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Tonk

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Some years ago A. N. Prior started a controversy about the meanings of logical connectives.[1] Although the dispute has long since died down, I believe that its real points have not been understood. Yet, as I shall try to show, these are of continuing interest.

1 I begin with brief summaries of [11] and [1]. My neglect of [13] does not stem from disrespect. I believe that insofar as they are both clear and right [13] and [1] are at bottom alike, and that the attempt to extract further insights from [13] would take too long.

In [11] Prior attacks the idea that there are certain ("analytically valid") inferences "whose validity arises solely from the meanings of certain expressions occurring in them". For example, the inference from a conjunction to one of its conjuncts is supposed to be valid purely in virtue of the conjunction symbol ('&' or 'and'). As Prior explains:

> For if we are asked what is the meaning of the word 'and' . . . the answer is said to be *completely* given by saying that (i) from any pair of statements *P* and *Q* we can infer the statement formed by joining *P* to *Q* by 'and' . . . and that (ii) from any conjunctive statement *P*-and-*Q* we can infer *P*, and (iii) from *P*-and-*Q* we can always infer *Q*. ([11], ¶2)

Prior expresses doubts about this view of 'and', suggesting that an expression "must have some independently determined meaning before we can discover whether inferences involving it are valid or invalid". He finds these doubts confirmed by his observation that if the proffered account of 'and' is correct, we may make *every* inference analytically valid. To do so we introduce the connective 'tonk' according to the rules

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(iv) $P \vdash P$ tonk Q

and

(v) P tonk $Q \vdash Q$

which allow $P \vdash Q$ for arbitrary P and Q. Since this is obviously absurd, Prior concludes that the notion of an analytically valid inference must be rejected.

Prior calls (i)-(iii) and (iv)-(v) "inferential definitions" (technically, each is a set of introduction and elimination rules). One might take the contradictions these can generate to show that something is wrong with inferential definition; this is a moral that both Prior and Stevenson seem to find attractive. Belnap, however, holds that inferential definition is perfectly all right as long as, unlike Prior, we bear in mind the general constraints on definition:

> . . . we are not defining our connectives *ab initio* . . . but rather in terms of an antecedently given context of deducibility . . . before arriving at the problem of characterizing connectives, we have already made some assumptions about the nature of deducibility. ([1], ¶5)

And, Belnap continues, (iv)-(v) are inconsistent with our assumptions.

We can see this as follows. Our notion of deducibility may be codified by the axiom $A \vdash A$ and Gentzen's structural rules of contraction weakening, permutation, and transitivity. (The details of these do not matter, but contraction, for example, allows us to infer $A_1, \ldots, A_n \vdash B$ from $A_1, \ldots, A_n, A_n \vdash B$.) Let us treat these statements about deducibility as a formal system S. Then it is our view that S expresses "all and only the universally valid statements and rules expressible in the given notation". But then any extension of S by definitions must be conservative (i.e., must yield no new theorems in the old notation), lest we contradict our assumption that S completely characterizes deducibility. Since adding (iv)-(v) to S would of course be nonconservative, they are unacceptable.[2] As Belnap puts it, "given that our characterization of deducibility is taken as complete, we may with propriety say 'there is no such connective as *tonk*'." To this he adds an important caveat:

> . . . the existence of a connective having such and such properties is relative to our characterization of deducibility. If we had initially allowed $A \vdash B$(!), there would have been no objection to *tonk*. . . . Also, there would have been no inconsistency had we omitted . . . the rule of transitivity. ([1], ¶13)

Belnap's general conclusion is that one may, without having any antecedent idea of their meanings, introduce connectives in terms of $\vdash$, but one is then responsible for the consistency of one's definitions.

Let us turn to criticism. My own initial reaction to Prior was that his reasoning is exceptionally puzzling. Three objections, in particular, seem to raise doubts about the coherence of his position.

1. Prior is evidently quite concerned with the claim that (i)-(iii) *completely* give the meaning of 'and' (it is discussed again at some length in [12], ¶6, 8-11), yet this claim is irrelevant to the argument about analytical validity. Prior's opponent is supposed to hold that (i)-(iii) follow from or are part of the meaning of 'and'; it is simply no part of this theory (to which, incidentally,

I have no commitment) that (i)-(iii) exhaust or completely represent that meaning.[3]

2. Prior also credits his opponent with the view that a connective hitherto unheard of can be *introduced into our language* by an inferential definition ([11], ¶3; cf. [12], ¶6, 12). But whatever the value of that view, the question whether consequence relations hold in virtue of meaning is independent of how new language can be made. All the opposition needs to maintain is that however our use of language might bestow meaning on 'and' (for example), it is just in virtue of that meaning that (P and Q) $\vdash P$ obtains.

3. A further problem is that Prior never does argue against the claim that (i)-(iii) are part of the meaning of 'and'. What he argues, in effect, is that not *every* inferential definition can belong to or express the meaning of a word, because this would lead to contradictions as in the case of 'tonk'. But since the notion of analytical validity carries no commitment to the universal correctness of any form of definition, Prior leaves the legitimacy of (i)-(iii) as a definition untouched.

Prior's critics more or less pass over these points. And while [1] is apparently much the favored contribution to the 'tonk' controversy,[4] it has at least one bad flaw of its own. My summary already revealed a metaphysical gaffe: the fact that there *is* no such connective as 'tonk' ([1], ¶4) has to do with the *truth* about deducibility, not, as Belnap seems to suggest, with our *beliefs*. As long as '$\vdash$' is no mere symbol but means *deducibility*, it is nonsense to suppose that the existence of a connective satisfying (iv)-(v) depends on our characterization of $\vdash$ ([1], ¶13). As long as it is not up to us what follows from what (except trivially, due to our power to change the meanings of words), it is similar nonsense to speak of our "allowing $A \vdash B$" ([1], ¶13). We can, of course, falsely believe that $A \vdash B$, but that will not help the definition of 'tonk'. Fortunately, Belnap's error (which we might trace to an overly formalistic viewpoint) is easily patched up without damage to the rest of his article. The claims I have just criticized can be dropped; what remains is the observation that (iv)-(v) are (jointly) unsatisfiable. Belnap's general response to Prior could then be that while we can expect contradictions to flow from an unsatisfiable definition, there is no reason to throw out the satisfiable definitions of similar form. This is very reasonable.[5] It is, in fact, essentially my objection 3.

But Belnap, even once corrected, does not provide a satisfying view of [11]. Rejecting an entire class of definitions because of an unfortunate example is on its face a crude error, and Belnap does not explain what more, if anything, Prior has to offer. If there is nothing more, [11] would hardly be worth discussing. Who ever believed, for example, that all inferential definitions are proper? Now I think that although Belnap and other readers have not made sense of [11], this article by itself *is* almost incomprehensible. Prior's intent can be divined only from the sequel [12]. Even this is extremely confusing, and what I now offer is not a full interpretation but simply the attempt to present the central line of thought.

II I believe that Prior's real concerns have to do less with pure logic or semantics than with broadly psychological questions about understanding and the mechanisms of reference. Let me introduce these with a puzzle.

Prior seems to protest that an inference cannot be valid just because we call it so ([11], ¶3, [12], ¶12). I applaud and would add that this fact has often not been fully appreciated (witness Belnap), but the question is what, in particular, this has to do with inferential definitions. If we reject, say, (i)-(iii) because 'and' must have an "independently determined meaning" before we can tell whether inferences involving it are valid or invalid, then it looks as if we ought to reject *every* definition. We would, for example, have to rule out standard metalinguistic definitions of the form

(vi) a denotes b

and object-language explicit definitions of the form

(vii) $a = b$

because only expressions which are *already* meaningful can denote anything or be used in true identity statements. But surely no philosophy should make us forego all definitions.

I believe that Prior's intent is less radical. His idea is that no definition in any of the standard forms logicians use (this includes (i)-(iii), (vi), and (vii)) can be read at face value because no such statement can give a truly novel expression its meaning. Taken literally, something like (vi) is simply a false statement (and (vii) false or at least not true) if a lacks prior meaning. Now this seems an odd cavil. We know well enough that (vi) is just a conventional way of expressing one's intention to use a to denote b and that (roughly speaking, of course) such intentions are what make new symbols meaningful. To make this absolutely clear we could eschew (vi) in favor of something like

(vi)′ I hereby resolve henceforth to use a to denote b,

and adjust our other definitions similarly. But it is technically both convenient and harmless for logic to proceed in happy neglect of these niceties.

Prior might reply that he is not talking logic but philosophy of language, where mathematically irrelevant considerations may matter. He might also observe that his problem about definition is sufficiently nontrivial to have been discussed by a logician of some eminence, who dealt with it in a quite curious way,[6] so that his own remarks are perhaps not entirely pointless. And thus far his position would be well taken. What needs explaining is how it could come into a discussion of the logical connectives.

Prior's real question is how words get their meanings. What we have just seen are his reasons for thinking that a word w does not get its meaning (let us assume it has just one) by being asserted in the sort of sentence which a logician might regard as the axiom defining w. Although I have not defended this view of Prior's, I agree with him and would add that what he says goes just as well for purely mental assertions as for public ones. I am less sure how much trouble has ever come from neglect of his point here, but in any case we are now dealing only with a subsidiary observation. The main purpose of

[11] and its sequel is to show that logical connectives do not get their meanings by being associated with what we may call logical roles.[7] That is, one might hold that while simply being put into the asserted sentences (i)-(iii) does not give 'and' a meaning, meaning does result if we perform the mental act, so to speak, of assigning to 'and' the logical role defined by (i)-(iii); this is what Prior denies. And the main steps of his argument are clear enough. He thinks that otherwise a word could acquire its meaning by being associated in a speaker's mind with the logical role we have given for 'tonk', which (allegedly) leads to absurdity. In more detail, I would *guess* that Prior reasoned as follows: suppose that a word *w* could become meaningful just by being paired with a logical role. What would its meaning then be? Presumably, to *have* that role (plus whatever further semantic properties this might entail). This seems to work for 'and', which we associate with a certain role and which, one might think, has just that role in virtue of this association. But as 'tonk' shows, there are impossible logical roles. On the theory in question, connectives could nonetheless have these when used by speakers confused enough to bestow them. This is contradictory.

Prior has an alternative to the rejected theory. Again his position is unclear, but I speculate that he holds a view close to Frege's in [5]. Briefly, he believes that there are abstract entities, senses, which we apprehend by quasi-perceptual cognitive acts, and that a word gets its meaning by our decision to use it with a certain previously identified sense. This explains why 'tonk' is impossible. There is no such sense as it would have to have, hence none for us to apprehend and assign, so a word with this meaning could never enter our language. But Prior's treatment of connectives is apparently only one case of a general theory of the acquisition of meaning.

I expect widespread agreement that there is a lot of wrong with this sort of theory and do not intend to criticize it here. (Wittgenstein rightly attacked it in the *Investigations*.) It is also easy to answer Prior's argument about meaning and logical roles, but this answer will be worth spelling out anyway. I shall give it in Fregean terms, but its essence should survive whatever revisions of Frege might be necessary.

Once we distinguish between sense and denotation for connectives, just as we do for names and predicates, Prior's argument breaks down. Although it is unclear what, in general, senses are, let us suppose that specifying the logical role we take a connective to have might (at least partway) specify its sense. Thus, the sense of 'and' would be given by (i)-(iii), the sense of 'tonk' by (iv)-(v), and so on.[8] Turning to denotation, we can now say that a connective *c* denotes a truth function *f* if and only if *f* conforms in the obvious way to the sense of *c*. 'And', for example, denotes the usual truth function because that denotation confers just the logical properties we think 'and' has. If, on the other hand, no truth function reflects our use of *c*, *c* lacks a denotation. We are unaccustomed to speaking of denotationless connectives, which may not exist in actual languages, but there is nothing paradoxical about them,[9] and the account of how they fail to denote is much like the corresponding account for terms like 'Pegasus': broadly speaking, lack of denotation is in each case due to the absence of anything conforming to the conception we associate with a word.[10]

Why did Prior miss this reply? The sense-reference distinction does seem less natural for connectives than for other semantic categories, maybe because difference in sense between coreferential connectives is harder to imagine. It is not clear whether two (semantically unstructured) conjunction or negation signs, for instance, could have distinct senses. But here Prior supplies a better example. A sense-reference distinction is motivated by sensical, denotationless items as well as by cases like 'Hesperus' and 'Phosphorus', and 'tonk' seems to fill the bill: it lacks a plausible denotation without being nonsense (contrast [1], ¶ 4). One of the real, if unintended merits of [11] is to make a case for the senses of connectives.

Of course there is more to be said here. On the one hand, 'tonk' *can* seem unintelligible. If we imagine speakers using it in accordance with rules (iv)-(v), we may well feel that we somehow do not know what they mean. I believe that this is not because the word would lack a sense, but because the state of mind in which one could accept a connective as having this sense is inscrutable to us. More discussion, however, is obviously required. On the other hand, the introduction of senses for expressions of any kind requires the application of systematic considerations about language. Examples *alone* are insufficient. But I only wish to claim that in giving a vivid example which raises the issues of sense, reference, and lack of reference for connectives, Prior has performed a modest but valuable service.

I believe that if we go somewhat beyond the limits of Prior's own discussions we can extract some further, interesting lessons. These are the subject of my last section.

III (A) Prior's own way out of his problem about 'tonk' is defective quite independently of the existence of alternative solutions. Even if senses are apprehended and assigned as Prior (apparently) believes they are, the entrance of 'tonk'-like connectives into our language is ruled out only on a further, unjustified assumption. Prior thinks that 'tonk' cannot happen because there is no such meaning for anyone to grasp and give to a word, but why should one's faculty of sense-"perception" not present one with the *appearance* of such a sense? Why, in other words, should this faculty not be as vulnerable to hallucinations as vision or touch?[11] Of course the account of how we apprehend senses is too minimal to allow a decisive objection along these lines, but insofar as the perceptual model has any content at all, it is hard to see why the usual failures of perception should not threaten here as elsewhere.

That Prior should not answer or even notice this question may not appear to be very interesting. After all, virtually no one believes in the perception of senses anyway, and even believers should not endorse Prior's views on 'tonk'. But Prior's error is important as an instance of something more general: the inconsistent application of perceptual models to apparently nonperceptual modes of cognition. It is *superficial* to treat understanding as perception without allowing for standard sorts of misperception, and positions much more influential than Prior's tend to suffer from an analogous superficiality. I have in mind perceptual approaches to (some of) our knowledge about abstract, mathematical objects.[12] At the cost of digressing I should like to *suggest* two ways in which these approaches may run into the kind of difficulty

Prior neglects. My excuse is that the importance of Prior's error, and of his instructively clear commission of it, cannot be made plausible without any identification of its more subtle counterparts.

(i) It is questionable whether there is a phenomenology of mathematical perception, or whether there is mathematical *experience* properly speaking. The intended sense of experience is notoriously resistant to definition, but it does seem that acquiring beliefs about objects of a certain kind (even in a "causally reliable way", or whatever) does not suffice for perception. If what we might call experience or sensory consciousness is also necessary, then perceptual approaches to mathematical knowledge face a neglected obstacle.

(ii) The attractive idea of proper objects for the senses has no clear application to the mathematical case. In the case of vision, for example, there seems to be a difference between such properties as hue, motion, and distance on the one hand and age, flavor, and material constitution on the other, even though sight informs us about all of these.[13] Analogous distinctions seem to be possible for the other senses, but it is open to question whether the perceptually more basic properties of sets or numbers can be similarly demarcated.

Each of these (quite possibly related) objections to the mathematical epistemology of Gödel and others calls for considerable elaboration, and each may apply with different force to more or less similar positions. Still I hold that far too little thought has been given to the possible disanalogies between genuine perception and the acquisition of mathematical beliefs.[14] It is to Prior's credit that he has at least indirectly helped us to see this.

(B) Prior has more to offer than an instructive error. While his own discussions are inadequate, they are almost the only places in the entire philosophical literature where the question of how logical connectives get their denotations is raised in a useful way. This is very striking. Problems about denotation have been prominent throughout this century, and one might be surprised that philosophers interested in general theory should have attended exclusively to a few semantic categories. The excuse might be that the favored expressions, notably names, are at once apparently more tractable yet also quite hard enough, so that we already have our hands full. But it is obviously poor strategy to approach the problem of (say) names without an eye to the context into which one's theory of names will have to fit. Neglect of connectives may also have been fostered by a view of such particles as being "syncategorematic" or otherwise incapable of reference, so that no account of how they refer is needed. But such views have been outdated at least since Frege, who saw clearly that an account of the truth values of sentences must assign referential properties to connectives and quantifiers along with predicates and terms. This gives us good reason to speak of the former as denoting certain functions; even if one objects to this terminology, the denotation, by whatever name, of a connective is there to be explained. Now I think that current accounts of reference have indeed suffered from undue concentration on names (and names of persons, at that), but this is not a point I shall substantiate here. Instead let us reconsider Prior's views on the meanings of connectives.

We have seen that Prior is deficient in his failure to distinguish sense and denotation for connectives. He can nonetheless be credited with the view that an inferential practice involving the connective *c* is not enough to establish a denotation for *c*. In particular it cannot make *c* denote the truth function which, in the obvious sense, reflects our use of *c*. His argument, we observed, overlooks the possibility of taking *c* to denote the corresponding truth function only when such a function exists, leaving *c* denotationless otherwise, but we did not seriously consider the tenability of this alternative. One may somehow feel that *c* cannot simply denote or fail to denote depending on whether a function matching our inferential practice happens to be around. Some further connection might seem to be needed, and if so, Prior's hand would certainly be strengthened.

I did remark that the alternative Prior overlooks promises to fit well with an attractive, not yet refuted account of denotation and failure of denotation for proper names. It is also very difficult to imagine what the added "connection" between a word and the truth function it denotes could be. Certainly, talk of "causal contact" between connectives and truth functions lacks appeal, and in the absence of better ideas one might conclude that a connective simply does receive that denotation, if any, which validates our inferential use of it. I myself am inclined to agree. This same position has recently been taken by Hartry Field at the end of [4]. Since Field's discussion is important in its own right and is also, in my view, the closest thing to a proper reply to Prior (though it was not so intended), I would like briefly to explain what he is up to. (I should note that I am dealing with only one aspect of an exceptionally rich and stimulating article.)

Field observes that standard theories of truth handle the logical connectives with clauses like:

(a) '$\neg A$' is true iff 'A' is not true.

He finds plausible the idea that (a) "does not really illuminate the meaning of '$\neg$' and 'not' but merely establishes a synonymy between them", and therefore asks whether it is possible to "say something else about the meaning of '$\neg$' which is more obviously illuminating". One approach to this problem is to replace (a) and the like with clauses that explicitly display the referents of connectives, which can of course be done in Fregean style.

(b) '$\neg$' refers to f, where $f(T) = \bot$ and $f(\bot) = T$.

Field, however, comments:

> . . . this formal account really isn't of much help in illuminating the meaning of the connectives, for there seems to be no prospect of giving any interesting account of what it is for a word to "refer to" a function that maps T into $\bot$ and $\bot$ into T. Or, to put the point more accurately: it is hard to see how to give an account of "reference to" truth functions *unless we construe the claim that '$\neg$' refers to a certain truth function as really a claim about the role that '$\neg$' plays in our conceptual scheme* [i.e., about our inferential practice with '$\neg$']. And if we do so construe the claim, then we see that talk of "reference to truth functions" is an unnecessary diversion: what is really needed, if we are to give a more illuminating account of the meaning of '$\neg$' than is provided by [(a)], is to explain its meaning in terms of its conceptual role.

The difficulty of the issues with which Prior was grappling may be judged from the fact that even Field does not get his own position quite right. From the viewpoint of referential semantics it is hardly an "unnecessary diversion" explicitly to say what '¬' refers to (!), any more than it is unnecessary to say what 'Beethoven' refers to. The truth-conditions of sentences depend in part on the referents of words, so clauses like (b) which *give* the referents are necessary. And depending on what one wishes to call an account, there is a perfectly good account of what it is for (b) to obtain. If one were asked what it is for '¬' to refer to the indicated function, one could well say that such talk of reference for connectives is to be understood in terms of their effect on contained and containing expressions. For example, (b) holds because affixing '¬' to a sentence reverses the truth value of the whole. It is at least puzzling why this should not be a quite appropriate explanation.

But what Field means is of course clear. Just as a clause like

(c) 'Beethoven' refers to Beethoven

is, although necessary, no substitute for a theory of name reference, so statements like (b) leave unilluminated the further questions about how connectives acquire just these denotations. The answers must have to do with psychological facts about language users (Field would say "physical" facts), and in the quoted passage Field proposes that the denotation of a connective is due to its conceptual role.

As I indicated earlier, I suspect that he is right. But this is not an unproblematic position, for at least two reasons. First, determinations of conceptual or inferential role are complicated by our occasional logical mistakes. Someone might, for example, carelessly infer a conjunction from one conjunct, yet we would not want this bit of practice to show that his 'and' did not after all mean conjunction. We want to be able to charge him with a mistake rather than crediting him with the sort of novel usage which would make 'and' denote something else. But what counts as a mistake? As long as we want denotation to be a function of use, it is question-begging to say that mistakes consist in usage at odds with the denotation of 'and'. Field passes over this problem. He might well suggest, however, that we can identify such error-producing factors as inattention and haste in a noncircular way, which would allow us to set aside usage influenced by such factors when we look at the use of a connective in order to determine reference.[15] I would agree, but I expect working out this defense to be a nontrivial task. The second problem for Field is just to explain *why* conceptual role determines denotation in the way he proposes. This might seem an odd or even confused question, yet it reflects the legitimate feeling that even if Field is right, we are too unclear about reference in general fully to understand why he is right. Even in the absence of objections to Field, one should not rest content with his account of the meanings of connectives.[16]

Perhaps Prior considered Field's sort of position in some form and found it unpalatable. It is certainly easy for intuition to go astray in these matters, and we should be grateful for the intuitions which led Prior to address a problem about meaning and reference which neither he nor his contemporaries fully understood. The interest of his problem, and of the error discussed above under heading (A), seem to me to justify the attention given Prior in this paper. I have also tried to make the limits of Prior's work clear. I doubt that there is much more in [11] than I have been able to extract. Yet if [11] is not quite the gem Belnap takes it to be ([1], ¶5n.), it is at least a semiprecious stone which, once polished, rewards us with quite unexpected reflections.

NOTES

1 See [11], [13], [1], and [12].

2. Notice that Belnap's objection to (iv)-(v) is not simply the creativity (in the technical sense) of this definition. Insisting on noncreative definitions is normally an important terminological preference, a way of reserving the word 'definition' for mere enlargements of our vocabulary. But Belnap sees that, verbal points aside, there is nothing inherently illegitimate about creative definitions, which is why he stresses the *completeness* (now I am speaking nontechnically) of Gentzen's account of $\vdash$.

3. Cf. [12], ¶8-10, where Prior makes some good points.

4. I get this impression mostly from conversations, but see also the approving reference to Belnap in Hacking's impressive [8].

5. Even so, it must be added that Belnap's choice of the truths with which (iv)-(v) can be seen to clash is needlessly distracting. In order to reject the claim that a certain definition is satisfied we need only find one fact from which its unsatisfiability follows. The simple fact that $A \vdash B$ does not generally hold is entirely sufficient in the present instance; Belnap's more elaborate considerations about conservative extensions and the system S, which in their own right are an attractive part of his article, can be bypassed as far as criticism of Prior is concerned. They become relevant only when we go on to give a general theory of which inferential definitions work.

6. G. Frege, in *Foundations of Arithmetic*, §67: "The definition of an object does not, as such, say anything about it, but fixes the meaning of a symbol. After this has happened, the definition transforms itself into a judgment about the object, but now it is on a level with other statements made about the object and no longer introduces it."

7. Cf. [11], ¶2: ". . . there is simply nothing more *to* knowing the meaning of 'and' than being able to perform these inferences." For the Fregean view attributed to Prior below, see in particular the talk about our knowledge and assignment of senses in [12], ¶6. Further confirmation of this interpretation and also of my view of Prior's target is his discussion of the difference between "informal pedagogy" and real givings of meaning in [12], ¶12.

8. It should be noted that our "taking a connective to have" a certain role might well consist not in our explicit logical beliefs but in our use of sentences containing this connective in inferences.

9. Although it is indeed hard to imagine an empirically plausible community of tonkers.

10. This view of vacuous names has been denied. We cannot enter into this controversy here–I am in any case concerned only to show how a natural Fregean reply to Prior might proceed–but interested readers should consult McKinsey's excellent [10] for references and a sensible position.

11. It may seem unreasonable to credit Prior with the view that no connective with the sense we have given 'tonk' could exist. I admit that I am unsure about my reconstruction of his very puzzling reasoning, but I would appeal to [12], ¶11, where Prior says that (iv)-(v) could not suggest a meaning to us because there is no such meaning for us to catch on to. And as I remarked above, Belnap also regards '*A* tonk *B*' as meaningless, so that my reading of Prior does not appear to be too far-fetched.

12. The standard reference is [6]. (Russell's views on acquaintance with universals, as given in, e.g., *The Problems of Philosophy*, are also very relevant.) Gödel is defended in [9], where other references appear. But what gives Gödel's position its force is not the number of its adherents but its status as an attractive response to a widely felt problem. See especially [2].

13. Much more discussion is required. An interesting contemporary reference is [7].

14. To my knowledge [9] is the first paper in which the problem of making mathematical "perception" conform to a detailed account of perception is taken seriously.

15. It would in any case be reasonable to let reference be determined by dispositions to use rather than by actual use, but these dispositions would also have to be idealized.

16. There is an interesting relation between the views of Stevenson and Field. I read Stevenson as suggesting that referential properties of connectives are somehow more basic than what one might call their *objective* inferential properties; that is, that what actually follows from sentences containing a connective should be understood in terms of the truth function it refers to. This seems to be close to Frege's viewpoint. Field need not disagree, but he would add that reference is in turn determined by *subjective* inferential properties ("conceptual role").

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