PREDICATIVITY, FOUNDATIONS OF ARITHMETIC AND FREGEAN ARITHMETICS

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A received view concerning impredicativity is that a global ban against impredicative definitions, as perhaps suggested at one point by Russell's vicious circle principle, is excessive and ill-motivated.

However there might be problems with their employments in particular contexts. For example, if as Goedel (1944) argued, impredicative definitions can only be justified from the point of view of a set-theoretic realist, there is a potential conflict between the use of a strictly impredicative system and the claim that statements derived within the system are truths of logic. Is there really a problem here that could be potentially harmful for Neo-Fregean projects in the philosophy of mathematics?

The technical half of this problem concerns whether such projects could be carried out without recourse to impredicative definitions. Linnebo (2004) studies some predicative fragments of Frege Arithmetic (i.e. Second-Order Logic plus Hume's Principle), with essentially limitative results. The philosophical half of the problem is to give a defense of the relevant use of impredicative principles. Linnebo argues that the use of impredicative comprehension principles undercuts the epistemic aims of the logicist program : are his arguments cogent?

As Linnebo notes, traditional Frege Arithmetic offers another impredicative definition in the form of Hume's Principle, when regarded as a definition of the cardinality operator. Is the impredicativity in HP a serious obstacle to the Neo-Fregean project? Wright (1998) takes up this challenge, and, at the opposite end, the relevant sections of Fine (2003) try to sharpen it.

On the more historical side, Michael Dummett famously conjectured that the impredicativity of the second-order quantifier is in some sense 'responsible' for the occurrence of contradiction in Frege's *Grundgesetze*'s system. This philosophical view, partially supported by Heck's discovery (Heck, 1996) that a substantial class of subsystems of Frege's Grundgesetze's system are consistent, naturally raises the question whether anything like Frege's program can be carried out in such systems (the programs are described extensively in Burgess and Hazen (1998) and Burgess (2005)).

Moving away from Fregean projects, Parsons (1992) presents an argument to the effect that a certain kind of impredicativity is indispensable to any attempt to justify the principle of induction from an account of the concept of natural number. This article simultaneously raises two major questions: first, how many kinds of impredicativity are there? Second, is it really true that an admissible concept of impredicativity is indispensable to such an account? Feferman and Hellman (1995, 1997) use a framework for an answer to the first question originally characterized by Feferman (and described in Feferman (2005)), to give a negative answer to the latter question (some fragments of the larger framework are, on the other hand, questioned by Hellman (2004)). Velleman and George (1997) provides a useful map of positions and problems in this area.

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VAGUENESS AND CLASSICAL LOGIC

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Do the paradoxes of vagueness give reason to abandon the idea that classical logic is the logic of natural language, or to reject classical semantic principles such as the principle of bivalence?

Part of the question is how much we can do in solving the sorites paradox without committing ourselves to a full semantic account of vague expressions, and an accompanying logic. The pressure against classical logic is relieved if an answer to the sorites paradox is possible that manages to account for the appeal of its premises and, at the same time, to reject one of them as (at least) not true. Epistemicist and contextualist accounts of vagueness seek to supply precisely such an account: do they succeed?[1]

Even if they do, however, it would be a mistake to set the question of classical logic and semantics aside. For, obviously, if it is true that the epistemicist and contextualist solutions to the sorites are compatible with different accounts of logic and semantics of vague terms, then further argument is needed to answer the original question. The question might remain pressing, even in spite of Williamson's arguments for bivalence and classical logic. There is an intuition that vague discourse is, in some non-epistemic sense, indeterminate : can this sense of indeterminacy be explained, or even adequately modeled, within a classical semantic theory or does accepting the intuition lead to a rejection of bivalence or classical logic ?

Supervaluationists have answered the second question by adopting a non-bivalent semantics, but one that validates all of the classical validities. Part of the rationale for supervaluationism came from its smooth account of what Fine (1975) called 'penumbral connections'. The problems of interest to me, in connection with supervaluationism, are: (i) what is the rationale of the rejection of bivalence and how is Williamson's argument for bivalence (in ch. 7 of *Vagueness*) to be disarmed? And, (ii) does 'validating all of the classical validities'

[1]The epistemicist account of the sorites paradox is defended in chapter 8 of Williamson (1994): discussion of Williamson's solution can be found in chapter 3 of Keefe (2000) and in Burgess (2001), Williamson replies to some of the problems there discussed in Williamson (1996) . Contextualist accounts are defended in Soames (1997), and Graff (2000). Their effectiveness in solving the sorites paradox is criticized by Stanley (2003).

suffice to say that supervaluationist semantics for vague discourse vindicates classical logic? (These questions are discussed in Williamson (1994) Keefe (2000), McGee and McLaughlin (1998), Williamson (2004))

It turns out that the answer to (i) turns crucially on what model of propositional content for vague utterances the supervaluationist chooses. Braun and Sider (2005) propose a (non-supervaluationist) view from which one can extract two possibilities for a supervaluationist account of propositional content of vague terms.

Hartry Field (2001) objected to the supervaluationist account that the semantic model of indeterminacy in that it offers fails to supply an explanation (reductive or otherwise) of the concept of indeterminacy. Field sees the prospect of supplying such an explanation as lying in an account of the conceptual role of the concept of indeterminacy—an explanation, cast in terms of degree of belief, of what is it to ascribe indeterminacy to a proposition. But, again, one might wonder that if the required explanation must be supplied at the psychological level, it might fail to adjudicate among competing semantic and logical theories. Field (2001) toys with this thesis, but more recently Field (2004) has argued that the explanation of the role of the concept of indeterminacy is naturally at home in the context of a certain non-bivalent, non-classical theory? Is Field's strategy of explanation successful? Does he supply sufficient motivation for his choice of semantics and logic?

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DUMMETT AND THE JUSTIFICATION OF DEDUCTION

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In a series of essays (Dummett (1959, 1973, 1978), culminating with Dummett (1991)) Michael Dummett proposes elaborate arguments in favor of intuitionistic logic. Dummett argues that the intuitionistic meanings for the logical constants are capable of being justified by means of a certain kind of proof-theoretic procedure which is developed in chapters 11-13 of *The Logical Basis of Metaphysics.*

For us to take this as good support for Dummett's kind of logical revisionism, we need to ask four questions: (1) why do the meanings of logical constant even need a justification? (2) can we clarify and defend the claim that this procedure is indeed a *justification* of an assignment of meanings to logical constants? (3) can we defend the claim that this is the most powerful proof-theoretic procedure available? (4) can we motivate the priority assigned to proof-theoretic procedures over model-theoretic justifications?

The first question arises historically from Prior (1961). Prior argued that a license to accept connectives characterized by any combination of introduction and elimination rules commits one to the intelligibility of operators that make the language inconsistent. Building on Belnap (1962), Dummett argued that in order to show that a deductive pratice is in order we must show that each expression in the language satisfies (at least) the requirement of Harmony. What does Dummett mean by Harmony? And how is it related to Belnap's requirement of conservative extension? What kind of justification does the deductive practice gain from being shown in Harmony?

(2) is mostly internal to Dummett's program. The controversial bit of its answer turns on the applicability of what he terms 'the Fundamental Assumption' to logical constants. This is the assumption that whenever we are in a position to assert a complex statement, we could have arrived to that position by means of one of the introduction rules. In other words, the assumption consists in the claim that the introduction rules for the given constant characterize canonical means of introducing a complex statement with that constant as a main connective: can this assumption be adopted in full generality?

The third question is sharpened by an argument (in Rumfitt (2000)) to the effect that, by modifying certain assumptions of Dummett's regarding the notion of sense, it becomes possible to give a proof-theoretic procedure that is stronger than the procedure that Dummett envisages–one that validates classical meanings for the logical constants. The question whether Rumfitt's project can be successfully carried out, then, becomes directly relevant to the question of whether amenability to Dummett's style of proof-theoretical justification gives support to the choice of intuitionistic logic over classical logic.

As for the fourth question, Dummett attempts to close off the possibility that it raises, by arguing that the proof-theoretic procedure is a natural choice in the context of a verificationist theory of meaning, and at the same time that a verificationist theory of meaning should be adopted, because a theory of meaning whose central concept transcends our ability of recognition must violate the requirement that knowledge of meaning must be capable of being manifested. Are Dummett's arguments convincing, here? If not, it becomes important to ask (i) what is the significance of Dummett's proof-theoretic procedure within a realist framework? (ii) can an account of the possibility and utility of deduction that is not strictly proof-theoretic, but at least partially semantic be proposed in this context?

Harman (1986) and Peacocke (1987) defend related quasi-semantic procedures of justification on broadly realistic assumptions. The procedures turn out to vindicate the classical meanings for the logical constants (Wright (1992) points out why Peacocke's proposal won't work in a verificationist setting). On Peacocke's proposal assignments of semantic value justify the validity of logical principles in the model-theoretic way, but assignments of semantic value to a given constant, in turn, require justification in terms of normative facts about acceptance of basic principles involving that constant.

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