

Good and Bad Bach

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This paper is concerned with Bach's stand on the "semantics-pragmatics" issue. A bit of Good Bach is his skepticism about the evidential role of intuitions. Another bit is his firm stand against the widespread confusion of what constitutes the meanings of utterances with how hearers interpret utterances. The paper argues at length against two bits of Bad Bach. (1) There is no sound theoretical motivation for his excluding the reference fixing of demonstratives, pronouns and names from "what-is-said". (2) His methodology for deciding what is "semantic" is flawed in three respects: first, in its commitment to the mistaken Modified Occam's Razor; second, in its placing inappropriate syntactic constraints on conventional meanings; and, third, in explaining many regularities in usage as standardizations rather than conventionalizations. This flawed methodology has the conservative effect of ruling out new meanings.

Key words: semantics, pragmatics, meaning, communication, intuitions, methodology, what is said, Modified Occam's Razor, dead metaphors, definite descriptions, conventions, standardization.

1. Introduction

Paul Grice (1989) made it clear, if it was not already so, that we need to distinguish between two sorts of properties that a particular utterance may have. Grice himself distinguished "what is said" from what is "implied, suggested, meant" (1989: 24), giving many interesting examples. A famous one concerns a philosopher who writes a reference for a student. The philosopher *says* that the student's English is excellent and his attendance regular but what the philosopher *means*, his "conversational implicature", is that the student is no good at philosophy (33). It is common to distinguish the two sorts of properties using the vexed terms "semantic" and "pragmatic".¹ Thus the problem of drawing

¹ These terms have distressingly many uses in the literature, as Kent Bach nicely demonstrates (1999). I shall explain my usage in sec. 2.2; see also 2013b, secs. 3 and 4.

the line between the two sorts of properties becomes the “semantics-pragmatics” issue.

We can roughly divide those struggling with this issue into two camps. In one camp are the conservatives who want to stay close to the traditional “semantic” way of thinking about linguistic phenomena, a way that has its roots in formal semantics; Herman Cappelan and Ernie Lepore (2005), and Jason Stanley (2007) are examples. In the other camp are the “contextualists” or “pragmatists” (as Stephen Neale calls them), who reject the tradition and emphasize “pragmatic” variations in the truth conditions of utterances from context to context; Dan Sperber and Deirdre Wilson (1995), and François Recanati (2004, 2010) are examples.

Kent Bach has staked out a distinctive position on the semantics-pragmatics issue and cannot readily be placed in either camp. I like to think the same of myself. But our positions are strikingly different. Nonetheless, I think that there is a lot of Good Bach. Those are the bits I agree with. I shall have something to say about them. But agreement is boring. So my focus will be on the bits I disagree with, Bad Bach. These concern his notion of “what is said” and what he counts as “semantic”. Despite these disagreements, I have learnt a great deal from Bach over the years, not only from his wonderfully rich publications but also from many exchanges. Doubtless he thinks I have not learnt nearly enough.

2. *Methodology*

2.1 *The Received Methodology*

How are we to get to the truth about linguistic phenomena in general and about the semantics-pragmatics issue in particular? The received answer is clear: we should look to our meta-linguistic intuitions. Neale gives the following sweeping endorsement of this practice:

Our intuitive judgments about what *A* meant, said, and implied, and judgments about whether what *A* said was true or false in specified situations constitute the primary data for a theory of interpretation, the data it is the theory’s business to explain. (Neale 2004: 79)

Robyn Carston thinks that the various criteria in the pragmatics literature for placing “pragmatic meanings” into “what is said”, “in the end...all rest...on speaker/hearer intuitions” (2004: 74). I have argued that this intuition-based methodology is very wrong (1996, 2012a). Bach seems to agree. Thus, consider his response to the claim of Jason Stanley and Zoltan Szabó that “accounting for our ordinary judgments about the truth-conditions of various sentences is the central aim of semantics” (2000: 240). Bach has none of this: “the central aim of semantics is to account for semantic facts” (2000: 267). And consider his response to Recanati’s “availability” criterion. According to this criterion, “what is said is the proposition determined by the truth-conditional intuitions of the participants in the talk-exchange themselves” (2010: 14). Bach comments: “But he needs to explain why the intuitions he takes to con-

cern what is said actually do concern that, as well as why we should be confident in their accuracy” (2001: 26). So, on the one hand, Bach rejects the puzzling view that the *very task* of semantics is to account for intuitive linguistic judgments and, on the other hand, he is appropriately skeptical of the *evidential role* of those judgments. I’d like to conclude that his position on intuitions is a piece of Good Bach. But there is a worry about this. If he does not rest his views on intuitions, what does he rest them on? So far as I can see, he never says. And his practice often seems to rest them ultimately on intuitions.

2.2 *My Methodology*

If we are not to proceed by simply consulting intuitions, how should we get to the truth on the semantics-pragmatics issue? I have offered an answer in “What Makes a Property ‘Semantic?’” (2013b). This answer forms the basis for the rest of my discussion of Bach. I summarize.

We need a *theoretical basis* for distinctions that play a role in the semantics-pragmatics issue; for example, for the Gricean distinction above. I argue that the required basis is to be found by noting first that languages are representational systems that scientists attribute to species such as bees, prairie dogs, and humans to explain their communicative behaviors.² We then have a powerful theoretical interest in distinguishing two sorts of properties of any particular utterance in a language: (a), the representational properties that it has simply in virtue of being a token-expression in that language, that it has simply as a result of the organism’s exploitation of that language; (b), any other properties that may constitute the organism’s “message”. I call the (a) properties part of “what is said”, and “semantic”,³ and the (b) ones—for example, implicatures and certain “enrichments”—part of “what is meant but not said”, and “pragmatic”. “Semantics” is the study of semantic properties, “pragmatics”, pragmatic ones. This theoretical basis then provides an argument for the view that what is said is constituted by properties arising from three sources: (i) from (largely)⁴ conventional linguistic rules in the speaker’s language, rules that determine what is *encoded* in the language; (ii) from disambiguations where more than one rule governs an expression in the language; (iii) from the reference fixings of indexicals (and tenses), deictic demonstratives and pronouns. This is a fairly traditional view. However, whereas that view is typically promoted on the basis of intuitions, I claim to have given it a theoretical basis.

² This view of human languages is rejected by Chomskians. They see these languages as internal states not systems of external symbols that represent the world. I argue that this is deeply misguided (2006a: chs 2 and 10; 2006b; 2008a,b).

³ Bach likes to emphasize the Austinian distinction between “locutionary” and “illocutionary” acts. So I should note that my what-is-said is a property an utterance has as a locutionary act.

⁴ I say “largely” because I accept the Chomskian view that some syntax is innate. The qualification should be taken as read in future.

From my perspective, “semantics” is concerned with the representational properties that symbols have in virtue of being uses of a language, the properties that constitute what is said. These properties contribute to conveying the message of an utterance. Other properties may also contribute to conveying this but these are the concern of “pragmatics” not semantics. So the key semantic issue is the nature of those linguistic representational properties. The symbols have those properties in virtue of being part of a representational system of conventional rules. So the key issue comes down to: What are the conventions that constitute the system?⁵

How do we answer this key question? We look for evidence from regularities in behavior. Is this expression regularly used to express a certain “speaker meaning”? If so, is this regularity best explained by supposing that there is a convention of so using the expression?

In sum, my methodology for tackling the semantics-pragmatics dispute starts from the view that a language is a representational system posited to explain communication.⁶ From this start I provide a theoretical motivation for a sharp distinction between two sorts of properties of utterances: semantic ones constituting what is said; and other, pragmatic, ones that contribute to the message conveyed. Finally, we take properties to be semantic if that is the best explanation of regularities in behavior.

Presumably, in light of what is to come, Bach does not agree. But we certainly agree about the next matter.

2.3 *Meaning vs Communication*

The semantic theory of the representational properties of symbols in a language is one thing, the theory of communication among users of that language, another.⁷ Yet the semantic theory should be center stage for the theory of communication. For, it is largely, even if not entirely, because symbols of the language have those semantic properties, and because the organisms have the capacity to exploit those properties in sending and receiving messages, that communication occurs. So the theory of communication must start by assigning “semantic values” to

⁵ I think that conventions should loom very large in our view of human language. In stark contrast, Chomsky thinks that the “regularities in usage” needed for linguistic conventions “are few and scattered” (1996: 47; see also 1980: 81–3). Furthermore, such conventions as there are do not have “any interesting bearing on the theory of meaning or knowledge of language” (1996: 48). I think these views are very mistaken (2006a: 178–89; 2006b: 581–2, 598–605; 2008a: 217–29).

⁶ Not necessarily *only* communication. Human languages, at least, have uses other than communication, as I emphasize in section 3.

⁷ To add to the terminological nightmare, the theory of communication is usually called “pragmatics”. So, as typically used, “pragmatics” is ambiguous, referring sometimes to the study of the non-semantic properties of utterances and other times to the study of the communicative processes; see Devitt 2013b, sec. 4, for discussion.

symbols in the language in question. And whereas the focus of the semantic theory is on the speaker because the speaker determines both what is said and the message, the focus of the theory of communication is on the hearer because the task of receiving a message is more difficult than the task of sending one.

We might say that semantics is concerned with *the metaphysics of meaning* whereas the theory of communication is concerned with *the epistemology of interpretation*. Although these metaphysical and epistemological studies are related they are clearly distinct. Perhaps the most serious failure of linguistic pragmatism is the almost ubiquitous confusion of these distinct studies.⁸

It is a central plank of pragmatism that conventions, disambiguation, and reference fixing *semantically underdetermine* what is said. What then makes up the shortfall? As Carston (2004: 67) points out, pragmatists believe that the shortfall in what is said is made up by “pragmatic inferences”. These are inferences—Gricean derivations of conversational implicatures are paradigms—that *hearers* may go through to *interpret* utterances. I have argued that this pragmatist view of the shortfall is badly mistaken (2013a: 89–90; 2013c: secs. 2–3). If there were a shortfall, it would be made up, just like the standard disambiguation and reference fixing, by something noninferential that the speaker has in mind or intends. Pragmatic inferences that a hearer may need to interpret an utterance have absolutely nothing to do with any shortfall in the constitution of what is said.

Bach is one of the few to take a stand against this confusion.⁹ In particular he has drawn attention to the equivocation over ‘determine’ (and cognates) that has facilitated the confusion:

It is one thing for something to be determined by context in the sense of being *ascertained* on the basis of contextual information and quite another for it to be determined by context in the sense of being *fixed* by contextual factors (*epistemic* vs. constitutive determination). (2000: 271)

He points out that although the context typically provides the information on which the hearer bases her identification of the speaker’s communicative intention, “it is that intention, not the context, that determines (constitutes) what the speaker is trying to communicate” (272; see also 1999: 72; 2001: 29–30). This is really Good Bach.

I turn now to some Bad Bach. I start with a criticism of his notion of “what is said”. Then, in section 4, I criticize his view of what counts as “semantic”.

⁸ Some examples: Bezuidenhout 2002; Carston 2004; Korta and Perry 2008; Levinson 2000; Recanati 2004, 2010, Sperber and Wilson 1995; Stainton 2005; Stanley and Szabó 2000. The confusion is presumably related somehow to a lack of attention to the ambiguity of ‘pragmatics’ mentioned in note 7.

⁹ Others are Stephen Neale (2004, forthcoming), Jerry Fodor and Ernie Lepore (2004). I first took a stand against this confusion in a discussion of proper names (1976).

3. *What Is Said*

3.1 *Bach's Notion*

My view (sec. 2.2) is that we have a powerful theoretical interest in distinguishing the properties that an utterance has simply as a result of the speaker's exploitation of her language from any other properties it may have. The former properties are, briefly, those arising from (i) convention, (ii) disambiguation, and (iii) reference fixing. I call these properties "semantic" and together they constitute "what is said" by the utterance. Their study is "semantics". Any other properties that go into what was meant but not said I call "pragmatic". Their study is "pragmatics". Where does Bach stand on this?

First we must attend to a verbal issue. Many philosophers favor a narrow use of 'semantic' according to which it refers primarily to the properties of expression-*types* fully determined by the conventional linguistic rules of a language, rules that determine what is encoded in the language. These properties of expressions are "known" by every competent speaker of the language. Bach is among those who favor this use of 'semantic':

the semantics of an expression gives the information that a competent speaker can glean from it independently of any context of utterance....If a token of an expression carries any information not encoded by the type of which it is a token, that information is not *linguistic* information, hence not semantic. So to the extent that such non-linguistic information is relevant to the truth of a sentence token and/or to the reference of a token of a singular term, truth and reference, as properties of tokens, are not semantic properties, at least not in the linguistic sense. (1987: 5)

Indeed, Bach rather deplores the wide use of 'semantic' "for features pertaining to truth and reference" (1994: 133 n. 9). Later he demonstrates his usage in a contrast with "pragmatic" properties: "we should attribute semantic properties to sentences and pragmatic properties to utterances" (2005: 18). This is accompanied by a helpful note:

I am supposing that utterances are acts of producing tokens of sentences. Uttered sentences have semantic properties; acts of uttering them do not (of course, speakers' communicative intentions have contents). As for tokens of uttered sentences, they have the semantic properties of the sentences of which they are tokens; they have no semantic properties of their own. Any seemingly semantic properties of tokens are really pragmatic properties of utterances. These depend entirely on the communicative intentions of speakers; semantic properties of sentences and their constituents do not. (n. 7)

Now, there should be no objection to Bach's use of 'semantic'. Our theoretical interest in what is said by the utterance of a sentence (in my sense, just explained) motivates an interest in any property that constitutes what is said. And properties that the sentence-token in the utterance inherits from its sentence-type, properties determined by linguistic rules, constitute a large part of what is said. So we have a strong motivation for identifying and naming those properties. I use the term

‘narrow-semantic’ for that purpose. However, from the theoretical perspective I have urged, we also need a term for *all* the properties that constitute what is said, properties of types (i) to (iii), not just for the ones determined by the rules of the language, not just properties of type (i). ‘Semantic’ seems ideal for this job too. Hence my usage. There should be no objection to that usage either.¹⁰

So what is Bach’s term for *my* what-is-said? He does not have one. His term, ‘what is said’ is for something else. This reflects a real difference between us, not just a verbal one. Bach does not share my theoretical perspective.

In the nicely named paper, “You Don’t Say?”, Bach claims to defend “a purely semantic notion of what is said” by the utterance of a sentence (2001: 16). We have just noted how Bach uses ‘semantic’ and so we expect his “purely semantic” notion to cover simply the property that the sentence-token in the utterance inherits from the encoded conventional meaning of its sentence-type: we expect his term ‘what is said’ to apply only to the property of an utterance that is fully constituted by the linguistic rules of the speaker’s language, fully constituted by my properties of type (i). Of course, such a restricted notion of what is said should make us feel the lack of a term for a richer property of an utterance than this narrowly semantic one, a property like *my* what-is-said. For, we have a theoretical need to distinguish some richer property of the utterance from its indubitably pragmatic properties (such as its implicature). But our expectation about Bach’s notion goes surprisingly unfulfilled. Here is Bach’s characterization of the *semantic information* of what is said:

Semantic information is information encoded in what is uttered—these are stable linguistic features of the sentence—together with any extralinguistic information that provides (semantic) values to context-sensitive expressions in what is uttered. (22; my emphasis)

Any other information conveyed is “pragmatic”. So, despite Bach’s official narrow use of ‘semantic’, his use of “semantic information”, and elsewhere of “semantic content” (27), cover some properties that are not encoded but rather *determined in context*. So, his “purely semantic” notion of what is said is a bit tainted. Just how tainted? Could Bach’s ‘what is said’ be like mine after all, covering not only properties of type (i) but of types (ii) and (iii) as well, thus being just the notion we need? Could this be more Good Bach?

A passage from an earlier paper encourages a positive answer about (iii). After pointing out that “context plays a role in semantics as well as pragmatics” he goes on to claim that “with indexicals and demonstratives (and tense also),...it is on the semantic side of the ledger that content varies with context” (1999: 71). But it seems that Bach changed his mind about demonstratives, for the later paper describes “the extralinguistic information” that gets into semantics as follows:

¹⁰ I defend this claim in Devitt 2013b, sec. 4.

This sort of contextual information is limited to a short list of parameters associated with indexicals and tense, such as the identity of the speaker and the hearer and the time of an utterance. (2001: 22)

This does not seem to include demonstratives like ‘that’ and personal pronouns like ‘she’. Before long he makes it clear that it does not. Unlike such “pure indexicals” as ‘I’ and ‘today’, the values of these expressions cannot be “read off of objective features of the context”. They

are not *semantic* values, precisely because they depend on speakers’ communicative intentions. Even though the values are intended for lexical items in the sentence..., it does not follow that the proposition resulting from supplying the values...is the *semantic* content of the utterance (i.e., of the sentence relative to the context). (2001: 32)

So, Bach’s semantic what-is-said covers properties arising from conventions *and* the reference fixing of pure indexicals (and tenses) *but not* that of demonstratives and pronouns; it covers my properties of type (i) but *only some* of those of type (iii).¹¹

What about properties of type (ii), arising from disambiguation? Bach does not have much to say about this but there is the following:

When a speaker utters a given sentence in a given context, the only intention that is relevant to what he is saying is his semantic intention, i.e., *his intention concerning the resolution of any ambiguities* and the fixing of any indexical references. (2001: 22; my emphasis)

In light of this, I take it that Bach’s what-is-said is arrived at after the speaker has resolved any ambiguities.¹²

We can capture Bach’s position so far by considering these two utterances:

- (1) I went to a bank.
- (2) He went to a bank.

Suppose that Bruce utters (1) having a financial institution in mind. Then, Bach holds, what Bruce says is the complete singular proposition that he himself went to such a place (but not the complete, but less precise, proposition that he went to either such a place or a river side). In contrast, suppose that Sheila utters (2) having Bruce and a financial institution in mind. Then, Bach holds, what she says is not a complete proposition at all. Yet, of course, a complete singular proposition is *conveyed*, the proposition that he, Bruce, went to such a place. What does Bach say about what is thus conveyed?

To answer, we need Bach’s novel term, ‘implicature’. This term is not to be confused with Grice’s familiar term, ‘implicature’ (but, Bach notes, it concerns “roughly what Sperber and Wilson (1986) call *expli-*

¹¹ Actually, Bach prefers to say only that *speakers use* demonstratives and pronouns to refer in a context and not that those expressions themselves refer in that context; similarly, proper names (which we will be discussing in a moment). But this is just a verbal point. I shall continue to talk of these expressions referring.

¹² But perhaps not: Bach does not place the resolution of scope ambiguities in ‘Five boys washed ten cars’ into what is said (1998: 716).

capture"; 2001: 41, n. 5). An implicature is a matter of "saying something but communicating something else instead, something closely related to what is said" (1994: 126). The implicature is "built out of what is said." (2001: 19). Bach gives many examples of implicatures but none of them is of a completion that assigns a referent to a demonstrative or pronoun. Nonetheless, Bach's view is that the complete singular proposition arising from such an assignment—for example, what Sheila conveys by (2)—is indeed an implicature. (I know because I asked him.)

One final part of Bach's position on what-is-said needs to be added: his position on proper names. But first a brief word about my own position on names.

Influenced by Saul Kripke (1980), I developed a causal theory of names (1974, 1981a). According to this theory, the reference of a name is determined by a certain sort of causal link between its bearer and the speaker. And the name's conventional meaning is its property of referring to its bearer in a particular causal way, its causal mode of reference.¹³ Yet a name typically has many bearers; for example, 'Bruce' is the name of all the philosophers at the University of Woolloomooloo, of David Lewis' late cat, of a movie tough guy, and so on. How does the theory accommodate that? The theory claims that since 'Bruce' has many distinct causal modes of reference it has many meanings: it is "ambiguous" (1976). So, an utterance's property of referring to a certain object by 'Bruce' gets into my what-is-said as a property of type (ii), arising from disambiguation.

Bach is very dismissive of my idea that proper names are ambiguous, describing it as "absurd" (1987: 137). His own view is that "a name 'N' is semantically equivalent to the description 'the bearer of 'N'''" (135). So 'N' has just one meaning despite its many bearers. The description 'the bearer of 'N'' does not, however, determine that 'N' refers in context to one particular bearer; that reference is determined causally in much the way I proposed.¹⁴ I shall not fuss about Bach's charge of absurdity nor about Bach's alternative view of names. The key point for our purposes is that, for Bach, a name's reference in context, just like a demonstrative's or pronoun's, does not go into what is said but only into an implicature; it is on the pragmatic side of the ledger not the semantic (2002). So, if Sheila utters

(3) Bruce went to the bank

instead of (2), what she says is not the complete singular proposition that a particular person, Bruce, went to a bank (financial institution). The proposition to that effect is only an implicature.

¹³ This has not been the popular idea of a name's meaning among those influenced by Kripke. Indeed, it has mostly been treated as too shocking to mention (as I have demonstrated; Devitt 2001, 2012b). The popular idea has been "direct reference", the view that the name's meaning is its bearer. But, ironically, my sadly unloved view has recently been urged by the father of direct reference himself, David Kaplan (2012), apparently unaware that the view has been around for forty years. For more on the history of this issue, see Devitt forthcoming a.

¹⁴ But see note 11.

3.2 Criticisms of Bach

What are we to say about Bach's notion of what-is-said? I start by noting that, with its exclusion of the references of demonstratives, pronouns, and names, it is certainly not the intuitive ordinary notion (nor Grice's close relative of that). But I'd be the last to rest a criticism on intuition and a folk notion. Bach's notion has a deep problem.

In commenting on Grice's use of 'say', Bach remarks that "how one uses the term, even in the context of a theory, should not be entirely arbitrary" (1994: 141). Indeed! Because it should not be arbitrary at all! It needs a sound theoretical motivation. And that is what Bach's notion of what-is-said lacks: some Bad Bach.

I have argued that we have a powerful theoretical interest in a what-is-said constituted by properties arising from (i) convention, (ii) disambiguation, *including* of names, and (iii) reference fixing, *including* of demonstratives and pronouns. We have this interest because what is thus said by an utterance is simply a result of the speaker's exploitation of her language. It is the property of the utterance that makes it an instance of the language. The fact that the utterance has that property supports the very attribution of the language to the speaker in order to explain her communicative behaviors. So there is a theoretical point in identifying and naming this what-is-said. And there is then a theoretical point in putting this what-is-said on one side of the ledger, that I call the "semantic" side, and any other properties of the utterance on the other side, that I call the "pragmatic" side.

Bach's what-is-said moves a whole lot of my what-is-said from the semantic side to the pragmatic side. What is the motivation for this? What is the theoretical point of his what-is-said? Bach does not have an adequate answer. This is not to say that Bach has nothing to say about the difference between what he does and does not put on the semantic side. The problem is that what he says does not show why that difference is theoretically significant to what-is-said.

In one place Bach has this to say:

Contextual information that combines with linguistic information to determine what is said (in the sense of fixing it) is restricted to a short list of parameters associated with indexicals and tense, such as the identity of the speaker and the hearer and the time and place of an utterance. This determination is independent of speakers' intentions and beliefs. (2000: 271–2)

In contrast, the parameters associated with demonstratives and the like are not on the list because in those cases determination is dependent on speakers' intentions and beliefs. But Bach's contrast is mistaken because even the reference of pure indexicals is dependent on speakers' intentions.

Speaking English is an intentional act that requires participating in the conventions of English.¹⁵ 'I' in (1) refers to Bruce only because, in in-

¹⁵ This is not to say that in speaking English one must *intend* to participate in its conventions. That would be too intellectualized a view of the activity; c.f. my

tionally uttering (1) to express a certain thought, Bruce participates in the English convention for ‘I’. That convention determines that the utterer refers to himself, and that utterer is Bruce. If Bruce were doing something else—for example, simply mimicking an English utterance that he does not understand—there would be no reference to Bruce (or banks). Similarly, ‘he’ in (2) refers to Bruce only because, in intentionally uttering (2) to express a certain thought, Sheila participates in the English convention for ‘he’. That convention determines that Sheila refers to the object she “intends to refer to” or “has in mind”, as we might ordinarily say, or to the object that her thought is causally linked to in a certain sort of way, as we should theoretically say (as Bach and I agree). And that object is Bruce. An intentional act is as necessary for Bruce’s ‘I’ as for Sheila’s ‘he’ to refer to Bruce. The difference in the *ways* of referring, determined by the conventions for ‘I’ and ‘he’, is of no significance when considering what to include in a theoretically interesting notion of what is said, just as the difference in the ways of referring determined by the conventions for ‘water’, ‘soup’, ‘dog’, ‘hammer’, and ‘bachelor’ is not.

It might well be objected that I am overlooking Bach’s idea that the reference determination of ‘he’ depends on a special sort of intention, a communicative intention; the speaker’s referential intention is “essentially audience-directed” (2006: 521). The dependence on this sort of intention makes all the difference. Thus, in a passage quoted earlier, Bach claims that the values of demonstratives and pronouns “are not *semantic* values, precisely because they depend on speakers’ communicative intentions” (2001: 32). In contrast, Bach thinks that the values of pure indexicals are semantic because they do *not* depend on speakers’ communicative intentions. So the latter values get into Bach’s what-is-said but the former do not: “what one says does not depend on whether or not one has any communicative intention in saying it” (2001: 18). But there is no good reason to think that any reference depends on communicative intentions, that the use of any singular term is “essentially audience-directed”. In uttering (2), Sheila might have no intention of communicating with anyone: she might be simply musing, making notes, trying out a line for a poem, or whatever. Yet she is still referring to Bruce because she is intentionally expressing the thought that Bruce went to the bank. The intentional act that is necessary for reference, whether of ‘he’ or ‘I’, is not an act of *communicating* a thought but one of *expressing* a thought. And what goes for reference goes for meaning in general. Doubtless the vast majority of meaningful utterances are attempts at communication but, in my view (2006a), and contrary to

discussion of intending to refer (2013b: note 3). And it would not handle slips of the tongue right. When Canon Spooner intentionally uttered the sentence “A student is hissing my mystery lectures”, he thereby participated in the conventions for ‘hissing’ and ‘mystery’ and hence *said that* the student was hissing his mystery lectures, but he did not intend to participate in those conventions, for what he *meant* was that the student was missing his history lectures.

Gricean “intention-based semantics”, they needn’t be, and aren’t all.

Suppose that I were wrong about this and that referring with ‘he’ required a communicative intention, then why would this not also be true of referring with ‘I’? Bach elsewhere makes a claim that implies it is indeed also true of ‘I’:

...the only respect in which an utterance has content over and above that of the uttered sentence is as an intentional act performed by a speaker. And in that respect, the content of an utterance is really the content of the speaker’s communicative intention in making the utterance.” (2005: 23–4)

Bruce’s utterance of (1) has content over and above that of the sentence ‘I went to the bank’. In particular, its content includes the referent of ‘I’. So, for Bach, that additional content coming from the use of ‘I’ should surely be the content of Bruce’s communicative intention. If so, then this additional content should be on a par with that coming from the use of ‘he’ in (2). So those contents should both be in what-is-said, or neither should.

In sum, the role of speakers’ intentions does not provide a theoretically sound motivation for excluding the referents of demonstratives, pronouns and, we should add, names, from what is said while including the referents of pure indexicals. The referents of the latter depend as much on speakers’ intentional participation in referential conventions as does the referents of the former. The intentions needed for reference are not communicative intentions but, even if they were, that would not support discrimination against demonstratives, pronouns, and names when including referents in what is said.

We have been using ‘I’ as our example of a pure indexical. The lack of a basis for discrimination against demonstratives, pronouns, and names is even more obvious when we consider two other paradigm pure indexicals, ‘now’ and ‘here’, for all these terms are rather similar in their reference fixing. Thus, it has often been noted that the reference of ‘now’ depends very much on what period the speaker has in mind: it might refer to this very instant, today, this year, this epoch, or...¹⁶ So its way of referring is much more like that of demonstratives, pronouns, and names than is the way of ‘I’. But the main point remains: differences in ways of referring dictated by linguistic conventions are of no significance to a theoretically interesting notion of what is said.

Finally, we should compare Bach’s handling of ambiguity with his handling of demonstratives, pronouns, and names. Intentionally speaking a language requires resolving its ambiguities (ignoring puns). So when Sheila utters (2) intending a financial institution by ‘bank’, she determines its reference in the context. Similarly, intentionally speaking a language requires determining the reference of its pronouns. So when Sheila utters (2) intending Bruce by ‘he’, she determines its reference in the context. Bach puts Sheila’s reference to a financial institu-

¹⁶ Similarly, one might add, the pure indexical ‘today’ might refer to this very day, this year, this epoch, or....

tion into what is said and thus on the semantic side of the ledger, but not her reference to Bruce. This is highly anomalous on the face of it.

I hear Bach objecting: “Intentionally resolving an ambiguity and intentionally referring with a pronoun are different.” And of course they are: everything is different from everything else. But the challenge for Bach is finding a difference that is relevant to a theoretically interesting notion of what is said. I’ll bet that he can’t meet this challenge.

Bach hankers after a purely semantic what-is-said, with ‘semantic’ understood as covering only properties fully constituted by the conventional linguistic rules of the speaker’s language, covering only what I call “narrow-semantic” properties. All other properties of an utterance would then be placed on the pragmatic side. But the pressure to move to a theoretically interesting what-is-said leads Bach to include a little of what is determined in context into what-is-said. I have argued that he needs to go further down this path: a theoretically interesting what-is-said is not just a little tainted in context but very tainted.

Bach has a well-motivated distinction between his semantic properties of sentences (my “narrow-semantic”) and the properties of utterances other than ones inherited from those semantic properties. But we also need a distinction among those other properties in constituting what is said. Bach’s exclusion of the reference fixing of demonstratives, pronouns, and names from what-is-said is not well-motivated.

In this section we have been concerned with what other sorts of properties should be added to Bach’s semantic ones to arrive at a theoretically interesting what-is-said. In the next we shall consider what should be included among Bach’s semantic ones in the first place.

4. *Bach’s Semantic (My Narrow-Semantic) Properties*

Bach’s methodology for deciding which properties are semantic (my narrow-semantic)¹⁷ has the conservative effect of keeping out new meanings: Bach’s Semantic Club is very exclusive. I shall argue that this methodology is flawed in three respects.

4.1 *Modified Occam’s Razor*¹⁸

Where an expression has a meaning that linguists and philosophers accept as conventional and yet the expression is used to convey a different meaning, there is widespread resistance in linguistics and philosophy to positing ambiguities: these theorists resist concluding that this usage reflects *another* conventional meaning. This resistance is best illustrated by the enthusiasm for Grice’s “Modified Occam’s Razor”: “Senses are not to be multiplied beyond necessity” (1989: 47). Now this Razor might be construed, on the model of the original Occam’s

¹⁷ This parenthetical addition to talk of the semantic should be taken as read in what follows.

¹⁸ This section draws on Devitt 2013c, sec. 4.

Razor (“Entities are not to be multiplied beyond necessity”), as wisely advising against positing a sense, a conventional meaning, *unless it is needed for the best explanation* of the message conveyed by the utterance. But, so far as I can see, the Razor is uniformly construed in a way that is significantly different from this: it is construed as advising against positing a sense *wherever there is a pragmatic derivation of the message* from an already accepted meaning, without any consideration of whether that derivation is part of *the best explanation* of the message.¹⁹ Georgia Green nicely sums up the thinking that underlies this construal of the Razor:

the possibility of accounting for meaning properties...of linguistic expressions in terms of conversational inferences rather than semantic entailments or grammatical ill-formedness was welcomed by many linguists as a means of avoiding redundant analyses on the one hand and analyses which postulate rampant ambiguity on the other. (1989: 106)²⁰

Bach construes the Razor in the common way and then embraces it. He claims that although appeals to ambiguity are tempting they are unwarranted “where one standard use of a sentence can be explained, with the help of conversational principles, by another standard use which uncontroversially corresponds to a literal meaning” (1987: 78; see also: 69, 81; 1999: 66).

Grice’s Modified Occam’s Razor, as commonly construed and as construed by Bach, cannot be right because *it would make all metaphors immortal*. The metaphorical meaning of a word is derived from its conventional meaning. Over time, a metaphorical meaning often becomes regularized and conventional: the metaphor “dies”. A large part of natural languages is made up of dead metaphors.²¹ Yet a derivation of what is now a new conventional meaning from the old conventional meaning will still be available. Indeed, it will be center stage in the diachronic linguistic explanation of the presence of this new meaning in the language. So, it would still be the case that the “standard use” of a sentence to convey the formerly metaphorical meaning “can be explained, with the help of conversational principles, by another standard use which uncontroversially corresponds to a literal meaning”. Yet, obviously, metaphors do die and words become ambiguous. *The mere existence of a pragmatic derivation of a meaning does not show that the meaning is not conventional*. As commonly construed, Grice’s Razor gives very bad advice.

The common construal loses sight of why we posit a language in the first place. A language is a representational system constituted by linguistic rules. We posit this system of linguistic rules because it provides the best explanation of communication, whether among bees

¹⁹ See, for example: Grice 1989: 47–8; Recanati 2004: 157; Carston 2002: 184–5, 277.

²⁰ For some other examples of this sort of thinking, see the following: Ruhl 1989; Cruse 1992: 599; Atlas 1989; Kripke 1977: 268; Szabo 2006: 32.

²¹ Even ‘dead metaphor’ is a dead metaphor! (Thanks to Jesse Rappaport.)

or humans (2013b). With each lexical rule comes a sense. If the lexical rule really is part of the best explanation of communication then positing that sense does accord with Grice's Razor, *properly construed* on the model of Occam's Razor. And the same explanatory consideration that leads us to posit one linguistic rule for a word, hence one sense, may lead us to posit another. There is no theoretical basis for the miserliness about senses recommended by the common construal. We have no reason to think that nature favors parsimony with senses over parsimony with pragmatic derivations.²² Indeed, suppose, as seems plausible, that human language is an adaptation, selected for aiding communication and thought. Then nature will favor senses over pragmatic derivations because, crudely, the more the senses, the more the benefits of language.

A community's regular use of an expression with a certain speaker meaning is good evidence that it has that meaning conventionally in the community. This evidence is not conclusive, of course: other explanations of the regularity are possible. Still, Modified Occam's Razor, as commonly construed, puts the explanatory onus in the wrong place. The Razor reflects a conservative climate of opinion according to which the theoretical *status quo* can be defended by simply showing how a new usage *could* be explain pragmatically as a derivation from an already accepted meaning; for example, explained as a Gricean implicature. But what that defense really requires is an argument to show that the new usage *should in fact* be explained in this way. We need an argument that the pragmatic explanation is *better* than the semantic one. So the following passage has the onus all wrong:

Though it may not be impossible for what starts life, so to speak, as a conversational implicature to become conventionalized, to suppose this is so in a given case would require special justification. (Grice 1989: 39)

Given the plenitude of dead metaphors, the phrase "may not be impossible" is way too grudging. More importantly, there is no basis for thus putting the onus on showing that conventionalization *has* taken place rather than on showing that it *has not*, so we still have an implicature. In light of the fate of metaphors, the onus should *generally* be equal. But where there is a *regularity* in usage, and that regularity seems to be accepted within the community, the onus is much more on those who want to deny a new meaning than on those who posit one. It may well be hard to tell whether to posit one but the Razor, as commonly construed, should have no role in telling.

²² On this, see Ben Phillips (2012), who is even more skeptical of the role of Grice's Razor than I am.

4.2 *The Example of Definite Descriptions*

The discussion of definite descriptions over the last half-century, including that by Bach, provides a dramatic example of both the misplaced onus and the damage of Modified Occam's Razor. Indeed, I first used an argument along the above lines, including the appeal to dead metaphors, in discussing definite descriptions (1997: 126–8; 2004: 283–5).²³ So, it is worth summarizing that discussion.

According to the received Russellian quantificational analysis, the definite description 'the *F*' refers to whatever is alone in being *F*. An "attributive" use of 'the *F*' simply exploits that quantificational meaning. However, as Keith Donnellan (1966) emphasized, 'the *F*' can also be used "referentially" to refer to a particular *F* that the speaker has in mind. Some responded to this by proposing that definite descriptions have a conventional referential *meaning* as well as the uncontroversial quantificational meaning; so they are ambiguous (e.g. Devitt 1981b, Wettstein 1981). But the popular conservative response was to apply the Razor: there was no need to posit a new meaning because referential uses can be explained pragmatically as, for example, conversational implicatures.²⁴ Yet there was no interest in showing that they were *better* explained pragmatically, no interest in examining the independent merits of the semantic explanation.

I pointed out (1997: 126; 2004: 283), as did Marga Reimer (1998), that not only *can* definite descriptions be used referentially they are *regularly* so used. This seems hard to deny and is certainly not denied by Bach (2004: 201). We can go further. As Bach notes (220), most uses of definite descriptions are of "incomplete" ones, ones like 'the table' that do not uniquely describe an object. And, as Bach also notes (2007b: 56), most uses of incomplete descriptions are referential. All in all, setting aside superlatives and anaphoric uses, I'd guess that the vast majority of uses of definite descriptions are referential.

What is the best explanation of this regular referential use? Reimer and I claimed that the regularity is strong evidence that there is a *convention* of using 'the *F*' to express a thought about a particular *F*, that this is a *standard* use.²⁵ If so, then a speaker uses a definite referentially by participating in that convention. This participation will fix reference in much the same way as participation in the convention for a demonstrative: the use will refer to the object that is linked in

²³ I accompanied it by what was, in effect, a *reductio ad absurdum* of the sort of thinking behind the common construal of the Modified Razor. The *reductio* is built around a character I call "a fundamentalist Gricean". The fundamentalist uses a Gricean argument about the beginnings of language to claim that "there are no conventional meanings at all: it is pragmatics all the way down" (2004: 284). Bach criticizes the *reductio* (2004: 178n) but, as I point out (2007a: 13n), he has missed its point.

²⁴ Neale (1990) presents the most thorough defense of this conservative position.

²⁵ Neale calls this "The Argument from Convention" (2004: 71).

the appropriate causal way to the thought being expressed. The hearer understands that use by the pragmatic processes of identifying the relevant convention and noting contextual clues to the reference that has been fixed.

This semantic explanation of the regularity is obviously a good one. So, even if conservatives can provide a pragmatic explanation of the regularity, there is, contrary to the Razor, an onus on them to show that their explanation is better. The onus has not been fulfilled. But the conservative position is really much worse: *there is no satisfactory pragmatic explanation of the regularity at all*; or so I argued, in “Referential Descriptions and Conversational Implicatures” (2007a).

Dead metaphors have shown that to explain regular referential uses as implicatures, or anything similarly pragmatic, it is not sufficient to simply point out that there *exists* a pragmatic derivation of the uses.²⁶ *An adequate explanation must place that derivation within the cognitive lives of speakers and hearers* (2007a: 12–15). There would have to be regular processes in speakers and hearers that differ from the typical convention-exploiting ones (whatever they may be). In speakers there would have to be thoughts about hearers and their expected Gricean derivation or other non-convention-exploiting inference (“implicitly” at least); in hearers there would have to *be* those derivations or inferences (“implicitly” at least). And these sorts of non-convention-exploiting processes *are* plausibly posited in paradigm cases of “particularized” conversational implicatures; for example, Grice’s reference letter (15–17).²⁷ But these particularized implicatures are not, of course, regularities. Nobody has even attempted to show that mental processes of these sorts are plausibly posited with referential uses of descriptions. And, I argue, they are not plausibly posited.

Any pragmatic explanation of referential uses must be based on the view that a person using a description referentially in uttering a sentence conveys a singular proposition *while saying a general quantificational proposition* (18). It is plausible that we can give a pragmatic explanation of this sort where the description is “complete” and so *identifies* the subject of the singular proposition. But, as noted, the descriptions are mostly “incomplete”. Then, I argue, “*there is frequently no general proposition that the speaker might plausibly be thought to have said*” (19).

So, I conclude that the regular referential uses of definite descriptions cannot be explained pragmatically. But, to repeat, even if they could be, the job of rejecting the ambiguity thesis would not be done. It

²⁶ Note that my claim is not that referential descriptions *are* dead metaphors. Indeed, it seems to me unlikely that they are. Dead metaphors play their important role in the dialectic as part of the demonstration that Modified Occam’s Razor (as usually construed) is false. Since the Razor is false, it is not properly wielded against *any* claim of a new conventional meaning.

²⁷ But I rather doubt that they are plausibly posited in some of the paradigm cases of alleged “generalized” conversational implicatures.

would still need to be shown that the pragmatic explanation was *better* than the semantic one.

Bach and I have published an extensive exchange on referential descriptions. For, Bach (2004) claims to give just what I conclude cannot be given: a pragmatic explanation of referential uses. I argue (2007a: 20–5) that his pragmatic explanation is crucially deficient. In particular, it fails to provide what

a pragmatic account of referential uses must provide: an explanation of *why* the speaker would think that saying the general proposition will convey the singular one and *why* the hearer would take the saying of a general one to convey the singular one. (24)

Bach has responded to this criticism (2007a) but, I claim, “simply persists in not answering [the] crucial questions” (2007b: 51). He concedes as much but offers an excuse;

True, I do not spell how the pragmatic story goes, but my excuse is that there is nothing special that needs to be said in regard to the particular case of identifying a speaker’s reference in using a definite description that isn’t part of the general explanation for how hearers figure out what speakers mean when they don’t make what they mean fully explicit. Philosophy can say only so much in this regard—the rest is a matter for cognitive and interpersonal psychology. (2007b: 56)

I direct interested readers to this exchange.

I cannot resist adding a word (surely not the last). Bach’s excuse strikes me as lame. In all the standard case of a speaker uttering a sentence with one meaning but conveying a different or additional meaning, philosophy can, without waiting for psychology, tell a story of why the speaker would think that the hearer could figure out what the speaker means and of how the hearer does figure that out. Such stories have been provided, for example, for paradigm conversational implicatures. Bach owes us such a story for referential uses.

In this exchange, I claim that Bach has no pragmatic explanation of referential uses. Perhaps this goes too far. But the important point for my criticism of Bach’s methodology is that there is no sign that Bach acknowledges the need to show that such explanation as he does offer is *better* than a semantic one. This is a dramatic illustration of his commitment to the damaging Razor.

4.3 *The Tyranny of Syntax*

Modified Occam’s Razor is not the only feature of Bach’s methodology that leads him to exclude a lot from semantics. His view of the relation between syntax and semantics does so too.

It is uncontroversial that quantifier expressions are regularly used to convey a message about a domain that is implicitly restricted. Thus, when François says

- (4) Everyone went to Paris

what he means, and is taken to mean, is that everyone *in a certain group* went to Paris. I would argue (forthcoming b) that the best explanation of this regularity is that there is a semantic *convention* of using quantifiers to convey a thought about objects in a particular domain that the speaker has in mind; there is a “slot” to be filled by what is in mind. The domain can be made fairly explicit, as in

(5) Everyone at the conference went to Paris

or it can be left largely implicit as in (4). So, on this view, it is a semantic fact (in Bach’s sense) that (4) depends for its truth on whatever group François has in mind (though, of course, it is not a semantic fact which group that is). This is not Bach’s view. For him, the restriction on the domain is not a semantic matter: the literal truth of (4) depends on whether every person in the world went to Paris. Why is this? Bach thinks that “it is gratuitous to suppose that there are hidden domain markers that restrict the ‘universe of discourse’” (1998: 715). But, it is *not* gratuitous if there is a convention of restricting the universe and if positing hidden markers is a way of capturing the convention. What is going on?

Two other discussions point to an answer. Consider

(6) I’m not ready.

Bach claims, plausibly, that (6) “does not express a complete proposition” because it has “a missing argument”. It “cannot be used to mean that one is not ready simpliciter”. I would argue (forthcoming b) that the *semantic convention* for ‘ready’ demands completion by some argument that the speaker has in mind; again, a slot to be filled. So the demand for completion goes into semantics (though not, of course, the value of the argument that meets the demand). But, once again, not for Bach. Why not? Because there is nothing “in the sentence that corresponds to the implicit argument” (1998: 716)

Bach calls sentences like (6) which do not express complete propositions, “semantically underdeterminate”. My slot-filling response to (6) shows that I favor the idea that these sentences are incomplete in rather the same way as any sentence with an indexical element is incomplete. Bach concludes a discussion of many examples of his semantically underdeterminate sentences by rejecting this approach. He thinks there is a relevant difference between indexical reference and “filling in conceptual gaps” in these sentences:

indexical reference fixes the interpretation of an element that occurs in the utterance, be it a pronoun, a demonstrative phrase, a temporal or locational adverb. An indexical is like a free variable needing to be assigned a value. On the other hand, the conceptual gaps in utterances of semantically underdeterminate sentences *do not correspond to anything in the sentences themselves, not even empty syntactic categories. Not being sentence constituents, they enter in not at the linguistic level but at the conceptual level. An indexical is there in the sentence.* (1994: 133; my emphasis; see also: 2001: 15; 2005: 24)

So we can see that Bach excludes from semantics anything that does not, according to the syntactic theory he favors, “correspond” to something that is “there in the sentence”. This reflects a commitment to what Carston calls “the Isomorphic Principle” according to which the structure of a sentence is an image of the structure of the thought it expresses. Carston does not embrace the principle herself but says, I think rightly, that it “has been fairly widely held by philosophers” (2002: 22).²⁸

This principle puts the cart before the horse. *First*, we need to establish whether or not an expression has a certain conventional meaning. Suppose that the expression does; for example, suppose it has a meaning that requires a slot to be filled when it is properly used. *Then* we need to consider what syntactic structure, if any, has to be ascribed to the expression to explain its meaning; for example, to explain the fact that it has a slot to be filled. I am no syntactician and so would venture no proposal on this matter. But I do insist that no prior assumptions about the bearing of syntax on meaning, like Bach’s correspondence assumption and the Isomorphic Principle, *could* show that such a conventional meaning is *impossible!* Indeed, we can do the Kripke trick and *specify* a language, English* in which the convention holds; for example, the slot-filling convention suggested above for quantifiers or ‘ready’. No acceptable syntactic theory could show that English* is impossible. Hence it could not show that English is not English*.

Some other examples may help to make the point. There was once a widely used language of naval flags. A yellow flag on a ship’s masthead conveyed the proposition that people on the ship had yellow fever. This was a semantic convention. And it is obviously not appropriate to deny that it was by arguing that parts of this meaning do not “correspond” to something in the syntax of the flag. Indeed, does the flag even have a syntax that contributes to explaining its meaning? Perhaps so. We note that the meaning “demands completion” by reference to a ship: it has a slot to be filled. This slot is filled by raising the flag to a ship’s masthead, thus asserting a proposition about that particular ship. So, perhaps this fact should be accommodated in a syntactic theory of naval flags that posits a “hidden referential marker” in the flag. Whatever. The key point is that flag has its meaning and if a syntactic theory is relevant to this meaning at all it has to accommodate that fact not reject it.

Consider also road signs and phone books. A certain street sign is a conventional way of conveying the message that the speed limit in a location is 30 mph. A certain entry in a phone book is a conventional

²⁸ She cites Frege 1977, Fodor and Lepore 1991, and Fodor 2001, as examples. See also the recent debate between Rob Stainton (2005) and Jason Stanley (2007) about sub-sententials. Fodor 2001 is, in fact, an effective *criticism* of the Isomorphic Principle: “If you read a sentence as though it were compositional, then the thought that it *ought* to be conventionally used to express often turns out not to be the one that it *is* conventionally used to express” (13).

way of conveying the message that the telephone number of X, living at Y, is Z. What does this show about the syntaxes of the sign and entry? Who knows? But no syntactic theory could nullify the conventional meanings of street signs and phone books.

These examples, along with many idioms ('spill the beans', 'kick the bucket', etc.), suggest that although the explanation of the complex propositional meaning of a symbol often involves ascribing a matching complex syntax, *it often does not*. Conventional meaning is not under the tyranny of syntax in the way Bach claims. Rather, it is always an open question to what extent, if any, a meaning is to be explained in terms of a syntactic structure, whether a "surface" or "deep" structure. One might well insist, of course, that the *most theoretically interesting* explanations of meaning posit rich syntactic structures (hence the excitement of generative grammars). But that is a different matter. It does not undermine the point that there is no simple inference from a symbol having a complex meaning to its having a matching complex syntactic structure.

4.4 *Standardization*

The third feature of Bach's conservative methodology is the application of his notion of *standardization*.

In arguing for what is semantic, I put a great deal of evidential weight on regularities in usage. Bach is strikingly unimpressed with this sort of evidence of an expression's conventional or literal meaning:

What a sentence is most typically used to communicate is one thing; what *it* means may well be something else...the most natural use of many sentences is not their literal use...not ...what is strictly and literally said. (2001: 30)

a great many sentences are very hard to use literally, even if one uses the individual words and phrases in them literally...*sentence nonliterality*. (2005: 28)

it is naive to suppose that the semantic content of a declarative sentence (relative to a context) can be defined as what a speaker would normally assert in uttering the sentence. (29)

Pragmatic regularities include regularized uses of specific expressions and constructions that go beyond conventional meaning...These regularities are pragmatic because it is the speaker's act of uttering a given sentence, not the sentence itself, that carries the additional element of information. (31)

One shouldn't let features of stereotypical utterance contexts get incorporated into the semantics of a sentence. (32)

For Bach, what a sentence is typically, naturally, normally, stereotypically used to communicate is not a reliable guide to what it means nor to what is being literally said by it. Indeed, he thinks it naïve of people like me to suppose otherwise. Why is this? A clue is to be found in the passage we quoted earlier when discussing Modified Occam's Razor: Bach holds that a use of an expression might be "standard" and yet that not be its "literal meaning"; rather that use might be explained pragmatically (1987: 78). The literal meaning of an expression is, of course,

what it means according to the conventions of the language. Central to Bach's position is a distinction between *conventional* meanings and *standardized* uses.

What Bach needs to supply, then, is an account of the nature of standardization that distinguishes it from conventionalization and yet still leaves standardization able to provide a rival explanation to conventionalization of the regular use of an expression with a certain meaning. Furthermore, Bach's distinction needs to be theoretically motivated. I don't find what we need in Bach's discussion, for reasons I shall indicate. But perhaps Bach could remedy this and so I shall not rest my criticism on this. Rather, I shall rest it on an epistemic point. We need some evidence, some reason to think, that whereas the paradigm cases of conventions have whatever property Bach thinks distinguishes conventionalization from standardization, his favorite examples of standardization lack that property. I don't think that Bach has provided this evidence. I doubt that those examples are in fact relevantly different from the paradigm cases.²⁹

Two ideas feature in Bach's discussion: (A) "mutual belief"; (B) the "streamlining" of pragmatic inference.

(A) "Mutual Belief" Since the works of David Lewis (1969) and Stephen Schiffer (1972), at least, it has been common to suppose that for the members of a community to share a *convention* of using an expression with a certain meaning there must be some sort of mutual dependence among the members' dispositions to so use the expression. Lewis captures the dependence with talk of "common knowledge", Schiffer, with talk of "mutual knowledge*". Bach captures it with talk of "mutual belief". All such propositional-attitude talk strikes me as giving too intellectualized a picture of the mutual dependence.³⁰ I prefer to say simply that any speaker has her disposition *because other speakers have theirs*. (2006a: 179–80). (This leaves a lot to be explained, of course.) But I'll set that preference aside and go along with Bach's talk of "mutual belief".

There is a puzzling twist to Bach's account of convention that seems to make his notion stronger than those of Lewis, Schiffer, and me. And this extra strength seems to be crucial to Bach's distinction between conventionalization and standardization:

Conventionalization entails that an utterance of a certain form of words would not have the force it has but for the existence of a general mutual

²⁹ At one point, Bach offers a simple test for standardization, largely in terms of the possibility of a certain sort of pragmatic explanation (1987: 81). But my argument in section 4.1 shows that the test is inadequate because many dead metaphors would pass it.

³⁰ I am even more resistant to the following: "the meaning of [a language's] words is conventional. In our terms, what words mean is what we mutually believe them to mean" (Bach and Harnish 1979: 133). In my view, competent speakers of a language need have no beliefs about the language (2006a, 2012a).

belief that it counts as such. Standardization entails no such thing. (1995: 683)³¹

Although standardization lacks the entailment Bach specifies, he thinks standardization is nonetheless like conventionalization in involving “mutual belief” (Bach and Harnish 1979: 195). What are we to make of the specified entailment? I don’t know. Crucial here is Bach’s talk of “force” in describing the entailment. It is obviously no explanation of the difference between conventionalization and standardization to say that with the former but not the latter, the form of words would not have its *conventional* force but for the “mutual belief” that it does. So, presumably, the force in question is not conventional but rather the force of what the *speaker means*. But then the entailment will surely often not hold in clear cases of conventionalization. Consider Reimer’s example of a dead metaphor, the verb ‘to incense’ (1998: 97–8). That verb now *conventionally* means *make very angry* and so typically the *speaker means* that by the verb. Yet it clearly is not the case that ‘incense’ would not have that speaker meaning but for the “mutual belief” that it has. For, that meaning was once metaphorical, derived from the earlier conventional meaning, *make fragrant with incense*. At that earlier time, when speakers used ‘incense’ metaphorically it had the force *make very angry* but there was no “mutual belief” that it did. So ‘incense’ does not depend on the “mutual belief” for its force and the specified entailment fails for ‘incense’. Yet ‘incense’ now has that force conventionally. What goes for ‘incense’ goes for countless other dead metaphors.

I hope that Bach will try to clear up this puzzle, showing how conventionalization entails some stronger dependence on “mutual belief” than does standardization. Suppose that he succeeds. He would still face two tricky questions, the first, constitutive, the second, epistemic. (I) Why do we need this stronger dependence, whatever it may be, for an explanation of linguistic meaning? Why isn’t my (apparently) weaker notion of convention all we need for this task? (II) What is the evidence that this stronger dependence, whatever it may be, *is lacking* in Bach’s favorite examples of standardization (1987: 78–83; 1998: 715–6)? More generally, what sign is there of *any difference* between the role of “mutual belief” in his cases of standardization and its role in paradigm cases of conventionalization? Consider referential uses of definite descriptions, for example. We have the “mutual belief” that such a use refers to a particular object the speaker has in mind. Similarly, we have the “mutual belief” that the use of a demonstrative refers to a particular object the speaker has in mind. The latter use is uncontroversially conventional and yet the former, for Bach, is mere standardization. So, the use of demonstratives must have some dependence on “mutual belief”

³¹ Bach has lengthier accounts of conventional illocutionary acts (Bach and Harnish 1979: 109, 189–90) and standardized ones (195) but these accounts are no help with the problem I raise.

that the referential use of descriptions lacks. What evidence is there of this lack? And what evidence is there that, in the just-discussed examples of quantifier domain restriction and ‘ready’, there is any lack of the dependence on “mutual belief” that Bach takes to be constitutive of a convention?

(B) “Streamlining” Bach gives a more positive characterization of standardization in the following passage:

What is standardization? A form of words is standardized for a certain use if this use, though regularized, goes beyond literal meaning and yet can be explained without special conventions. In each case, there is a certain core of linguistic meaning attributable on compositional grounds but a common use that cannot be explained in terms of linguistic meaning alone. The familiarity of the form of words, together with a familiar inference route from their literal meaning to what the speaker could plausibly be taken to mean in using them, streamlines the process of identifying what the speaker is conveying. The inference is compressed by precedent. But were there no such precedent, in which case a more elaborate inference would be required, there would still be enough contextual information available to the hearer for figuring out what is being conveyed. That is why special conventions are not needed to explain these cases. So standardization is different...from...conventionalization, such as dead metaphor and idiomatization. (1998: 712)

With our interest in distinguishing standardization from conventionalization, it helps to tell this story as follows. A sentence contains an expression, E, with literal meaning, M1; for example, it contains a definite description with a quantificational meaning. When the sentence is used in an utterance, there is a pragmatic inference from what the sentence literally means, including E meaning M1, to what the speaker could plausibly mean by the sentence in this utterance, taking account of the circumstances. That inference takes the speaker to mean M2 by E; for example, it takes the speaker to mean a definite description referentially, referring to the particular object the speaker has in mind in using it. Initially, speakers expect hearers to make this inference and hearers do so. Then comes standardization: because of precedent, that inference is compressed and streamlined in hearers—elsewhere Bach talks of its being “short-circuited” (1995: 682). E is now standardly used to mean M2 even though it still literally means M1.

What does this streamlining, compression, short-circuiting of inferences amount to? With many of Bach’s favorite cases of standardization, at least, it is plausible to think that speakers no longer expect hearers to make a pragmatic inference from M1 to M2 and hearers no longer do so. Rather, hearers of E go directly to M2. This suggests that the streamlining amounts to *disappearance*. But then, in light of our earlier discussion, there is an obvious objection to this as an account of standardization: the account fits dead metaphors perfectly: the process of standardization is the same as the death throes of a metaphor. The streamlining, compression, short-circuiting of inferences is precisely what happens as a metaphor is conventionalized.

So, if standardization is to be distinguished from conventionalization, we must take a streamlined inference to be still present somehow, albeit shortened: the hearer goes through some but not all the steps of the inference. So the inference would involve what Bach elsewhere calls “default reasoning” (1984). Such reasoning involves “implicit” assumptions: “To say that a person implicitly assumes a certain proposition in his reasoning means that the reasoning would not occur unless he believed that proposition” (42). Bach thinks that a lot of our reasoning is default, in this sense. I agree. Taking standardization to involve default reasoning from M1 to M2 would distinguish it from conventionalization, which involves no such reasoning.³²

We should note that, on Bach’s view, standardization

does not eliminate any information that is available to [the required pragmatic inferences] but merely eliminates the need to access certain of that information. Even though the inference is “compressed by precedent”, the success of the performative utterance would be “vitiating if any of the steps of the [original, uncompressed] inference were blocked” (1975: 235). (1995: 683)

Bach thinks that the information needed for the pragmatic default inference from a conventional meaning, M1, to a speaker meaning, M2, is still available to the hearer after standardization and so the hearer *could* make the full inference even though she *doesn’t*. But it is important to note that the mere availability of the information could not distinguish standardization from conventionalization. For, as I have pointed out (2004: 284–5), information about the origins of conventional meanings could be made available to us by an education in diachronic linguistics but this would obviously not eliminate conventional meanings. What must distinguish the standardized use of E to mean M2 from a conventional use of it to mean M2 is that in the former case but not the latter *the speaker expects the hearer to make an inference and the hearer actually does make it*, even if in a streamlined form.

Once again, Bach’s account faces two tricky questions, the first constitutive, the second epistemic. (I) We noted in (A) that Bach’s standardization involves “mutual belief”. How does that mesh with the view that standardization involves streamlined inference? Why would people who “mutually believe” that speakers mean M2 by E go through any inference at all from M1 to M2? And, I emphasize again, if they don’t go through any inference, even a streamlined one, standardization has not been distinguished from conventionalization. (II) There seems to be no reason to suppose that hearers *do* go through such default reasoning in Bach’s favorite cases of standardization. The problem is that there is no sign that hearers start their interpretation by assigning M1 to E; for example, no sign that hearers start understanding a referentially used

³² Neale gives some detailed examples of Gricean pragmatic inferences including ones from the quantificational meaning of a definite description to a referential meaning (78–90). These give a good idea of the full length of such an inference.

description by interpreting it quantificationally; no sign that they start understanding “Everyone went to Paris” as concerning everyone in the world; no sign that they start understand ‘ready’ as simply meaning readiness. And if there is no such start, the inference has not been simply shortened it has disappeared. We have conventionalization.

It is important to distinguish Bach’s alleged inference from a pragmatic process that we already believe in: disambiguation. When a person hears an ambiguous expression, she goes through a process of disambiguating it. We know that this mostly happens “automatically” in a non-central language system, without the hearer being aware of it.³³ Sometimes, however, it happens centrally and the hearer is conscious of it. Now, if Bach is to sustain his thesis that his favorite cases are indeed cases of standardization, he needs evidence that in those cases *hearers go through a different process from disambiguation*: they must go through the (possibly streamlined) process of pragmatic inference required by his standardization. I think that we can be confident that, in his favorite cases, hearers do not *consciously* go through such a different process. So Bach needs hearers to go through this different process subconsciously in the language system. I doubt that we have any reason to think that they do.

Standardization is offered as a rival to conventionalization as an explanation of regular uses. And the problem is that this standardization, explained in terms of streamlined inferences, has heavy psychological commitments that seem unlikely to be realized. These streamlined inferences must have an appropriate place in the cognitive lives of speakers and hearers. Speakers must have thoughts about hearers and their expected pragmatic inferences, and hearers must make those inferences even if only with “default reasoning”. But we have no reason to think that these psychological demands are met in the favorite examples. Those examples are likely to be better explained as conventionalizations.

I am not claiming that there are no examples of streamlined inferences. I am just claiming that the regular uses of expressions that are Bach’s favorite examples of standardized uses are unlikely to be so. Here is my picture. In the beginning, there is a novel use of an expression, probably one that already has a conventional meaning. The speaker expects the hearer to be able to infer her meaning. Suppose the hearer does. So he really goes through all the steps of a pragmatic inference. Then the novel use of the expression gains popularity. As it does, the pragmatic inferences begin to shorten: we have default reasoning. We do not yet have “mutual belief” and the use is still barely regular. Then comes “mutual belief” and the pragmatic inference disappears. The use has become really regular, perhaps ubiquitous like referential

³³ Disambiguation starts immediately on encountering the very first bit of the sentence and proceeds at extraordinary speed; for some discussions, see Gernsbacher and Kaschak 2003, Pickering 2003, Tanenhaus 2003.

descriptions, and we have a convention. Bach's favorite examples of standardization seem to be of that sort. So the default reasoning of Bach's standardization is real enough, but it has little or no role in explaining the regularities in usage that concern us.

In sum, in (A) I claimed that Bach has not shown how his conventionalization has a stronger dependence on "mutual belief" than his standardization. And I wondered why our theoretical concerns required a stronger dependence anyway. Finally, I sought evidence that there is any difference between the role of "mutual belief" in Bach's favorite cases of standardization and its role in paradigm cases of conventionalization. In (B) I emphasized that Bach's streamlined inferences must be present somehow in cases of standardization if standardization is to be distinguished from conventionalization. I wondered how the view that they are present meshes with the view that standardization involves "mutual belief". Finally, I doubted that we have any reason to believe that hearers do perform Bach's streamlined inferences in his favorite cases of standardization. All in all, Bach has not satisfactorily distinguished standardization from conventionalization. More importantly, he has not shown that his favorite cases are not conventionalizations. And that is what I suspect they mostly are.

I have argued that Bach's methodology for deciding what is semantic is flawed in three respects: first, in its commitment to the mistaken Modified Occam's Razor; second, in its placing inappropriate syntactic constraints on conventional meanings; and, third, in explaining many regularities in usage as standardizations rather than conventionalizations. This flawed methodology has the conservative effect of ruling out new meanings. The exclusiveness of Bach's Semantic Club is not principled.

5. *Conclusion*

I have identified Good and Bad Bach. Some Good Bach takes a critical view of the received view of the role of intuitive judgments in getting to the truth about language. (But I do worry that he himself relies on intuitions in the end.) Another piece of Good Bach takes a firm stand against the near ubiquitous confusion in the literature of what *constitutes* the various meanings of an utterance with *how hearers tell* what the utterance means.

I then turned to Bad Bach. I have two major criticisms of Bach's position.

My criticisms arise from an approach to the semantics-pragmatics dispute that I have argued for elsewhere (2013b). This approach starts from the view that a language is a representational system posited to explain communication. From this start I provide a theoretical motivation for a sharp distinction between two sorts of properties of utterances. The first sort are properties that an utterance has simply as a result of the speaker's exploitation of her language; these are "seman-

tic” properties constituting “what-is-said”. The second sort are other properties of an utterance that contribute to the message conveyed; these are “pragmatic” properties constituting “what is meant but not said”. I argue that the properties in what-is-said are those arising from (i) convention, (ii) disambiguation, and (iii) reference fixing. Finally, we take properties to be semantic if that is the best explanation of regularities in behavior.

Bach’s notion of “what-is-said” is different. It includes properties of types (i) and (ii) but only some of types (iii): it includes properties arising from the reference fixing of pure indexicals (and tenses) *but not* of demonstratives, pronouns and names. I have argued that there is no justification for these exclusions. Bach’s notion lacks theoretical motivation. This is my first major criticism of Bach’s position.

My second major criticism concerns Bach’s methodology for deciding what counts as properties of type (i), properties I call “narrow-semantic” and he calls “semantic”. His methodology has the conservative effect of keeping out new conventional meanings. I argue that this methodology is flawed in three respects.

First, Bach is committed to Modified Occam’s Razor, taken as advising against positing a new conventional meaning for an expression wherever there is a pragmatic derivation of the meaning from an already accepted conventional meaning. I have pointed out that the phenomenon of dead metaphors shows that the Razor cannot be right: metaphors frequently become conventionalized and yet there is still a pragmatic derivation of their formerly metaphorical meanings. The Razor puts the explanatory onus in the wrong place. Where there is a *regularity* in usage, and that regularity seems to be accepted within the community, the onus is much more on those who want to deny a new meaning than on those who posit one. A dramatic example of both the misplaced onus and the damage of Modified Occam’s Razor is provided by the discussion of definite descriptions over the last half-century, including that by Bach. The best explanation of the regular use of descriptions referentially is that there is a convention of so using them. The contrary view urged by Bach is mistaken.

Second, Bach is led away from positing conventional meanings by a syntactic constraint. He excludes from semantics anything that does not, according to the syntactic theory he favors, “correspond” to something in the syntax. But this puts the cart before the horse: syntactic theory must accommodate the semantic facts, not reject them.

The third feature of Bach’s conservative methodology is that he explains many regularities in usage—for example, referential uses of descriptions—not as conventionalizations but as *standardizations*. Standardizations are thought to differ from conventionalizations in their relation to “mutual beliefs” and in their involving streamlined pragmatic inferences. I have argued that Bach’s account of this distinction is not satisfactory. More importantly, he has not shown that his favorite

cases of standardization are not conventionalizations. And that is what I suspect they mostly are.³⁴

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