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Philosophy of Mathematics – Set Theory, Measuring Theories, and Nominalism

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Contents

Representationalism and Set-Theoretic Paradox

Douglas Patterson

Abstract
I defend the "settist" view that set theory can be done consistently without any form of distinction between sets and "classes" (by whatever name), if we think clearly about belief and the expression of belief—and this, furthermore, entirely within classical logic. Standard arguments against settism in classical logic are seen to fail because they assume, falsely, that expressing commitment to a set theory is something that must be done in a meaningful language, the semantics of which requires, on pain of Russellian paradox, a more powerful set theory. I explore the consequences of this response to the standard argument against "classical logic settism" for our notion of belief, and argue that what is revealed is that representationalist theories of belief cannot be right as long as it is possible to believe that every set is self-identical.

For a number of years now I have been developing an account of the semantic paradoxes along the following lines (2007a, b, forthcoming). Consider a sentence like Liar, which reads "Liar is not true". Apparent truths about meaning—e.g. that "Liar" refers to "Liar is not true", that "is not true" applies to true sentences, that meaningful declarative sentences are true when what they say is the case—jointly imply a contradiction in the presence of sentences like Liar. My view is that what this shows is that the semantics of natural language that speakers of natural language are inclined to believe is simply logically false. The centerpiece of the strategy is an argument that communication requires only that speakers cognize the *same* semantic theory, rather than that they cognize a *true* one, so that we can account for everything simply by allowing that the paradoxes show that the cognized theory is in fact untrue.

As long as I have been at this I have had it in mind that it would be interesting to consider whether a view of my sort could be put to work in addressing the set theoretic paradoxes and related problems about unrestricted quantification—interesting because there are significant enough differences that the account of the semantic paradoxes cannot simply be transferred over without modification. Two major differences are these. First, the account of the semantic paradoxes rests on a thoroughgoing non-factualism about meaning in natural language, while I have no interest in being a non-factualist about sets. Second, the account of the semantic paradoxes works by explaining how beliefs about

Who's Afraid of Inconsistent Mathematics?

Mark Colyvan

Abstract
Contemporary mathematical theories are generally thought to be consistent. But it hasn't always been this way; there have been times in the history of mathematics when the consistency of various mathematical theories has been called into question. And some theories, such as naïve set theory and (arguably) the early calculus, were shown to be inconsistent. In this paper I will consider some of the philosophical issues arising from inconsistent mathematical theories.

1. A Five Line Proof of Fermat's Last Theorem

Fermat's Last Theorem says that there are no positive integers x, y, and z, and integer $n > 2$, such that $x^n + y^n = z^n$. This theorem has a long and illustrious history but was finally proven in the 1990s by Andrew Wiles. Despite the apparent simplicity of the theorem itself, the proof runs over a hundred pages, invokes some very advanced mathematics (the theory elliptic curves, amongst other things), and is understandable to only a handful of mathematicians.[1] But now consider the following proof.

Fermat's Last Theorem (FLT): There are no positive integers x, y, and z, and integer $n > 2$, such that $x^n + y^n = z^n$.

Proof: Let $\mathcal{R}$ stand for the Russell set, the set of all sets that are not members of themselves: $\mathcal{R} = \{x : x \notin x\}$. It is straightforward to show that this set is both a member of itself and not a member of itself: $\mathcal{R} \in \mathcal{R}$ and $\mathcal{R} \notin \mathcal{R}$. Since $\mathcal{R} \in \mathcal{R}$, it follows that $\mathcal{R} \in \mathcal{R}$ or FLT. But since $\mathcal{R} \notin \mathcal{R}$, by disjunctive syllogism, FLT.

This proof is short, easily understood by anyone with just a bit of high-school mathematics. Moreover, the proof was available to mathematicians well before Wiles' groundbreaking research. Why wasn't the above proof ever advanced? One reason is that the proof invokes an inconsistent mathematical theory, namely, naïve set theory. This theory was shown to be inconsistent toward the end of the 19th century. The most famous inconsistency arising in it was a para-

1 See S. Singh, *Fermat's Last Theorem: The Story of a Riddle that Confounded the World's Greatest Minds for 358 Years,* London 1997, for a popular account of Fermat's Last Theorem.

Logical and Semantic Puritiy

Andrew Arana

Abstract
I distinguish two different views on what makes a proof of a theorem 'pure', firstly by characterizing them abstractly, and secondly by showing that in practice the views differ on what proofs qualify as pure.

Many mathematicians have sought 'pure' proofs of theorems. There are different takes on what a 'pure' proof is, though, and it's important to be clear on their differences, because they can easily be conflated. In this paper I want to distinguish between two of them.

I want to begin with a classical formulation of purity, due to Hilbert:

> In modern mathematics one strives to preserve *the purity of the method*, i.e. to use in the proof of a theorem as far as possible only those auxiliary means that are required by the content of the theorem.[1]

A pure proof of a theorem, then, is one that draws only on what is "required by the content of the theorem".

I want to continue by distinguishing two ways of understanding "required by the content of [a] theorem", and hence of understanding what counts as a pure proof of a theorem. I'll then provide three examples that I think show how these two understandings of content-requirement, and thus of purity, diverge.

1. Logical purity

The first way of understanding purity that I want to consider takes what is "required by the content of [a] theorem" to be just what suffices for proving that

1 Translation in [25], pp. 393–4. The original ([16], pp. 315–6) reads, "In der modernen Mathematik wird solche Kritik sehr häufig geübt, wobei das Bestreben ist, *die Reinheit der Methode* zu wahren, d.h. beim Beweise eines Satzes wo möglich nur solche Hülfsmittel zu benutzen, die durch den Inhalt des Satzes nahe gelegt sind." Hilbert continues by remarking that "Dieses Bestreben ist oft erfolgreich und für den Fortschritt der Wissenschaft fruchtbar gewesen." Hilbert seems to have had in mind recent work on circle quadrature and the parallel postulate.

On Using Measuring Numbers according to Measuring Theories

Wilhelm K. Essler

Abstract

It was shown by Frege that four of the five axioms of Peano can be regarded as analytical truths; and it was shown by Russell that the remaining axiom cannot be regarded as being analytically true or even as being analytically false, that this axiom thus is to be regarded as a synthetic statement. In using the concept of apriority in the sense of Reichenbach, it can be shown that this synthetic axiom is to be regarded as an apriorical truth within the usual background theory of measuring theories, which are used not as generalizations of empirical results but as— not moreover provable— preconditions of receiving measuring results and of ordering these results. Furthermore, the systems of numbers, starting with the natural numbers, are developed in a way such that the pre-rational numbers— but not the rational ones— turn out to be those ones which are used in performing measurements according to such theories, while the pre-real numbers— but not the real ones— then turn out to be those ones which are used in using such measuring theories together with their background theories for purely theoretical reasons.

As it was pointed out already by Reichenbach[1], Kant's concept "synthetic-apriorical" may be understood in a *relative* way, but also in an *absolute* way:

(1) In its *relative* meaning, the sentence "*A* is a synthetic-apriorical truth" is to be understood as a statement *related* to *some fixed* case of application, i.e. in the sense of "*A*, being a synthetic judgement, is *used* in some given situation of application as an apriorical truth".

(2) In its *absolute* meaning, the sentence "*A* is a synthetic-apriorical truth" is, however, to be understood *without* such being *related* to *any* case of application, receiving its validity therefore *not* from some suitable *case of applying* a given background theory in order to receive empirical results, but in the sense of "*A*, being *synthetic* truth, is *provable* by purely *apriorical means*, which—in establishing its truth—are furthermore proving its *necessity*".

Like Reichenbach, I am using these methodological instruments "apriorical" und "synthetic" not in the—logically unmaintainable—sense of (2), but in the—philosophically very useful—sense of (1)[2]. And in this very sense of (1),

1 See Reichenbach (1920).

2 In fact, I used them in this way since 1966. And it was about 1990, when Andreas Kamlah

The Compulsion to Believe: Logical Inference and Normativity

Jody Azzouni

Abstract
The interaction between intuitions about inference, and the normative constraints that logical principles applied to mechanically-recognizable derivations impose on (informal) inference, is explored. These intuitions are evaluated in a clear testcase: informal mathematical proof. It is argued that formal derivations are not the source of our intuitions of validity, and indeed, neither is the semantic recognition of validity, either as construed model-theoretically, or as driven by the subject-matter such inferences are directed towards. Rather, psychologically-engrained inference-packages (often opportunistically used by mathematicians) are the source of our sense of validity. Formal derivations, or the semantic construal of such, are after-the-fact norms imposed on our inference practices.

I

Mathematical proof amazed ancient Greeks. Here was a method—reasoning—from assumptions to unexpected new results. Furthermore, one saw that the conclusions *had to follow*. On my reading of Plato's *Meno*—and his other dialogues—the Greek discovery (of deduction) not only provoked Plato to the hopes of finally resolving ethical differences (by importing the method of reasoning from geometry), but also provided him—by means of a best explanation for why mathematical proof works—support for reincarnation.

Those were glorious days for *philosophy*, weren't they? So much seemed possible then by sheer reasoning alone—and there are *still* philosophers living off the meager echoes of *that* project. But some thousand years later most of us are—comparatively speaking—rather jaded about deduction; indeed, many philosophers, sociologists of knowledge, and others, are jaded enough to find tempting social constructivist views about mathematical proof. Social constructivists take mathematical proof as no different—sociologically speaking—from other practices that humans conform to: cuisine, tacit restrictions on polite conversation, linguistic rules, and so on. On such views, the plethora of alternative logics—and within them—the plethora of alternative mathematical systems, that were such a shocking discovery of the twentieth century, should have been expected; indeed, only sheer historical (and contingent) facts are

Nominalism and Mathematical Intuition

Otávio Bueno

Abstract
As part of the development of an epistemology for mathematics, some Platonists have defended the view that we have (i) intuition that certain mathematical principles hold, and (ii) intuition of the properties of some mathematical objects. In this paper, I discuss some difficulties that this view faces to accommodate some salient features of mathematical practice. I then offer an alternative, agnostic nominalist proposal in which, despite the role played by mathematical intuition, these difficulties do not emerge.

1. Introduction

For the purpose of this paper, I'll take *mathematical intuition* to be any sort of intuition involved in mathematical activity. The intuition in question may be invoked in grasping the truth of certain mathematical statements (whether they are taken to be axioms or not); constructing and evaluating proofs; assessing the cogency of the use of certain pictures, templates, or diagrams in a proof; or appreciating the reasonableness and fruitfulness of certain mathematical definitions. Clearly, mathematical intuition plays a central epistemological function: it's supposed to help us obtain knowledge of certain basic mathematical facts (typically, those that are described in certain mathematical principles or axioms). And it's common to find the development of accounts of mathematical intuition as part of a defense of Platonism. The crucial idea is that we have intuition of certain mathematical facts—facts about mathematical objects and their relations—and we then extend that basic mathematical knowledge to other, more complex, recherché facts.

There have been extensive discussions of mathematical intuition in the literature.[1] Although I won't review the discussions here, I will examine a prominent Platonist conception, and raise some difficulties that it faces *vis-à-vis* mathematical practice. My main goal is to examine whether a certain conception of mathematical intuition can support a particular, agnostic form of nominalism.

1 For insightful accounts, see Parsons [1980], Parsons [2008] (particularly Chapter 5), and Giaquinto [2007].

Jobless Objects: Mathematical Posits in Crisis

Yvonne Raley

Abstract
This paper focuses on an argument against the existence of mathematical objects called the "Makes No Difference Argument" (MND). Roughly, MND claims that whether or not mathematical objects exist makes no difference, and that therefore, we have no reason to believe in them. The paper analyzes four different versions of MND for their merits. It concludes that the defender of the existence of mathematical objects (the mathematical Platonist) does have some retorts to the first three versions of MND, but that no adequate reply is possible to the fourth, and most crucial, version of MND. That version argues that mathematical objects make no difference to our epistemic processes: they play no role in the process of obtaining mathematical knowledge.

I. Introduction

As the debate over the existence of mathematical objects continues, a "new" type of argument has surfaced as a challenge to mathematical Platonism. Alan Baker (2003) calls it the "Makes No Difference" argument (MND). Roughly, the "Makes No Difference" argument says that whether or not mathematical objects—objects that are said to be neither spatial nor temporal, and that are causally inert—exist makes no difference, and that therefore, we have no reason to believe in them. How is this lack of difference-making cashed out? Here, the literature offers various options. Horgan (1987: 281, 282) has it that the "world's spatio-temporal causal nexus" would be unaltered if sets did not exist. For Ellis (1990: 113), "the world we can know about" would be the same if there were no abstract objects. Azzouni (1994: 56) believes that if "mathematical objects ceased to exist", … "[m]athematical work" would "go on as usual." For Balaguer (1999: 113), "if there were no such things as abstract objects, science would be practiced exactly as it is right now". And lastly, Baker (2003: 247) describes MND as saying that "[i]f there were no mathematical objects, then … this would make no difference in the concrete, physical world."[1]

1 Not everyone on this list endorses the Makes-No-Difference argument, however.

Is Indispensability Still a Problem for Fictionalism?

Susan Vineberg

Abstract

For quite some time the indispensability arguments of Quine and Putnam were considered a formidable obstacle to anyone who would reject the existence of mathematical objects.[1] *Various attempts to respond to the indispensability arguments were developed, most notably by Chihara and Field.*[2] *Field tried to defend mathematical fictionalism, according to which the existential assertions of mathematics are false, by showing that the mathematics used in applications is in fact dispensable. Chihara suggested, on the other hand, that mathematics makes true existential assertions, but that these can be interpreted so as to remove the commitment to abstract objects. More recently, there have been various attempts to show that the indispensability arguments contain assumptions that are conceptually misguided in ways having little to do with mathematical content.*[3] *All of this work is of considerable interest, and the result has been a gathering consensus that the indispensability arguments, as put forth by Quine and Putnam, do not provide convincing reason to accept mathematical realism. The focus here will be on the ways of responding to the indispensability arguments, and in particular on the obstacles to fictionalism that remain after the versions of Quine and Putnam are undercut.*

The Quine-Putnam Indispensability Argument for Mathematical Realism

It is common to find references to the Quine-Putnam indispensability argument in contemporary discussions of mathematical realism. Although their arguments are often run together, it will be useful in evaluating some recent

1 See, Putnam, H. (1979). Philosophy of Logic. *Mathematics, Matter and Method: Philosophical Papers vol. 1*. Cambridge, Cambridge University Press: 323–357. Quine, W. V. O. (1969). Existence and Quantification. *Ontological Relativity and Other Essays*. New York, Columbia University Press: 9 1–113.

2 See, Chihara, C. (1990). *Constructibility and Mathematical Existence*. Oxford, Oxford University Press. Field, H. H. (1980). *Science Without Numbers*. Oxford, Basil Blackwell.

3 See, Maddy, P. (1997). *Naturalism in Mathematics*. New York, Oxford University Press., Sober, E. (1993). “Mathematics and Indispensability.” *The Philosophical Review* 102(1): 35–57.

Mill, Frege and the Unity of Mathematics

Madeline Muntersbjorn

Abstract:
This essay discusses the unity of mathematics by comparing the philosophies of Mill and Frege. While Mill is remembered as a progressive social thinker, his contributions to the development of logic are less widely heralded. In contrast, Frege made important and lasting contributions to the development of logic while his social thought, what little is known of it, was very conservative. Two theses are presented in the paper. The first is that in order to pursue Mill's progressive sociopolitical project, one must embrace Frege's distinction between logic and psychology. The second thesis is that in order to pursue Frege's project of accounting for the unity of mathematics, we must understand mathematics as a human activity and consider the role that history and psychology play in the growth of mathematics.

This essay considers the unity of mathematics and the relationship between psychology and logic by contrasting the views of John Stuart Mill (1806–1873) and Gottlob Frege (1848–1925). This contrast is instructive for several reasons. Tradition has it that these two men held radically distinct views on mathematics, logic and language and that Frege's more rigorous realism decisively won out over Mill's naïve naturalism. As with many traditional stories, a closer look reveals the extent to which the central plot is plausible yet overly simplistic. One thesis articulated below is that the traditional story is correct because, as it turns out, in order to pursue Mill's sociopolitical vision for humankind, we must cultivate a commitment to something like Frege's "laws of thought" as a domain of inquiry that logicians pursue independent of psychology. The other thesis developed below—and by far the more controversial claim—is that in order to pursue Frege's project of explaining the unity exhibited by mathematics, a discussion of human practices must be included as part of our explanation. The two theses may be summed up as follows: While there are many good reasons to distinguish between logic and psychology, articulating a satisfying account of the unity of mathematics is not one of them. For the relation between the laws of thought and the reliable habits of thinking people is like that between the chicken and the egg—they are so dependent upon one

Descartes on Mathematical Essences

Raffaella De Rosa and Otávio Bueno

Abstract
Descartes seems to hold two inconsistent accounts of the ontological status of mathematical essences. Meditation Five apparently develops a platonist view about such essences, while the Principles seems to advocate some form of "conceptualism". We argue that Descartes was neither a platonist nor a conceptualist. Crucial to our interpretation is Descartes' dispositional nativism. We contend that his doctrine of innate ideas allows him to endorse a hybrid view which avoids the drawbacks of Gassendi's conceptualism without facing the difficulties of platonism. We call this hybrid view "quasi-platonism." Our interpretation explains Descartes' account of the nature of mathematical essences, dissolves the tension between the two texts, and highlights the benefits of Descartes' view.

Descartes seems to provide two *prima facie* inconsistent accounts of true and immutable mathematical essences. *Meditation Five* suggests that Descartes was a platonist about mathematical essences. The *Principles* suggests that he held some kind of "conceptualist" view about such essences. We argue that, despite recent defenses of either Descartes' platonism or conceptualism, he was neither a platonist nor a conceptualist.[1] Crucial to our interpretation of Descartes is his dispositional nativism. We contend that his doctrine of innate ideas allows him to endorse a hybrid view that we will call "quasi-platonism" which avoids the pitfalls of Gassendi's conceptualism without falling into the troubles of platonism. Descartes' account of the nature of mathematical essences is explained, the tension between the two texts dissolved, and the benefits of Descartes' considered view are explored.

1. Ideas of Mathematics *qua* Ideas of True and Immutable Essences

What are ideas of mathematics according to Descartes? Or what do they rep-

1 For a neo-platonic interpretation of Cartesian mathematical essences, see (Schmaltz 1991) and (Rozemond 2008), and for a conceptualist interpretation, see (Chappell 1997), (Nolan 1997) and (Nolan 1998).

Presumption and the Judgement of Elites

Nicholas Rescher

1. Definitions and Distinctions: Elites and Second-Order Elites

Elites arise whenever there is a group within whose membership there is some feature of more or less. They consist of those group members that exhibit this feature none the general run—to a greater extent than most. To symbolize this we shall designate by <*F*, *G*> the elite constituted by the subgroup of those *G* members that exhibit the feature *F* to a greater than ordinary extent.

However, the special focus on the present discussion will be upon *reflexive* groups—those amongst whose membership certain intra-group relations obtain, so that some of them can stand in relation *R* to others. With such a group there will (or can) be

1. The active elite <$R^{\rightarrow}$, *G*> consisting of those *G*-members that *R* a more than ordinarily larger number of others.
2. The passive elite <$R^{\leftarrow}$, *G*> consisting of those *G*-members that are *R*d by a more than ordinarily larger number of others.

With reflexive groups there will accordingly be second-order elites as for example the people most trusted (or resented) among those who are themselves most trusted (or resented). This second-order elite may be designated by <$R^{\leftarrow}$, <$R^{\leftarrow}$, *G*>>.

Let the reflexive group *G* consist of *A*, *B*, *C*, plus a couple of others (say *D*, *E*). We can now contemplate a relation tabulation to indicate who *R*s whom as per

	A	*B*	*C*	[*D*, *E*]
A			√	
B			√	
C	√	√		
[*D E*]				

Thus *A R*s *C* alone, as does *B*, while *C R*s both *A* and *B*. Such a tabulation can obviously also be viewed inversely to identify items that are *R*d by *A* or *B* or *C* etc. (We suppose too that there are a couple of further items beyond *ABC*, but

Social Science Research and Policymaking: Meta-Analysis and Paradox

Steven I. Miller, Marcel Fredericks, Frank J. Perino

Abstract
The purpose of this article is to explore some of the non-obvious characteristics of the social science research-social policy (SSRSP) paradigm. We examine some of the underlying assumptions of the readily accepted claim that social science research can lead to the creation of rational social policy. We begin by using the framework of meta-analysis as one of the most powerful means of informing policy by way of empirical research findings. This approach is critiqued and found wanting in several ways. Several conceptual and definitional issues connected to the term "policy" are explored as well. A central argument is that even the best social science research is no guarantee of enlightened policymaking because the very (inductive) basis of empirical research militates against the possibility of going from research findings to policy. This claim is explored within the context of a central paradox. This paradox is explored in some depth. Finally, within the SSRSP claim, we analyze related issues such as the possibility of utilizing Mixed Methods and the politics of policymaking. We conclude that the SSRSP framework is, at best, a subjective one which ironically is needed, but one which is constrained by the very methods that is uses to formulate policy.

The purpose of this article is to explore a number of claims linking the supposed relation of social science research to social policy. While such investigations are not wholly new (Miller and Fredericks 2000; Miller and Safer 1993), we wish to give the topic a perspective that is rather distinct. The issue is important in a very basic way: the justification for the conduct of social science research (especially in terms of increasing methodological sophistication) can only be made if such efforts are tied to the development of social policy in some way. This is rather a strong claim but careful reflection must always bring social science research to social policy; otherwise, what would be the point of conducting it? One may, of course, argue that the further development of some particular methodological technique (e.g., hierarchical analysis) should be pursued for its own sake as a contribution, for instance, to the development of statistical theory. While such a position is plausible, the purpose of doing what we call "social science research" has always been viewed as being directed toward social "practices", or more broadly, the formulation of

Hidden Indexicals and Pronouns

Adam Sennet

Preliminaries

Cappellen and Lepore (2002, hereafter, C&L) offer the following test for putative hidden structure:

> Positing hidden linguistic expressions incurs certain obligations…on the syntactic side, a posited indexical should enter into anaphoric relationships… Overt indexicals can participate in anaphoric relationships…Since hidden indexicals are just the same indexicals, they too should be capable of entering into anaphoric relationships. (p. 273)

C&L claim that it is a constraint on any posited hidden indexical that it license anaphoric relations[1]. Hidden variables that account for domain restriction, like the sort proposed by Stanley and Szabo (1999)[2] propose (hereafter, S&S variables) don't respect this constraint and so should be rejected. The argument is as follows:

(1) If an LF contains a hidden variable then the variable must be capable of licensing anaphora.
(2) *S&S variables don't license anaphora.*
(3) No LF contain S&S variables.

C&L claim that their argument provides fairly substantial syntactic evidence against S&S. In support of (1), C&L point out that Davidsonian event variables are both hidden and license anaphoric reference.

In this squib I will suggest that C&L's argument should not trouble S&S or theorists of their ilk. First, the argument is far too strong. Many uncontroversial cases of hidden syntactic structure fail to license anaphora. Fans of

1 C&L (2002) also argue against hidden indexical theorists by providing a *reductio* of the binding argument and an argument based on *a priority*. I'll restrict my focus to the argument from anaphora.

2 Stanley and Szabo aren't the only hidden indexical theorists. Two other notables are Pelletier (1995) and von Fintel (1995). One difference between these theorists involves where the variables contribute to the interpretation of the nominal itself or the entire determiner phrase. I chose Stanley and Szabo only because C & L's argument focuses on them.

ent in the case of conditionals, for at least some conditional propositions are determinately true and some are determinately false. But again, there is a catch, for all conditionals whose false antecedents don't metaphysically or physically entail their consequents are indeterminate. Moreover, in this case, as in the moral case, there are serious glitches in our conceptual practices – a recurring theme in the book. The topic of chapter eight is the role of propositions in information and explanation. Schiffer has it that we often exploit someone's propositional attitudes as a source of information about the world: I come to know that p because I come to know that you believe that p. We also use the world as a source of information about the propositional attitudes of others. From the fact that p is obviously the case I can (defeasibly) infer that you believe that p. Secondly, we often make use of propositional attitude ascriptions to explain and predict behaviour, as when we use what we know about someone's beliefs and desires in order to explain what he did or to predict what he will do. And, not surprisingly, Schiffer takes pleonastic propositions to be perfectly suited to serve in those propositional attitude ascriptions. So let me start by taking a closer look at his account of the nature of propositions.

At the beginning of chapter one, Schiffer explains that a „major theme of this book will be that all notions of linguistic and mental content are ultimately to be explained in terms of the things we mean and believe, and that these fundamental units of content are propositions of a certain kind, which, for reasons that will emerge, I call *pleonastic propositions.*" [11] Part of his reason for thinking that the things we mean and believe are propositions is that he accepts the Face-Value-Theory of belief reports – reports such as "A believes that there is life on Venus". According to the Face-Value-Theory, the verb "to believe" is a two-place predicate whose argument places are to be occupied by singular terms. In the above report, the singular term "A" refers to A and the singular term "that there is life on Venus" refers to *that there is life on Venus.* And *that there is life on Venus* is a proposition. Now given that the referents of that-clauses are propositions, the next question is as to their nature. According to Schiffer, propositions are abstract, mind- and language-independent entities. But more needs to be said: Are they structured or unstructured? And if they are structured, what are the components supposed to be? He discusses two opposing views: According to the Russellian, propositions are structured entities whose basic components are the objects and properties our beliefs are about. The Fregean, on the other hand, has it that the components are not the objects and properties themselves but rather concepts thereof. But, as Schiffer convincingly argues, both the Russellian and the Fregean face a couple properties determined by those concepts be deficient too—and can we make sense of this idea at all? Others may find the claim that there are no determinately true moral propositions and hardly any determinately true conditional propositions of interest hard to swallow. So Schiffer's account seems to compel a radical departure from classical views. Future discussion will show whether that is justified.