmanian theory may well be true while Trope-Nominalism

[110] Note that it is very hard to say what an ontological theory is. After all, the ontologist just gives arguments for there being abstract objects or not. I suppose that in this sense it is quite hard to say that he builds theories. But note that in Chapter 5, I have defended a view according to which metaphysical and ontological debates are very tightly interwoven.

I quote myself: "He [Schaffer] has reminded us that people give different answers to questions like "What is it to be a number?", or "What is it to be a property?" and that these answers influence answers to questions like "Are there numbers?", or "Are there properties?" in crucial ways. Metaphysical and ontological debates are strongly knit together. Philosophers were fully aware of this when they were interested in ontological reduction. But somewhere down the road, philosophers forgot ontological reduction. And with it they forgot that we cannot answer ontological questions without having made progress in metaphysics. The boundaries of our progress in ontology are settled by the progress we make in metaphysics." This makes it easy to understand how ontologists can build models. Ontologists make a lot of metaphysical assumptions, as for instance when they choose to include individuals in their ontology in contrast to concreta, like Goodman (1950) and by the relational and structural constraints they impose on the entities that are assumed (another example is Martin (1958)). These assumptions are best captured by theories.

may be wrong.) But I think that there is *currently* absolutely no point in trying to assess which of these theories is right. There is only a point in assessing which of the models these theories make up is the best and in building further models and refining the old models.

Furthermore, note that I am not arguing that the best model corresponds to the true theory. I've actually never understood the point of this pragmatist claim if it is formulated in a certain way. Why should it be the case that the best theory is the true theory? Aristotle's theory of motion was the best theory of motion in ancient times, but it isn't today. So, Aristotle's theory was true in ancient times but is false today even though the relevant facts stayed invariant. Contradiction. Moreover, induction over the past scientific theories *seems* to show exactly the contrary to the pragmatists claim. No best scientific theory in the past has turned out to be true, and, no best scientific theory will turn out to be true. But even if the best theory is not the true theory, there can still be various models competing against each other when it comes down to strength, coherence and simplicity. But this is not what is meant with the thesis that the best theory is the true theory. What is meant is that the true theory is the best theory in the limit. And this, indeed, is a very plausible thesis.[111]

Thus dissolving ontology from the idea that its sole task is to find out what there is, and insisting that there may be something to ontology which is independent of whether this task can be fulfilled, supplies us with new insights into the nature of ontology. But, to borrow a phrase of Winston Churchill's, this dissolution is not the end of a proper understanding of ontology. It is not even the beginning of the end. But, maybe, it is the end of the beginning.

[111] Thanks to Godehard Link for pressing me to get clear on this.

APPENDIX: Triviality

A.1 Overview

As we saw, skeptical metaontological positions often involve triviality claims. According to Jonathan Schaffer's skeptical metaontological view, for instance, traditional existence questions like "Are there numbers?" and "Are there properties?" have trivial affirmative answers. In the face of this, metaontological skepticism is sometimes even labelled "Trivialism" (as for instance in Rayo (forthcoming)). But the use of the notion of triviality is not restricted to metaontology alone. We come across it in almost every other branch of philosophy, in mathematics and the sciences. In short and general, it is used in almost every discourse of theoretical and practical reasoning. Given this ubiquitousness, it is rather astonishing that there haven't been any attempts so far to explicate this notion. This gets even more astonishing once we see that it raises some interesting questions. In this Appendix I begin to fill this gap in the literature. Closing this gap is important and interesting in its own right. But it will also supply a general framework in which the claims of the various metaontological positions that are considered in this work, can be developed. My explication exploits the obvious fact that there is a dependence of triviality on lack of information and irrelevance. In section 1 I will review some examples in- and outside of philosophy where the concept of triviality plays a crucial role. Section 2 and 3 take a closer look at the role of triviality in philosophy and argue that it cannot be reduced to other notions. Among the discussed notions are necessity, logical truth and analyticity. Section 4 presents an alleged puzzzle about triviality. Section 5 develops an explication. Section 6 contains a thorough formal treatment of this explication in terms of possible worlds. Section 7 answers some objections to this explication.

A.2 Triviality and Philosophy

The notion of triviality is a crucial ingredient of many metaontological theories. Triviality in metaontology is thus our first example.

First example – Metaontology:
As we saw in this work, the notion of triviality is regularly used to undermine the legitimacy of ontology. Metaontological skepticism seeks to achieve this by arguing that ontological statements are trivial. According to a reasonable metaphilosophical assumption a scientific discipline cannot be cognitively valuable if it contains nothing but trivial statements. This is thought to be sufficient to reject any *raison d'être* of ontology. If this is true, there is no place for ontology in philosophy.

But use of the notion of triviality is not confined to metaontology alone. Rather, one can encounter this notion in almost all branches of philosophy.

Second example - Frege's Puzzle:
Frege asked how it can be that it isn't at all informative to hear, that a is a, while it is highly informative to hear, that a is b. Today this problem goes under the name *Frege's Puzzle*.[112] Even though Frege didn't use the expression "triviality" or "trivial" himself in his (1892a), the puzzle which bears his name is often construed as a problem about triviality as above. We learn it as a puzzle about triviality[113], and we are doing research with it as a puzzle about triviality.[114] It is trivial that "a=a" while it is not trivial that "a=b". While it is totally uninformative to hear that the morning star is identical with the evening star it isn't uninformative at all to hear that a is identical to b. Tightly connected with triviality is the

[112] This name first appeared in Salmon (1986).

[113] For instance Lycan (2000: 14f.) writes in his introductory book to the philosophy of language: „It seems, then, that what the statement says is simply that the person is identical with that person, that that person is identical with herself. If so, then the statement is *trivial*. Yet (3) [that is the sentence "Elisabeth Windsor = the present Queen of England") seems nontrivial, in each of two ways: first, (3) is informative, in that someone might learn something new upon reading (3); second, (3) is *contingent*…".

[114] Sider and Braun (2006: 669) write: "Over 100 years ago Frege (1892a) pointed out the problem with Millianism: sentences containing co-referential names seem semantically inequivalent. $a=b$ is trivial, *a priori*, etc.; $a=b$ is not, even if *a* and *b* have the same referent; $\phi(a)$ and $\phi(b)$ embed differently in the scope of propositional attitude verbs".

notion of cognitive significance. The notion of triviality plays a role in this example in explanation and motivation of the puzzle. A puzzle which constitutes one of the most important problems which a semantical theory has to solve.[115]

Third example - Lewis's triviality results:

David Lewis (1976) brought forward some famous triviality results against the Adam's-Stalnaker account of the probability of a conditional. According to this account the probability of a conditional was just the conditional probability, i.e. Pr(p→q) was defined as Pr(p&q)/Pr(q), where the probabilities are regarded as subjective probabilities. Adams and Stalnaker went further to claim that this captured the whole meaning of the conditional "p→q".

Against this, Lewis argued that there is no proposition that has truth-conditions matching the profile of p→q. His proof runs as follows:

(1) $Pr(p \rightarrow q) = Pr(p \rightarrow q \ \& \ q) + Pr(p \rightarrow q \ \& \ \neg q)$
(2) $Pr(p \rightarrow q) = Pr((p \rightarrow q)/q).Pr(q) + Pr((p \rightarrow q)/\neg q).Pr(\neg q)$
(3) $Pr(p \rightarrow q) = Pr(q/p\&q).Pr(q) + Pr(q/p\&\neg q).Pr(\neg q)$
(4) $Pr(p \rightarrow q) = 1.Pr(q) + 0.Pr(\neg q)$
(5) $Pr(p \rightarrow q) = Pr(q)$

Given that it is absurd that the probability of "p→q" cannot diverge from that of "q", it is trivial that no proposition can have the truth-conditions given by the conditional probability.

But the notion of triviality is not confined to philosophy alone. Rather it can be encountered in every branch of science and more generally in almost every branch of theoretical and practical reasoning.

Fourth example - Triviality in Logic:

The relation SUBST(y,x,t) is defined to hold for every variable y. Given this definition it is trivial to prove that the formula SUBST(u,x,x) holds if u is a variable. Suppose we would

[115] Dissenters include Wettstein (1986) und Almog (2008). They argue that semantical theory has to give an exact picture of the truth-conditions of sentences but not a picture of cognitive significance. Perry (2001: 8) comments aptly: "I cannot accept that a semantic theory can be correct that does not provide us with an appropriate interface between what sentences mean, and how we use them to communicate beliefs in order to motivate and explain action."

have used the following definition: SUBST(y,x,t) holds for every variable which fulfils condition C, where C narrows down the set of variables for which the relation holds. Such a condition could have prevented the proof from being trivial.

Fifth example - Trivial filters:

A filter on a nonempty set S is a collection F of subsets of S such that, the following conditions hold:

(i) $S \in F$ and $\varnothing \notin F$,

(ii) if $X \in F$ and $Y \in F$, then $X \cap Y \in F$

(iii) if $X,Y \subset S$, $X \in F$, and $X \subset Y$, then $Y \in F$.

$\{S\}$ is called a *trivial* filter. The name is not accidental. After all, $\{S\}$ is trivially a filter. Look at conditions (i) – (iii). (i) is directly fulfilled. (ii) and (iii) are vacuously true given their false antecedents.

Since the concept is so widespread in use in any discourse of reasoning and *ipso facto* in philosophy it is astonishing that triviality hasn't received any attention in the literature, so far. Almost more astonishing is the fact that a closer look at triviality reveals that triviality possesses some interesting features. But apart from this general interest in triviality, the concept plays a crucial role in metaontology. The interest of a clarification of this notion for the work is thus self-evident.

A.3 The Role and Irreducibility of Triviality

On the one hand, it is commonly assumed that arguments can be rejected outright if they are trivially wrong. According to a reasonable philosophical assumption, trivialities are philosophically not relevant and worthless, they belong to the realm of the cognitive insignificant. After all, philosophy is a highly non-trivial discipline which is interested in formulating and answering extremely difficult questions. Trivialities have no place in this picture. Triviality has to be discredited.

But on the other hand, a strong argument is an argument that starts out from trivial premises alone, but establishes a highly non-trivial conclusion. A paradox gets worse, if the contradiction making it up can be derived from trivialities alone. It seems that no ground is more solid than triviality. So why cast this notion out of philosophy?

Rather, it deserves and needs crucial investigation. The first thing we can say about triviality is, that it is a genuine phenomenon and irreducible to other philosophical notions like necessity, logical or analytic truth.

Necessity: That there are primes between 10 and 20 is both necessary and trivial. But in contrast to there being primes between 10 and 20, it is highly non-trivial but still necessary that there are arbitrarily long arithmetic progressions of prime numbers (the Green-Tao theorem). Necessary truths need not be trivial.

To see that trivial truths need not be necessary listen to the following conversation between two doctors who try to find out why there was a problem in the operation of a patient:

A: I don't have any idea why there were problems with the wall thickness of his right heart chamber?

B (literally, sincerely, seriously and without implicature): The heart is pumping blood.

A: Boohoo, that's trivial!

Even though what B said is not necessary, it is trivial *in this context*. As it is a contingent matter that the heart is pumping blood – after all, it could have also been the brain – but trivial in this context, we have an example of a trivial truth which is not necessary. So, trivial truths need not be necessary. By the way, this example also shows that triviality is not the same as logical truth.

Logical Truth. A simple logical truth like

(1)

$\varphi\rightarrow\varphi$

is certainly trivial. But a cumbersome and long winded logical truth like

(2)

$((((\varphi\rightarrow\psi\rightarrow\chi)\rightarrow((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi)))\rightarrow((\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow((\varphi\rightarrow\psi\rightarrow\chi)\rightarrow((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi))))\rightarrow((\varphi\rightarrow((\varphi\rightarrow\psi)\rightarrow\psi))\rightarrow(\varphi\rightarrow(\psi\rightarrow\varphi))))\rightarrow((((\varphi\rightarrow\psi\rightarrow\chi)\rightarrow((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi)))\rightarrow((\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow((\varphi\rightarrow\psi\rightarrow\chi)\rightarrow((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi))))\wedge(\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow(\varphi\rightarrow((\varphi\rightarrow\psi)\rightarrow\psi)\rightarrow(\varphi\rightarrow(\psi\rightarrow\varphi)))$

is not at all trivial. Just compare, (2) with (3):

(3)

$(((\varphi\rightarrow\psi\rightarrow\chi)\rightarrow(((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi)))\rightarrow((\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow((\varphi\rightarrow\psi\rightarrow\chi)\rightarrow((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi))))\rightarrow((\varphi\rightarrow((\varphi\rightarrow\psi)\rightarrow\psi))\rightarrow(\varphi\rightarrow(\psi\rightarrow\varphi))))\rightarrow((((\varphi\rightarrow\psi\rightarrow\chi)\rightarrow((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi)))\rightarrow((\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow((\varphi\rightarrow\psi\rightarrow\chi)\rightarrow((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi))))\wedge(\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow(\varphi\rightarrow((\varphi\rightarrow\psi)\rightarrow\psi)\rightarrow(\varphi\rightarrow(\psi\rightarrow\varphi))),$

where one open bracket sign "(" before the antecedent of the first logical truth "$(\varphi\rightarrow\psi\rightarrow\chi)$" was omitted and one was added to the second "$((\varphi\rightarrow\psi)\rightarrow(\varphi\rightarrow\chi))$". (3) is not a logical truth. (2) and (3) are certainly not trivially true.
Compare (2) with (4):

(4)

$(((((\exists x\varphi)\rightarrow\psi\rightarrow\chi)\rightarrow(((\exists x\varphi)\rightarrow\psi)\rightarrow((\exists x\varphi)\rightarrow\chi)))\rightarrow((\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow(((\exists x\varphi)\rightarrow\psi\rightarrow\chi)\rightarrow(((\exists x\varphi)\rightarrow\psi)\rightarrow((\exists x\varphi)\rightarrow\chi))))\rightarrow((\varphi\rightarrow((\varphi\rightarrow\psi)\rightarrow\psi))\rightarrow(\varphi\rightarrow(\psi\rightarrow\varphi))))\rightarrow(((((\exists x\varphi)\rightarrow\psi\rightarrow\chi)\rightarrow(((\exists x\varphi)\rightarrow\psi)\rightarrow((\exists x\varphi)\rightarrow\chi)))\rightarrow((\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow(((\exists x\varphi)\rightarrow\psi\rightarrow\chi)\rightarrow(((\exists x\varphi)\rightarrow\psi)\rightarrow((\exists x\varphi)\rightarrow\chi))))\wedge(\varphi\rightarrow(\psi\rightarrow\varphi))\rightarrow(\varphi\rightarrow((\varphi\rightarrow\psi)\rightarrow\psi)\rightarrow(\varphi\rightarrow(\psi\rightarrow\varphi)))\rightarrow\perp,$

(4) is a logical falsehood, but certainly not trivially so. In fact, we need a proof to determine the logical status of sentences (1) – (3). (Mind you, monadic first-order logic is decidable)

Analyticity. “All bachelors are bachelors” is a paradigm example for an analytic truth. Coincidentally, it is also trivial. “All bachelors are unmarried men” is also a paradigm example of an analytic truth. It is not trivial, however. Just remember Frege’s Puzzle from the beginning where it was argued that “a=a” is trivial but “a=b” not, since one could learn something when one hears it. By the same pattern, it can be argued that “All bachelors are bachelors” is trivial, but “All bachelors are unmarried men” is not, since we can learn something new, when we hear it. Just consider the sentence “Every vixen is a female fox”. These sentences are often called trivial. Jeff King, (1998: 21), for example writes:

> “However it is important to see that they will always seem trivial to the members of the linguistic community ... For any speaker competent with the word is, in virtue of that competence, able to produce such a philosophical analysis or recognize that another has produced one. Thus, for example, the analysis **expressed by is trivial.”**

And Timothy Williamson (2006: 2) calls these sentences “boring trivialities”. Just consider Williamson’s example “Every vixen is a female fox”. Actually, I learned something from this alleged triviality. I learned from it that “vixen” means “female fox”. So, if we accept Frege’s puzzle - that relies on the assumption that the standard of triviality is that one *cannot* learn something from a triviality - as a genuine puzzle, then “Every vixen is a female fox” is certainly not trivial.

An example which shows that trivial truths do don’t have to be analytic is the prime number example from above.
To put it in a nutshell, triviality can be neither reduced to necessity, nor logical truth, nor to analyticity. It is a genuine phenomenon.

A.4 Dilemma

Triviality is torn between two incompatible looking aspirations: subjectivity and objectivity.

A.4.1 One Side

On the one side, it seems that whether or not something is trivial is a decidedly subjective matter. It is commonly granted that our disposition to ascribe the predicate "trivial", that is what we classify as trivial, crucially depends on who we are, and on how our cognitive endowments and abilities are. We admit that there are people who are cleverer than us. If Gödel, for instance, doesn't carry out a proof on the board since he thinks that it's not worth the efforts, in face of its being trivial, his students do not have to concur. They might still want a proof. If Gödel publishes a paper in which he omits some steps of a proof since he thinks that these steps are trivial, not all readers of this paper might agree with him on this. In fact, most of them might have serious problems to fill in the missing steps on their own. This appearance perfectly matches the often encountered view that "it is trivial" is just another way of saying "it is very easy". According to this view, a trivial inference is an inference that is very easy to draw, and a trivial proposition is one which is very easy to understand a self-evident proposition. It clearly stands that what is easy for someone need not be easy for someone else. And of course, what is easy for someone is a matter of his cognitive abilities and endowments. If driving five people in a four wheeled car, for three thousand miles on two wheels is very easy for Mr. Gogo, it need not be *very* easy, let alone easy, for me. In fact, it might be almost – in the relevant sense – impossible. If it is pretty easy for a memory artist to enumerate four hundred positions of π after the decimal point, again, I need not agree. If some problem has a certain degree δ of complexity, for some people δ is pretty easy, for others it is pretty hard.

But is this all there is to triviality? Is it really such a subjective matter whether something is trivial or not? If so, then, how are we ever able to argue with someone about whether something is trivial? According to this view, we can do nothing more than to speak past each other. Given that the dispositions to assent to sentences of the form "x is trivial" differ so much from speaker to speaker, and these dispositions manifest the use and thereby also the meanings of the expressions in our language, speakers use just different words. Insofar they

could only argue about whether "trivial$_1$" or "trivial$_2$" can be ascribed to something. We are just arguing about which words to use. The dispute is merely verbal.
Moreover, how can it be that something which is to such a large extent subjective, be relied upon in philosophical argument? How can it be that something which is to such a great extent dependent on one's cognitive faculties, is so often used in the justification of a premise or an inference?

A.4.2 The Other Side

These considerations cast doubt on the view that triviality is something subjective. But there is another side of the coin. According to this idea, triviality is an absolute and objective feature of the world. If a proposition or an inference is trivial, this seems to be a property of the proposition or the inference and not of us who entertain the proposition and draw the inference.[116] There is a perfectly legitimate sense, in which we want to be able to say that something is trivial in this absolute and objective sense. We think that there is some objective fact of the matter of whether something is trivial. We are not speaking past each other when we are ascribing the predicate "trivial" to something. Even if it is taken for granted that triviality is a subjective notion, we do not think that we apply another predicate when we say "That's trivial" than Gödel when he says "That's trivial".
I don't agree with these arguments. Triviality is a subjective phenomenon. Consider easiness. Easiness is obviously a subjective phenomenon. But even though easiness is a subjective phenomenon, this does not prevent is in still being able to argue with someone else, whether something was really easy. Furthermore, one is not committed to claim that there are different concepts of easiness "easy$_1$", "easy$_2$", …, relativized to various agents. There is still one concept of easiness.
In the next section, I will suggest an analysis that locates triviality in the subjective realm.

[116] As was said above "trivial" is just elliptical for "trivially true" or "trivially false". Syntactically, in the complex expression "trivially true/false", "trivial" functions as a verbal modifier. If we take this syntactic fact at face value on a semantical level, it could be argued that triviality is a property of truth. And that this shows that triviality must be some objective feature of the world. I disagree on both counts. The expression "seemingly" has the same syntactic function – verbal modifier – in "seemingly true" as "trivially" in "trivially true". But it would be absurd to claim that this shows that seemingly expresses a property of truth even if we take the syntactic fact at face value on a semantic level. Furthermore, it does not show that what is expressed by "seeming" (if there is something at all!) is an objective feature of the world.

A.5 The Explication

According to the view I suggest triviality is explicated by means of information. The idea is, of course, as follows: Lack of information is a necessary condition for a proposition to be trivial. So any theory faithful to the notion of triviality must take this condition into account.

Information can be represented in a possible worlds framework. This idea was developed by Robert Stalnaker in numerous papers among them his (1974), (1998) and (2002). But note that using possible worlds in an explication of information is only one way of explicating information. In fact, it is a question of considerable debate what information is (Cf. Floridi (2004)) and as a consequence there are numerous conceptions of information and perspectives from which information can be viewed. Information can be viewed for instance as a part of reality, or as about reality.

First. The communication theory approach of Wiener (1949) and Shannon (1948) defines information as a part of reality in terms of probability space distribution.

Second. Dretske (1981) defines information about reality (not as a part of reality), that is semantic information, in terms of probability space and the inverse relation between information in p and probability of p.

Third. Barwise and Perry (1981) define semantic information in terms of states space and consistency.

Fourth. Floridi (2004) prefers a definition of semantic information in terms of a space of well-formed, truthful and meaningful data.

I used the possible worlds approach, for a theoretical and a practical reason. From a practical point of view it is the most workable and suggestive for my purposes. From a theoretical point of view, it is the most general framework. In fact, it is so flexible and general that other approaches as, for instance, the Shannon or the Dretske approach can be developed in it.

Stalnaker introduced his possible worlds framework to account for the apparent non-compositionality of presupposition projection. In this framework, the context in which a speech act takes place is defined with a set of possible situations or possible worlds – the situations that are compatible with the information of an agent. Stalnaker calls this set the *context set*. The context set will include all the situations among which the agents intend to distinguish with their speech acts. The presumed common information or what he also calls *common ground* – that is what is presupposed in the context – is what is understood as a

proposal to alter the context by adding the information that is the content of the assertion to the body of information that defines the context. Or equivalently, by eliminating from the context set – the set of possible worlds available for agents to distinguish between – those possible worlds in which the proposition expressed in the assertion is false. As he nicely puts it:

> "Every proposition, relevant or not, that is taken for granted by the participants in a conversation will be true in all of the possible worlds that define the context. Since speakers in the actual worlds take for granted when they are talking that they are talking, they will be talking not only in the actual world, but also in each of the possible worlds that define their context. Within each possible world in the context set, a discourse is taking place, and it has a context represented by its own context set. If, when talking about American politics, I take for granted that Bill Clinton is the President, then it will be true in each possible world in the relevant context, not only that Bill Clinton is President, but also that I am, in that world, taking for granted that he is. Facts about what is presupposed in a context are not only facts about the actual world in which the discourse is taking place (which may or may not be a member of the context set), but also facts about the worlds that define the context" (Stalnaker (1998: 99)).[117]

Speech acts with trivial contents do not add anything to the body of information available to the agent. They do not enable the participants to eliminate some of the worlds they are considering. From an informational point of view a speech act with trivial content is treated as if the speech act never had occurred. This is due to the fact that an agent does not receive any new information when he is confronted with a trivial speech act. An agent cannot further distinguish among these worlds.

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[117] Stalnaker championed the subjectivity of context. The objective notion of context was influenced by David Kaplan. This context was the *context of the utterance,* the context available from a particular perspective. That perspective might be as simple as the beliefs of a single participant, or else as complex as the mutual presuppositions of all participants.

The subjective notion of discourse is thought to be connected with the effect of linguistic context on interpretation. In addition to being dependent on factors of the external environment, the interpretation of a sentence is affected by the sentences that have come before it.

A standard example is as follows. Dice are rolled. You are supposed to predict what the outcome is. If you do not receive any information about the actual outcome, you consider all those worlds where the outcome was 1, all those worlds where the outcome was 2, all those worlds where the outcome was 3, all those worlds, where the outcome was 4, all those worlds, where the outcome was 5, and all those worlds, where the outcome was 6. This changes when someone tells you that the outcome was an even number. Now you are only considering those worlds, where the outcome was 2, those worlds, where the outcome was 4, and those worlds, where the outcome was 6. You excluded all those possible worlds where the outcome was 1, 3 or 5.

But if you are now being told that the outcome was the number that was the actual outcome, you cannot further exclude any possible worlds. You are still considering all those possible worlds, where the outcome was 2, 4, or 6.

When we communicate, we want to convey information, want to communicate our beliefs and our knowledge to other people and want to be able to receive beliefs and knowledge from other people. Sentences expressing trivial propositions are entirely useless for achieving this goal.[118] When we are inferring, we want to increase our knowledge. If inferences are supposed to extend our knowledge of a given domain, trivial inferences do nothing to help us in achieving this goal.[119] After all, trivial inferences lack information and thus do not contribute anything to extend our knowledge of a given domain. Thus, trivial inference is useless.

From a formal point of view, the possibilities among which the agent wants to distinguish, can be regarded as the cells of a partition of the set of possible worlds. The worlds which he considers, correspond to cells in this partition. As was said, if something is trivial for an agent, the sentence which contains this proposition does not enable the agent to further distinguish between some possibilities. In other words, we can say that a partition is unchanged. It remains the same partition.

In other words we can express this as follows: Some proposition p is trivial for an agent, if in all and only the worlds he considers this proposition holds (see section A.6).

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[118] Of course, we can use trivialities to communicate non-trivial content to others. Consider for instance the sentence “War is war”. According to Grice (1967), this sentence contains information. However, I won’t discuss this in any further deep. The pragmatic dimension of trivial sentences is not in the least relevant for my discussion. Quite the opposite, I want to shed some light on the uselessness of triviality.

[119] Cf. Williamson (2000)

So let me now give you some examples for applications of my explication.
Metaontology. According to Schaffer's theory discussed in chapter 5, it is trivial that there are abstract objects. Thus the information whether there are numbers does not enable agents to further distinguish among possible worlds in general reasoning. That sounds pretty bold.

Frege's Puzzle. "a=a" does not enable an agent, who wants to know what a is, to distinguish among further possibilities. The agent has to consider all possibilities. Thus he is in the initial information set, that I will call 'I_0' (see section A.6). But hearing that a=b does enable the speaker to further distinguish among possibilities. He is now able to exclude these possibilities where a does not equal b. Thus he does no longer consider all possibilities and is therefore no more in I_o.

Analytic truth. If the sentence "Every vixen is a female fox" is trivial for an agent, he is not able to further distinguish among possibilities. But what happens if "Every vixen is a female fox" is not trivial to an agent? This can be the case, when an agent learns the meaning of "vixen". Then the agent is able to further distinguish among possible worlds. He can exclude all possibilities, where "vixen" is not synonymous to "female fox". So in the case of trivial analytic truths, the agent has to have enough linguistic competence to understand the constituent expressions. To claim that all analytic truths are trivial is to demand too much of the linguistic competence of speakers.

It is pretty trivial whether there are primes between 10 and 20. But is it still trivial whether there are primes between 10^{10} and 20^{10}? When does something start to be trivial? Is it easy to say when something is trivial? Probably not. The reason is that the predicate "trivial" is vague. A standard test for showing that a given predicate is vague, examines whether it is Sorites-susceptible.
Here we go. Is it trivial that there is a prime number between 1+ε and 100+ε, where $\varepsilon \in \Upsilon$ and $0 < \varepsilon < 0{,}1$. It is trivial whether there is a prime number between 1+ε+ε and 100+ε+ε. And it is trivial whether there is a prime number between 1+ε+ε+ε and 100+ε+ε+ε, etc. So, it seems reasonable to state the following induction hypothesis:
(P1) If it is trivial that there is a prime number between n and n+100, then it is trivial that there is a prime number between n+ε and n+100+ε, where $\varepsilon \in \Upsilon$ and $0 < \varepsilon < 0{,}1$.
Together with the already cited assumption, that
(P2) It is trivial that there is a prime number between 1+ε and 100+ε,

we can infer that:

(C1) It is trivial that there is a prime number between 10^{20} and 10^{20}+100.

But this is clearly false. (C1) is highly non-trivial.

On the other hand, it is non-trivial whether there is a prime number between 10^{20} and 10^{20}+100. It is non-trivial whether there is a prime number, between 10^{20}-ε and 10^{20}+100-ε, where ε is as above, etc. So, it seems reasonable to state the following induction hypothesis:

(P3) If it is trivial that there is a prime number between n and n+100, then it is trivial that there is a prime number between n-ε and n+100-ε, where $\varepsilon \in \Upsilon$ and $0 < \varepsilon < 0{,}1$.

Together with the assumption that

(P4) There is a prime number, between 10^{20}-ε and 10^{20}+100-ε,

we get the following wrong conclusion:

(C2) It is trivial whether there is a prime number between 1 and 100.[120]

So, the predicate "trivial" passes the Sorites-test for vagueness. Thus, it is a vague predicate. This means that there is no sharp cut-off which allows to distinguish trivial from non-trivial propositions.

A.6 A Formal Model for Triviality

To express the notion of triviality precisely, we take a modal propositional language $\mathcal{L}$. Assume that we have a group of n agents named 1,…,n. For simplicity, we assume that these agents wish to reason about a world that can be described in terms of a nonempty set Φ of primitive propositions p, p', q, q',… As is standard, these propositions stand for basic facts about the world such as "Munich is a city" and "Sam is hungry". To express statements like "Agent i knows that φ" and "It is trivial for agent i that φ" respectively, we enrich the language by the modal operators $K_1,\ldots,K_n$ and $T_1,\ldots,T_n$ (one for each agent), respectively. $\mathcal{L}$ is just the set of formulas which contains the primitive propositions and is closed under negation, entailment and the modal operators. $\mathcal{L}_0$ is the fragment of $\mathcal{L}$ that does not contain any modal operators. $\mathcal{L}_K$ is the modal fragment of $\mathcal{L}$ without triviality operators.

[120] The prime number example goes back to a suggestion of Godehard Link's in the Logik Oberseminar. Link also applies this test in his (1998) to show that plural expression do not denote any special plural objects. However, I doubt that the argument establishes this.

For the semantics we use the familiar Kripke structures. Those structures were first introduced in Kripke's seminal (1959). A Kripke structure M for n agents over Φ is a tuple $(S,\pi,\mathcal{K}_1,\ldots,\mathcal{K}_n)$, where S is a non-empty set of possible worlds, π is an interpretation which associates with each world in S a truth assignment to the primitive propositions in Φ (i.e. $\Phi:\pi(s)\rightarrow\{$**True**, **False**$\}$) for each world $s\in S$, and $\mathcal{K}_i$ is a binary relation on S, that is a set of pairs of S.

The binary relation $\mathcal{K}_i$ is intended to capture the possibility relation according to agent i: $(s,t)\in\mathcal{K}_i$ iff agent i considers world t possible, given his information in world s. Let $\mathcal{K}_i$ be an equivalence relation.

So here is our proposal for an explication of knowledge and triviality in terms of possible worlds:

(1) $(\mathsf{M},s) \models K_i\varphi$ if and only if $(\mathsf{M},t)\models\varphi$ for all t such that $(s,t)\in\mathcal{K}_i$.

(2) $(\mathsf{M},s) \models T_i\varphi$ if and only if $(\mathsf{M},t)\models\varphi$ for all and only all t such that $(s,t)\in\mathcal{K}_i$

$(\mathsf{M},s) \models K_i(\varphi\wedge\psi)$ if and only if $(\mathsf{M},t)\models\varphi$ for all t such that $(s,t)\in\mathcal{K}_i$ and $(\mathsf{M},t)\models\psi$ for all t such that $(s,t)\in\mathcal{K}_i$. Obviously, if φ is trivial for agent i, then i knows that φ.

In the text, the triviality explication was sketched by means of possibilities among which agents want to distinguish and which they can iteratively eliminate. Formally, this idea is best captured by means of Aumann structures. The name of these structures traces back to Robert Aumann, who introduced them in his (1976).[121] Now Aumann structures are defined as:

$$(S,\mathcal{P}_1,\ldots,\mathcal{P}_n).$$

Aumann structures are like Kripke structures, with two differences: S is still a set of possible worlds, but, first, there is no analogue to the π function, since in the event-based approach, there are no primitive propositions. The second difference is that to define what worlds agent i considers possible, in Aumann structures there is a partition $\mathcal{P}_i$ of S for each agent I, rather than using a binary relation $\mathcal{K}_i$. If $\mathcal{P}_i = \{S_{i1},\ldots,S_{in}\}$, then the sets S_{ij} are called the cells of the partition $\mathcal{P}_i$, or the information sets of agent i. The intuition is that if S_{ij} is an information set of agent i, and if $s\in S_{ij}$, then the set of states the agent i considers possible (which corresponds

[121] See Fagin, Halpern, Moses, and Vardi (1995). I am very close to their treatment of Aumann structures here.

to the information of agent i) is precisely S_{ij}. P is the row vector $(P_1,\ldots,P_n)^T$, consisting of the column vectors $P_i = (S_{i1},\ldots,S_{in})$. The initial information set $I_0 = S$.

Formally, given an Aumann structure $(S,\mathcal{P}_,\ldots,\mathcal{P}_n)$, we define knowledge and triviality operators K_i: $2^S \to 2^S$, and T_i: $2^S \to 2^S$, for agent i=1,…,n, respectively, as follows:

(12) $$K_i(e) = \{s \in S: \mathcal{P}_i(s) \subseteq e\} \text{ and}$$

(13) $$T_i(e) = \{s \in S: \mathcal{P}_i(s) = e\}.$$

$K_i(e)$ and $T_i(e)$ are itself events, namely the *event of agent i knowing e* and the *event e being trivially true for agent i*. Thus, if event e is trivial for agent i, i knows that e.

We can now define an event $ev_M(\varphi)$ for each formula φ by induction on the structure of φ:

1. $ev_M(p) = e_p^M$, where e_p^M, is the event that p is true, for each primitive proposition p (i.e. $e_p^M = \{s \in S: (M,s) \models p\}$).
2. $ev_M(\varphi \wedge \psi) = ev_M(\varphi) \cap ev_M(\psi)$
3. $ev_M(\neg\varphi) = S - ev_M(\varphi)$
4. $ev_M(K_i\varphi) = K_i(ev_M(\varphi))$
5. $ev_M(T_i\varphi) = T_i(ev_M(\varphi))$

Now we will show that we can go from any Kripke structure to a corresponding Aumann structure. We get the corresponding Aumann structure $A^M = (S,\mathcal{P}_1,\ldots,\mathcal{P}_n)$ of a Kripke structure M (with the same set S of states) by taking $\mathcal{P}_i$ to be the partition corresponding to $\mathcal{K}_i$.

PROPOSITION I Let M be a Kripke structure where each possibility relation $\mathcal{K}_i$ is an equivalence relation, and let A^M be the corresponding Aumann structure. Then for every formula φ, we have $\varphi^M = ev_M(\varphi)$, where $\varphi^M = \{s \in S: (M,s) \models \varphi\}$.

PROOF by induction on the structure of the formulas

1. $p \in \Phi$: $p^M = \{s \in S: (M,s) \models p\} = e_p^M = ev_M(p)$.
2. $(\varphi \wedge \psi)^M = \{s \in S: (M,s) \models \varphi \wedge \psi\} = \{t \in S: (M,t) \models \varphi\} \cap \{u \in S: (M,u) \models \psi\}$ = (by induction hypothesis) $ev_M(\varphi) \cap ev_M(\psi) = ev_M(\varphi \wedge \psi)$

3. $(\neg\varphi)^M = \{s \in S: (M,s) \models \neg\varphi\} = \{s \in S: (M,s) \models \varphi\} = S - \{t \in S: (M,t) \models \varphi\}$ = (by induction hypothesis) $S - ev_M(\varphi) = ev_M(\neg\varphi)$

To prove the desired condition for the K_i and T_i operator, we use the following lemma (where $O \in \{K, T\}$).

LEMMA: $s \in O_i(ev_M(\varphi))$ holds in A $\Leftrightarrow (M,s) \models O_i\varphi$.

PROOF

$s \in O_i(ev_M(\varphi))$ holds in A $\Leftrightarrow s \in ev_M(O_i\varphi)$ holds in A $\Leftrightarrow$ (by correspondence) $s \in \{x \in S: (M,x) \models O_i\varphi\} \Leftrightarrow (M,s) \models O_i\varphi$.

Q.E.D

PROOF

4. $(O_i\varphi)^M = \{s \in S: (M,s) \models O_i\varphi\}$ = (by LEMMA) $= ev_M(O_i\varphi)$.

Q.E.D

We have just shown that for any Kripke structure there is a corresponding Aumann structure to which we can go to from the Kripke structure. What about the other direction? Is there for any Aumann structure a corresponding Kripke structure? Yes.

To see this, assume that we are given not only an Aumann structure A but also an arbitrary set Φ of primitive propositions and an arbitrary function π that associates with each state in S a truth assignment to the primitive propositions in Φ. We need these additional constraints since Aumann structures normally do no presuppose some set of propositions and no truth assignment to some subset of the set of propositions. However, the events that are worked with in Aumann structures can be given names. These names correspond to the primitive propositions. By the assumption that it is clear when these events hold, we get the truth assignment.

So let A be the Aumann structure $(S, \mathcal{P}_1, \ldots, \mathcal{P}_n)$, and define $M^{A,\pi} = (S, \pi, \mathcal{K}_1, \ldots, \mathcal{K}_n)$ as the Kripke structure, where $\mathcal{K}_i$ is the partition corresponding to $\mathcal{P}_i$, for i=1,…, n. (Generally, it holds that $\mathcal{R}$ is an equivalence relation that we obtain from partition $\mathcal{P}$ if and only if $\mathcal{P}$ is a partition that we obtain from equivalence relation $\mathcal{R}$.)

PROPOSITION II *The* Aumann structure corresponding to the Kripke structure $\mathsf{M}^{A,\pi}$ is A.
PROOF

Let A = (S, $\mathcal{P}_1$,…, $\mathcal{P}_n$) be an Aumann structure. Suppose that $\mathsf{M}^{A,\pi}$ and $\mathsf{M}_2^{A,\pi}$ = (S, π, $\mathcal{K}'_1$,…, $\mathcal{K}'_n$) are Kripke structures corresponding to A. Then $\mathsf{M}_2^{A,\pi} = \mathsf{M}^{A,\pi}$, since $\mathsf{M}_2^{A,\pi}$ and $\mathsf{M}^{A,\pi}$ have the same set S of states, the same function π, and $\mathcal{K}_i = \mathcal{K}'_i$ for all i∈ {1,…,n}, since $\mathcal{K}_i$ and $\mathcal{K}'_i$ both correspond to $\mathcal{P}_i$ for all i∈ {1,…,n}. Q.E.D

Thus by PROPOSITION 1, the intensions of formulas in $\mathsf{M}^{A,\pi}$ and the events corresponding to these formulas in A coincide.

PROPOSITION 1 and PROPOSITION 2 establish the close connection between the logic-based and event-based approaches that was claimed previously.

Together with a calculus, we could now prove completeness for the Aumann structure by proving completeness for the Kripke structures. However, to give the rules making up this calculus is not really easy since triviality turns out to be very instable and elusive when inferences are involved (see Sec. 5.2.1).

A.7 Objections

A.7.1 Context and Triviality

The first worry with which I am concerned, attacks my assumption that triviality does not have the potential to alter the context set. A proponent of such a view argues as follows.

In your discussion you cite Stalnaker who says the following:

> "We can represent the information that defines the context in which the speech act takes place with a set of possible situations or possible worlds – the situations that are compatible with the information. This set, is the set

> which Stalnaker (1974) called the context set will include all the situations among which the speakers intend to distinguish with their speech acts. The presumed common information – what is presupposed in the context – is what understood as a proposal to alter the context by adding the information that is the content of the assertion to the body of information that defines the context, or equivalently, by eliminating from the context set – the set of possible worlds available for speakers to distinguish between – those possible worlds in which the proposition expressed in the assertion is false"

It comes out true for Stalnaker, that a trivial assertion does not alter the context since it does not add any (new) information to the body of information that is the context. Here is the worry then. Uttering trivialities can sometimes have a deep and devastating impact on the context. Trivial utterances are revealing of the perspective from which the utterance is made and they can have a dramatic impact on how current and future utterances are perceived. For instance, utterances of someone who sometimes seriously and with no pragmatic content in mind utters trivialities are no more taken so serious and thought about carefully, etc. This is something they have in common with utterances of expressives like “shit” and “damn”. But if trivial assertions can and in fact do alter the context, this casts doubt on Stalnaker’s definition of context in terms of information.
One can answer this objection by introducing two kinds of context. A descriptive and an expressive one. This is done in Kaplan’s and Potts’s work on expressives.[122] A descriptive context is the context that tightly interacts with the truth-conditions of sentences. In contrast, the expressive context is completely independent of the truth-conditional context. This context keeps track of the emotive states of the conversational participants. Utterances of trivialities then do not alter the descriptive but only the expressive context. I other words, the fact that someone utters trivialities does not have any impact on the truth-conditions of sentences but only on the emotive states of participants. Stalnaker's context set is then to be identified with the descriptive context.

[122] Kaplan (2005), Potts (2007)

A.7.2 Triviality and Necessary Propositions

The second objection is as follows. Imagine a mathematician who ponders about whether a non-trivial theorem holds. Suppose that the theorem holds. Now, in all the worlds which he considers (which is a subset of all possible worlds), the non-trivial theorem holds. After all, the theorem is necessary. So it seems that the theorem is trivial for the mathematician. But this contradicts the assumption that the theorem was supposed to be non-trivial.

We can answer this objection by pointing out that the mathematician does not consider all and only the worlds in which this theorem holds. That is, absolutely all possible worlds. He also considers some impossible worlds in which the theorem does not hold. If the theorem is true, then it is necessarily true, that is, it is true in all possible worlds. So the only worlds in which it does not hold are impossible worlds.

Nothing in the framework introduced in this Appendix, prohibits incorporating impossible worlds.[123] An impossible worlds structure M is a is a tuple $(S, W, \sigma, \mathcal{K}_1,\ldots,\mathcal{K}_n)$, where $W \subseteq S$ is the set of possible worlds and $S \backslash W$ is the set of impossible worlds, σ is a syntactic assignment that behaves standardly on possible states such that if $s \in W$, then

$\sigma(s)(\varphi \wedge \psi) = 1$ iff $\sigma(s)(\varphi) = 1$ and $\sigma(s)(\psi)=1$

etc.

but can behave arbitrarily on $S \backslash W$.

Of course, $(S, \mathcal{K}_1,\ldots,\mathcal{K}_n)$ is a Kripke frame as above.

[123] See Fagin, Halpern, Moses and Vardi (1995: 385).

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