

The Linguistic View of *a Priori* Knowledge¹

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Abstract

This paper presents considerations against the linguistic view of *a priori* knowledge. The paper has two parts. In the first part I argue that problems about the individuation of lexical meanings provide evidence for a moderate indeterminacy, as distinct from the radical indeterminacy of meaning claimed by Quine, and that this undermines the idea of *a priori* knowledge based on knowledge of synonymies. In the second part of the paper I argue against the idea that *a priori* knowledge not based on knowledge of synonymies can be explained in terms of implicit definitions.

Some of our knowledge appears to be *a priori*, i.e. not evidentially based on experience. Much logical and mathematical knowledge is counted *a priori*; so is our knowledge of what Hume called relations of ideas, such as knowledge that every journey has duration. But how do we get such knowledge if not from the evidence of experience? What warrants these beliefs?

One answer is that they are known just by knowing facts of linguistic meaning, in particular the meanings of words by which *a priori* knowledge is expressed. This view was popular among 20th century philosophers until Quine destroyed their confidence.² In an

¹ Work on this paper was supported by the Arts and Humanities Research Council. I am very grateful to Paul Boghossian, Paul Horwich and Steven Gross for helpful comments on an earlier version of this paper, and to Genoveva Marti for discussion when we ran a graduate seminar together on analyticity.

² The relevant publications by Quine are these. 'Truth by Convention', *Philosophical Essays for Alfred North Whitehead*, O. Lee (ed.) (New York: Longmans 1936), reprinted in *Philosophy of Mathematics*, 2nd ed., P. Benacerraf & H. Putnam (eds.) (Cambridge: Cambridge University Press 1983); 'Two Dogmas of Empiricism.' *Philosophical Review* (1951), 20–43, reprinted in W. Quine, *From a Logical Point of View* (Cambridge, Mass: Harvard University Press 1980); 'Carnap and Logical Truth.' *Synthèse* (1960), 350–74, reprinted in W. Quine, *The Ways of Paradox and Other Essays* (Cambridge, Mass: 1976); *Word and Object* (Cambridge, Mass: MIT Press 1960).

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illuminating and persuasive paper Paul Boghossian tries to restore our confidence, by arguing that Quine's case is cogent only if one accepts his extreme view of meaning indeterminacy, and by presenting a positive account of *a priori* knowledge in terms of knowledge of meaning.³ Boghossian's account is far superior to older linguistic accounts and deserves attention. This paper is my response. While agreeing with much of what Boghossian says, I will argue that the linguistic approach to *a priori* knowledge is misguided.

1. Moderate Indeterminacy of Lexical Meanings

One way of getting *a priori* knowledge, on the linguistic view, is by validly deducing it from synonymies and logical truths that one already knows *a priori*. For example one might infer as follows:

Every akem is an akem.
“akem” means the same as “mula”.
Hence every mula is an akem.

There are three ways one might doubt that reasoning of this kind is a possible route to *a priori* knowledge of the conclusion. One might doubt that the relevant logical truth, the first premiss, is knowable *a priori*. Boghossian answers this in terms of implicit definitions, a topic that is dealt with in the second half of this paper. So put this doubt aside for now. Secondly, one might doubt that the relevant meaning fact is knowable *a priori*. Surely, the worry goes, one has to use the evidence of experience in order to come to know what a word means. That is true if one is thinking of a language as an essentially social entity; but if the language is your own idiolect, what you need to know is what you yourself mean by the relevant words, and that, it can be argued, is knowable directly, without any self-observation, just as you know whether you intend to watch a movie tonight. I strongly doubt that one has such knowledge of one's own idiolect, but I will not argue the point here (though my doubts occasionally surface). Finally, one might think that only in a narrow range of special cases are claims of the form “X means the same as Y” strictly true; in most cases, if the claim is not outright false, it is indeterminate, that is, there is no fact of the matter. This is the main topic of this section. I will argue that a doubt of this kind can be reasonable without commitment to the radical thesis that there are no meaning facts about individual words or

³ Paul Boghossian, ‘Analyticity Reconsidered.’ *Nous* (1996), 360–391.

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sentences. I will also argue that even if this is wrong, even if the meanings of the words of one's idiolect are completely precise and determinate, one's knowledge of the meanings of those words is not sufficient to deliver knowledge of synonymies beyond the special cases.

Boghossian invites us to consider the English word "cow" and the French word "vache". He points out that if they have determinate meanings the question whether they have the same meaning has a factual answer. For if "cow" has a determinate meaning, its having that meaning consists in its fulfilling some determinate condition. Then it must be determinate whether "vache" also fulfils that condition; if it does, it is a fact that the words mean the same; if not, it is a fact that they do not mean the same. Of course nothing in this argument depends on the difference in language; it goes through for the English expression "female bovine" in place of the French "vache" in just the same way. So we can draw a general conclusion, one not restricted to translation: non-factualism about synonymy entails indeterminacy of meaning. The view that I will defend is that there is enough indeterminacy of lexical meaning for ordinary statements of synonymy to fall short of fact-hood, but not so much indeterminacy that there are no facts of lexical meaning. I will call this *moderate indeterminacy* of lexical meaning, to distinguish it from *radical indeterminacy*, Quine's view that the smallest unit of meaning is a corpus of sentences testable without additional assumptions, so that words and even sentences are too fine-grained to have meaning at all.

Let's look at the English word "cow" and the French word "vache". Do they have the same meaning? The answer depends on the individuation of word meanings. Both "vache" and "cow" are used to refer to adult females of a bovine species.⁴ But in English "cow" is also used as "intimidate"; "vache" is not. We can deflect this problem by appealing to homonymy: that is, we can claim that English has *two* words with the pronunciation and spelling "cow", one a verb meaning "intimidate", the other a noun used to refer to female bovines.⁵ Grounds for such a claim are that the usages

⁴ There are several bovine species, including ox, bison, buffalo as well as domestic cattle (*bos taurus*). It is not clear to me whether the French word "vache" is applied to females of bovine species other than domestic cattle, but I will suppose for the sake of argument that it is.

⁵ The *Oxford English Dictionary* (*OED*) has eight top-level entries for "cow", six of them for nouns and two for verbs. I discuss only those corresponding to uses belonging to my idiolect, one noun and one verb. The entry for the noun has several subentries some with subentries of their own.

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belong to different syntactic categories, they have no apparent association of meaning, and they have different etymological roots, hence different cognates (“cowed” and *perhaps* “cower” and “coward” going with the verb “cow” but not with the noun).

Set aside the verb. There remain differences between the English noun “cow” and the French noun “vache”. The former is used to apply also to female elephants and female whales, as well as to the females of certain other species the males of which are called bulls;⁶ the latter is not. We cannot reasonably claim that there are several homonymous nouns “cow”, one for each of the relevant species: there appears to be some conceptual association and there is no difference of etymological root. If the noun “cow” has one meaning covering adult females of the bovine, elephant and whale species, it does not mean the same as “vache”. But the noun “cow” might be *polysemous*: it might have two or more related meanings.⁷ In that case *one* of the meanings of the English noun “cow” may be the same as the meaning of the French word “vache”.⁸ So the question whether the noun “cow” has a meaning with respect to which it is synonymous with the noun “vache” depends on a further question: Does the noun “cow” have *one* meaning covering several kinds (female bovines, female elephants, female whales etc.), or *two or more* related meanings with disjoint extensions, one of which covers just female bovines? Only if this question has a determinate answer does the question of synonymy have a determinate answer. So which is it: one meaning or many?

Lexicography is not going to help us here. There is no agreed taxonomic criterion for classifying uses as belonging to the same meaning of a given word. While the *Oxford English Dictionary*

⁶ The *OED* mentions also rhinos and seals; *Merriam-Webster Unabridged* mentions also moose and alligators.

⁷ The following example may help to convey the difference between homonymy and polysemy. (i) An otter will sometimes take over the den of muskrats dug into a river bank. (ii) My bank does not charge interest on overdrafts of less than £100. (iii) Do not bank on Clinton’s winning the nomination. The meanings of “bank” in (i) and (ii) are unrelated; so the occurrences of “bank” in (i) and (ii) are occurrences of distinct homonymous words. The meanings of “bank” in (ii) and (iii) are related; so the occurrences in (ii) and (iii) are occurrences of a single polysemous word.

⁸ For ease of exposition let us pretend that the usage of “vache” does not exhibit similar complexity. For the record, “vache” is also used for untreated cowhide (and there are slang and metaphorical uses, as for “cow”). This pretence makes no difference to the strength of the case for moderate indeterminacy.

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places the use of “cow” for female bovines under one subentry (of the relevant main entry) and its use for females of other species under a separate subentry, *Merriam-Webster Unabridged* places these uses together under the same subentry.⁹ In any case, we are interested in a person’s idiolect – yours, for example. So if there is a fact of the matter there must be something about *you* that makes it the case that the noun “cow” has (or does not have) just one meaning in your idiolect. What is this fact? Lexicography of course does not give us the answer.

A better place to look might be cognitive semantics. But the results do not support meaning determinacy. Several criteria for distinguishing the meanings of polysemous words have been used, but none are unproblematic and they produce conflicting results.¹⁰ The absence of a coherent set of criteria for establishing polysemy is one factor leading the lexical semanticist Dirk Geeraerts to deny that there is “a unique and optimal solution to drawing dividing lines around and between the meanings of a lexical item.”¹¹ Demarcational fuzziness, as Geeraerts puts it, is something we can and should allow for in our theorizing. The lexicologist Alan Cruse also underlines demarcational fuzziness; at the same time he resists radical indeterminacy:

... although in principle word meaning may be regarded as infinitely variable and context sensitive, there are nonetheless regions of higher semantic ‘density’ (metaphor is unavoidable here), forming, as it were more or less well-defined ‘lumps’ of meaning with greater or lesser stability under contextual change.¹²

There are two ways in which demarcational fuzziness may affect judgements of lexical meaning. First, given two occurrences of a phonologically individuated item, we may be unable to say whether their meanings are unrelated (homonymy) or related (polysemy). Secondly, we may be unable to say whether their meanings are

⁹ This is not an isolated case of discrepancy between dictionaries. For other examples see C. Fillmore and B. Atkins ‘Describing Polysemy: The case of “Crawl”.’ in Ravin and Leacock (eds.) *Polysemy: Theoretical and Computational Approaches*. Oxford: Oxford University Press 2000.

¹⁰ D. Geeraerts, ‘Vagueness’s puzzles, polysemy’s vagaries’, *Cognitive Linguistics* 1993, 223–272.

¹¹ D. Geeraerts, ‘The definitional practice of dictionaries and the Cognitive Semantic conception of polysemy’. *Lexicographica* 2001, 6–21.

¹² A. Cruse, ‘Aspects of the Micro-structure of Word Meanings.’ in Ravin and Leacock *Op. Cit.*

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related but distinct (polysemy) or the same (univocality).¹³ The present concern is with the second; it is a question of the individuation of meanings. Without going into technicalities we can get a sense of the difficulties by considering a couple of examples. Here is one.

Peter felt the cup.

Peter felt warm.

The cup felt warm.

In the first sentence “felt” is a transitive verb; that is, it takes a direct object, hence signifies a binary relation. In the second and third it is intransitive. So this distances the first use of “felt” from the other two. But also, in uttering the second sentence we do not attribute to Peter what, in uttering the third, we attribute to the cup: Peter *has* an experience of warmth, while the cup *gives* an experience of warmth. So there are clear differences between the three uses of “felt”. But if we postulate three meanings for the three uses, we shall be ignoring the tightness of the conceptual connection between them. For contrast, consider the looser connection between the uses of “bank” in

Don’t bank on winning the nomination.

Don’t bank with Lloyds.

A way to deal with this is to say that the displayed occurrences of “felt” have one and the same meaning, but the meaning is not a *use* of the word “felt”; rather it is a function¹⁴ or an input-output system that takes representations of features of utterance context and pragmatically relevant background beliefs as input, and delivers as output a unique use (or use-governing representation).¹⁵ A functional account of lexical meaning accommodates the close conceptual connection between the three uses of the verb “felt” mentioned earlier: those uses are outputs of a single meaning.

However, while a functional account of lexical meaning may be an advance, it does not eliminate moderate indeterminacy. One pair of

¹³ Linguists sometimes use the word “monosemy” for singularity of meaning, as in *Polysemy or monosemy: interpretation of the imperative and the dative-infinitive construction in Russian* by E. Fortuin.

¹⁴ These are intensional functions (algorithms), not extensional functions (sets of ordered pairs).

¹⁵ For ease of exposition I will oversimplify by writing as though (i) individuation of uses is not also afflicted by a degree of indeterminacy and (ii) the outputs are uses, rather than use-governing representations.

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uses may be clearly too different to be outputs of the same meaning and another pair may be clearly too close to be outputs of distinct meanings; but sometimes a pair of uses may be neither different enough nor close enough to settle the matter. Cruse's discussion of the noun "book" illustrates the problem.¹⁶ The noun "book" can be used to refer to a material item or an abstract text:

Please return my book.

My book was translated into Japanese.

One property that serves to distance the use of "book" for a material object from its use for an abstract text is that one and the same question involving the noun "book" may be truly answered both positively and negatively:

A: Is that my book you are reading?

B: (i) No, I borrowed it from my brother.

(ii) Yes, it is your first novel.

Cruse presents seven such distancing properties of the two uses of "book", thereby suggesting that the two uses are outputs of different meanings. But Cruse also lists five other properties which count as unifying, thus suggesting that the two uses are outputs of the same meaning. For example, both uses can be operative simultaneously:

Ken is reading a book.

Were you happy with the book John gave you?

Uses issuing from distinct meanings can be operative simultaneously, but the result is usually incongruous and sometimes amusing, as with the following occurrence of "took":

He took his leave and my wallet.

But there is no incongruity in the occurrences of "book" I have cited, nor in the following example, where "book" has to refer to both abstract text and material object:

I found the book riveting but the print is way too small.

Are the two uses of "book" outputs of different meanings? Or the same meaning? Surely to insist that there is a fact of the matter in cases like this is to be carried away by a theoretical inclination to discretize lexical meaning. This inclination would be justified only if we had a clear understanding of what constitutes an individual lexical meaning and good reason to believe that the criteria of individuation

¹⁶ Cruse, *Ibid.*

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for lexical meanings permit no indeterminacy. Our attempts to grapple with the linguistic data show that we do not have these things. The demarcational fuzziness found by lexical semanticists is not an artefact of averaging over different idiolects; individual people do not report precise meaning boundaries for the words they use.

A diehard might respond that, though the words of a person's idiolect have totally precise meanings, the person does not know what they are. There are two problems with this response. First, while this is a possibility, no reason has been found to believe the hypothesis of precise lexical meanings; secondly, even if it were true that the words of a person's idiolect have precise meanings unknown to the person, that would be ultimately irrelevant for our epistemological concerns, as the linguistic view of *a priori* knowledge requires that people know just what their words mean.

Let us return to the original idea using one of the examples discussed earlier, namely, that we can come to know that all cows are female bovines without empirical evidence, by making the following inference:

All cows are cows.

“Cow” means the same as “female bovine”.

So, all cows are female bovines.

For this to result in *a priori* knowledge a number of conditions have to be met. First, as “cow” also means “intimidate”, the statement of synonymy must allude implicitly to the current context; we are talking about the meaning that “cow” has as used in the sentences of this argument. Let us grant this. Secondly, it must be a fact that while that meaning of “cow” yields its use to refer to female bovines, it does not also yield its use to refer to females of non-bovine species. Its use to refer to female elephants, for example, must be the output of a different lexical meaning, otherwise the statement of synonymy is false. Finally, this putative meaning fact must be known *a priori* by the speaker. This final requirement is not met. We lack grounds for thinking that the lexical meanings of the noun “cow” divide so that its use to refer to female bovines is the output of one meaning and its use to refer to female elephants is the output of another; we lack grounds for thinking even that the question whether the two uses belong to the same lexical meaning has a determinate answer. The general point is this. For the linguistic story to work at all, words must have determinate meanings *and* speakers must know just what those meanings are. In many examples this double requirement is not satisfied.

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Perhaps I have been using examples that are untypical. So let us consider an example that has often been regarded as favourable to the idea of truths known solely on the basis of knowledge of meanings: the claim that every bachelor is an unmarried man. Quine introduced the example in order to dispute the linguistic view.¹⁷ But this is a case that has helped to persuade people that Quine's claims of meaning indeterminacy were exaggerated.¹⁸ Surely "bachelor" does mean the same as "unmarried man", doesn't it? This is not a straightforward case, as the two expressions are not even co-extensional in the idiolects of some English speakers, myself included. Here are two counter-examples to the putative synonymy, adapted from actual situations.

Peter once married a refugee merely to save her from deportation to a country suffering civil war; after the marriage ceremony he never saw her again, but continued to live as young single men do, available for a long-term personal partnership. In the eyes of those whose idiolects are now under discussion, Peter remained a bachelor; beyond legal contexts it would be wrong to deny it. (At the same time it would be accepted, without any inconsistency, that there is a *legal* use of "bachelor" under which Peter was no longer a bachelor, just as there is a *legal* use of "guilty" according to which a murderer is not guilty of the murder if he has not been declared so as a result of a legal trial.) Mike, in contrast, never married; he and his partner have been together for several decades, have two adult daughters and continue to live together as companions. While Peter is a married bachelor, according to those whose idiolects are under consideration, unmarried Mike is not a bachelor.

One response to this is to say that the non-legal meaning of the word "bachelor" has changed: it used to mean the same as "unmarried man" in the past, but under the impact of a change of social mores it has lost that meaning. So, one may think, what was in the past expressed by "every bachelor is an unmarried man" was known to be true solely on the basis knowledge of linguistic meanings.

¹⁷ Quine, 'Two Dogmas of Empiricism.' *Philosophical Review* (1951), 20–43, reprinted in W. Quine, *From a Logical Point of View* (Cambridge, Mass: Harvard University Press 1980).

¹⁸ Putnam, H. 1962: "The Analytic and the Synthetic" reprinted in H. Putnam *Mind, Language and Reality: Philosophical Papers, volume 2*. Cambridge: Cambridge University Press 1975. Originally published in Feigl, H. and Maxwell, G. eds. *Minnesota Studies in the Philosophy of Science*, III. Minneapolis: University of Minnesota Press.

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It is certainly true that (British) social mores have changed in a relevant way: significantly less importance is placed on marriage status now than in the mid-20th century and before. It is not very unusual for people to form permanent partnerships and raise a family without getting married; the expression “living in sin” for cohabitation without marriage is now jocular, and no stigma attaches to a child whose parents did not go through the relevant ceremonies.

But there is an alternative description of the situation: what changed under the impact of changes in social mores was not the ordinary non-legal meaning of “bachelor”, but a belief about bachelorhood. The belief that for a man being unmarried is necessary and sufficient for bachelorhood has been discarded. It was a reasonable generalization *given* the social mores of yesteryear. But we failed to consider the possibility that marriage could be given much less importance, that long-term personal partnerships between unmarried people could become respectable hence less rare, and that being married need not inhibit a bachelor’s conditions of life. Had we given due consideration to this possibility, we would have seen that being unmarried, for an adult male, was merely a reliable indicator of bachelorhood in the circumstances that then prevailed, not a necessary condition of bachelorhood.

Either of these descriptions might apply to someone who would once have assented to “all bachelors are unmarried men” (and its converse) but would now deny it. The challenge for meaning-determinists is to say what fact about such a person makes one rather than the other of these descriptions the correct one, and to explain their reasons for the choice of fact. If there is no such fact, as seems to be the case, it is indeterminate whether even in the mid-20th century, “bachelor” was synonymous with “unmarried man” in the relevant idiolects.

This kind of situation is liable to arise with big intellectual changes. Under the impact of advances in mathematics and physics, people came to disbelieve that between any two points there is a unique shortest path which is a straight line, whereas it had been believed to be obviously true. Again there are two accounts and no clear ground for regarding one rather than the other as correct. On one account what was expressed was indeed known true by knowing that “straight line” means “shortest path between two points”; but the meaning of one or both of these expressions changed. The alternative possibility is that there was no change of meaning, just a change of belief that comes with acceptance of the cosmic geometry of the General Theory of Relativity. Which of these accounts is correct? Moderate indeterminacy allows that there is no fact of the

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matter, and that that is why we have not found a principled way of deciding it. When novel situations or theoretical advances bring significant changes in the statements people accept, we may be unable to decide whether the changes are due to changes of lexical meaning or changes of belief. The most plausible explanation may be simply that there is no fact of the matter.

The kind of indeterminacy proposed here is moderate. There are lexical meaning facts, on the moderate view. For example, the noun “cow” does have a meaning which allows (i.e. has as one of its possible outputs) its use for female domestic cattle, but does not have a meaning which allows its use for female spiders. The fact that some questions of lexical meaning lack answers does not entail Quine’s radical indeterminacy thesis that words and even sentences are too fine to bear meanings at all.

Quine infers radical indeterminacy from his claim that we can correctly translate utterances from one language into another and correctly translate them back into the first language, but end up with something radically non-equivalent to the original.¹⁹ His argument works by excluding as irrelevant the fact that in translating (and interpreting) we have the evidence of contextual features and non-linguistic background beliefs²⁰ to single out thoughts expressed.²¹ But this is an important error. Correct translation (and interpretation) is almost never possible in the absence of these extra data — witness the continuing weakness of computer translation, despite a huge research effort and a massive increase in computing power. On the functional view of meaning this is only to be expected. The use of a word on a particular occasion depends not only on its standing meaning but also on contextual features and background beliefs. Translation and interpretation involve semantic processing of course; but they also require pragmatic processing, first to select one meaning for each word, and then to decide its use on that occasion given context and background beliefs.

Although the sample of words on which the case for moderate indeterminacy rests is small, any plausible explanation of indeterminacy

¹⁹ Quine, *Word and Object*. Ch. 2.

²⁰ This will include the belief that speakers of other languages are cognitively much like us, and so they do not parse biological entities as finely sliced temporal stages etc. See Quine, ‘On the Reasons for Indeterminacy of Translation.’ *Journal of Philosophy* (1970), 178–183; ‘Indeterminacy of Translation Again.’ *Journal of Philosophy* (1987), 5–10.

²¹ I ignore the possibility that a person may make a statement without expressing a determinate thought.

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phenomena is likely to apply widely beyond the sample. Words such as “cow” and “bachelor” would seem to be most favourable cases for determinacy. If determinacy fails for these words, it is likely to fail for many others, especially words philosophers care about, such as “know”, “explain”, “rational”, “just” and “free”. It is reasonable to think that moderate indeterminacy of meaning, hence of synonymy, is the norm.

But there are exceptions. Even Quine admits one kind of exception to the indeterminacy of synonymy. These are given by explicit definitions introducing a new notation (or a special use of an old notation) for the sake of abbreviation, as is common in mathematics and some parts of science. Quine writes “Here we have a really transparent case of synonymy created by definition; would that all species of synonymy were as intelligible.”²² Despite this, Quine downplays the importance of these cases of determinate synonymy. To do justice to the linguistic view of *a priori* knowledge, we should examine Quine’s reasons for doing so.

Quine holds that the defining sentence is true by conventionally established synonymy and by logic.²³ But he also holds that the conventional force of an explicit act of definition is a passing trait that does not outlast the act itself: “It is a trait of events, not of sentences.”²⁴ Usage of the defined term subsequent to the act of defining may go its own way; it may even involve rejection of the defining sentence. So determinate synonymy, on Quine’s view, is fleeting; it does not support enduring *a priori* knowledge.

It seems to me that Quine is doubly wrong here. Consider first the claim that at the time of the defining act the defining sentence expresses a true statement of synonymy created by definition. One problem, brought to my attention by Dorit Bar-On, is that no truth has been stated until the act, the uttering or inscribing of the defining sentence, is finished. Before the act is over the definiendum does not have its new meaning, nor any meaning at all if it is a new expression. The act is one of giving meaning to an expression, not one of making a statement. So during the process of uttering the sentence we have not yet got the synonymy that would make what is said true; it becomes true only after the act, contrary to Quine’s view.

²² Quine, ‘Two Dogmas of Empiricism.’

²³ “Even an outright equation or biconditional connection of the definiens and the definiendum is a definitional transcription of a prior logical truth of the form ‘ $x = x$ ’ or ‘ $p \equiv p$ ’.” Quine, ‘Carnap and Logical Truth.’

²⁴ Quine, *Ibid.*

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The second claim is that the conventional force of the act cannot long outlast the act itself. For concreteness, consider the introduction of a term in a mathematics book:

A *regular open* set is an open set that is equal to the interior of its closure.

In fact this definition or something trivially equivalent is found in many books. But let us just focus on the occurrence of this sentence as a definition in a particular book. It would be absurd to claim that this definition was only in force immediately after the author's originally typing out the defining sentence or only immediately after publication. For then it would no longer have the force of a definition by the time most of the readers came to read it. Clearly it can have the force of a definition years after the publication date. The scope of its force as a definition is not a temporal period, short or long: the definition will be in force for people while reading and thinking about the text following the definition²⁵ whenever they do it, and it will be in force in the thought of those whose use of the term is taken from the definition in the book.

So explicitly introduced abbreviating definitions provide clear, knowable cases of determinate synonymy. Hence one obstacle to the linguistic view of *a priori* knowledge can be overcome in these cases. But we need to be cautious. There is a pre-condition on the reliable use of definitions, and this threatens the linguistic view. This matter is discussed in the next section. There may also be a small number of knowable synonymies not resulting from explicit abbreviating definitions, such as "penultimate" and "last but one", but my impression is that such cases are quite rare. So the general situation is just this. In a narrow range of cases claims of the form "X means the same as Y" are strictly true; but in most cases the claim, if not false, has no determinate truth-value. So *a priori* knowledge via knowledge of synonymies is at best very restricted.

²⁵ An author could explicitly revoke and replace the definition later in the text. In that case the definition is operative only up to its explicit revocation. The question of scope arising when an author uses a defined term in obvious contravention of the definition without its explicit revocation is trickier. But I would guess that such occurrences in mathematics texts are rare.

2. Implicit Definitions

While some *a priori* knowledge, on the linguistic view, is based on knowledge of synonymies, much is not. This other kind of *a priori* knowledge is taken to include logical knowledge and knowledge of non-logical truths such as “Everything that has shape is extended” and “There are no negative cardinal numbers”. How is *a priori* knowledge in these cases possible? Boghossian’s answer appeals to implicit definitions. He sets out the strategy for basic logical knowledge,²⁶ and suggests that the same strategy can be adapted to deal with other cases. I will first set out the strategy and then proceed to evaluate it.

The thesis of implicit definition, as applied to logical expressions (constants), is this:

It is by arbitrarily stipulating that certain sentences of logic are to be true, or that certain inferences are to be valid, that we attach meaning to the logical constants. More specifically, a particular constant means that logical object, if any, which makes valid a specified set of sentences and/or inferences involving it.²⁷

Boghossian illustrates the case for the logical constant ‘and’, setting out the introduction rule and two elimination rules of propositional logic. Having implicitly defined logical constant C by stipulating that inference forms A_1, A_2, \dots, A_n , (involving C) are valid, appeal is made to the following sort of argument (for k such that $1 \leq k \leq n$).²⁸

1. C means that logical object which makes valid all inference forms A_1, A_2, \dots, A_n .
2. If C means that logical object which makes valid all inference forms A_1, A_2, \dots, A_n , then A_k is valid.
3. So A_k is valid.

Given that the premisses are known *a priori*, the argument then provides *a priori* warrant for belief in the validity of an inference form in the specified set.

²⁶ Boghossian gives an account of basic logical knowledge substantially different from the linguistic one given in ‘Analyticity Reconsidered’ in a more recent paper, ‘Blind Reasoning’ *Proceedings of the Aristotelian Society, supp. vol. 77*, (2003), 225–48.

²⁷ This is taken verbatim from Boghossian, ‘Analyticity Reconsidered’ p. 376.

²⁸ This is adapted from Boghossian, *ibid.* p. 386. This adaptation retains a use of “means” that amounts to “refers to” or “has as its semantic value”.

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It is no part of the linguistic view that anyone actually acquires logical knowledge via meaning-fixing stipulations and arguments of the form just given. It is clear that it is not so. People surely know, without coming across any defining stipulation for “every” or “and”, that every dog is a dog, and that if Don is cold and Abe is tired, Abe is tired. So how does the whole implicit-definition story relate to our actual logical knowledge? The line of thought seems to be this: the implicit-definition story shows how one *could get a priori* logical knowledge, and our actual logical knowledge is related to possible knowledge by implicit definition in such a way that it too counts as *a priori* knowledge.

In what follows I will question the claim that the implicit-definition story is a possible route to *a priori* knowledge. To do this I would like to get clear about the difference between implicit and explicit definition. The terminology is slightly confusing, because a term can be implicitly defined by means of an explicit act of stipulation. This is what is under consideration here. Boghossian’s proposal is that one explicitly stipulates, for example, that inferences of the following form are valid, where “p” and “q” are schematic variables for sentences with truth values and “C” is the constant term to be defined:

pCq . Therefore p. pCq . Therefore q. p; q. Therefore pCq .

Let us follow custom in taking the semantic value of such a constant to be a function of truth values. A definition of a function is explicit when it specifies the function’s outputs for any given inputs. The truth table for conjunction does precisely this. In fact the truth table is an ultra-explicit definition, because the function’s output is named separately for each possible input to the function (each ordered pair of truth values). Here is a familiar example of a definition that is explicit but not ultra-explicit. Let ‘*a*’ and ‘*b*’ be variables for sets and ‘ \cap ’ be the term to be defined:

$a \cap b =_{df}$ the set whose members are exactly
the members of both *a* and *b*.

The line of my argument in what follows is this. For a term introduced by an explicit definition to be usable to gain knowledge, a certain acceptability condition must be fulfilled; that same condition holds if the definition is implicit. But meeting the acceptability condition calls on further resources, and so the implicit-definition story is at best incomplete.

For a singular term introduced by an explicit definition to be usable to gain knowledge, we must already know that exactly one thing fulfils

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the defining condition. If more than one thing fulfils the defining condition, false identity statements follow. If nothing fulfils it, false existential statements follow. In some cases this situation leads even to contradiction: if g is defined to be the greatest natural number, it follows from arithmetical axioms that $g < g + 1$ and from the definition that $\neg [g < g + 1]$. The same problem can arise for definitions of function terms. Define a term for a set function thus:

$a\Psi b =_{\text{df}}$ the set whose members are exactly
the things that are members of neither
 a nor b , or of both a and b .

Given that there is a set a , it follows that $a\Psi a$ is universal; using Zermelo's Separation Axiom we can then derive the Russell-Zermelo antinomy. If we extend our language by defining a singular term or function term without knowing that the defining condition is fulfilled by exactly one thing, we have grounds to fear that reasoning with the defined term is liable to result in error.

Even if in fact exactly one thing does fulfil the defining condition of a definition, we need to know this independently, if reasoning that uses the definition is to yield knowledge. To see this, consider the following. Gauss conjectured, correctly as it turned out, that the number of primes less than or equal to n , $\pi(n)$, is asymptotic to a certain function $\text{Li}(n)$ —the details need not detain us.²⁹ Gauss also conjectured that this function always over-estimated $\pi(n)$, that is, $\pi(n) < \text{Li}(n)$ for all integers n greater than 2. Now suppose we define a new constant term thus:

$@ =_{\text{df}}$ the least integer n greater than 2 for which $\pi(n) \geq \text{Li}(n)$.

With this definition in hand we can now deduce quite trivially that there is a counter-example to Gauss's second conjecture.³⁰ Is this a way of coming to know that that conjecture is false? Of course not. In fact the conjecture *is* false, and the defining condition for $@$ is uniquely fulfilled, but this is not easy to prove.³¹ The moral is that regardless of whether the definition succeeds in defining something,

²⁹ $\text{Li}(x) = \int_2^x 1/\ln(u).du$ This is sometimes known as 'the logarithmic integral'.

³⁰ $@ = @$; so $\exists n [n = @]$; so $\exists n [n > 2 \ \& \ \neg [\pi(n) < \text{Li}(n)]]$.

³¹ It was proved by Littlewood in 1912. The least counter-example is almost certainly greater than 1.3×10^{316} . In 1914 Littlewood proved that there are infinitely many counter-examples.

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we have to *know* that the defining condition is uniquely fulfilled, if reasoning that involves the defined term is to yield knowledge. In mathematics this requirement is standardly met by a proof of existence and uniqueness prior to giving the definition.

So much for explicit definitions. What about implicit definitions? An implicit definition of a function term does not specify the outputs for given inputs; it is just a stipulation that certain inference forms involving the term are to be treated as valid. An implicit definition of ‘ Ψ ’ corresponding to the explicit definition of this symbol given earlier, for example, stipulates that all inferences of the following forms are to be treated as valid:

$x \in a\Psi b$. Therefore $x \notin a$ and $x \notin b$, or $x \in a$ and $x \in b$.

$x \notin a$ and $x \notin b$, or $x \in a$ and $x \in b$. Therefore $x \in a\Psi b$.

It is easy to see that this leads to just the same trouble as the explicit definition for ‘ Ψ ’, for the two inference forms license the following:

$x \in a\Psi a$ if and only if $x \notin a$ or $x \in a$.

So $a\Psi a$ is universal; hence by the Separation Axiom contradiction follows. This kind of problem besets expressions for relations and predicates just as much as singular terms and function terms. Here is an implicit definition for a binary relation symbol ‘ T ’ (which can be read ‘is true of’):

[Ti] $F(x)$. Therefore ‘ F ’ Tx . [Tii] ‘ F ’ Tx . Therefore $F(x)$.

Now we define a predicate symbol ‘ H ’ (which can be read ‘is heterological’):

[Hi] $H(x)$. Therefore $\neg xTx$. [Hii] $\neg xTx$. Therefore $H(x)$.

From these we get the ‘heterological’ paradox.³²

These examples illustrate the fact that implicit definition is subject to the dangers mentioned earlier no less than explicit definition; if our use of an implicitly defined term is to be reliable, we need to know, independently of the definition, that exactly one thing, when taken as the semantic value of the defined term, makes valid all the specified inference forms. The point is not restricted to logical and mathematical expressions implicitly defined by means of inference forms. If we define a non-logical expression by means of sentences in which the

³² $H('H') \rightarrow \neg 'H'T'H$ [by Hi] $\rightarrow \neg H('H')$ [by Ti]. $\neg H('H') \rightarrow \neg 'H'T'H$ [by Tii] $\rightarrow H('H')$ [by Hii].

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expression figures, we do not thereby ensure that there is a unique thing which, taken as the semantic value of that expression, makes the sentences true. Think of “incubus”, “phlogiston”, and “vis viva”, for example.

To see the relevance of the point for the claim that implicit definition gives *a priori* knowledge of the validity of an inference form, let us look again at the argument that is supposed to establish this. Let k be one of the integers 1 to n .

1. C means that logical object which makes valid all inference forms A_1, A_2, \dots, A_n .
2. If C means that logical object which makes valid all inference forms A_1, A_2, \dots, A_n , then A_k is valid.
3. So A_k is valid.

This argument cannot deliver knowledge that its conclusion is true unless the first premiss is known, and that cannot be known unless we already know that there is a unique logical object which makes valid all the inference forms A_1, A_2, \dots, A_n . So the argument on its own is insufficient to give us knowledge of its conclusion.

One might think that we can know, without prior argument, that there is a unique logical object which makes valid the relevant inference forms, because we can make it so just by stipulation. Boghossian seems to have thought this: ‘It is by arbitrarily stipulating that certain sentences of logic are to be true, or that certain inferences are to be valid, that we attach meaning to the logical constants.’ We can stipulate that certain inference forms are to be *treated as* valid. But it is an illusion to think that by stipulating we can ensure that inference forms *are* valid, as the example of the ‘true of’ operator and the ‘heterological’ predicate illustrate.³³ This point is substantiated in Paul Horwich’s more thorough discussions of stipulation.³⁴

But what about Kripke’s ‘metre’ example? Surely the stipulation that ‘metre’ names the length of a given stick S at specific time t suffices for *a priori* knowledge of the fact that the length of stick S at time t is a metre? Under certain assumptions this *may* be right, but parallels of those assumptions do not hold in the case of logical

³³ So do Prior’s rules for ‘tonk’ in ‘The Runabout Inference Ticket’ *Analysis* (1960), 38–9. But one cannot generally follow those rules, whereas the rules for ‘true of’ and ‘heterological’ fail only at isolated points.

³⁴ Horwich, ‘Stipulation, Meaning, and Apriority’ in P. Boghossian and C. Peacocke (eds.) *New Essays on the A Priori*. (Oxford: Oxford University Press 2000). See also Ch. 6 of Horwich, *Reflections on Meaning*. (Oxford: Oxford University Press 2005).

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stipulations. If the stipulation is a dubbing of something present, something we are directly aware of, and if we are using the description 'the length of S at t' merely to indicate the thing right there in front of us that we want to name, it *may* be right to say that we can know merely by the stipulation that the length of stick S at time t is a metre. But if the length of S at t is not something present to us, if it might be the case that S has no length at t, or that S has more than one length at t (as would be the case if length is relative to some independent parameter), then we do not know by the stipulation alone that the length of stick S at time t is a metre; for all we know the situation might be as for the stipulation that Vulcan is the planet whose mass and orbit explain the anomalous perihelion of Mercury.

It is clear which of these two sorts of situation obtain if we stipulate that a term C is to stand for the logical object which makes valid inference forms A_1, A_2, \dots, A_n . This is not a naming of something present, something we are directly aware of; on the contrary, it is a theoretical posit. Hence it might be the case that no logical object, when taken to be what C stands for, makes valid those inference forms; or more than one logical object might make them valid. So the analogy with Kripke's example does not show that we can know *a priori* that a unique logical object, if taken as the semantic value of C, makes valid the inference forms A_1, A_2, \dots, A_n .

So the implicit definition strategy is at best incomplete. Perhaps we can make up the deficiency by appending the master argument to a proof that there is exactly one logical object that makes the relevant inference forms valid. But if that were our route to knowledge that a given inference form is valid, the knowledge would depend on knowledge of the premisses of the proof, which are not going to be about linguistic meanings.

There is a further problem for any attempt to provide warrant for basic logical beliefs by means of deductive arguments. Those arguments themselves use logical inferences, so there is a threat of circularity. The master argument uses *modus ponens* and conjunction elimination, for example; so it cannot be used to provide warrant for those inference forms.³⁵

Boghossian has a response to this. The circularity involved does not consist in the use of a *premiss* that could not be rationally believed unless one already believed the conclusion; rather it consists in the use

³⁵ Perhaps we can find for each basic inference form a suitable argument for it that uses only other basic inference forms. But as the totality of basic inference forms is finite, there would still be an epistemic circularity even if no individual argument were circular.

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of an *inference rule* that could not be rationally used unless one already accepted the validity of that inference rule. In short, the relevant arguments are rule-circular, not premiss-circular. Citing Michael Dummett, Boghossian suggests that while a rule-circular argument is not rationally compelling for people who genuinely doubt that the inference rule is valid, a rule-circular argument (without any other flaw) does provide warrant for people who do not genuinely doubt its validity.³⁶

Without some further refinement this cannot be right. To see this, consider someone who regards as sacred a certain book — call it SB — and consequently does not genuinely doubt the validity of the following rule, which I will call *the sacred book rule*:

It says in SB that p. Therefore p.

Suppose moreover that it says in SB that everything said in SB is true. Then the following rule-circular argument is available to our believer:

It says in SB that everything said in SB is true.

So, everything said in SB is true.

So, any inference of the form “It says in SB that p. Therefore p.” is truth-preserving.

So, the sacred book rule is valid.

This argument, it is clear, does not provide the believer with any genuine warrant for believing that the sacred book rule is valid. But if this rule-circular argument cannot do the job for believers in this rule, why would any other rule-circular argument succeed for believers in other rules?

A possible reply is this.³⁷ Some other rules, such as those used in Boghossian’s argument, really are valid, while the sacred book rule is not. So rule-circular arguments employing those other rules provide justification whereas the rule-circular argument employing the sacred book rule does not. This reply goes wrong in claiming that the sacred rule book is not valid, as the data of the story leave it open whether SB says something untrue. It may be that all claims made in SB, aside from its self-endorsement, are true; in that case its self-endorsement would also be true, and so the sacred book rule would be valid. But if one does not have independent

³⁶ ‘Analyticity Reconsidered’ p. 374, 386–7. Boghossian is not quite so explicit, but less than this would not meet the objection.

³⁷ Steven Gross brought this possible reply to my attention, without expressing confidence in it.

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reason to think that the sacred book rule is valid, the rule-circular argument does not give us warrant to believe that it is valid. This seems to be the situation that Boghossian considers with respect to basic logical rules, such as *modus ponens*. It is not assumed that we have reasons independent of the rule-circular argument he offers to think that *modus ponens* is valid. So the rule-circular argument provides no warrant for believing that *modus ponens* is valid, even if one does not doubt its validity.

What if we relax the account of implicit definition? In Boghossian's account, a word is implicitly defined by an act of stipulation. But we seem to have *a priori* logical knowledge independently of any such acts of stipulation. The same goes for other candidates for *a priori* knowledge, such as "whatever is actual is possible" and "every journey has duration." What are the consequences if we allow that words can be implicitly defined without stipulation? Suppose we treated certain inference forms (sentences) involving a given word not merely as valid (true) but as neither in need of justification nor in danger of refutation; suppose also that all our uses of the word, apart from their use in these inference forms (sentences), could be simply explained by reference to these attitudes to the inference forms (sentences). This supposition will be unlikely given the considerations of the previous section; but for the sake of argument let us set them aside. Then we could take the inference forms (sentences) to implicitly define the word, without any stipulation.³⁸

This certainly reduces the artificiality of the implicit definition story. But does it help to show how any of our knowledge (logical or not) can be *a priori*? Don't we still have to know that exactly one thing makes all the implicit definers valid (or true)? Moreover, implicit definition without explicit stipulation produces another problem: How can one know *a priori* which inference forms (sentences) are the implicit definers? There will be many inference forms to which we have the appropriate attitudes, more than we need for an implicit definition of the given term. In the case of 'and' for example, we surely treat the following as neither needing justification nor vulnerable to refutation:

p and q. Therefore q and p.

How do we know whether this is an implicit definer of 'and'? Is our inclination to accept 'p only if q' after following a deduction of q from p an implicit definer of 'only if'? Is negation introduction

³⁸ Something akin to this is proposed by Horwich in *Meaning*. (Oxford: Oxford University Press 1998), ch.3 and in *Reflections on Meaning*, ch.2.

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by *reductio* an implicit definer of 'it is not the case that'? Our attitudes to all of these inference forms may be of the appropriate kind. If we are to have knowledge of meanings constituted by implicit definition in the way described earlier, we need to know which of the inferences forms figure as basic in the best explanation of our overall uses of the word. While we can know just by reflection what attitude we have to an inference form, we cannot know just by reflection whether that inference form is basic in the best explanation of our overall use of the word. That can only be known indirectly, by trying to explain the overall regularities of usage revealed by empirical investigation. So the resulting knowledge would not be *a priori*. The same goes for the implicit definers of the non-logical words in such sentences as 'everything shaped is extended'.

Finally, the problem of moderate indeterminacy again threatens, once we adopt the more realistic story of implicit definition: for some words there may be no determinate set of implicit definers. What, for example, are the implicit definers of the logical word 'if'?

3. Conclusion

I conclude that we should not try to account for *a priori* knowledge in terms of knowledge of linguistic meanings. The linguistic approach has a distinguished pedigree and proves attractive. So its weaknesses need to be exposed in order to prevent a time-wasting return to it. I have concentrated on Boghossian's version of the linguistic approach; while it is conceivable that there are better versions, his is better than any I know of; and I cannot discern a substantially stronger case. Hence my general conclusion.

In opposing the linguistic approach, I have argued for several claims. One is that *a priori* knowledge via known synonymies is threatened by a moderate indeterminacy of meaning which does not collapse into radical indeterminacy. Moreover, even if in fact lexical meanings were completely determinate, it is clear that our knowledge of meanings does not in general enable us to tell which statements of synonymy are strictly true. I have not contested the assumption that one can know the meanings of words in one's idiolect without empirical study of one's actual uses of words, though this assumption seems doubtful to me. The other major claim is that to get logical or other (non-linguistic) knowledge by definition, we need to know a substantial fact, namely, that exactly one thing makes the defining inference forms valid (or the defining sentences true), and we have no reason to think that this can be rationally inferred from facts of meaning. In the

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case of basic logical knowledge the linguistic approach also suffers from vicious circularity.

The problems facing any attempt to devise a satisfactory epistemology of *a priori* knowledge are substantial. Recognising the difficulties, one might suspect that no epistemology of *a priori* knowledge is possible, or even that there is no *a priori* knowledge. But we do not have good grounds for these despairing claims. At the present time we are still at the stage of exploring possible avenues; my conclusion here is only that the linguistic avenue takes us in the wrong direction.

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