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Overlooking Conventions

The Trouble With Linguistic Pragmatism

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Preface

I suppose that everyone who starts theorizing about language and its use is struck early by the need for some sort of distinction between what is "semantic" and what is "pragmatic"; between the meanings that expression have in a shared language and meanings that are bestowed on them in context. The folk are endorsing such a distinction when they comment that a person "*said* that" such and such but "*meant* that*" so and so. The temptation for philosophers who want to "get on with semantics" is to push this distinction under the rug. I did this in my first book, *Designation* (1981a).

But the distinction was already pressing in on me in my discussion of "referential" uses of definite descriptions in "Donnellan's Distinction" (1981b). For, the standard response to Donnellan was to treat these referential uses as "pragmatic", whereas I argued in that paper, and later works (1997b,c 2004, 2007a,b), that the uses should be treated as "semantic": the meaning conveyed is not just a "speaker meaning" but rather a literal linguistic meaning arising from participating in a convention. I claimed that there was no principled theoretical basis for treating referential uses pragmatically. Another issue soon put further pressure on me to attend to the distinction. What is the meaning of a proper name? A surprising answer took hold in the 1980s. Influenced by the Kripkean revolution in the theory of *reference*, many adopted a "direct reference" view of a proper name's *meaning*: the name's meaning is simply its referent. How could this be, given the difference, obvious since Frege, between "Hesperus = Hesperus" and "Hesperus = Phosphorus"? The favorite direct-reference answer exported this problem to pragmatics: the difference between the two identity statements was not in the linguistic meanings of "Hesperus" and "Phosphorus" but rather in the information they pragmatically convey. I rejected this in "Against Direct Reference" (1989) and later works (1996, 2001, 2012d, 2015c, 2020b), arguing that the difference was semantic. Again, I claimed that there was no theoretical basis for a pragmatic treatment. These claims about a theoretical base forced me to develop, through these various works, a view of the semantics-pragmatics distinction.

Definite descriptions and direct reference are just two examples of how large the semantics-pragmatics distinction has loomed in recent philosophy of language. The

main cause of this has been an exciting movement roughly identified as "linguistic pragmatism" and/or "linguistic contextualism". Its seminal work is Dan Sperber and Deirdre Wilson's *Relevance* (1995). The movement challenges traditional "truth-conditional semantics" by arguing for pragmatic explanations of a large range of linguistic phenomena; there is "semantic underdetermination"; many think that we need to move to "truth-conditional pragmatics". Major contributors to the debate include Kent Bach, Robyn Carston, François Récanati, John Searle, and Stephen Neale. This book aims, first, to look critically at the methodologies at work in the debate, and second, to tackle substantive issues about the semantic properties of a range of linguistic expressions and constructions.

My view of the semantics-pragmatics distinction starts with the idea that languages are representational systems that scientists attribute to species to explain their communicative behaviors. We then have a powerful theoretical interest in distinguishing, (a), the representational properties of an utterance that arise simply from the speaker's exploitation of her language from, (b), any other properties that may constitute the speaker's "message". I call the former properties "semantic", the latter, "pragmatic". This motivates a fairly traditional semantics-pragmatics distinction. The key thing about semantic properties, emphasized throughout the book, is that they are (largely) conventional: conventions *create* linguistic meanings. This book aims to show that that there are many more such conventions than linguistic pragmatists and contextualists have acknowledged. Many of their striking examples are like the referential uses of descriptions in being semantic not pragmatic. That is the substantive thesis of the book.

But how do we tell what properties are semantic? For years I have resisted the ubiquitous unscientific practice in linguistics and philosophy of appealing to intuitions about syntax, meaning, and reference. Instead of consulting these intuitions, we should look to language itself for evidence. In the present case, we look for a regularity in using an expression with a certain speaker meaning as evidence of a linguistic convention and hence of a linguistic meaning. That simple idea is my methodology in a nutshell.

It has taken me a long time to write this book partly because I have often been distracted by other fascinating topics, particularly biological essentialism and experimental semantics. The book started, in effect, with a paper delivered at an international conference, "Meaning", at the University of Erfurt in Germany in September 2009. That paper was programmatic, the first presentation of the positive ideas that guide this book. The paper was subsequently delivered in many places, changing titles a couple of times as it progressed. It was finally published as "What Makes a Property 'Semantic'?" (2013c). It formed the basis for Chap. 3 of this book. That paper began a series of publications that developed the main themes of the book. Many of these papers were first delivered in Croatia at the Dubrovnik conferences on the philosophy of linguistics and language that had been held in September every year since 2005 until Covid-19 struck. These conferences, organized by Dunja Jutronić, were as good as they get: focused, ample time for discussion, convivial company, marvelous swimming and weather; and all this in one of the most beautiful settings in the world. The second in the series of publications was

one of those, "Three Methodological Flaws of Linguistic Pragmatism" (2013d), which formed the basis for Chaps. 7 and 8. It was delivered (under another title) at the 2010 Dubrovnik conference. "Good and Bad Bach" (2013f) was delivered at the 2011 conference, with Kent Bach in attendance. It formed, along with "Unresponsive Bach" (2013g), the basis for many discussions of Kent's work in the book. "Is There a Place for Truth-Conditional Pragmatics?" (2013b) was delivered at the 2012 conference. Modified versions of some of the ideas in that paper appear at various places in the book. The unpublished "On Handling Linguistic Pragmatism's Examples in the Spirit of the Tradition" was delivered at the 2013 conference. It was my first serious attempt at what its title describes and was the beginnings of Chaps. 10 and 11. "Sub-Sententials: Pragmatics or Semantics?" (2018a) was first delivered at a workshop, "Topics in the Philosophy of Language" in Warsaw in April 2016. It formed the basis for Chap. 12. "Three Mistakes about Semantic Intentions" (2020c) was first delivered in Barcelona in September 2018. It formed the basis for Chap. 4. "A Methodological Flaw? A Reply to Korta and Perry" (2019) was first delivered at the 2018 Dubrovnik conference in response to a paper by Kepa Korta and John Perry (2019a), delivered by Kepa. It is part of the discussion of Korta and Perry in Chap. 7. Finally, "Semantic Polysemy and Psycholinguistics" (2021) was first delivered at the 2019 Dubrovnik conference. Chap. 11 is an expanded version of its main ideas.

At the 2015 Dubrovnik Conference, when my book project was well on the way to completion, I heard about Ernie Lepore and Matthew Stone's book, *Imagination and Convention* (2015), It was clear from what I heard that a major theme of their book, like of mine, was that many allegedly pragmatic phenonema were actually the result of linguistic conventions and hence were semantic. I decided to wait until I had otherwise completed my book before reading theirs. When I did read theirs, I discovered that our books mostly discuss different phenomena and so are complementary. Both books argue that many meanings thought to be the result of pragmatic modifications of one sort or another are in fact conventional; as they say, "the rules of language…are richer than one might have at first suspected" (p. 148). But my focus in arguing for conventions is on "saturations" and polysemous phenomena that they do not consider. And theirs is on phenomena that I do not consider: speech acts, discourse reference, and "information structures" (encoded by intonation in English). Furthermore, their book includes a lengthy and fascinating discussion of figurative language (for example, metaphor, sarcasm, and irony) and evocative language (for example, humor and hinting), topics that are largely missing from mine. Aside from footnotes on "and" and intonation, I have not related the discussion in their admirable book to that in mine.

Kent Bach and Stephen Neale have both staked out interesting, distinctive, and detailed positions on the semantics-pragmatics issue. I am a great admirer of their works and have learnt much from them, and from many personal exchanges. However, we have some fundamental disagreements in our views of language and hence in our approach to the semantics-pragmatics issue. These differences loom quite large in my book. But this does not diminish my debt to Kent and Stephen.

New York, NY, USA   Michael Devitt
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Chapter 1
Introduction

1.1 Background

Perhaps the most exciting development in recent philosophy of language has been the debate surrounding a group of philosophers and linguists that emphasizes the "pragmatic" features of language over the traditional "semantic" ones. The group emphasizes the extent to which the truth-conditional content (meaning)[1] conveyed by a sentence varies in the context of an utterance. They argue that this content, the utterance's message, is constituted pragmatically in context much more than has been customarily thought. This characterization is a bit vague but it has to be to cover the diversity of views in question. The group's seminal work is Dan Sperber and Deidre Wilson's *Relevance* (1995).

My book has two main aims: to look critically at the methodology of the debate and propose a better one; to use the proposed methodology to argue for a fairly traditional position on the substantive semantics-pragmatics issue and against the radical views of the group.

I need a convenient way of referring to the group, abstracting from the many differences among them. This poses a problem. Sperber and Wilson, and many others in the group, are often called "linguistic contextualists" (Cappelen and Lepore 2005). But there are people, notably Kent Bach and Stephen Neale, who are not so-called and yet who share what I am taking to be the definitive property of the group: putting much more on the pragmatic rather than semantic side of the ledger than has been customary and, I will argue, than is appropriate. I wish my term to cover these people too. Given the group's emphasis on the pragmatic over the semantic it seemed to me appropriate in an earlier work (2013c) to use the term 'Linguistic Pragmatist' for the group (and 'Linguistic Pragmatism' for their movement). I borrowed the term from Neale (2004), though I don't claim to be using it exactly as he does. The term is not perfect because it has some very different uses: for example,

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[1] I use 'content' and 'meaning' fairly interchangeably.

in Dewey, and as a label for conceptual-role semantics.[2] (My usage will become much clearer in Sect. 3.5.) Finally, in labeling a position criticized as "Pragmatist", I mean to imply that the position is common among Pragmatists not universal. Indeed, in at least one important case (Chap. 7), my criticism allies me with Bach and Neale. And sometimes a position labelled "Pragmatist" is held by people not in the group, sometimes by very many people; consider reliance on intuitions, for example (Chap. 2).

The folk seem to distinguish what a person *says*, or *literally says*, in an utterance from what the person *means*, from the intended message of the utterance. Paul Grice emphasizes a distinction along these lines between "what is said" and what is "implied, suggested, meant" (1989: 24).[3] Sperber and Wilson's distinction between *explicature* and *implicature* is related. And there are other similar distinctions. Canon Spooner provided an entertaining example of the need for a distinction: he once returned to the pulpit after a sermon to say, "When in my sermon I said 'Aristotle', I meant St. Paul". And Grice gives many interesting examples including a famous one: a philosopher writes a reference in which he *says* that a 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 (1989: 33). Many would say that what is said is a "semantic" matter but what is merely meant is a "pragmatic" matter.

According to a traditional view, stemming from Grice, a large part of "what is said" by an utterance is constituted by the conventional linguistic meaning of the sentence in the language employed by the speaker. These meanings are "known", in some sense, by a competent speaker of the language simply in virtue of her being competent. They are said to be "encoded". However, those meanings do not usually exhaust what is said, for two reasons. (1) A sentence will frequently be ambiguous: more than one meaning is conventionally associated with it. If a sentence is ambiguous, what is said when it is used, when it is "tokened", will be partly determined by which of its meanings is in question in context. (2) An utterance may contain indexicals (and tenses), deictic demonstratives, or pronouns, the references of which are not fully determined simply by conventions. The reference of a "pure" indexical is partly determined by facts about the speaker: 'I' refers to whoever is the speaker, 'here' to his spatial location, 'now' to his temporal location. It is natural to say that the reference of a demonstrative like 'that' or a pronoun like 'it' is determined by what the speaker "intends" or "has in mind" in using the term. I shall say more about this in Sect. 4.1.

It is taken for granted by almost all that "what is said" involves disambiguation and reference determination as well as the conventional meanings of the language employed, as well as what is strictly encoded. The controversy is over whether there

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[2] Thanks to Andrea Bianchi for drawing this to my attention.

[3] I have earlier placed quite a bit of trust in ordinary ascriptions of *saying that*: "So it is likely that, at least, we ought to ascribe to tokens that are thought and uttered the properties that we do ascribe and hence that those properties are meanings" (1996: 71). I acknowledged the apparent folk distinction between saying that and meaning that but did not make much of it (p.59 n.).

is anything else that is determined in context and goes into the truth-conditional message, perhaps into "what is said". And over whether the constitution of any such context-determined extra is "semantic" or "pragmatic". Pragmatists think there is a lot extra and that it is "pragmatic". This yields their theses of "semantic underdetermination" and "truth-conditional pragmatics".

Pragmatists are led to their theses by a range of interesting phenomena. Consider the following utterances:

(1) I've had breakfast.
(2) You are not going to die.
(3) It's raining.
(4) Everybody went to Paris.
(5) The table is covered with books.
(6) John is a lion.
(7) The party was fun until the suits arrived.
(8) The road was covered with rabbit.

Taken literally, (1) seems to say that the speaker has had breakfast sometime in the past and yet, in context, it likely means that she has had breakfast this morning. Similarly, (2) seems to attribute immortality to the addressee but, in context, will mean something like that he will not die from that minor cut. Although (3) does not say so explicitly it surely means that it is raining in a certain location. (4) seems to say that every existing person went to Paris and yet the message it surely conveys is that everyone in a certain group went to Paris. According to the standard Russellian account, (5) makes the absurd claim that there is one and only one table and it is covered with books. Yet it is surely being used to say that a certain table is so covered. (6) says that John is a charismatic feline but means that he is courageous. What ruined the party according to (7) was not really the suits but the business executives wearing them. And what covered the road according to (8) was the remains of rabbits. Examples like these are taken to show that a deal of "pragmatic" enrichment is needed to get from what is "semantically" determined to the message, perhaps to "what is said".

The debate has yielded many theories and a bewildering array of distinctions, and terminology. The many uses of "semantic", "pragmatic", and "what is said" are particularly troubling.[4] One wonders immediately which terms and distinctions are appropriate. And this leads to a deeper question: *How should we get to the truth of the matter on the semantics-pragmatics dispute?* This methodological question receives a clear answer from the literature: we should rely largely, if not entirely, on our intuitions. I have earlier been very critical of this methodology in the theory of language (1996, 2006a, b, 2012a) and continue to be here. I think that we need to do much more: we need to find a respectable scientific motivation for our theories and distinctions and a scientifically respectable way of testing them.

So, in Chap. 2, I criticize the practice of relying on intuitions, calling it the "*First Methodological Flaw of Linguistic Pragmatism*". I later argue that there are two

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[4] See Bach (1999: 81–2) for a nice "chronology of formulations" of the semantics-pragmatic distinction in the last century.

more important flaws, discussed in Chaps. 7 and 8, respectively. The "*Second Methodological Flaw of Linguistic Pragmatism*" is that of confusing "the metaphysics of meaning", focused on the speaker and concerned with *what constitutes* what is said, meant, etc., with "the epistemology of interpretation", focused on the hearer and concerned with *how we tell* what a speaker said, meant, etc. The "*Third Methodological Flaw of Linguistic Pragmatism*" is the acceptance of "Modified Occam's Razor", understood as advising against the positing of a new sense wherever the message can be derived by a pragmatic inference.

The methodology I urge instead yields a theoretically principled distinction between two sorts of properties of an utterance. On the one hand, there are properties that the utterance has *simply in virtue of the speaker's exploitation of her language*. On the other hand, there are properties, which may or may not be different from those ones, that constitute "the message" the speaker intends to convey. I call the first sort "semantic" and part of "what-is-said" or "the proposition said" and the second sort part of "the message" or "the proposition meant". I call any of the latter that are not semantic "pragmatic". Evidence of what-is-said is to be found in evidence of the linguistic rules that have been largely established by conventions. For that evidence we look to the best explanations of regularities in linguistic usage (Chap. 3).

After discussions of speaker meanings (Chap. 4) and conventions (Chap. 5), I take a critical look at the different notions of what-is-said proposed by Kent Bach and Stephen Neale (Chap. 6).

From this methodological perspective, I confront the challenge that Linguistic Pragmatists have posed to the tradition. I argue that three sorts of properties constitute what-is-said: those arising from (i) convention, (ii) disambiguation, and (iii) reference fixing. This view of what-is-said is close to the traditional one that the Linguistic Pragmatists oppose. I then argue, controversially, that almost all of the striking phenomena that they have emphasized exemplify properties of sorts (i) to (iii). There are more of such properties than we have previously acknowledged: much more of the content of messages should be put into the convention-governed what-is-said– into semantics – than has been customary; conventions have been overlooked. Contrary to what the Pragmatists claim, there is no extensive "semantic underdetermination". The new theoretical framework of "truth-conditional pragmatics" is a mistake. The striking phenomena should be accommodated within a traditional framework (Chaps. 9, 10 and 11).

Some terminological points. (a) The words we use to talk about linguistic phenomena, like 'sentence' and 'word' itself, have a familiar type-token ambiguity. Where it is not clear from the context, and where it matters, I will indicate whether I am talking of a type or a token. (b) I shall type such linguistic tokens solely by their overt physical properties, thus yielding what I have called "physical types" (1981a: 10). They might also be typed partly by their semantic properties thus yielding what I have called "semantic types" (*ibid*). I shall not be so typing them. So, on my usage, tokens of 'bank' referring to financial institutions and ones referring to river sides are tokens of the same word-type (which is not to deny, of course, that for some purposes it may be appropriate to type them semantically and so of different types).

(c) In talking about language, the physical types in question are typically sound or inscription types but they can be types in other media. (d) I use 'utterance' to refer to an act of expressing a thought by producing a sentence (or, in circumstances to be discussed in Chap. 12, producing a sub-sentence). And the reference should always be taken to be to an utterance-*token* unless otherwise indicated.

Finally, the convention in philosophy is to use single quotation marks to refer to an expression. In linguistics, the convention is to use italics. I shall mostly follow the philosophical convention but not when discussing a linguist and it would be off-putting.

1.2   Summary of Chapters

1.2.1   Chapter 2: Reliance on Intuitions

How should we discover the truth about language? The received view among Linguistic Pragmatists, indeed among philosophers of language generally, is that we should proceed by consulting intuitions about language. I argue that this is a mistake and constitutes the *First Methodological Flaw of Linguistic Pragmatism*.

Why is it thought to be appropriate to consult intuitions? (1) A seemingly popular answer is that these intuitions are a priori. I argue that, even if we suppose that there is some a priori knowledge, we should not suppose that we have it of meanings. (2) Stich has suggested that philosophers might be guided by linguistics in answering the question: competent speakers of a language derive their semantic intuitions, like syntactic ones, from linguistic rules of the language embodied in their minds. I argue that this "voice of competence" view of our linguistic intuitions is mistaken.

Instead, I urge that intuitions are empirical theory-laden central-processor responses to phenomena differing from other such responses only in being immediate and fairly unreflective. This being so, we should prefer the intuitions of the more expert philosophers to those of the folk. More importantly, we should seek direct, less theory-laden, evidence by studying what the intuitions are *about*, the linguistic reality itself.

1.2.2   Chapter 3: The Semantics-Pragmatics Distinction

I draw two morals from Chap. 2. (1) We should not take any of the intuitively appealing notions thought to be relevant to the semantics-pragmatics distinction – for example, *what is said*, *explicature*, *proposition expressed* – for granted. Notions like these need *theoretical motivation*. (2) Second, we should not look simply to our intuitions for evidence of what our favored notions apply to: we need more direct evidence from linguistic usage.

Concerning (1), I argue that the theoretical basis we need is to be found by noting that languages are representational systems of symbols that scientists attribute to species to explain their communicative behaviors. We then have a powerful theoretical interest in distinguishing, (a), the representational properties of an utterance that arise simply from the speaker's exploitation of a linguistic system from, (b), any other properties that may constitute the speaker's "message". I call the former properties "what-is-said" and "semantic", the latter, "what is meant but not said" and "pragmatic". From this theoretical basis I argue that what-is-said is constituted by properties arising from (i) linguistic conventions, (ii) disambiguations, and (iii) reference fixings.

I then frame the semantics-pragmatics dispute as one between these two doctrines:

> **SEM**: Setting aside novel uses of language, what-is-said (in my sense) is typically the truth conditional speaker-meaning (content) of an utterance, the message conveyed.

> **PRAG**: Even setting aside novel uses of language, the truth conditional speaker-meaning (content) of an utterance, the message conveyed, is seldom, perhaps never, constituted solely by what-is-said (in my sense). The message is always, or almost always, the result of pragmatic modification.

I foreshadow the argument in Chaps. 6, 7, 8, 9, 10, 11 and 12 that the striking examples produced by Linguistic Pragmatists can be accommodated by SEM. This argument counts against the popular Pragmatist theses of "semantic underdetermination" and "truth-conditional pragmatics". SEM is very much in the spirit of the tradition that Pragmatists reject.

Concerning (2), we need evidence from usage of the linguistic rules, rules that have been largely established by conventions. These rules reveal themselves in the regular use of certain forms for certain speaker meanings. We seek evidence of such regularities in the corpus and by the method of "elicited production". Where we find a regularity we must consider whether it is best explained by supposing that there is a linguistic, probably conventional, rule of so using the expression.

1.2.3   Chapter 4: Speaker Meanings and Intentions

There is much talk of intentions in semantics that I argue is mistaken:

(I) In virtue of what does a speaker using a name or demonstrative refer to *x*? A popular answer is: because he *intends to refer to x*. I have four objections. (1) This answer, unlike another popular one – because *he has x in mind* – is too intellectualized to be even a good starting point. (2) It is theoretically incomplete: In virtue of what did the speaker intend to refer to *x*? (3) Once completed, it is redundant. (4) It is misleading.

(II) What explains the speaker meaning of a sentential utterance? A central idea of
      Gricean "intention-based semantics" is that this meaning is constituted by the
      speaker's intention to communicate a certain content to an audience. I argue
      that the idea that this intention is necessary for a meaningful utterance is psy-
      chologically implausible and theoretically unmotivated. The basic act of
      speaker meaning is one of *expressing a thought*. For a speaker to mean that *p*
      by an utterance is for her to be intentionally expressing a thought that *p*.
      Expressing a thought is an act common to speaking, writing, emailing, and so
      on. Game theoretic considerations bear on the choice of an utterance to make
      but not on the explanation of the nature of the one that is made.

(III) It is standard among Griceans to believe that there is some constitutive/norma-
      tive constraint on what a speaker can intend by an utterance, a belief arising
      from one about a constraint on intentions in general. The alleged constraint
      varies from the astonishingly strong "positive" one that *X* cannot intend to *A*
      unless *X* believes that she will *A* to the much weaker "negative" one that *X*
      cannot intend to *A* unless she lacks the belief that she cannot *A*. I argued that
      there are no such constitutive/normative constraints on intentions.

1.2.4   Chapter 5: Linguistic Conventions and Language

Conventions are important to a theory of language because they are the typical
cause of a linguistic expression having its meaning. But, contrary to what some
seem to think, conventions do not *constitute* the meanings of a language. And a
linguistic convention is not constituted by the regularity it usually gives rise to. The
literal meaning of a word in a person's idiolect is constituted by her disposition to
associate the word with that meaning in the production and comprehension of lan-
guage. If she has that disposition *because*, in an appropriate way, other members of
her community have it, then that meaning is conventional.

   John Collins, a follower of Chomsky, is highly critical of appeals to conventions
in explaining language. Indeed, he doubts that talk of conventions could be theoreti-
cally respectable anywhere until it has been fully explained. I argue against this
view. Chomsky himself claims that the "regularities in usage" needed for linguistic
conventions "are few and scattered". Furthermore, such conventions as there are do
not have "any interesting bearing on the theory of meaning or knowledge of lan-
guage". I argue that these claims are very mistaken: there are many linguistic con-
ventions and these are central to explaining language.

   The chapter concludes by discussing malapropisms and related phenomena in
responding critically to Davidson's claim that "there is no such thing as a lan-
guage…we should give up the attempt to illuminate how we communicate by appeal
to conventions".

1.2.5   Chapter 6: Bach and Neale on "What Is Said"

Kent Bach's "semantic" notion of what-is-said is more austere than mine. Like mine, his notion includes properties of an utterance arising from conventions, disambiguations, and the reference fixing of pure indexicals (and tenses) but unlike mine it excludes properties arising from the reference fixing of demonstratives, pronouns, and proper names. What a speaker says using those referential devices is "semantically incomplete", only "a propositional radical". Bach supports this austerity by claiming that the reference fixing of demonstratives, pronouns, and proper names, unlike that of pure indexicals, is dependent on speakers' communicative intentions. I argue that this is not a theoretically sound motivation for the exclusion. The referents of pure indexicals depend as much on speakers' intentional participation in referential conventions as does the referents of demonstratives, pronouns, and names. Reference does not require communicative intentions but, even if it did, that would not support discrimination against demonstratives, pronouns, and names when including referents in what is said.

This criticism of Bach was first presented in my "Good and bad Bach" (2013f). Bach responded (2013) without mentioning the criticism but accusing me of neglecting the motivation he has presented in a passage he cites. I argue that the passage does not motivate his austerity.

Stephen Neale is strongly opposed to notions like Bach's and mine which he calls "transcendental". Underlying Neale's position is a radical dismissal of utterance meanings altogether. He thinks that expression-types have meanings and that a speaker means something by her utterance. However, what is meant is the content of her intention *but not* a property of the utterance itself. He sees no theoretical role for utterance meanings. In particular, he claims that there is a technical difficulty in their being the subject of a compositional semantics. In contrast, my theory is committed to utterances, and their expression-tokens, having meanings.

I present three reasons for this position. (i) Drawing on Chap. 3 I argue that the most basic task for a theory of language is to explain in virtue of what utterances play their striking role in the causal nexus of human lives. It is for that purpose that we should ascribe meanings. And we need to ascribe them to tokens not types because it is tokens that play the causal role. (ii) Talk of types is redundant. Much linguistic theory and its application can be taken straightforwardly to be talking about expression-tokens. And parts that seem to be talking of types can be paraphrased into talk of tokens. This applies, I argue, even to talk of "aphonics", thus removing Neale's technical difficulty. (iii) Even if there were meaningful expression-types, meaningful expression-tokens would be more fundamental: the types would be abstractions from the tokens; they would depend for their very existence on the tokens.

I also take issue with Neale's view that the meaning of an expression-type *constrains* what a speaker can mean by it but *never constitutes* that meaning. It is a consequence of SEM that that type meaning typically constitutes the speaker meaning.

1.2.6   Chapter 7: Confusion of the Metaphysics of Meaning with the Epistemology of Interpretation

This confusion is the *Second Methodological Flaw of Linguistic Pragmatism*.

There is an obvious difference between the study of the properties of utterances – what is said and what is meant – and the study of how hearers interpret utterances. We might say that the former study is concerned with *the metaphysics of meaning*, the latter, with *the epistemology of interpretation*. Yet confusion of these two studies is almost ubiquitous in the pragmatics literature (Levinson, Carston, Sperber and Wilson, Recanati, Bezuidenhout, Stainton, Korta and Perry). We have noted that many Pragmatists think that conventions, disambiguation, and reference fixing semantically underdetermine what is said. As Robyn Carston says, these Pragmatists believe that "pragmatic inference (that is, maxim-guided inference) is required to make up the shortfall" (2004: 67). This is the confusion. If there were a shortfall, it would be made up, just like the standard disambiguation and reference fixing, by something non-inferential that the speaker has in mind. Pragmatic inferences, of which Gricean derivations of conversational implicatures are an example, have absolutely nothing to do with any shortfall in the constitution of what is said. Pragmatic inference is something the hearer may engage in to interpret what is said.

Reinaldo Elugardo and Robert Stainton acknowledge the distinction between the metaphysical and epistemic determination but then surprisingly claim that it does no "undue harm sometimes to ignore the distinction in practice" (2004: 446)! They claim that doing so is not a "mere confusion" and that the topics are "inextricably linked". I argue that their reasons for thinking this are flawed. First, their defense of the confusion would not get off the ground without two Gricean assumptions that I have rejected in Chap. 4: that communicative intentions determine content; second, that there is a certain constitutive/normative constraint on a speaker's intentions. But suppose we grant these two assumptions. Elugardo and Stainton's argument still has two failings. (A) At most it demonstrates an evidential impact of epistemic on metaphysical determination. That is no justification for ignoring the distinction between the two determinations. (B) There is no reason to believe that what a hearer can figure out about what is asserted/stated/said provides evidence of what could be a metaphysical determinant of what is asserted/stated/said.

Ignoring the distinction between the metaphysical and the epistemological wrongly encourages the idea that meanings are pragmatically constituted and hence a doctrine like PRAG.

1.2.7   Chapter 8: Modified Occam's Razor and Meaning Denialism

The chapter looks critically at two conservative strategies that work against SEM by excluding new meanings. First, Grice's "Modified Occam's Razor": "Senses are not to be multiplied beyond necessity". This is commonly construed as advising against

positing a new sense *wherever there is a Gricean (or other pragmatic) derivation* of the message from an uncontroversial old sense, without any consideration of whether that derivation is part of the best explanation of the message. I argue that this way of thinking (Grice, Recanati, Carston, Bach, Ruhl, Cruse, Falkum) is the *Third Methodological Flaw of Linguistic Pragmatism*.

Grice's Razor, as commonly construed, 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". Yet a derivation of what is now a new conventional meaning from the old conventional meaning will still be available. The common construal loses sight of why we posit a language in the first place.

Where a word is regularly used polysemously, the onus is much more on the denier of a new meaning than on the positer of one. A pragmatic explanation of such a regularity needs to meet the "psychological-reality requirement", showing that the explanation's pragmatic derivation has an appropriately *active* place in cognitive lives. In interesting cases, like alleged generalized conversational implicatures, speakers and hearers are not *conscious* of derivations having such a place. A pragmatic explanation then faces two powerful objections. The Occamist Objection that the explanation posits subconscious processes that need, but lack, independent evidence. The Developmental Objection that we have good reason to suppose, a priori, that these processes do *not* exist. We should expect that a pattern of successful communication using a word with a certain once-novel speaker meaning would lead to it having that meaning conventionally. For that is the sort of process that gave us our language.

Bach is an extreme meaning denialist. Modified Occam's Razor is one of his strategies for this. He (2013) has responded dismissingly to an earlier version of my criticism (2013f) of his embrace of the Razor. I argue that he has not answered the criticism.

Bach has another strategy for excluding new meanings: many regularities in usage are to be explained not as conventionalizations but as *standardizations*. Standardizations are said to differ from conventionalizations in their relation to "mutual beliefs" and in their involving "streamlined" pragmatic inferences. I argue 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.

The strategies for meaning denialism discussed in this chapter have been very costly, misleading people into thinking that meanings that are, as a matter of fact, context-relative because expressions are ambiguous must be treated as pragmatically constituted. This has led to the mistaken thesis of PRAG.

1.2.8   Chapter 9: Referential Descriptions: A Case Study

My discussion elsewhere of referentially used definite descriptions is a paradigm of the approach I am urging to support the SEM view of the examples that motivate Pragmatism. Many Pragmatists treat these referential uses in a Gricean way as involving conversational implicatures (or something similar). Others treat them in a Relevance-Theoretic way where both referential and attributive uses involve other sorts of pragmatic modifications. I argue that referential (and attributive) uses are best explained semantically.

The Argument from Convention (as Neale calls it) provides the positive argument for this SEM view. The basis for the thesis that descriptions have referential meanings is not simply that we *can* use them referentially for, as the Pragmatists point out, we can use *any* quantifier referentially. The basis is rather that we *regularly* use descriptions referentially. Indeed, the vast majority of uses of descriptions are referential. The regular use exemplifies a semantic convention.

The argument in Chap. 8 that appealed to dead metaphors shows that for the alternative PRAG explanation to be right, more is required than that the singular proposition arising from a referential use of a definite can be pragmatically derived from the literal meaning of the utterance. The PRAG explanation, whether Gricean or Relevance-Theoretic, must meet the "psychological-reality requirement", showing that the explanation's pragmatic derivation has an appropriately *active* place in cognitive lives. This requirement has not been met, nor has there been any psycholinguistic attempt to meet it. According to the Developmental Objection it is unlikely to be met. According to the Occamist Objection, we should then prefer the SEM view.

There is a further objection to Gricean versions of PRAG explanations. An important feature of such explanations of a referential use of 'the *F*' is that a speaker who is pragmatically implying a singular proposition about a particular object in mind must also be conventionally conveying a general proposition about whatever is uniquely *F*. Attending to incomplete descriptions, I use an "ignorance and error" argument to show that this condition could frequently not be met.

I reinforce this case by considering Bach's lengthy defense of the Russellian *status quo*. He argues that referential uses are "akin to" generalized implicatures. The heart of his argument is the claim that the quantificational meaning plays a "key role" in referential uses. This claim is not supported and the argument fails.

That concludes my main case for the SEM view, but I add three further arguments. Finally, I look critically at Neale's view (2004) that the debate between referentialists and Russellians is "the product of a powerful illusion". The debate is over a real issue and the referentialists have won.

1.2.9   Chapter 10: Saturation and Pragmatism's Challenge

The challenge posed by Linguistic Pragmatists stems from the many examples of context relativity that their investigations have revealed. In arguing for SEM and against PRAG, I divide these examples into two groups, one concerned with saturation, discussed in this chapter, the other concerned with polysemy, discussed in the next.

After a brief rejection of "meaning eliminativism", I argue that a range of phenomena, including weather reports, quantifications, 'ready', 'qualified', genitives, and 'enough' should be explained by taking their linguistic meanings to demand saturations in context; the conventionally established meaning has an implicit slot to be filled. This is the best explanation of the regular saturation of these expression to convey messages.

The rival explanation is that these saturations arise from pragmatic modifications of one sort or another. But these PRAG explanations, like those before (Chaps. 8 and 9), face the heavy onus of the psychological-reality requirement: they must show that the pragmatic processes have an appropriately *active* place in cognitive lives. Once again, this requirement has not been met, nor has there been any psycholinguistic attempt to meet it. According to the Developmental Objection it is unlikely to be met. According to the Occamist Objection, we should then prefer SEM.

I conclude by considering an objection to SEM's positing of implicit slots to be filled. Bach objects to positing any slot that is not "there" in the favored syntax, even if only as an aphonic. 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. I reject this "tyranny of syntax", arguing that no such principle *could* show that a slot-filling convention for, say, quantifiers or 'ready' is *impossible*. It is always an open question to what extent, if any, a meaning is to be explained in terms of a matching syntactic structure. I reinforce this point by siding with Perry against two syntax-driven critics of his discussion of "unarticulated constituents", Stanley and Sennet. As Neale argues in criticizing Stanley, Perry's position implies no interesting syntactic thesis. Neither does mine.

1.2.10   Chapter 11: Polysemy and Pragmatism's Challenge

According to SEM the regular use of a polysemous expression with a certain meaning is typically a semantic phenomenon not a pragmatic one: absent novel spur-of-the-moment modifications or implicatures, the polyseme typically contributes one of its encoded meanings, selected in context, to the message of an utterance. Regular polysemy is discussed and it is argued that some popular alleged examples of this – for example, 'book', 'city' – involve a mistaken metaphysics and should be dismissed. Other issues discussed include "underspecified" monosemy, co-composition, and generalized conversational implicature.

I aim to bring the psycholinguistic literature on polysemy to bear on the SEM-PRAG. This requires some preliminary work discussing some expressions that

feature prominently in that literature: "Sense Enumeration Lexicon"; "underspecification"; "overspecification"; "represented and stored"; "information-rich". I then defend SEM from objections, including some from psycholinguistics and one from the phenomenon of "copredication".

The contrary view of polysemous phenomena, PRAG, is that a pragmatic modification of meaning typically plays a role in constituting messages. This generates a psychological-reality requirement: pragmatic explanation is committed to there being regular subconscious psychological processes in speakers and hearers that are appropriately different from the standard convention-exploiting ones, whatever they may be, involved in the use of ambiguous terms. I offer Occamist, Developmental, and Abstract-Core Objections to PRAG, which place a heavy evidential burden on PRAG. I argue that the evidence on linguistic processes provided by psycholinguistics does not come close to fulfilling that burden. At this point there seems to be no evidence from the study of the mind that counts significantly against the evidence from behavioral regularities for the SEM view of polysemy.

1.2.11   Chapter 12: Sub-Sententials: Pragmatics or Semantics?

Stainton points out that speakers "can make assertions while speaking sub-sententially". He argues for a "pragmatics-oriented approach" to these phenomena and against a "semantics-oriented approach". In contrast, I argue for SEM, a semantics-oriented approach: typically, what is asserted by a sub-sentential of a form that is regularly saturated in context is a proposition, a truth-conditional semantic property of the utterance. Thus, there is an "implicit-demonstrative convention" in English of expressing a thought that a particular object in mind is *F* by saying simply '*F*'. I note also that some sub-sentential assertions include demonstrations and argue that these exploit another semantic convention for expressing a thought with a particular object in mind. So, sub-sententials are to be treated like the sentential examples in Chap. 10.

I consider four objections that Stainton has to a semantics-oriented approach. One is aimed at Stanley's semantics-oriented approach in particular. Stanley claims, in Stainton's words, "that much, or even all, of such speech is actually syntactically elliptical – and hence should be treated semantically, rather than pragmatically" (2005: 383–4). Stainton objects that there is no such ellipsis with the sub-sententials in question. My semantics-oriented approach does not make the syntactic-ellipsis claim and so is not open to the objection. Still, the objection is theoretically interesting. I wonder about its appropriateness. I go on to reject the other three objections: "too much ambiguity"; "no explanatory work"; and "fails a Kripkean test".

Nonetheless, the message of *novel* sub-sentential utterances, perhaps asserting only a fragment of a proposition, must come at least partly from pragmatic enrichment. To this extent I am in accord with a pragmatics-oriented approach. But it is not an interesting extent because novel nonconventional uses of language must obviously be explained pragmatically.

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Chapter 2
Reliance on Intuitions

I noted in Sect. 1.1 that the received view among Linguistic Pragmatists is that we should find the truth about language by consulting intuitions. And I claimed that this is the *First Methodological Flaw of Linguistic Pragmatism*. This chapter will support that claim.[1]

2.1 The Received View

First, we need to support the attribution of this methodology to the Pragmatists. Consider what some leading figures have to say. Stephen Neale gives the following sweeping endorsement of the role of intuitions:

> 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. (2004: 79)[2]

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). François Recanati claims that "'what is said' must be analysed in conformity to the intuitions shared by those who fully understand the utterance" (2004: 14).

This enthusiasm for consulting intuitions is not confined to the Linguistic Pragmatists, of course. Indeed, it would be hard to exaggerate both the apparently dominant role of such intuitions in the philosophy of language and the agreement among philosophers that these intuitions should have this role. This emphasis on

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[1] The discussion is a short version of a position developed in many earlier works cited below. For more, see particularly Devitt 2006a, b, Chap. 7, 2006d, 2012b, 2014c, 2020a.

[2] In his latest work, Neale urges a very different and, in my opinion, much more appropriate view of the role of intuitions (2016: 231, 234).

intuitions reflects, of course, a widely held view about the methodology of "armchair philosophy" in general.[3] Saul Kripke's *Naming and Necessity*, one of the most influential works in the philosophy of language, is often, and rightly, cited as an example of heavy reliance on intuitions. And Kripke is explicit that intuitions should have this important role:

> Of course, some philosophers think that something's having intuitive content is very inconclusive evidence in favor of it. I think it is very heavy evidence in favor of anything, myself. I really don't know, in a way, what more conclusive evidence one can have about anything, ultimately speaking. (1980: 42)

Jason Stanley and Zoltan Szabó claim that "accounting for our ordinary judgements about the truth-conditions of various sentences is the central aim of semantics" (2000a: 240).

A similar practice is also to be found in linguistics. Noam Chomsky claims that "linguistics …is characterized by attention to certain kinds of evidence…largely, the judgments of native speakers" (1986: 36). Liliane Haegeman, in a popular textbook, says that "all the linguist has to go by...is the native speaker's intuitions" (1994: 8).[4] Geoffrey Nunberg remarks: "The standard practice in both linguistics and lexicography has been to rely on intuition to distinguish conventional and non-conventional word uses" (1979: 146).

2.2   The Task?

It is clear from these claims, and other similar ones, that intuitions are commonly thought to provide *the evidence* or, at least, *the main evidence* for theories of language. Yet, often, claims of this sort seem to suggest a stronger view: *the very task* of theories of language is to explain or systematize competent speaker's intuitions about language.

Such a view of the task is very puzzling. For, the obvious way to describe the task of the theory of language is to *explain the nature of language*, to explain properties like *meaning*, *truth*, *reference*, and *grammaticality*, real properties of linguistic symbols playing some sort of explanatory role. If we start from this view, surely as good a starting place as one could have, why take the task to be to capture *ordinary intuitions about* such properties, intuitions that must simply reflect folk theory? Nobody would suppose that the task of physics, biology, or economics was to explain folk intuitions, why should the situation be any different in linguistics and semantics?

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[3] This view of the philosophical method does have its critics though; see Deutsch 2009 and Cappelen 2012; Devitt 2015b is a response.

[4] Despite this, linguistics, unlike the philosophy of language, does use evidence other than intuitions; see Sect. 3.6.

So let us set aside the idea that the study of linguistic intuitions is the task of theories of language and turn to the more plausible view that these intuitions are evidence for those theories.

2.3 Experimental Semantics

The evidential role of intuitions in semantics, indeed, the evidential role of intuitions in philosophy generally, has recently grabbed the attention of a group known as "experimental philosophers". They have noted that the intuitions that play this evidential role are usually not those of the folk but rather those of the philosophers themselves. In response, they have conducted experiments to test whether the folk share these intuitions. Thus, in a well-known experiment, the intuitions that Kripke airs about reference were tested against those of undergraduates in Rutgers and Hong Kong (Machery et al. 2004). The results raised serious doubts in the experimenters' minds about Kripke's refutation of the description theory of names. There is plenty of room for debate about whether, and to what extent, the results *should* raise these doubts.[5] But one thing such experiments surely should do is focus our attention on two important questions: Why is the evidential role given to intuitions thought to be appropriate? Is it really appropriate? These questions will be the focus of this chapter.

2.4 "Cartesianism"

It would, of course, be appropriate to give a person's intuitions an important evidential role if we could be confident that they reflected *knowledge*. And the received view is that a competent speaker of a language does indeed have knowledge about her language, *propositional* knowledge, "tacitly" at least, *simply in virtue of being competent* in the language:

> It is an undeniable feature of the notion of meaning... that meaning is *transparent* in the sense that, if someone attaches a meaning to each of two words, he must know whether these meanings are the same. (Dummett 1978: 131)

> The natural view is that one has *some kind of* privileged semantic self-knowledge. (Loar 1987: 97)

The idea of this sort of privileged access - that we are in a special position to know about our own linguistic competence - is an instance of general "Cartesianism":

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[5] See Ludwig 2007; Jackman 2009; Martí 2009, 2012, 2014; Devitt 2011b, 2012a, c; Ichikawa et al. 2011; Sytsma and Livengood 2011.

> Since Descartes, it has seemed undeniable to most philosophers that each of us has a privileged way of knowing about his or her own mental states.… whenever we have a thought, belief, intention, or desire, we can in principle come to know *what* we think, believe, intend, or desire just by internal examination, without engaging in an empirical investigation of the external world. (McKinsey 1994: 308)

The idea that we have a Cartesian access to semantic facts seems to be an almost unquestioned part of the semantic traditions of Frege and Russell. Consider, for example, the assumption that our competence consists in propositional knowledge of truth conditions. Herbert Heidelberger (1980) has shown how widespread this assumption is with references to Wiggins, Strawson, Davidson, Frege, Wittgenstein, Quine, and Carnap. He points out that it seems to be regarded as "uncontroversial …harmless…perhaps unworthy of serious discussion" (p. 402). Gareth Evans says, "perhaps no one will deny it" (1982: 106).

It is worth noting that one of the most famous arguments in the philosophy of language seems to rest on Cartesianism. This is the Fregean argument that 'Hesperus = Hesperus' and 'Hesperus = Phosphorus' must differ in meaning, hence 'Hesperus' and 'Phosphorus' must differ in meaning, because they differ in cognitive significance or informativeness. Now I accept (1989), of course, that they do differ in meaning but why would the latter *epistemic* difference be taken to establish this *semantic* difference? The reason, I suggest (1996: 172–3), is that it is taken for granted that two sentences mean the same only if competent speakers *know* that they do; see Dummett above. And speakers could know this only if sentences did not differ in cognitive significance for them. For, if they differed in significance, speakers would not think that they were synonymous.

2.5   A Priori Knowledge?

Why should we suppose that ordinary competent speakers have this knowledge of semantic facts? Many seem to think that the knowledge is a priori. Thus Jerrold Katz claims: "We know sense properties and relations of expressions on the basis [of] the speaker's a priori linguistic intuitions in clear cases" (1997: 21). And Michael McKinsey thinks that it is "fairly clear" that "the principle that the meanings of words are knowable a priori…is taken for granted by most philosophers of language and by many linguists" (1987: 1).

Now, of course, the idea that some knowledge is a priori is widespread in philosophy. Nonetheless, Quine has raised serious doubts about it. The main problem with the idea, in my view (1994, 1996, 1998, 2011a), is that we do not have even the beginnings of an account of what a priori knowledge *is*. We are simply told what it *isn't*, namely empirical knowledge.[6] Still, suppose we set such general doubts aside and accept that at least our knowledge of mathematics and logic is a priori, what

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[6] The following is an exchange on the a priori: Bon Jour 2005a, b, c; Devitt 2005a, b.

could be the basis for supposing that our knowledge of *meanings* is too? The meaning of a word is presumably constituted by relational properties of some sort: "internal" ones involving inferential relations among words and/or "external" ones involving certain direct causal relations to the world referred to. Where the meaning is partly constituted by a certain external relation – plausible examples are the meanings of 'Cicero', 'elm', and 'water' (Kripke 1980; Putnam 1975) – then it is hard to see how a priori reflection on what is "inside the head" could establish that that external relation constitutes a meaning. But even meanings constituted by internal relations pose serious problems for the a priori view. Let Susan be a competent speaker alleged to have a priori knowledge that, say, the inferential relation between 'bachelor' and 'unmarried' is part of the meaning of 'bachelor'. The first problem for the a priori view is that it requires that Susan comes to this belief about meaning rather than the belief that the inferential relation is simply the reflection of familiar knowledge about bachelors. Why should we suppose that her competence alone leads her to make this apparently large *theoretical* step? Suppose, however, that she does make it. The second and more serious problem for the a priori view is that we seem to have no basis for thinking that, simply in virtue of her competence, Susan's belief about meaning is *justified*. We have no basis for giving her belief any special epistemic authority, and thus turning it into *knowledge*. We need a plausible explanation of these allegedly nonempirical processes of belief formation and justification and some reasons for believing in them.

2.6   Embodied Theory?

If the view that competent speakers have a priori knowledge of linguistic facts does not hold up, what else could justify the ubiquitous reliance on intuitions in the philosophy of language? Philosophers provide little in the way of an answer. As Jaakko Hintikka remarks, talking of philosophy generally: "One searches the literature in vain for a serious attempt to provide" a justification for the appeal to intuitions (1999: 130). In similar vein, Timothy Williamson remarks: "there is no agreed or even popular account of how intuition works, no accepted explanation of the hoped-for correlation between our having an intuition that P and its being true that P." He describes this as "a methodological scandal" (2007: 215). Still, we can look to linguistics for a possible answer for *semantic* intuitions. (Perhaps we should see this answer as an explication of the obscure a priori answer.)

The common linguistic view of intuitive judgments is expressed in passages like the following:

> it seems reasonably clear, both in principle and in many specific cases, how unconscious knowledge issues in conscious knowledge...it follows by computations similar to straight deduction. (Chomsky 1986: 270)

The "unconscious knowledge" is of the language's grammar, a knowledge that constitutes a speaker's competence in the language; the "conscious" knowledge" is

intuitions about the syntactic properties of expressions in the language. I have described this common view as follows: linguistic competence, all on its own,

> provides information about the linguistic facts…. So these judgments are not arrived at by the sort of empirical investigation that judgments about the world usually require. Rather, a speaker has a privileged access to facts about the language, facts captured by the intuitions, simply in virtue of being competent… (2006a: 483–4, 2006b: 96)

On this view, intuitive syntactic judgments are, "noise" aside, "the voice of competence". Let's call this thesis "VoC".[7] We can identify two versions of it, a "standard" one which requires linguistic rules (and principles) to be *represented* in the mind and a "nonstandard" one which does not. According to the standard version, suggested by the Chomsky quote and described in detail by Graves et al. (1973), speakers derive their intuitive judgments from their representations of linguistic rules by a causal and rational process like a deduction. According to the nonstandard version, the intuitions must be provided somehow by embodied but unrepresented rules (2006a: 482–6, 2006b: 96–8).

Stich has suggested that philosophers of language might be guided by linguistics in seeking a justification for the authoritative role given to semantic intuitions (1996: 40; see also Hintikka 1999; Williamson 2007). Stich's particular concern is with referential intuitions. He suggests that philosophers may think that speakers derive their referential intuitions from a representation of referential principles. So, just as, according to the standard version of VoC, the true grammar that linguists seek to discover is already represented in the mind of every speaker, so too, according to this suggestion, are true semantic theories of reference, meaning, and the like. Semantic intuitions, like syntactic ones, are the result of something like a deduction from a represented theory. Thus, speakers have Cartesian access to linguistic facts simply in virtue of being competent.

Stich's suggestion concerns the standard version of VoC, which posits representations of theories of language in the minds of ordinary speakers. What about the nonstandard version which does not posit these representations? Stich does not consider this version as a model for semantic intuitions but if the model worked for syntax it should work for semantics as well.

Someone who took the usual linguistic view of grammatical intuitions might well be tempted by this analogous view of semantic intuitions. So, it is interesting to note that Chomsky is not tempted. He expresses skepticism about "contemporary philosophy of language" and its practice of "exploring intuitions about the technical notions 'denote', 'refer', 'true of', etc." He claims that

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[7] I thought that the evidence for this attribution of VoC to linguists was overwhelming and so was surprised to find three knowledgeable philosophers rejecting the attribution: John Collins (2008a: 17–19), Gareth Fitzgerald (2010), and Peter Ludlow (2011: 69–71). I have responded to Fitzgerald (Devitt 2010c: 845–7) and to Ludlow (Devitt 2014c: 274–8). Ludlow's discussion is notable for its egregious misrepresentation of the evidence. I have also provided more evidence (2014c: 273) in the works of Barry Smith (2006), Mark Textor (2009), and Georges Rey (2014). I still think that the evidence is overwhelming. But see Jeffrey Maynes and Steven Gross (2013) for a nice discussion of the matter.

> there can be no intuitions about these notions, just as there can be none about 'angular velocity' or 'protein'. These are technical terms of philosophical discourse with a stipulated sense that has no counterpart in ordinary language. (1995: 24)

So Chomsky is skeptical about the use philosophers make of semantic intuitions. But he is not, of course, similarly skeptical about the use linguists make of syntactic ones. Why the difference? If skepticism about semantic intuitions is appropriate, then surely just the same skepticism is appropriate about the syntactic ones, and for just the same reason. All the terms in linguistic theory are, in the relevant sense, technical and theory-laden. A few like 'grammatical' and 'sentence' have counterparts in ordinary language but so too do 'denote' and 'refer'. Semantic and syntactic intuitions are on a par. Chomsky seems to have given a good reason for rejecting VoC altogether.

In the last section we looked dimly on the idea that linguistic intuitions are a priori. We have just raised a preliminary doubt about VoC. Before raising more doubts, we need to describe an alternative view. This alternative accompanies the view that linguistic competence is simply a skill.

2.7 Competence as a Skill

It is natural to follow the folk in talking of a person's linguistic competence as a form of "knowledge". We noted in Sect. 2.4 that it is common to think of this knowledge as *propositional*, knowledge-*that*, leading to the idea that speakers have Cartesian access to linguistic facts. But there is an alternative. If we must follow the folk in talking of competence as knowledge, we can think of it as mere knowledge-*how*. However, I think it is better to talk of the competence as simply a skill or ability.

What skill or ability is the competence? Accepting, as we should, that "language expresses thought" (Sect. 3.2.3), I give the following answer:

> the competence is the ability to use a sound of the language to express a thought with the meaning that the sound has in the language in the context of utterance; and the ability (together with some pragmatic abilities) to assign to a sound a thought with the meaning that the sound has in the language in the context of utterance (similarly for inscriptions, etc.). (2006b: 148)

We can move to a more theory-laden view of competence if we adopt the popular, and in my view correct, representational theory of the mind according to which any thought involves standing in a certain functional relation to a mental representation. Competence is then "the ability to *translate* back and forth between meaningful mental representations and the sounds of the language". And if we go further to the controversial language-of-thought hypothesis according to which the mental representation is language-like, the translation is "between mental *sentences* and the sounds of the language" (*ibid*). Finally, linguistic competence is complex, consisting of syntactic competence and lexical competence. Thus, going along with the

language-of-thought hypothesis, syntactic competence is the ability to translate back and forth between the syntactic structures of the sounds of the language and the structures of mental sentences. Lexical competence is the ability to translate back and forth between the words of the language and mental words. (I now prefer to express the least theory-laden view of lexical competence as follows (see Sect. 5.1): it is a *disposition*, in production and understanding, to associate the word with the meaning it has in the language.)

Why think that that linguistic competence is just a skill or ability? Briefly, because it has all the marks of one: it has limited plasticity; it is extraordinarily fast; the process of exercising it is unavailable to consciousness; once established, it is "automatic" with the result that it can be performed whilst attention is elsewhere (2006b: 209–10); it is very likely acquired by "implicit learning" (2006b: 219). But shouldn't we suppose that in the case of linguistic competence, the skill involves knowledge-that? I argue not (2006b). We should suppose that only if we have some powerful reasons for doing so. Otherwise the supposition seems gratuitous. Why suppose that simply in virtue of being competent in a language a person must have propositional knowledge about the language? Why suppose that speakers have this sort of Cartesian access to linguistic facts? Why not suppose, rather, the modest view that any knowledge of these facts that a speaker may have comes from ordinary empirical reflection on linguistic phenomena?

Assumptions that competence involves propositional knowledge are very immodest. One would expect, therefore, that they would be well supported by arguments. This is not what we find. Arguments for them are few and far between and remarkably thin. (Stanley and Williamson 2001 may rate as an exception; Devitt 2011c is a response.) One gets the impression that these propositional assumptions are thought to be too obvious to need argument.

2.8   Intuitions as Empirical Judgments

If linguistic competence is simply a skill, requiring no propositional linguistic knowledge, it is hard to see how it could be the source of our linguistic intuitions as VoC requires. But what is the alternative? I think that we should see intuitions as ordinary empirical judgments (1994, 1996, 2006a, b; see also Kornblith 1998). Like the skill-view of competence, this alternative is modest: it makes do with cognitive states and processes we were already committed to.

The alternative is based on a view of intuitions *in general*: they "are empirical theory-laden central-processor responses to phenomena, differing from many other such responses only in being fairly immediate and unreflective, based on little if any conscious reasoning" (2006a: 491, 2006b: 103). To say that intuitions are "theory-laden" is *not* to say that they are theoretical judgments or even the result of theorizing. Rather, the intuitions are mostly *the product of experiences* of the world. They are like 'observation' judgments; indeed, some of them *are* observation judgments. As such, they are 'theory-laden' in just the way that we commonly think

observation judgments are. The anti-positivist revolution in the philosophy of science, led by Thomas Kuhn and Paul Feyerabend, drew our attention to the way in which even the most straightforward judgments arising from observational experiences may depend on background expertise. We would not make the judgments if we did not hold certain beliefs or theories, some involving the concepts deployed in the judgments. We would not make the judgments if we did not have certain predispositions, some innate but many acquired in training, to respond selectively to experiences.[8]

From this perspective, we should trust a person's intuitions to the degree that we have confidence in her empirically based expertise about the kinds under investigation. Sometimes the folk may be as expert as anyone: intuitions laden with "folk theory" are the best we have to go on. Perhaps this is the case for a range of psychological kinds. For most kinds, it clearly is not: we should trust intuitions laden with established scientific theories. Consider, for example, a paleontologist in the field searching for fossils. She sees a bit of white stone sticking through grey rock, and responds immediately "a pig's jawbone". We trust her intuitive judgment in a way that we would not trust folk judgments because we know that it is the result of years of study and experience of old bones; she has become a *reliable indicator* of the properties of fossils. Similarly we trust the intuitions of the physicist over those of the folk about many aspects of the physical world where the folk have proved notoriously unreliable. And recent experiments have shown that we should have a similar attitude to many psychological intuitions. Thus, the cognitive psychologist, Edward Wisniewski, points out that

> researchers who study behavior and thought within an experimental framework develop *better* intuitions about these phenomena than those of the intuition researchers or lay people who do not study these phenomena within such a framework. The intuitions are better in the sense that they are more likely to be correct when subjected to experimental testing. (1998: 45)

Although we may often be right to trust an intuition in the short run, it is crucial to see that nothing rests on it in the long run. We can look for more direct evidence in scientific tests. In such a scientific test we examine the reality the intuition is *about.* These scientific examinations of reality, not intuitions about reality, are the primary source of evidence. The examinations may lead us to revise some of our initial intuitions. They will surely show us that the intuitions are far from a complete account of the relevant bit of reality.

What then should we make of *linguistic* intuitions? All these intuitions arise from reflection on linguistic data (understood as pieces of linguistic usage, on the model of "primary linguistic data"). The competent speaker has ready access to a great deal of linguistic data just as the competent typist has to a great deal of typing data and the competent thinker has to a great deal of thinking data: the competent speaker and her competent fellows produce linguistic data day in and day out. So she is

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[8] So 'theory' in 'theory-laden' has to be construed *very* broadly to cover not just theories proper but also these dispositions that are part of background expertise.

surrounded by tokens that may, *as a matter of fact*, refer to so and so, be true in such and such circumstances, be grammatical, be ambiguous, and so on. So she is in a position to have well-based opinions about language by reflecting on these tokens. This is not to say that she will reflect. Indeed, a totally uneducated person may reflect very little and hence have few if any intuitive judgments about her language. Still it is clear that the normal competent speaker with even a little education *does* reflect on linguistic reality just as she reflects on many other striking aspects of the world she lives in. And this education will usually provide her with the terms and concepts of folk semantics and linguistics, at least. As a result she is likely to be able to judge in a fairly immediate and unreflective way that a token *does* refer to so and so, *is* true in such and such circumstances, *is* grammatical, *is* ambiguous, and so on. Such intuitive opinions are empirical central-processor responses to linguistic phenomena. They have no special authority: although the speaker's competence gives her ready access to data it does not give her Cartesian access to the truth about the data.

Still, are these intuitions likely to be right? I think we need to be cautious in accepting them: thinking about language is notoriously hard and the folk are a long way from being experts. Still it may seem likely that their "simplest" intuitions, involving syntactic and semantic vocabulary that we suppose the folk have mastered well enough, are quite likely to be right. So perhaps we can often be confident about judgments that a name "refers" to *x*, that this pronoun must "refer to the same thing" as that name, and that this expression is "ambiguous". Perhaps the core judgments of folk linguistics, reflecting the "linguistic wisdom of the ages", are good evidence for linguistic theories. But recent experiments on referential intuitions suggest that this may be too optimistic (Sytsma and Livengood 2011; Domaneschi et al. 2017; Devitt and Porot 2018.).

This having been said, the intuitions that philosophers and linguists should prefer are the ones that they do, as a matter of fact, mostly prefer: those of philosophers and linguists themselves. For, they are much more expert. This is particularly so when we get beyond the simple cases to fanciful ones like Kripke's Gödel (1980) and Putnam's Twin Earth (1975). Just as the intuitions of paleontologists, physicists, and psychologists in their respective domains are likely to be better than those of the folk, so too the intuitions of philosophers and linguists.[9]

To say that intuitions, whether those of philosophers or of the folk, are evidence is not to say that they are the only, or even the primary, evidence. We do not rest our biological theories on evidence from the intuitions of biologists about living things, let alone from the intuitions of the folk. We do not rest our economic theories on

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[9] This line of thought yielded an example of what has become known as 'the Expertise Defense' against the findings of Machery et al. 2004. The Expertise Defense has led to a lively exchange of opinion: Weinberg et al. 2010; Machery and Stich 2012; Machery et al. 2013; Machery 2012a; Devitt 2012a; Machery 2012b; Devitt 2012c. See also the following exchange arising out of the analogous claim that we should prefer the grammatical intuitions of linguists over those of the folk: Devitt 2006a: 497–500 or 2006b: 108–11; Culbertson and Gross 2009; Devitt 2010c; Gross and Culbertson 2011.

evidence from the intuitions of economists about money and the like, let alone from the intuitions of the folk. No more should we rest our semantic theories on evidence from the intuitions of philosophers about reference and the like, let alone from the intuitions of the folk. Such intuitions from people who are reliable about the reality in question, are of course good evidence about the nature of that reality. But, at best, they are only indirect evidence. *The primary evidence for a theory about a certain reality comes not from such intuitions about the reality but from more direct examinations of that reality*. In the case of language, that reality is to be found in linguistic usage.[10] I shall discuss the gathering of evidence from usage in Sect. 3.6.

2.9   Rejecting "Voice of Competence"

VoC, whether as a proposal for syntactic intuitions or as one, following Stich's suggestion, for semantic intuitions, is much more promising than the view that we have a priori knowledge of such matters. Nonetheless, I have argued against VoC, claiming that we should prefer the "Modest Explanation" (2006a, b, 2010b, c, 2014c, 2020a).[11] I have summed up my most general criticism of VoC as follows:

> first, …to my knowledge, it has never been stated in the sort of detail that could make it a real theory of the source of intuitions. Just *how* do the allegedly embodied principles yield the intuitions? We need more than a hand wave in answer. Second, again to my knowledge, no argument has ever been given for VoC until Georges Rey's recent attempt (2014) which, I argue (2014bc), fails. Third, given what else we know about the mind, it is unlikely that VoC could be developed into a theory that we would have good reason to believe. (2015a: 37)

Here are more detailed criticisms.

I identified two versions of VoC, a "standard" one which requires linguistic rules to be *represented* in the mind and a "nonstandard" one which does not. The main

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[10] One might well respond that even direct evidence from usage rests on intuitions *of some sort*, for example, the intuitive perceptual judgment that a speaker uttered "Blah" conveying the message that *p* (Sect. 3.6). But such intuitive judgments are not the metalinguistic intuitions we are talking about: they are not judgments that expressions have semantic and syntactic properties *of the sort adverted to in the very theories being tested*; so, they are not like a judgment that an expression refers to *x*, is ambiguous, is synonymous with a certain other expression, and so on. For discussion of this contrast between the metalinguistic intuitions that are only indirect evidence and the more basic, less theory-laden, intuitive judgments about usage that constitute the primary evidence, see Devitt 2015a: 39–45.

[11] My discussion of intuitions immediately generated the following criticisms: Collins 2006, Matthews 2006, Miščević 2006, Rattan 2006, Rey 2006b and Smith 2006. Devitt 2006c is a response. For some later criticisms, see: Pietroski 2008, responded to in Devitt 2008c; Textor 2009, responded to in Devitt 2010b; Culbertson and Gross 2009, which led to the exchange, Devitt 2010c, Gross and Culbertson 2011; Fitzgerald 2010, responded to in Devitt 2010c; Ludlow 2011 and Rey 2014, responded to in Devitt 2014c; Gross 2020 and Rey 2020a, responded to in Devitt 2020a. Miščević 2009 takes up the issue again in a paper in honor of Dunja Jutronić, with a follow up (2012). Devitt 2014b and Jutronić 2014 are responses; finally (everyone hopes), Miščević 2014a, b, Devitt 2018b, Jutronić 2018.

consideration against the standard version of VoC is as follows. It is of course *possible* that the competent speaker's intuitions about her language are reliable because they are derived from her mental representations of theories of the language. It has been argued, mistakenly in my view (2006b: 195–272), that we need to posit such representations to explain language acquisition and use. In any case, we surely do not need the posit to explain the reliability of intuitions about language. Consider the analogous phenomena for typing and thinking. We can explain the reliability of intuitions about those processes by adverting to cognitive states and processes that we are already committed to, without positing representations of the rules that govern the processes. These modest explanations seemed perfectly adequate for the job and, indeed, much more plausible than their representational rivals. So do the similarly modest explanations in the linguistics case. Language is an important part of the human environment. It would not be surprising that empirical reflection on linguistic data, aided by some education, should have made people fairly reliable detectors of the most obvious facts about language. We are surely similarly reliable about other important parts of the environment, for example, the physical, biological, and psychological parts.

The main consideration against the nonstandard version of VoC is that we do not have *any idea* how embodied but unrepresented rules might provide linguistic intuitions. Not only do we lack the details needed for a plausible explanation but attention to other similar systems gives good reason to suppose that the linguistic system does not provide these intuitions and so we *could never* have the details. The explanation would require a relatively direct cognitive path from the embodied rules of the language to beliefs about symbols of that language, a path that does not go via central-processor reflection on the data. What could that path be? Consider some other examples. It is very likely that rules that are embodied but not represented govern our swimming, bicycle riding, catching, typing, and thinking. Yet there does not seem to be any direct path from these rules to relevant beliefs. Why suppose that there is such a path for linguistic beliefs? Why suppose that we can have privileged access to linguistic facts when we cannot to facts about these other activities? We do not have the beginnings of a positive answer to these questions and it seems unlikely that the future will bring answers.

I have pointed out some other implausibilities of VoC. These are briefly as follows. (i) If competence really spoke to us, why would it not use the language of the embodied theory and why would it say so little? (ii) There would be a disanalogy between the intuitions provided by the language faculty and by perceptual modules. (iii). Developmental evidence suggests that the ability to speak a language and the ability to have intuitions about the language are quite distinct, the former being acquired in early childhood, the latter, in middle childhood as part of a *general* cognitive development.

Contrary to VoC, I have argued that "a person could be competent in a language without… knowing anything about it" (2006b: 5). Hence, partly, the title of the book in which this is argued, "*Ignorance of Language*".

2.10   Conclusion

How should we find out the truth about language? The received view among Linguistic Pragmatists, indeed among philosophers of language generally, is that we should proceed by consulting intuitions about language (Sects. 2.1–2.3). I argued that this is a mistake and constitutes the *First Methodological Flaw of Linguistic Pragmatism*.

Why is it thought to be appropriate to consult intuitions? (1) A seemingly popular answer is that these intuitions are a priori. I argued that, even if we suppose that there is some a priori knowledge, we should not suppose that we have it of meanings (Sects. 2.4 and 2.5). (2) Stich has suggested that philosophers might be guided by linguistics in answering the question: competent speakers of a language derive their semantic intuitions, like syntactic ones, from linguistic rules of the language embodied in their minds (Sect. 2.6). I argued that this "voice of competence" view of our linguistic intuitions is mistaken (Sect. 2.9).

Instead, I urged that intuitions are empirical theory-laden central-processor responses to phenomena differing from other such responses only in being immediate and fairly unreflective. This being so, we should prefer the intuitions of the more expert philosophers to those of the folk. More importantly, we should seek direct, less theory-laden, evidence by studying what the intuitions are *about*, the linguistic reality itself (Sects. 2.7 and 2.8).

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Chapter 3
The Semantics-Pragmatics Distinction

3.1 Introduction

A general methodological moral to be drawn from the last chapter is that the claims of any theorist about language should not be assessed simply by consulting intuitions. These intuitions reflect empirical theories, largely folk theories, that are themselves open to question. The study of language is empirical and theoretical through and through. Applying this moral to the semantics-pragmatics dispute has two consequences.

First, we should not take the intuitively appealing notions thought to be relevant to the semantics-pragmatics distinction for granted. This includes the ordinary, and traditional Gricean, notion of *what is said*. We should not follow François Recanati in analyzing *what is said* "in conformity to the intuitions shared by those who fully understand the utterance" (2004: 14). We need to argue that the notion in question is theoretically interesting. And we should make the same theoretical demands on all the notions that Pragmatists have introduced; for example, Sperber and Wilson's notion of an *explicature* (1995), and many people's notions of *the proposition expressed*. We should look critically at the *theoretical motivation* for all of this conceptual machinery.

Second, we should not rest simply on our intuitions as evidence of what our favored notion applies to: we should not continue the practice, described by Robyn Carston, of placing "pragmatic meanings" into "what is said" on the basis of "speaker/hearer intuitions" (2004: 74). We need more direct evidence from linguistic usage.

Sections 3.2, 3.3, 3.4 and 3.5 address the matter of motivation, Sect. 3.6, the matter of evidence.[1]

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[1] Earlier versions of some of the points in this chapter can be found in my "What makes a property 'semantic'?" (2013c) and my *Ignorance of language* (2006b).

3.2   The Theoretical Motivation

What theoretical point is there to the traditional distinction between what is said and what is meant? I aim to be very careful in answering this question because the truth of the matter about the semantics-pragmatics dispute turns on the answer.

We need to start our answer by considering why we are interested in language in the first place. Our theoretical interest in language comes from our theoretical interest in thoughts and their communication.

3.2.1   Human Thoughts

It is a piece of folk wisdom that people have thoughts, which is to say that they have beliefs, desires, and other such "propositional attitudes", mental states with intentional contents or meanings. So the folk are "intentional realists". This realism is relatively uncontroversial but still some people reject it: they are *eliminativists*, taking thoughts to be posits of a discredited folk theory. Famously, this was the attitude of the behaviorists. More recently, it has been the attitude of Patricia Churchland (1986) and Paul Churchland (1989), and some others drawn to connectionism. I think that we have a very good reason for supposing that we do indeed have thoughts (2006b: 125–7). We need to ascribe them to people for at least two reasons: to explain people's behaviors and to explain the way they use others as a guide to a largely external reality.

Consider the explanation of behavior first. We observe Mark putting on a raincoat and picking up an umbrella before leaving a room. Why is he doing that? Central to our explanation is that Mark believes that it is raining. Such "intentional" explanations of "intentional" behavior are familiar and central parts of ordinary life, of history, of economics, and of the social sciences in general. They all ascribe thoughts.

Ascribing beliefs serves another remarkably valuable purpose. If a person believes that the world is such and such, and if the person is reliable, then we have good reason to believe that the world is such and such. Thus, ascribing to Mark the belief that it is raining not only helps to explain his behavior but also gives us evidence about the weather. We have a wide range of interests in learning about the world. The direct way to serve these interests is to examine the world. The indirect way is to use reliable indicators. Sometimes these indicators are "natural" ones like tree rings. Sometimes they are artifacts like thermometers. Very often they are the beliefs of others. Some belief ascriptions serve our *theoretical* interest in explanation. Many, however, are like ascriptions of desires, hopes, and so on in serving interests that are not really theoretical at all. We have the most immediate *practical* interest in finding out quite humdrum facts about the world to satisfy our needs for food, shelter, a mate, and so on. So it helps to know what is on sale at the supermarket, where there is a hotel, who is available, and so on. Ascribing beliefs is a very good way of finding out about anything at all.

This practice of ascribing thoughts is generally *successful* at serving these two purposes. Day in and day out we explain people's behaviors with these ascriptions. Almost everything we know about the world – what we learn at mother's knee, in classrooms, and from books – we get from ascribing beliefs to people and assessing them for reliability. If there really were not any thoughts, this success would be very hard to explain. We clearly have a great theoretical interest in the details of this process of explaining behavior and learning from each other.

It is a familiar piece of folk psychology that, without any involvement of language, we can sometimes use our insight into other minds and knowledge of the world to figure out what a person thinks. Thus, we came to our view that Mark believes it is raining from observing his rain-avoidance behavior. And he might deliberately communicate his belief to us, without using language, by pointing upwards meaningfully as he puts on the raincoat.

3.2.2 Animal Communication

We have a similar theoretical interest in the inner states of other organisms and their communication. We posit these states to explain behavior and to explain how one organism can communicate "information" to another. There is much debate in cognitive ethology and comparative psychology about these matters. There is no presumption that an organism's learning from another must involve a language. At one extreme, chemical detectors may sometimes do the job. At the other extreme, the idea is seriously entertained that this learning should sometimes be explained by attributing to an organism something like human insight into other minds.

So, we don't always have to posit languages to explain this learning. Still, scientists often do. What are they thus positing? What is a language? It is a system of representations or symbols that is constituted by a set of governing rules, and that a group of organisms use to communicate with each other. Most such languages are not very interesting because they simply communicate information about the animal's own current state; for example, that the animal is hungry, or wants a mate. The interesting ones are the ones known as "referential", ones that convey information about the environment. The honey bee provides a famous, and very surprising, example. The bee uses a "waggle dance" to communicate the direction and distance of a food source. Gunnison's prairie dogs provide another example: they have a system of "barks" that convey information about which sort of predator is threatening and about the characteristics of a particular predator of that sort. Clearly, the whereabouts of food is a pressing concern for the bee, the presence and nature of a predator, for the prairie dog. A bee that has returned from a food source has reliable information about the former, a prairie dog that has observed a predator, the latter. Their languages enable them to communicate this valuable information.[2]

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[2] And it is worth noting that sometimes we are confident that an animal has a language because we have *taught* it one; think of some dolphins and primates that have been taught surprisingly complex languages.

The rules of the bee's language are very likely entirely innate. The rules of the prairie dog's language seem to be partly learned and, perhaps we should say, "conventional": its alarm calls vary a bit from colony to colony; and when an experimenter used a plywood model to simulate a new sort of predator, the prairie dogs introduced a new call (Slobodchikoff 2002). In any case, whether a language used to communicate information is innate or conventional, we have a powerful theoretical interest in that language and its rules. Serious scientists work to discover the natures of the symbols in these representational systems, to discover their *meanings*.

Karl von Frisch is a notable example. He won a Nobel Prize for his discoveries about the bee's dance. I shall simplify by ignoring what he discovered about how the dance conveys the distance of the food source, attending only to what it conveys about direction. Von Frisch found the following remarkable rule:

> To convey the direction of a food source, the bee varies the angle the waggling run makes with an imaginary line running straight up and down…If you draw a line connecting the beehive and the food source, and another line connecting the hive and the spot on the horizon just beneath the sun, the angle formed by the two lines is the same as the angle of the waggling run to the imaginary vertical line. (Frank 1997: 82)

In hypothesizing that a certain behavior in members of a species involves a symbol that represents something in their language, we are supposing that the behavior was produced *because*, in some sense, it involves that symbol representing something in their language; and it is *because* of what the symbol represents in their language that other members of the species respond to the behavior as they do. And it is because of what it represents that the symbol plays its striking role in the life of an organism.

Evidence for such hypotheses is to be found, of course, in regularities in behavior. Thus, von Frisch's hypothesis was offered as an explanation of his many painstaking observations of the bee's behavior. But is it the *best* explanation? For some time it was not obvious that it was. A rival hypothesis was that a bee heading off in the direction of the food source was not responding to information communicated by a bee's dance but rather was following an odor trail left by other bees. But this rival did not stand up to ingenious experiments. The consensus now is that the hypothesis that the bee is using the language described by von Frisch is indeed the best explanation of the bee's behavior (Dyer 2002; Riley et al. 2005; Vladusich et al. 2006).[3]

3.2.3   Human Language

Return to humans. It is a truism that they have languages which they use to communicate "messages": as the folk say, "language expresses thought". This idea seems irresistible once one has accepted intentional realism, accepted that humans have thoughts (2006b: 127–8). As Fodor, Bever, and Garrett say, "there is much to

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[3] For more on this issue see Devitt 2006c (pp. 585–6) responding to Smith 2006 (p. 440–1).

be said for the old-fashioned view that speech expresses thought, and very little to be said against it" (1974: 375). So, just as the bees and the prairie dogs have representational systems used to communicate the contents of inner states to each other, so do we.[4] The evidence for this in our behavior seems overwhelming.[5]

Consider again our example of Mark and the ascription to him of the belief that it is raining. Suppose that the people present ascribe this belief on the basis of his production of the sound, /It is raining/. According to the rules of English, this sound means that it is raining at the location in question (ignoring here, for convenience, Pragmatists' worries about the "underdetermination" of such weather reports; see Sect. 10.3). If the people assume that Mark is being literal and straightforward, they will take that meaning to be the message the speaker intentionally communicates, his "speaker meaning". As a result, they have evidence of his thoughts. Taking him to be sincere in his expression, they conclude that he has a belief with that meaning (content), the belief that it is raining. In this way, language is an extraordinarily effective way of making the thoughts of others accessible to us, thoughts that otherwise would be largely inaccessible; and of making our thoughts accessible to others, often in the hope of changing their thoughts and hence their behavior. Even though, as we noted, the thoughts of others are sometimes accessible to us without language, they mostly are not.

Just as we have a powerful theoretical interest in the languages of bees and prairie dogs, we have one in human languages and their rules: we need to know about the natures of the representations used to communicate in these systems.[6]

The rules of human languages are largely conventional. Indeed, it is a truism that symbols in a language (like English) have their meanings by convention. As David Lewis points out at the beginning of his classic, *Convention*, it is a "platitude that

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[4] Just as the non-referential languages of animals (Sect. 3.2.2) have other functions that do not utilize *representational* properties, so has ours: we greet ("Hi"), cheer ("Bravo"), abuse ("Bastard"), and curse ("Shit"). I will return to this briefly in Sect. 4.2, but will mostly set it aside. The focus is always on the representational properties.

[5] Chomskians have a different view. They see a human language as an internal state not a system of external symbols that represent the world. I argued (2003, 2006b: 17–41) that this is deeply misguided. This led to some always lively and sometimes nasty exchanges: Collins 2006, Matthews 2006, Rattan 2006, Rey 2006b, and Smith 2006, responded to in Devitt 2006c; Collins 2008a, b and Rey 2008, responded to in Devitt 2008a, b, c, 2013e; Antony 2008 and Pietroski 2008, responded to in Devitt 2008c; Longworth 2009 and Slezak 2009, responded to in Devitt 2009; Ludlow 2009, responded to in Devitt 2013h; Collins 2020 and Rey 2020b, responded to in Devitt 2020b. (Rey's paper is marred by a serious misrepresentation: the allegation that my view of language arises from a methodological "commitment to Moorean commonsense" (2020b: 307). This charge is obviously baseless, as I point out, 2020b: 429–31.)

[6] Some philosophers and linguists, impressed by the great difference between a human language and the representational systems used by other animals, resist calling those systems "languages". I can see no theoretical point to this resistance. In any case, the point is merely verbal.

language is ruled by convention" (1969: 1).[7] Still I say only that the rules of human languages are "largely" conventional. The qualification is necessary for two reasons. First, if Chomsky is right then quite a lot of syntax is innate. I think he probably is right (2006b: Chap. 12). Second, the language of each human, her idiolect, is to some extent, mostly small, idiosyncratic. So the rules of her language are largely conventional but probably partly innate and partly her own work. I shall mostly ignore this qualification in this chapter.

Conventions should loom very large in our view of human language in general and in our response to Linguistic Pragmatism in particular. So I will say much more about them in Chap. 5. Meanwhile, I will get by with the following brief remarks on the origins of linguistic conventions.

On occasions like baptisms, linguistic conventions are established by people in a community stipulating that a certain form has a certain meaning and the community concurring. However, following Grice (1989) and Stephen Schiffer (1972), I think that the conventions associated with a linguistic form – a sound, an inscription, etc. – in a community typically come from the regular use in the community of that form to convey certain parts of messages. That regular use of it in utterances with a certain speaker meaning, leads, somehow or other, to that form having that meaning conventionally in the language of that community. Speaker meanings are explanatorily prior to conventional meanings.

This being so, we find evidence of linguistic conventions, hence evidence of the rules that constitute a human language, in such regularities in behavior. We should posit a conventional rule for a human language, just as we posit an innate rule for the bee's, if it provides the best explanation of the observed behavior. We shall say more about this evidential matter in Sect. 3.6

3.2.4 Our Theoretical Interest in a Language

Return to the bee's language. Our theoretical interest is not simply in the rules of this language, discovered by von Frisch, but in the representational properties of any *particular* dance that is governed by those rules. For, it is in virtue of that token dance having those properties that it plays its striking role in the lives of the bees that are present: it is because the dance has those properties that the one bee produces it and the other bees respond to it with certain flight behaviors. *And that dance*

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[7] Consider also these from the classics:

> [N]o one is able to persuade me that the correctness of names is determined by anything besides convention… No name belongs to a particular thing by nature, but only because of the rules and usages of those who establish the usage and call it by that name—Hermogenes in Plato's *Cratylus* (384c–d)
>
> A name is a spoken sound significant by convention… I say "by convention" because no name is a name naturally but only when it has become a symbol.—Aristotle *De Interpretatione* (16a.20–28)

does not get all its explanatory representational properties, all its meaning, simply from the rules of the bee's language. Thus, a dance at a certain angle to the vertical represents a particular direction of the food source *only partly* in virtue of being governed by the rule described above (Sect. 3.2.2), *only partly* in virtue of what is encoded. The dance represents that direction also partly in virtue of the actual direction of the spot on the horizon beneath the sun from the beehive at the time of the dance. Those two factors constitute the message about direction that the dance communicates; they constitute "what the bee says" by that token dance "utterance". And it is important to note that the second factor, part of what constitutes the representational property or meaning that we are theoretically interested in, is provided "in the context of utterance". The linguistic rule "demands" that this contextual "saturation" be taken into account in constituting the property that interests us; it demands that the "slot" for the direction of the spot on the horizon be filled.

Similarly with human languages, the linguistic rules for any word with an indexical or demonstrative element demand saturation in the context by reference fixing. These rules differ from those of the bees in being conventional not innate, but that is beside the point. On my view (1974, 1981a, b), the rule for a demonstrative like 'that' is that a token of it in an utterance refers to whatever object is linked to it in the appropriate causal-perceptual way. So that rule captures the convention for expressing a demonstrative part of a thought in an utterance, the convention for expressing a concept with that particular sort of meaning. It is simply in virtue of being governed by that rule that a demonstrative has its encoded linguistic meaning. A consequence is that any token that is governed by this rule has its representational property partly in virtue of the object that is as a matter of fact linked to it in that causal-perceptual way. So it has that property partly in virtue of something that is not encoded. Just as with the bee, a linguistic rule "demands" a certain contextual saturation in constituting the property of theoretical interest, the representational property that plays a striking role in the lives of the humans that are present. It is because of what the demonstrative refers to as a result of saturation that a speaker produces it and that hearers respond to it with behaviors that are likely to relate, in some way, to the object of reference.[8]

We have earlier noted (Sect. 1.1) that what is said by a human utterance, according to the tradition, is often partly constituted by whatever determines the reference of indexical or demonstrative elements. We have just seen the theoretical need for a notion with this property. The tradition has got it right here.

We have earlier noted also that, where an expression is ambiguous in the language employed by the speaker then, according to the tradition, the constitution of what is said takes account of which of the expression's linguistic meanings is in question in context (Sect. 1.1): it takes account of the speaker's "disambiguation" of the expression. This contribution to what is said is also motivated by our interest in the representational properties or meanings that play the striking role. Ambiguity

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[8] Stephen Neale has the contrary view that although an expression *type* has a meaning we have no theoretical need to ascribe a meaning, a reference, etc. to a linguistic *token*; see Sect. 6.4 and 6.5 for discussion.

arises when more than one linguistic rule governs a symbol. With human languages, this multiplicity arises from multiple conventions for the symbol; the symbol encodes more than one meaning in the language.[9] The multiplicity demands disambiguation to arrive at the representational properties that are of theoretical interest. Thus, we are interested in *which* of the conventionally possible representational properties or meanings of 'bank' is actually instantiated in a particular utterance of 'He went to a bank'; we are interested in *which* of the two conventions for 'bank' the speaker is participating in, financial institution or side of a river. A language is constituted by a system of linguistic rules. It is because speakers exploit those rules that the language is such an effective device for communicating with those who share the language. The explanatory role of a particular symbol depends on which rule has been exploited. The tradition is right again.

So, our theoretical interest in the representational properties that the utterance of a token expression has simply in virtue of the speaker's exploitation of a language motivates an interest in three sorts of properties.[10] (i) Our interest starts with encoded properties, ones that the utterance has solely as a result of the largely conventional (but probably partly innate and perhaps partly idiosyncratic) linguistic rules of the language, including rules for using certain physical forms to express certain parts of thoughts, certain concepts (Sect. 5.1). But we are interested also in two other sorts of properties. (ii) Where a symbol in an utterance is ambiguous and so covered by more than one linguistic rule, we have an interest in *which* linguistic meaning the speaker has in mind, hence which rule is in fact governing the symbol in this utterance. (iii) Where a symbol is governed by a rule that demands saturation – for example, an indexical, tense, or demonstrative – we are interested in the results of that saturation. I call any property, like (i) to (iii), that is simply a language-exploiting representational property of an utterance part of "what-is-said", thus staying close to the tradition.[11] Waiving my usual resistance to talk of propositions (1996: 210–15, 2013a), we could call this what-is-said, "the proposition said".[12]

Does any sort of property of an utterance other than (i) to (iii) go into what-is-said? Does our interest in language-exploiting representational properties motivate an interest in any other sort? I know of no reason for supposing so and will assume not.

My notion of what-is-said is (or is close to) the traditional one that the Linguistic Pragmatists oppose. The usual case for this notion rests on appeals to intuitions. I

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[9] Multiplicity could, in principle, arise from multiple innate rules for a symbol but, so far as I know, there is no example of this in nature.

[10] We doubtless have some theoretical interest also in the *non*-representational properties of our language used in greeting, cursing, and the like. But I am mostly setting them aside; see note 4.

[11] What-is-said by an utterance of an "eternal sentence" would be constituted solely by properties of type (i).

[12] This needs qualification because what-is-said using a sub-sentential is sometimes only a fragment of a proposition; see Chap. 12. Kent Bach has a more austere notion of what-is-said, discussed in Sects. 6.1, 6.2 and 6.3.

certainly think that the notion is intuitively appealing, but that is not my case for it. I claim to have provided a theoretical motivation for the notion.

So, we have theoretically motivated a notion of what-is-said by an utterance.[13] We have earlier motivated the familiar talk of "thoughts" and have identified the "message" of an utterance as the thought that the speaker intentionally conveys, "what-is-meant" by the speaker, the "speaker meaning" of the utterance (about which more in Sect. 4.2).[14] Waiving my resistance again, we could also call this, "the proposition meant". Earlier examples from Spooner and Grice in Sect. 1.1 show vividly that what-is-meant can differ dramatically from what-is-said. Spooner said many things about Aristotle in his sermon and the philosopher said that a student's English is excellent and his attendance regular: those, respectively, are the properties that these utterances have simply in virtue of the speaker's exploitation, in the context, of the conventional linguistic rules of the language. Yet what Spooner meant, the message he intended to convey, was about St. Paul. Similarly, the philosopher, and the message that the student was no good at philosophy.

If the linguistic form used by a speaker to convey a thought expresses that thought according to the conventions of the language, then the message will be what-is-said. If not, the message might be quite different from what-is-said, as Grice's example illustrates. Something important follows from that possible difference: the fact that *p* is *what-is-said* by an utterance does not entail that *p* is *meant* by the utterance (does not entail that *p* is the utterance's *message*). This is a consequence of the notions of saying and meaning that I claim to have theoretically motivated. In contrast, many theorists have notions of saying and meaning that have the consequence that saying that *p* entails meaning that *p*. I can see no theoretical motivation for this.[15]

Grice calls the message conveyed in his example a "conversational implicature". Implicatures are one way that an utterance's what-is-meant can depart from its what-is-said. Another way is through a "pragmatic modulation" of what-is-said, either an "enrichment" or an "impoverishment", to be explored later (Chaps. 10, 11 and 12). Where what-is-meant does not depart from what-is-said, I say that what-is-meant is the "literal" meaning (content) of the utterance. And where it departs by pragmatic modulation or implicature, I say that what-is-meant is not the "literal" meaning (content). For, the literal meaning of a speaker's utterance is what it means in her language. I take it that this usage is close to ordinary usage.

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[13] My what-is-said abstracts from differences between utterances that are assertions, rehearsals, questions, commands, and so on.

[14] A speaker might intend to convey more than one message. I am simplifying by setting this possibility aside.

[15] Of course, if it were not for the fact that a speaker's utterance was *intentionally produced*, she would no more have thereby *said* the proposition *p* than she would have *meant* the proposition q. So we might ordinarily say that the speaker "meant" both propositions. But this ordinary way of speaking does not provide a theoretical motivation for treating the distinct items, what-is-said and the message, as parts of the one "meaning". (There is a subtle issue about the relation of the intentional act of uttering to what-is-said; see Sect. 6.2.)

The notions of what-is-said and what-is-meant that I have explained are at the absolute center of my response to Linguistic Pragmatism.

3.3 "Semantic(s)" and "Pragmatic(s)"

The naming of what-is-said raises the vexed matter of terminology. We need to address this matter thoroughly to avoid merely verbal disputes. In addressing it, we should introduce terms only for notions that have been theoretically motivated. And, given the confusing array of terminology already in the debate, we should try to stay as close as possible to familiar uses of familiar terms. I take it that my uses of 'what-is-said' and 'the proposition said', and 'the message' and 'the proposition meant', meet these criteria. But how should we use 'semantic(s)' and 'pragmatic(s)'?

Many philosophers favor a narrow use of 'semantics' according to which it refers to the study of only the (largely) conventional linguistic rules of a language, rules that determine linguistic meanings that are encoded in the language. Related to this, these philosophers take 'semantic property' to refer only to those properties of expression *types* fully determined by those linguistic rules, properties "known" by every competent speaker of the language. There can be no objection to this usage. Our theoretical interest in what-is-said by an utterance (in the sense just explained) motivates an interest in any property that constitutes what-is-said. And our discussion has shown that 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 and their study. Indeed, von Frisch won a Nobel as a result of his interest in the rules of the bee's dance. However, we also need a term for the study of what-is-said itself. And we need a term for *all* the properties that constitute it, properties of types (i) to (iii), not just for the ones determined by the rules of the language, properties of type (i). 'Semantics' and 'semantic' seem, respectively, ideal for these jobs too. And the property of *being what-is-said*, constituted by (i) to (iii), must then itself be semantic. So, given what I said above about 'literal', what-is-said is both the literal and semantic meaning (content) of an utterance on my usage. Semantics in my wide sense includes semantics in the narrow sense because the conventional properties of an expression type are among the ones that constitute what-is-said by any particular use of that type in an utterance.

Kent Bach is one who favors the narrow uses of 'semantics' and 'semantic' and rather deplores their wide uses "for features pertaining to truth and reference" (1994: 133 n. 9). Nathan Salmon is another, and launches a polemic against "the speech-act centered conception" which takes "semantics as concerned with what a speaker *says* or *asserts* in uttering a declarative sentence" (2005: 321). In response, I emphasize first that this *is* only a verbal matter and hence not worthy of much

passion.[16] Second, there is a custom, as both Bach and Salmon acknowledge, of using these terms widely. Third, this wide use relates the semantics of language nicely to what Jerry Fodor has wittily called, "psychosemantics" (1987), the study of thought meanings (contents). For, those meanings are truth-referential. In any case, I use the terms widely unless they are accompanied by the qualification 'narrow'.

I call the *properties* that constitute what-is-said, "semantic". So it is appropriate to also call "semantic" the *processes* in a speaker that are responsible for utterances having those semantic properties, processes of convention participation and reference fixing (saturation, slot-filling). However, there seems to be little occasion to do so.

Finally, we must turn to the *really* vexed matter of 'pragmatics' and 'pragmatic'. These terms have distressingly many uses in the literature.[17] One common way to use 'pragmatics' is for the study of communication or, as it is usually called, "interpretation", the study of the way that a hearer comes to understand the message conveyed by a speaker's utterance: "the goal of pragmatics is to explain how the hearer's task…can be carried out" (Sperber and Wilson 1995: 179; see also: 10). And the processes that are thus studied are called 'pragmatic'. I go along with this usage. And I take it that the terms so used also cover processes in the *speaker* of choosing among ways to communicate a message (not to be confused with above processes in the speaker that constitute the semantic properties of the way chosen (Sect. 4.2.3)).

A word of warning about "interpretation". Like many English nouns this one is ambiguous between a process and a product. So it can refer to the process of interpreting the meaning of an utterance or it can refer to the meaning assigned by that process. In calling the theory of interpretation "pragmatics", as I have just done, I am taking it to be concerned with the process of interpreting. If we took it to be concerned with the meaning assigned by the process then it would be akin to semantics, a very different matter. Because of this ambiguity, I do not talk of the theory or study "of interpretation".

So we have defined a use of 'pragmatic' to describe certain processes, the ones involved in interpreting communications. And we have defined a use of 'pragmatics' to describe the study of those processes. Understood it this way, pragmatics and what is pragmatic are *obviously* very different from semantics and what is semantic: pragmatics is about certain *processes* (mostly) in a hearer, semantics is about certain *properties* of an utterance. Of course, those semantic properties play a big role in the pragmatic processes: the hearer's grasp of what-is-said is a key part, at least, of her process of figuring out the message communicated (Sect. 3.4). Still, understood in

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[16] Salmon is used to hearing this response and is not impressed: "'It's all just terminology' is the last refuge of the speech-act centered conception" (2005: 327). Kepa Korta and John Perry's view (2007: 98–9) that "something has gone awry" in Cappelen and Lepore's account of "semantic minimalism" (2005: 143–5) seems to rest simply on an insistence that 'semantics' be used narrowly.

[17] This is nicely demonstrated by Bach 1999 and Ezcurdia and Stainton 2012.

this way, there is no way in which semantics and pragmatics are *rivals*. What then are we to make of the pragmatics versus semantics dispute?

The answer is that it is common to use 'pragmatic' also to describe *properties of an utterance that are not semantic*, in some preferred sense, and yet contribute to a message conveyed; for example, Gricean implicatures and certain enrichments and impoverishments. And it is common to use 'pragmatics' also to describe the study of these nonsemantic properties. So, understood in this way, "pragmatics" and "semantics" offer different accounts of the properties of utterances and *are* rivals of some sort (see, for example, Bach 1999; Carston 2008). It is only when 'pragmatics' is understood in this way that the dispute over where to draw the line between pragmatics and semantics is intelligible. For want of better terms, I shall reluctantly go along with this common usage, *with "nonsemantic" understood in my sense*, of course.

So 'pragmatics', as commonly used and as used by me, is ambiguous, referring sometimes to the study of communication and sometimes to the study of these pragmatic properties of utterances.[18] This ambiguity does not get the attention it deserves.[19]

Many use 'pragmatic' to describe more properties of an utterance than the ones that are non-semantic in my sense: it is used to describe all the properties of what-is-said other than conventional properties of expression types, other than encoded *narrow*-semantic properties (e.g., Sperber and Wilson 1995: 36, 217; Recanati 2004: 8; Carston 2008). Using the term in this way to describe what we might call "wide-pragmatic" properties is unfortunate because it conceals the theoretically crucial distinction between two sorts of nonconventional properties of an utterance that are determined in context: those that constitute what-is-said and those that constitute what-is-meant *but not said*. For the former properties, but not the latter, are the ones the utterance has simply in virtue of the speaker's exploitation of her language. They are ones that make the utterance an instance of her linguistic system just as, in the case of a bee's dance, its property of representing the direction of the food source, a property it has partly in virtue of the actual direction from the beehive of the spot on the horizon beneath the sun, makes the dance an instance of the bee's linguistic system. So, in my usage, pragmatic properties are not part of what-is-said.

It is important to note that although some properties of an utterance may be pragmatic in one sense, *all* the hearer's processes of understanding the utterance's message are pragmatic in the other sense, whether they involve grasping pragmatic properties or semantic properties (what-is-said). And all of these processes are studied by pragmatics as the theory of communication (and none of them by pragmatics as the theory of pragmatic properties).

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[18] Note that the common description of pragmatics as "the science of language in use" does not distinguish the two meanings.

[19] This is presumably related somehow to the confusion of the metaphysics of meaning with the epistemology of interpretation that I discuss in Chap. 7.

3.4   Communication

It has always been clear that we can use our insight into other minds to successfully convey messages without using language; Mark's pointing upwards meaningfully as he puts on the raincoat is an example (Sect. 3.2.1). But we are interested in communication that involves a language. With such communication, the representational properties of symbols in the language should be center stage in a theory of communication among the organisms. For, it is largely, even if not entirely, because symbols of the language have those properties, and because the organisms have the capacity to exploit those properties in sending and receiving messages, that communication occurs.[20] So pragmatics, as the theory of communication, must start with what Stanley and Szabó call the "descriptive" problem of semantics, the assigning of "semantic values" to expressions of the language in question (2000a: 223). (They contrast the descriptive problem with the "foundational" one concerned with in virtue of what expressions have those values.). But the theory of communications requires much more than this description of the representational properties of the symbols in that language.

Consider communication among the bees, for example. We start with the theory of their language provided by von Frisch, together with the obvious fact that the bees are competent in that language. But then we need to know how bees can use that competence to send and receive particular messages. How does a sender bee use its competence to turn its "knowledge" of the direction of the food, and of the direction of the spot on the horizon beneath the sun that saturates its dance, into a dance at an angle that, according to the rules of its language, conveys the direction of the food? How does a receiver bee use its competence to turn its "knowledge" of that angle, and of the saturating direction of the spot on the horizon, into the message about where the food is? We know sadly little of the answers.

Consider next human communication using the demonstrative 'that'. We start with the theory of this word: it refers to an object that the speaker has in mind, determined by a causal-perceptual link, that saturates the demonstrative (Sect. 3.2.3). Communication with it then involves processes analogous to those in bee communication. The speaker exercises her competence with 'that' to send a message that refers to the object she has in mind. The hearer's competence enables him to decode 'that' as a demonstrative and his mind-reading skill discovers which object the speaker has in mind that fills the slot in the demonstrative. But human communication obviously involves another process. Suppose the utterance is "That is a bank", and consider the processes in the hearer. After using his linguistic competence to decode the utterance, he has to use his mind-reading skills not only to discover what object the speaker has in mind but to decide which of the two meanings that 'bank'

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[20] See Pagin and Pelletier (2007: 35) for a nice statement of this sort of view.

encodes is part of the message. His knowledge of the language no more disambiguates than it fixes reference.[21]

Communication among humans can involve even more, of course. In speakers it can involve a process of pragmatic enrichment or impoverishment and/or a process of implicature. And in hearers it can involve the mind-reading process of detecting these, so admirably demonstrated by the Pragmatists. In such a case, a hearer must use his insight into other minds to infer a message that differs from what-is-said; thus, in Grice's example, the philosopher's audience grasped the message that the student was not good at philosophy from an utterance that did not use any conventional form for expressing that message but rather said something entirely different. But, to emphasize, pragmatics, *as the theory of this communication*, starts with the semantics of the language, with what-is-said, but goes much further. And whereas the focus of the semantics is on the speaker because the speaker determines both what-is-said and the message, the focus of pragmatics, as the theory of communication, is on the hearer because the task of receiving a message is more difficult than the task of sending one.

3.5   The Semantics-Pragmatics Dispute

My methodology for tackling the semantics-pragmatics dispute starts from the view that a language is a representational system posited to explain communication.[22] From this start I provide a theoretical motivation for a sharp distinction between two sorts of properties of an utterance. On the one hand there are its (wide-)semantic properties. These are properties that an utterance has simply in virtue of the speaker's exploitation of the conventional rules of her language. They include, but are not limited to, an utterance's encoded narrow-semantic properties, the linguistic meanings that are fully determined by the rules governing the symbol type of which the utterance's symbol is a token. Semantic properties constitute what-is-said, the proposition said. On the other hand, there are the utterance's other properties which, perhaps along with semantic ones, contribute to the message conveyed, the proposition meant. These are the utterance's pragmatic properties.[23]

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

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[21] Relevance theorists take the traditional view of communication to be "the *code model*" according to which "communication is achieved by encoding and decoding messages" (Sperber and Wilson 1995: 2). This is a bit misleading because the tradition had surely noted the obvious fact that understanding a message requires not just decoding but also reference fixing and disambiguation.

[22] And to explain some non-communicative uses of language; see Sect. 4.2.

[23] As Manuel García-Carpintero says: "the semantics/pragmatics divide is in my view ultimately about which meaning-properties are constitutive of natural languages and which are not" (2006: 43).

utterance. Other factors may also contribute to conveying the message but these are not the concern of semantics. So the key issue for the semantics-pragmatics dispute is the nature of those linguistic representational properties. The symbols have those properties largely in virtue of being part of a representational system of conventional rules. So the key issue comes down to: What are the conventions that established the system? How do we answer this question? We shall consider this in the next section.

I go along with the common view that there can be conversational implicatures and pragmatic modulations, the latter consisting of enrichments/expansions and impoverishments of my what-is-said. Implicatures and modulations are two ways that the speaker-meaning of an utterance can depart pragmatically from its semantic meaning. I am not committed to these two ways being sharply distinct nor to there being no other ways; perhaps, for example, we should also include "implicitures" (Bach 1994: 126). Differences among these pragmatic ways are not significant to my theses and so I will often lump them together under the term "pragmatic modifications".

So, in effect, I have a three-way distinction among the possible "meaning" properties of an utterance: an encoded linguistic (largely conventional) meaning of its sentence type (EC); a what-is-said (WIS), arising from (i) encoding, (ii) disambiguation, (iii) linguistically demanded saturation or slot filling; a pragmatic modification (PM), perhaps a modulated what-is-said or an implicature. EC and WIS are semantic properties; PM are at least partly pragmatic.

From this perspective, what is the disagreement with the Linguistic Pragmatists over the context relativity of content, over the dependence of the truth conditions of an utterance on context? Let me start by emphasizing what it is *not*. First, variations in content arising from well-known disambiguation and linguistically demanded saturation by reference fixing are obviously not the issue. And, it is important to note, no more are variations arising from *novel* implicatures or modulations, variations in the uses of language made "on the spur of the moment", "on the fly". That there are novel implicatures that must be treated as pragmatic phenomena has been famously demonstrated by Grice with examples like the philosopher's reference letter (Sect. 1.1).[24] But more needs to be said about novel modulations.

We should all accept, and surely the tradition did, that there can be novel modulations of conventional meanings. Utterances are often elliptical: a *more precise* message than the truth-conditional what-is-said is conveyed in the context. This acknowledges that it is sometimes ponderous and boring to convey the precise message by fully exploiting the available linguistic conventions. Carston has a nice example: 'Ann wants to meet a bachelor' meaning that she wants to meet a bachelor who is "heterosexual, youngish, interested in marriage, etc." (2002: 27). And Ingrid Falkum has another: A biology teacher talking of feces says, 'Rabbit is smaller than hare' (2015: 88). Sometimes the only available conventions determine a meaning

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[24] These novel implicatures are "particularized conversational implicatures". What about "generalized" ones? These are more controversial and I'm dubious that there are any such pragmatic phenomena; see later (Sects. 8.4, 9.4, 11.2.6).

that is vaguer than the desired message. So a somewhat vague what-is-said is spontaneously *enriched* in context into a more precise message; the speaker conveys the precise proposition she means with the help of the imprecise proposition she expresses. This is a plausible story for a novel nominal compound like Recanati's 'burglar nightmare' (2004: 7). And pragmatic modulations can be *impoverishments*: the proposition meant is *less* precise than the proposition said; for example, 'Her face is oblong' (Carston 2002: 27). And consider Recanati's "The ATM swallowed my credit card" (2004: 26). This use of 'swallow' is probably conventionalized now but initially it would have been a pragmatic impoverishment of the conventional meaning. Now one might argue that novel uses of these sorts are more widespread than has been traditionally thought. That is an empirical issue that does not seem theoretically interesting because, however widespread these phenomena, they obviously must be explained pragmatically.

The interesting issue is whether there are pragmatic variations in content from context to context *other than* these novel ones. According to the doctrine that I aim to defend, there generally are not. I sum up this view as follows:

> **SEM**: Setting aside novel uses of language, what-is-said (in my sense) is typically the truth conditional speaker-meaning (content) of an utterance, the message conveyed. That message is typically a WIS-meaning.

Linguistic Pragmatists think that this is quite wrong. They have used a range of striking examples, illustrated by utterances (1) to (8) in Sect. 1.1, to mount a formidable challenge to traditional views like SEM. These examples are mostly not of novel ways of conveying messages but rather of *standard regular* ways; thus 'everyone' is regularly used to convey a message about a certain group in mind, 'lion', to convey courage. Many Pragmatists use these examples to argue for extreme "semantic underdetermination" and "truth-conditional pragmatics", views that we can sum up as follows:

> **PRAG**: Even setting aside novel uses of language, the truth conditional speaker-meaning (content) of an utterance, the message conveyed, is seldom, perhaps never, constituted solely by what-is-said (in my sense), a WIS-meaning. The message is always, or almost always, the result of pragmatic modification, a PM-meaning.[25]

PRAG is an example of the somewhat vaguely defined Linguistic Pragmatism that I introduced in Sect. 1.1. It is held by those who are often called "linguistic contextualists":

> According to contextualism, the sort of content which utterances have…can never be fully encoded into a sentence; hence it will never be the case that the sentence itself expresses that content in virtue solely of the conventions of the language. Sentences, by themselves, do not have determinate contents. (Recanati 2003: 194)

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[25] According to Falkum "the distinction between linguistic semantics and pragmatics is seen as corresponding to different processes involved in utterance comprehension" (2015: 89). The word "corresponding" is important here. For, categorizing utterances as having PM-meanings rather than the traditional WIS-meanings does indeed demand a corresponding difference in the process of understanding the utterances: "most of the interpretive work is performed…by pragmatic processes operating over underspecified semantic representations" (p. 89).

Now, of course, everyone accepts that encoded conventional meaning alone does not usually determine truth-conditional content: reference determination and disambiguation are also needed. But contextualists hold that much more is needed to get the content, as Anne Bezuidenhout makes very clear:

> meaning underdetermines truth-conditions. What is expressed by the utterance of a sentence in a context goes beyond what is encoded in the sentence itself. Truth-conditional content depends on an indefinite number of unstated background assumptions, not all of which can be made explicit. A change in background assumptions can change truth-conditions, even bracketing disambiguation and reference assignment….contextualists claim that there is a gap between sentence meaning and what is asserted, and that this gap can never be closed….the radical context-dependence of what is said (2002: 105)

This passage presents another striking aspect of contextualism and its truth-conditional pragmatics: sentence meaning can be supplemented in an *indefinite* number of ways to yield truth conditions. As a result, this supplementation can yield an *indefinite* number of different truth conditions.

As we shall see (Chap. 6), Bach and Neale both reject SEM, though in very different ways. Bach is not usually called a contextualist but I think that we should see him as subscribing to PRAG. Neale is a very different story.

So I conceive of the semantics-pragmatics issue as the SEM-PRAG issue. This conceptions rests on my semantic notion of what-is-said that I motivated in Sect. 3.2, WIS. There are many other notions of what-is-said in the literature, as noted (Sect. 1.1). In particular, contextualists' emphasis on the context-relativity of meaning leads them to partly pragmatic notions that, in my terms, are notions of a *pragmatically modulated* what-is-said. With these notions goes their belief in "the radical context-dependence of what is said", as Bezuidenhout puts it above. These different notions of what-is-said are motivated by substantive theoretical differences. To avoid "merely verbal" disagreements it is, of course, important to focus on those substantive differences not on the different notions.

In Chaps. 6, 7, 8, 9, 10, 11 and 12 I confront the Pragmatist challenge in general and PRAG in particular by arguing, controversially, that the truth-conditional meaning communicated by the Pragmatist examples is typically constituted only by properties of sorts (i) to (iii); it is a WIS-meaning after all, a literal meaning. There are more of such properties than we have previously acknowledged: much more of the content of messages should be put into the convention-governed what-is-said– into semantics – than has been customary. The Pragmatists have made the wrong response to their examples, treating far too many as conveying PM-meanings instead of WIS-meanings. That is my argument for SEM.

The position on the semantics-pragmatics issue that I am urging, in the spirit of the tradition, goes against Pragmatist theses of extreme "semantic underdetermination" and "truth-conditional pragmatics" (which I have attempted to capture with PRAG). We all accept, of course, that there has to be disambiguation and reference fixing in context to get from conventional linguistic meanings to messages. And we all think, or should think, that messages can be conveyed in novel ways. But Pragmatists think, and indeed must think if their thesis is to be interesting, that there is much more semantic underdetermination than this. A consequence of my view is

that there are no interestingly true theses of semantic underdetermination and truth-conditional pragmatics. Novelty aside, semantic properties alone typically constitute the message.

So much for the matter of theoretical motivation. I turn now to the matter of evidence.

3.6   The Evidence

In Chap. 2 I argued that the primary evidence for our theories of language should not be the indirect evidence of metalinguistics intuitions but rather the direct evidence provided by linguistic reality itself. Now that linguistic reality, consisting of linguistic rules largely established by conventions, is revealed in regularities of behavior, the regularities of using certain forms for certain speaker meanings. So, we find evidence of those linguistic rules by finding evidence of such regularities. Where we find a regularity we must consider whether it is best explained by supposing that there is a linguistic, probably conventional, rule of so using the expression. If that supposition is indeed the best explanation then we should conclude that the speaker meaning in question is a semantic meaning. Regularities provide evidence of the conventions which establish the linguistic rules that, along with reference fixing and disambiguation, constitute the semantic properties of utterances.

How are we to gather this evidence? I have urged elsewhere (2011b, 2012a, 2015a) that we should get inspiration from linguistics. Even though the received methodology in linguistics, like in philosophy, is dominated by attention to the role of intuitions – far too much so, in my view (2006a: 486–8, 2006b: 98–100) – linguistics is importantly different from philosophy in that linguists often acknowledges the role of usage as a source of evidence. In particular, they seek evidence in the corpus and in elicited production.[26] I shall consider these in turn.

The Corpus   The corpus is the vast mass of linguistic sounds and inscriptions that competent speakers produce as they go about their ordinary business without prompting from experimenters. We can seek evidence in this corpus of the regularities that may be explained by linguistic rules. And it is important to note that a vast amount of such evidence has been gathered by lexicographers and recorded in dictionaries. Philosophers are rightly scornful of the idea that we can simple take our theory of *meaning* from dictionaries, but what we can take from them is valuable evidence of how expressions are *regularly used*. Such evidence of regularities can be gathered by scientific study, and doubtless often has been, but it can also be gathered informally by simply observing our language. And that, I suppose, is how dictionaries have mostly been composed. The significance of the informal gathering of evidence of usage should not be underestimated. Thus, any one of us, on the basis

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[26]Also in reaction time studies, eye tracking, and electromagnetic brain potentials. Perhaps philosophers can get inspiration from these experiments too.

of a lifetime of linguistic experience, can easily conclude "from our armchairs" that definite descriptions are regularly used "referentially". I shall make much use of this sort of evidence in Chaps. 8, 9, 10, 11 and 12.

Elicited Production Fortunately, we don't have to rely on the corpus for direct evidence in usage: we can *induce* usage from competent speakers in experimental situations. Consider this description of "the technique of *elicited production*" in linguistics:

> This technique involves children in a game, typically one in which children pose questions to a puppet. The game orchestrates experimental situations that are designed to be uniquely felicitous for production of the target structure. In this way, children are called on to produce structures that might otherwise not appear in their spontaneous speech. (Thornton 1995, 140)

Clearly much direct evidence could be gathered in this way. However, contriving appropriate situations in an experiment is likely to be a laborious business.

I proposed an easier technique of elicited production for linguistics. Instead of *constructing* situations to see what people say and understand in those situations, 'we can *describe* situations and ask people what they would say or understand in those situations' (2006a: 487, 2006b: 99). Note that this method is not the common one, criticized in Chap. 2, of prompting *metalinguistic intuitions* about described situations, yielding indirect evidence about language. Rather it is prompting *linguistic usage* in described situations, yielding direct evidence about language.[27] I have suggested (2011b) that this method could provide a rich source of evidence about *reference* and some evidence of this sort about the reference of proper names (Domaneschi et al. 2017; Devitt and Porot 2018) and "natural" kind terms (Devitt and Porter 2021) has recently been produced. I think that this is the way forward in experimental semantics generally (2012a: 29–30).

What we are looking for in the corpus and elicited production is evidence of the regular association of an expression and a speaker meaning. How do we tell when there is such an association? Judging what expression is used is easy: we can simply observe it. But what about judging the speaker meaning, the message?

It is a striking fact that theorists, however much they otherwise disagree, agree about the message in the controversial cases; the dispute is over how to account for the message that they agree on. Thus, consider the Pragmatist examples (1) to (8) in Sect. 1.1. All theorists agree that a speaker uttering "I've had breakfast" is likely to mean that she has had breakfast this morning; that by saying "You are not going to die" to a child in a certain context she means that the child will not die from that minor cut; that in uttering "It's raining" the speaker means that it is raining in a certain location; that, in context, "The table is covered with books" is being used to affirm that a certain table is so covered; that "Everybody went to Paris" conveys the

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[27] I sum up my discussion of linguistic evidence: "the main evidence for grammars is not found in the intuitions of ordinary speakers but rather in a combination of the corpus, the evidence of what we would say and understand, and the intuitions of linguists" (2006a: 488, 2006b: 100).

message that everyone in a certain group went to Paris; that "John is a lion" means that he is courageous; that "The party was fun until the suits arrived" is about the arrival of the executives; and that "The road was covered with rabbit" is about the remains of rabbits.

Why is there such agreement over the messages of utterances? The agreement simply reflects the fact that theorists, like everyone else, are highly competent at understanding utterances. A large part of this competence though, as the Pragmatists insist, certainly not all, is constituted by our competence in the language of the utterance. So, if there is any doubt about a theorist's view of the message we can test that view against the opinion of ordinary competent speakers.

These intuitions of ordinary speakers about the message differ from those we were dubious of in Sect. 2.8, metalinguistic intuitions about reference, ambiguity, and the like. Just as the non-central vision system provides the central processor with "the immediate and main basis for judging what is seen" so too does the non-central language system provide the central processor with "the immediate and main basis…for judging 'the message'" (2006a: 500–1, 2006b: 112, 2010c: 850–2, 854). These intuitions, unlike the metalinguistic intuitions, really are largely produced by linguistic competence and should indeed be reliable.

Another way to get evidence of an utterance's message is to ask the speaker. The paraphrase she gives us should be a reliable guide to the message (which is not to say, of course, that the paraphrase should be taken as *synonymous with* the utterance). If there is still doubt about the message, we could look for behavioral evidence of it.

In sum, to establish that expression *E* has linguistic meaning *M* we need evidence, first, that a speaker using a token *E* means *M* by it. This evidence is not hard to come by. Second, we need evidence that such uses of *E* are regular. Finally, it needs to be the case that the best explanation of tokens of *E* being regularly so used is that *E* means *M*.

So the case that *E* means *M* rests ultimately on an "inference to the best explanation", what is often called, following Peirce, an "abduction". How should we judge a good abduction? We are a long way from having an answer to this crucial epistemological question. Still here a two criteria. They do not take us far but they are, I hope, both helpful and relatively uncontroversial.[28]

Criterion (A) of a good abduction is that it involves *a good explanation*. As is well known, it is hard to say much about what makes an explanation good. But goodness requires at least two things. (A1) The explanation must be plausible given what else we know, it must be *plausible relative to background knowledge*. A corollary of this requirement is that if none of our candidate explanations meet this requirement, then we should *not* accept an abduction involving the best of this implausible bunch. Rather than such a rush to judgment we should follow a course of action the virtues of which are sadly underappreciated: we should *suspend judgment and keep looking*. (A2) Next, a good explanation must have *an appropriate*

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[28] I draw on my 2010a: 94.

level of detail: if the explanation posits *x* as the cause of *y*, it must say enough about *the mechanism by which x* causes *y* to not leave this mysterious; a wave of the hand is not sufficient. If we do not have the details we need, we should, once again, suspend judgment.

Criterion (B) of a good abduction is that the explanation it features must be better than any *actual* alternative or even any alternative that is *likely*, given what we already know. Attention to likely alternatives is important. Even if the featured explanation does not face any worked-out alternatives, we may have some ideas for alternatives that, given our background knowledge, seem promising. Until those ideas have been explored sufficiently to be set aside, we should suspend judgment on the abduction.

Both (A) and (B) emphasize the role of background knowledge in judging an abduction. This turns out to be important to the SEM-PRAG issue.

It is perhaps striking that I have made no mention of psycholinguistic evidence for the presence or absence of the linguistic rules that constitute a language. In principle, evidence of the psychological processes involved in speakers and hearers exploiting the language, or of the different processes associated with pragmatic modulations and implicatures, could be helpful. But, in fact, as we shall see in Chap. 11, we lack any such evidence in the controversial cases. And, we should note, such evidence played no role in decisions about the languages of bees and prairie dogs. The promising place to find evidence for an organism's language is in its behavior not its "mind".

3.7 Conclusion

How should we discover the truth about language? In answering the question, we started by considering what a language is and why we sometimes suppose that organisms have one. A language is a representational system posited to explain communication. This gives a powerful theoretical interest in the natures of the representations yielded by this system. For human languages, I argued these natures are constituted by three sorts of properties, briefly, those arising from convention, disambiguation, and linguistically demanded saturation (slot filling): these three sorts of properties constitute "what-is-said" by an utterance, "the proposition said". This amounts to a theoretical motivation for a fairly traditional Gricean notion of *what is said* (Sect. 3.2).

Evidence of the properties that constitute what-is-said is to be found in behavior. In particular, the regular use of an expression with a certain speaker meaning provides evidence of a convention of so using it and hence of what makes up the language (Sect. 3.6).

The theoretical motivation provides a sharp distinction between two sorts of properties of a linguistic utterance: its "semantic" properties which constitute what-is-said and the proposition said; its other, "pragmatic", properties which, perhaps

along with semantic ones, contribute to "the message" conveyed, "the proposition meant" (Sect. 3.3).

The "semantic" theory of the representational properties of symbols in a language is one thing, the "pragmatic" theory of communication among users of that language, another. Yet the semantic theory should be center stage for the pragmatic one. 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. 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 pragmatic theory is on the hearer because the task of receiving a message is more difficult than the task of sending one (Sect. 3.4).

I embrace a view, "SEM", that is in the spirit of the tradition: absent a novel use of language, what is said by an utterance, in the sense I hope to have motivated, is typically the truth-conditional content communicated. I have foreshadowed an argument that confronts the challenge that the Linguistic Pragmatists have posed to this view. The argument is that the Pragmatists' striking examples typically exemplify properties arising from convention, disambiguation, and linguistically demanded saturation and so go into constituting my semantic what-is-said. This counts against the view, "PRAG", and popular Pragmatist theses of "semantic underdetermination" and "truth-conditional pragmatics" (Sect. 3.5).

I confront the Pragmatist challenge in Chaps. 7, 8, 9, 10, 11 and 12. Before that I need to develop further the view of language that underlies SEM. The view presented in the present chapter has been very Gricean. I have urged a roughly Gricean view of what-is-said. I operate with a Gricean distinction between speaker meaning and literal (or semantic) meaning and embrace the fundamental Gricean idea that linguistic meaning is to be explained in terms of speaker meaning; so speaker meaning is explanatorily prior. But there are some important respects in which my view of language is not Gricean. I will bring these out in the next chapter. I distance myself from some Gricean ideas about speaker meaning and the role of intentions in semantics. In Chap. 5, I say more about linguistic conventions and their significance for language. In Chap. 6, I look critically at the views on what-is-said of the prominent Gricean Stephen Neale, and of the influential Kent Bach, thereby throwing more light on my own view on this, and thus on language in general.

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Chapter 4
Speaker Meanings and Intentions

There is much talk of intentions in semantics. Intentions to refer are said to determine reference. The central idea of Gricean "intention-based-semantics" is that speaker meanings are constituted by the speakers' communicative intentions. It is common, particularly among Griceans, to believe that there is a certain constraint on what a speaker can intend to communicate by an utterance. I think that all of this is mistaken.[1]

4.1 Intending to Refer

Stephen says, "Grice is great"; or, pointing to Grice, "He is great" or "That man is great". Who does Stephen refer to with these "singular referring terms"? One answer that is very popular among philosophers is that he refers to "whoever he intends to refer to" (e.g., Schiffer 1972, 1981; Kaplan 1989b; Bach 2001; Neale 2016). Another answer that has some popularity among philosophers is that he refers to "the particular person he has in mind" (e.g., Donnellan 1966; Devitt 1974; Almog 2012). Both answers are initially appealing because they are in line with what the folk might say in answer to our question. I think that having-in-mind is indeed a helpful *starting point* for a good answer, but no more than that. Intending-to-refer is not even that. I shall begin by arguing for this and then make three more objections to the intending-to-refer hypothesis. I conclude that it should have no place at all in a theory of language.

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[1] This chapter draws on my 2020c.

4.1.1   Objection 1: Implausible Starting Point

What is wrong with intending-to-refer as a starting point? Think of what is required for someone *A*-ing to intend to *V* to *x*; for example, what is required for someone leaving her apartment to intend to bicycle to the Met. This intention is a thought, a "propositional attitude", about bicycling and so contains the concept *bicycling*. Similarly, for a speaker uttering a singular referring term in a sentence to *literally* intend to refer to *x* with the term. She must have a thought containing the concept *reference*. So, according to the intending-to-refer hypothesis, taken literally, she can't refer without thinking about reference! There is no reason to believe this, no reason to believe that any expression of a thought about something *must* be accompanied by a further thought about reference. That is far too intellectualized a picture of referring and is psychologically implausible (Speaks 2016: 304–5).[2] Referring is a cognitive *skill*, mere *know-how*; or so I have argued (1981a, 1996, 2006b, 2011c). One could refer without even *having* a concept of reference (1981a: 97).

Perhaps the talk of referential intentions should not be taken literally but seen as just a way of saying that referring is intention*al*. And it surely is: it's an *action* like walking, kissing, etc.[3] But we clearly make no theoretical progress in explaining reference by saying that the speaker refers to *x* in virtue of *intentionally* referring to *x*: referring *is* intentional.

This is not to deny that any normal adult speaker with a minimal education could probably tell you after the event what she was "talking about". That's a very easy bit of semantic knowledge. Yet even that easy bit is surely beyond the capacity of many organisms that nonetheless refer: for example, humans at the age of three[4]; bees, prairie dogs, vervet monkeys, and other nonhuman species that have what cognitive ethologists call "referential" languages (Sect. 3.2.2). In the face of this, one could of course restrict the intending-to-refer hypothesis to adult humans, but that seems *ad hoc*. In any case, to repeat, it is not plausible that such metalinguistic intentions are constitutive of anyone's referring.

Having-in-mind does not have this problem as a starting point. For, having *x* in mind simply requires that the concept the speaker is expressing refers to *x*; the term used refers to x in virtue of being produced to express a concept that refers to *x*.[5]

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[2] Some may be tempted to clutch at the popular weasel word 'tacit' at this point. But, absent an account of what it is to *tacitly* intend to refer, this move yields no explanation.

[3] According to what Michael Bratman calls "the Simple View" (1984), a behavior is intentional in virtue of having a certain intention as its immediate cause. I am following Bratman in presuming that this view is wrong. If it is not wrong, then we would need some other notion than *intentional*, a notion with less "intellectual" commitment, to distinguish these behaviors from mere bodily movements. Perhaps we need that anyway given that the distinction is needed for the behaviors of (all?) other animals.

[4] Developmental evidence suggests that the capacity to have metalinguistic thoughts comes later, in middle childhood; e.g. Hakes 1980.

[5] So my starting point for singular reference is similar to Radulescu's (2019) finishing point, "Expressive Intentionalism", for demonstratives in particular. But whereas Radulescu thinks that the term's reference is fixed by the speaker's *intention to express* "the corresponding demonstrative element of the thought", I think it is fixed simply by the speaker *(intentionally) expressing* that element; his view is too intellectualized. For discussion of some famous puzzles about demonstrative reference, including Kaplan's Carnap-Agnew, see Sect. 12.3.

That is the first objection to including intending-to-refer in a theory of language. There are three more objections, two of which apply also to having-in-mind.

4.1.2   Objection 2: Incomplete

Even if intending-to-refer was a plausible starting point it would immediately raise the question: *In virtue of what* did the speaker intend to refer to *x* in using the singular term rather than to *y* or *z*? Without an answer to this question, the intending-to-refer hypothesis could not advance our theory of reference significantly: the explanatory problem has simply been moved a short distance from the reference of the utterance to the reference of the intention.

Now, of course, the having-in-mind hypothesis raises an analogous question and so is similarly incomplete: In virtue of what did the speaker have *x* in mind? In virtue of what does the concept, the part of the thought, that causes the use of the term refer to *x*. What reality is this somewhat vague bit of folk talk getting at?

As noted (Sect. 3.2.4), I have argued that, for the sort of having-in-mind and singular reference exemplified in Stephen's utterances, the reality is causal (1974, 1981a, b).[6] A part of Stephen's thought determines that he had x in mind (in this sense) in virtue of standing in a certain sort of causal relation to *x*, a relation involving the perceptual grounding of someone's thought in *x* and, perhaps, reference borrowings (Kripke's wonderful insight).[7] Explanations of the reference of demonstratives, pronouns, and names will appeal to these causal-perceptual relations in somewhat different ways, of course. (Note that it follows from this that the reference of the term is determined by *a mental state of the speaker*. So, contrary to the tradition arising from formal semantics – see, e.g., David Lewis (1983) – the "indices" of the context external to the speaker's mind play a reference-determining role only to the extent that relations to that context partly *constitute* that mental state.)

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[6] I was clear from the start that the vague talk of "having in mind" is but a "stepping stone" to a causal theory of reference, not something that features in the theory (1974; 202). Still, Andrea Bianchi's criticisms (2020b) have made me realize that I should have been more careful in my handling of this stepping stone (2020b: 394). I should not have claimed, early, to offer an "analysis" (1974: 202) of "having in mind" and, late, "an explanation – better, an explication – of this somewhat vague folk talk" in causal terms (2015c: 111). The folk talk arguably covers more than the causal relation we want; see Sect 4.1.4 below on how it can mislead. So my causal explanation was, in reality, an explanation of a restricted, "technical", notion of having-in-mind.

[7] (a) Joseph Almog has recently proposed a similar causal explanation of having in mind (2012: 177, 180–2), attributing it to Donnellan without evidence; for discussion, see Devitt 2015c: 111 n. 4. As Julie Wulfemeyer has aptly remarked recently: "The grounding cognitive relation went largely unexplained by Donnellan" (2017: 2). (b) Gareth Evans (1982) thought that my account, which he called "the Photograph Model", was too austere: a singular thought about an object requires *more than* this sort of causal-perceptual link: the thinker must also `have the capacity to distinguish the object...from all other things' (p. 89). Martin Davies (1981: 97) has a similar view tentatively endorsed by Neale (1990: 18). I have argued against Evans' extra requirement (1985).

The intending-to-refer hypothesis could also be completed by a causal explanation of the intention's reference along the lines just sketched. But that would make the explanatory redundancy of the intention, to be discussed next, *obvious*. So, not surprisingly, the hypothesis seems never to have been completed in that way. I set that possible completion aside. Where the hypothesis is completed, it is always implicitly completed by some *description* of the intended referent. Stephen's intention will be described as being, say, to refer to "the discoverer of conversational implicatures", thus entailing that the reference of Stephen's intention is to whoever fits that description.

4.1.3   Objection 3: Redundant

Even when the intending-to-refer hypothesis is completed in this descriptive way, its positing of a referential intention is redundant. Thus, suppose that Stephen's referential intention is the one just suggested: he intends to use 'Grice', 'he', or 'that man' to refer to whoever fits the description 'the discoverer of conversational implicatures'. But then *why posit this intention* to explain Stephen's reference? The reference of the alleged intention is determined by Stephen's association of the supposed description with his utterance of the singular referring term. So why not cut out the middle man and say simply that the reference of Stephen's "original" thought, perhaps *Grice is great*, is determined by that association? We can simply adopt a description theory of Stephen's use of 'Grice' in expressing that thought, thus concluding that 'Grice' refers to whatever fits that associated description. The positing of the additional thought, the reference-determining intention involving that description, does no explanatory work and is theoretically redundant.

Objection 1 is that it is implausible that a speaker must have a metalinguistic intention in order to refer. Objection 3 is that such an intention is not required to explain reference.

4.1.4   Objection 4: Misleading

The completed intending-to-refer hypothesis misleads in that it entails that the reference of a singular referring term is determined by a description that the speaker associates with the term: strip away the redundant intentions from the hypothesis and we are left with a description theory of reference. Yet, it is highly plausible that the reference of such a term is to be explained by some direct causal-perceptual relation to the referent not be an associated description of it. The hypothesis distracts from the likely truth about demonstratives.

That is the most important respect in in which the hypothesis misleads. But there is another. It arises because, if reference is determined by a referential intention, then *any* referential intention that can plausibly be ascribed, hence the description it contains, should be a plausible reference-determiner. Consider the use of the name,

'Gödel' by a reasonably well-informed philosopher, Fiona. Since she certainly intends to refer to Gödel, and believes that he is the man who discovered that arithmetic was incomplete, it is plausible to ascribe to her the intention to refer to the man who discovered that arithmetic was incomplete. We have just noted that the intending-to-refer hypothesis, thus completed, misleadingly entails that the reference of Fiona's 'Gödel' is determined by the description 'the man who discovered that arithmetic was incomplete' rather than by a direct relation to Gödel. The hypothesis misleads further given that there may be many other referential intentions that we can plausibly ascribe to Fiona, *given her beliefs about Gödel*; for example, she intends to refer to the most famous logician at the Princeton Institute of Advanced Study; the man who used modal logic to prove the existence of God; and so on. We face an explosion of referential intentions. And these can conflict: What if Schmidt proved the incompleteness? This is really a very old problem for description theories in a new guise. For, stripped of the redundant intentions, it is the problem of finding a principled basis for taking any particular description, or even any particular cluster of descriptions, as reference-determining (Devitt and Sterelny 1999: ch. 3). But why suppose that *any one* of these descriptions is reference determining? The intending-to-refer hypothesis misleadingly encourages an inference from a plausibly ascribed referential intention to the thought that its contained description is a serious contender for determining reference.

The having-in-mind hypothesis can be just as misleading: Fiona might be said to have in mind the man who discovered that arithmetic is incomplete, the most famous logician at the Princeton Institute of Advanced Study, and the man who used modal logic to prove the existence of God' – "having in mind" is vague enough for that – with a similar potential to mislead us about the reference of 'Gödel'.

The possibility that talk of intentions to refer might mislead in this way is not an idle one. In the seminal work in "experimental semantics" mentioned earlier (Sect. 2.3), Machery et al. (2004) conducted a Gödel experiment in which participants were asked who John, a character in the vignette, was "talking about" in using 'Gödel'. The choice was between "the person who really discovered the incompleteness of arithmetic", according to the vignette, Schmidt; and "the person who got hold of the manuscript and claimed credit for the work" (p. B7). This question was criticized as ambiguous. Machery et al. wanted the participant's intuition about who 'Gödel' conventionally refers to, its *semantic* reference. But the participant might take the prompt as asking about who John means by the name, its *speaker* reference. So the experiment may not have yielded what was wanted.[8] Here is one such criticism:

> Examples…suggest that a question of the form, "Who is *S* talking about in using *n*?" does not have a univocal meaning….In particular, although the communicative intention of the speaker (John) is not made explicit, the vignette may be taken to suggest that he intends to talk about the man who discovered that arithmetic is incomplete since the vignette insists that this is the only piece of information associated with 'Gödel'. It is thus unclear whether those participants who answer that John is talking about the man who discovered that arithmetic is incomplete are making a genuine descriptivist judgment about the semantic

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[8] I think that the criticism is mistaken because, in the circumstances described, participants should take John to mean the semantic referent. (Devitt and Porot 2018: 1579–80 n. 17).

> reference of 'Gödel' or rather making a judgment about speaker's reference. Consequently, it may be that almost everybody has causal–historical intuitions about semantic reference, but that some participants…report their intuitions about speaker's reference. (Machery et al. 2015: 67)

The vignette may indeed suggest that John "intends to talk about the man who discovered that arithmetic is incomplete" but, in light of our discussion, we can see that John's having this intention should not cause concern about Machery et al.'s (2004) results. The concern is that participants may take the question to be about speaker reference instead of the desired semantic reference, and *that speaker reference is thought to be determined by the above referential intention to be Schmidt*. But consider this from the perspective of the two theories of semantic reference being tested.

First, on the causal theory, that referential intention *does not* determine the speaker reference of John's 'Gödel': that intention is as irrelevant to that speaker reference as it is to the semantic reference of 'Gödel'. Indeed, John's *speaker* reference is determined by a causal link to the man who stole the proof; and the *semantic* reference of 'Gödel' to that man is explained by a convention of speaker referring to him by such causal links (2015c: 126; 2020b, 391–5). Semantic meanings are explained by speaker meanings (Sect. 3.2.3), as Grice proposed. So even if participants take Machery et al.'s question to be about the speaker reference of John's 'Gödel', their answer that John is talking about the man who discovered that arithmetic is incomplete – Schmidt – does indeed amount to a judgment against the causal theory of the semantic reference.

Second, on the description theory, that referential intention presumably does determine that the speaker reference is to Schmidt. So, if participants take Machery el al's question to be about the speaker reference of John's 'Gödel', and answer that John is talking about the man who stole the proof – Gödel – then that amounts to a judgment against the description theory of semantic reference. In conclusion, even if the participants take John to be intending to talk about the man who discovered that arithmetic is incomplete, and take Machery et al.'s question to be about the speaker reference of John's 'Gödel', the answers to that question bears on the two theories of semantic reference being tested. The alleged ambiguity of the question is no cause for concern.

In sum, if taken literally, the intending-to-refer hypothesis is: (1) implausible; (2) incomplete; (3) redundant once completed; (4) misleading. The having-in-mind hypothesis does not share faults (1) and (3).

4.2   Speaker Meanings and Intentions to Communicate

4.2.1   Intending to Communicate or Expressing Thoughts?

What explains the speaker meaning of a sentential utterance? A central idea of Gricean "intention-based semantics" is that this meaning is constituted by the speaker's intention *to communicate a certain content to an audience*. As Stephen Schiffer puts it, "Meaning entails audience-directed intentions, and one cannot

mean something without intending to be understood" (1992: 515). Stephen Neale expands the idea as follows: "In doing *x*, *S*, meant that *p* iff (roughly) for some audience, *A*, *S* did *x* intending *A* to think that *p* via *A*'s recognition that *S* intended *A* to think that *p*" (2016: 281).

No doubt speakers *typically* produce utterances to communicate messages to an audience. We should not, in semantics, describe these utterances as intentions to communicate, for much the same reason that we have just seen that we should not talk of intentions to refer (Sect. 4.1). Still, the utterances are surely intention*al* acts and that is how I will describe them. Furthermore, I'm inclined toward the view that representational systems like human languages owe their very existence, whether they are innate or conventional, to their role of communicating messages to audiences (2006b: 130–1). But it does not follow that the *only* meaningful use of language is to convey a message to an audience. More importantly, it does not follow that it is *essential* to its meaningful use that it involves intentionally communicating. And it is not essential, as Noam Chomsky points out:

> Under innumerable quite normal circumstances—research, casual conversation, and so on—language is used properly, sentences have their strict meaning, people mean what they say or write, but there is no intent to bring the audience (not assumed to exist, or assumed not to exist, in some cases) to have certain beliefs or to undertake certain actions. (1975: 62)

The basic intentional act in using language is not one of communicating a message but of *expressing a thought*: as quoted before (Sect. 3.2.3) "there is much to be said for the old-fashioned view that speech expresses thought, and very little to be said against it" (Fodor et al. 1974: 375). Expressing a thought can be done in various ways: by *speaking*, *writing*, or *signing* in some other way. And the point of this expressing may *not* be to communicate a message to an audience. There may *be* no audience, or the speaker may not think there is. The speaker may be musing ("To be, or not to be"), cursing ("Shit"), making notes, trying out a line for a poem, and so on. Even where the speaker is addressing an audience, she may not, for a variety of possible reasons, *care* if the message gets through. She may even be happier if it doesn't! Thus, it is common – particularly, I think, among the well-educated English – for people who are bored by ordinary small talk to regard conversation as a challenge to be *cleverly and wittily indirect*. Such a person may well not *intend* his hearer to grasp his message. Perhaps he will *hope* that the hearer is smart enough to do so, but he may not: he may anticipate the pleasure of watching the hearer fail. In sum, communicating a thought to an audience is just *one form* of expressing that thought.

If this is right, for a speaker to mean that *p* by an utterance is for her to be intentionally expressing a thought that *p*. It is not necessary that the speaker intentionally communicates that thought to some audience.

Now some philosophers will surely be tempted to make moves to save the view that communicative intentions are essential[9]: "when making notes one is communicating with *one's future self*"; "when musing, one is communicating to a *possible audience*"; "when having fun at one's audience's expense, one is communicating... – tricky, I'll have to get back to you on that one". But, first, is any of this

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[9] Indeed, Grice himself was tempted (1989: 113–16).

psychologically plausible? That is to say, is it plausible that speakers making utter-
ances that are at least *apparently* non-communicative *must* have some such thought
about some audience or other? *Is it really plausible that it is impossible to mean
something by an utterance without thinking about an audience?* Second, and more
important, what is the point of saving the view? Why do we need to go beyond
expressing a thought to the more demanding *attempting to communicate that
thought to an audience* in order to explain speaker meaning? *Expressing a thought*
will do the job. *There is no theoretical motivation to require the intentional com-
munication of the thought*.

   The idea that speaker meaning is constituted by communicative intentions is the
fundamental mistake of intention-based semantics. But that mistake is compounded
by the accounts given of such communicative intentions. Here is one of Grice's
later ones:

   "*U* meant something by *x*" is true iff *U* uttered *x* intending thereby:

(1) that *A* should produce response *r*
(2) that *A* should, at least partly on the basis of *x*, think that *U* intended (1)
(3) that *A* should think that *U* intended (2)
(4) that *A*'s production of *r* should be based (at least in part) on *A*'s thought that *U* intended (1)
     (that is, on *A*'s fulfilment of [2])
(5) that *A* should think *U* intended (4). (Grice 1989: 96–7)

Even this definition proved insufficiently complex to rule out all the suggested
counter-examples (Schiffer 1972). What are we to make of this? I have just indi-
cated the psychological implausibility of the claim that meaning something by an
utterance *must* be accompanied by a thought about an audience *A*. What about the
claim that it must be accompanied by *this* thought about *A*?! It is hardly plausible
that this baroque structure of intentions is psychologically real in any speaker. Grice
was sensitive to this concern from the start, disclaiming "any intention of peopling
all our talking life with armies of complicated psychological occurrences" (1989:
222). But then if there are no such armies we have no theory: we are left with no
explanation of speaker meaning. I know of no satisfactory solution to this problem.
I think this program of explaining communicative intentions is misguided (Devitt
and Sterelny 1999: 149–50).

4.2.2   The Expression of Thought

I have used the ordinary locution, "expressing a thought" to describe the basic act
which constitutes speaker meaning.[10] I don't say that the locution is perfect for the
task. We want something that describes the intentional act *common to* speaking,
writing, signaling, emailing, tweeting, and so on, something that abstracts from the
differences between these behaviors. Certainly these behaviors are covered by

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[10]This discussion of "expressing a thought" has benefited from the criticisms of Elmar Geir
Unnsteinnson, Daniel Harris, and Gary Ostertag.

"expressing a thought" but very likely some other ones are too. Thus, we might ordinarily say that a person's face expresses the belief that her company is boring even if its so doing is *un*intentional. On the other hand, one might argue that when humans greet ("Hi"), cheer ("Bravo"), abuse ("Bastard"), curse ("Shit") and the like, they are expressing *some* mental state but often not a *thought* or even something representational (Sect. 3.2.3, n. 4). So, English seems not to have an ordinary locution that perfectly describes the action we want to pick out for our theoretical purposes. And that is hardly surprising: the folk are not bent on explaining speaker meaning. Still, "expressing a thought" seems closest to what we want. So I will stick with it, perhaps narrowed to exclude the unintentional and widened to include the likes of greetings and curses.

A further complication should be mentioned. Humans are not the only animals that make meaningful "utterances" in a language; consider, for example, bees and prairie dogs (Sect. 3.2.2). What constitutes the "speaker meaning" of a particular bee dance? Not presumably the content of the *thought* expressed but rather the content of some "lesser" representational state in the bee that causes the dance. So to cover nonhumans perhaps we should take speaker meaning to be something like "the content of the inner representational state causing that use of language". But I set this complication aside.

So what precisely is it to "express a thought", in this perhaps slightly technical sense? I have nothing more to say about this than I have just said. And I don't think that philosophers should expect to have anything more to say at this time. So, *expressing a thought* is partly unexplained in the theory, and to some extent a "primitive". This is not to say that *expressing a thought* is inexplicable. Psycholinguists studying language production are in effect trying to throw light on it (see, e.g., Vigliocco and Vinson 2003). The point is just that this is now beyond philosophers.

Our theory explains speaker meaning in terms of the content of the thought expressed. What about that content? Grice has nothing to say about this. I think that we should answer along the following lines.[11] The content of a thought is constituted by its structure and the content of the concepts that are in that structure. In virtue of what does a concept have its content? For basic concepts, probably including those expressed by demonstratives, proper names and some "natural" kind terms, this must be a matter of some sort of direct, presumably causal, relation to their referents.[12] In explaining these relations we are likely to have to appeal to ideas from historical-causal, teleological, or indicator theories of reference. At bottom, I suspect, we will have to rest on a teleosemantic theory of *perceptual* content of the sort urged by Karen Neander (2017). I think (1996), though Fodor (1987) does not, that some concepts will surely not be basic but rather covered by a description theory; <bachelor> is a likely example. Meanings for these concepts will come from inferential associations with others; for example, <bachelor>'s association with

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[11] I draw on my 2006b: 155–6.

[12] But see Devitt and Porter 2021 for some experimental evidence against this view of the folk's "natural" kind terms. What about "empty" terms like 'Pegasus' that lack a referent? This is a tricky question that must be set aside. (My 1981a, Chap. 6, is an attempt at it.)

<adult>, <unmarried>, and <male>.[13] What about the structure? In virtue of what does a thought have its structure? Presumably, in virtue of the way in which thoughts with that structure interact inferentially with other thoughts and relate ultimately to the world. Thus, the thoughts <Hillary is secretive> and <Trump is unhinged > share a structure in virtue of a similarity in these respects. Similarly, <All politicians are rich> and <All police are corrupt>, which share a different structure in virtue of their similarities. None of this takes us far, of course.

(It can be helpful in thinking of thought content to adopt the language-of-thought hypothesis (Fodor 1975). Then the *vehicle* of a thought is a language-like mental sentence, a group of mental words set in a syntactic structure. The *content* of the thought is the meaning of its mental sentence. And that meaning is constituted by the sentence's syntactic structure and by the meanings of the mental words that are in the structure.)

In light of this, in virtue of what does a sentential utterance have its speaker meaning? In virtue of the causal story of its production. Thus the speaker meaning of a word in that utterance is determined by the concept it expresses on that occasion. The "speaker syntax" of the utterance's sentence is determined by the structure of the underlying thought that the utterance expresses and by the way in which the utterance was produced from that underlying thought.[14]

4.2.3 Parikh's Game-Theoretic Objection

Finally, Prashant Parikh objects to my account of speaker meaning, claiming that the content of the thought expressed by an utterance "is just a necessary condition that is not sufficient [to constitute its speaker meaning]. After all, a speaker will have many thought contents…" (2019: 68). Yes, but she is only *expressing* one (or perhaps a few) in her utterance. Parikh thinks that to get her speaker meaning we need to add a condition that takes account of the addressee and other ambient facts. This reflects his impressively detailed *game-theoretic use theory* of meaning: "My approach squarely pursues the possibility of describing use directly and deriving meaning from use…. reducing meaning to communication" (p. 8). In my view, in contrast, utterances play their causal role in communications in virtue of their meanings but those communications have no part in constituting those meanings.[15]

This is not the place to argue the matter, but here are some brief words on the place of game-theoretic considerations in my picture. Any person does indeed have an indefinite number of thoughts that she might decide to communicate to an

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[13] See Devitt and Sterelny 1999, particularly Sect. 7.7, for a discussion of these various theories of concepts.

[14] In my 1996 (p. 158) I failed to note the importance of the way in which the utterance was produced to its syntax.

[15] Reference borrowings aside. As Kripke (1980) showed, we all refer to Aristotle with his name in virtue of borrowing that name in communications (Devitt 2015c: 115–118).

audience and an indefinite number of expressions she might use for that purpose in utterances. I am concerned to say what constitutes the meanings of any expression she chooses to utter. I am concerned to explain the nature of those meanings. There is another concern: How does the speaker decide on one thought-expression pair rather than any other (or none)? As noted (Sect. 3.3), this process is a concern of "pragmatics", understood as the theory of communication. This is very different from my concern with the nature of the meaning of any expression used as the result of that decision. Game-theoretic considerations get into the picture *in that decision process*. Thus, when someone wants to communicate a certain thought to an audience she should choose an expression with a meaning that is likely, in the circumstances, to achieve this. So, the game-theoretic decision process is *causally* responsible for a person producing one expression with a certain meaning rather than another expression with that meaning or any other meaning. Hence that decision process is causally responsible for the meaning of that utterance being among the meanings the natures of which we are trying to explain. But that decision process has no role in the explanation of those natures. Indeed, the explanation runs the other way: *it is partly because the expression has a meaning with the nature it does that the decision was made to produce it.*

Analogy Any person has an indefinite number of jobs that she might decide to perform and an indefinite number of tools she might use for that purpose: hammers, screwdrivers, wrenches of all shapes and sizes, etc. The concern analogous to mine is with saying what constitutes a particular tool, what explains the nature of that tool. The very different concern is with how to decide on one job-tool pair rather on any other (or none). The decision process is *causally* responsible for a person using one tool with a certain nature rather than another tool with that nature or any other nature. Hence that decision process is causally responsible for that tool being among the tools the natures of which we are trying to explain. But that decision process has no role in the explanation of those natures. Indeed, the explanation runs the other way: *it is partly because the tool has the nature it does that the decision was made to use it.*

Objection "But suppose that a tool has two natures, just as a word often does. Then its nature on any occasion of use is determined by the environment in which it is used. How can the analogue of your speaker meaning have any coherence in cases of dual-nature tools?"[16]

Reply First some preliminaries. A tool can indeed have two natures; for example, it can be both a hammer and a paperweight (cf. ambiguity). What constitutes something being a hammer is roughly that it was made to hammer (cf. conventional meaning by stipulation) or that it is standardly used to hammer (cf. conventional meaning arising from use). Similarly, paperweights are made or standardly used to hold down paper. The one object could clearly have both natures. Many things that

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[16] Parikh made a response to my tool analogy along these lines in an email exchange.

are not hammers and are not *standardly* used to hammer can occasionally be used to hammer; e.g. a shoe (cf. speaker meaning that is not conventional meaning).

(i) When something that is both a hammer and a paperweight is used to hammer, what constitutes its being so used is the thought of the user (cf. speaker meaning). (ii) The environment may *cause* a person to use the object, indeed any object like a shoe, as a hammer but it does not *constitute* it being so used (cf. speaker meaning). (iii) An object that is both a hammer and a paperweight remains a paperweight even when it is used in some environment as a hammer. And the environment does not constitute it being either (cf. conventional meaning).

4.3 Constraints on Intentions

Griceans standardly believe that there is a constraint on what a speaker can intend to communicate by an utterance, a belief that reflects the belief that there is a constraint on intentions in general. (Given what has gone before in this chapter, it would be better to express such constraints as being on what a speaker can intentionally communicate and do. But let us set that aside.) The literature contains many versions of this constraint, some of them astonishingly strong. Here is a sample.

Talking of intentions in general, Grice claimed in one place that

(1) a condition on 'X intends to do A' is that X "is sure that he will in fact do A" (1971: 266).[17]

Talking of intentions to refer, Stephen Schiffer claimed that

(2) "the speaker cannot intend to refer to a particular female [by 'she'] unless he expects his hearer to recognize to which female he is referring" (2005: 1141).

Reinaldo Elugardo and Robert Stainton make the weaker, hence more plausible, claim that the speaker's expectations must be about what the hearer *can* do rather than what she *will* do:

(3) "the intentions that a speaker can have are importantly constrained by her reasonable expectations about what the hearer can figure out" (2004: 445).[18]

And Grice, in another place, urges a similarly weaker constraint: talking about intentions to mean something by a hand wave HW, Grice proposes that

(4) a speaker "must (logically) be in a position, when uttering HW, to suppose that there is at least some chance that these intentions will be realized" (1989: 125).

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[17] See also Harman 1976; Velleman 1989.

[18] See also Brand 1984.

These are all "positive" claims, requiring the intender *to have* a certain propositional attitude (or "be in a position" to have one). Elsewhere Grice's constraint is a much weaker "negative" one, requiring the intender *not to have* a certain attitude:

(5)   "one cannot in general intend that some result should be achieved, if one knows that there is no likelihood that it will be achieved" (Grice 1989: 101; also: 98).

In an early paper Neale urges a slightly weaker negative claim:

(6)   the *formation* of genuine communicative intentions by *U* is constrained by U's expectations: *U* cannot be said to utter *X M*-intending *A* to φ if *U* thinks that there is very little or no hope that *U*'s production of *X* will result in *A* φ-ing". (1992: 552)[19]

Later Neale urges a constraint that is much the same as Grice's negative one:

(7)   "I cannot intend *what I believe to be impossible* (2004: 77).[20]

We can capture well enough the range of possible constraints as follows, with two positive ones followed by two negative ones, the stronger first in each case:

X cannot intend to *A* unless:

P1: *X* believes that she will *A*. [cf. (1), (2)]
P2: *X* believes that she can *A*, has some chance of *A*-ing. [cf. (3), (4)]
N1: *X* lacks the belief that she will not *A*.
N2: *X* lacks the belief that she cannot *A*, has no chance of *A*-ing. [(5), (6), (7)]

Before assessing these constraints, we should think about the force of "cannot intend". Perhaps it yields a *constitutive* constraint: if the constraint is not met, it is not metaphysically possible for any mental state of *X*, whatever else it might be, to *be* an intention to *A*. That seems very strong. So perhaps "cannot intend" yields a weaker *normative* constraint: if the constraint is not met, it is not metaphysically possible for *X* to *rationally* intend to *A* (though *X* might irrationally intend to). I shall focus on the normative constraints. Clearly, anything that counts against a normative constraint will count against the corresponding constitutive constraint.

Consider the positive constraints, P1 and P2. Neither is plausible. First, and simply, *X* might have no beliefs at all about the likely outcome of trying to *A*. Perhaps she has no relevant evidence; or she has some evidence but is unable or unwilling to assess its significance. Yet she might still think, rationally, that it's worth trying to *A* anyway; "I don't know if I can but I might as well give it a go", she says to herself. Second, as Keith Donnellan (1968: 212, n. 10) and Stephen Schiffer (1972: 69) bring out, if *X*'s life is at stake she might try *anything* however bleak she thinks the prospects of success. Suppose, for example, that *X* is shipwrecked and her only hope is to swim to shore. Although *X* is too realistic to believe that she *will* reach shore (P1), or even that she *can* reach shore (P2), she may still form the rational intention

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[19] See also Mele 1992.

[20] Neale's constraint on intentions plays a key role in his theory of language, as we shall see (Sects. 6.4 and 6.5). See also its role for Stainton (Sect. 7.5).

to reach shore. For similar reasons, the negative constraint, N1, is not plausible. Even if *X* believes that she won't reach shore however hard she tries, if she is in dire enough circumstances it may not be just possible for her to try to do so, it may be rational for her to try: "What have I got to lose?", she says to herself.

But what about the last and weakest constraint, N2? Could *X* really intend to swim to shore even if she believes that she has *no chance* of doing so, that her doing so is *impossible*?[21] Initially it may well seem not. Still, on further thought, even this constraint seems dubious. Suppose *X* has an appropriate amount of epistemic humility and is well aware of her own fallibility. So she realizes that her belief about the impossibility of her reaching shore *may be wrong*: Then, given what is at stake, she may still intend to swim to shore in the hope that she is wrong. And so she should.

So, I'm inclined to think that intentions are not subject to any of the constraints contemplated by Griceans, whether construed as constitutive or normative. I suspect that we should settle for a weak *psychological*, not *constitutive* or *normative*, constraint on intentions along the following lines: the less a person believes that she will succeed in *A*-ing if she tries then, *other things being equal*, the less likely it is that she will intend to *A*.

Finally, *why* is it so common among Griceans to insist on some constraint on what a person can intend? One reason seems to be, the Humpty Dumpty worry. If speaker meaning is simply a matter of what the speaker intends to communicate, then we need some constraints on that intention to avoid the unacceptable consequence that the speaker might mean absolutely anything by an utterance. I think that the above psychological, not constitutive or normative, constraint on intentions is sufficient for the purpose. Thus it is unlikely that Humpty Dumpty would intend to communicate to Alice that that is a nice knock-down argument by saying "There's glory for you" because he should believe that he is very unlikely to convey that message to Alice by that expression, given the expression's conventional meanings in English.

4.4   Conclusion

There is much talk of intentions in semantics that I have argued is mistaken:

(I) In virtue of what does a speaker using a name or demonstrative refer to *x*? A popular answer is: because he *intends to refer to x*. I had four objections. (1) This answer, unlike another popular one – because *he has x in mind* – is too intellectualized to be even a good starting point. (2) It is theoretically incomplete: In virtue of what did the speaker intend to refer to *x*? (3) Once completed, it is redundant. (4) It is misleading (Sect. 4.1).

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[21] Neale recently concluded a subtle discussion of suggested constraints by endorsing this one (2016: 278–80).

(II) What explains the speaker meaning of a sentential utterance? A central idea of Gricean "intention-based semantics" is that this meaning is constituted by the speaker's intention to communicate a certain content to an audience. I argued that the idea that this intention is necessary for a meaningful utterance is psychologically implausible and theoretically unmotivated. The basic act of speaker meaning is one of *expressing a thought*. For a speaker to mean that *p* by an utterance is for her to be intentionally expressing a thought that *p*. Expressing a thought is an act common to speaking, writing, emailing, and so on. Game theoretic considerations bear on the choice of an utterance to make but not on the explanation of the nature of the one that is made (Sect. 4.2).

(III) It is standard among Griceans to believe that there is some constitutive/normative constraint on what a speaker can intend by an utterance, a belief arising from one about a constraint on intentions in general. The alleged constraint varies from the astonishingly strong "positive" one that *X* cannot intend to *A* unless *X* believes that she will *A* to the much weaker "negative" one that *X* cannot intend to *A* unless she lacks the belief that she cannot *A*. I argued that there are no such constitutive/normative constraints on intentions (Sect. 4.3).

It is time now to say more about conventions.

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Chapter 5
Linguistic Conventions and Language

As the discussion in Chap. 3 demonstrates, conventions loom large in my view of language and meaning in general and in my response to Linguistic Pragmatism in particular. David Lewis remarks that "it is a platitude – something that only a philosopher would dream of denying – that there are conventions of language" (1983: 166). A leading philosopher, Donald Davidson, is indeed skeptical of the role of convention in language and a leading linguist, Noam Chomsky, gives conventions no place in his theory. It is time to say more about conventions and their role.

I set out my position on linguistic conventions in Sects. 5.1 and 5.2 and respond to convention deniers in Sects. 5.3, 5.4, 5.5 and 5.6.

5.1 Conventions and Linguistic Meanings

What is it for an expression *E*, a sentence or part of a sentence, to have a certain literal meaning *M* in a person's idiolect? *E*'s having *M* is *constituted* by linguistic rules, lexical rules and syntactic rules. First, take *E* to be a word and consider its lexical rule. That rule is part of the person's idiolect in virtue of her disposition to associate *E* with *M* in the production and comprehension of language.[1] In production, she has a disposition to use *E* to express a concept (part of a thought) that has meaning (content) *M*; so, she has a disposition to use *E* with speaker meaning *M* (Sect. 4.2). In comprehension, she has a disposition to interpret a use of *E* as having that meaning. Such a disposition constitutes her competence with *E* (Sect. 2.7) and is an "entry" in her "mental lexicon".[2] Next, take *E* to be a complex expression,

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[1] It should go without saying that the dispositions in question here are real mental states, not subject to some behaviorist reduction.

[2] I have oversimplified for convenience in at least two ways. (1) An indexical has a type meaning that yields token meanings that vary with contexts. We might say that the indexical has that type meaning in virtue of the person having a disposition to use it to express concepts with token mean-

perhaps a sentence. Then *E*'s meaning *M* in her idiolect is constituted not only by lexical rules for *E*'s words but also by syntactic rules governing its structure. A syntactic rule governs part of *E* in virtue of her being disposed, in language production and comprehension, to associate expressions with the structure of that part of *E* with parts of thoughts with that structure.[3]

Where *E* is ambiguous in a person's idiolect, she has dispositions to associate it with more than one meaning. In production, the thought/concept she is expressing selects which dispositions she exercises. In comprehension, she uses contextual clues to select which to exercise. Sometimes in production, a person will not do what she has a disposition to do; she will deliberately express a thought/concept with *E* even though she has no disposition to use *E* for that purpose, as in a metaphor or other pragmatic modification; or she will make a performance error. Correspondingly, in comprehension. In these cases, *E* will have a speaker or audience meaning that is different from its literal meaning (if any) in the person's idiolect. More on this in Sect. 5.6.

The dispositional view of a person's competence that I have presented is a *minimal, least theory-laden*, view. It is common to have a much richer view. Thus, lexical semanticists typically assume that a person is disposed to associate a word with a certain meaning in virtue of *representing allegedly semantic and syntactic facts* about the word, often quite rich facts; we shall see examples later (Sect. 11.3). I am dubious of such assumptions and have tentatively favored a much more "brute-causal" view of language processing (2006b: 220–41). So I do not assume that the lexicon is rich.

What *causes E* to mean *M* in a person's idiolect? (i) As we noted in Sect. 3.2.3, if Chomsky is right, as he probably is, some syntactic rules are *innate*. So any part of the meaning of *E* constituted by syntactic rules may be innate. In principle, *E*'s meaning could be entirely innate, like that of the bee's dance. (ii) But a large part of *E*'s meaning *M* is typically the result of the person's participation in *linguistic conventions*; to that extent, *M* is a conventional meaning. A person participates in the convention of *E* meaning *M* when she uses *E* to mean *M because* of the convention. (iii) Finally, some of a person's idiolect may be her own work and so a bit *idiosyncratic*: the literal meaning of *E* for her may not be a meaning it has according to any linguistic convention. Davidson brings this out nicely in his discussion of Mrs. Malaprop's "a nice derangement of epitaphs" (2005: 89–107): what she literally means, *what she says* exploiting *her* language, is *a nice arrangement of epithets* and yet her words do not mean this according to any convention. Indeed, in principle, a language could be entirely idiosyncratic, like that of the isolated desert islander.

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ings that are thus dependent on the context; and being disposed to interpret it as having a meaning that is thus dependent on context. (2) A lexical entry really concerns a set of words related through inflection; for example, 'run', 'runs', 'ran', 'running'.

[3] See Sect. 4.2.2 on the structure of thoughts. The text draws on an earlier discussion (2006b: 155–6). Of course, the syntactic rules for the person's idiolect generate expressions that she never uses, indeed could never use because they are too complicated and/or long.

An idiolect is shared in a community, and hence is the language of that community, to the extent that the members of the community are disposed to associate the expressions of the idiolect with the meanings they have in that idiolect. This sharing could, in principle, come about "by chance" as the result of each member's own work. But, of course, it does not. It probably partly comes about because of some innate syntax. It is largely the result of the community's participating in the same linguistic conventions. We shall say more about this in the next section.

So, conventions are the typical, though probably not only, *cause* of linguistic meanings. If anything close to this story is right, there can be meanings without conventions and so conventions obviously could not *constitute* meanings. It rather looks, however, as if some may think otherwise. Daniel Harris (forthcoming) emphasizes the distinction between causing and constituting meanings – he uses the term "grounding" for the latter – and argues persuasively that David Lewis and others should be seen as taking conventions to ground meanings.[4] Thus, consider the following from Lewis' classic, "Languages and Language":

> a language *L* is *used by* a population *P* if and only if there prevails in *P* a convention of truthfulness and trust in *L*, sustained by an interest in communication….A string of sounds or of marks…has a certain *meaning in P* if and only if it has that meaning in some language L which is used in P (1983: 169)

It is hard not to read this as the view that what constitutes the string or sound having that meaning in *P* is *P*'s participation in a certain linguistic convention. Similarly, Stephen Schiffer's bald claim: "That *x* means something in a group *G* is a matter of convention" (1972: 118–9).

Interpretation of an author on this matter is often difficult however. Thus, it is hard to know where Grice stands from his complicated discussion (1989: 122–9).[5] And consider Lewis' response to a couple of objections:

> *Objection*: Language is not conventional…
> *Reply*: It may be that there is less conventionality than we used to think: fewer features of language which depend on convention, more which are determined by our innate capacities, and therefore are common to all languages….But there still are conventions… (p. 180)

It is natural to take Lewis here to be acknowledging that meanings may be caused by innate states – my (i) above – whilst insisting that some are caused by conventions – my (ii) above. So this does not sit well with the idea that conventions *constitute* meanings.

> *Objection*: A man isolated all his life from others might begin – through genius or miracle – to use language, say to keep a diary….

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[4] He also argues persuasively that Ruth Millikan (2005) takes conventions to cause *not* ground meanings.

[5] The problem centers on understanding the second clause in his definition, D3, of timeless meaning for a group (1989: 127). At best the inclusion of this clause seems to show an insensitivity to the key cause/constitute distinction. A reason for charity is Grice's later remark: "I do not think that meaning is essentially connected to convention" (p. 298).

Reply: Taking the definition literally, there would be no convention. But there would be something very similar. The isolated man conforms to a certain regularity at many different times….We might think of the situation as one in which a convention prevails in the population of different time-slices of the same man. (181–2)

It is natural to take Lewis here to be acknowledging that meanings in a person's language may be her own work, caused by her – my (iii) above. But still the suggestion may be that these meanings are *constituted* by a "very similar" regularity. It follows from my discussion, of course, that this suggestion would be wrong.

One problem in interpreting authors on this matter is the ambiguity of certain English expressions: "determining meaning" might be causing something to have it or constituting its having it; "explaining meaning" might be explaining the cause or explaining the nature; "explaining what constitutes a conventional meaning" might be explaining what constitutes the meaning that was caused by conventions or explaining what constitutes the convention that caused or constitutes the meaning. I suspect that these ambiguities also partly explain some failures, including my own at least, to honor the distinction between conventions causing and constituting meanings.[6]

The linguistic conventions that concern us are conventions of using an expression with a certain meaning. There are many other conventions in the use of language that are not of this sort and are not our concern; for example:

the convention of not swearing in church;
the convention of answering the phone by saying 'Hello';
the convention of speaking loudly enough for one's audience to understand;
the convention of starting a letter "Dear…".[7]

When I talk of "linguistic conventions", I should always be taken to be talking of conventions of speaker meaning by expressions.

The great majority of the linguistic rules that govern a person are the result of her participation in linguistic conventions. So, as noted in Sect. 3.5, the key issue for the SEM-PRAG dispute, the identification of the linguistic rules, comes down to: What are the linguistic conventions that a person participates in?

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[6] The above dispositional account of what constitutes linguistic meaning was my "official position" in *Ignorance of Language* (2006b: 179–80). And it is clear from this account that I do not hold that conventions constitute linguistic meanings. Still, some of my previous writings have not been nearly as sensitive as they should have been to the importance of distinguishing causing a linguistic meaning from constituting it (e.g. 1981a: 80–6, 1996: 157–8). For example, I should replace my recent definition of "Conventional-Designation" for proper names that talks of "participating in a convention" (2015c: 126) with a definition of *Linguistic*-Designation that talks of being "governed by a linguistic rule". Indeed, in at least three places, I have written as if conventions were constitutive (2006b: 139–40, 2013b: 92–3, 97–8, 2013c: 106). I am very indebted to Harris for making me much more careful about this important distinction. He rightly insists that there can be no reduction of meanings to conventions.

[7] Thanks to Rob Stainton for some of these examples.

5.2   Linguistic Conventions

In Sect. 3.2.3, I talked briefly of a linguistic convention typically coming from the regular use in a community of a sound, an inscription, etc. to convey certain parts of messages. That regular use of it in utterances with a certain speaker meaning, leads, somehow or other, to that form having that meaning conventionally in the language of that community. Thus, speaker meanings have an explanatory priority, as Grice (1989) emphasized.

We have just noted the mistake of taking linguistic conventions to be constitutive of meanings. Another mistake to avoid is that of taking regularities to be constitutive of the conventions: "Conventions are regularities in action, or in action and belief" meeting certain conditions (Lewis 1983: 164); "conventions are behavioural regularities" (Geurts 2018: 118). Conventions may cause, and be caused by, certain regularities in action but they could not *be* the regularities. For, there can be conventions without regularities. (1) We have already noted that linguistic conventions can be created by stipulation and agreement within a community (Sect. 3.2.3). Icelanders successfully use this method to keep their language unique and some French would love to use it to keep out English. A stipulated convention of using a certain form with a certain meaning can exist whether or not anyone does, as a matter of fact, use that form with that meaning, hence whether or not people regularly do so. (2) There can be a convention in a group of doing A when X-ing, however caused, and yet members of the group seldom do A: perhaps X-ing is illegal; or only possible in seldom realized circumstances; or seldom desired by the group.[8] Perhaps doing A is taboo[9];

So, what is a linguistic convention? What is it for the meaning of a word and the syntax of an expression in a language to arise from a convention in a community and hence be a conventional meaning? The members of that community are all disposed to associate the word with that meaning and the expression with that syntax, thus normally generating a regularity of so associating them. These shared dispositions amount to a linguistic convention if their sharing is explained by a certain sort of causal relation between the dispositions. Lewis (1969) and Schiffer (1972) have a lot to say about this relation, of course, but their accounts, particularly requirements of "common knowledge" (Lewis) or "mutual knowledge*" (Schiffer), seem too

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[8] For more on conventions without regularities, see Millikan 2005: 11–14; Gilbert 1989: Sec. 4.5; Davis 2003: Sec. 10.5.

[9] When Evangelical Christian Americans talk of sexual intercourse, the venerable English word 'fuck' may never pass their lips; similarly, politically correct white people talking about people of color, and certain infamously racist epithets. Nonetheless, 'fuck' and the epithets have their conventional meanings in the respective languages of these two groups; indeed, that's *why* uttering them is taboo. Thus, a person in one of these groups is disposed to interpret the word in question with that conventional meaning in comprehension and would mean that by it in production if she broke the taboo. And she could break the taboo and participate in this convention any time she wished.

intellectualized.[10] And this appearance is reinforced by the thought that we should see the linguistic behavior of some nonhuman animals as conventional; see earlier on the prairie dogs (Sect. 3.2.2). As Ruth Millikan says:

> for those [conventions] that spread because they [solve coordination problems], the 'because' is almost never a reasoned because but some more mundane kind of causal because. The rest of us conform to linguistic conventions in exactly the same unreasoned way that the idiot and the child do. (2005: 56)

We should seek a less intellectualized view.

It is, of course, hard to say precisely what causal relation is appropriate for the shared linguistic meaning to be conventional, but the center of what has to be said is that any speaker has her disposition *because other speakers in her community have theirs* and hence are likely to use the expression regularly with that meaning; a speaker *notices* the disposition in others and this *causes* her to have it; there is a certain sort of dependency of the disposition of each member of the community on the dispositions of others. And for a person to participate in this convention is for her to act on such a dependent disposition. The norm is for speakers in a community to share a linguistic meaning because they stand in the required causal relation. As a result, the linguistic meaning of almost all expressions for almost all speakers will be the conventional meaning of those expressions in the speakers' community. Mrs. Malaprop's divergence from the norm is exceptional.

Finally, in thinking about linguistic conventions it is important to note two features of conventions in general. First, there can be indeterminacy about whether there is a convention. For, there is a sorites problem about when a regular practice has established a convention. Consider a convention among graduate students of meeting in O'Reilly's for a drink around 6 pm on Fridays. At semester beginning there was no such convention. By semester end, there was. There was never any explicit agreement. Rather, two or three went there the first week. A few more went next week. The word of this got around. More and more people started to go regularly. People started to expect others would go. A convention was established. But there was no determinate point in the semester at which it was established. Similarly, where linguistic conventions arise out of regularities in usage, which they typically do, there will be no determinate point at which the convention is established.

Second, conventions are essentially conventions *in a group*, a group that can be as small as two. Thus, the O'Reilly convention is a convention in that group of graduate students. And most of the conventions of "English" are conventions in a large group including most people in in Britain, America, and Australasia, some people in India, and so on. But some "English" conventions are only in sub-groups of the peoples in those many countries. More on this in Sects. 5.5 and 5.6.

Why are linguistic conventions important to the SEM-PRAG issue? To settle that issue we need to settle what linguistic meanings expressions have in a community

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[10] Stephen Laurence (1996, 1998) rejects "convention-based semantics" largely on the basis of criticisms of Lewis' account of conventions in general and of linguistic conventions in particular. These criticisms are partly of the highly intellectualized nature of Lewis' account. I sympathize with those criticisms. I reject the rest (2006b: 180).

C, to settle what meanings are encoded in the shared language of *C*. Wherever there is a convention in *C* of using *E* to speaker mean *M*, *M* is a linguistic meaning of *E* in *C*. And wherever *M* is a linguistic meaning of *E* it will be largely because that meaning was established by convention. So, the best evidence we are likely to have that *M* is a linguistic meaning of *E* is that *E* means *M by convention*. And the best evidence of that is that speakers *regularly* mean *M* by *E* (Sect. 3.6).

That concludes my presentation of my position on linguistic conventions. We turn now to the convention deniers.

5.3   Collins Against Conventions

I began this chapter with Lewis' remark that "it is a platitude – something that only a philosopher would dream of denying – that there are conventions of language" (1983: 166).[11] In a related vein, Fodor says: "think of a natural language as a system of conventional vehicles for the expression of thoughts (a view to which I know of no serious objections)" (1981: 196). This is, in effect, the view I have urged. In stark contrast to these views, 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). John Collins, an enthusiastic follower of Chomsky, goes even further. In criticizing my appeal to conventions in theorizing about language, Collins doubts that talk of conventions could be theoretically respectable *anywhere* until it has been adequately explained. And he thinks it has not been (2008b). I think that these Chomskian doubts about conventions and their role in theorizing about language are deeply misguided. I shall discuss Collins first.[12]

The account of conventional meaning in the last section does not go nearly as far as one might like, as I acknowledged when giving a similar one before (2006b: 179–80). Because of this, Collins claims that "the relevant notion of convention remains wholly opaque" (2008b: 245; see also 2008a: 35) and hence, by implication, unacceptable. This is simply not so. First, there should be no doubt that there really are conventions. Even aside from stipulated or explicitly agreed conventions, Lewis has given a range of examples which demonstrate the frequency of conventions without stipulation or agreement (1969: 36–42).[13] Second, Lewis, Schiffer and

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[11] In calling the view a "platitude", Lewis is rightly emphasizing how ubiquitous it is among theorists, perhaps even among folk. But I am not of course urging it *because* it is thus "commonsensical"; c.f. Rey's misrepresentation (2020b), and my response (2020b), mentioned earlier (Sect. 3.2.3, n. 5).

[12] This discussion of Collins draws on Devitt 2008b.

[13] Collins' criticism of my discussion of ellipsis makes me wonder whether he is under the misapprehension that conventions have to be by explicit agreement: discussing 'Mary went to the bank and so did Bob' he asks: "Are we to imagine parents agreeing with their children to use *do* so that it picks up a particular antecedent?" (2008a: 36n. 45).

many others[14] have attempted accounts of this that are much more complete than mine. I am not committed to the details of any of these and so have rested with my brief sketch. However, if I had to commit I would go for something along the lines of Millikan's account of "natural conventions" which require

> neither coordinations, regular conformity, nor rational underpinnings [cf. Lewis]. Natural conventionality is composed of two, quite simple, related characteristics. First, natural conventions consist of patterns that are "reproduced" in a sense to be defined. Second, the fact that these patterns proliferate is due partly to weight of precedent, rather than due, for example, to their intrinsically superior capacity to perform certain functions. (1998: 162; see also 2005)

I do not need to commit to any of these accounts, even Millikan's, because, even if none of them is fully adequate, they do not leave the notion of convention seeming opaque, let alone "wholly opaque". Third, we need a notion of convention to explain much human behavior including, as just noted, human linguistic communication. So, even though the notion may not have been adequately explained, it is much needed and not mysterious.

Science is full of such notions. Indeed, it must be since, as they say, at any time "explanations must stop somewhere". Think, for example, of the notion *gene*, which was introduced in the nineteenth century, played the central role in Mendelian genetics, and yet remained unexplained for decades. And consider a notion that is more analogous to convention; the notion of *social play*. This is a much needed notion in cognitive ethology yet, like convention, it is not fully explained (Allen and Bekoff 1994). And in thinking about it in a human context, as in thinking about convention, one is tempted by a highly intellectualized account. Thus Alex Rosenberg (1990) argues that social play involves "third order intentionality": thoughts about thoughts about thoughts. Yet it is hard to accept that kittens, who surely play with each other, could really have such sophisticated thoughts. *Social play* is another notion that is not fully explained, much needed in science, and not mysterious.

Collins wonders how linguistic conventions could be established. Since we have not fully explained these conventions it is, of course, hard to say. But it is just as hard to say how *any* convention (that is not stipulated) is established. Yet, as Lewis demonstrates, there are thousands of such conventions. A linguistic convention typically comes from the regular use of a linguistic form with a certain speaker meaning. For this to become a convention, hearers must notice this regularity and catch on that this is an "accepted" way to express that meaning in their community. But this noticing and catching on are likely not high-level-cognitive processes; likely, they are "implicit" and "procedural" rather than "explicit" and "declarative" (2006b: 210–20). That is the favored way to explain human learning of the conventions of "artificial grammars" in some well-known experiments (Reber 2003). And

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[14] For example, Bach and Harnish 1979; Stalnaker 1984; Gilbert 1989; Millikan 1998; Miller 2001; Geurts 2018.

it is surely what dolphins exemplified in learning the conventions of a primitive OSV language.[15]

Some final points. First, the claim is that conventions are, *as a matter of fact*, the explanation of the uniformity of idiolects among members of a community, just as they are of the many other social uniformities illustrated by Lewis and others. This is not to say that conventions *must* explain such uniformities: "in another possible world", any of these uniformities might be innate or even come about "by chance" (Sect. 5.1). The point is just that, "in the actual world", conventions do the causal work. Second, if the ordinary term 'convention' is thought to bring unwanted baggage, then take what we are using here to be a somewhat technical term, partly explained above, and needed to explain the social uniformities indicated. Indeed, if the word 'convention' offends, pluck it out and replace it with another.[16]

In sum, it is quite acceptable to talk about conventions in theorizing about language even though we lack a complete explanation of their natures and of how they are established.

5.4   Chomsky Against Linguistic Conventions

Turn now to Chomsky's views.[17] First, he claims that there are few regularities in usage and hence few linguistic conventions. Chomsky thinks that this claim should be a truism (1996: 48). Yet it is surely astounding. Although it is possible in principle to have an idiolect that is not based on regularities in usage and hence is entirely unconventional, nobody in fact comes close to having such an idiolect.

Of course, if Chomsky is right, a great deal of syntax is innate and hence not conventional. (We should note that the innate syntax will certainly lead to regularities albeit not conventional ones.) Still, the syntactic differences between public languages show that much syntax is not innate. These differences are captured, on the received Chomskian view, by different settings of "parametric values". What could explain these different settings other than different conventions in different linguistic communities? Indeed, more fundamentally, what else but conventions could explain *the uniform settings within the one community*? Thus, most people in the USA participate in parameter-setting conventions that lead them to speak an

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[15] A dolphin, Akeakamai, mastered three-item relational sequences like 'Surfboard swimmer fetch' meaning that she was to bring the swimmer to the surfboard, and three-item nonrelational sequences like 'Left hoop through' meaning that she was to swim through the hoop to her left. Then, without training, she was given a four-item ('Right hoop frisby fetch') and a five-item ('Right basket left ball in') sequence which combined both syntactic structures. "Her understanding was immediate." (Herman 2006: 445)

[16] For more along this line, see Sects. 8.6, 11.4.1.

[17] I draw on lengthier criticism of Chomskian views that I have made elsewhere (2006b: 178–89; see also 2006c: 581–2, 598–605; 2008a: 217–29).

SVO language; most people in Japan participate in parameter-setting conventions that lead them to speak an SOV language.

The ubiquity of regularities in usage and linguistic conventions is even more apparent when we turn to words. Despite Mrs. Malaprop, almost any word in anyone's idiolect is conventional[18]: if we take any such word, we are almost certain to find that its literal linguistic meaning is the meaning it has conventionally among some, usually very large, group of people with whom the person is in communication. I emphasized that a linguistic convention, like any convention, is always a convention in a certain group (Sect. 5.2). The group for most of the linguistic conventions participated in by a monolingual native speaker of "English" will be *roughly* the same for the great majority of such conventions. It does not follow, of course, that the group for each of those conventions will be even roughly the same as that for *all* such conventions. Indeed, we can expect to find considerable variation; thus, I share my meaning of 'wowser' with nearly all Australians and hardly any Americans; I share my meaning of 'chutzpah' with many Americans and few Australians; I share a meaning of 'wet' with most English, many Australians, and few Americans; I share my meanings of 'disinterested' and 'begging the question' with a rapidly diminishing sub-group of Australians, Americans, and English. In brief, I would say, in contrast to Chomsky's claim, that almost all non-innate syntax and almost all the word meanings of anyone's idiolect are conventional.

There is indeed something a little paradoxical about denying the frequency of linguistic conventions. The linguistic method of consulting the intuitions of linguists and other speakers to discover facts about a language, discussed in Chap. 2, presupposes masses of conventional regularities among them, even while allowing for some differences in idiolects. Books are written and papers are given about expressions in this or that language, all of which presuppose a great deal of regularity in usage among speakers.

Turn next to Chomsky's claim that conventions have no interesting bearing on the theory of meaning or knowledge of language. Chomsky thinks that this claim also should be a truism (1996: 48). Yet, if it were true it would seem to be at odds with what most linguists are actually doing. For, what they are mostly doing in constructing grammars is theorizing about the largely[19] conventional syntactic and

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[18] In claiming this I do not mean to deny that there may be some innate constraints on word meanings. Suppose, for example, that we are innately programmed to respond to primary linguistic data that include 'lion', 'tiger', etc. as referring to lions, tigers, etc. rather than to other possible categories in nature. This would show that the convention that 'lion' refers to lions is *partly* innately determined but it would still be the case that 'lion' refers to lions by convention. However, for ease of exposition, it will do no harm to ignore this possibility.

[19] Chomskians are rightly impressed by their innateness hypothesis and so may bridle at my use of "largely". I use it because, although theorists disagree about the list of parameters, they agree that the list is quite long. This yields literally millions of alternative settings for a language. That yields an awful lot of non-innate syntactic rules which, I'm arguing, are caused by conventions. I think this warrants the "largely", but I don't insist on that; perhaps the innate rules "count more" than the others.

semantic properties of expressions. And they are right to be doing so, in my view. Conventions are important to theory in at least two ways.

Consider first the story of language acquisition. A child acquires the language of the culture she is born into remarkably fast. How? Chomskians have argued persuasively that a lot of the syntax of the acquired language is innate and hence shared by all natural languages. But what about the rest of the syntax? And what about the lexicon? How is all that acquired? It is acquired by the child learning to participate in the linguistic conventions of her culture. Acquiring a language is almost entirely a matter of moving, under the causal influence of primary linguistic data that are (performance errors aside) instances of local linguistic conventions, from an innate "initial state" of readiness for language to a "final state" of participation in those very linguistic conventions. Indeed how else could a language be naturally acquired?

The second way in which conventions are important is related. As I have been emphasizing, an expression has its meaning in a language largely because of conventions: conventions bring about meanings Sects. (3.2.3, 5.1 and 5.2).[20] As a result, whenever one has evidence of a linguistic convention one has evidence of a linguistic meaning (Sect. 3.6).

5.5 Chomsky Against Common Languages

Chomsky is not only dubious of linguistic conventions but also of "languages" like Mandarin or English. A language in that sense has a "sociopolitical dimension" which makes it an unsuitable notion for the science of language (1986: 15–16); "the notion 'common language' has no place in efforts to understand the phenomena of language and to explain them" (1996: 47). Rather our primary concern should be, in effect, with *idiolects* (1986: 16–17, 1996: 48).[21] I think all this is quite mistaken.

First, linguistics should not be primarily concerned with idiolects. The primary concern should be, and I think is, with the meanings of linguistic expressions, hence their syntactic properties, that are *shared* in the idiolects of a group of people, shared largely because the meanings are conventional in that group but also, in the case of syntactic properties, perhaps partly because they are innate. Although the idiolects of two members of a group, *X* and *Y*, could in principle be the same, they are

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[20] So "convention-based semantics" does not lack motivation; c.f. Laurence 1996: 270.

[21] The view that linguistics should be about idiolects is standard in Chomskian circles: "in linguistic theory…the object of study is the idiolect….References to community languages, or to dialects of languages, if they are needed at all, are in any case references to derivative things, characterized loosely in terms of the overlapping idiolects of members of groups whose individuals are in frequent serious communication with each other" (Higginbotham 1989: 155); "A Chomskian (and I am one) will think…that we should reject the idea that English and French are natural languages and instead take the notion of an idiolect as basic" (Pietroski 1994: 103); "A linguistic theory is correct exactly to the extent that it is the explicit statement of a body of linguistic knowledge possessed by a designated individual language-user" (Barber 2001: 263); "the proper object of linguistic inquiry is a speaker's *idiolect*" (Smith 2001: 285).

obviously in fact always a bit different. But any meaning that *X*'s idiolect does not share with *Y*'s will typically be shared with the idiolect of someone else, *Z*, and many others. The concern of linguistics should be with meanings shared between the idiolects of *X* and *Y*, other meanings shared between the idiolects of *X* and *Z*, and so on. For, it is because of these shared meanings that the expressions play their important causal role of making people's thoughts accessible to others in communication. And it is because the meanings play this role that explaining them is so theoretically interesting. The primary concern in linguistics should not be with idiolects but with linguistic expressions that share meanings in many idiolects.

Any idiolect is a representational system that might be the object of linguistic study. But insofar as it does not share meanings with any other idiolect it is of little theoretical interest because to that extent it has little causal role. However, *the fact that there are idiosyncratic idiolects*, even if not ones as eccentric as Mrs. Malaprop's, is theoretically interesting. I shall have something to say about it in Sect. 5.6.

Second, the dismissal of concern with a "common language" like English is misguided. First, we should note that talk of such languages *seems* to be useful in linguistics, for linguistics books and articles are full of it![22] Could this talk be a mere manner of speaking that can be paraphrased away when the serious linguistic work is to be done? I think not. Take "English" as an example. Talk of "English" identifies a set of symbols with meanings largely shared by a vast group of people; those meanings are largely *conventional in that group*. And it is partly *because* these people largely share these meanings that the people produce and respond to the symbols as they do. That they speak *English* is explanatory. So studying the nature of *English* is theoretically interesting. Similarly, it is explanatory of the behavior of a colony of prairie dogs that they use a certain language X and that's why a cognitive ethologist has studied the nature of X (Slobodchikof 2002). Of course, a different group of people will motivate the study of a different "common language", perhaps Mandarin, and a different colony of prairie dogs will motivate the study of a somewhat different language Y. Sometimes, of course, the explanatorily significant classification of symbols may not be as "English" but as "Australian-English" or "the Somerset dialect of English". A "sociopolitical dimension" does occasionally intrude into such classifications but the intrusion can be resisted by linguists; for example, a linguist

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[22] Thus, picking four books almost at random from my shelves I found the following. (i) The first few pages of Haegeman's GB textbook (1994) have many uses of 'English' to classify shared meanings. Then "English" is compared with "Italian"; for example, "In Italian a subject of a subordinate clause can be moved to the main clause domain across the overt conjunction *che*, corresponding to *that*; in English this is not possible" (p. 20). And then with Spanish and French (p. 23). And so on throughout the book. (ii) A book following the generative approach is called "English Syntax" (Baker 1995). (iii) A long article, "X-bar Theory and Case Theory" (Webelhuth 1995) is full of references to various languages (and a language group); for example, "English", "German" (p. 40), "Icelandic" (p. 50), "French" (p. 51), "the Australian aboriginal language Warlpiri" (p. 65), "Japanese" (p. 66), "Arabic" (p. 76), "Welsh" (p. 78), "Breton" (p. 78), "Hindi" (p. 80), and "Romance" (p. 81). (iv) Early in Chomsky's Managua Lectures we find: "These sentences illustrate a certain feature of Spanish not shared with such similar languages as Italian" (Chomsky 1988: 12); "Here we observe a difference between English and Spanish" (p. 13).

may think that, for almost all expressions, there is no theoretical point to the politically inspired division of Serbo-Croatian into Serbian and Croatian and can simply refuse to go along (unless she is a Serb or a Croat). In any case, the point is not that linguistics should be focusing on expressions in, say, English rather than *x*-English for various values of '*x*'. (And the point is certainly not about "who gets to own" a term like 'English'.) The point is that the theoretical interest is in linguistic expressions that share meanings in the idiolects of a group of people and so that shared language has to be identified one way or another. All linguistic generalizations, save those of UG that cover *all* expressions, must make use of these identifications. So these identifications are important.

5.6   Davidson, Malapropisms, Spoonerisms, and Slips

Davidson, like Chomsky, has problems with talk of a shared language, but for very different reasons. Davidson concludes his malapropism paper with these striking words: "there is no such thing as a language…we should give up the attempt to illuminate how we communicate by appeal to conventions" (2005: 107). He thinks that the problem with talk of a shared conventional language comes from malapropisms.

I shall set aside Davidson's problem for a moment. From the perspective on language that I have been developing, I shall consider not only malapropisms but also some other linguistic phenomena that strike us as errors, often humorous ones.[23]

We have already come across the malapropism that Davidson uses as one of his delightful examples: "a nice derangement of epitaphs" (Sect. 5.1). Marga Reimer has some other delightful ones in her discussion of Davidson, including "a doggy-dog world" and "Old Timer's disease" (2004: 317). Now, in all these cases, the most likely explanation of the error is that the speaker has an idiolect that is not just different from yours or mine – probably everyone has that – but one that is (amusingly) eccentric in this small respect: her idiolect has expressions with linguistic meanings that are *not conventional at all* but are the same as the meanings of similar expressions that are conventional. What an expression literally means in her idiolect, and what she speaker means by it, differs from its conventional meaning in her community. And that's why, as I noted (Sect. 5.1), what Mrs. Malaprop "literally means, *what she says* exploiting *her* language, is *a nice arrangement of epithets*"; so that is the proposition *said* (Sect. 3.2.4).[24]

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[23] An exchange with Elmar Geir Unnsteinsson was helpful in forming my views.

[24] Davidson's opening example of Mark Singer talking about Goodman Ace is different. Singer is *imitating* Ace's malapropisms for humorous effect. As Reimer says: "Singer intends to conform to, rather than flout, conventions" (2004: 325). The literal meanings of his words are their conventional meanings not the meanings those words would have had, the joke presumes, if spoken by Ace himself.

'Malapropism' is a sufficiently vague term – the OED tells us that it refers to a "ludicrous misuse of words" (OED) – as to cover errors that are theoretically quite different from these ones that arise from eccentric idiolects. These other errors are spoonerisms or slips of the pen or tongue. One of Reimer's malapropisms is a spoonerism, for example: Tom Seaver's report on a tennis match between Bobby Riggs and "Jelly Bean King". I mentioned another example earlier of Spooner giving a sermon with many uses of 'Aristotle' instead of 'St. Paul' (Sect. 1.1). Spoonerisms differ from the earlier malapropisms in being inadvertent and having a very different etiology. 'Aristotle' does not mean St. Paul in Spooner's idiolect. Rather, Spooner has made an error in producing the expression, a performance error, with the result that the literal and conventional meaning of his 'Aristotle' differs accidentally from what he meant by it. In contrast, there was nothing accidental about Mrs. Malaprop's use of 'epitaph'; there was no performance error. And she meant by it just what it literally means in her idiolect, albeit not what it means in her community.[25]

All of these apparent errors contrast with metaphors, which don't strike us as errors at all. In a metaphor, a speaker exploits the literal and conventional meaning of the expression to intentionally mean something different. (There is some discussion of metaphors in Sects. 8.2 and 11.2.1)

There was no problem with what-is-said in cases of eccentric idiolects but what about in cases of performance errors leading to spoonerisms/slips? To answer we need to attend to an important psycholinguistic distinction between two sorts of performance errors, semantic ones and phonological ones. At the semantic stage of production, words with the appropriate meanings have to be retrieved from the speaker's lexicon and put into a syntactically appropriate frame. The retrieval of a word is "competitive" and occasionally leads to errors because "other words similar in meaning are also activated" (Vigliocco and Vinson 2003: 184). Similarly, the phonological stage may produce a word similar in sound to the desired one. "When words exchange, they exchange almost exclusively with other words from the same syntactic class…. When sounds exchange, they exchange almost exclusively with other sounds from the same phonological class" (Bock 1999: 455).

Consider semantic errors first. Spooner's sermon is a clear example. He has a thought about St. Paul and has both 'St. Paul' and 'Aristotle' in his lexicon. In the "competitive" retrieval process, 'Aristotle' somehow won. So we have a good

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[25] From this perspective, Stefano Predelli's (2010) discussion of malapropisms starts off on the wrong foot with his

Simple Picture of linguistic communication: on a particular occasion of speaking, a speaker *A* communicates a content *P* by virtue of employing expressions associated with *P* according to *R*, where *R* are the linguistic regularities appropriate for the linguistic community of which *A* is a member. (p. 329) In Mrs. Malaprop's case, the "linguistic regularities" that Predelli has in mind are, of course, those of English (p. 332). But the linguistic regularities governing her communication of *a nice arrangement of epithets* is in her idiolect not in English. *Typically*, of course, the regularities in *A*'s idiolect will be those of other members of *A*'s linguistic community, because idiolects in the community are largely shared. That is the simple picture of communication suggested by our earlier discussion. Mrs. Malaprop' utterance is interestingly *a*typical.

theoretical basis for claiming that Spooner *said* something about Aristotle whilst meaning something about St. Paul. (And this claim has the minor advantage of conforming with our intuitions.) Less entertaining examples of semantic error are common: saying 'cat' when you mean 'dog'; 'knife' when you mean 'fork'.

However, we should note that the theoretical basis for taking this line on what-is-said in a semantic error is not overwhelming. Consider normal cases of what-is-said where there is no slip. The speaker not only intentionally expresses a thought using a certain word but does so by intentionally participating in the convention for the word. The latter is not the case where there is a semantic slip: Spooner's retrieval of 'Aristotle', hence participation in the convention for that word was inadvertent. So, it would be theoretically respectable to go the other way, saying that, to the extent of the error, nothing is said. So far as I can see, nothing of theoretical significance hangs on our choice here.

Next consider phonological errors. Spooner again provides a clear example. He once walked up to a woman sitting in a church and said, "Mardon me padam, you are occupwing my pie" meaning to say "Pardon me madam, you are occupying my wing". This has to be phonological: Spooner has no words 'mardon', etc. Given my technical notion of what-is-said (Sect. 2.2.4), there is then no basis in such phonological cases for claiming that there is any proposition *said*.

But now consider another spoonerism that I have used as an example. Spooner is reputed to have once reported, "A student is hissing my mystery lectures". Clearly, Spooner *meant that* a student was missing his history lectures. I have claimed that Spooner *said that* a student was hissing his mystery lectures (2013c: 101) but I now wonder if this is right. For it to be right, the error has to be (at least partly) semantic, as in the 'Aristotle' case. But is it? It seems more likely that it is entirely phonological like the 'mardon' case. The 'hissing' case is not so *clearly* phonological, of course, because Spooner does have the words 'hissing' and 'mystery' in his lexicon (indeed, several items for 'mystery' because it is ambiguous). The key question is: Do those words play any causal role in his production? This is a subtle empirical issue!

In sum, if the error in an utterance reflects a speaker's eccentric idiolect, as it does in many malapropisms, then we should ascribe a proposition *said* that is the same as the proposition she *meant* (though different from what others in her community would conventionally mean by those words). If the error is a spoonerism/slip that is (at least partly) semantic then we have a theoretical basis for ascribing a proposition said that is different from the proposition meant. However, the basis is not overwhelming. If the slip is entirely phonological, there is no basis for ascribing a proposition said.

Against this background, I turn to Davidson's problem. Davidson introduces the term "first meaning" for the literal meaning of an utterance: "The concept applies to words and sentences as uttered by a particular speaker on a particular occasion" (2005: 91). The problems he finds for languages and conventions arise from "three plausible principles concerning first meaning in language":

(1) *First meaning is systematic*. A competent speaker or interpreter is able to interpret utterances, his own or those of others, on the basis of the semantic properties of the parts, or words, in the utterance, and the structure of the utterance. For this to be possible, there must be systematic relations between the meanings of utterances.

(2) *First meanings are shared*. For speaker and interpreter to communicate successfully and regularly, they must share a method of interpretation of the sort described in (1).

(3) *First meanings are governed by learned conventions or regularities*. The systematic knowledge or competence of the speaker or interpreter is learned in advance of occasions of interpretation and conventional in character. (2005: 93)

Davidson rightly thinks that the hearer of a malapropism "has no trouble understanding the speaker in the way the speaker intends" (p. 90) and that this causes trouble for these principles, particularly for (3) (p. 96).

These principles are about the literal meanings of expressions in a particular speaker's idiolect at a particular time. As such, the principles seem roughly right in light of our discussion, but in need of qualifications and clarifications. Thus, in thinking about (2), we note that though shared conventional meanings are obviously a massive aid to communication, communication can take place without them and must have done so "in the beginning". And almost all human communication is between people who do not participate in *all* the same conventions; think of communication between Australians, Americans, and English. So, there are many differences in idiolects among people who communicate successfully. Insofar as conventional meanings are not shared, communication is simply a matter of "mind reading", but even with shared meanings mind reading is needed for disambiguation, fixing reference, and much else, as the Pragmatists have demonstrated. In thinking about (3), we note that idiolects need not come from learning conventions, as indeed Mrs. Malaprop so charmingly demonstrates. Furthermore, they can change in the very act of interpretation: a hearer comes to participate in a convention for the first time in the process of understanding an utterance, most vividly in reference borrowing a proper name (Kripke 1980).

So what is Davidson's problem? His initial statement of it is: "My problem is to describe what is involved in the idea of 'having a language'" (2005: 100). There is obviously no problem in describing the having of an idiolect. Davidson's problem is with describing the having of a *common language* like English. Well, from the perspective I have been developing, having English is a vague matter: a person has it, first, if she has near-enough all of the syntax of English, as revealed by grammarians; and, second, a large proportion of its vocabulary, as revealed in dictionaries, including nearly all of its basic vocabulary of the most commonly used expressions.[26] And it is appropriate to say that having English comes in degrees. But there seems to be no theoretically interesting problem about this.

Davidson's later statement of his problem is:

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[26] This stands in contrast to Lewis' view that a population *P* uses a language *L* iff there is in *P* a convention of truthfulness and trust in *L* (1983: 169).

> what interpreter and speaker share, to the extent that communication succeeds, is not learned and so is not a language governed by rules or conventions known to speaker and interpreter in advance; but what the speaker and interpreter know in advance is not (necessarily) shared, and so is not a language governed by shared rules or conventions. (pp. 105–6; see also p. 102).

It is true that successful communication between people typically involves more than shared conventions: it involves a deal of mind reading. And it is true that the rules or conventions of any person's language will typically differ a bit from those of any other person in her community: their idiolects will not be totally the same. But where's the problem? It is still the case that people who "speak the same language" communicate successfully in virtue of jointly participating in a massive number of conventions. And it is still the case that it is theoretically necessary to identify what they mostly share, even if a bit roughly, as, English, Croatian or whatever (Sect. 5.5). There is such a thing as a common language.

I conclude with some brief remarks on Reimer's discussion (2004). She argues that a malapropism means "what the words standardly (or 'conventionally') mean, relativized to the context of utterance" (p. 320). Karen Green (2001) has a similar response to Davidson. But the normally appropriate identification of the literal meaning of a person's expression with the conventional meaning of the expression in her community cannot be appropriate in this case because *the speaker of a malapropism is not participating in the relevant convention*. We can, of course, *assign* the utterance a meaning according to that convention, or indeed according to *any* convention for the expression in *any* language, but such an assignment does not make it the case that that expression, as produced by that speaker at that time, *has* that meaning. No more does a monkey randomly typing "Hillary wins" literally mean that Hillary wins nor does a desert wind shaping "Jesus loves you" in the sand literally mean that Jesus loves you. For more along these lines, see Chap. 7.

5.7 Conclusion

Conventions are important to a theory of language because they are the typical *cause* of a linguistic expression having its meaning. But, contrary to what some seem to think, conventions do not *constitute* the meanings of a language. And a linguistic convention is not constituted by the regularity it usually gives rise to. The literal meaning of a word in a person's idiolect is constituted by her disposition to associate the word with that meaning in the production and comprehension of language. If she has that disposition *because*, in an appropriate way, other members of her community have it, then that meaning is conventional (Sect. 5.1 and 5.2).

John Collins, a follower of Chomsky, is highly critical of appeals to conventions in explaining language. Indeed, he doubts that talk of conventions could be theoretically respectable anywhere until it has been fully explained. I argued against this view (Sect. 5.3). Chomsky himself claims that the "regularities in usage" needed for linguistic conventions "are few and scattered". Furthermore, such conventions as

there are do not have "any interesting bearing on the theory of meaning or knowledge of language". I argued that these claims are very mistaken: there are many linguistic conventions and these are central to explaining language (Sect. 5.4 and 5.5).

The chapter concluded by discussing malapropisms and related phenomena in responding critically to Davidson's claim that "there is no such thing as a language…we should give up the attempt to illuminate how we communicate by appeal to conventions" (Sect. 5.6).

I turn now to the interestingly different views of Bach and Neale on what is said.

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Chapter 6
Bach and Neale on "What Is Said"

Kent Bach and Stephen Neale are two leading philosophers on the semantics-pragmatics issue. They have staked out interesting, distinctive, and detailed positions from which I have learnt a lot. But we have some fundamental disagreements. Central ones are over "what is said". Bach has a more austere notion than mine. I shall consider that first. Neale is strongly opposed to notions like Bach's and mine which he calls "transcendental". Underlying Neale's position is a radical dismissal of utterance meanings altogether. In this chapter I shall look critically at their views on these matters, thereby throwing more light on my own view. Other disagreements will be discussed in the chapters to follow.

6.1   Bach's Notion

In Chap. 3 I argued 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?[1]

   First we must attend to a verbal issue. I noted Sect. (3.3) that Bach is among the many philosophers who favor a use of 'semantic' that is narrower than mine. On that use, 'semantic' refers primarily to the properties of expression-*types* fully determined by the conventional linguistic rules of a language:

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[1] My discussion of Bach in this section and the next draws heavily on Devitt 2013f: Sects 3.1 and 3.2.

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)

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)

I saw no basis for objecting to this usage of 'semantic' (Sect. 3.3). Our theoretical interest in what-is-said by the utterance of a sentence, in my sense, motivates an interest in any property that constitutes what-is-said. Properties of an utterance's sentence-token that are determined by linguistic rules constitute a large part of what-is-said. And those properties of the token are of course shared with the type of which it is a token, So we have a strong motivation for identifying and naming those properties which Bach calls "semantic". I call them "narrow-semantic'. However, from the theoretical perspective I have urged, we also need a term for *all* the properties that constitute what-is-said, not only these ones that are of my type (i) but also those of my types (ii) and (iii). I use 'semantic' for that job. There should be no objection to that usage either.

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).[2] 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

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[2] All citations of Bach in Sects. 6.1, 6.2 and 6.3 are to this work unless otherwise indicated.

its implicature). But our expectation about Bach's notion goes surprisingly unful-filled. 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*. (p. 22; emphasis added)

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" (p. 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 proper-ties of type (i) but of types (ii) and (iii) as well, thus being just the notion I think we need?

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:

> 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 inten-tions. 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). (p. 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).[3]

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. (p. 22; emphasis added)

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[3] 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.

In light of this, I take it that Bach's what-is-said is arrived at after the speaker has resolved any ambiguities.[4]

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 "semantically incomplete", only a "propositional radical" (1994: 127). 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, 'impliciture'. This term is not to be confused with Grice's now-familiar term, 'implicature' (but, Bach notes, it concerns "roughly what Sperber and Wilson (1995) call *explicature*"; 2001: 41, n. 5). An impliciture is a matter of "saying something but communicating something else instead, something closely related to what is said" (1994: 126). The impliciture is "built out of what is said" (2001: 19). Bach gives many examples of implicitures 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 impliciture. (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) mentioned in Sect. 4.1. 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.[5] Yet a name typically has many bearers; for example, 'Bruce' is the name of all the philosophers at Monty Python's 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"

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[4] But perhaps not: Bach does not place the resolution of scope ambiguities in 'Five boys washed ten cars' into what-is-said (1998: 716).

[5] 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, 2012d). The popular alternative 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 40 years. For more on the history of this issue, see Devitt 2015c. For more against direct reference, see Devitt 1989, 1996, 2012d, 2020b: 408–414.

(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 the 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''" (p. 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.[6] 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, the reference of a name in context, just like that of a demonstrative or pronoun, does not go into what-is-said but only into an impliciture; 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 impliciture.

6.2   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, I shall argue, is what Bach's notion of what-is-said lacks.

I have argued in Sect. 3.2 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.

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[6] But see note 3.

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. (I set aside my objection that we should not talk of intentions to refer; Sect. 4.1).

Speaking English is an intentional act. This act requires participating in the conventions of English, a matter of exercising linguistic dispositions that exist because of the conventions (Sect. 5.1). 'I' in (1) refers to Bruce only because, in intentionally 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 (Sect. 4.1), as Bach and I agree. And that object is Bruce. An intentional referential 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" (2008: 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" (p. 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" (p. 18). But communicative intentions are no more essential to reference than, I argued (Sect. 4.2), they are essential to meaning. There is no good reason to think that *any* reference depends on them, 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.

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[7] 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…[8] 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

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[7] E.g. by Carston (2008). Carston criticizes the motivation for Bach's what-is-said along lines that overlap with mine.

[8] Similarly, one might add, the pure indexical 'today' might refer to this very day, this year, this epoch, or….

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 institution 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 doubt that he can meet this challenge.

Bach seems to hanker 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, and hence from what is semantic rather than pragmatic, is not well-motivated. However, my more significant disagreements with Bach on the SEM-PRAG issue (Sect. 3.5) come later.

6.3   Bach's Response

My objections to Bach's what-is-said first appeared, along with three other objections to Bach's position on the semantics-pragmatics issue, in "Good and bad Bach" (2013f) in an issue of the *Croatian Journal of Philosophy* largely devoted to Bach's work, arising out of a conference in Dubrovnik in September 2011. Bach responded to these papers, including a section, "Reply to Michael Devitt on Meaning and Reference" (2013: 232–44). This "Reply" was, as I noted in "Unresponsive Bach",

"a real disappointment" (2013g: 464).[9] One source of the disappointment was that he discussed the *topics* of two of my objections at some length without mentioning my *objections*! One of those topics was Bach's what-is-said. (The other topic was Bach's interesting notion of "standardization", to be discussed in Sect. 8.6.)[10]

I posed the crucial question then (2013f: 178), as just now, "What is the theoretical point of [Bach's] what-is-said?" And my objections then (2013f: 178–81), as just now, are explicitly to his answer to this question. Yet Bach makes the following accusation: "Devitt completely neglects the theoretical motivations underlying my semantic notion of what is said" (p. 233).[11] Well, as can be seen, I found Bach's motivation in the significance he gives to intentions, particularly communicative ones. It is my criticism of this that goes unmentioned. So perhaps Bach thinks his actual motivation is to be found elsewhere. He gives just one clue as to what the missed motivation might be: he cites a passage in which he claims to motivate

> a notion of what is said that applies uniformly to four situations: (1) where the speaker means what he says and something else as well (cases of implicature and of indirect speech acts in general), (2) where the speaker (intentionally) says one thing and means something else instead (nonliteral utterances), (3) where the speaker does not say what he intends to say, as in the misuse of a word or a slip of the tongue, and (4) where the speaker says something but doesn't mean anything at all. (2001: 17-18)

I neglected this passage for two reasons. First, because I entirely agree with it: we do need a notion of what-is-said that applies uniformly to those four situations. Second, the passage is quite beside the point because it does nothing to motivate Bach's austere what-is-said over a more generous one that most people, including me, favor. This is easily seen.

Thus, here's a demonstration that my notion applies uniformly to Bachs' four situations. (1) What the philosopher (intentionally) *said* in Grice's famous reference-writing case was something about the student's English and attendance but what the philosopher *meant* was indeed something else, the implicature (Sect. 3.2.4). (2) In a metaphor, a speaker does indeed literally *say* one thing while *meaning* something else instead (Sect. 5.6; more in Chaps 8 and 11). (3) In one of the Spoonerisms we discussed, Spooner *said* something about Aristotle but *meant* something about St. Paul (Sect. 5.6). (4) What Bach has in mind are *sayings* that are "cases of translating, reciting, or rehearsing" (p. 17) in contrast to *sayings* that are statings or assertions (Sect. 3.2.4, n. 15).

I stand by my conclusion that Bach's austere notion of what-is-said does indeed lack theoretical motivation.

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[9] This section draws on "Unresponsive Bach" (2013g).

[10] Another source of disappointment was that, although Bach mentioned two other objections, he made no serious attempt to address them. These two objections concern Modified Occam's Razor, to be discussed in Sect. 8.5, and "the tyranny of syntax", to be discussed in Sect. 10.5.

[11] He also insists, as if it settles the issue in his favor, that "what is said is…the content of a locutionary act" not an illocutionary one (2013: 233). But, as I make clear, that is my view too (2013f: 171 n. 3), and is beside the issue. The issue is over what goes into the content of the locutionary act.

6.4   Neale's Notion[12]

Neale's notion of what-is-said is introduced by a distinction:

> we should separate (i) *what A intended to say by uttering X on a given occasion*, and (ii) *what a rational, reasonably well-informed interpreter in B's shoes would think A intended to say by uttering X on that occasion* (which is not to say there are not problems with the idea of *a rational, reasonably well-informed interpreter in B's shoes*). In cases where (i) = (ii), we can talk freely about *what the speaker said*. (2005: 182)

Where what *A* intended to say agrees with what that qualified interpreter would take *A* to say then we can identify what they agree on with what A said. What about when *A* and the qualified interpreter disagree?

> In cases where (i) ≠ (ii), certainly we *could* argue about which of (i) or (ii) or some third thing has the 'right' to be called *what is said*, but what would be the point? First, what third thing distinct from (i) and (ii) could be of any significance to a theory of interpretation? There is simply no rôle for a transcendent notion of what is said upon which (i) and (ii) converge when all goes well. It is the *coincidence itself* of (i) and (ii) that constitutes success, and it is the *potential* for such coincidence, independently of some third thing, that *gives sense* to the very idea of saying….Second, why is a choice between (i) and (ii) needed in cases where (i) ≠ (ii)? Conceptually they are distinct, and *both* are needed in a theory of interpretation…There is no philosophical payoff in bestowing the honorific 'what was said' on one rather than the other, or on some third thing, when they diverge. (2005: 182)

In sum, when (i) = (ii), we can talk of what-is-said, but such talk does not seem to have much theoretical significance for Neale. When (i) ≠ (ii), the talk has no theoretical point at all. He is strongly opposed to "transcendent" notions like mine and Bach's which do not identify what-is-said with either (i) or (ii). Why?

A recent work, the long and sophisticated article, "Silent Reference" (2016),[13] provides the first step in answering. Referring to "the sheepishly passive nominal 'what is said'", Neale asks:

> "Said by *what*?"…we have three notions of saying to contend with:
> (a) *Sentence saying*: What sentence *x* said relative to a given context or utterance
> (b) *Utterance saying*: What a given utterance of *x* said
> (c) *Speaker saying*: What a speaker said in uttering *x* on a given occasion.
> …When Grice and Schiffer talk about saying…they consistently talk about what a *speaker* says in uttering *x*… (p. 293)

So Neale takes Grice and Schiffer to be talking about (c). *And that is what Neale is talking about*. In contrast, I am talking primarily about (b): what-is-said is a property of the utterance, which is the production of a token-sentence. The key to Neale's position on what-is-said is a resistance to attributing *any* meanings to utterances.

What is it, according to Neale, for an entity, whether a sentence, utterance, or speaker to "say" something? So far as I can see, Neale does not address this

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[12] I have always been puzzled by Neale's positions on the main issues discussed in the rest of the chapter. I thank him and Daniel Harris for valiant attempts to clarify those positions for me. Sorry if they think that they have failed.

[13] All citations of Neale in Sects. 6.4–6.5 are to this work unless otherwise indicated.

question. So I suppose we should take his meaning of 'say' to be its ordinary-language meaning.

A word about terminology. Neale, Bach, and I all use 'utterance' to refer to token acts of language production. Talking of 'word' in particular, Neale demonstrates nicely (pp. 262–3) that, in ordinary parlance, words like 'word', 'sentence', or 'expression', are ambiguous between types and tokens, between what are, as Neale says, "abstracta" and "concreta" (p. 363). (This sort of type-token ambiguity is common – 'flower' is another example - and exemplifies what linguists call "regular" or "systematic" polysemy, to be discussed in Sect. 11.2.3) For theoretical purposes, Neale uses such words to refer to types only. As already noted (Sect. 1.1), I use them to refer to both types and tokens and so will always disambiguate them by adding 'type' or 'token' where there is any danger of confusion.[14]

Neale has the orthodox Gricean position (as do I) that "speaker meaning is the most *basic* notion of non-natural meaning" (p. 233). He shares the interest of many in the meanings of expression-types but his lack of interest in the meanings of utterances and hence of their expression-tokens seems quite radical:

> it is unclear that there is a theoretically significant notion of utterance meaning construed as a form of non-natural meaning distinct from expression meaning and speaker meaning. Certainly some invocations of utterance meaning are the products of confusion. Statements purporting to be about *utterance meaning* are, I maintain, either (a) cloaked statements about expression meaning, speaker meaning or natural meaning, or else (b) statements that involve fatal conflations of at least two of these notions. A compositional semantic theory forming part of an account of how humans communicate with language must be an expression-semantics, a theory built on the idea that the objects of semantic composition are the meanings of *expressions* (relativized to contexts or utterances, to be sure) not the meanings of *utterances* of expressions. (p. 234)

> From the point of view of theorizing, we do not need to countenance two different notions of word—word-type and word-token. There are just *words* (which are abstract artifacts) and their *perceptible proxies*, utterances, signings, and inscriptions. (263)

So, Neale has the view that there is no theoretical role for utterance meanings.[15] He thinks talk of such meanings leads to confusion and that compositional semantics cannot be about them. He holds that a *speaker means something by* her utterance, of course, but that something is the content of her intention not the property of the utterance itself.

Turn now to what Neale says about reference:

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[14] Neale does not like this talk of types and tokens for reasons related to his view of utterance meaning and reference (p. 329, n. 21).

[15] Perhaps this is not as radical as it might seem if Georges Rey (2006a) is right (and I think he is) that the standard Chomskian view is that (apart perhaps for a few accidents) there are no external-to-the-mind entities having the usual range of linguistic properties. Rey goes on to defend this "antirealism", arguing and that our belief in the existence of these linguistic entities is the result of a "perceptual illusion" like taking a certain Kanizsa figure to be a triangle; such linguistic entities are "intentional inexistents". This argument led to the following exchange: Devitt 2006b: 184–9; Rey 2006b; Collins 2006; Smith 2006; Devitt 2006c: 597–604; Rey 2008; Devitt 2008a: 221–9.

we have three distinct *vehicles* of reference to contend with:

(a) *Speaker reference*: What a given speaker refers to in uttering expression *e* on a specific occasion;
(b) *Expression reference*: What *e* itself refers to (relative to a given context);
(c) *Utterance reference*: What a given utterance of *e* refers to.

The key notion I maintain, is speaker reference. It's fine for theory of semantic composition to operate on what it takes to be the references of expressions, but that's only because what determines the reference of an expression in the relevant sense is what the speaker is referring to with it. (p. 236)

Neale aligns himself with Strawson (1950) and Linsky (1963) in thinking that there is no "primitive notion of expression reference"[16]: we should "take the reference of an expression *x*, as uttered on a given occasion, to be whatever the *speaker* is referring to with *x* on that occasion" (p. 234). He is fine with a compositional theory talking of the reference of an expression-type on an occasion, fine with talk of (b), only because that talk is underpinned by (a), speaker reference.

Neale has a much bleaker view of talk of (c), of utterance reference, and hence of expression-token reference, the sort of talk that is rampant in my discussion (see Sect. 3.2.4, for example):

Talk about utterance reference is another matter. Firstly, there are technical difficulties involved in semantically composing properties of utterances which become acute in connection with some cases of implicit reference….Secondly, statements purporting to be about utterance reference appear to be wholly dispensable because they are either (a) cloaked statements about expression reference, speaker reference or natural meaning, or else (b) statements that conflate at least two of these notions. (p. 232)

So Neale rejects utterance reference for much the same reasons as he rejected utterance meaning: it is not needed, is problematic for compositional semantics, and leads to confusion.

Clearly I am in deep disagreement with him about the metaphysics of semantic theory. For, I take that theory *to be* the study of utterance meaning and reference (Sect. 3.3). Indeed, on my view, our theoretical interest in the meanings of expression-types *arises from* our interest in the semantic meanings of utterances hence in the semantic meanings of the tokens of those expression-types. Neale has a different reason for his interest in the type meanings:

both the meaning of a word and the syntactic position it occupies in a given sentence constrain what we can hope to assert using that sentence. So it has seemed perfectly natural to say that *sentences* have meanings too, and that their meanings are determined by principles

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[16] The popular Strawson-Linsky restriction that "people refer, expressions don't" emerged in the heyday of ordinary language philosophy and was not, to my knowledge, accompanied by any theoretical justification. So, the restriction was presumably supposed to be in accord with ordinary usage. So it is worth noting that it is *not* in accord, as a quick look at the OED shows. Thus, one of many meanings of the verb 'refer' is "to have reference or relation *to* a thing; *esp* to have allusion, to apply *to*". Among the cited entities doing the referring are: a "conclusion" (Chaucer 1386); a "word" (1647); a "phrase" (1718); "measurements" (1850); "words" (1891); see also the entries for the cognates, 'reference' and 'referential'. Neale does not rest his use of 'refer' on any appeal to ordinary usage, as we shall see.

respecting their syntactic structures and the meanings of the words of which they are composed. (p. 232)

For Neale, the theoretical role of the meanings of expression-types is solely as a *constraint* on "what we can hope to assert" with them. Neale totally rejects my view that these meanings *partly constitute* the semantic meanings of tokens of that type that occur in utterances.

So where does Neale stand on the SEM-PRAG issue, which is my way of framing the semantics-pragmatics issue (Sect. 3.5)? This is tricky. SEM and PRAG are rival views of the speaker-meanings of utterances and yet, as just emphasized Neale does not ascribe such meanings – indeed *any* meanings - to utterances at all. So his position is outside the issue as I have framed it. However, I shall now argue that he is quite wrong about utterance meanings. I will then contemplate where Neale *would* stand on the SEM-PRAG issue if he *were* to drop his rejection of utterance meanings. But that forces attention on another difficulty in assessing Neale on the SEM-PRAG issue: his just-mentioned view that expression meanings merely constrain what speakers mean. I shall argue against that view too. I shall then return to the question of his stance on the SEM-PRAG issue.

6.5   Criticisms of Neale

6.5.1   *The Metaphysics of Semantics*

I think that Neale has the metaphysics of semantic theory precisely wrong. He thinks that the theory should be ontologically committed to expression-types with meanings and not to utterances and their expression-tokens with meanings. In contrast, I think that the theory should be committed to utterances and their expression-tokens with meanings (Sect. 3.2.3) and that any apparent commitment to types having meanings should be paraphrased away.[17]

I shall now present three reasons for talking of the meanings of utterances and their tokens in our theory of language. In so doing, I shall answer two of Neale's objections to this talk: in (i) below, that it is not needed; in (ii), that it poses a technical problem for compositional semantics. I trust that my talk is not open to his third objection: confusion.

(i) **Explaining the causal role of tokens**. I draw here on Sect. 3.2. Linguistic utterances are parts of the concrete spatiotemporal world playing a striking role in

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[17] So my view is a version of "nominalism" according to Jerrold Katz's way of classifying views of what linguistic theories are about (1981, 1984, 1996). Katz labels Chomsky's view, according to which the theories are about psychological entities, "conceptualism" (which I have argued against; see Sect. 3.2.3, note 5.). The third view Katz identifies as "platonism", which takes the theories to be about abstract platonic objects. This is Katz's view. Where does Neale fit in this three-way classification? His commitment to expression-types suggest Platonism. But there are problems with that interpretation; see note 21 and accompanying text below.

the causal nexus of human lives: a great range of stimuli have roles in causing those utterances; and the utterances have roles in causing a great range of behavior. In virtue of what? What properties do they have that explains why people produce them and respond to them as they do? *The most basic task for a theory of language is to answer these questions*. And these are questions about concreta not abstracta. They are analogous to countless others in science. Scientists observe concrete entities playing causal roles and try to identify the properties they have that explain this. For example, that is what von Frisch did on observing the waggle dances of the bees and Slobodchikoff did on observing the barks of the prairie dogs. And it is what cognitive ethologists do on observing the clever caching strategies of the Western Scrub Jay (Clayton et al. 2006). The theories that scientists come up with in the latter case are about the cognitive capacities of the jays, concrete entities if ever there were. The theories that they come up with in the former cases treat human utterances, bee waggle dances, and prairie dog barks as meaningful symbols in a language. It is *because* of what these concrete representations mean in their language that the organisms in question produce them and respond to them as they do. *In sum, the basic task of the theory of language requires that we assign meanings to utterances and their token expressions to explain their causal role.*

Neale poses a "Master Question" and rightly regards theories of language that are not answering some part of it as not being "empirically substantive" (p. 230):

> **Master Question**: What general facts about our cognitive states and processes, about particular physical circumstances and social relations, about the various noises, bodily movements, and marks we are capable of producing, and about acts of producing or displaying them, explain the fact that *by* producing or displaying them we can communicate to others information about the external world or about our own beliefs, desires, plans, hopes, fears, feelings etc., express or sharpen our thoughts, and create or discharge obligations, and do such things so efficiently (i.e. so quickly, systematically, and consistently)? (p. 252)

One part of this Master Question concerns the "facts…about the various noises, bodily movements, and marks we are capable of producing". These are the facts about concrete utterances and their tokens that I see a theory of language as being primarily concerned to reveal. And those facts feature in answers to the rest of the Master Question; for example, answers about the cognitive states and processes that produce those noises and movements.

So our theory of language needs to attribute a meaning to an utterance. Neale doubts that such a meaning would be "distinct from expression meaning and speaker meaning" (p. 234). Yet, the literal utterance meaning can be different from both of those others. (a) The difference from the meaning of the expression used in the utterance is obvious if that meaning demands saturation. Thus the linguistic meaning of the word-type 'she' is such that it is used to refer to a female identified in context. The literal meaning of a word-token resulting from such a use includes reference to the female in question, but reference to that particular female is not, of course, part of the meaning of the type; similarly, the literal meaning of each token bee's dance is different from the meaning of the dance-type (Sect. 3.2.4). (b) The difference from what the speaker meant is also obvious if we think of irony or metaphor. Thus the literal meaning of a token of "She's a genius" is that the female in

question is a genius but the speaker may mean that she is stupid. I'm fond of an example that illustrates how what a speaker designates (hence means) by a name can differ from what the token conventionally (literally) designates (hence means):

> a cynical journalist, Bruce, observing General Westmoreland at his desk during the Vietnam War, comments: "Napoleon is inventing his body count." The thought underlying Bruce's use of 'Napoleon'—the thought he expresses—is grounded in Westmoreland. So he speaker-designates Westmoreland. But the journalist is participating in a convention grounded in Napoleon: he uses 'Napoleon' because he has a dependent disposition to use it to express thoughts grounded in Napoleon. So he conventionally designates Napoleon. (2015c: 126)

And a difference can even arise with singular referring devices like 'she', 'that cat', and 'this book' that have descriptive elements, as we shall see (Chapters 9 and 12).

In sum, we need to ascribe a literal meaning to the token expression in an utterance to explain the causal role of the utterance. That meaning can differ from both the type meaning and the speaker meaning.

(ii) **The redundancy of types**. A great deal of linguistic theory and its application can be taken straightforwardly to be talking about expression-tokens. Consider these bits of theory, for example, taken from a typical discussion of binding, case and *wh*-movement:

> An anaphor must be bound by another expression in its governing category.
> A pronoun must not be bound by another expression in its governing category.
> Accusative case is assigned by a governing verb or preposition.
> A verb which fails to assign accusative case fails to theta-mark an external argument.
> Movement cannot cross more than one bounding mode.

Such claims about anaphors, pronouns, verbs, prepositions, and the like all appear to be concerned, without any re-interpretation, with the properties of expression-tokens in a language. ('Anaphor', 'pronoun', etc., like 'word' and 'expression' (Sects. 1.1, 6.4), are ambiguous between types and tokens.) And when such theoretical claims are applied to examples displayed on the pages of an article or book, there is no call to treat those examples as other than what they seem to be on their face, expression-tokens.

Despite this, theories of language may often seem to be talking of types not tokens. Neale offers this example:

> A semantic theory has two components. The lexical component specifies the meanings of individual *words*. The compositional component specifies the meanings of *sentences* on the basis of the meanings of the *words* they contain and principles of meaning composition that respect the syntactic configurations of those *words*. (p. 262)

And then there is a matter that is a central to his article:

> Some theories of natural language syntax do in fact postulate expressions that are often described as "phonologically null", "phonetically unrealized", "unvocalized" or "unpronounced", expressions whose presence in sentences is to be revealed by work in generative syntax….I'll call them *aphonic* expressions, or just *aphonics*, for short, a label that is meant to capture the fact that I am talking about individual expressions that are unpronounced and unheard *by their nature*, expressions that intrinsically lack phonological features or instructions for pronunciation. (p. 237)

Now it seems, as Neale emphasizes (p. 307), that the postulated aphonic – what a neat term! - has to be an expression-type, for *there is no* aphonic token: aphonics, by their very nature, go unuttered. And this apparent fact about aphonics is the technical reason for Neale's rejection of the possibility of a compositional semantics of utterances (p. 307).

I have elsewhere stated my position on the apparent commitment of linguistic theory to types as follows:

> It is often convenient to talk of the objects posited by these theories as if they were types not tokens, as if they were abstract Platonic objects, but this need be nothing more than a manner of speaking: when the chips are down the objects are parts of the spatio-temporal physical world. (2006b: 26)

Any apparent commitment to types having meanings should be construed as nothing but a convenient manner of speaking to be paraphrased away in favor of a commitment to utterances and their tokens having meanings. The paraphrasing away of apparent commitment to types is easy in the case of Neale's example of the two components of semantic theory, more difficult in the case of aphonics. We will get to that in a moment.

Why should we *want* to paraphrase away commitment to types? Because, as Occam advised, "entities are not to be multiplied beyond necessity". Utterances and their tokens certainly exist and play explanatory roles in the spatio-temporal world; see (i) above. We should not also posit types unless they play some further explanatory roles. We should not allow ourselves, like Quine's protagonist McX (1961: 9–14), to be beguiled by the use of abstract nouns that can be paraphrased away.[18]

Generative grammars often posit an aphonic in the syntactic structure of a sentence. How could this be taken to be about a sentence-token? Those tokens are made up of expressions that are uttered whereas aphonics are essentially *un*uttered. My answer (2006c: 599–600; 2008a: 217–19; 2008b: 252–4) is as follows. To posit an aphonic expression at a certain position in a sentence-type is to claim that the structure of any token of that type, the structure that is respected by "the principles of meaning composition", is *as if* that expression had been uttered at that position in the structure. Let me expand on this with some examples.

Suppose I assert:

> (I) Mary went to visit the zoo and John, the museum.

Clearly, it is *as if* I had said:

> (E) Mary went to visit the zoo and John went to visit the museum.

(I) says implicitly what (E) says explicitly. How are we to explain that? The grammar posits an aphonic (in this case an *uninscribed* expression). I construe this as claiming that the token (I) has a certain syntactic *property*. (I) contains word-tokens which have linguistic properties; for example, 'zoo' is a noun. In virtue of those

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[18] I lean toward philosophical nominalism and the rejection of abstract entities *altogether* but do not pretend to have an adequate argument for this (2010a: 13–30). My case against expression-types rests on Occam not nominalism.

properties, *and the physical arrangement of the parts that have them*, larger parts of (I) have other linguistic properties; for example, 'the zoo' is an NP; 'Mary went to visit the zoo' is a sentence. One of those other properties *arising from a particular arrangement* is that the largest part, (I) itself, is *as if* it had the part 'went to visit' as in (E) even though (I) does not in fact have that part. The talk of an aphonic boils down to this claim about the syntactic structure of tokens like (I). Thus the aphonic 'went to visit' can contribute to the meaning of (I) without being inscribed in (I). And aphonic talk should not be seen as positing a (mysterious) abstract entity in an expression-type but as describing a syntactic property of a concrete expression-token. The syntactic property of having a certain aphonic in a certain position, like the property of being a sentence or a relative clause, is encoded in an utterance but not, of course, by a word-token but rather by a certain arrangement of word-tokens. It is, we might say, a property that "emerges" from that arrangement.

Consider also the famous aphonic, PRO. The standard analysis of the string 'Bob tried to swim' has two instances of PRO, one being a copy of the other:

Bob tried [PRO to [<PRO> swim]]

Any utterance of the string is a series of word-tokens, each having a syntactic property; for example, 'Bob' is a noun. When those words with their syntactic properties are put together in that order the resulting whole has certain further syntactic properties; these further properties emerge from the combination. The most familiar of these properties is that the string is a sentence. A striking discovery is that the string is *as if* it had certain referential devices in the positions of PRO even though those devices are not inscribed in a word-token.

Finally, consider Neale's example of the utterance of a two place predication, one place being an aphonic *e* (p. 307). The whole sentence uttered provides "the character" "associated with *e*". It does so in virtue of its syntactic structure: whenever that sentence is uttered it is *as if* the token-sentence had *e* in the designated place. (In this case, the whole sentence, in others it could be a complex part of a sentence.) Given this, the whole utterance yields, and is the bearer of, "the content" of *e* in context; that content is what the utterance of *e* would have yielded in the context.

This removes Neale's "technical difficulties involved in semantically composing properties of utterances" (p. 236).[19] As Neale notes, the principles of meaning composition "respect the syntactic configurations" of words (p. 262). The impact of aphonics on the meanings of utterances is to be found in just such syntactic configurations of an utterance's word-tokens, configurations that have to be respected by the principles of meaning composition.[20]

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[19] Aside from this, I think we should be suspicious of any attempt to found a metaphysics on "technical difficulties" of this sort.

[20] It is, as Neale (2007) notes, an open empirical question whether all implicit reference is to be handled by positing aphonics in underlying syntax. Relatedly, although a compositional semantics is clearly right for a vast amount of our language, it remains to be seen just how far the compositional approach can go. These matters are discussed in Sects 10.5 and 10.6.

Interestingly, early in his discussion of aphonics, Neale himself describes, and describes well, the *as-if* response to the phenomena that prompt the positing of aphonics (p. 243). But he concludes, disappointingly:

> But why all the *as if*-ness? Why not explore the idea that the embedded subject position in these sentences really *is* occupied by a variable bound by the subject of the main verb, an *aphonic* variable with the same semantics as stilted-'self'. (p. 243)

"Why not explore the idea"? Because Occam advises against positing the existence of entities to explain phenomena that can be otherwise explained. So far as I can see, Neale never mentions *as-if*-ism again. In particular, it doesn't feature as one of his "possible ways out" of the technical problem posed by aphonics for utterance semantics (p. 308). I wonder why not.

(iii) **The fundamentality of token meanings**. Suppose, for the sake of argument, that there are expression-types. What is the nature of one? It is a certain physical kind, a kind of shape, sound, etc., with a certain meaning, constituted by linguistic rules. Now consider this question: In virtue of what does that physical kind have that meaning? Why those the rules? The answer is to be found by noting that expression-types are *abstractions from tokens, from utterances*. Neale puts this nicely:

> Utterances are physical events that take time. Expressions themselves are *abstractions* over them, abstract artefacts brought into existence by sufficiently many physical artefacts we have produced: utterances and signings (which are events) and inscriptions (which are objects). (p. 233)

What exactly do we abstract from token physical events and objects to bring the expression-type into existence? Those physical artifacts have indefinitely many properties. Which ones do we abstract? The answer is obvious: we abstract *their being* of a certain physical kind with a certain meaning, the very kind and meaning that constitute the expression-type. So the expression-type has its nature in virtue of those physical artifacts having those meanings. *So, those utterances and their expression-tokens, have those meanings*. (And, in the terminology of Sect. 1.1, what is brought into existence here is an expression *semantic*-type not a mere physical-type, which is what I am taking an expression to be.)

So even if there really were meaningful expression-types, meaningful expression-tokens would be more fundamental. As Neale brings out, the expression-types would depend on the tokens for their very existence: "Words are *abstracta* we bring into existence by means of our actions, and destroy by means of other actions or just by inaction" (p. 262).[21]

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[21] So Neale's view of these abstracta seems inconsistent with Platonism, what David Armstrong (1978) calls "transcendent realism". Is Neale's view an example of Armstrong's "immanent realism"? Probably not. Neale's view may not fit into Armstrong's classification of "theories of universals" at all. Insofar as I understand such theories – which, as an old Quinean, is probably not far – they require "particulars to partake of universals". Yet, as we have noted, Neale does not allow for any expression-tokens partaking of expression-types.

We have noted that Neale thinks of utterances as "*perceptible proxies*" for expression-types (p. 263). He goes on:

> If utterances and signings of expressions did not stand proxy for expressions, they would not be able to function in the ways they do in speakers' and signers' dealings with one another. (p. 265)

Neale's talk of "function" is alluding to the causal role of utterances and their tokens that is, I have argued, our theoretical motivation for positing meanings at all; see (i) above. But given that it is the tokens not the types that do the causal work, this proxy talk is revealingly out of place. If there were any room for proxy talk here, it would be talk of the expression-types standing proxy for the tokens from which they are abstracted.

This concludes my case against Neale's view that our semantic theory should not ascribe meanings to utterances and their tokens. I noted earlier (Sect. 6.4) that this view puts Neale right outside the SEM-PRAG issue because SEM and PRAG are rival views of the speaker-meanings of utterances. But let us suppose that Neale's position did not include that mistaken view on utterance meanings, a change which, so far as I can see, would leave every other aspect of his position untouched. Neale emphasizes, of course, that a *speaker*, uttering *E* with the intention of communicating *M*, *means M by E*. Then, given what we are now supposing, *this utterance and its token of E speaker-mean M*. Where then would Neale stand on the SEM-PRAG issue? There is a further difficulty answering.

6.5.2   The Explanatory Role of Expression Meanings

The further difficulty arises from Neale's view of the relation between the meaning of an expression-type and what a speaker means by a token of that type in a particular utterance; hence, on our supposition, from his view of the relation between the meaning of the type and the speaker-meaning of its token in the utterance. The only explanatory role he assigns to the meanings of types is that they "constrain what we can hope to assert" with those types (p. 232); hence, on our supposition, constrain what tokens of that type can mean:

> Expression meaning gets into the picture as follows: The psychological processes involved in the formation of any communicative intention with which *S* utters *x,* and in *A*'s reaching a conclusion about the content of that intention are constrained by what *S* and *A* take to be *x*'s meaning (p. 281)

Expression meanings constrain what the speaker can hope to mean because they are thought to constrain what she can intend. I looked critically at this popular idea in Sect. 4.3, concluding that intentions do not have any of the *constitutive*, or even *normative*, constraints on them of the sort that Griceans, including Neale, seem to have in mind. Still, I allowed that there may well be a *psychological* constraint on intentions. And this may be enough for Neale. In any case, my point now is that he should give expression-type meaning a much bigger role than any role it may have

in constraining what the speaker can mean; we have much stronger reasons for positing these meanings than positing their constraining role. (I accept here, for the sake of argument, the existence expression-types.)

I take it that Neale would agree with the near-truism that expression-type E has its meaning M as a result of a convention of speakers meaning M by E. Here's another convention: that of producing a well-known type of physical behavior, S, to salute Nazis. Now the fact that S has the conventional property of being a Nazi salute may well constrain the acts that X can intentionally perform with S: it would be hard for her to use S to hail a cab or wave to a friend. But S's being the conventional form of a Nazi salute has a much greater explanatory role. The *typical* production of S is by someone intentionally saluting Nazis. Consider such a typical production by X: X intentionally salutes Nazis *by* participating in that convention for S. Why that way? X saluted Nazis that way *because S* has the conventional property of being a Nazi salute. So, S's having that property *explains* what X did. And its having that property explains why the crowd roared in response to X's action. That S has the property of being a Nazi salute not only constrains what people *can do with S* but partly causes what people *typically do with S*, and *how people respond to*, S. Furthermore, because X's action had that cause her action *was* a Nazi salute. So X's action has the same conventional property as S. In sum, *S's conventional property has both a causal and, typically, a constitutive role in X's Nazi salute*.

The story for E is similar. That E means M may well constrain what X can do with E, but it has a much greater explanatory role than that. And this is so whether or not SEM or PRAG is right. Thus, suppose that SEM is right:

> **SEM**: Setting aside novel uses of language, what-is-said (in my sense) is typically the truth conditional speaker-meaning (content) of an utterance, the message conveyed. That message is typically a WIS-meaning. (Sect. 3.5)

Now, what-is-said in my sense is constituted by the property of an utterance arising from conventional expression meanings, disambiguations, and linguistically demanded saturations. For simplicity, let us ignore disambiguations and saturations here. Then, according to SEM, the *typical* production of E is by someone intentionally meaning M just as the *typical* production of S is by someone intentionally saluting Nazis. Consider such a production of E by X: X intentionally means M *by* participating in the convention for E. Why that way? X used E to mean M *because E* has the conventional property of meaning M. So, E's having that property *explains* what X did. And its having that property explains how the audience responded to X's action. That E has the property of meaning M not only constrains what people *can do with E* but partly causes what people *typically do with E*, and *how people respond to*, E.[22] Furthermore, because X's action had that cause, the token she produced *did* speaker-mean M (on our supposition). So X's token of E has the same conventional property as E, the property M. In sum, *E's conventional property has both a causal and a constitutive role in X's meaning M*.

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[22] This story of the causal role of E's meaning is inspired by the earlier story of the causal role of expression-tokens (6.5.1 (i)).

Everyone agrees, of course, that *some* uses of E are not of SEM's typical sort: a person means something other than *M*; for example, a pragmatic modulation or implicature. Indeed, PRAG holds that *all, or almost all,* uses of E are not of that allegedly typical sort.

> **PRAG**: Even setting aside novel uses of language, the truth conditional speaker-meaning (content) of an utterance, the message conveyed, is seldom, perhaps never, constituted solely by what-is-said (in my sense), a WIS-meaning. The message is always, or almost always, the result of pragmatic modification, a PM-meaning. (Sect. 3.5)

The point to make now is that whenever an utterance of E is not of SEM's allegedly typical sort, *however rare or frequent that may be*, it will be *because* of what E means that E is produced: what E means will play a role in the pragmatic modulation or implicature; and it will play a role in how an audience responds to E's production. In such a case, we obviously cannot go on to say that because of the causal role of E's meaning in the utterance of E, the utterance and its token means *M*, because it doesn't mean *M*! Nonetheless, with some modulations at least, *M* will be part of what the token means.

In conclusion, *E's meaning always plays a causal role when E is uttered*, whether SEM or PRAG is right. *And, if SEM is right, E's meaning is also typically constitutive of what a speaker means by E.* These roles for E's meaning are much more important than its constraining role.

Since that constraining role is the only role that Neale assigns to the meanings of expression-types, *prima facie* Neale would be completely opposed to this conclusion (even after allowing that utterances have speaker-meanings). But, maybe not.

It is already very clear that Neale believes that expression meanings have the constraining role. But is it clear that he believes that they have no other role? In the recently quoted passage, Neale introduces the constraining role with the following words: "Expression meaning gets into the picture as follows". These words *imply*, but *do not say*, that there is *no other way* in which that meaning gets into the picture. Still, I see no sign of his even contemplating the *causal* role that I have ascribed to that meaning. And he seems elsewhere to deny the *constitutive* role that, according to SEM, is typical. Thus, he asks the constitutive question:

> (CQ) In virtue of what facts does someone, S, mean whatever he or she means by uttering something, *x*, on a given occasion? (p. 271)

He answers:

> the content of what *S means* by uttering something *x* (or, more generally, doing something) on a given occasion is wholly determined by the communicative intentions *S* had in uttering *x*…Even linguistic meaning is left out…(271)

And later: "So where does *expression meaning* fit into this account of speaker meaning?…it plays no role whatsoever in answering" (CQ) (p. 277; see also p. 334 n. 70). So Neale seems to be decisively against my conclusion that this meaning is often constitutive. Yet it is crucial to note that the claim that what is meant is wholly constituted by the communicative intention *does not entail* that linguistic meaning has no role in constituting what is meant. *Whether or not linguistic meaning has a*

role depends on what constitutes the content of the communicative intention. Thus, Neale's answer to (CQ) immediately generates an analogous question about intentions:

(CQ)* In virtue of what facts is the content of the communicative intention of someone, *S*, whatever it is in S's uttering something, *x*, on a given occasion?

The answer to (CQ)* *might* include a constitutive role for linguistic meaning. If it did then, linguistic meaning would constitute the intention that constitutes what the speaker means and so would have a role in constituting the latter. And, according to SEM, as we have seen, an answer to (CQ)* *does* include a constitutive role for linguistic meaning: the conventional linguistic meaning of expression *E* in an utterance will typically be constitutive of the communicative intention and hence of what the speaker meant by *E*. This is, of course, at odds with Neale's decisive rejection of a constitutive role for linguistic meaning.

Aside. Consider my position. My answer to (CQ) is similar to Neale's, but where he talks of communicative intentions, I talk of thoughts expressed (Sect. 4.2). So my answer also immediately generates an analogous question. Thus, my answer is that the content of what *S* means is wholly determined (=constituted) by *the content of the thought S expresses in uttering x* (Sect. 4.2). This generates:

(CQ)** In virtue of what facts is the content of the thought expressed by someone, *S*, whatever it is in *S*'s uttering something, *x*, on a given occasion?

According to SEM, which I embrace, an answer to (CQ)** will include the claim that the conventional linguistic meaning of *E* in an utterance will typically be constitutive of the thought expressed and hence of what the speaker meant by *E*.

Now according to PRAG, the conventional linguistic meaning of *E* in an utterance will never, or hardly even, be constitutive of the thought expressed and hence of what the speaker meant by *E*. Neale's decisive rejection of a constitutive role for linguistic meaning perforce puts him in the radical extreme of that Pragmatist camp. But suppose that he withdrew that rejection – again something he could do without consequences for his other positions so far as I can see - where would he then stand on the SEM-PRAG issue? So, we are asking about Neale' position without his rejection of both utterance meanings and of a constitutive role for linguistic meanings in speaker meanings. As emphasized, I embrace SEM because I think that a great deal of polysemy should be treated as semantic ambiguity; and because I think that there are more cases of conventional linguistic meanings demanding saturations in context than is acknowledged by the Pragmatists. Would Neale join SEM in holding that speaker meanings are typically constituted by linguistic meanings after disambiguation and linguistically demanded saturations, or join PRAG in holding that they are hardly ever so constituted? Clearly the answer depends on whether he agrees with SEM or PRAG on the conventional linguistic meanings of expression types. Does he think that these types are ambiguous, or demand saturation, to the extent that SEM claims? I don't know.

Still, I note two important issues where he seems to be leaning SEM-wise, at least. (a) The Pragmatist treatment of referentially used definite descriptions is my

poster child of Pragmatist error. I argue that the case for treating these uses semantically is overwhelming (Chap. 9). Neale and I have had our differences over this but are now pretty much in agreement (Sect. 9.9). (b) I see the saturation of weather reports as a semantic matter, demanded by the language (Sect. 10.3). I take that to be Neale's position too. Indeed, my view has been influenced by his detailed discussions (Sect. 10.6). So, maybe our differences do not go much beyond the crucial ones over utterance meanings and the constitutive role of linguistic meanings in speaker meanings.

6.6   Languages and Media

Finally, Neale raises interesting issues about languages and media:

> I hold that words are medium-relative. Every word is a word in some language, and every language is a language in some medium. Words of English are intrinsically words of a *spoken* language (even though some people can produce and perceive only inscriptions of them). And words of American Sign Language (ASL) are intrinsically words of a *signed* language. It is a contingent fact that there is a writing system (orthography) for English. The writing system is a method of representing English words (and thereby larger units) by making inscriptions. (Neale 2016: 263)

We can easily identify what we might call, following the folk, "spoken-English" and "written-English"; and, I suggest, "morse-English"; "signed-English" (not to be confused with a "sign language" like ASL); "flagged-English" (naval flags); and so on. Neale's remarks prompt the following questions:

(a) Are written-English, morse-English, etc. "languages"?
(b) If they are, are they "the same language" as spoken-English, which is indubitable English, to be treated accordingly in our theory?

I can see no theoretical justification for answering (a) "No". Written-English is a representational system serving much the same functions and having the same sorts of syntactic and semantic properties as spoken-English.

What about (b)? In an earlier work (1981a: 102), I presupposed that written-English, morse-English, etc. are indeed the same language as spoken-English; they are all to be identified with English. I assume that the basis for Neale's contrary view that only spoken-English is really English is that spoken-English "comes first" for native English speakers (similarly, signed-ASL comes first for native ASL users), *and this is no accident*: it is a fact about humans that the only language that they can learn "naturally" is a spoken (or signed) one. Still, this is surely a contingent fact about organisms: there could have been an organism that naturally learnt written-English and then had to be taught spoken-English on that basis. The "natural" priority of spoken-English over the others is not a persuasive reason for treating the others theoretically as different languages.

Typically, written-L (morse-L, etc) is *built to match* spoken-L *almost* word for word (but note that there are typically ambiguities in the one that are not in the

other), and is governed by the same syntactic rules. The semantic theory of a word, spoken-W, will apply also to its matching written-W. The syntactic theory of an expression, spoken-E, will apply also to its matching written-E. And, as a matter of fact, semantic and syntactic theory proceeds as if this were the case; thus, grammars of English are presented in books using many examples of written-English, displayed on the page, that are taken to exemplify syntactic features of spoken-English. Of course, one part of linguistic theory applies only to the spoken-language: phonology. Still, phonology aside, we are surely right to treat the spoken and written as one for theoretical purposes.

What is typically the case is not always the case, however. Thus, the classical Japanese writing system, Kanji, based on characters taken from the Chinese, was not built to match spoken Japanese. A convincing case could surely not be made that the Kanji system is the same language as spoken-Japanese. And it may be difficult to make the case that the modern Japanese writing system, a combination of Kanji, Hiragana, and Katakana, is the same as spoken-Japanese.[23]

6.7   Conclusion

Kent Bach's "semantic" notion of what-is-said is more austere than mine. Like mine, his notion includes properties of an utterance arising from conventions, disambiguations, and the reference fixing of pure indexicals (and tenses) but unlike mine it excludes properties arising from the reference fixing of demonstratives, pronouns, and proper names. What a speaker says using those referential devices is "semantically incomplete", only "a propositional radical". Bach supports this austerity by claiming that the reference fixing of demonstratives, pronouns, and proper names, unlike that of pure indexicals, is dependent on speakers' communicative intentions (Sect. 6.1). I argued that this is not a theoretically sound motivation for the exclusion. The referents of pure indexicals depend as much on speakers' intentional participation in referential conventions as does the referents of demonstratives, pronouns, and names. Reference does not require communicative intentions but, even if it did, that would not support discrimination against demonstratives, pronouns, and names when including referents in what is said (Sect. 6.2).

This criticism of Bach was first presented in my "Good and bad Bach" (2013f). Bach responded (2013) without mentioning the criticism but accusing me of neglecting the motivation he has presented in a passage he cites. I argued that the passage does not motivate his austerity (Sect. 6.3).

Stephen Neale is strongly opposed to notions like Bach's and mine which he calls "transcendental". Underlying Neale's position is a radical dismissal of utterance meanings altogether. He thinks that expression-types have meanings and that a speaker means something by her utterance. However, what is meant is the content

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[23] For information on these systems, see https://en.wikipedia.org/wiki/Japanese_writing_system

of her intention *but not* a property of the utterance itself. He sees no theoretical role for utterance meanings. In particular, he claims that there is a technical difficulty in their being the subject of a compositional semantics. In contrast, my theory is committed to utterances, and their expression-tokens, having meanings (Sect. 6.4).

I presented three reasons for this position. (i) Drawing on Chap. 3 I argue that the most basic task for a theory of language is to explain in virtue of what utterances play their striking role in the causal nexus of human lives. It is for that purpose that we should ascribe meanings. And we need to ascribe them to tokens not types because it is tokens that play the causal role. (ii) Talk of types is redundant. Much linguistic theory and its application can be taken straightforwardly to be talking about expression-tokens. And parts that seem to be talking of types can be paraphrased into talk of tokens. This applies, I argue, even to talk of "aphonics", thus removing Neale's technical difficulty. (iii) Even if there were meaningful expression-types, meaningful expression-tokens would be more fundamental: the types would be abstractions from the tokens; they would depend for their very existence on the tokens (Sect. 6.5.1).

I also took issue with Neale's view that the meaning of an expression-type *constrains* what a speaker can mean by it but *never constitutes* that meaning. It is a consequence of SEM that that type meaning typically constitutes the speaker meaning (Sect. 6.5.2).

Finally, I argued, contra Neale, that written-English should mostly be treated for theoretical purposes as the same language as spoken-English (Sect. 6.6).

I turn now to confronting directly the Pragmatist challenge. Over the next two chapters I reveal two more methodological flaws of Linguistic Pragmatism. These flaws have facilitated the embrace of Linguistic Pragmatism.

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Chapter 7
Confusion of the Metaphysics of Meaning with the Epistemology of Interpretation

Check for updates

In Chap. 2 we discussed the *First Methodological Flaw of Linguistic Pragmatism*, the reliance on intuitions as evidence. In this chapter we shall consider the *Second Methodological Flaw of Linguistic Pragmatism*, the confusion of the metaphysics of meaning with the epistemology of interpretation. This confusion is presumably related somehow to the fact, pointed out in Sect. 3.3, that 'pragmatics', as commonly used, is ambiguous, referring sometimes to the study of communication and sometimes to the study of the "pragmatic" properties of utterances. The confusion is important because it encourages PRAG, the Pragmatist side of the semantics-pragmatics issue, as I construe it.

7.1   The Meaning/Interpretation Distinction[1]

Consider the "meaning-properties" of an utterance in as broad a sense as you like, covering all its "semantic" and "pragmatic" properties, in whatever preferred sense. So the properties we are concerned with include conventional properties, "what is said", "what is meant", "what is implicated", and so on, in any of the various senses of these expressions. Now, what *constitutes* the utterance having one of those properties is one thing, how the hearer *discovers* the property, another. The utterance's having the property is constituted by what the *speaker* does, by the conventions she participates in, the objects she has in mind, or messages she intentionally conveys.[2] That is where we look for the "metaphysics of meaning". And what needs

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[1] Sections 7.1 to 7.4 draw heavily on Devitt 2013d Section 2.

[2] But note two things. (1) All these meaning properties of utterances determined by the speaker are themselves ultimately constituted by the contents of thoughts. See Sect. 4.2 for some brief words on what, in turn, constitutes those contents. (2) The conventions that the speaker participates in are not of course constituted solely by her. They are constituted by the interdependent linguistic dispositions of the speech community that she is a member of; see Sect. 5.2.

emphasizing is that none of these meaning-properties is constituted *in any way at all* by what the *hearer* does in trying to *interpret* what is said or meant.[3] The hearer's problem is an epistemic one and pragmatic, in the sense of the theory of communication (Sect. 3.3). Grice made very clear that something like his "Cooperative Principle" – "Make your conversational contribution such as is required, at the stage at which it occurs, by the accepted purpose or direction of the talk exchange in which you are engaged" (1989: 26) – and its associated maxims must play a role in the hearer's decision about what the speaker implicated but did not say: using the principle and maxims, the hearer can derive the speaker's meaning by a "pragmatic inference". Later, Pragmatists have demonstrated that something like that principle – perhaps the "Principle of Relevance" (Sperber and Wilson 1995) – must play a role also in the hearer's interpretive decision about what is said. Some such principle, along with contextual clues, will guide her in figuring out what conventions the speaker is using (including what language or dialect the speaker is using),[4] what objects the speaker has in mind, and so on. The processes that the hearer uses to interpret an utterance might indeed provide *evidence* about an utterance's meaning-property – see the many discussions of the "psychological-reality requirement" to follow (Sect. 8.4, 9.4, 10.4 and 11.5) – but they do not *constitute* it. The hearer might do everything right, acting in accord with all appropriate communicative principles, and still get the wrong interpretation: she might *mis*understand.[5]

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[3] This speaker-centered view of meaning, implicit in previous chapters, flies in the face of Davidsonian "interpretationism" (1984). I have argued against this interpretationism elsewhere (1981a: 115–18; 1997a: 186–99; see also Simken 2017). It rest on an unacceptable behaviorism: "Meaning is entirely determined by observable behavior, even readily observable behavior" (Davidson 1990: 314). I don't think that this interpretationism is playing any role with the Pragmatists.

[4] Thus, for me to interpret 'shallot' in the company I keep, I need to determine whether a person is speaking English-English, Australian-English, or American-English.

[5] I made a related point in a paper long ago (1976) critical of David Lewis' solution (1970) to the problem posed by the fact that a proper name type typically has many bearers. What makes it the case that a token of such a name designates one of the bearers not another? Lewis' solution is that the reference is determined by the contextual "coordinates" of the token's production. I argue: (i) that what makes it the case that the token has its referent is a matter of what the speaker has in mind, which is in turn constituted by a certain causal relation to the referent (but see Sect. 4.1.2 above, note 4); (ii) that the coordinates – such features of the external context as time, place, and audience – have no role in making it the case that that token has that referent (except possibly playing a role in constituting what the speaker has in mind); (iii) that the role of the coordinates is epistemic, providing clues to the audience of what the speaker has in mind; (iv) that the clues might mislead and so the audience misunderstands. See also Devitt 1981a, Devitt and Sterelny 1999.

7.2   Confusing Meaning and Interpretation

In light of this distinction, we can detect a serious confusion in the literature. It is the confusion of the metaphysics of meaning with the epistemology of interpretation: a failure to keep theorizing about the properties of utterances sharply distinct from theorizing about communication. A symptom of the failure is a focus on the hearer rather than the speaker when discussing meaning.

We have noted that Pragmatists think that conventions, disambiguation, and reference fixing semantically underdetermine what is said (Sect. 3.5). According to Robyn Carston, in a helpful survey, Pragmatists believe that "pragmatic inference (that is, maxim-guided inference) is required to make up the shortfall" (2004: 67). We shall see in a moment that Carston is right that they do believe this. But this belief is badly mistaken. If there is a shortfall, it is made up, just like the standard disambiguation and reference fixing, by something non-inferential that the speaker has in mind; thus, to take a favorite example, the implicit reference in an utterance of 'It is raining' is to a location the speaker has in mind. *Pragmatic inferences, of which Gricean derivations of implicatures are an example, have absolutely nothing to do with any shortfall in the constitution of what is said.* Pragmatic inference is something the hearer may engage in to interpret what is said.

The confusion is striking in the following passage from Stephen Levinson, quoted by Carston (p. 68):

> Grice's account makes implicature dependent on a prior determination of "the said". The said in turn depends on disambiguation, indexical resolution, reference fixing, not to mention ellipsis unpacking and generality narrowing. But each of these processes, which are prerequisites to determining the proposition expressed, may themselves depend crucially on processes that look indistinguishable from implicatures. Thus what is said seems both to determine and to be determined by implicature. Let us call this *Grice's circle*….Then truth-conditional content depends on most, perhaps all, of the known species of pragmatic inference… (Levinson 2000: 186–87)

Carston endorses this interdependence of saying and implicating: it is "an inescapable issue once one accepts that the proposition expressed (what is said) is heavily dependent on pragmatics (the underdeterminacy thesis) and puts this together with the standard Gricean assumptions" (2004: 69). Yet the appearance here of a circle and interdependence arises simply from equivocation. (a) What is said (the proposition expressed) may indeed be determined, in the sense of *constituted*, by whatever disambiguates, fixes reference, unpacks ellipses, and so on. But the states of the speaker that do those jobs are not "pragmatic inferences". Pragmatic inferences determine what is said only in the *epistemic* sense that a hearer may use them to figure out what the speaker has said.[6] (b) Implicature is dependent on the "prior determination of 'the said'", according to Grice, only in the sense that the hearer's

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[6] 'Determine' is a tricky word. Aside from its constitutive and epistemic senses, it also has a causal sense; for example, "the banking collapse determined the recession." Bach lists its constitutive-epistemic ambiguity among "Some Dangerous Ambiguities" (2005: 43); see also Neale (2016: Sec. 7).

inference to *discover* what the speaker implicates starts from an assumption of what the speaker said.[7]

Consider Grice's characterization of conversational implicature:

> A man who, by (in, when) saying (or making as if to say) that *p* has implicated that *q*, may be said to have conversationally implicated that *q*, provided that (1) he is to be presumed to be observing the conversational maxims, or at least the Cooperative Principle; (2) the supposition that he is aware that, or thinks that, *q* is required in order to make his saying or making as if to say *p* (or doing so in *those* terms) consistent with this presumption; and (3) the speaker thinks (and would expect the hearer to think that the speaker thinks) that it is within the competence of the hearer to work out, or grasp intuitively, that the supposition mentioned in (2) is required.[8] (1989: 30–1)

This is tortured prose but I take it to be saying that the proposition implicated is constituted by *something the speaker thinks about* the hearer's competence in relation to the inference not by anything about the hearer. In particular, the implicature is not constituted by the hearer's competence to perform, let alone actual performance of, a pragmatic inference. Grice says that "when a conversational implicature is generated…a maxim is *exploited*" (p. 30). I take it that, for Grice, the person doing the exploiting that generates the implicature is the speaker. Certainly this sort of speaker-centered notion of implicature is what our theory needs.

This having been said, there is evidence that Grice himself may have been a bit confused about conversational implicatures. (1) When he later "take[s] stock", he claims to "have suggested", presumably in the just-quoted characterization, that "what is implicated is what is required that one assume a speaker to think in order to preserve the assumption that he is observing the Cooperative Principle (and perhaps some conversational maxims as well)" (1989: 86). Here, it seems, it is not what the speaker thinks that is constitutive but what "one", presumably a hearer, must assume about what the speaker thinks. (2) A few pages after the initial characterization, Grice remarks:

> Since, to calculate a conversational implicature is to calculate what has to be supposed in order to preserve the supposition that the Cooperative Principle is being observed, and since there may be various possible specific explanations, a list of which may be open, *the conversational implicatum in such cases will be a disjunction of such specific explanations*" (1989: 39–40; emphasis added).

It seems here as if what makes the implicatum disjunctive is the range of good explanations available to a hearer calculating the implicatum. The implicatum may well be disjunctive, but not because of this epistemic problem for the hearer but because there is *no determinate matter of fact* which of a range of propositions the speaker intends to communicate; consider, for example, Grice's reference letter case (1989: 33) or a metaphor.[9]

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[7] See, for example, the opening premise in Neale's detailed working out of the sort of inference in question (1990, pp. 82–3).

[8] Devitt 2007a, pp. 12–15, is a critical examination of this characterization.

[9] As Stephen Neale has emphasized to me. Thanks to him and Kent Bach for advice on this paragraph (which I mostly did not take).

7.3   Examples of the Confusion

Carston, Levinson, perhaps even Grice, are far from alone in confusing the meta-physics of meaning with the epistemology of interpretation. Among major players in the Pragmatism debate, so far as I can tell, only the following explicitly identify the distinction in question and insist on its observance: Kent Bach (1999, 2005), Stephen Neale (2004, 2016, Forthcoming), and Jerry Fodor and Ernie Lepore (2004). I shall now give some other examples of the confusion.

But, before I do, I want to emphasize that my claim is *not* that the authors thus criticized *always* take epistemic processes in hearers to constitute meanings, nor that it is the authors' *considered opinion* that those processes should be so taken. Indeed, one frequently comes across the idea that meanings are constituted, for example, by the intentions of speakers.[10] The problem is that authors *quite often* take meanings to be constituted by those epistemic processes.

7.3.1   Jason Stanley and Zoltan Szabó

Following her discussion of Levinson, Carston describes Stanley and Szabó's "solu-tion" to Grice's circle interdependence problem as if Stanley and Szabó (2000) share her view of the nature of this alleged problem (2004: 70). Indeed, as Bach demonstrates (2000: 270–1), Stanley and Szabó do seem to equivocate over 'deter-mine', conflating what constitutes what is said with how the hearer tells what is said.

7.3.2   Dan Sperber and Deidre Wilson

(a) In an early paper discussing Grice, Wilson and Sperber claim that "at least two aspects of what is said (what proposition is expressed) – disambiguation and the assignment of reference – are not semantically but pragmatically determined: they are not explicitly given by semantic rule but implicitly determined by context and the maxim of relevance" (1981: 157). But the contributions of disambiguation and reference assignment to what is said comes from what the speaker has in mind not the maxim of relevance. That maxim bears on the hearer's attempt to figure out what is said. (b) Similarly, in their trail-blazing book, *Relevance*, Sperber and Wilson talk of "the pragmatic processes that contribute to explicit truth-conditional content" (p. 258; see also p. 9), having in mind inferential processes in the hearer (see also 2005: 477, 494). But the only processes that contribute to this content are in the speaker and they are not inferential. (c) Finally, they write as if what an utterance says is to be identified with what a hearer, using their "Principle of Relevance",

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[10] I have a problem with this talk of intentions too, of course (Sects. 4.1 and 4.2).

interprets it as saying (1995: 184–6). But a hearer might use that Principle, indeed use any pragmatic principle of interpretation, and still be mistaken about what is said.

7.3.3   François Recanati[11]

Recanati has similar views. He equates what is said "with what a *normal interpreter* would understand as being said, in the context at hand" (2004: 19; see also p. 16). But what is said by a speaker must be equated with something resulting from what *she* did. And there is no guarantee that the normal interpreter will understand what she said: the context may be so misleading that any normal interpreter would *mis*-understand. In fact, the metaphysics-epistemology confusion runs right through Recanati's discussion. Thus, he mostly writes as if what is said is constituted *by processes in the hearer*. So, he sees his disagreement with the traditional view as over *which* of these processes does the job:

> The dominant view is that the only pragmatic process that can affect truth-conditional content is saturation. No "top-down" or free pragmatic process can affect truth-conditions – such processes can only affect what the speaker means (but not what she says). (2010: 4)

In contrast, Recanati holds that the "top-down" process of "modulating" senses, of "free enrichment", also goes into what is said:

> pragmatics is appealed to not only to assign contextual values to indexicals and free variables but also to freely modulate the senses of the constituents in a top-down manner. (2010: 10)

The modulation processes that are supposed to do the job are those of the hearer in comprehension:

> we…equate what is said with (the semantic content of) the conscious output of the complex train of processing which underlies comprehension. (2004: 16)

Yet the truth of the matter is that only the speaker could modulate any sense that might go into what she says. And Recanati does not entirely lose sight of this truth (see, for example, 2004: 17, 56–7). What we have here is a genuine confusion (see, for example, 2010: 1–2).[12]

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[11] This discussion of Recanati is taken from Devitt 2013b: 89–90.

[12] Kenneth Taylor's (2001) critical discussion of Recanati also reflects the confusion by taking Recanati's distinction between "primary" and "secondary" psychological processes in the hearer as possibly constitutive of a "distinction between semantic contents and pragmatic externalities". At best, Recanati's distinction could be *evidence* for that distinction.

7.3.4   Anne Bezuidenhout

Consider Bezuidenhout's wonderfully explicit article, "Truth-Conditional Pragmatics" (2002), speaking not only for herself but for a group including Carston, Sperber and Wilson, and Recanati. She nicely describes the intuitive case for a Pragmatism that differs from the semantic tradition by embracing the underdetermination thesis: "truth-conditional content is underdetermined by sentence meaning" (p.116). She goes on to explore what else must go into determining truth-conditional content. So her concern is with the metaphysics of content/meaning. (a) Yet, in gesturing toward what does the determining, she talks of "background knowledge" that includes knowledge of "one's conversational partner", knowledge of "the general principles governing conversational exchanges", and the like. She comments that she is here "simply invoking the standard sorts of contextual factors that those working in pragmatics have thought play a role in the comprehension and production of verbal utterances" (p. 117). And so she is. But these factors bear on the epistemology of interpretation; they have nothing to do with the metaphysics of content/meaning. (b) Later she talks of "sense/content" being created in context by "a joint action of the conversational partners" (p. 118). Elsewhere she talks of content "being negotiated between…interlocutors" (2006: 9). Conversation is of course a joint action that may well involve negotiation as a speaker seeks the best way to convey messages and a hearer struggles to understand the messages. But the content of any utterance the speaker does produce is *constituted* solely be what the speaker does in producing it and not in the least by any negotiations that led to its production.

7.3.5   Robert Stainton

Stainton claims: "Non-asserted content is inferentially arrived at content" (2005: 388). Later, he claims that "'pragmatic processes' (e.g. general-purpose inference, on the basis of all available information) supposedly play a role in the determination of what is asserted."[13] In this way, "truth conditions are affected by pragmatics" (p. 445). So, Stainton thinks that inferential processes constitute *both* the non-asserted content (my "what-is-meant but not said") and the asserted content (my "what-is-said"). In fact they constitute neither. They play a role in the hearer's attempt to *discover* those contents.

Elsewhere, with Reinaldo Elugardo, Stainton notes the distinction that I am emphasizing and then continues:

> we mean to claim, in cases of sub-sentential speech, that what is asserted/stated/said is both epistemically and metaphysically determined by free pragmatic enrichment. There must be metaphysical determination at play because what is asserted, in the cases in question, is

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[13] See also the alleged role of an "all-purpose inference" in determining what is asserted (p. 389) in the passage quoted in Sect. 12.5 below.

fully propositional; but what is metaphysically determined by slot-filling and disambigu-
ated expression-meaning is something less than propositional; hence what is asserted must
supervene on some other factor, beyond these two. (Elugardo and Stainton 2004: 445)

I shall argue in Chap. 12 that this plausible claim about slot-filling in those cases is
not right. But suppose it were. Then that would be a good argument that what is
asserted in those cases supervenes "on some other factor", on something other than
what metaphysically determines my what-is-said. But it would *not* be a good argu-
ment for that other factor being *pragmatic inferences made by the hearer to epis-
temically determine* what is asserted. That is the mistake. Rather, on the view of
meaning that I am urging, that other factor would be something the speaker does
aside from what constitutes what-is-said. All the metaphysical determination comes
from speaker.

Elugardo and Stainton go on to present an interesting defense of the conflation of
the two senses of 'determine'. I shall consider this in Sect. 7.5.

7.3.6   Kepa Korta and John Perry

Finally, in a paper titled "The Pragmatic Circle" (2008), Korta and Perry ("K&P")
note the familiar fact that "we don't get to what is said without resolving ambigui-
ties, and the reference of proper names, indexicals and demonstratives." They intro-
duce the expression "near-side pragmatics" for the Gricean reasoning alleged to
resolve these. They go on:

Near-side pragmatics…is pragmatics in the service of determining, together with the
semantical properties of the words used, what was said. But this raises the specter of 'the
pragmatic circle.' If pragmatics seeks explanations for why someone said what they did,
how can there be near-side pragmatics? Gricean reasoning seems to require what is said to
get started. But then if Gricean reasoning is needed to get to what is said, we have a circle.
(p. 349)

This description of a "circle" is reminiscent of Levinson's, including the equivoca-
tion over 'determining': if the initial 'determining' claim about Gricean reasoning
is to be true it has to be epistemic, yet to generate the circle it has to be constitutive.
K&P take this spurious problem very seriously: they take it to show that what is said
cannot be "a starting point for pragmatic reasoning" (p. 351) and so they seek
another.

This point about equivocation is the crux of my earlier brief discussion of K&P
(2013d: 292–3). They have responded, trenchantly rejecting my charge that they
ignore the distinction that I am emphasizing and claiming that "it's not easy to see
why Devitt or any other should think otherwise" (2019a: 175). This is surprising.

In response to my point about the alleged "circle", K&P say:

Our description is certainly reminiscent of Levinson's. But, if there is any equivocation in
Levinson's, this is not repeated in ours. When we talk of determination here, we mean *epis-
temic* determination; that is, we are talking about the hearer's understanding process.
(2019a: 176)

I had three reasons for thinking otherwise.[14]

First, K&P's description *is* reminiscent of Levinson's. As K&P note (note 2), their 2008 paper did not cite Levinson's account of "Grice's circle" (2000: 186), but their subsequent discussions have often done so (2011: 11, 147–8, 2013: 292). In these discussions, K&P imply that their circle is *the same as* Levinson's. And the reminiscent description certainly led me to think that these alleged circles were the same. If they are the same then, of course, the criticism I made of Levinson above (Sect. 7.2) apply equally to K&P and I was right to see them as equivocating. Presumably K&P think that Levinson is not equivocating, despite the evidence that he is; or that their circle is not his, despite what they imply; or both.

Second, consider K&P's earlier talk of the determination of what is said in the following passage:

> the natural dividing line between semantics and pragmatics is based on the intuitive concept of *what is said*. Setting subtleties aside, Grice's picture is that what the speaker says is determined by the semantics of the sentence he uses, and then pragmatics takes over… (2008: 348)

I took this "natural dividing line" to be clearly about constitutive not epistemic determination. The line is between a "semantics" concerned with what is said by an utterance, *constitutively* determined by the semantics of its sentence, and a "pragmatics" concerned with the utterance's other properties, *constitutively* determined otherwise by the speaker (Sect. 3.3). As K&P put it nicely in a later work, the distinction is between "'the proposition expressed'…and implicatures and any other aspect of meaning not pertaining to the truth-conditional content of the utterance" (2013: 292). The displayed passage makes no sense if "determined" is construed epistemically.

In that later work, K&P go on to find the above distinction too simple because "figuring out the reference of many natural language expressions seems to require…pragmatic reasoning" leading to "Grice's circle"; "the processes of intention recognition that invoke the Cooperative Principle and the conversational maxims seem to be needed to determine the proposition expressed by the utterance" (2013: 292–3). This "determine" has to be epistemic for this passage to be true. But in assessing the distinction our concern should be with what *constitutively* determines the proposition expressed. So, this later discussion exemplifies, more clearly than the 2008 one, just the methodological flaw I am criticizing: the hearers' pragmatic reasoning, processes of intention recognition, and the like, do not constitute the proposition expressed.

My third reason was that only if there was equivocation could there even *appear* to be a problematic "pragmatic circle". K&P disagree:

> Grice's circle arises if we take this *general pattern for the hearer's working out of implicatures* to require the identification of what the speaker said to get started, as Grice's suggests, and at the same time we assume that the same pattern is involved in the hearer's process of understanding what is said. (2019a: 176)

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[14] My discussion draws heavily on my "Reply" (2019) to their response.

But there is no *circle*, just a *sequence* of pragmatic processes of understanding. On the Gricean picture, as K&P emphasize, the hearer first figures out what is said and, on the basis of that, goes on to figure out any implicatures. Let's go along with the idea that both these figurings fit "the same pattern". This is just to say that they are the same *type* of process, perhaps a type of inference. So *one token* of that type of process yields what is said and then, on that basis, *another token* yields the implicatures. There is no circle; just one inference providing premises for the next.

K&P also take exception to the following footnote in my earlier discussion:

> The mistaken view of what constitutes what is said seems to be suggested by Perry's much earlier discussion of weather reports (Crimmins and Perry 1989: 699–700): the suggestion is that the reference of "unarticulated constituents" is constituted by how hearers would understand the utterance in the context. (2013d: 292 n. 9).

They quote passages from Perry's introduction of his notion of unarticulated constituents – an admirable notion that I shall defend (Sect. 10.6) – and rightly point out that "the suggestion…that 'the reference of 'unarticulated constituents' is constituted by how hearers would understand the utterance in the context,'…is not there" (2019a: 177). But these passages do not include the one from Crimmins and Perry (1989) that the footnote cites, as K&P appreciate. So they turn to that article, concluding a brief discussion: "There is nothing in this article to suggest that the unarticulated constituent of a belief report is determined by the hearer's inferences on the matter" (2019a: 177). Well, there *is* something there to suggest that the unarticulated constituent of *a weather report* is determined by the hearer's inferences; it's in the passage I cite (pp. 699–700)! Consider the conclusion of that passage:

> a change in wording can affect the unarticulated constituent, even though it is not a change in an expression that designates that constituent. Suppose I am in Palo Alto talking on the phone to someone in London; we both know that it is morning in Palo Alto and evening in London. If I say, "It's exactly 11 A.M.," I will be taken to be talking about the time in Palo Alto; if I had said, in the same context, "It's exactly 8 P.M.," I would be taken to be talking about the time in London.
>
> The important principle to be learned is that a change in wording can precipitate a change in propositional constituents, even when the words do not stand for the constituents. (p. 700)

The change in the wording alone does not affect the unarticulated constituent, it affects how that constituent is interpreted. The unarticulated constituent of "It's exactly 11 A.M." would be London if the speaker, for whatever crazy reason, had London in mind, even though he would surely be taken to be speaking of Palo Alto.[15]

I emphasized earlier that my criticism of authors for taking epistemic processes in hearers to constitute meanings does not imply that those authors always do so. It seems clear from this exchange that K&P do *not* always do so and that it is not their *considered opinion* that those processes should be so taken. Nonetheless, I think I have shown that K&P have so taken them on occasions.

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[15] In a brief response, K&P say that they agree with me about this but that "there is no suggestion [in the passage quoted above] that the identity of the unarticulated constituent in those utterances is constituted by the hearer's uptake" (2019b: 184). This is very odd because the first and last sentences of that quoted passage, in context, do clearly seem to suggest just that.

7.4   A Principled Position?

In light of all this I conclude that there is a widespread and serious confusion in Linguistic Pragmatism of the metaphysics of meaning with the epistemology of interpretation.[16] Is this conclusion too hasty? Perhaps what I am calling a confusion is really a principled position. Indeed Recanati claims as much in response to my criticism:

> the mix of metaphysics and epistemology which [Devitt, Bach and Neale] detect in my writings and complain about is deliberate — to some extent at least. It is a genuine metaphysical thesis (not just the result of a confusion) that what is said by an utterance is constituted by epistemological facts about what counts as understanding that utterance. (2013: 103)

But, as he notes, he does not accompany this claim with "detailed arguments". I think that there is no place for such a metaphysical thesis.

Here are two indubitable facts about communication. (1) A speaker typically produces an utterance to convey a message to a hearer. (2) A hearer typically responds by attempting to grasp the message. Consider (1). As a result of the speaker's action the utterance will have certain properties that are relevant to her attempted communication, the sort of properties theorists call "what is said", "truth-conditional content", "implicature", and so on. Consider (2). The hearer attempts to grasp the message by identifying those properties. If all goes well, the properties he assigns to the utterance will be ones it actually has: so he will *understand* it. If all does not go well, he will assign other properties and he will *misunderstand* it. The hearer's process of assigning these properties may involve "pragmatic inferences" and a mass of background knowledge, as the Pragmatists have nicely demonstrated. But the hearer's attempt to identify the communication-relevant properties of an utterance cannot make it the case that it has those properties any more than a person's attempt to identify an echidna can make it the case that it is an echidna. An utterance has all its relevant properties in virtue of what the speaker did in producing it; in virtue of her attempt to convey the message by means of the utterance and in virtue of the consequent process of producing the utterance. The message is solely hers, constituted by her intentional act. Of course, there will be a story about what led to that intentional act rather than any other, presumably a game-theoretic story that will involve background knowledge and perhaps pragmatic inferences (Sect. 4.2.3). But that causal story is beside the point of what constitutes the message conveyed by whatever act she chooses to perform. We can go further. There will be a story about what led the hearer to assign properties to the utterance in the process of interpretation, properties that constitute the message he receives (perhaps not the one sent). And Pragmatists have told quite a lot of that story. But that causal story is beside the point of what constitutes the message received.[17]

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[16] Other examples include Green 1989: 94; Saul 2002; Capone 2010; Almog et al. 2015: 360–3.

[17] Prashant Parikh takes the metaphysics/epistemology distinction that I am emphasizing to be the distinction between "speaker meaning and addressee meaning" (2019, 68). It is not: it is the dis-

It is worth noting that the common appeal to "salience" in the semantic tradition often reflects a similar confusion. Thus it is often thought that what makes a demonstrative refer to an object is that the object is particularly salient in the context; for a recent example, consider Jeffrey King's "condition 2" (2013). (Reimer 1991a is a helpful exploration of such requirements.) Yet salience has no proper place in semantics; its place is in the theory of communication. For, salience is determined by the context that is *both external to the speaker's mind and apparent to the hearer*. So it is crucial to how the hearer *interprets*. But semantic properties like the reference of demonstratives are determined by what the *speaker* has in mind, and the only reference-determining role for the external context is its role in constituting that mental state. A speaker does have a conversational obligation to see that what she has in mind is salient or is made salient to her hearer. This is a social convention but not a semantic one. The semantic convention for a demonstrative (Sects. 3.2.4 and 4.1), as for any word, is to use it to express certain concepts. The social convention is of participating in that semantic convention only when the contents of those concepts are salient. The meaning of her demonstrative is constituted by her participation in that semantic convention in the context, whether or not that participation conforms to the social convention.

How are we to explain the Pragmatists' intrusion of claims about a hearer's interpretative processes into a theory of meaning/content? I wonder whether the mistake has arisen from failing to keep the problem of meaning – the nature of the representational properties of linguistic symbols – in clear focus and sharply distinct from the problem of communication. This failing is doubtless reflected in the earlier-noted ambiguity of 'pragmatics', sometimes referring to the theory of interpretive processes and sometimes to the theory of nonsemantic (pragmatic) properties of utterances (Sect. 3.3). For, a theory of communication must put a lot of emphasis on the hearer. We note that Sperber and Wilson's book, *Relevance* (1995), which is so influential among Pragmatists, is primarily a theory of communication rather than a theory of meaning.

7.5   Elugardo and Stainton's Defense[18]

7.5.1   Introduction

According to our discussion, the *Second Methodological Flaw* of Pragmatism is confusing the metaphysical and epistemic determinations of meaning/content. As noted (Sect. 7.3), Elugardo and Stainton ("E&S") acknowledge the distinction

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tinction between the meanings of an utterance, including its speaker meaning, and *the epistemic processes that lead to* the addressee meaning.

[18] This section modifies and expands on Devitt 2013d: Section 3. I am indebted to Rob Stainton for some trenchant criticisms which have led to several changes, though not as many as he would like!

between these sorts of determination but then, surprisingly, "do not think that it does undue harm sometimes to ignore the distinction in practice" (2004: 446)! They claim that doing so is not a "mere confusion" and that the topics are "inextricably linked":

> the two [senses of 'pragmatic determinants'] are (likely) not as independent as some might suppose. A key determinant of content, in the metaphysical sense, is speakers' intentions. Or so we assume.[19] And… we insist that the intentions that a speaker can have are importantly constrained by her reasonable expectations about what the hearer can figure out…. Thus it is that what the hearer can figure out (something epistemic), ends up constraining what a speaker can intend – which, in turn is part of the metaphysical determinants of utterance content. So, epistemic determining indirectly impacts on metaphysical determining after all. (2004: 445–6)

I have heard similar remarks from other Pragmatists. I suspect that E&S's view is quite common. It is instructive to see where this ingenious defense of the indefensible goes wrong.

I start by noting that the defense assumes that *communicative intentions* are "a key determinant of content, in the metaphysical sense". Without that popular Gricean assumption E&S's argument would not get off the ground. I have rejected the assumption, arguing that contents are determined by the intentional *expression of thoughts* (Sect. 4.2). Still, the assumption is standard among Griceans and my rejection is controversial. And, perhaps the defense could be reconstructed around the expression of thoughts. So, I shall suppose, for the sake of argument, that the assumption is true.

In the next subsection I will clarify the defense a little. In Sect. 7.5.3, I will point out how the defense fails.

7.5.2   Clarifying the Defense

1. E&S "insist that the intentions that a speaker can have are importantly constrained by her reasonable expectations about what the hearer can figure out". As noted in Sect. 4.3, it is standard among Griceans to believe that there is some such constraint, apparently constitutive or normative, on a speaker's intentions. Neale provided an example in Sect. 6.5.2. These views of constraints on communicative intentions arise from views about constraints on intentions in general. Constraints are thought to range from an astonishingly strong "positive" one – *X* cannot intend to *A* unless she will *A* – through to a much weaker "negative" one – *X* cannot intend to *A* unless *X* lacks the belief that she cannot *A*. E&S's constraint is in the middle of the range. If I am right, all of these constitutive/normative constraints are false. Still, I allowed that a weak *psychological* constraint on intentions may be in order. Maybe E&S's

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[19] I regret to say that this sentence was mistakenly omitted without indication from the passage quoted in Devitt 2013d (p. 295).

defense could be reconstructed around such a psychological constraint.[20] In any case, let us go along with E&S's constraint for the sake of argument.

2. E&S continue on from their constraint: "Thus it is that what the hearer can figure out (something epistemic), ends up constraining what a speaker can intend". It could only so constrain if we assume that speaker *S* will have enough insight into the mind of hearer *H* to know what *H* can figure out. So it seems that E&S's defense needs the following unstated premise: that what *H* can, as a matter of fact, figure out about the contents of *S*'s utterances constrains what *S* can reasonable expect that *H* can figure out. And it may *often* seem likely that if *H* could not in fact figure out the content of an utterance that *S* is contemplating then *S* would know that *H* could not. But it is surely most unlikely that this is *always* the case. Still, let us grant E&S their unstated premise.

7.5.3 The Failure of the Defense

(A) Consider what sense of 'constrain' features in the following two parts of the defense: first, in E&S's claim that what *H* can figure out ends up "constraining" what *S* can intend; second, the unstated premise that what *H* can, as a matter of fact, figure out about contents of *S*'s utterances "constrains" what *S* can reasonable expect that *H* can figure out. If these statements are to be true, then the sense of "constrain" must be *epistemic*. Suppose that *S* is contemplating a certain utterance and that, as a matter of fact, *H* would not be able to figure out its content. Then it is plausible, given *S*'s insight into *H*'s abilities, that this fact about *H* would function for *S* as *evidence against* expecting that *H* would figure out that content. So it would not be *reasonable* for *S* to expect that *H* would figure it out. But clearly, the fact that *H* would not be able to figure out the content does not *metaphysically* constrain *S*'s expectations. So that fact does not metaphysically constrain the intention that metaphysically determines content. So, the most that we may be entitled to claim is that what *H* can figure out about content is *evidence* for what could be a metaphysical determinant of content. *That* is how *H*'s epistemic determining "indirectly impacts" the metaphysical determining.

But it is no surprise that a hearer's interpretative processes can provide *evidence* about content; indeed we noted this possibility in Sect. 7.1 and will make much of it later (Sects. 8.4, 9.4, 10.4 and 11.5). The problem for E&S's defense is that this obvious fact does not begin to justify ignoring the distinction between epistemic and metaphysical determination, ignoring the distinction between the processes of discovering a content and the constitution of the meaning so discovered. Indeed, I suggest that there is *never* an occasion where we are justified in ignoring the distinction between what is *evidence for* something and what *constitutes* it.

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[20] It would be a constraint along the following lines: The less *S* believes that *H* will figure out the content of an utterance then, other things being equal, the less likely it is that *S* will intend to communicate that content to *H* with that utterance.

(B) I concluded in (A) that E&S may be entitled to claim that what *H* can figure out about content is evidence for what could be a metaphysical determinant of content. But in what sense of "content" might this claim be true? It might if "content" is construed as *the message that S is intentionally conveying, its speaker meaning*. So the claim would be that, because *S* cannot intend to convey a message that *S* does not reasonably expect *H* to figure out, what *H* can figure out about speaker meanings is evidence for what could be a metaphysical determinant of speaker meaning. That is plausible. But the *speaker* meaning/message, is not the metaphysical issue in contention because everyone pretty much agrees about the intended message in the examples that are discussed (Sect. 3.6). The metaphysical issue that *is* in contention is the constitution of a different meaning property, a convention-exploiting "*what is asserted/stated/said*" that is thought to be theoretically interesting. As noted, Linguistic Pragmatists hold that this property is "semantically underdetermined" and in need of pragmatic supplementation. What bearing does E&S's defense have on *that* issue? E&S claim that the defense applies to it: "This lesson about utterance content in general presumably applies quite directly to the determinants of what is asserted/stated/said" p. 446). So, E&S hold that, because *S* cannot intend to convey something asserted/stated/said that *S* does not reasonably expect *H* to figure out, what *H* can figure out about what is asserted/stated/said is evidence for what could be a metaphysical determinant of what is asserted/stated/said.

This is not so. First, *S* could surely intentionally communicate the message that *q* by, as a matter of fact, asserting/stating/saying that *p* even if *S* lacks any reasonable expectation that *H* could figure out the message that *q from figuring out that S is asserting/stating/saying that p*. *S* could believe that *H* might figure out the message *somehow*. Second, and more important, consider what is required for *S* to have these intentions and expectations about what is asserted/stated/said. Having them seems to require that *S* has sufficient grip on the nature of what is asserted/stated/said, and sufficient insight into *H*'s mind, to know whether the processes by which *H* understands an utterance will detect what is asserted/stated/said by the utterance. But this is hard to believe! For, the property of what is asserted/stated/said that is appropriate here is supposed to be the theoretically interesting one and it is hard to exaggerate the controversy over this among theoreticians. Indeed. that controversy is at the very center of the semantics-pragmatics debate. To what extent is the property we need "semantically underdetermined"? Is there a "shortfall" here that has to be filled "pragmatically"? If so, with what? There is vigorous debate over these theoretical issues. They are not issues that we can expect ordinary speakers to have much grip on. The little known, even by theorists, about the processes of interpretation in a hearer provide very little information about the nature of what is asserted/stated/said; see Sects. 11.3, 11.4, 11.5 and 11.6. The idea that ordinary speakers could have the knowledge the defense requires of what is asserted/stated/said, and of language processing, should be dismissed.

Stainton's discussion of a sub-sentential utterance elsewhere illustrates how little metaphysical help we can get from what we know about the processes of interpretation. He says "that someone could hold up a letter and say 'From Spain', thereby claiming, about the displayed letter, that it comes from Spain" (2005: 384–5).

And the hearer is likely to figure out that this is the message. How? Stainton describes the process as follows. The hearer starts with the thought that the speaker could not have meant to convey only the property of being from Spain. As a result the hearer "must not only recover the semantic content of the word or phrase, but must also draw on linguistic and non-linguistic information, from context, to supply…the missing object" (p. 385). This is a plausible story. But it is no help at all in choosing between the following two theories: Stainton's theory that the utterance conveys the message about the letter as a result of a *pragmatic* addition to what is asserted/stated/said; and my theory, to be presented in Chap. 12, that it conveys the message as a result of a *semantic* addition to what is asserted/stated/said demanded by a convention exploited by the speaker, the convention for "implicit demonstratives".

In sum, even if we grant – as I do not – that communicative intentions are determinant of content, at most E&S have demonstrated an evidential impact of epistemic on metaphysical determination. That is no excuse for ignoring the distinction between the two determinations. That is point (A). Further, according to (B), if we accept – which I do not – that *S* cannot intend to convey a message that *S* does not reasonably expect *H* to figure out, then perhaps we should hold that what *H* can figure out about a message provides evidence of what could be a metaphysical determinant of the *message*. But the message is not really in contention. What is in contention is what is asserted/stated/said. There is no reason to believe that, even if we accept that *S* cannot intend to convey something asserted/stated/said that *S* does not reasonably expect *H* to figure out, then we should hold that what *H* can figure out about what is asserted/stated/said provides evidence of what could be a metaphysical determinant of what is asserted/stated/said. There is no reason to believe that *S*'s expectations about *H* can provide any evidence about *that*.

In saying that the epistemology and the metaphysics are "inextricably linked", E&S exaggerate. More seriously, repeatedly calling attention to the epistemology in discussing the metaphysics is indeed a "mere confusion", given how little light discussions of a hearer's interpretative processes in fact cast on the metaphysical issue. Pragmatic inferences *always* may play an epistemic role in telling what a speaker means but *never* play a metaphysical role in constituting that meaning. Ignoring the distinction has encouraged Pragmatists to think that meanings are pragmatically constituted and hence to embrace doctrines like the one introduced in Sect. 3.5:

> **PRAG**: Even setting aside novel uses of language, the truth conditional speaker-meaning (content) of an utterance, the message conveyed, is seldom, perhaps never, constituted solely by what-is-said (in my sense), a WIS-meaning. The message is always, or almost always, the result of pragmatic modification, a PM-meaning.

The *Second Methodological Flaw* is very real. PRAG should get no support from ignoring the distinction between the metaphysics of meaning and the epistemology of interpretation.

7.6 Conclusion

There is an obvious difference between the study of the properties of utterances –
what is said and what is meant – and the study of how hearers interpret utterances.
We might say that the former study is concerned with *the metaphysics of meaning*,
the latter, with *the epistemology of interpretation* (Sect. 7.1) Yet confusion of these
two studies is almost ubiquitous in the pragmatics literature (Levinson, Carston,
Sperber and Wilson, Recanati, Bezuidenhout, Stainton, Korta and Perry). We have
noted that many Pragmatists think that conventions, disambiguation, and reference
fixing semantically underdetermine what is said. As Robyn Carston says, these
Pragmatists believe that "pragmatic inference (that is, maxim-guided inference) is
required to make up the shortfall" (2004: 67). I have argued that this is the confu-
sion. If there were a shortfall, it would be made up, just like the standard disambigu-
ation and reference fixing, by something non-inferential that the speaker has in
mind. Pragmatic inferences, of which Gricean derivations of conversational impli-
catures are an example, have absolutely nothing to do with any shortfall in the con-
stitution of what is said. Pragmatic inference is something the hearer may engage in
to interpret what is said (Sects. 7.2, 7.3 and 7.4).

Reinaldo Elugardo and Robert Stainton acknowledge the distinction between the
metaphysical (constitutive) and epistemic determination but then surprisingly claim
that it does no "undue harm sometimes to ignore the distinction in practice" (2004:
446)! They claim that doing so is not a "mere confusion" and that the topics are
"inextricably linked". I argue that their reasons for thinking this are flawed. First,
their defense of the confusion would not get off the ground without two Gricean
assumptions that I have rejected in Chap. 4: that communicative intentions deter-
mine content; second, that there is a certain constitutive/normative constraint on a
speaker's intentions. But suppose we grant these two assumptions. Elugardo and
Stainton's argument still has two failings. (A) At most it demonstrates an evidential
impact of epistemic on metaphysical determination. That is no justification for
ignoring the distinction between the two determinations. (B) There is no reason to
believe that what a hearer can figure out about what is asserted/stated/said provides
evidence of what could be a metaphysical determinant of what is asserted/stated/
said (Sect. 7.5).

Ignoring the distinction between the metaphysical and the epistemological
wrongly encourages the idea that meanings are pragmatically constituted and hence
a doctrine like PRAG.

The *First Methodological Flaw* that I identified in Chap. 2 was Pragmatism's
reliance on intuitions in theorizing about language. The discussion in this chapter
has revealed a particularly troubling reliance: taking intuitions about interpreting to
be insights into meaning whereas, in fact, these intuitions throw little if any light on
meaning.

We move on now to the *Third Methodological Flaw of Linguistic Pragmatism*,
"Modified Occam's Razor".

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Chapter 8
Modified Occam's Razor and Meaning Denialism

In the last chapter, we discussed the *Second Methodological Flaw of Linguistic Pragmatism*, the confusion of the metaphysics of meaning with the epistemology of interpretation. Earlier, in Chap. 2, we discussed the *First Methodological Flaw of Linguistic Pragmatism*, the reliance on intuitions as evidence. In this chapter we shall consider the *Third Methodological Flaw of Linguistic Pragmatism*, the embrace of "Modified Occam's Razor" (briefly, "the Razor"). We shall also look critically at Kent Bach's appeal to "standardization", another conservative strategy to exclude new linguistic meanings, a strategy for "meaning denialism". Such strategies facilitate Linguistic Pragmatism.

8.1 Embracing the Razor[1]

Grice introduced and urged "Modified Occam's Razor": "Senses are not to be multiplied beyond necessity" (1989: 47). This brings with it the authority of the original Occam's Razor: "Entities are not to be multiplied beyond necessity". And it deserves that authority if it is understood, on the model of the original, as advising against positing a sense, a conventional linguistic meaning, *unless it is needed for the best explanation* of the conveyance of a message by the utterance.[2] That is certainly one natural construal of Grice's Razor given his related suggestion that we should not allow "the supposition that a word has a further (and derivative) sense unless the supposition that there is such a sense *does some work*" (*ibid*; emphasis added). But,

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[1] The discussion in Sects. 8.1–8.4 is an expansion of those in my 2013d, Sec. 4, and 2013f, Sec. 4.1.

[2] The rationale for the original is not an a priori assumption that the world is uncluttered. The rationale is primarily epistemological. If we fail to posit entities where there are some, we are likely to come across evidence of those unacknowledged entities: our explanations are likely to be inadequate. In contrast, if we posit entities where there are none, it may be difficult to come across evidence that there are none, because with enough entities almost anything can be explained.

so far as I can see, that construal, which we should all embrace, has played hardly any role in the debate. The construal that has been active in the debate is very different and constitutes the *Third Methodological Flaw*.

Given the context in which Grice introduces the Razor, it is naturally understood as advising against positing a sense *wherever there is a pragmatic derivation of the message*, without any consideration of whether that derivation is part of the best explanation of the message. And that seems to be what Grice is advising in the following passage:

> one should not suppose what a speaker would mean when he used a word in a certain range of cases to count as a special sense of the word, if it should be predicable, independently of any supposition that there is such a sense, that he would use the word (or the sentence containing it) with just that meaning. (pp. 47–8)

Recanati, who accepts the Razor (1993: 285), construes it along those lines: the Razor requires that

> the analyst who observes that a sentence has two different interpretations when uttered in different contexts…must, if possible, ascribe this difference to a property of the context of utterance rather than to an ambiguity in the sentence itself. (2004: 157)

Similarly, Carston, who thinks that the Razor "is much used by philosophers of language, including those who give pragmatics an extensive role in establishing truth-conditional content" (2002: 218 n. 50), takes the Razor to entail

> that, instead of positing a linguistic ambiguity to account for multiple interpretations of a linguistic expression, pragmatic principles and inferences, which are independently motivated, should be employed, wherever possible. (2002: 184–5)

And she has this to say about the Razor when discussing negation in particular:

> if we can give a pragmatic account of what is going on, that is, if we can derive the understandings of negative sentences by pragmatic inference from a single semantics, then that is preferable to positing two or more senses. (p. 277)

Bach takes the Razor to show 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).

The thinking that underlies this common construal of the Razor is not limited to those who explicitly embrace the Razor. Georgia Green nicely sums up this widespread thinking as follows:

> 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)

Geoffrey Nunberg begins a well-known paper:

> "Radical pragmatics" appears to be an idea the time of whose name has come…For some, it is primarily a methodological predisposition: when in doubt about the explanation for a regularity in the use of an expression, treat it as due to pragmatic rather than semantic considerations, unless there is strong evidence to the contrary. (1979: 143)

Ingrid Lossius Falkum illustrates the coming of this idea among Pragmatists:

> Given that consideration of pragmatic factors is clearly required in order to derive the contextually appropriate meaning in

> [(13) John is a lion.
> (14) The ham sandwich is getting impatient.],

> it is unclear what is gained by introducing lexical rules here….the pragmatic mechanism(s) that allow(s) us to construct the range of metaphorical and metonymic meanings that are clearly not rule-governed, should also enable us to derive the senses in (13) and (14). (Falkum 2015: 88; see also: 93)

Charles Ruhl (1989) is an extreme example of this "meaning-minimalism". He is committed to something close to the common construal of the Razor in his "MONOSEMANTIC BIAS":

> the hypothesis that a form…has a single meaning, and that all the complicating factors that make it appear polysemic have their sources in contextual contributions to meaning. (p. viii)

> a researcher's initial efforts are directed towards determining a unitary meaning for a lexical item, trying to attribute apparent variation to other factors. (p. 4)

> a word should always be assumed to contribute as little meaning as possible to its context. (p. 8)

And D. A. Cruse (1992) concludes a nice critical discussion of Ruhl: "It is no doubt right to minimize the number of lexical entries" (p. 599).

8.2   The Falsity of the Razor (as Commonly Construed)

The Razor, as commonly construed, cannot be right because it *would make all metaphors immortal*! Metaphors are Gricean paradigms: a derivation from the conventional linguistic meaning yields an implicature that is the metaphorical speaker meaning. In time, a metaphor often "dies": an expression comes to mean conventionally what it once meant metaphorically. So, there is then an ambiguity. This is not a rare phenomenon. Indeed, a large part of a language is made up of dead metaphors.[3] Yet, of course, in each case there is still a Gricean pragmatic derivation of the new meaning from the old. Marga Reimer has a nice example (1998: 97–98). The only literal meaning of the verb 'incense' was once *make fragrant with incense* but it came to be used metaphorically to mean *make very angry*. That usage caught on, the metaphor died, and the latter became its most common conventional meaning. Yet, we can still derive the new meaning from the old, as Reimer demonstrates. Indeed, the explanation in diachronic linguistics of the origin of the expression's

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[3] Even 'dead metaphor' is a dead metaphor! And we can easily figure out its derivation. (Thanks to Jesse Rappaport.)

new meaning will *always* give a key role to that derivation. Yet, despite this permanent explanatory role for the derivation, the expression really does conventionally mean *make very angry*. Indeed, if it didn't there wouldn't be anything for diachronic linguistics to explain! What goes for 'incense' goes for countless other dead metaphors.[4]

One might well object that this misunderstands the common construal of the Razor. "It is not sufficient for the Razor's application that there be a pragmatic derivation of a meaning in the minimal sense that some such derivation caused the meaning in the first place, a cause that *linguists* are likely to work out. There has to be a derivation in the sense that *speakers and hearers are competent* to give it (Grice 1989: 31). So the Razor does not apply in the 'incense' case because most people could not give the derivation of *make very angry* from *make fragrant with incense*." Probably not (although the derivation is not too hard to figure out as Reimer demonstrates). But, first, what may be true of 'incense' is not true of many other derived meanings. As Bart Geurts remarks:

> Once a derived lexical sense has conventionalised, it often remains transparent that it is a derived sense. For example, though it is fully conventional to measure lengths in "feet," the mensural sense of the word "foot" is clearly derived from its anatomical sense. (2018: 126)

In other words, speakers are often competent to give the derivation of what is now a conventional meaning. Aside from that, once linguists have worked out a derivation, people could be *educated* to give it, perhaps by a course in diachronic linguistics. So, with education, we might *all* become competent to give the derivation. Yet, despite this, a speaker using a sentence containing 'incense' could, and likely would, *say* something about anger not incense. *The general presence of a competence to derive a meaning that was once metaphorical, even if well-known, obviously does not prevent that meaning from now being conventional*. So, as I have pointed out (2004: 284–5; 2007a: 13–14), this version of the common construal does not save the Razor.

Dead metaphors demonstrate the erroneous nature of claims by supporters of the Razor quoted above. When a word *E* has a new sense *M* as a result of a metaphor dying, a speaker's use of *E* to mean *M* could still "be predictable, independently of any supposition" that *E* has that new sense *M* (cf. Grice). It might remain possible to ascribe the differences in interpretations of utterances of a sentence containing *E* "to a property of the context of utterance rather than to an ambiguity in the sentence itself" (cf. Recanati); and possible "to account for multiple interpretations" by employing "pragmatic principles and inferences" (cf. Carston). It might still be the case that the "standard use" of a sentence to mean *M* by *E* "can be explained, with the help of conversational principles, by another standard use which uncontroversially

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[4] I first used this argument to defend the view that definite descriptions are ambiguous against the Gricean view that their referential uses are to be explained pragmatically (1997b: 127). I later developed the argument further (2004: 284–5). (Definite descriptions are discussed in the next chapter.)

corresponds to a literal meaning" (cf. Bach). Yet, obviously, metaphors do die and words become ambiguous. As commonly construed the Razor is mistaken.

The common construal loses sight of why we posit a language in the first place. As noted in Sect. 3.2.1, 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 or humans; the posit is the conclusion of an "abduction" (Sect. 3.6). 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 implicatures and the like. 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 language improves fitness.

Thomas Bontly (2005a) has brought out nicely how parsimony seems to favor senses over implicatures: "implicature accounts look, at least superficially, to multiply inferential labor, leaving it to the hearer to infer the speaker's intended meaning from the words uttered, the context, and the conversational principles" (p. 297). He develops this point, contrasting the labor with that for disambiguation:

> The worry is not…so much that implicature accounts increase the number of inferences which conversants have to perform; the issue concerns rather the complexity of these inferences. Disambiguation is a highly constrained process: in principle, one need only choose the relevant sense from a finite list represented in the 'mental lexicon'. (*ibid*)[5]

Carston has some similar thoughts. Though she favors the "'if at all possible, go pragmatic' strategy", she is dubious of Grice's Razor:

> Within a theory of utterance interpretation conceived as a matter of on-line cognitive processes, it might well be more economical to retrieve a clutch of stored senses and choose among them, than to construct an interpretation out of a single sense and contextual information, guided by principles of rational discourse. The more so if all senses of an ambiguous word are automatically activated by its phonological form (so they come very cheap), as much work in psycholinguistics seems to indicate. (2002: 218–19 n. 50)

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[5] After noting the lack of a justification for the Razor in the literature, Bontly goes on to supply one. He argues that "language acquisition is semantically conservative: children will posit new meanings for familiar words only when necessary" (2005a: 302). He claims that this assumption provides the needed justification because it "is precisely the sort of process assumption that would make [the Razor] a reasonable principle for theory choice in semantics" (p. 303). But, as Ben Phillips (2012) points out, that claim is false. Talking of the alleged ambiguity of a definite description, Phillips (who is even more skeptical of the Razor than I am) says: "The fact that the *language learner* does not need to posit a referential meaning for 'the *F*' does not mean that the *semantic theorist* does not" (p. 80). My related objection is this: semantic conservatism in learning a language is one thing, semantic conservatism in creating one is another.

(I shall return to this parsimony issue in discussing psycholinguistics in Sect. 11.4.2.)

Neale captures nicely the reasons people have "for preferring pragmatic explanations over the postulation of semantic ambiguities": "A pragmatic explanation is, in some sense *free*: the machinery is needed anyway" (1990: 80–1). This "mind-reading" machinery is indeed needed anyway to explain human communication before we had a common language, to explain many aspects of communication once we got that language, and to explain the way that language changes. But that does not bely the point that positing linguistic meanings is the best way to explain most aspects of communication.

An invention I have used before helps to demonstrate that the sort of thinking underlying the common construal is basically an enemy of language. The invention is of "the fundamentalist Gricean". This person, swept away by the common construal of the Razor, argues that

> *there are no conventional meanings at all: it is pragmatics all the way down.* Consider the time before there were languages. Then there were clearly no conventional meanings to feature in derivations of speaker meanings. Yet, presumably, there came a time when people spoke and others derived the speaker meanings of these utterances *simply* from appropriate assumptions about the context and the mind of the speaker (just as we still often have to in a foreign country). There came to be regularities in the associations of speaker meanings with sounds and, *we* are inclined to say, over time some of these "caught on" and became the first conventional meanings of a language…. But *our fundamentalist* is not inclined to say this at all: "There is no need to take these first meanings to be conventional because they can all be derived from assumptions about the context and other minds. We must always remember the word of Grice. His 'Modified Occam's Razor' tells us 'Senses are not to be multiplied beyond necessity' (1989: 47). I say unto you that *no* senses are necessary." (2004: 284)

Now I intended this argument as a *reductio ad absurdum* of the thinking behind the common construal of the Razor. But it must be acknowledged that its conclusion that there are no conventional linguistic meanings hence, in effect, no human languages, *may* have some currency. I shall look briefly at this meaning eliminativism in Sect. 10.2.

In Sect. 8.4, I shall argue that the onus is mostly more on those who deny ambiguities than on those who posit them. But, first, it is time to introduce some familiar but important distinctions.

8.3 Polysemy, Homonymy, Monosemy

Dead metaphors like 'incense' that coexist with their source meaning are examples of *polysemous* ambiguity. This occurs whenever an expression, a physical form (Sect. 1.1), has at least two *related* conventional linguistic meanings; "polysemes are etymologically and therefore semantically related, and typically originate from

metaphoric usage" (Ravin and Leacocke 2000: 2).[6] In contrast, an expression like 'bank' in English is an example of *homonymous* ambiguity: a symbol that has at least two *un*related conventional linguistic meanings.[7] Finally, expressions like 'lucrative' that have only one meaning are examples of *monosemy*. Now, it is at least arguable that some cases of polysemy should not to be treated semantically, as distinct conventional meanings that yield ambiguities, but rather as pragmatic phenomena.[8] We shall consider this in the next section.

Given the vagueness of talk of meanings being "related", we should not expect the homonymy/polysemy distinction to be sharp: as the linguists Ilse Depraetere and Raphael Salkie remark, "the boundary between accidental homonymy and motivated polysemy is sometimes hard to draw" (2017a, b: 21). And questions have been raised about the *interest* of the distinction:

> Many scholars see the distinction between polysemy and homonymy as being of little theoretical interest (e.g., Cruse, 1986; Kempson, 1977), and the significant distinction as being that between lexical ambiguity and monosemy. However, there is recent work in psycholinguistics that suggests that related and unrelated senses (or meanings) may be associated with different storage profiles (e.g., Klepousniotou and Baum, 2007; Rodd et al., 2002), although the results are to some extent conflicting (e.g., Foraker and Murphy, 2012; Klein and Murphy, 2001). (Falkum and Vicente 2015: 2)

When the focus is on the nature of language itself, the polysemy/homonymy distinction is indeed "of little theoretical interest", in my view. There are some interesting differences between polysemy and homonymy over "copredication" (Sect. 11.4.6) but, beyond that, the differences are of origin rather than of nature. When the focus is on language processing, however, we shall see in Chap. 11 that the distinction is of theoretical interest. And we will then discuss these allegedly "different storage profiles".

I have drawn the polysemy/homonymy distinction in a standard way, but we should note that some theorists think that there is more to it than this. Thus, in his book, *Truth-Conditional Pragmatics* (2010), Recanati makes the following claim:

> Even though, in polysemous expressions, the sense that has been derived from a primary sense through modification has got conventionalized, the derivation is still alive in the minds of the language users, who are aware that the distinct senses of the expression form a family. That distinguishes polysemy from homonymy" (p. 70n).

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[6] The "typically" is a bit dubious, given the extent of metonymy.

[7] But, note, 'bank' is arguably *also* polysemous: it can refer to a branch bank or to the financial institution that owns the branch, as well as to a river bank. 'Bill' is a clear example of a word that is both polysemous ('dollar bill', 'electric bill') and homonymous ('duck bill') (Li and Slevc (2016: 2).

[8] Two terminological matters. (1) Some use 'ambiguous' so that it applies only to homonyms (Klein and Murphy 2001: 260). On my usage, for a word to be ambiguous is for it to have more than one conventional linguistic meaning; so a polyseme could be ambiguous, which is important to answering the Pragmatist challenge. (2) Some talk of the "meanings" of homonyms but the "senses" of polysemes (Frisson 2015: 17–18). I treat those terms as synonyms.

In the abstract for a later article, Recanati puts his position like this: "The difference [between polysemy] and straightforward ambiguity [homonymy] is that the modulation relations between the senses are transparent to the language users" (2017: 379). The psycholinguist, Steven Frisson, makes a similar claim: "the main difference [between polysemes and homonyms] is whether current language users see a relationship between the different interpretations or not" (2009: 124 n. 2). I think not. In discussing the Razor (Sect. 8.2), we rejected the view that speakers' knowledge of the derivation of one meaning of a word from another is *sufficient* to show that the former meaning is not a conventional meaning and hence that the word is not semantically polysemous. We should also reject the view that speakers' knowledge of the relations between conventional meanings of a word is *necessary* for it be a polyseme. There is no reason to think that semantic competence must bring such knowledge about meanings (Sects. 2.7–2.8). What *essentially* distinguishes polysemy from homonymy is a matter of history (Sect. 8.4) not of something in the minds of current speakers.

This brief discussion is a sign of how problematic polysemy has seemed: "questions concerning polysemy and monosemy are some of the most fundamental in lexical semantics" (Cruse 1992: 577); "polysemy has proved notoriously difficult to treat both theoretically and empirically" (Falkum and Vicente 2015: 1). I am urging that there is much more semantic polysemy than Linguistic Pragmatists acknowledge. So I will need to confront the many objections that have been made to semantic polysemy. This I will do in Sect. 11.4.

We return now to Modified Occam's Razor and meaning denialism in general.

8.4   The Onus on Pragmatic Polysemy (1)

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, the Razor, as commonly construed, puts the explanatory onus in the wrong place. Strategies of meaning denialism like the Razor reflect a conservative climate of opinion according to which the theoretical *status quo* can be defended by simply showing how a new usage *could* be explained pragmatically as a derivation from an already accepted meaning; perhaps a pragmatic modulation, perhaps 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 both *good* and, given the demands of an abduction summarized in Sect. 3.6, *better than the semantic one*. So the following passage from Grice 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. (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. Bontly points the way to a stronger point: "One might indeed argue that the burden of proof here is on the pragmatist, not the ambiguity theorist: perhaps we ought to assume (ceteris paribus) that every regular use of an expression represents a special sense" (2005a: 300). I do argue along those lines. 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 sometimes be hard to tell whether to posit one using the methods described in Sect. 3.6, but the Razor, as commonly construed, should have no role in telling.

The application of the Razor, as commonly construed, has led Pragmatists to overlook cases of *semantic* polysemy. Pragmatists have drawn attention to hitherto unnoticed examples of the content of expressions varying in context. I shall argue that many of these expressions have several related conventional senses. Where a word is ambiguous in this way, its content in context depends on which of its senses the speaker has in mind; which is to say, its content is constituted by the content of the concept (part of the thought) that the speaker is expressing with the word (cf. Sect. 4.1.2). Then, whatever sense a particular speaker has in mind gets into the convention-governed what-is-said (in my sense, of course). So the polysemy is semantic not pragmatic.

So what form does the onus take for someone who wants to explain a polysemous phenomenon pragmatically? Suppose *E* has an encoded semantic meaning *M1* but is polysemous.[9] Speakers regularly use *E* to mean *M2*, perhaps also to mean *M3*, and so on, where these meanings are all different but conceptually related. On a particular occasion, a speaker uses *E* to mean *M2*. SEM, the doctrine that I am defending in the semantics-pragmatics dispute (Sect. 3.5), entails that the following semantic explanation is typical:

> **SEp**: *E*'s property of speaker-meaning *M2* on this occasion is semantic (in my sense). So *M2* partly constitutes what-is-said, my "WIS"-meaning (Sect. 3.5). On other occasions, the speaker meaning of *E* may be a different semantic meaning, *M3*.

PRAG, the Pragmatist side of the dispute, entails that the following rival pragmatic explanation is typical:

> **PEp**: *E*'s property of speaker-meaning *M2* on this occasion is a pragmatic modification of *E*'s semantic meaning *M1* and so is at least partly pragmatic (in my senses). So *M2* partly constitutes a pragmatic modification of what-is-said, my "PM" -meaning (Sect. 3.5). On other occasions, the speaker meaning of *E* may be a different modification, *M3*.

According to SEp, *E* is thereby ambiguous, according to PEp, not. According to SEp, the polysemy is semantic, according to PEp, pragmatic. We have seen, with

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[9] I have words mainly in mind but the range of '*E*' should be taken to cover more complex expressions.

dead metaphors and the fundamentalist *reductio*, that we cannot establish PEp as the correct explanation simply by showing that speakers and hearers are competent to give a pragmatic derivation of the speaker meaning *M*. For PEp to be a good explanation it needs to be made plausible that *this derivation has an appropriately active role in the cognitive lives of the speaker and hearer in this communication*. We can express this "psychological-reality requirement" on pragmatic polysemy as follows:[10]

> There has to be evidence of a different process in the mind of a competent speaker who, in participating in a convention for *E* in an utterance, says that *p* but i*mplicates or otherwise pragmatically implies* that *q* from that in the mind of a speaker who, in participating in another convention for *E* in an utterance, *says* that *q*. And there has to be evidence of a different process in the mind of a competent hearer who grasps that *q* when it is thus pragmatically implied from that in a hearer who grasps it when it is said.

This is what is needed for PEp to be a *good* explanation. But we also need an argument that PEp is a *better* explanation than SEp.

The psychological-reality requirement on PEp, hence on PRAG, demands the *actual presence of the derivation* in the cognitive lives of speakers and hearers. For the pragmatic explanation to be good, there would have to be regular processes in speakers and hearers that differ from the standard convention-exploiting linguistic ones, whatever they may be, involved in the use of ambiguous terms.[11] In hearers there would have to be regular processes that differ from selecting *E* meaning *M2* from the lexicon in understanding the message as what-is-said, *q*, that differ from the process of participating in a convention for *E*: there would have to be regular *mind-reading processes of detecting speakers' pragmatic modulations, Gricean implicatures, and the like in order to infer from the utterance that E, in the circumstance, means M2 not its conventional meaning M1, and hence to infer the message q from p, what-is-said*. In speakers there would have to be regular processes that differ from expressing a concept (part of a thought) with the content *M2* by selecting *E* meaning *M2* from the lexicon: there would have to be *partly mind-reading processes of selecting E to express that concept, even though E means M1 not M2 in the lexicon, partly because the speaker expects that the hearer will go through the above mind-reading process*. This is the psychological-reality requirement on PEp, hence on PRAG.

Evidence that either speaker or hearer meets this requirement would be evidence for PEp. Given how difficult it is to find out about psychological processes in general, we should expect problems in getting evidence of these non-convention-exploiting processes even where they exist. We shall see that that the evidence PEp needs is strikingly absent in the controversial cases (Sect. 11.6).

Non-convention-exploiting mind-reading processes *are* plausibly posited in paradigm cases of "particularized" conversational implicatures like Grice's reference letter (1989: 33). And consider this examples of Neale's:

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[10] My discussion of the requirement is based on an earlier one (2007a: 12–16).

[11] See Cruse (1992: 589) for a related point.

> Suppose it is common knowledge that Smith is the only person taking Jones' seminar. One evening, Jones throws a party and Smith is the only person who turns up. A despondent Jones, when asked the next morning whether his party was well attended, says,
>
> (7) Well, everyone taking my seminar turned up
>
> fully intending to inform me that only Smith attended. The possibility of such a scenario, would not lead us to complicate the semantics of 'every' with an ambiguity; i.e., it would not lead us to posit semantically distinct quantificational and referential interpretations of 'everyone taking my seminar'. (1990: 87–8)

What makes (7) such a convincing example of an implicature is that it is plausible to suppose that Jones expects that the hearer, Neale, will go through a Gricean derivation to grasp what is implicated – **Only Smith turned up** – and plausible to suppose that Neale actually does go through that derivation in grasping that implicature; it is plausible to suppose that the expectation is "psychologically real" in the speaker and the derivation, in the hearer. So, it is plausible to suppose that the processes required for a pragmatic explanation are psychologically real. These suppositions are plausible because we are often *conscious* of such psychological processes in situations of alleged particularized conversational implicatures.

It is similarly plausible that the speaker meaning of the earlier cynical journalist's, "Napoleon is inventing his body count" (Sect. 6.5.1), is to be explained pragmatically. The journalist wants to be witty and thinks to himself that that his audience will surely derive from his use of 'Napoleon' that he means Westmoreland. And, of course, the audience does consciously derive that meaning.

But there is no similar plausibility in the paradigm cases of what are supposed to be "generalized" implicatures; for example, those involving 'and' and 'some'. Consider two cases. First, suppose that, instead of (7), Jones says "Smith turned up and got drunk", implying that he *first* turned up and *then* got drunk. Second, suppose that Jones has lots of people in his seminar and responds to Neale's question by saying "Some people in my seminar turned up", implying that some in his seminar did not turn up. Now it seems rather unlikely that Jones actually has the expectation that Neale will grasp what is implied in each case *by going through the relevant Gricean derivation*; and it seems rather unlikely that Neale actually does go through it. Yet these cases are supposed to be paradigms of generalized conversational implicatures. It seems implausible that the psychological-reality requirement will be met in these cases. (We shall return to 'and' and 'some' in Sect. 11.2.6.)

But perhaps this is too hasty. Consider the processes in the hearer first. Our judgment that hearers probably do not go through the derivation in the cases thought to be paradigm generalized conversational implicatures may seem to rest on the assumption that hearers are not *conscious* of any such derivational process.[12] But maybe they are not conscious of them because they are not "rational" processors in "the central processor". The speedy automatic part of language processing, which is most of it, is subconscious and noncentral, perhaps more brute-causal than

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[12] As Bontly remarks:"we are rarely conscious of engaging in any reasoning of the sort Griceans require. Consequently, the claim that communicators actually work through all these complicated inferences seems psychologically unrealistic." (2005a: 307)

rational-causal.[13] This applies not only to syntactic and semantic processes arising simply out of "linguistic knowledge" but also to processes arising out of "world knowledge", including those mind-reading processes that disambiguate and assign referents to indexical and demonstrative expressions. So, here's a suggestion. Perhaps with a generalized implicature, the hearer typically, "without thinking", goes through what may be a fairly brute-causal mind-reading process that takes appropriate account of the clues for a Gricean derivation, maybe without even representing these *as* clues: she has become "hard-wired" to process these clues appropriately. This speedy subconscious non-central process "mirrors" the rational mind-reading process of a Gricean derivation. And, of course, this mirroring process must be very different from the convention-exploiting one of understanding *what-is-said*.[14]

Our related suggestion for the speaker is as follows. We must not insist on the speaker expecting that the hearer will consciously go through the mind-reading derivation; the speaker may typically expect that the hearer will go through a subconscious process of the sort just described. And perhaps we can weaken this. Although the speaker's attitude to the hearer may be a conscious thought, it need not be. It is sufficient that the speaker's speedy production of E is based on a "subconscious expectation" that the hearer will go through the mind-reading derivation consciously or subconsciously.

So we have a suggestion as to how to meet the psychological-reality requirement for pragmatic explanation PEp when considering a putative generalized conversational implicature. And the suggestion can be extended to meet that requirement for other pragmatic phenomena like alleged modulations.[15] But this suggestion to save PEp, hence PRAG, faces two powerful objections.

Occamist Objection: The suggestion has heavy psychological commitments that we have no reason to suppose can be met. I shall argue later (Sect. 11.6) that though psycholinguistics has discovered many interesting facts about language processing, they have not produced any evidence at all of the typical processes described above: of the partly mind-reading processes in speakers involving expectations about the hearers, whether conscious or subconscious; of the subconscious mind-reading derivational processes in hearers described above. One might reasonably respond that, despite years of impressive work by psycholinguists, we know quite little about how we process language *in general*.[16] Still we do know that there *must*

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[13] I have argued for the tentative proposal that these processes are indeed fairly brute-causal, involving no metalinguistic representations (2006b: 220–43).

[14] I made the above suggestion in trying to explicate Grice's talk of "grasp intuitively" (2007a: 15).

[15] Recanati emphasizes: "In their contextual understanding of words, users of the language are not necessarily aware of the literal meaning undergoing modulation." (2017: 381)

[16] Here are some gloomy assessments from not so long ago: "Very little is known about how [a device for sentence comprehension] might operate, though I guess that, if we started now and worked very hard, we might be able to build one in five hundred years or so" (Fodor 1975: 167); "we know so little about the actual machinery engaged in human sentence parsing" (Berwick and

be the largely subconscious convention-exploiting processes of disambiguation in speakers and hearers, even if we do not know the details. For, those are standard processes of language use. So we already know that there must be the sort of processes required by explanation SEp, the SEM rival to PEp. This is a crucial part of the background knowledge that is so important in assessing which of PEp and SEp is the better explanation of the phenomena in question (Sect. 3.6). SEp is committed to mechanisms that we already know exist, even though we are short on the details. PEp is committed to subconscious processes for which we need independent evidence before we should suppose that they exist at all. So, unless we find that evidence, we should prefer SEp. Ironically, when it comes to putative generalized conversational implicatures, whether with 'and', 'some', or whatever, the *original* Occam's Razor councils us against *Modified* Occam's Razor: we should not posit PEp's subconscious processes.

I have made this Occamist Objection before (2021). Robyn Carston (2021) is not convinced. She insists that we *do* know of the existence of the processes that PEp and PRAG require: they are exemplified frequently in "novel ad hoc uses" which, we both agree, "have to be accounted for pragmatically". So SEM "is in no better position" than PRAG "when it comes to Occamite parsimony –the (independently motivated) machinery is there in both cases." But this overlooks the crucial word "subconscious" in the Occamist Objection. We know of the pragmatic mind-reading processes in novel uses because we are conscious of them. But our concern is not with novel uses but with regular ones. We are typically not aware of any pragmatic mind-reading processes in regular uses. If those processes exist, they must be subconscious, like the speedy "automatic" convention-exploiting ones for handling homonyms. Absent independent evidence of this existence, PRAG still faces the Occamist Objection. It gets worse for PRAG: it faces the Developmental Objection.

Developmental Objection: Not only should we prefer SEp to PEp on Occamist grounds, absent evidence that PEp's new psychological processes exist, there is a good reason to suppose, a priori, that these processes do *not* exist. Consider our word *E* that conventionally meant *M1* and was then used, for the first time, to mean *M2* in a successful communication. That success depended on the speaker and hearer consciously going through the mind-reading processes we have described. The success led other speakers to follow, establishing a pattern of communicative success in using *E* to mean *M2*. *Why would this pattern not have led to the end of those demanding mind-reading processes and the development of a convention of E meaning M2*? Consider monosemous and homonymous words. It should be uncontroversial that any such word has a meaning as a result of convention (Sects. 5.3–5.6). How did this convention come about? Setting aside the occasional meaning stipulation (Sect. 5.2), we can presume that mind-reading processes in speakers and hearers using the word led to successful communications and hence to the

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Weinberg 1984: 35); the relation between the grammar and the parser "remains to be discovered" (Pritchett 1988: 539); "we know very little about the computational machinery involved in language processing" (Matthews 1991: 190–1). We know more now, of course, but I think the consensus is that we still have a very long way to go.

convention. There are two possible versions of this story. On one version, these mind-reading processes developed a new meaning from an old one which then died out. But then these processes that led to a convention are *just the same* as those for our *E* (the only difference being that *E* retained its old meaning). So why wouldn't they lead to a convention for *E*? On the other version, the processes had no old meaning to work with and people were in the situation described in the fundamentalist Gricean story: "people spoke and others derived the speaker meanings of these utterances *simply* from appropriate assumptions about the context and the mind of the speaker" (Sect. 8.2). These processes are not the same as those for our *E* but they are similar. So given that they developed into a convention why wouldn't the similar ones for *E* do so? A convention eliminates the need for the demanding, mostly conscious, mind-reading processes that brought it about. It is hard to exaggerate how much communication has benefited from the conventions that gave us monosemous and homonymous words, a syntax, and hence a language. Why would we have denied ourselves that benefit with polysemous words? To suppose that we have denied ourselves is like supposing that the graduate students who meet for drinks at O'Reilly's on Fridays (Sect. 5.2) do not do so by participating in a convention but rather by *still* go through the sort of mind-reading about what people are likely to do that developed the convention in the first place. As Lewis brought out with a range of examples (1969: 36–42), conventions enable us to co-ordinate our actions to our mutual benefit, whether in communication, meeting for drinks, rowing, and so on, *without the complicated mind-reading that brought them about*. That, we might say, is the point of conventions.

This discussion demonstrates the importance to PRAG of finding the psycholinguistic evidence that PEp needs (Sect. 11.6). For, in the absence of this evidence, consider how we should assess PEp and SEp, the rival explanations of the regular uses of polysemous words. In Sect. 3.6 I made some brief, and I hope uncontroversial, remarks about what makes for a good abduction. Criterion (A) requires that the explanation involved be "good". And that requires that the explanation be "plausible relative to background knowledge". PEp does not fare well. It posits subconscious processes that we would have no independent reason to suppose exist: the Occamist Objection. Worse, our background knowledge makes it unlikely that those processes do exist: the Developmental Objection. Absent the psycholinguistic evidence, PEp is far from plausible. SEp, in contrast, is clearly a good explanation, as good as the analogous semantic explanations of the regular uses of monosemous and homonymous words. And SEp would also meet criterion (B) of a good abduction: it would not only be better than its "*actual* alternative", PEp, but better than "any alternative that is *likely*", given what we already know about monosemous and homonymous words, and about the origins of language in general: the Developmental Objection again. Absent the psycholinguistic evidence that PEp needs, SEp provides a good abduction. We shall see in Sect. 11.6 that psycholinguistics does not provide that evidence.

According to what is perhaps the most popular version of PRAG, the encoded semantic meaning adverted to in PEp, *M1*, is not a familiar meaning but rather an

"abstract", "underspecified", "core" meaning that is hard to describe. This "abstract-core" meaning is alleged to be modified in context into more precise meanings, *M2*, *M3*, and so on. We shall see in Sect. 11.5 that this version of PRAG prompts a further objection, the "Abstract-Core Objection". This makes it even more unlikely that the required evidence for PRAG can be found and so strengthens the case for SEM.

The Occamist Objection and the Developmental Objection to pragmatic explanations like PEp will come into play when we consider alleged examples of pragmatic modulations and implicatures in the chapters to come: referentially used descriptions in Chap. 9; saturations in Chap. 10; polysemy in Chap. 11; and sub-sententials in Chap. 12. The Abstract-Core Objection will come into play in Chap. 11.

More needs to be said, of course, to defend SEM's view that (non-novel) polysemous phenomena are typically to be explained by SEp. But let me say now that I shall not be subjecting SEM's many ambiguity claims to the various diagnostic tests for ambiguity that have been proposed, notably in a classic paper by A. Zwicky and J. Sadock (1975); see also Quine (1960).[17] These tests have been famously found wanting, particularly by Dirk Geeraerts (1993); see also D. A. Cruse (1986).[18] I think that they are all dubious, but will not argue the matter. My case for an ambiguity is always that it best explains regularities in speaker meanings (Sect. 3.6).

In the rest of this chapter I shall start my defense of SEM's view of polysemous phenomena by considering two aspects of Bach's view. I shall finish the defense in Chap. 11.

8.5 Bach on the Razor

In the last section, I referred to meaning denialism, a conservative climate of opinion on the matter of linguistic meaning. Kent Bach is an extreme denialist. The crux of his interesting and distinctive position on the semantics-pragmatics issue is a methodology that has the conservative effect of keeping out new meanings and maximizing the linguistic phenomena that get placed in "pragmatics". One aspect of this conservatism is his embrace of Modified Occam's Razor, described in Sect. 8.1.

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[17] One intuitively appealing test of polysemy is the generation of zeugmas. Some lovely examples can be found in the rollicking song, "Have some madeira m'dear", from the album "At the Drop of a Hat" by Michael Flanders and Donald Swan. The song is about a "dirty old man" trying to seduce an "innocent young woman" with madeira: "She lowered her standards by raising her glass, her courage, her eyes and his hopes"; "She made no reply, up her mind, and a dash for the door".

[18] I am indebted to Justyna Grudzińska for introducing me to this debate and much else about polysemy in a workshop at the University of Szczecin in May 2011. The introduction was in "Polysemy: An Argument Against the Semantic Account" (2011), critical of a very early draft of parts of this book.

My paper, "Good and bad Bach" (2013f), made four objections to Bach's position on the semantics vs pragmatics issue. We have already presented my objection to Bach's what-is-said in Sects. 6.1–6.3. The other three objections were to Bach's conservatism, starting with his embrace of the Razor. (2013f: 181–3). This objection was an early version of the present Sects. 8.1–8.4. As already noted (Sect. 6.3), Bach's response to my paper, "Reply to Michael Devitt on Meaning and Reference" (2013: 232–44) was disappointing. One source of the disappointment was his response to my objection to his handling of the Razor. As I point out in "Unresponseive Bach" (2013g: 466–71), he makes hardly any serious attempt to address the objection.[19]

The discussion of the Razor in "Good and bad Bach" argued for the following two theses relevant to Bach: (1) that Bach embraces the Razor, as commonly construed; (2) that, so construed, the Razor is methodologically unsound. Bach's response is highly dismissive: I am accused of missed points, conflation, confusion, lack of documentation, and overlooked facts (2013: 234–236, 239–241). So Bach obviously disagrees with something! But it is not as clear as it should be precisely what. Is his target (1) or (2)? In "Unresponsive Bach", I presented some evidence that Bach, in his response, actually agrees with (2), the unsoundness of the Razor (2013g: 467–8), and he certainly does not argue against it. So I took him to be rejecting (1), that he embraces the Razor. Yet he does nothing to undermine my evidence for (1). I cite four passages from Bach that seem to be explicit endorsements of the Razor as commonly construed (2013f: 182). But actions speak louder than words and so we should consider the evidence from Bach's actual practice.

Evidence from practice that an author accepts the Razor as commonly construed is not hard to identify. The Razor, as it *should be* construed, advises against positing a new conventional meaning *unless it is part of the best explanation* of the message conveyed. The Razor, as it *commonly* construed, has no such explanatory requirement. To reject a new conventional meaning it is thought to be sufficient to show that there *is* a pragmatic derivation of the message, the idea apparently being that this derivation *can* then be part of a pragmatic explanation of the message. But no attempt is made to show, first, that the derivation *really is* part of a *good* pragmatic explanation. Showing this requires much more than a hand wave toward an explanation. It requires making it plausible that the pragmatic derivation has a place, *of the sort just specified by the psychological-reality requirement*, in the cognitive lives of current speakers and hearers (Sect. 8.4). On the common construal, no attempt is made to show, second, that any such pragmatic explanation is *better than* or, at least, *as good as* an explanation that posits a new meaning. So, whenever someone rejects a new meaning without accepting the onus of producing *both* a *good* explanation of the sort specified, involving a pragmatic derivation, *and* an argument that this

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[19] This is true also of his response to a third objection, "the tyranny of syntax", to be discussed in Sect. 10.5. The fourth objection was to Bach's interesting notion of "standardization", to be discussed in the next section.

explanation is better, we have good reason to think she subscribes to the Razor as commonly construed. Many philosophers – and linguists – are of that sort; see the citations in Sect. 8.1.

Is Bach one of them? My discussions in this book of two examples of Bach's practice show that he is. The first, discussed in the next section, is Bach's treatment of his favorite examples of "standardization". The second, discussed in Sects. 9.6–9.7, is Bach's pragmatic account of referentially used definite descriptions. In both cases, there is no sign that Bach accepts the explanatory onus that I have just described.

These two examples, together with Bach's apparently explicit endorsement of the Razor as commonly construed, make the evidence for (1) strong. Bach embraces the Razor, as commonly construed.

8.6   Bach's "Standardization"

Modified Occam's Razor is one aspect of Bach's meaning denialism, of his methodology for building a wall against neologisms. His application of his notion of *standardization* is another. I have the following objection to this application.[20]

In arguing for what is semantic, I put a great deal of evidential weight on regularities in usage (Sect. 3.6). 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. (p. 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. (p. 31)

> One shouldn't let features of stereotypical utterance contexts get incorporated into the semantics of a sentence. (p. 32)[21]

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[20] I draw on "Good and bad Bach" (2013f: 190–5).

[21] In his "Reply" to "Good and bad Bach", Bach claims that I seem to assume that in these passages "the pragmatic regularities [he] was talking about involve uses of individual expressions" rather than "complete sentences" (2013: 236). I didn't assume that.

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), in effect by PEp. 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, at least, 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, a rival to SEp. 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 mainly 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 of conventions.[22]

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". As noted in Sect. 5.2, all such propositional-attitude talk seems to give too intellectualized a picture of the mutual dependence.[23] I prefer to say simply that any speaker has her disposition *because other speakers have theirs*. (2006b: 179–80).[24] But I'll set that preference aside and go along with Bach's talk of "mutual belief".

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[22] 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 Sect. 8.2 shows that the test is inadequate because many dead metaphors would pass it.

[23] 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 (2006b, 2012b).

[24] This leaves a lot to be explained but, as I argue in Sect. 5.3, this is acceptable in the circumstances.

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 belief that it counts as such. Standardization entails no such thing. (1995: 683)[25]

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.[26] 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 the earlier-discussed example of the dead metaphor 'to incense' (Sec. 8.2). 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.

In "Good and bad Bach", I expressed the "hope that Bach will try to clear up this puzzle, showing how conventionalization entails some stronger dependence on "mutual belief" than does standardization" (2013f: 191). Bach did not oblige in his "Reply" (2013).[27]

Suppose, nonetheless, that Bach can clear up the puzzle. He would still face two tricky questions, the first, constitutive, the second, epistemic.

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[25] In "Good and Bad Bach", I noted that "Bach has lengthier accounts of conventional illocutionary acts (Bach and Harnish 1979: 109, 189–90) and standardized ones (p. 195) but these accounts are no help with the problem I raise" (2013f: 191 n. 31). In his "Reply", Bach claims that this note "ignores what was at issue there" (2013: 237), but he does nothing to show how what *was* at issue helps with the problem I raise, that of distinguishing standardization from conventionalization.

[26] I take it that this requirement is part of what Millikan is rejecting in her criticism of the position of "the speech-act tradition" on conventions (1998: 176–80).

[27] But he does curiously misrepresent my view of conventions, claiming that I "clearly" resist the idea that that something like what he calls "mutual belief" "is necessary for a certain use of an expression to be conventional" (2013: 237). In fact, I clearly affirm just that (2013f: 190) in the passage reproduced a page back.

(I) I pointed out earlier that "wherever there is a convention in *C* of using *E* to speaker mean *M*, *M* is a linguistic meaning of *E* in *C″*" (Sect. 3.5). Now supposing that Bach is right and conventionalization's dependence on mutual belief is indeed stronger than standardization's, why do we need that extra strength, whatever it may be, for an explanation of shared linguistic meaning? Why wouldn't it also be the case that wherever there is a *standardization* in *C* of using *E* to speaker mean *M*, *M* is a linguistic meaning of *E* in *C*? Why isn't standardization, like conventionalization, sufficient for linguistic meaning?[28] If it is, my earlier claim that "wherever *M* is the linguistic meaning E it will be largely because that meaning was established by convention" (Sect. 3.5) should simple be amended by adding "or standardization"; and Bach's examples of *E standardly* meaning *M* turn out to be examples of *E literally* meaning *M* after all. I don't see an answer to these key questions in Bach.

(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 (to be discussed in detail in Chap. 9). 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" that the referential use of descriptions lacks. What evidence is there of this lack? And what evidence is there that, in the examples of quantifier domain restriction and 'ready', to be discussed in Sect. 10.3, 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 need 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

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[28] See also Sects. 5.3 and 11.4.1.

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.[29]

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*; the psychological-reality requirement again (Sect. 8.4). 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" (p. 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.[30]

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 "vitiated 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

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[29] "'Short-circuited' implicature-calculation is thus hard to differentiate from disambiguation, making the Gricean hypothesis look more like a notional variant than a real competitor to the ambiguity hypothesis." (Bontly 2005a: 132)

[30] Neale (1990: 78–90) gives some detailed examples of Gricean pragmatic inferences including ones from the quantificational meaning of a definite description to a referential meaning. These give a good idea of the full length of such an inference.

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 (Sect. 8.2). 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 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 understanding '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 and a new linguistic meaning.[31]

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 noncentral language system, without the hearer being aware of it.[32] 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, hence SEp, as an explanation of regular uses; it is a version of PEp. And the problem for this standardization, explained in terms of streamlined inferences, is like the one that

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[31] Ingrid Falkum's talk of "inferential short-cuts" in a "pragmatic routine" (2015: 92), discussed in Sect. 11.5, faces a similar problem.

[32] 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.

emerged for generalized conversational implicatures (Sect. 8.4): it has heavy psychological commitments – the psychological-reality requirement – that it is not plausible to think can 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 there are no signs that these psychological demands are met in the favorite examples (Sect. 11.6). Absent those signs, we should suppose that those examples are better explained as conventionalizations; see Occamist and Developmental Objections (Sect. 8.4).

In Sect. 8.5, I noted that a sure sign that an author endorses the mistaken Modified Occam's Razor is a failure to acknowledge the explanatory onus on any claim that certain phenomena are pragmatic not semantic. To acknowledge that onus in the case of standardization, Bach should argue both that standardization offers a good explanation of the phenomena in question and a better one than the rival semantic explanation (SEp). Bach does not offer such arguments.

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 linguistic 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 descriptions, and we have a conventional linguistic meaning. Bach's favorite examples of standardization seem to be of that sort. So the default reasoning of Bach's standardization may be real enough in some cases, 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.[33]

I have argued that Bach's methodology for deciding what is semantic is flawed in two respects: first, in its commitment to the mistaken Modified Occam's Razor; second, in explaining many regularities in usage as standardizations (PEp) rather than conventionalizations (SEp). In Sect. 10.5, I shall argue that it is flawed in a third respect: its inappropriate syntactic constraints on conventional meanings yields a pragmatic treatment of saturation. With this flawed methodology Bach has built a very effective "wall" against new meanings. This leads him, in effect, to embrace PRAG, as we shall see.

8.7   Conclusion

We have looked critically at two conservative strategies that work against SEM by excluding new meanings. First, Grice's "Modified Occam's Razor": "Senses are not to be multiplied beyond necessity". This is commonly construed as advising against positing a new sense *wherever there is a Gricean (or other pragmatic) derivation* of the message from an uncontroversial old sense, without any consideration of whether that derivation is part of the best explanation of the message. I argued that this way of thinking (Grice, Recanati, Carston, Bach, Ruhl, Cruse, Falkum) is the *Third Methodological Flaw of Linguistic Pragmatism* (Sect. 8.1)

Grice's Razor, as commonly construed, 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". Yet a derivation of what is now a new conventional meaning from the old conventional meaning will still be available. The common construal loses sight of why we posit a language in the first place (Sect. 8.2).

Where a word is regularly used polysemously, the onus is much more on the denier of a new meaning than on the positer of one. A pragmatic explanation of such a regularity needs to meet the "psychological-reality requirement", showing that the explanation's pragmatic derivation has an appropriately *active* place in cognitive lives. In interesting cases, like alleged generalized conversational implicatures, speakers and hearers are not *conscious* of derivations having such a place. A pragmatic explanation then faces two powerful objections. The Occamist Objection that the explanation posits subconscious processes that need, but lack, independent evidence. The Developmental Objection that we have good reason to suppose, a priori, that these processes do *not* exist. We should expect that a pattern of successful communication using a word with a certain once-novel speaker meaning would lead to

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[33] These concerns were first raised in "Good and bad Bach" (2013f). Bach's "Reply" (2013) does not address them.

it having that meaning conventionally. For that is the sort of process that gave us our language (Sect. 8.4).

Bach is an extreme meaning denialist. Modified Occam's Razor is one of his strategies for this. He (2013) has responded dismissingly to an earlier version of my criticism (2013f) of his embrace of the Razor. I argued that he has not answered the criticism (Sect. 8.5).

Bach has another strategy for excluding new meanings: many regularities in usage are to be explained not as conventionalizations but as *standardizations*. Standardizations are said to differ from conventionalizations in their relation to "mutual beliefs" and in their involving "streamlined" pragmatic inferences. I 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 (Sect. 8.6).

The strategies for meaning denialism discussed in this chapter have been very costly, misleading people into thinking that meanings that are, as a matter of fact, context-relative because expressions are ambiguous must be treated as pragmatically constituted. This has led to the mistaken thesis of PRAG.

With methodological issues behind us, the rest of the book is devoted to SEM-PRAG, my version of the semantics-pragmatics debate. We start with the issue of definite descriptions.

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Chapter 9
Referential Descriptions: A Case Study

9.1 Introduction

In Chap. 3, I introduced my usage according to which "semantics" is concerned with the representational properties that symbols have in virtue of being uses of a language; these are the properties that constitute "what is said". I argued that these include properties of three sorts: briefly, those arising from (i) convention, (ii) disambiguation, and (iii) linguistically demanded saturation (slot filling). And I assumed that what-is-said is constituted by no other sorts of properties. I described a view in the spirit of the tradition:

> **SEM**: Setting aside novel uses of language, what-is-said (in my sense) is typically the truth conditional speaker-meaning (content) of an utterance, the message conveyed. That message is typically a WIS-meaning.

The Pragmatists have mounted a formidable challenge to the likes of SEM with a range of striking examples most of which are not novel implicatures or modulations, not variations in the uses of language made "on the spur of the moment", "on the fly". Rather, they are examples of *standard regular* uses with meanings that vary in context. This has led to a view that I summed up as follows:

> **PRAG**: Even setting aside novel uses of language, the truth conditional speaker-meaning (content) of an utterance, the message conveyed, is seldom, perhaps never, constituted solely by what-is-said (in my sense), a WIS-meaning. The message is always, or almost always, the result of pragmatic modification, a PM-meaning. (Sect. 3.5)

There is "semantic underdetermination" and many think that a new theoretical framework is called for, "truth-conditional pragmatics". I promised to confront this challenge by arguing, controversially, that the Pragmatists' examples typically exemplify properties of sorts (i) to (iii). There are more of such properties than we have previously acknowledged: much more of the content of messages should be put into the convention-governed what-is-said – into semantics – than has been customary. It is time to start on this project.

Where do we look for evidence of what-is-said? Not simply to our metalinguistic intuitions (Chap. 2). Evidence is to be found in behavior, in particular, in the regular use of an expression with a certain speaker meaning. For, the best explanation of that regularity is likely to be that there is a convention of so using it. If there is a convention then the expression has that meaning in the language (Sect. 3.6).

We get a helpful view of how to respond to the Pragmatist challenge by looking at what seems to me a striking example of Pragmatist failure. This is the much discussed matter of referentially used definite descriptions. An example of this sort was among the eight I gave in Sect. 1.1 of the phenomena that have led to Pragmatism:

(5) The table is covered with books.

Pragmatists have tried very hard to resist the view that these uses of "definites" exemplify a distinct non-Russellian linguistic meaning, which is the view that SEM would predict. Pragmatists argue instead that referential uses should be handled pragmatically, either in a Gricean or in a Relevance-Theoretic way. This is what PRAG would predict. I have argued that Gricean Pragmatist ways are quite mistaken (1981b, 2004, 2007a, b).[1] I think that Relevance-Theoretic ways are too (if they really are pragmatic), It is instructive to see why these approaches are mistaken.[2]

9.2   Referentially Used Definites

Under the influence particularly of Keith Donnellan (1966, 1968), many think that definites are "ambiguous", having not only the "attributive" quantificational meaning captured by Russell[3] but also a "referential" meaning like that of a name or demonstrative. Under the influence particularly of Charles Chastain (1975),[4] some think the same of indefinite descriptions, "indefinites". This is my SEM view of definites and indefinites.[5]

It is generally agreed that definites have a referential *use* as well as an attributive *use*. When 'the *F*' is used attributively in 'The *F* is *G*' the sentence conveys a thought about whatever is alone in being *F*; when 'an *F*' is used attributively in 'An *F* is *G*' the sentence conveys a thought about some *F* or other. So, the sentences convey

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[1] I have also argued, along similar lines, against another paradigm of treating semantic phenomena as pragmatic (1989, 1996, 2012d, 2020b: 408–414). According to "direct reference", alluded to in Sect. 6.1 (n. 5), the meaning of a proper name is its bearer. Yet it can be shown that names are regularly used to contribute the name's mode of reference to the message conveyed. There is no principled basis for treating such uses as a pragmatic rather than semantic matter.

[2] This chapter draws heavily on my 2007a in particular.

[3] Any differences that there may be between what Donnellan says of 'attributive' descriptions and Russell's theory of descriptions are beside the point of this chapter.

[4] See also Strawson 1950, Wilson 1978.

[5] It is widely accepted that these descriptions also have a "generic" meaning illustrated, for example, by 'the dog' in "The dog is man's best friend". I shall not be discussing this meaning.

"general" thoughts or propositions. When either description is used referentially, its sentence conveys a thought about a particular *F* that the speaker has in mind, about a certain *F*. So, the sentences convey "singular" thoughts or propositions.[6]

Despite agreement that descriptions have these two *uses*, two *speaker* meanings, there is no agreement that they have two *linguistic* meanings. Many, most famously Saul Kripke 1979,[7] accept the quantificational attributive linguistic meaning described by Russell, but appeal to ideas prominent in the work of Paul Grice (1989) to resist the Donnellan-inspired idea that definites also have a referential linguistic meaning. They argue that the referential use of a definite in an utterance does not affect "what is said" by the utterance. For what is said is the content of the Russellian general thought. The content of the singular thought is indeed conveyed but only by a conversational implicature or the like. So, on this PRAG view, what is thereby conveyed is not the meaning of the sentence on this occasion and hence not the concern of semantics; rather it is the concern of pragmatics.

More recently, often reflecting the influence of the Relevance Theory of Sperber and Wilson (1995), others have offered a different PRAG view (Recanati 1989; Rouchota 1992; Bezuidenhaut 1997, 2013; Powell 2010). Unlike the Griceans, they do not see themselves as embracing the Russellian view of the encoded linguistic meaning of a description. Still, their views of that meaning are close to Russell's (see, for example, Bezuidenhout 1997: 394; Powell 2010: 162). Their significant difference from the Griceans is in their view of the pragmatic modification involved in a referential use, the modification that takes us from what is encoded to the expression of a singular thought. For them, the modification is *not* an implicature (or anything like one) from a semantic what-is-said to a proposition meant. And they think that an attributive use *also* involves a pragmatic modification of what is encoded. The idea is that the encoded meaning has, in effect, a "slot" that is filled pragmatically by the speaker's intentions, yielding *either* a singular ("object-dependent", "*de re*")[8] proposition in a referential use, *or* a general ("object-independent", "*de dicto*") proposition in an attributive use. And each pragmatic modification yields an utterance that expresses a *pragmatic* truth-conditional what-is-said, another difference from the Griceans. Here are some helpful brief statements of Relevance-Theoretic approaches:

> descriptions are taken to be tools for communicating individual concepts which can, of course, be either *de re* or descriptive. (Powell 2010: 162)

> the proposition expressed by an utterance containing an attributively used definite description is object-independent, whereas the proposition expressed by an utterance containing a referentially used definite description is object-dependent. (Powell 2010: 168)

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[6] In calling such a thought "singular", I am not endorsing the view that the content contributed by a referential definite in expressing the thought is simply its referent; in my view it contributes a causal mode of reference; cf. Sect. 6.1 on proper names.

[7] For some others, see Neale 1990, King 1988, and Ludlow and Neale 1991, Bach 1994.

[8] It is common to call singular thoughts and propositions '*de re*' or 'object-dependent'. I think that there are reasons against both usages (1996: 144n; 1985: Sec. 3).

> The claim is that what the speaker says is captured either by an object-dependent or a general proposition, where this is resolved on pragmatic grounds. (Bezuidenhout 1997: 377)

> contextual evidence of the speaker's directive intentions will determine whether the complete proposition expressed by the speaker existentially quantifies over the slot for a unique individual in the LF representation, or, rather, contains a *de re* concept of a unique individual which can be tracked by its possession of the enriched set of features.[9] (Bezuidenhout 1997: 394)

Using the terminology of "saturation", we can capture the SEM side of the disagreement over referentially-used definites and indefinites as follows: these descriptions have a conventional meaning that demands saturation in context by the particular object the speaker has in mind. The term 'saturation' is often *reserved* for such linguistically-demanded slot fillings, but it is convenient to extend the term to cover the process proposed by the PRAG side of the disagreement. So the process of providing the object in mind by a pragmatic modification of some sort is also a saturation. Then, the disagreement is over whether the saturation of a definite or indefinite by the particular object the speaker has in mind is semantic or pragmatic. (This extended usage of 'saturation' will be much exemplified in Chap. 10.)

I emphasize immediately that if a saturation is to be pragmatic not semantic it has *not to be linguistically-demanded*. The Gricean process of conversational implicature clearly meets this requirement: a speaker can use a definite with its encoded Russellian meaning without any saturation at all. But it is not at all clear that the Relevance-Theoretic process does meet the requirement on a pragmatic saturation. The view seems to be that the encoded meaning of a definite *demands* saturation in one of two ways, either the attributive way or the referential way. How then does this view differ, other than verbally, from the view that definites are ambiguous?[10] In any case, one thing is clear: if a saturation is to be pragmatic it has to be by a process in the speaker that differs from the sort of saturation process exemplified when a speaker participates in the referential convention for deictically used demonstratives (Sect. 4.1.2). We shall be making much of this (Sect. 9.4)

In "The case for referential descriptions" (2004), I claimd that the case for the thesis that descriptions have referential linguistic meanings has been greatly underestimated. I argued that the referential uses of both definites and indefinites constitute such meanings: the uses are semantically significant, not merely pragmatically so. I call this thesis 'RD'. It is a consequence of RD that descriptions are ambiguous. A key part of my argument for RD was the rejection of the above Gricean defense of Russell ("Argument I" in Sec. 2).[11] In "This, that, and the other" (2004),

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[9] Contextual evidence "determines" this only in an epistemic sense, of course. In the metaphysical sense, the intentions themselves "determine" which proposition was expressed; see Sect. 7.2 and the Second Methodological Flaw.

[10] Neale describes himself as "baffled" (1990: 110–112 n. 36) by Recanati's (1989) view. My question reflects a similar bafflement.

[11] The following are earlier presentations of such a rejection: Devitt 1997b: 125–8; 1997c: 388; Reimer 1998.

Stephen Neale calls this sort of rejection "the Argument from Convention" and concedes "an important point" to it (p. 71); "I no longer think the difference between *saying* and *meaning* lies at the heart of a characterization of referential usage." This does not, however, lead him to embrace RD. Indeed, he still thinks that "the Russellian analysis is basically correct for both attributive and referential uses of descriptions" (p. 106). Furthermore he now thinks that debate over RD "seems to lack real substance" (p. 71); it "is the product of a powerful illusion" (p. 106).

I shall look critically at Neale's new position in Sect. 9.9 of the chapter. But most of this chapter will be concerned with other matters. In Sect. 9.3 I present the Argument from Convention, acknowledging that "Case" did not take adequate note of the idea that referential uses are supposed to be examples of *generalized*, not *particularized*, conversational implicatures. In Sect. 9.4, I consider the onus on any pragmatic explanation and the failure of both Gricean and Relevance-Theoretic explanations to meet it. In Sect. 9.5, I shall present a further argument against Gricean pragmatic explanations of referential uses by focusing on "incomplete" definites and the problem of "ignorance and error". I shall reinforce these arguments in Sects. 9.6 and 9.7 by responding to Kent Bach's lengthy defense of the Russellian *status quo* against the Donnellan-inspired revolution in general, and against my contribution to the revolution in particular. It seems to me that Bach's defense strengthens rather than weakens the case for the revolution. That concludes my main case for RD but I add three further arguments in Sect. 9.8.

The discussion will all be about definites but much of it can easily be applied to indefinites (and, I'm inclined to think, to the expression 'some *F*').

9.3 The Argument from Convention

Donnellan and others produced a number of examples of the referential use of definites that led many to embrace RD. The Gricean response to these examples made this embrace seem too hasty because the response raised the possibility that all these referential uses could be explained pragmatically. This possibility is made very real by the indubitable fact that, with the help of Grice's "Cooperative Principle" – "Make your conversational contribution such as is required, at the stage at which it occurs, by the accepted purpose or direction of the talk exchange in which you are engaged" (1989: 26) – and related maxims, we seem to be able to give a pragmatic explanation of the referential use of *any* quantifier. Neale has an example with 'everyone' that I have already discussed in Sect. 8.4: Jones says "Well, everyone taking my seminar turned up" in circumstances in which the hearer can use the Cooperative Principle to derive the message that only Smith attended. As Neale says: "The possibility of such a scenario, would not lead us to complicate the semantics of 'every' with an ambiguity" (1990: 88). Indeed it would not: the saturation of 'everyone taking my seminar' by Smith is clearly a pragmatic modification. Similarly, Neale is suggesting, the possibilility of Donnellan's scenarios should not lead us to complicate the semantics of 'the *F*' (1990: 87–8).

The Argument from Convention in "Case" (labeled "Argument I") is a response to this nice point. I presented the core of the argument as follows:

> The basis for RD is not simply that we *can* use a definite referentially, it is that we *regularly* do so. When a person has a singular thought, a thought with a particular *F* object in mind, there is a regularity of her using 'the *F*' to express that thought. And there need be no special stage setting enabling her to conversationally imply what she has not literally said, nor any sign that her audience needs to use a Gricean derivation to understand what she means. This 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. This convention is semantic, as semantic as the one for an attributive use. In each case, there is a convention of using 'the *F*' to express a thought with a certain sort of meaning/content.
>
>   'Every' and other quantifiers are different. There is no convention of using them to convey a thought about a particular object in mind. With special stage setting they certainly can be used for that purpose, as Neale illustrates. But then Grice shows us that with enough stage setting almost any expression can be used to convey almost any thought. (2004: 283)

The idea is that there is a convention for 'the F' but not for 'every' that demands saturation by the particular object in mind; the saturation is semantic. So 'the' is ambiguous, having both a quantificational meaning that yields attributive definites and a referential meaning that yields referential definites.[12]

What is it for the speaker to have a particular *F* object x in mind in using an expression? It is for the concept that she is thereby expressing to stand in a certain sort of causal relation to x, a relation involving the perceptual grounding of someone's thought in x and, perhaps, reference borrowings (Sect. 4.1.2).

(So, when all goes well, a referentially used token of 'the *F*' contributes x to the literal meaning of an utterance of 'the *F* is *G*'. But suppose that all does not go well: suppose that there is a *mis*description and x, the object in which the token is causal-perceptually grounded, is not *F*. I think it is clear that '*F*' is semantically significant, with the consequence that if x is not *F* then the utterance is *not literally true* (2004: 282). There are at least three ways to capture this semantic significance (p. 292), summarized below in Sect. 9.9, but my preferred one has always been to take '*F*' to be partly determinate of reference (1981a: 54; 1981b: 519). So a referential token of 'the *F*' refers to x in virtue of x being an *F* object in which the token is grounded. If x is not *F* then x is the speaker-referent but not the semantic-referent.)

The Argument from Convention rests on the assumption that definites *are* regularly used referentially, regularly have a referential speaker meaning. This is an empirical assumption about usage if ever there was one.[13] The assumption may not yet be supported by scientifically gathered data but it is by every dictionary I have consulted. Presumably lexicographers have arrived at their view by informal observation. This is in order because, as noted in Sect. 3.6, this regularity, like many

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[12] My argument for an ambiguity here, as elsewhere, rests on regularities in usage not on any of the diagnostic tests that linguists sometimes use, as emphasized in Sect. 8.4; c.f. Sennett 2002; Koralus 2013.

[13] Kasia Jaszczolt takes the referential reading of definites to be the default (2005: 106); Alessandro Capone also argues for this view (2019: 118–20).

others we shall mention, is obvious to anyone who reflects on her linguistic experiences.[14] My observations lead me to think that most uses of definites are of "incomplete" ones (to be discussed in Sect. 9.5). And almost all nonanaphoric uses of incompletes ones are referential. All in all, setting aside superlatives and anaphoric uses, I'd guess that the vast majority of uses of definites are referential. Whether that guess would hold up to scientific testing, the regularity of referential uses surely would.

What is the best explanation of the regularity? The Argument from Convention offers a good explanation. If it is the best, then we should adopt RD; see the discussion of abduction in Sect. 3.6. "Case" accompanied the Argument from Convention with some critical remarks about the plausibility of explaining this regularity pragmatically as conversational implicatures (2004: 284–6). This discussion in "Case" still seems to me to make RD very plausible. Yet, by focusing on regularity and special stage setting, it leaves itself open to a possible Gricean objection along the following lines:[15]

> The argument may show that referential uses are not examples of *particularized* conversational implicatures like Neale's but it does not show that they are not examples of *generalized* conversational implicatures. For, particularized implicatures are *not* regularities and *do* require special stage setting, as Neale's example shows. But generalized implicatures are different in both respects. As Grice says, in the generalized case "the use of a certain form of words in an utterance would *normally (in the absence of special circumstances)* carry such-and-such type of implicature" (1989: 37; emphasis added); and the generalized case is, as Neale says, "relatively independent of the details of the particular conversational context" (1990: 83).

"Case" did not consider an objection in this form. However, it did provide some of the tools for dealing with it.

"Case" pointed out (2004: 285–6) that to defend the Russellian view of referential uses it is not enough to show that these uses *could* be explained pragmatically as generalized conversational implicatures. We need a good argument to show that it *should in fact* be explained in this way. This is, of course, an instance of the general point about onus in Sect. 8.4. I knew of no such argument. I still don't: I don't know of an argument that meets the onus on an explanation of referential uses as implicatures or, indeed, as anything pragmatic. I turn to this.

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[14] So perhaps it is an "intuition". But, if so, it is not one of the sort that I have argued (Chap. 2) we should not rely on: for, it is not a metalinguistic judgment that an expression has properties *of the sort adverted to in the very semantic theory being tested*; see Sect. 2.8, note 10. Given how obvious it is that definites are regularly used referentially, as dictionary entries reflect, one wonders whether Russell's Theory of Descriptions would have been so dominant had it not been proposed by a philosophical giant.

[15] For example, see Bach 2004: 227–8; Bontly 2005b: 6–9

9.4   The Onus on a Pragmatic Explanation

Utterances using definites attributively assert general propositions by exploiting the
definite's Russellian (Gricean), or near-Russellian (Relevance-Theoretic), encoded
semantic meaning. But definites are regularly used with a referential speaker mean-
ing to assert singular propositions. Suppose that *E* is a definite. On a particular occa-
sion, a speaker uses *E* referentially to mean *M*. We have to choose between two rival
explanations. The pragmatic explanation, predicted by PRAG, is like PEp in Sect.
8.4. I shall call it "PEd".

> **PEd**: *E*'s property of speaker-meaning *M* on this occasion is a pragmatic modification of
> *E*'s semantic meaning and so is at least partly pragmatic (in my senses), So *M* partly consti-
> tutes a singular proposition that is a pragmatic modification of a semantic meaning, my
> "PM"-meaning (Sect. 3.5). On other occasions when *E* is used attributively, its speaker
> meaning partly constitutes a general proposition, either *E*'s semantic meaning (Gricean) or
> a different pragmatic modification of its semantic meaning (Relevance-Theoretic).

The semantic explanation, RD, predicted by SEM, is like SEp in Sect. 8.4 (and so
might also be called "SEd"):

> **RD (SEd):** *E*'s property of speaker-meaning *M* on this occasion is semantic (in my sense).
> So *M* partly constitutes what-is-said, my "WIS"-meaning (Sect. 3.5), a singular proposi-
> tion. On other occasions when *E* is used attributively, its speaker meaning is a different
> semantic meaning, partly constituting a general proposition.

According to RD, the definite is thereby ambiguous, according to PEd, not. What
would have to be the case for PEd to be the right explanation? There is a heavy onus.

The argument in Sect. 8.2 that appealed to dead metaphors and "the fundamen-
talist Gricean" shows that, for PEd to be right, more is required than that the singu-
lar proposition arising from a referential use of a definite can be pragmatically
derived from the literal meaning of the utterance. Indeed, my first use of dead meta-
phors was against a Gricean pragmatic explanation of referential uses (1997b: 127;
2004: 284–5). The role of that discussion of dead metaphors in the argument for RD
can be misunderstood. (1) Thus, Alessandro Capone presumably has this discussion
in mind in stating:

> the main reason why [Devitt] opposes the standard implicature view is that in this case,
> according to him, the implicature, if there is one, has become frozen or conventionalized
> (2019: 111).

Change the tense to yield "if there was one" and this would approximate my main
reason for opposing the Gricean PEd, but it would be *more apt* to say that my main
reason was the Argument from Convention; for that argument provides a better
explanation than the implicature view (Sect. 9.3). (2) Bach has a serious misunder-
standing of the role of my dead metaphor discussion. He criticizes what he describes
as my "analogy of referentially used descriptions with dead metaphors" (2013:
239). The point of my discussion of dead metaphors, as I have emphasized before,
is *not* to draw this analogy but rather to undermine "a particular Gricean *argument*

for the Russellian view" (2007a: 14n).[16] The argument I am targeting is that the referential meaning *M* should be treated as an implicature because *M can be* pragmatically derived from the attributive meaning. Dead metaphors show that a pragmatic explanation needs more than this possibility. (3) Bontly suggests that dead metaphors "form the basis of a positive argument for the ambiguity thesis" in virtue of similarities that they have to referential uses (2005b: 17). He goes on to undermine that argument by pointing to dissimilarities (pp. 17–20). I emphasize that this is not my positive argument for RD, the Argument from Convention is. Indeed, I don't think referentially used descriptions are similar to dead metaphors.

PEd requires a pragmatic derivation or other modification that yields the referential speaker meaning *M*. The dead metaphor discussion makes clear that this derivation has to be placed in the cognitive lives of the speaker and hearer. And that demands more than that speaker and hearer are merely *competent* to make the pragmatic modification. There is what I called a "psychological-reality requirement" on a pragmatic explanation (Sect. 8.4). Applying this requirement, PEd demands the actual presence of the modification that yields *M* in the cognitive lives of speaker and hearer. For that explanation to be good, there would have to be regular, typically subconscious non-central, processes in speakers and hearers *that differ from the saturation processes required to participate in referential conventions*, in particular, to participate in the convention for deictically used demonstratives. In hearers there would have to be regular mind-reading processes of detecting speakers' pragmatic modifications of the definite's semantic meaning in order to infer from its utterance, in the circumstance, its referential speaker meaning *M*. In speakers there would have to be regular partly mind-reading processes of meaning *M* by the definite partly because the speaker expects that the hearer will go through the above mind-reading process. These typically subconscious mental processes need to be *both psychologically real and different from the convention-exploiting ones*.

I was concerned earlier (Sect. 9.2) that the Relevance-Theoretic view of referentially used definites may differ only verbally from RD (SEd), the SEM view. Consider this for example:

> the encoded meaning of a definite description 'the F' has two dimensions: on the one hand, it tells the hearer to look for an individual concept and, on the other, it encodes the property of being a unique F to help the hearer in his hunt. (Powell 2010: 165)

An encoded meaning that "tells the hearer to look for an individual concept" sounds very like the RD view of what the hearer has to do when presented with a referentially used definite. For, according to RD, the encoded referential meaning of the definite demands saturation by the particular object in mind, by "an individual concept". So, we might well say that this encoded meaning "tells the hearer to look for"

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[16] This was in response to Bach's earlier presentation of this criticism (2004: 226). Bach has apparently overlooked this response, despite Bach 2007a being a reply to my 2007a in which the response was made. And I near enough repeated the response in "Good and bad Bach" (2013f: 186 n. 26), the paper Bach 2013 is a reply to. But he still overlooks the response.

that concept.[17] In any case, to emphasize, *if the Relevance-Theoretic view is to be distinct from RD it must involve processes in speaker and hearer that differ from RD's convention-exploiting processes.*

The same two objections that we brought against positing regular subconscious non-convention-exploiting processes for putative generalized conversational implicatures, like those involving 'and' and 'some' (Sect. 8.4), count against positing them for referentially used definites.

Occamist Objection: We already know that there must be the sort of processes required by RD (SEd), the semantic rival to PEd, though we are short on the details. For, the required processes are those exemplified when a speaker participates in the referential convention for deictically used *demonstratives*. So, in the absence of independent evidence to suppose that the subconscious mind-reading processes required by PEd exist, we should not posit those processes and so should prefer RD. So far as I know, *there is no evidence of these processes, nor even any attempt to find evidence: none of the psycholinguistic experiments cited in Section 11.6 seeks evidence on the processing of definite descriptions.*

Developmental Objection: There is good reason to suppose, a priori, that these processes do *not* exist. Consider an expression that does not have a certain meaning conventionally in a language but that comes to be regularly used with that meaning in successful communications by speakers of that language. That success tends to lead to the expression having that meaning by convention. *Why would the regular use of definites referentially not have had that same happy result?* A convention eliminates the need for the demanding mind-reading processes in speakers and hearers that PEd requires. Why would we have denied ourselves that benefit with referentially used definites? To suppose that we have is no more plausible than to suppose that other successful practices, like meeting for drinks at O'Reilly's on Fridays (Sect. 5.2), have not developed into a convention. Our discussion of abduction in Sect. 3.6 shows that we should accept RD on the basis of an abduction only if it seems better than any alternative explanation that is *likely*, given our background knowledge. Given what we know about the development of linguistic conventions, it is *un*likely that we will find the evidence that PEd requires to make it a good alternative to RD.

The psychological-reality requirement on PEd has not been met. According to the Developmental Objection the requirement is unlikely to be met. According to the Occamist Objection, we should then prefer RD. That is the most general

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[17] Indeed, one wonders why theorists urge the Relevance-Theoretic view of referential uses of definites given that much of what they say seems to describe a saturation *convention*. Two tentative suggestions. First, these theorists implicitly embrace Modified Occam's Razor (as commonly construed); see, for example, Powell's (2010) enthusiasm for a "univocal", and Bezuidenhout's (1997) for a "unitary", treatment of definites. So the Third Methodological Flaw (Chap. 8) may be at work here. Second, they are motivated by the insights of Relevance Theory. But those insights are largely about a hearer's process of understanding a meaning rather than about what constitutes a meaning. The insights about understanding are quite compatible with RD. So the Second Methodological Flaw (Chap. 7) may be at work here.

objection to PEd, whether Gricean or Relevance-Theoretic versions. But we have even more against Gricean versions.

9.5   The Incompleteness Argument Against Gricean Pragmatic Explanations

This extra comes from an important feature of Gricean pragmatic explanations. The feature is that in conversationally implicating, or otherwise pragmatically implying something, *q*, a speaker *S also says something else*, *p,* whereas in simply saying that *q* he need not. So if we can show that a person who somehow conveys that *q*, does *not* also say something else then we have shown that the person is not pragmatically implicating or implying that *q*. This does not settle the fate of all paradigm putative generalized conversational implicatures but it does add to the case against the Gricean PEd. For, if we can show that *S*, in using a definite referentially, thus conveying somehow the singular proposition that *q*, does *not* also say a general proposition that *p*, then *S could not* exemplify the partly mind-reading process that PEd requires: *S* could not have the required expectation that the hearer will infer the singular proposition from what she says because there is no general proposition that could be the basis of the inference. I think that a consideration of incomplete definites shows this very persuasively.

An incomplete definite is one that fails to uniquely describe an object; for example, 'the table' in 'The table is covered with books'. Incomplete definites pose a problem for Russell's theory of descriptions because, typically, they seem to pick out an object despite this failure. Two sorts of modification to Russell have been proposed to cope with the problem, what Neale calls "the explicit approach" and "the implicit approach". According to the explicit approach, an incomplete definite is elliptical for a longer description that the speaker could supply. According to the implicit approach, "the context of utterance delimits the domain of quantification" of the definite (1990: 95). It is not in contention that one or other approach must be right for attributively used incomplete definites. However, I argued in "Case" ("Argument V", 2004: 297–303), neither approach works for referentially used ones.

The same sort of "ignorance and error" arguments that Kripke showed to be so devastating against description theories of proper names are also devastating against any Russellian theory of referentially used incomplete definites. Yet, interestingly, these arguments have been largely ignored by defenders of Russell. Here is the recipe for generating the arguments. Take a referential use of an incomplete 'the *F*' where the object in mind, *x*, is indeed *F*. Then attribute to the speaker beliefs about *x* that are too inadequate – ignorance – or too wrong – error – to enable her to supply the completion demanded by the explicit approach or to delimit the domain of quantification as demanded by the implicit approach.

I gave some brief examples of applying this recipe. Here are some of ignorance:

> Each of the following cases of referential uses can be developed into plausible examples of a speaker not knowing enough about the object in mind to supply the needed restriction: (i) Wilson's case of the aged and forgetful Valjean, who has many old enemies, saying "My old enemy is back in town" (1991: 373) (ii) Suppose that Wettstein's speaker made his remark ["The table is covered with books"] about a table in a room with many similar tables;…(iv) Suppose that *S* passes on some juicy gossip by saying "The man at the party told me…" with a particular man in mind whom she can but dimly remember. (2004: 301–2)

Reimer has a nice example of ignorance:

> Suppose that John and Mary have recently hired two men to do some remodeling on their home: Joe, a carpenter and Fred, an electrician. Suppose further that, after watching the two men argue all day long, John says to Mary…"the carpenter is not getting along with the electrician". (1998: 97)

Now suppose that unbeknownst to John and Mary, Joe is also an electrician and Fred is also a carpenter. And here are some examples of error:

> Each of the following cases of referential uses can be developed into plausible examples of a speaker providing the wrong restriction: (i) Wilson's case of a man saying to his wife, "The girl ought to be punished", having in mind a girl he wrongly believes to be their daughter (1991: 374); (ii) Suppose that Wettstein's speaker has various mistaken beliefs about the location of the table;…(iv) Suppose *S* wrongly believes that the source of the gossip was a man from Columbus in a black suit (he was from Cleveland in a blue suit). (2004: 302)

These examples are brief because it is really very easy to develop the examples into plausible counterexamples to Russell.[18] And yet in each development, despite ignorance and error, the speaker successfully uses an incomplete definite to express a thought about a particular object in mind, *x*.[19]

If this is right then the Gricean PEd is wrong. According to it, a speaker *S* who uses 'The *F*' in 'The *F* is *G*' referentially to refer to *x* would be saying that *p* where *p* is a certain general proposition. And the problem posed by incomplete definites is that *there is frequently no general proposition that S might plausibly be thought to have said*. Everyone agrees that *S means*, at least partly, the singular proposition that *x* is *G*, where *x* is some particular object. What general proposition might *S* be saying in order to convey that singular one? We have a promising answer if *x* is the one and only *F*, for then 'The *F* is *G*' asserts a general proposition that identifies *x* as the object in question. But the problem is that 'the *F*' will very often be incomplete and so will not identify *x*, or anything as the unique *F* in the world. And this will be obvious to all. So *S* will not suppose that asserting a general proposition about the unique *F* – for example, the unique table – will enable a hearer *H* to identify *x* as the object

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[18] A defender of Russell may be tempted to go parasitic at this point, allowing the speaker to do the job with a description like 'the object that I am perceiving' which specifies the causal-perceptual relation that, in the view of the referentialist, does identify *x*. This temptation should be resisted because it is theoretically redundant (2004: 300, 302).

[19] Objection: "If this argument were good it would count also against any Russellian theory of *attributively* used incomplete descriptions. Yet some such theory must be right." I agree it must, but claim that the argument does not count against it (2004: 302–3).

of thought. And the arguments from ignorance and error show that *S* is often not in a position to modify that general proposition, by completing 'the *F*' or delimiting its domain, into one that does identify *x*. Where *S* is not, *S* is not in the position to go through the process that PEd requires: in uttering 'The *F* is *G*', *S* would not expect that *H* would infer the singular proposition from any general proposition about *F*'s that *S is* in a position to say, a proposition that does *not* identify *x*. *We lack a plausible account involving a general proposition that S might be saying and that S would think conveys the singular one, and that H would take to convey the singular one*. I conclude that the Gricean PEd is false.

If this is right there can be no good Gricean pragmatic explanation of referential uses. This adds to the already powerful Occamist Objection and Developmental Objection to PEd. RD (SEd) is the only good explanation. So it is the best.[20]

9.6   Bach's Pragmatic Defense of Russell

The defender of a Gricean PEd has two important tasks. On the positive side, he needs to provide what I have just argued can't be found: a good argument that referential uses are akin to indubitably pragmatic phenomena. On the negative side, he needs to criticize the argument that the uses are semantic, pointing out, for example, how they differ from what is uncontroversially semantic. Bach attempts both. I shall now look critically at these attempts. This serves to reinforce the case for RD

First, however, let me start on a happy note of agreement. Bach's conception of "singular thoughts" is as follow:

> We can have singular thoughts about objects we are perceiving, have perceived, or have been informed of…The connection is causal-historical, but the connection involves a chain of representations originating with a perception of the object. Which object one is thinking of is determined relationally, not satisfactionally… there must be a representational connection, however remote and many linked, between thought and object. (2004: 191–2)

This is very much the same as my view (1974, 1981a, b, 2004: 290), described briefly in Sect. 4.1.2.[21] And it is an important piece of the background. Now on to the disagreements.

I noted in Sect. 9.3 that Bach and I agree that definites are regularly and standardly used referentially. He would also agree that RD is a *possible* explanation: definites might have referential meanings. In a benign mood, he might even agree that this semantic explanation is beautifully simple. But he thinks that he has a

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[20] Paul Elbourne finds "little to choose between the Russellian theory augmented by Gricean manoeuvring and Devitt's ambiguity theory" largely it seems because he thinks that "the burden of proof" on each side is equal (2013: 109). I agree that, "in general, the onus should be equal" (2007a: 17). I have argued that whereas RD has discharged its explanatory onus, PEd has failed rather dismally to do so.

[21] This view of singular thoughts is challenged by John Hawthorne and David Manly (2012). My 2014a is a response.

pragmatic one that is better: "I take referential uses to be akin to *generalized* implicatures" (2004: 227). So I count Bach's explanation as a "Gricean" PEd. The heart of this explanation is to be found in his Point 5:

> The distinctive quantificational character of definite descriptions helps explain how and why they can readily be used to refer, *because it plays a key role in their referential use*. (p. 201; emphasis added)

This point also provides the basis for his main criticism of my explanation: "if definite descriptions are ambiguous, their ambiguity is most extraordinary: one of their senses (the quantificational) plays a role when the other (the referential) is operative" (p. 224).

I shall start with Bach's Point 5 and then respond to his criticism.

Note first that Bach's view that the quantificational character of definites helps explain how they can be readily used to refer requires that that character came before that ready use. For, it has to be already present to do the explaining.[22] This raises a question. Do we have any reason to believe that the history meets this requirement? Set that question aside until Sect. 9.8 ("First Argument").

Bach thinks that the quantificational meaning of definites does more than explain the *development* of referential uses, which is quite compatible with RD, it still "plays a key role in" that use. So, the idea is that the best explanation of what is now going on in a referential use involves the quantificational meaning. And this makes a referential use "akin to" a conversational implicature. I don't think that Bach's explanation is a good one.

Here, briefly, is my rival explanation, RD (SEd). There is a semantic convention of using 'the *F*' to refer to *x* which exploits both a causal-perceptual link between the speaker and *x* and a meaning of '*F*' (which might be ambiguous). So the truth underlying Bach's claim is that a descriptive meaning of '*F*' plays a role in what-is-said. But, of course, a descriptive meaning of '*F*' is not the same as a quantificational meaning of 'the *F*'. Let us fill this out a bit. In a referential use, a singular thought is conveyed. Bach agrees that a person's singular thought is about a certain object in virtue of a causal-perceptual link to that object. So such a link accompanies all referential uses. It is then plausible to think that it plays a semantic role, just as it does with a deictic pronoun or demonstrative (this is, in effect, my "Argument III" for RD; 2004: 288–90).[23] A speaker expressing a singular thought about a certain object participates in the referential convention and thus exploits the causal-perceptual link to that object; a hearer participates in the referential convention and thus takes account of clues to what has been thus exploited. (This is not to say that either has a *theory* of the link, even, to use a popular weasel word, a "tacit" one. The capacities to exploit such links and recognize their exploitation are linguistic *skills*,

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[22] Bach may be right in claiming that "to endorse the Russellian claim…does not commit one to the view that their attributive use came first and their referential use somehow developed later" (2004: 226). But any pragmatic *explanation* of the referential use like his does seem to have the commitment.

[23] Bontly (2005b: 12–16) is not convinced.

pieces of knowledge-how consisting mostly of subconscious states that function automatically and speedily; see Sect. 2.7). Bach rightly points out that "singular definite descriptions…imply uniqueness" (p. 210).[24] But that implication need not come from a quantificational meaning; it can also come from a referential meaning, as it does with deictic uses of pronouns, demonstratives, and, I am claiming, referential definites.

Return to Bach's explanation, PEd. He has this to say about the "key role" of the quantificational meaning in the referential use of a definite:

> The speaker thinks of a certain object, takes that object to be the *F*, and uses 'the *F*' to refer to it. The [hearer], on hearing 'the *F*', thinks of a certain object that he takes to be the *F*, and takes that to be what the speaker is referring to. (2004: 203; see also his later example on pp. 223–4)

> one cannot understand a referential use of a definite description without grasping its literal, quantificational meaning (p. 226)

What does this amount to? We should note first that there is a way of understanding these claims *about the hearer* that is uncontroversial and quite compatible with RD. A consequence of RD is that a definite is ambiguous. Now, the evidence about a hearer's processing of *any* ambiguity – think of a boring example like 'bank' – is that both meanings are entertained, mostly subconsciously, before one is eliminated with the help of contextual clues (Gernsbacher and Kaschak 2003). So RD would predict that, when faced with a referentially used definite, a hearer will typically entertain its quantificational meaning before eliminating that meaning with the help of contextual clues.[25] But this is clearly not the key role for the quantificational meaning that Bach has in mind. For one thing, if it were, then there would be nothing in the least "extraordinary" about the ambiguity that is a consequence of RD (*cf.* p. 224). For another, there is no plausible analogous way of understanding Bach's claim *about the speaker*.

Initially, we might suppose that Bach has in mind a much more striking role for the quantificational meaning. The speaker's route from the singular thought that *x* is *G* to the use of 'the *F*' to refer to *x* in 'the *F* is *G*' is *via* the general quantificational thought that, according to Russell, is expressed by 'the *F* is *G*'. So, the quantificational meaning is *what-is-said*. And the hearer's route to understanding the referential 'the *F* is *G*' is via thinking that quantificational meaning. The quantificational meaning can play this role because it *identifies* the referent of a referential use.

This might seem immediately plausible if the '*F*' in uses of 'the *F*' to refer to *x* typically applied *uniquely* to *x*. But this plausibility should disappear when we note that '*F*' in referential uses of 'the *F*' typically does not apply uniquely and hence could not identify anything. As we have been emphasizing, most uses of definites are of incomplete ones. If the quantificational meaning were playing a role in

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[24] Bach also rightly points out that 'the' alone indicates *totality*. It indicates uniqueness only when combined with a *singular* nominal (p. 202).

[25] Bach has some helpful things to say about this (2004: 198–204 *passim*, p. 224; *cf*. Devitt 1981b: 521–2).

what-is-said by a referentially used 'the *F*' it would indeed imply uniqueness,[26] as Bach insists, but given incompleteness it could not *achieve* uniqueness: it does not apply to one and only one object. As Bach says, "the speaker does not intend the description by itself to provide the hearer with the full basis for identifying the referent" (2004: 221). And Bach rejects the idea that the definite "contains some hidden modifier that would make it complete or some phantom variable of domain restriction" (p. 203); he rejects the explicit and implicit approaches which we have just seen fail anyway because of ignorance and error problems (Sect. 9.5). So our initial interpretation of Bach is clearly not right.

How then, on Bach's view, *does* what the speaker and hearer do in a referential use of an incomplete definite involve its quantificational meaning (beyond the just-noted uncontroversial involvement in the hearer's understanding that this *is* a referential use)? RD agrees with Bach that the descriptive meaning of the nominal '*F*' plays a role in a referential use. And that meaning is *part of* the quantificational meaning of 'the *F*'. But, of course, the quantificational meaning goes beyond that descriptive meaning, implying that there is a unique *F*. Bach's whole case against RD rests on his Point 5 claim that the quantificational meaning plays a role in a referential use but, so far as I can see, *he says absolutely nothing* about what that role is (beyond the role of the descriptive meaning of '*F*'). He talks of one object being "salient or contextually relevant", of it being the one the speaker "intends the listener to think of" (p. 203). But the quantificational meaning of the incomplete 'the *F*' quite plainly does not *make* that object salient, relevant, or intended since it does not *identify* the object. And Bach has not shown us that the meaning does anything else to communicate the singular thought. The quantificational meaning indicates *F*s but Bach needs to show that it is otherwise relevant. He has not done this and so his Point 5 claim is left unsupported. More generally, Bach has failed to provide what his 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.

Suppose that Bach were right and so what-is-said by a predication involving a referential use of 'the *F*' is a general proposition about whatever is uniquely *F*. Suppose further that it is obvious to speaker and hearer that 'the *F*' is incomplete and so it is not plausible that the speaker *means* that general proposition. Nor is it plausible that the speaker *means* some enriched general proposition that might be obtained by completing 'the *F*'. Since the speaker obviously means *something*, a hearer will then try to identify some other proposition that the speaker might mean. Bach rightly claims that the speaker "intends and can reasonably expect the hearer to take him to be talking about a certain *F* that is identifiable in the context of utterance" (1994: 126). But why would the speaker *think* that saying a general proposition about whatever is uniquely *F* would fulfill that expectation? Why think it would

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[26]Although, as I have noted, it is not the only meaning that implies uniqueness: referential meanings do too.

lead the hearer to identify a singular proposition about that *F* that the speaker has in mind rather than a singular proposition about any other *F* or, indeed, some other general proposition? And why *would* it lead the hearer? What would be the route from the general proposition said to the singular one meant? Plausible answers would involve something like a Gricean derivation of the singular proposition. Bach does not provide such answers. I doubt that there are any.

So, what *does* "provide the hearer with full basis for identifying the referent"? Bach's talk of "salience", "relevance", "what the speaker intends", and so on, simply labels the problem without solving it: *In virtue of what* is a certain object salient, and so on? A solution cries out to be heard. The object is salient and so on in virtue of a causal-perceptual link to the thought that the speaker is expressing. And *what provides the needed identification is the referential meaning of 'the F', a meaning established by the convention of exploiting causal-perceptual links between thoughts and objects*, just the same links that are exploited by similar conventions for demonstratives and deictic pronouns (Sect. 4.1.2). *The referential use of a definite, like the use of a demonstrative or pronoun, makes the object of thought salient to the hearer because she participates in the appropriate referential convention*. With this answer we abandon Bach's Point 5 explanation and arrive at mine. And we do so by adverting to something that Bach himself thinks accompanies all referential uses: a causal-perceptual link to the object in mind. Bach clearly agrees that the speaker and hearer exploit this link to identify the referent. But whereas RD's view that 'the *F*' has a referential meaning enables us to explain *how* uttering 'the *F*' exploits that link, Bach's view that 'the *F*' has only a quantificational meaning leaves the exploitation unexplained and inexplicable.

Finally, I turn to Bach's main criticism of RD. That criticism is that "if definite descriptions are ambiguous, their ambiguity is most extraordinary: one of their senses (the quantificational) plays a role when the other (the referential) is operative" (p. 224). The failure of Bach's pragmatic explanation enables us to deal with this criticism very swiftly. The quantificational sense is (mostly) *not* operative in referential uses except in the earlier-discussed uncontroversial way in which all meanings of an ambiguous expression are typically operative in a hearer's process of understanding. So there is nothing extraordinary about the ambiguity.

In sum, Bach has not provided an PEd for the standard regular use of definites to refer. The RD's simple semantic explanation is still the best.

9.7   Bach's Response[27]

I claim that a Gricean PEd for referential uses must be based on the view that a person using a definite referentially in uttering a sentence conveys a singular proposition *while saying a general quantificational proposition* (2007a: 18). And the

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[27] This discussion draws on my 2007b.

problem for any such explanation is that "*there is frequently no general proposition that the speaker might plausibly be thought to have said*" (p. 19). Now it is true that, in presenting this argument, the pragmatic explanation that I have particularly in mind is that referential uses are generalized conversational implicatures, but the argument is supposed to be quite general, aimed at *any* pragmatic explanation along roughly Gricean lines, including Bach's. Bach (2007a) likes the argument but thinks that it does not apply to his view. In supposing it does, he alleges, I have wrongly taken him to hold that referential uses are *more* "akin to generalized implicatures" than he thinks they are. This allegation is accompanied by a lot about what his view is *not*. In particular, it is not the "view that 'saying a general proposition' is the means by which a speaker conveys a singular proposition about the intended referent." He does not think that the quantificational meaning *identifies* the referent. Indeed, he does not think that there is a route from the general proposition said to the singular one meant. So he is under no obligation to explain the route (pp. 37–8).

This is puzzling: What then is "the role played by the univocal quantificational meaning of a definite description in effecting referential uses" (p. 38)? We clearly need a positive account of what this "key role" *is* and not simply a negative account of what it is *not*. Let us drop the somewhat technical talk of the speaker "saying" a general proposition because Bach and I differ in our understanding of this (Sects. 6.1–6.3). It is indubitably the case that, on his view, the meaning of the definite in a referential use is quantificational and yet the speaker intends to convey a singular meaning about a particular object the speaker has in mind. So we are still left with the question: *Why* would the speaker think that his utterance would convey this singular meaning and *why* would the hearer take the utterance to convey this? What is the link between the meaning uttered and the meaning intended?

Consider the referential use of an obviously incomplete 'the *F*'. Bach and I agree that "'*F*' signifies the property of being *F*". Bach goes on:

> What about the role of 'the'? When combined with a *singular* nominal, it signifies uniqueness….But if the description is incomplete, there is no unique F. Still, there must be some unique thing that is F that the speaker intends to refer to. The problem for the hearer is to figure out which thing that is. (p. 39)

The talk of "unique thing" can mislead. (What thing is *not* unique, in any sense that is relevant?). 'Unique' should be reserved for talk of 'the'. On Bach's Russellian view, 'the *F*' signifies an object to which '*F*' *uniquely applies*. And what the speaker intends in using 'the' referentially is to *uniquely refer* to a certain *F* object, to convey a thought about an F object that he *has uniquely in mind*. But unique application and unique reference are very different matters. *Why* would the speaker think that uttering a term that signifies unique application would convey unique reference? And *why* would the hearer take it to convey this? Since the definite obviously fails to apply uniquely and yet the speaker clearly means *something*, the hearer will search for something other than the quantificational meaning that the speaker might mean. But why would the hearer suppose that the speaker means something about the particular *F* he has in mind rather than about some other *F*? Bach simply persists in not answering these crucial questions. Distancing himself from the Gricean

conversational-implicature view does not make the questions go away. Bach simply takes for granted what he needs to explain.[28] So he has failed to give a pragmatic explanation of referential uses as, indeed, he later admits (2007b: 56).[29]

I summed up this exchange in "Good and bad Bach" (2013f: 184–6). Bach responded in his "Reply" (2013: 239–41). But, as I noted in "Unresponsive Bach" (2013g: 269), he still did not offer the needed explanation.

In Sect. 8.5, I considered whether Bach endorses the mistaken Modified Occam's Razor (as commonly construed). He certainly seems to do so explicitly but then, as I noted, actions speak louder than words. A sure sign that an author endorses the mistaken Razor is a failure to acknowledge the explanatory onus on any claim that certain phenomena are pragmatic not semantic. To acknowledge that onus in the case of definite descriptions, an author must attempt to offer both a plausible pragmatic explanation and an argument that this explanation is better than the rival semantic one. Bach has made no such attempt over this long exchange.[30] I conclude that he is indeed committed to the damaging Razor.

Finally, I need to address Bach's interesting accusation that, in presenting RD, *I* don't begin to address crucial questions! "Devitt does not say how the hearer manages to disambiguate the description, much less how, once he recognizes its use as referential, he identifies the intended referent" (2007a: 41). So there are two problems I am alleged not to address. (1) My disambiguation problem is virtually identical to *Bach's* problem of saying how the hearer manages to tell that the speaker is using a definite referentially rather than attributively. Since Bach discusses *his* problem at some length (2004: 198–204 *passim*, 224), I discuss *mine* elsewhere (1981b: 521–2), and, besides, the problems are instances of quite general problems, I thought that it was sufficient to cite these discussions, and some psychology (note 19), and leave it at that. I wonder why Bach thinks that it is not. (2) A speaker using a definite referentially refers to the object that it is linked to in a certain causal-perceptual way. So to identify the referent the hearer has to use clues to what object is so linked; similarly, to identify the reference of a deictic demonstrative or pronoun.[31] One clue provided by the referential use is that "the speaker *S* wishes to convey *S*'s intention that the audience *A* should identify the object *S* has in mind with an object that *A* has in mind independently of *S*" (2004: 293). Another clue is provided by what the speaker is, or has been, looking at. A lot more could clearly be said about the clues. I didn't attempt to say it in contrasting my view with that of Pragmatists like Bach because saying it is irrelevant to the contrast. Everyone, *including Bach*,

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[28] He refers (p. 40) to Bach 1994, Chap. 6, for "a fuller explanation" but I find nothing in that chapter that answers these questions.

[29] He offers an excuse for this failure but it strikes me as lame (2013f: 186).

[30] This is really "the main bone of contention" in our exchange about descriptions not, as Bach suggests, my "liberal view" of reference borrowing (2007b: 57). For that liberal view, see Devitt 2004, pp. 293–4, and 2007a, p. 9 n. 7.

[31] I am fond of likening referentially used descriptions to complex demonstratives (2004: Sec. 4). Bach resists this, making some claims about usage that I think are false (2007a: 43–5). But then neither of us have done the field work to gather the evidence of usage.

who accepts that a causal-perceptual link determines which object is referred to, and accepts that hearers typically manage to figure out which object this is, must accept that there are clues that enable the hearer to do so. We are all in the same boat. The difference between Bach and me is that I have an explanation of *how* the meaning of the referentially used definite helps direct the hearer to look for clues to that causal-perceptual link whereas Bach has none. For, on my view that meaning is a referential one that exploits that very link[32] whereas on Bach's view that meaning is a quantificational one that Bach has failed to relate to the link: see the earlier unanswered questions. That's one reason why my view is better.

This concludes my main argument against PEd and for RD (SEd). But I think that there are other arguments against it. Here are three.

9.8   Three Further Arguments

First, an argument against a Gricean PEd. Suppose that the Gricean PEd were right. Then it would have to have been the case that, historically, the quantificational convention for 'the', hence the attributive use of definites, *came before* their referential use. For that convention features in pragmatic explanations of the referential use. A person has to be already able to exploit that convention to say the general proposition, in order to convey the singular one. The raises a question, as noted in Sect. 9.6: Do we have any reason to believe that the attributive use did precede the referential use?

I don't doubt that if the attributive use did come first then the quantificational convention could be the basis of a pragmatic explanation of the referential use. But this alone does not give significant support to the idea that it did come first because, if the referential use came first, it would surely not be hard to come up with an explanation of the attributive use from the referential convention for 'the'. I doubt that we have any firm evidence about which use came first. Perhaps both uses arose together.[33] If we do lack firm evidence on this historical matter, then that is a problem for any Gricean pragmatic explanation of referential uses.

Bach objects that I am here "equating the attributive use with the quantificational meaning" (2007a: 42) and hence wrongly think "that on any pragmatic approach to referential uses the attributive use is more fundamental" (p. 42). I am not making

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[32] Bach raises the bizarre possibility that I think there is a "distinct referential convention for each object" (2007a: 41). What I think of course is that there is a convention of descriptions exploiting *a certain sort* of link to objects.

[33] Stephen Neale informs me that his research suggests that 'the' came from 'that'. A former student of mine, Boone Gorges, informed me that his research suggests that 'the' and 'that' have a common origin in a word of uncertain function. Another former student, Francesco Pupa has drawn my attention to the following: "Modern English 'the' is a continuation of the Indo-European pronominal stem *to. The function was not originally that of an article; we meet the word in historic times as a demonstrative pronoun." (Christophersen 1939: 84)

that equation and I think the pragmatic approach *is* committed to the attributive use being more fundamental (in the sense of coming first). Bach's contrary view detaches meaning from use in an unacceptable way. For, where did the quantificational meaning come from? The answer must be: from attributive uses. The meaning must have been established and sustained by the regular use of definites attributively. How else? So since, on the pragmatic approach, the quantificational meaning must have preceded the referential use, then the attributive use must have preceded the referential use and hence be more fundamental. Meanings are not God given, they are created by use (or, occasionally, by stipulation).[34]

Second, an argument for RD (SEd). Neale notes that a referential definite functions "like a name or like a demonstrative" (1990: 85–6). In "Case" I claim that when definites are used referentially, a complex demonstrative would usually serve the communicative goal well enough (2004: 288–9). And often the speaker could have used a name, a simple demonstrative, or pronoun. So, when a definite is used referentially, there are nearly always other devices available that, according to the conventions governing them, can to do the referential job.[35] If the referential use of definites was indeed not a semantic convention we would expect that use to be rare in situations where there is another device available that is conventionally used referentially. Yet that use is far from rare in such situations; indeed, referential uses of incomplete definites are ubiquitous. Given the usual availability of other devices for expressing singular thoughts, why would we, day in and day out, use definites for that referential purpose if it were not their conventional role to be so used?[36]

Third, rejecting an argument for PEd. In my view, the arguments we have been considering left only one argument *for* PEd still standing. That argument is due to Saul Kripke (1979) and has become known as "Kripke's Test".[37] If definites really were ambiguous we would expect to find some languages that removed the ambiguity. Yet we don't find such languages. How could an RD-theorist explain this surprising fact? Felipe Amaral (2008) has indicated how in the course of demolishing this last argument against RD. First, he found some languages that do, near enough,

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[34] I draw here on my 2007b. Bach has recently returned to this matter of temporal priority, but without mentioning the problem I allege it poses for PEd. Instead, on the basis of his persistent misrepresentation of my discussion of dead metaphors – see note 16 and accompanying text – he claims that *I* am committed to the priority of the attributive use and that the priority "is highly implausible, and not Devitt or anyone else has presented evidence for this" (2013: 239). In fact, I have no commitment to the priority of either sort of use over the other, as I made clear when I first raised the problem for Bach's view (2007a: 20). However my argument that Bach *is* committed to a priority that he now finds "highly implausible" still awaits a response.

[35] Bontly (2005b: 9–12 is not persuaded. And Bach disagrees (2004: 198–9, 228), but I argue that he is wrong (2007a: 25–7).

[36] This argument is, in effect, "Argument II" in "Case" (2004: 287–8). Two other arguments in "Case", "Argument IV: Weak Rigidity" (295–7) and "Argument VI: Exportation from Opaque Contexts" (pp. 303–5), add a little to the case for RD.

[37] Kripke also made a brief argument against RD, hence for PEd, based on the anaphoric role of a definite in a certain dialogue (1979: 21). I responded tentatively that the definite might be a 'pronoun of laziness' (1981b: 522). Neale (1997) provides some evidence for this response.

remove the ambiguity: they have two definite articles with roughly the two meanings, attributive and referential. But, more importantly, he points out that Kripke's Test presumes that if definites were ambiguous they would be cases of homonymy – unrelated meanings like those of 'bank' – not polysemy – related meanings like those of 'foot'. For, if they were polysemous we should *not* expect the ambiguities to be removed in other languages. And, he argues, they do seem to be cases of polysemy. As Neale says, RD goes with the view that "the ambiguity in question is meant to be explicable or derivable in some way, an instance of *polysemy* rather than *homonymy*" (2004: 69 n. 1). And Griceans are not in a position to deny that, *were definites ambiguous*, they would be polysemous, for Griceans take the pragmatic meaning of a referential use of the definite to be derived from its use with its quantificational meaning. Kripke's Test is misconceived. So far as I know, there has been no response to Amaral's argument from those who urge PEd.[38]

This concludes my arguments for RD and against PEd. All in all, the case for RD strikes me as fairly overwhelming.

I turn finally to Neale's latest view.

9.9   Neale's Illusion

Neale accepts the Argument from Convention and no longer holds a thesis that was central to *Descriptions* (1990): that "the difference between *saying* and *meaning* lies at the heart of a characterization of referential usage". So one might have hoped that he would embrace RD and the view that definites are ambiguous. But he does not. He still thinks that "the Russellian analysis is basically correct for both attributive and referential uses of descriptions" (2004: 106). Furthermore, he now thinks that debate over RD "seems to lack real substance" (p. 71); it "is the product of a powerful illusion" (p. 106).

At the center of Neale's new view is the idea of "Gödelian completions" used in spelling out Gödel's slingshot argument (Neale 2001). According to the basic Russellian view of definites, the only contribution that 'the *F*' makes to the truth conditions of the sentences containing it is as a unique applier. To deal with incomplete definites, we noted (Sect. 9.5) that this basic view had to be enriched: the unique application may be by a supplemented 'the *F*' (explicit) or as a result of restricting the domain of 'the *F*' (implicit). Ignorance and error arguments showed that this will not, in general, work for referential uses (Sect. 9.5). Neale's Gödelian completions avoid this difficulty (pp. 171–3). His proposal is that a referential use of 'the *F*' invites the Gödelian completion, 'the x: x is F and $x = a$', where 'a' is either a simple demonstrative or a name referring to the object the speaker has in

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[38] Bach is very fond of Kripke's Test (2004: 226, 229; 2007a: 45) and it seems now to be the only argument against RD that he is still offering (2007b: 56). So he owes us a response to Amaral.

mind.[39] The Gödelian completion is to be treated in the standard Russellian way, hence the claim that the Russellian analysis is "basically correct". So, to that extent, the conservative "Unitarian" school was right. But the radical "Ambiguity" school was also right to a certain extent. For, the referential use is "a special case" of incompleteness, it is "highly regular, perhaps even conventional" (p. 173). He is even prepared to allow that it may be "part of the *meaning* or *semantics* of 'the' that on one use it invites a Gödelian completion". So, "in a sense, everyone was right and everyone was wrong" (p. 172), hence the claim that the debate is "the product of a powerful illusion".

I have three responses to Neale's new view of referential uses. (i) I shall emphasize how much the Gödelian proposal concedes to the RD side of the debate. (ii) I shall urge that what it doesn't concede, it should. (iii) I shall claim that it is Russellian only in a technical not substantive sense. So the debate is not an illusion: RD has won.

Four claims define RD. The first is:

(1) The uncontroversially different referential and attributive uses of 'the *F*', one to convey a singular thought about a particular *F* object in mind, the other to convey a general thought about whatever is uniquely *F*, participate in different semantic conventions. The latter convention is Russellian but the former is not.

Consider the sentence 'The *F* is *G*' where 'the *F*' is incomplete. Neale agrees that there are two distinct regular uses of definites with the result that this sentence yields propositions with two distinct truth conditions. On the regular referential use it yields a Gödelian completion, a proposition equivalent to

$$(Ex)(Fx.x = a.Gx),$$

where '*a*' is a referential device that the speaker has in mind to pick out the referent. On the regular attributive use it yields one equivalent to the very different

$$(Ex)(Fx.Hx.(y)(Fy.Hy \rightarrow y = x).Gx),$$

where '*H*' is a predicate the speaker has in mind to compete the definite or restrict its domain. Neale does not commit to the view that these two regularities are distinct semantic conventions but he is prepared to go along with that view. For the reasons presented in this chapter, I think that he should go along.[40]

If the regularities are semantic conventions then the only respect in which Neale might be at odds with (1) is in his claim that the referential use is Russellian. But is

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[39] Notice that this proposal is not guilty of the parasitism that I criticize (2004: 300, 302). It does not involve *describing* the perceptual-causal link but rather including a demonstrative element in 'the *F*' that exploits that link.

[40] If he did, then Elbourne would no longer be right that Neale's solution does not require "an ambiguity in the lexical entry of the definite article" (2013: 111).

the Gödelian completion *really* Russellian? Note that it yields a proposition that is *equivalent* to a conjunction of singular propositions,

$$Fa.Ga.$$

So it is hard to see how the Gödelian completion yields something that is in any *interesting* sense a general proposition and hence Russellian. We might say that it is syntactically Russellian but not semantically so; it is only "pseudo-Russellian". Neale himself remarked earlier:

> A phrase of the form '[the *x: x = a*]' is technically a Russellian definite description; but the claim that referential uses of descriptions do not require distinctive non-Russellian interpretations would indeed be hollow if the Russellian position could be maintained only by employing the identity relation to concoct descriptions of this form (e.g. '[the *x: x = * that]'). (1990: 115 n. 53)

The hollowness is demonstrated by the fact that when a "concocted" description of this form is joined to the predicate '*G*' it yields a proposition that is equivalent to 'That is *G*'. And the hollowness of Neale's present claim that, because of the appeal to Gödelian completions, the Russellian position is "basically correct" is similarly demonstrated by the equivalence of '*(Ex)(Fx.x=a.Gx)*' to '*Fa.Ga*'.

The next claim that defines RD is:

(2) The referential convention for definites exploits a perceptual-causal link to the object in mind to achieve uniqueness.

Neale does not explicitly embrace (2) but doing so would fit well with his views. For, he has earlier accepted, a little tentatively, the view that an "object-dependent" thought – what I am calling a "singular" thought – is about its object partly in virtue of its perceptual-causal link to the object (1990: 18).

Another definitive claim is:

(3) The referential convention for definites is very similar to that for a complex demonstrative.

Neale says of complex demonstratives that they are "Gödelian *by nature*. An act of reference is signaled *as a matter of linguistic convention*" (2004: 174). 'That *F* is *G*' yields a proposition equivalent to

$$(Ex)(Fx.Gx = \text{that})$$

The proposition yielded by the referential 'The *F* is *G*' differs from this only in having '*a*' in place of the simple demonstrative 'that', where '*a*' stands in for *either* 'that' *or* a name. But I can see no motivation for this difference and the similarity in the roles of definites and complex demonstratives (Devitt 1981a, b, 2004: 288–90; 2007a: 25–7) counts against there being the difference. If one takes the Gödelian route for referential definites (but see (4) below), then we should take the referential 'The *F* is *G*' as expressing the *same* proposition as 'That *F* is *G*' (the differences between them being merely pragmatic). As I noted in "Case", we should treat the

referential 'the *F*' as "implicitly containing something like the simple demonstrative 'that'" (2004: 292).

This modification of Neale's proposal further undermines the idea that the Gödelian route for referential definites is genuinely Russellian. That idea gets support from the claim, reflected in the very name "Gödelian completion", that referential definites are typically *in*complete. So the suggestion is that they, just like the indisputably Russellian attributive definites, typically need to be completed, albeit in a different way. Now there is no doubt that token referential definites are typically incomplete in the usual sense of not uniquely describing an object. But it is important to see that if my modification is correct they are not incomplete in another sense: they are not *semantically* incomplete and so do not need to *be* completed to convey the intended message. Token referential definites are *already* semantically complete, as complete as token complex demonstratives. Like the demonstratives they already have underlying them the causal-perceptual link that determines reference. If we take the Gödelian route, referential definites are as "Gödelian *by nature*" as complex demonstratives.

The final definitive claim of RD is:

(4) The '*F*' in the referential 'the *F*' is semantically significant.

"Case" (2004: 292) mentions three ways of making '*F*' semantically significant but remains neutral on the choice between them.[41] The ways are: (i) '*F*' is partly determinate of the reference of 'the *F*';[42] (ii) 'The *F* is *G*' is equivalent to 'That is *F* and *G*'; (iii) the modified Gödelian route just discussed. Neale's unmodified Gödelian route clearly gives semantic significance to '*F*' and so is quite compatible with (4).

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[41] Bach notes my neutrality in "Case" but then strangely continues: "In this regard it is not clear that his discussion precludes an account on which 'the *F*' (on its alleged referential reading) means the same as 'the particular *F* I am talking about/thinking of', a description that is uniquely satisfied by the unique object the speaker has in mind" (2004: 225). It surely *is* clear that my discussion precludes this account: I say I am neutral between the three possibilities (i) to (iii) and this is not one of them! Furthermore, I *argue against* an account like Bach's – an account analogous to causal descriptivism for proper names (which is criticized by Devitt and Sterelny 1999: 61) – on the grounds that it is theoretically redundant (2004: 300, 302). You could roughly characterize my view as that the referential meaning of 'the *F*' is provided by the (causal-perceptual) link to the object thought about together with the contribution of '*F*'. A proposal like Bach's is an example of a description theory that is parasitic on this sort of causal theory. According to the causal theory, most of the meaning of 'the *F*' is determined by *a link* to the object. According to a parasitic description theory, most of the meaning is determined by *an associated description of that link* to the object. In my paper I remarked that, so far as I knew, no such parasitic theory had been proposed for referential descriptions. I went on to say that, "given the history of theories of names and demonstratives, it seems likely that one soon will be" (p. 300). It didn't take long!

[42] I have always favored (i) in earlier works (1981a, b). A consequence of (i) is that if the object in mind is not *F* then 'the *F*' will lack a semantic referent and the containing utterance will lack a truth-conditional what-is-said (though doubtless conveying a truth-conditional message). I take it that Esther Romero and Belén Soria (2019: 416–17), in their interesting criticisms of an earlier draft of my book, are insisting that the utterance must have a truth-conditional what-is-said nonetheless. And this partly motivates their introduction of a pragmatic notion of what-is-said. But I don't see a good theoretical basis for this insistence. I return to their criticisms in Sect. 11.4.6 note 35.

In sum, Neale's Gödelian proposal, as it stands, is close to RD. It would become even closer if he accepted, what should be congenial, that the regular referential uses constitute a semantic convention and that this convention exploits a causal-perceptual link. It would then *become* RD if he accepted, as I have argued he should, the modification that takes referential definites to be like complex demonstratives. Finally, this proposal is not in any interesting sense, Russellian: it is only pseudo-Russellian. So the debate over referential uses of definites is not an illusion: RD has won. Neale's contrary view reminds one of Senator Aiken's witty advice to President Johnson when the Vietnam War was going very badly for America: "Just declare victory and go home."

9.10   Conclusion

In Chap. 3 I argued that the "semantic" properties that constitute "what is said" – the properties that symbols have in virtue of being uses of a language – arise from (i) convention, (ii) disambiguation, and (iii) linguistically demanded saturation (slot filling). I promised to argue that the examples thought by Pragmatists to challenge the tradition that they seek to overthrow typically exemplify properties of sorts (i) to (iii). There are more of such properties than we have previously acknowledged: much more of the content of messages should be put into the convention-governed what-is-said – into semantics – than has been customary. I have started on this project of defending SEM in this chapter by considering definite descriptions, a favorite example used in support of PRAG and the Pragmatist challenge.

Definite descriptions illustrate previously unacknowledged semantic properties of all three sorts. Concerning (i), a referential use of a definite exemplifies an overlooked referential convention. Concerning (ii), since there is also a quantificational convention for definites, what-is-said when a definite is used referentially is partly constituted by disambiguation. Definites exemplify semantic polysemy. Concerning (iii), the convention for referential definites demands saturation in the context. Referential definites exemplify semantic saturation.

Many Pragmatists treat these referential uses in a Gricean way as involving conversational implicatures (or something similar). Others treat them in a Relevance-Theoretic way where both referential and attributive uses involve other sorts of pragmatic modifications (Sect. 9.2). I have argued that referential (and attributive) uses are best explained semantically.

The Argument from Convention (as Neale calls it) provided the positive argument for this SEM view. The basis for the thesis that descriptions have referential meanings is not simply that we *can* use them referentially for, as the Pragmatists point out, we can use *any* quantifier referentially. The basis is rather that we *regularly* use descriptions referentially. Indeed, the vast majority of uses of descriptions are referential. This regularity is because they exemplify a semantic convention (Sect. 9.3).

The argument in Chap. 8 that appealed to dead metaphors shows that for the alternative PRAG explanation to be right, more is required than that the singular proposition arising from a referential use of a definite can be pragmatically derived from the literal meaning of the utterance. The PRAG explanation, whether Gricean or Relevance-Theoretic, must meet the "psychological-reality requirement", showing that the explanation's pragmatic derivation has an appropriately *active* place in cognitive lives. This requirement has not been met, nor has there been any psycholinguistic attempt to meet it. According to the Developmental Objection it is unlikely to be met. According to the Occamist Objection, we should then prefer the SEM view (Sect. 9.4).

There was a further objection to Gricean versions of PRAG explanations. An important feature of such explanations of a referential use of 'the *F*' is that a speaker who is pragmatically implying a singular proposition about a particular object in mind must also be conventionally conveying a general proposition about whatever is uniquely *F*. Attending to incomplete descriptions, I used an "ignorance and error" argument to show that this condition could frequently not be met (Sect. 9.5).

I reinforced this case by considering Bach's lengthy defense of the Russellian *status quo*. He argues that referential uses are "akin to" generalized implicatures. The heart of his argument is the claim that the quantificational meaning plays a "key role" in referential uses. This claim is not supported and the argument fails (Sects. 9.6 and 9.7).

That concluded my main case for the SEM view, but I add three further arguments (Sect. 9.8). Finally, I looked critically at Neale's view (2004) that the debate between referentialists and Russellians is "the product of a powerful illusion". The debate is over a real issue and the referentialists have won (Sect. 9.9).

The project of responding to the Pragmatist challenge continues in the next three chapters, discussing other cases of saturation (Chap. 10), other cases of polysemy (Chap. 11), and finally sub-sententials (Chap. 12).

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Chapter 10
Saturation and Pragmatism's Challenge

10.1 Pragmatism's Challenge

Linguistic Pragmatism (contextualism) opposes traditional views on the semantics-pragmatics issue coming from "truth-conditional semantics" and with roots in formal semantics. François Recanati, a leading figure in the movement, calls the traditional view "Minimalism" (2010: 5). On this view, with the two important qualifications, disambiguation (my (ii)) and linguistically demanded saturation (slot filling), particularly reference fixing (my (iii)), a sentential utterance has its truth-conditional content simply in virtue of the (largely) conventional rules of the speaker's language (my (i)); the content of the message is what the utterance literally means. This content is typically thought to be "what is said" by the utterance and its constitution is typically thought to be a "*semantic*" matter. I have presented a doctrine of this sort in Sect. 3.5:

> **SEM**: Setting aside novel uses of language, what-is-said (in my sense) is typically the truth conditional speaker-meaning (content) of an utterance, the message conveyed. That message is typically a WIS-meaning.

In contrast, the Pragmatists think that these qualifications (ii) and (iii) do not go nearly far enough and so urge a "truth-conditional pragmatics" according to which the truth-conditional content varies from context to context and the variation is a "*pragmatic*" matter.[1] So the content is partly constituted pragmatically. I have attempted to captured this doctrine (Sect. 3.5) as follows:

> **PRAG**: Even setting aside novel uses of language, the truth conditional speaker-meaning (content) of an utterance, the message conveyed, is seldom, perhaps never, constituted solely by what-is-said (in my sense), a WIS-meaning. The message is always, or almost always, the result of pragmatic modification, a PM-meaning.

In Sect. 3.5 I quoted Recanati's statement of this sort of view:

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[1] It is a vexed issue exactly what "semantic" and "pragmatic" do and should mean; see Sect. 3.3.

According to contextualism, the sort of content which utterances have…can never be fully encoded into a sentence; hence it will never be the case that the sentence itself expresses that content in virtue solely of the conventions of the language. Sentences, by themselves, do not have determinate contents. (Recanati 2003: 194).[2]

Here are four more statements:

…in general, the meaning of a sentence only has application (it only, for example, determines a set of truth conditions) against a background of assumptions and practices that are not representable as a part of meaning. (Searle 1980: 221)

What words mean plays a role in fixing when they would be true; but not an exhaustive one. Meaning leaves room for variation in truth conditions from one speaking to another. (Travis 1996: 451)

lexical interpretation typically involves the construction of an *ad hoc concept* or occasion-specific sense, based on interaction among encoded concepts, contextual information and pragmatic expectations or principles. (Wilson and Carston 2007: 230)

All concepts occurring in communicated thoughts (explicatures) are pragmatically inferred and merely constrained by an encoded lexical schema/template or an array of activated encyclopaedic information (a grabbag). In that sense, all concepts expressed or communicated are 'ad hoc'. (Carston 2012: 622)

These authors are urging a context dependency of meaning that goes way beyond the uncontroversial dependency arising from ambiguity and reference determination, as Anne Bezuidenhout makes very clear in another passage I quoted in Sect. 3.5:

meaning underdetermines truth-conditions. What is expressed by the utterance of a sentence in a context goes beyond what is encoded in the sentence itself. Truth-conditional content depends on an indefinite number of unstated background assumptions, not all of which can be made explicit. A change in background assumptions can change truth-conditions, even bracketing disambiguation and reference assignment….contextualists claim that there is a gap between sentence meaning and what is asserted, and that this gap can never be closed….the radical context-dependence of what is said. (2002: 105)

The striking idea that sentence meaning can be supplemented in an *indefinite* number of ways to yield an *indefinite* number of different truth conditions is to be found in the following passages too:

What the English '___ grunts', or any other open English sentence, *means* leaves it open to say any of indefinitely many different things, at a time, of a given item, in using that open sentence of it. (Travis 2006: 40)

words can take on an indefinite variety of possible senses. (Recanati 2004: 134)

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[2] Recanati distinguishes two versions of truth-conditional pragmatics:

TCP is the weaker of the two. It holds that the linguistic meaning of an (ordinary, non-indexical) expression *need not be* what the expression contributes to propositional content. Radical Contextualism (RC) holds that it *cannot be* what the expression contributes to propositional content. (2010: 17)

Recanati used to urge the weaker TCP (2004, 2010) but has recently endorsed the stronger RC (the "Wrong Format View"; 2017: 394–5).

an expression may, but need not, contribute its sense – i.e. the sense it independently possesses in virtue of the conventions of the language; it may also contribute an indefinite number of *other* senses resulting from modulation operations… applied to the proprietary sense. (Recanati 2010: 19)

I conceive of the semantics-pragmatics issue as the SEM-PRAG issue. In so doing I have emphasized that the widely-accepted variation in truth-conditional content arising from ambiguity and reference determination is not the issue. The talk of a possibly indefinite number of other variations of content in context prompts emphasis of something else that is not the issue: variations arising from *novel*, spur-of-the-moment, on-the-fly, uses of language, discussed in Sect. 3.5.

(1) It is taken for granted by all that, by varying the background knowledge, a sentence can be used in a novel way to convey indefinitely many messages that it does not *literally* mean in context. Grice (1989) showed with examples like the philosopher's letter of recommendation that an utterance of a sentence that literally *said that p* might, given an appropriate background, be used to *conversationally implicate* that *q*. The hearer can then use a "pragmatic inference" to derive that implicature from what-is-said. It is easy to see then that, by varying the background, we can vary the truth-conditional implicature. The novel implicatures in question here are "particularized" ones. I am dubious that there are any "generalized" conversational implicatures (Sects. 8.4, 9.4, 11.2.6).

(2) We should also all accept that there can be novel *modulations* of conventional linguistic meanings. Utterances are often elliptical: a *more precise* message than the truth-conditional what-is-said (in my sense) is conveyed in the context. I gave a nice example from Ingrid Falkum: a biology teacher talking of feces says, 'Rabbit is smaller than hare' (2015: 88). Nominal compounds like Recanati's 'burglar nightmare' (2004: 7) may provide other examples. In such cases, a vague what-is-said is spontaneously *enriched* in context into a more precise message; the speaker conveys the precise proposition she means with the help of the imprecise proposition she expresses. And spontaneous pragmatic modulations can be *impoverishments*: the proposition meant is *less* precise than the proposition said. I gave Carston's example, 'Her face is oblong' (2002: 27).

It is obvious that the meanings conveyed by novel implicatures are to be explained pragmatically and those by novel modulations, partly pragmatically. The interesting issue is whether there are pragmatic variations in content from context to context *other than* these novel ones. The Pragmatists' challenge, revealed by the quotations displayed above, is that there are many more. PRAG is my attempt to capture their position. The Pragmatists believe that, even setting aside the novel, pragmatic modifications always, or almost always, play a role: the message is typically, perhaps even always, a PM-meaning. The meaning communicated is seldom, perhaps never, constituted solely by a WIS-meaning and so is "semantically underdetermined"; a new theoretical framework is called for.

This position leads Pragmatists to deploy notions of what-is-said that are different from my semantic WIS as I noted in Sect. 3.5. With my notion, a conversational implicature is of course pragmatic but the meaning that the speaker uses in making

the implicature may be unmodified, an entirely semantic what-is-said. With Pragmatists' notions, what-is-said is always, or almost always, the result of a pragmatic modification of some sort or other; see the discussion in Sect. 9.2 for an example. This goes with their belief in, as Bezuidenhout puts it above, "the radical context-dependence of what is said". There are thought to be pragmatic contributions not just at the "secondary" level of implicatures but at the "primary" level of what-is-said (Recanati 2004: 21); pragmatics is involved "from the beginning".[3]

These remarks about what-is-said indicate something else that the semantics-pragmatics issue is *not* about: it is not about which notion of what-is-said is *right*, a largely verbal issue. It is a dispute about the substantive differences that motivate the choice of a notion. I shall of course continue to deploy *my* notion of *what-is-said* in the response to the Pragmatist challenge to follow.

The challenge posed by Linguistic Pragmatists stems from the extensive context relativity that their investigations have revealed: they have discovered many examples, like (1) to (8) in Sect. 1.1, where the truth-conditional meaning communicated by a sentence varies in context. I claimed in Sect. 3.5 that that the challenge can be met. The Pragmatists' striking examples of context dependency typically exemplify semantic rather than pragmatic properties (in my senses, of course): the traditional ways of dealing with context dependency within truth-conditional semantics need to be extended to cover many more linguistic phenomena. There are many more meaning properties arising from conventions, disambiguations, and linguistically demanded saturations (slot fillings) than have been previously accepted. All of these go into the semantic what-is-said; there is no interesting "semantic underdetermination". This view yields SEM, in the spirit of the tradition that Pragmatism rejects. No new framework is called for. I aim to reject truth-conditional pragmatics and PRAG by showing how many of those striking examples might be accommodated semantically. I look for evidence, as always, in behavior, in particular, in the regular use of an expression with a certain speaker meaning.

In responding to this challenge, it is convenient to divide the phenomena identified by the Pragmatists into two groups. (I) I shall argue that one group, illustrated by examples (1) to (4) in Sect. 1.1, should typically be explained as linguistically demanded saturations in context not as saturations arising from pragmatic modifications of one sort or another. So I shall be arguing that these saturations are typically semantic not pragmatic. This exemplifies the extended use of 'saturate', introduced in Sect. 9.2, that does not reserve the term for linguistically demanded slot fillings but leaves it as an open question whether saturations are semantic or pragmatic. (II) I shall argue that the other group, illustrated by examples (6) to (8) should typically be explained as examples of semantic polysemy that are disambiguated in context. Here I follow the standard use of 'polysemy' which leaves it as an open question whether polysemies are semantic or pragmatic. Interestingly, definite descriptions,

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[3] Where do the "relevance theorists" stand on this? Carston has this to say: "the Travis/Recanati concept of 'what is said', as inevitably involving extensive pragmatic input, is very close to the relevance-theoretic view, though there the terms are 'proposition expressed' and 'explicature'" (2002: 170; see also: 20).

illustrated by example (5), are in both groups, as we have in effect just noted (Sect. 9.10).

The distinction between saturation, illustrated by (1) to (4), and polysemy, illustrated by (6) to (8), is intuitive, but a further word about it is appropriate. With a saturation, a meaning is supplemented in context by a reference to an "entity" of some sort that the speaker has in mind, perhaps an object, a relation, or whatever. But what is added could be *any* entity of the appropriate sort; there is no limit to what could fill the slot; "Everybody went to Paris" could be about any group. With a polysemy, in contrast, one of a set of meanings is conveyed in context but the meanings are all related and the set is often small and is certainly not limitless; there is no question of just *any* meaning being conveyed. The distinction between saturation and polysemy is doubtless not sharp but this does not matter to my case for SEM. What matters is that all of the phenomena that motivate PRAG are in either the saturation group or the polysemy group (perhaps both), for I argue that the members of each group is typically to be explained semantically.

The main argument about saturation is in this chapter, that about polysemy in Chap. 11. Finally, in Chap. 12, I turn to sub-sententials, a special case of saturation in context. There will be no pretense of comprehensiveness in any of these discussions. But I aim to treat enough examples to make it plausible that the Pragmatist challenge can indeed be me along the lines proposed in Chap. 3.

But first we shall look briefly at meaning eliminativism.

10.2   Meaning Eliminativism

According to PRAG, "the message is always, or almost always, the result of pragmatic modification". The most radical contextualists insist on "always". According to Recanati, they have problems with the idea that expressions have meanings or senses at all: they reject the Fregean presupposition "that the conventions of the language associate expressions with senses" (2010: 18). Recanati himself prefers to talk of expressions having "semantic potential" rather than meanings (2004: 97). As Herman Cappelen and Ernest LePore point out, other Pragmatists talk of "linguistic expressions…providing 'incomplete logical forms,' 'semantic skeletons,' 'semantic scaffolding,' 'semantic templates,' 'propositional schemas'" (2005: 7). But everyone should accept that expressions have *some* conventionally determined property, *whether called a "meaning", "sense", "semantic potential", or whatever*, that *constrains* truth conditions. For, this constraint is simply a consequence of supposing that people are using a language at all. In supposing this, we are supposing that, simply in virtue of being in the language, an expression has some (largely) conventionally-determined property that contributes to conveying the message. And it contributes by constraining truth conditions, thus making the interpretative task of the hearer not *simply* a matter of mind-reading. It is only because both speaker and hearer participate in the conventions that have led to this constraining property that the expression can play its crucial role in communicating a message.

The view that expressions do *not* have a conventionally determined property, like a meaning or semantic potential, is "Meaning Eliminativism". It challenges what Lewis rightly calls a "platitude" (1969: 1). Recanati describes Meaning Eliminativism as follows: "Meanings for types undergo wholesale elimination, in favour of the senses contextually expressed by particular tokens" (2004: 141). He finds the view "probably too extreme" but "surprisingly viable" (p. 151). Ruhl expresses Meaning Elimativism like this: "A fully minimalist position would claim that words have no inherent meaning at all, deriving all their apparent contribution from functional context" (1989: ix). Ruhl's own "meaning-minimalism" certainly does not go that far. Still, Yael Ravin and Claudia Leacocke, in their "Polysemy: An Overview" (2000), describe some views that seem to come perilously close. And Recanati (2004: 141–53) finds signs of Meaning Eliminativism in Wittgenstein, Austin, Searle and Travis. I trust that the signs are misleading because the view seems preposterous, apparently denying that we have a language at all. My "fundamentalist Gricean" argument (Sect. 8.2) was intended to be a *reductio ad absurdum* of this sort of thinking.[4]

We turn now to the Pragmatists' challenge arising from their many examples of the context-relativity of meaning.

10.3   Semantic Saturation

One way that the tradition has taken account of context relativity is by noting that the references of indexical elements are determined in context. Indexicals have a conventional linguistic meaning – what David Kaplan (1989a, b) calls a "character" – that does not fully determine a truth condition – what Kaplan calls a "content". To get a truth condition the convention demands saturation: there is a slot that must be filled in context.[5] So, according to the tradition, a Kaplanesque character provides the needed constraint on truth conditions; and variations in what the speaker has in mind in context can yield indefinitely many truth conditions. I suggest that many of the Pragmatists' context-relative phenomena can be explained in an analogous way and thus accommodated within that tradition. My first step to defend SEM was to argue, in Chap. 9, that one phenomenon can be thus accommodated: the indubitably regular referential use of definite descriptions is best explained as a convention that demands saturation in the context. I shall soon give some other examples.

But, first, a clarification. At one point, Recanati describe a doctrine that is like the most radical version of PRAG as "a form of contextualism that 'generalizes

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[4] Andrea Bianchi (2020b) has pointed out that a consequence of the views of a recently-formed group that he labels the "neo-Donnellanians" (see particularly, Almog et al. 2015 and Pepp 2018) seems to be that there are no languages. I have discussed this view (2020b: 388–97).

[5] Harking back to our discussion of the bee's waggle dance (Sect. 3.2.4), we might say similarly that the dance has a "character" that demands saturation in context by the direction of a spot on the horizon to determine a "content".

indexicality'" to all terms (2010: 19). In a paper discussing Recanati's views I expressed puzzlement about this: "If [PRAG] did thus generalize indexicality it would not be truth-conditional *pragmatics* but an implausibly radical version of truth-conditional *semantics*" (2013b: 93). Recanati has responded to this comment:

> It is essential to the traditional view that indexicality is a limited, circumscribed phenomenon, for the goal of the traditional view in its successive guises has always been to *minimize* the gap between linguistic meaning and representational content. (2013: 106)

Recanati has in mind a traditional view that restricts indexicality to what Cappelen and Lepore call a "Basic Set of Context Sensitive Expressions" (2005: 2): personal pronouns and demonstratives, and words like 'now', 'yesterday', and 'enemy'. Cappelen and Lepore's list comes from David Kaplan's in "Demonstratives" (1989a). They note that Kaplan gives no defense of his list and they don't either. But why should we take a restriction to this list as essential to the traditional view? I am arguing, in effect, that the same theoretical considerations that motivated the tradition's inclusion of those items in the Basic Set motivate the inclusion of other items (though one might reasonably object that all these examples of saturations in context are not examples of "indexicality").[6]

In Sect. 8.4 I pointed out that SEM entails that expressions with regular uses that make them polysemous are typically explained semantically by SEp; and that PRAG entails that those expressions be typically explained pragmatically by PEp. There are similar demands on expressions of a form that is regularly used with speaker meanings arising from saturation in context. Suppose that, on a particular occasion, a speaker uses such an expression *E* to mean *M*. SEM entails that the following explanation is typical:

> **SEs**: *E*'s property of speaker-meaning *M* after saturation is semantic (in my sense). So *M* partly constitutes what-is-said, my "WIS"-meaning (Sect. 3.5). On other occasions, the speaker meaning of *E* after saturation may be different semantic meanings.

PRAG entails the following rival explanation is typical:

> **PEs**: *E*'s property of speaker-meaning *M* after saturation is a pragmatic modification of *E*'s semantic meaning and so is at least partly pragmatic (in my senses). So *M* partly constitutes a pragmatic modification of what-is-said, my "PM"-meaning (Sect. 3.5). On other occasions, the speaker meaning of *E* may be different modification of *E*'s semantic meaning,

So SEM entails that *E*'s saturation is typically linguistically demanded and hence semantic; PRAG, typically not so demanded and hence a pragmatic modification.

I shall argue that *E*'s saturation is indeed typically semantic; SEs is the rule. Referentially used definite descriptions provide one example. Our initial examples of phenomena that have led to PRAG (Sect. 1.1) provide four more, each an utterance that implicitly has a slot to be filled; each requires saturation by an implicit reference to something the speaker has in mind:

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[6] So this is one major respect in which my handling of context-relativity differs from that of Cappelen and Lepore (2005). Another is in my attention to polysemy (Chap. 11), a subject that plays almost no role in their discussion.

(1) I have had breakfast. [Implicit reference to a period]
(2) You are not going to die. [Implicit reference to a potential cause of death]
(3) It is raining. [Implicit reference to a location]
(4) Everybody went to Paris. [Implicit reference to a domain]

Note that, in each case, the implicit reference, can be made explicit: "I have had breakfast this morning"; "You are not going to die from that minor cut"; "It is raining in New York"; "Everyone at the conference went to Paris". Note also that there are indefinitely many possible saturations for the sentences in utterances (1) to (4) just as there are for referential definites or demonstratives. We know from informal observation that each of (1) to (4) exemplify a *regularity* of saturating expressions of a certain form to convey a message. Indeed, Pragmatists don't just note these regularities they emphasize them. These regularities can be plausibly explained by supposing that there are linguistic rules for these expressions, brought about by conventions, that demand saturation in context. And that, I claim, is the best explanation of them.

I talk of what is implicit as something the speaker "has in mind". I have offered some thoughts on what this ordinary way of talking amounts to theoretically in the case of referentially used descriptions: an underlying causal-perceptual link to an object (Sect. 4.1.2). This provides a *model* of how to explain saturation but clearly work has to be done to apply this model to these other cases. This is most obviously so the further the cases depart from the paradigm cases of implicit demonstrative reference to a particular concrete object.

Recanati would insist that SEs could not be right for (1) to (4). He takes Minimalism, the traditional view, to involve the following constraint:

'what is said'…departs from the conventional meaning of the sentence (and incorporates contextual elements) *only when this is necessary to 'complete' the meaning of the sentence and make it propositional*. (2004: 7)

Incorporation of contextual elements is *semantic* saturation, and hence is in order for Minimalism, only when the incorporation is mandatory to make the utterance propositional:

A contextual ingredient is mandatory in the relevant sense, and is provided through saturation, only if *in every context* such an ingredient has to be provided (precisely because the need for saturation is not a contextual matter, but a context-independent property of the expression type). (p. 98)

If "the utterance would still express a complete proposition" without the contextual element, then the element is "optional" and "external to what is said" (p. 8); its incorporation in the semantics does not meet the "Optionality Criterion" (p. 101).[7] And the problem with (1) to (4) is that their saturations are all optional. 'I have had breakfast' might indeed have meant simply that I have had breakfast sometime or other just as, to take Kenneth Taylor's (2001) charming example, 'I have had sex' is

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[7] Bach has a similar idea: "Enrichment is free, a case of *expansion,* if the sentence semantically expresses a proposition, forced if it fails to." (2017: 50)

likely to mean that I have had sex sometime or other. These utterances are propositional without any addition. Similarly, although 'You are not going to die' is unlikely to state the proposition that you are immortal, and 'Everybody went to Paris', that everyone in the world went to Paris, they could. And though at first sight, 'It is raining' seems to demand saturation by a particular location to be propositional, Recanati has ingeniously imagined a situation where the utterance conveys the message that it is raining "in some place or other" (2002: 317). Furthermore, consider: "Why does it rain? It rains because water vapour in the air condenses and…" (Carston and Hall 2017: 61). The problem, we might say, is that the sentences in (1) to (4) have not only a "particular" but also a "general" interpretation.

My first response to this objection is to suggest that maybe (1) to (4) *do* meet the Optionality Criterion. Let us start with (4), 'Everybody went to Paris'. Now it is natural to talk of such uses of a quantifier as involving an implicit *restriction* of its domain. This presumes that whatever domain fits the quantifier's description, in this case the domain of all people, is the *default* or *standard* that then may be further restricted in context. But it would be more apt to talk of the use of a quantifier as involving an implicit *specification* of its domain. That specification is of whatever domain fits the quantifier's description or of a particular part of that general domain, depending on what the speaker has in mind. The specification of a domain would then be mandatory. So the saturation in (4) would meet Recanati's Optionality Criterion for being semantic: SEs applies, as SEM predicts.

We can try the same approach to (1) to (3). 'I have had breakfast' demands reference to a past period but that period might by just some time or other in the past. The implicit reference of 'You are not going to die' could be to *any* potential cause of death at all. And that of 'It is raining' could be to some location or other. In each case, saturation is mandatory. It is usually to something particular but can be quite general. Again the Optionality Criterion is met.

So, it is not obvious that (1) to (4) do fail to meet the Optionality Criterion for being semantic. But perhaps they do fail and my first response does not work. So, my second response is more important: the Optionality Criterion is false. It overlooks the possibility of polysemous ambiguity. SEM insists that each of the sentences in question in (1) to (4) have a conventional linguistic meaning with an implicit slot to be filled, a meaning that mandatea saturation. According to my first response this saturation may yield a general interpretation. Suppose that is wrong and so the saturation must be to something in particular. *That is quite compatible with the sentence having another meaning with no slot to be filled, that does not mandate saturation, and that yields the general interpretation*. The SEs explanation of, say, 'It is raining', rests on the obvious fact that this sentence is regularly used with a saturated meaning. SEM claims that SEs is the best explanation: 'It is raining' has a conventional meaning that demands saturation. The possibility of its having *another* meaning that does not demand this is beside the point.

Indeed, the Optionality Criterion – a sentence demands saturation only if saturation is necessary to make the sentence propositional – obviously fails when we take account of ambiguous sentences. The failure is particularly striking with definite descriptions. It is widely held, even if not quite by some Relevance Theorists

(Sect. 9.2), that 'the *F* is *G*' has a linguistic meaning captured by Russell, a general interpretation which does not demand saturation to express a proposition. It would be bizarre to suppose that *this alone* settles the case against the thesis, argued in Chap. 9, that the sentence *also* has a "referential" meaning that does demand saturation. The case for the referential meaning is the Argument from Convention, resting on the regularity of saturating 'the *F*' with a particular object in mind (Sect. 9.3). The case obviously does not depend on denying that the expression has another meaning that does not demand saturation.

In sum, perhaps we should see the sentences in (1) to (4) as demanding a saturation that could be either particular or general. But it may well be better to see the sentences as ambiguous, having both a general sense that does not demand saturation and a sense that demands saturation by a particular something in mind. One way or another, we have to capture the fact that there is a conventional use of sentences of these types that involves implicit reference to something in mind. The resulting meaning is the literal semantic meaning of such a use. The evidence for this, as I have been emphasizing, lies in the regular use of these types of sentences in this way.

"But how are these proposals to be handled in the syntax?" For example, is the second proposal following Louisa Martí (2006) in positing optional covert indexicals in the syntax of these sentences? No, it is not. *I take no stance at all on such interesting syntactic questions*. And for reasons I shall indicate in Sects. 10.5–10.6, I don't think that a defense of SEM requires such a stance.

I accept that all of the sentences in (1) to (4) have general interpretations as well as particular ones; 'eat', 'dance', and 'sing' provide other examples. But there are some expressions with particular interpretations that arguably lack a general one: they lack a conventional use that does not demand reference to something in particular; they are, as they stand, "semantically incomplete" (Taylor 2001: 53). Thus, Recanati claims: "'I heard' or 'I noticed' cannot be interpreted as meaning that the speaker has heard or noticed *something or other*" (2004: 99). I shall soon argue that SEs applies to these allegedly incomplete expressions, in particular 'ready' and 'qualified'. But first, we must consider a dubious argument for their incompleteness.

Cappelen and Lepore (2005) emphasize that the alleged semantic incompleteness of sentences has played a big role in contextualist arguments that utterances of these sentences involve pragmatic modifications. They rightly think that these incompleteness claims are, at bottom, typically metaphysical rather than linguistic; thus, the claimed incompleteness of 'It is raining' typically rests on the view that "there's no such thing as raining *simpliciter*" (p. 11). Bach brings out the metaphysical basis of incompleteness claims nicely:

> The fact that one cannot be just plain ready but only ready for something or other is a metaphysical fact, not a semantic fact. This is a fact about readiness, not a fact about 'ready'… consider adjectives like 'relevant', 'qualified', and 'legal' and what they semantically express. A topic can't be just plain relevant, a person can't be just plain qualified, and an action can't be just plain legal. That is, being relevant, being qualified, and being legal are not properties but what might we might call *property functions*. These are metaphysical

facts about being relevant, being qualified, and being legal, not linguistic (lexical) facts about the words that express them. (2013: 243–4)

Along similar lines in a later work discussing 'better', Bach claims: "Nothing can be just plain better" (2017: 46). Cappelen and Lapore firmly reject such metaphysical intuitions (2005: 155–75). And the intuitions do indeed seem dubious.[8] Thus, although it seems true that to be ready is to be "ready for something or other", this alone does not show that "one cannot be just plain ready". For, consider being a father. To be a father is to be the father of someone or other but nonetheless one can be just plain a father. Indeed, we should say that a person is just plain a father *in virtue of* being the father of someone or other. Similarly, one can just plain eat *in virtue of* eating something or other. I don't see a good empirical basis for denying that a person is similarly just plain ready *in virtue of* being ready for something or other. I conclude that we should not rest a case for anything, whether a PEs or SEs explanation, on dubious metaphysical incompleteness intuitions like that one cannot be just plain ready, qualified, and so on; and that it cannot be just plain raining.

Time for my argument that SEs applies to the likes of 'qualified'. Whether or not we can have the "general" thought that a person is *plain* qualified, we can certainly have one that a person is qualified *for something or other*. Yet, as a matter of fact, we hardly ever do: virtually all thoughts about a person being qualified that we ever have are about her being qualified for something in particular. That seems to me an incontrovertible empirical fact. These "particular" thoughts are ones we often want to convey to audiences. Whenever there are thoughts of a certain sort that we often want to convey, we have an interest in having an expression in our language that is a *conventional* way of conveying those thoughts. So we are likely to have such a convention. If we do, then the relevant expression is likely to be *regularly used* to convey such thoughts. The literature shows that many people have already observed that '*X* is qualified' is indeed regularly used to convey a speaker's thought that *X* is qualified for something in particular that the speaker has in mind. These informal observations of the regularity, like those of the referential use of descriptions (Sect. 9.3), are good evidence (Sect. 3.6). Still, the regularity could of course be tested scientifically. If the regularity of this use of '*X* is qualified' is real, which it surely is, then we have good, though not of course conclusive, evidence that this is a conventional way of conveying such thoughts, and hence that '*X* is qualified' literally means that *X* is qualified for *Y* where *Y* is something the speaker has in mind. For, the best explanation of the regularity is that this is a *semantic* fact about '*X* is qualified'. SEs applies. Similar remarks apply to 'ready', 'relevant', 'legal', 'better', 'noticed', 'finished' and many other expressions in English. (Bach has a "syntactic" objection to this claim which I will consider in Sect. 10.4.)

So, I am arguing that '*X* is ready' is indeed semantically incomplete. But this argument does not rest on denying the metaphysical fact that X can be just plain ready nor the semantic fact that '*X* is ready' could represent this metaphysical fact. It rests on an observed regularity in the speaker meaning of '*X* is ready'. Still, is

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[8] So I regret having once agreed with Bach's claims (2013g: 473).

there such a semantic fact? Does '*X* is ready' not only have a conventional meaning that demands saturation by something in particular but also one that *X* is ready for something or other and so just plain ready? Emma Borg certainly believes in that "general" meaning. Perhaps we could find evidence of it and so perhaps she is right. But she thinks that this general meaning is the *only* meaning of '*X* is ready' (2012: 92–102). That is where I think she is wrong: there is a convention of using '*X* is ready' to mean that *X* is ready for something in particular that the speaker has in mind. Furthermore, I predict that almost all uses of '*X* is ready' exemplify this convention. Whether or not '*X* is ready' also has a conventional general meaning is beside the point.

Genitives are among those other expressions to which SEs applies. Consider what Sperber and Wilson have to say about 'Peter's bat is grey':

> 'Peter's bat' might refer to the bat owned by Peter, the bat chosen by Peter, the bat killed by Peter, the bat mentioned by Peter, and so on indefinitely. It is hard to believe that the genitive is ambiguous, with as many senses as there are types of relationship it may be used to denote, or that all these relationships fall under a single definition which is the only meaning expressed by use of the genitive on any given occasion….Contextual information is needed to resolve what should be seen as the semantic incompleteness,… (1995: 188)

I agree. But the incompleteness is of the same sort as that of the referentially used definite description in 'The bat is grey'. As we saw in Chap. 9, the convention for such a use demands saturation in context by the entity that the speaker has in mind, in the case of 'The bat', by the particular bat in mind. Similarly, the convention for a genitive noun phrase demands saturation in context by the relation that the speaker has in mind, in the case of 'Peter's bat" by the particular relation between the bat and Peter that the speaker has in mind; perhaps, the relation of being chosen, of being killed, of being mentioned, or whatever; SEs applies. And, note that just as the convention for a referential definite is for reference to a particular entity not to some entity or other, so too is the convention for genitive noun phrase for reference to a particular relation not, as has sometimes been suggested, to some relation or other.

Sperber and Wilson continue:

> It can be similarly argued that an adverb such as 'too' is semantically incomplete. A bat is too grey *for something*. If you do not know what that something is, you do not fully know what 'too grey' is being used to express….Again similar arguments apply and have often been applied-to scalar adjectives such as 'big' in (22) ['The bat is big']: is the bat big for an adult bat, big for a bat of its age, big for a pet, etc.? And does 'big' without a scale of reference express a complete meaning? (p. 188)

The arguments alluded to strike me as good. Adverbs like 'too' and scalar adjectives like 'big' are indeed "semantically incomplete": they demand saturation in context by something the speaker has in mind. Similarly, 'enough', as illustrated in Bach's example, 'Steel isn't strong enough': "we need to know strong enough for what" (1994: 127). All SEs again. How do we know? Because these expressions are standardly and regularly used to convey messages that are saturated in context.

At first sight, the idea of saturation by an implicit *for something in particular*, might seem good for 'good': "a good book" may be good for reading, for entertaining, for holding the door open, and so on. But there is a problem. Saturation by an

implicit *for something* requires that the speaker have some such criterion in mind and this is often not plausible: think of "a good person" and "a good country". Clearly, more needs to be said.

Next, consider the verb 'begin' when followed by an NP; for example, 'begin the book'. There must be an implicit reference to some sort of action: reading, writing, printing, coloring, and so on. SEs again. And there is no end to the actions involving the book that a speaker *might* have in mind, including many she is *unlikely* to, like baking and jumping. Once again we have an expression that demands saturation in context.[9]

Finally, consider remarks within, or about, fiction, remarks like "Sherlock Holmes lives in Baker Street". As have often been noted,[10] remarks *within* fiction seem to have implicit operators along the lines of "let us pretend", and those *about* fiction implicit operators along the lines of "it is pretended that". So there seems to be conventions that any sentence in a language can be saturated by one or other of these operators. SEs applies.

10.4   The Onus on Pragmatic Saturation

I am arguing that SEs is typically the best explanation of expressions of a form that is regularly used with speaker meanings saturated in context: typically, the expression's property of speaker-meaning *M* on an occasion exemplifies semantic saturation. In contrast, according to PRAG, PEs is typically the best explanation of such expressions: the expression's property of speaker-meaning *M* on an occasion is a pragmatic modification of its semantic meaning and so exemplifies pragmatic saturation. But, as I pointed out in discussing polysemy in Sect. 8.4 and definite descriptions in Sect. 9.4, pragmatic explanations like PEs face a heavy onus: the "psychological-reality requirement". I shall now explore this.

The discussion in Sect. 8.2 showed that the mere existence of a pragmatic derivation of one meaning from another that linguists can give, even one that speakers and hearers can give, is far from sufficient to show that a speaker meaning should be explained pragmatically. Rather, for a pragmatic explanation to be adequate, it must place that pragmatic derivation actively within the cognitive lives of speakers and hearers: that is the psychological-reality requirement on PEs (Sect. 8.4). If *E*, having a semantic meaning *M1*, is saturated in context to mean *M2* and this is to be handled pragmatically, then there have to be psychological processes in speakers and hearers that are appropriately different from the standard ones, whatever they may be, involved in the use of terms governed by conventions that demand saturation in context. In hearers there have to be processes that differ from selecting *E* with a

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[9] In this way SEM has the "interpretative flexibility" in handling the likes of 'begin' that Falkum (2015: 93) thinks "rule-based" accounts lack.

[10] My own noting is in 1981a: 167–88.

saturation-demanding meaning *M1* from the lexicon and arriving at *M2* as part of the message by following clues to the saturation on this occasion, thus understanding the message as what-is-said, a WIS-meaning. There have to be *mind-reading processes of detecting a pragmatic modification involving a saturation in order to infer from the utterance that E, in the circumstance, means M2 not its non-saturation-demanding meaning M1, and hence to infer the message, a PM-meaning, from what-is-said, from a WIS-meaning*. In speakers there have to be processes that differ from expressing a concept (part of a thought) with the content *M2* by selecting *E*, with a saturation-demanding meaning *M1*, from the lexicon to yield a WIS-meaning. There have to be *partly mind-reading processes of selecting E to express that M2 concept, even though E means M1 in the lexicon, partly because the speaker expects that the hearer will go through the above mind-reading process to arrive at the interpretation M2*.

Paradigm particularized conversational implicatures provide persuasive examples of pragmatic explanations of saturation that *do* meet this psychological-requirement. Thus consider the earlier-discussed example of the despondent Jones saying "everyone taking my seminar turned up" meaning to convey to Neale the message that only Smith attended Jones' party (Sect. 8.4). It is plausible to suppose that Jones goes through the mental process required to modify the semantic meaning of his utterance: in particular, that Jones thinks to himself that Neale, will go through a Gricean derivation to grasp what is implicated. And, it is plausible to suppose that Neale actually does go through that derivation in grasping that implicature. All this is plausible because Jones and Neale are likely to be conscious of these mental processes and could tell us about them. And, it is important to note, the processes that they tell us about are different from those they would have gone through had Jones conveyed his message about Smith by using an expression covered by a saturation-demanding convention; for example, by saying "He turned up", with Smith in mind.

The problem for PEs is that we are not conscious of the different processes that PEs requires in the cases that concern us, the cases we have been discussing in Sect. 10.3. Indeed, I suggest, the processes of saturation and interpretation that we are aware of in these cases do not seem to involve any mind-reading processes that *differ in any relevant respect* from those in processing the paradigms of saturation-demanding expressions. So PEs must posit different subconscious processes in the cases discussed. And then the problem is finding any evidence of such processes.

Discussing polysemy in Sect. 8.4, we identified the sort of processing evidence that would show that PEp meets the onus on a pragmatic explanation of polysemy. And we then aired two objections to PRAG's idea that polysemies are typically pragmatic, objections that make the need for this processing evidence pressing. We have now identified the sort of processing evidence that would show that PEs meets the onus on a pragmatic explanation of saturation. The PRAG idea that saturations are typically pragmatic faces the following two similar objections that make the need for *its* processing evidence also pressing.

Occamist Objection   We already know that there must be the largely subconscious convention-exploiting processes of saturation in speakers and hearers, even if we do

not know much about the details (despite the ingenious efforts of psycholinguists). For, those are the standard processes for handling referential demonstratives and pronouns. So we already know that there must be the sort of processes required by SEs, the semantic rival to PEs. This is an important part of our background knowledge in assessing which of PEs and SEs is the better explanation of a regular saturation. SEs is committed to mechanisms we already know to exist, even though we are short on the details. *If we really do lack independent evidence of the subconscious processes that PEs requires, then we should prefer SEs over PEs on Occamist grounds.*

Developmental Objection   There is good reason to suppose, a priori, that the different psychological processes required by PEs do *not* exist. Consider a word *E* with a conventional meaning *M1* that does not demand saturation. Then a speaker, for the first time, saturates *E* in a certain way in a successful communication: she uses it with an implicit reference yielding *M2*, a modification of *M1*, as part of her message. The success of this communication depended on the speaker and hearer consciously going through the mind-reading processes we have described. Suppose that this success led other speakers to saturate *E* with an implicit reference yielding other modifications of *M1* as parts of their messages, thus establishing a pattern of communicative success in so doing. *Why would this pattern not have led to the end of those mind-reading processes and the establishment of a new conventional meaning of E, a meaning based on M1 but demanding saturation in context*? As noted in discussing polysemy (Sect. 8.4), a convention eliminates the need for the demanding, probably mostly conscious, mind-reading processes that brought it about. It is hard to exaggerate the extent to which communication has benefited from the conventions that gave us a language. Why would we have denied ourselves that benefit with saturated words? As I also noted (Sect. 8.4), conventions.

> enable us to co-ordinate our actions to our mutual benefit, whether in communication, meeting for drinks, rowing, and so on, *without the complicated mind-reading that brought them about*. That, we might say, is the point of conventions.

PRAG rejects the view that speakers' regular saturation of the expressions in question is to be explained by speakers having lexical entries for those expressions that demand saturation in context. Rather, it is as if each saturation of an expression in context is a novel modulation of its conventional meaning (or semantic potential, or…). PRAG badly needs processing evidence of this. So far as I know, *there is no evidence of these processes, nor even any attempt to find evidence: none of the psycholinguistic experiments discussed in Sect. 11.6 seeks evidence on the processing of saturations*. Given what we know about the development of linguistic conventions, it seems unlikely that the needed evidence will be found. PRAG is implausible. Abduction favors SEM.

But Bach has an objection.

10.5   The Tyranny of Syntax[11]

We have already discussed Bach's use of Modified Occam's Razor (Sect. 8.5) and "standardization" (Sect. 8.6) to exclude linguistic phenomena from semantics. He has an interesting further methodological device in the service of meaning denialism: a certain view of the relation between syntax and semantics. This device yields an objection to the "implicit saturation" approach I have just illustrated.

Consider an utterance of "Everyone went to Paris". On my view, it is a semantic fact that this utterance depends for its truth on whatever group the speaker has in mind. This is not Bach's view. For him, the restriction on the domain is not a semantic matter: the literal truth of the utterance 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 an utterance of "I'm not ready". Given our earlier discussion of Bach (Sect. 10.3), it is no surprise that he thinks that this sentence "does not express a complete proposition" because it has "a missing argument". It "cannot be used to mean that one is not ready simpliciter" (1998: 716). My response was that there is a *semantic convention* for 'ready' that demands completion by some argument that the speaker has in mind; there is a slot to be filled. So the demand for completion goes into semantics and we have an instance of SEs. 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 the utterances of sentences like "I'm ready", which he thinks do not express complete propositions, "semantically underdeterminate". My slot-filling response to them 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 idea. 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; emphasis added; 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", even if only "there" as an empty category or "aphonic". We saw examples of aphonics in Sec. 6.5.1 with 'Mary went to visit the zoo and John, the

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[11] This discussion draws heavily on Devitt 2013f (186–9).

museum' and 'Bob tried to swim': the uninscribed 'went to visit' is "there" after 'John' in the former sentence, two uninscribed instances of PRO, "there" in the latter sentence. Bach is airing 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. As Fodor and Lepore put it: "If a sentence S expresses the proposition that P, then syntactic constituents of S express the constituents of P" (1991: 333). Carston does not embrace the principle herself but says, I think rightly, that it "has been fairly widely held by philosophers" (2002: 22).[12]

The Isomorphic 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 (1979) 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 in that language. 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 an implicit 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 the 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 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 that the street

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[12] She cites Frege 1977 and Fodor 2001, as examples. See also the recent debate between Rob Stainton (2005) and Jason Stanley (2000) about sub-sententials, discussed in Chap. 12. 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" (p. 13).

sign has a conventional meaning that demands completion by reference to a location and that the phone book entry has a conventional meaning about telephone numbers.

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 may mostly ascribe a matchingly complex syntax, *it often does not*. Conventional meaning is not under the tyranny of syntax in the way Bach claims[13]: that there is no slot to be filled in the syntax of a sentence does not show that there *could not be* one in the semantics. Correspondingly, there being a slot to be filled in the semantics does not show that there *must be* one in the syntax: there is no tyranny of semantics either.[14] Rather, it is always an open question to what extent, if any, a meaning is to be explained in terms of a matching 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). And I am not for a minute denying the interest of these syntactic issues. But they are different issues. My present point is that *there is no simple inference from a symbol having a complex meaning to its having a matchingly complex syntactic structure*.

10.6 Perry's "Unarticulated Constituents"

My position on the examples discussed in Sect. 10.3 is like John Perry's on weather reports (1986; Crimmins and Perry 1989), as defended vigorously by Stephen Neale (2007) against Jason Stanley (2000). According to Perry, the proposition asserted by "It is raining" contains an unarticulated constituent ("UC"), a location of the raining (actually two UCs, also a time). Neale comments: "No interesting thesis about *the syntax of natural language* is implied by the mere postulation of unarticulated constituents" (2007: 267) "Perry appears to presuppose no particular syntactic theory or advance any particular syntactic thesis" (p. 317). In contrast, Stanley's criticism of Perry rests on a strong syntactic thesis:

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[13] In his "Reply to Michael Devitt", Bach responds, "I'm not sure what Devitt means by the 'tyranny of syntax'" (2013: 242). This is odd for the meaning of this (somewhat playful) expression is surely clear from the passage that has been repeated here. The rest of Bach's "Reply" to my discussion mostly changes the subject rather than responds to my argument. See "Unresponsive Bach" (Devitt 2013g: 471–3) on this.

[14] My thanks to Francesco Pupa for emphasizing this to me. He drew my attention to the fact that meaning differences resulting from differences in "focus" may not be captured in the syntax of a language. Thus consider the differences between the following three utterances in English:

JOHN kissed Mary
John kissed MARY
John KISSED Mary

See Lepore and Stone (2015: Chap. 8) on such matters.

> If the truth-conditions of constructions containing [overt expressions that are not pure indexicals, demonstratives, or pronoun ((s) are affected by extra-linguistic context, this context dependence must be traced to the presence of an obvious indexical, demonstrative, or pronominal expression at logical form, or to a structural position in logical form that is occupied by a covert variable. (2000: 400)

This thesis is a version of the just-discussed Isomorphic Principle and is the basis for Stanley's conclusion that there are no UCs and so Perry is wrong: a weather report contains "an unpronounced pronominal element in the logical form of the sentence uttered", an aphonic (2000: 410).[15]

Neale sees no reason to accept Stanley's syntactic thesis:

> We can leave it as an empirical question whether work in syntax will reveal an argument position in the syntax of (1) [It's raining] occupied by an expression corresponding to Reykjavík, qua occupant of an argument rôle. What we cannot do is simply *conclude* that there must be such a position in order to render intelligible the idea that Reykjavík occupies the argument rôle it does in the proposition I express by uttering (1). (2007: 322)

> not all potential differences in the propositions expressed by (or of the truth conditions of) distinct uses of a sentence *X* are attributable to the presence in *X*'s LF of phonic or aphonic indexicals to which different values are assigned on these different uses of *X*. (p. 332)

I think this is dead right.

Adam Sennet (2011) is exercised by Perry's view and provides another example of what I'm arguing against. He begins with the claim that

> there is significant slippage between the meaning a plausible, conservative semantic theory determines for a sentence in context and what speakers intend (and manage) to get across by uttering it. (412)

There is only "slippage" *given the conservative theory*. And that theory is conservative *in its syntax*, hence in what it allows syntax to contribute to meaning. Sennet later adopts the doctrine, "No-Structure: Hidden syntactic structure is to be posited in a sentence only if one is forced to do so by syntactic evidence." (p. 416) Why shouldn't we posit a structure because doing so helps explain what a sentence *means*? Unarticulated constituents alone provide a reason for positing hidden syntax though, as Neale emphasizes, not a sufficient reason. Sennet is in the grip of the tyranny of syntax.

Sennett describes the various definitions of UC as "woefully inadequate" (p. 414):

> In particular, UC definitions are inadequate when we consider cases in which the same constituent appears more than once in a proposition that only has one word with the constituen as its semantic value. (p. 412)

Consider, for example,

> (4) Brooke comes to Vancouver when it snows,

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where the speaker has in mind that it is snowing in Vancouver. Suppose that our definition of UC is: "UC is a constituent of a proposition expressed by a sentence that isn't the semantic value of any phrase in the sentence" (p. 414). This definition is "woefully inadequate" because "in (4), the constituent Vancouver is articulated: it is the semantic value of the word 'Vancouver', which is indeed part of the sentence" (p. 420).

In response, drawing particularly on Crimmins definition (1992: 16), I would propose, on Perry's behalf, the following definition: "for a proposition expressed by a sentence to contain an unarticulated occurrence of a constituent is for that occurrence in the proposition not to be in virtue of the constituent's explicit mention in the sentence". But taking talk of propositions and their structure seriously, as if propositions really were entities containing other entities, leads to trouble in my view. We can capture Perry's UC-idea without this: "for part of what is expressed by a sentence to be unarticulated is for that part not to be explicitly mentioned in the sentence". The move from constituents of propositions to parts of what-is-said arguably removes the need to fuss about occurrences in a structure.

And, if Neale is right, Sennett has Perry wrong in taking it as *definitive* of UCs that the constituent is *not* supplied by the syntax. Perry is neutral on matters syntactic, as am I.

Neale claims to "know of nothing in syntactic theory that undermines" Perry (2007: 320). The question I raised in Sect. 10.5 is: How *could* a syntactic theory undermine the view that there is a convention according to which a weather report has an implicit reference to a location? This may be outside the jurisdiction of syntax.

10.7   Conclusion

The challenge posed by Linguistic Pragmatists stems from the many examples of context relativity that their investigations have revealed. In arguing for SEM and against PRAG, I have divided these examples into two groups, one concerned with saturations, discussed in this chapter, the other concerned with polysemy, discussed in the next.

After a brief rejection of "meaning eliminativism" (Sect. 10.2), I argued that a range of phenomena, including weather reports, quantifications, 'ready', 'qualified', genitives, and 'enough' should be explained by taking their linguistic meanings to demand saturations in context; the conventionally established meaning has an implicit slot to be filled (SEs). This is the best explanation of the regular saturation of these expression to convey messages (Sect. 10.3).

The rival explanation is that these saturations arise from pragmatic modifications of one sort or another (PEs). But these PRAG explanations, like those before (Chaps. 8 and 9), face the heavy onus of the psychological-reality requirement: they must show that the pragmatic processes have an appropriately *active* place in cognitive lives. Once again, this requirement has not been met, nor has there been any psycholinguistic attempt to meet it. According to the Developmental Objection it is unlikely

to be met. According to the Occamist Objection, we should then prefer SEM (Sect. 10.4).

I ended by considering an objection to SEM's positing of implicit slots to be filled. Bach objects to positing any slot that is not "there" in the favored syntax, even if only as an aphonic. 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. I rejected this "tyranny of syntax", arguing that no such principle *could* show that a slot-filling convention for, say, quantifiers or 'ready' is *impossible*. It is always an open question to what extent, if any, a meaning is to be explained in terms of a matching syntactic structure (Sect. 10.5). I reinforced this point by siding with Perry against two syntax-driven critics of his discussion of "unarticulated constituents", Stanley and Sennet. As Neale argues in criticizing Stanley, Perry's position implies no interesting syntactic thesis. Neither does mine (Sect. 10.6).

This concludes my attempt to show how a range of examples of context relativity can be accommodated within the tradition by supposing that there are conventions of unarticulated constituents and saturation. I turn now to consider some other examples that are context-relative in another way. They are examples of polysemy.

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Chapter 11
Polysemy and Pragmatism's Challenge

11.1 Polysemy

Many of the examples of context relativity that Pragmatists have brought to our attention are polysemous phenomena. We have such phenomena whenever an expression *E* is used with more than one *related* sense (Sect. 8.3). These phenomena motivate PRAG:

> **PRAG**: Even setting aside novel uses of language, the truth conditional speaker-meaning (content) of an utterance, the message conveyed, is seldom, perhaps never, constituted solely by what-is-said (in my sense), a WIS-meaning. The message is always, or almost always, the result of pragmatic modification, a PM-meaning.

I urge a rival doctrine:

> **SEM**: Setting aside novel uses of language, what-is-said (in my sense) is typically the truth conditional speaker-meaning (content) of an utterance, the message conveyed. That message is typically a WIS-meaning (Sect. 3.5).

In defense of this rival, I claim that where the use of *E* with any of those senses is regular, the best explanation of this regularity is typically that this sense is a conventional linguistic meaning of *E*. And if this is the right explanation for more than one of the regular uses of *E*, we have semantic polysemy: *E* is *ambiguous*. According to SEM, this *is* typically the right explanation of the Pragmatists' polysemous phenomena. Whenever it is, the phenomenon can be accommodated by the tradition.

Disambiguation, like the saturation discussed in Chap. 10, is a way that the tradition takes account of context relativity. Favorite examples of ambiguous expressions, ones governed by more than one set of conventions, include 'bank' and 'visiting relatives can be boring'. According to the tradition (at its best), such expressions are disambiguated in context by whatever meaning the speaker has in mind. The conventions governing an expression constrain truth conditions by determining *a set of possible* truth conditions, one of which is made actual by the speaker in context. With variation in context an ambiguous expression yields more than one

truth condition but not, we should note, the indefinite number required by radical versions of PRAG (Sect. 10.1). This account of ambiguity *in general* must of course apply to polysemous ambiguity *in particular*. So context relativity in such cases would be just the familiar traditional one of disambiguation in context.

I emphasize that the polysemy that concerns us here arises from expressions that are regularly used with more than one related meanings. There can also be novel polysemies, probably arising from spur-of-the-moment modulation that are not our concern (Sects. 3.5, 10.1). What makes it plausible to think that these novel cases are examples of pragmatic rather than semantic polysemy is that their pairings of expression and message are not regular. But where the pairing is regular, my claim in support of SEM is that the best explanation is typically semantic polysemy. And typically the polysemous phenomena that Pragmatists draw attention to are of regular pairings of expression and message. How do we know that these pairings are regular? Well, as noted in Sect. 3.6, we can begin by looking in dictionaries. These, along with our own observations, provide good evidence of regularities in usage, even if often informally gathered.

So my argument that the Pragmatists' polysemous phenomena can typically be treated as ambiguities, and hence accommodated by the tradition, rests first on these claims of regularities. The argument rests next on claims that these regularities of usage are best explained by positing conventions. Since the regularities are usually obvious, the onus on the Pragmatist is to provide a better explanation of them. This, I shall argue (Sect. 11.6), Pragmatists fall far short of doing.

I earlier (Sect. 8.4) distinguished the rival explanations that SEM and PRAG claim to be typical as follows. Suppose *E* has an encoded semantic meaning *M1* but is polysemous. Speakers regularly use *E* to mean *M2*, perhaps also to mean *M3*, and so on, where these meanings are all different but conceptually related. The rival explanations of a speaker's use of *E* to mean *M* are:

> **SEp**: *E*'s property of speaker-meaning *M2* on this occasion is semantic (in my sense). So *M2* partly constitutes what-is-said, my "WIS"-meaning (Sect. 3.5). On other occasions, the speaker meaning of *E* may be a different semantic meaning, *M3*.

> **PEp**: *E*'s property of speaker-meaning *M2* on this occasion is a pragmatic modification of *E*'s semantic meaning *M1* and so is at least partly pragmatic (in my senses). So *M2* partly constitutes a pragmatic modification of what-is-said, my "PM" -meaning (Sect. 3.5). On other occasions, the speaker meaning of *E* may be a different modification, *M3*.

So, according to SEM's typical explanation, the polysemous *E* is thereby ambiguous; according to PRAG's, not. According to SEM, the polysemy is semantic, according to PRAG, pragmatic.

SEM claims that SEp is typically the best explanation of the regularity. The next section will give many examples. The rest of the chapter focuses on objections to SEM and on what we can learn about polysemy from psycholinguistics. In Sect. 11.3, I do some preliminary work to help bring the psycholinguistic discussion to bear on the SEM-PRAG issue. In Sect. 11.4, I shall defend SEM from objections, including some from linguistics/psycholinguistics. In Sect. 11.5, I shall emphasize the evidence of mental processes that we *need* to cast light on the SEM-PRAG issue.

In Sect. 11.6, I shall consider the bearing of the psycholinguistic evidence we *have* on that issue.

I discuss many examples of polysemy but there are of course many that I do not discuss. I hope that I treat enough examples to make it plausible that the Pragmatist challenge posed by polysemy can indeed be met along the lines proposed in Chap. 3.

11.2   Semantic Polysemy

11.2.1   *Metaphor-Based*

It is common to think of polysemy as either metaphor-based or metonymy-based. But our case study of definite descriptions in Chap. 9 arguably provides an example of semantic polysemy that is not clearly of either sort: there is not only the uncontroversial convention of using definite descriptions attributively but also a somehow-related convention of using them referentially, which is in fact participated in much more frequently. Then, whichever of these senses a particular speaker has in mind in a context gets into the convention-governed what-is-said. So it is a semantic property not a pragmatic one; SEp applies not PEp. And the ambiguity here is a matter of polysemy (Sect. 9.8).

Another example of SEp that was central to the argument against the Razor clearly concerns a metaphor-based polysemy: the dead metaphor 'to incense' meaning *to make very angry* coexists with its source meaning *to make fragrant with incense* (Sect. 8.2). There are many such examples of metaphor-based ambiguity in the language. We have mentioned some already: 'dead metaphor' itself (Sect. 8.2); 'lion' in our original Pragmatist example (6), already discussed (Sects. 1.1, 3.6, 8.1) and to be discussed again (Sect. 11.5); Amaral's example of 'foot' used to respond to "Kripke's Test" (Sect. 9.8), and mentioned again later (Sects. 11.3.1, 11.5). Consider the adjective, 'warm'. In one of its various related meanings it can truly describe a climate; in another, an overcoat, a support, a debate, a greeting, a sunset, a trail, and so on. There is a similar story for 'cold', 'hard', 'soft', etc. In these cases, as in countless others, a metaphorical use has become conventionalized into a new linguistic meaning. How do we know? That's the best explanation of a regular use.

11.2.2   *Metonymy-Based*[1]

Metonymy is a prolific source of new linguistic meanings. Consider 'suit', item (7) in our group of Pragmatist examples (Sects. 1.1, 3.6): 'suit' means a certain item of clothing but has come to have another meaning referring to business executives who

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[1] Thanks to a talk, "Polysemy and Metonymy", by Deirdre Wilson and Ingrid Falkum for guidance on metonymy.

wear suits. We shall return to this example (Sects. 11.3.1, 11.5, 11.6.1). Other examples include 'the crown' which has a meaning that refers to what monarchs wear but also one that refers to the monarch herself. 'Counting heads' can mean counting people; 'glass' can mean a material or drinking vessels that are usually made of that material. 'Philosopher' means a member of a certain profession but also a person who is deemed "philosophical".[2] Denominal verbs, verbs created from nouns, provide many examples: think of 'to wait-list', 'to blot', 'to footnote' 'to xerox' 'to google'. Compound nouns provide many more, as the following passage demonstrates:

> The compound noun structure is extremely varied in the types of meaning relations it can indicate. It can be used to indicate what someone does (*language teacher*), what something is for (*waste-paper basket, grindstone*), what the qualities of something are (*whiteboard*), how something works (*immersion heater*), when something happens (*night frost*), where something is (*doormat*), what something is made of (*woodpile*), and so on. (Carter and McCarthy 2006: 320)

Of course, the use of compound nouns can be novel, requiring a pragmatic modification; see the earlier example of 'burglar nightmare' (Sects. 3.5, 10.1).[3] But with all the above cases, we have regular uses of an expression that are best explained as distinct conventional meanings, SEp. What presumably started as pragmatic has become semanic.

Consider also the count noun 'dog'. It is often used to mean any member of the species *Canis familiaris*. But it also has a use where it means *male* dogs but not female ones (bitches). Similarly, 'drink' to mean any drink and 'drink' to mean alcohol in particular. The two uses of each word exemplify two conventions and hence semantic polysemy (SEp). How do we know? Well, we have good evidence that speakers do mean members of the wider class on some occasions and of the narrower class on others. And we have good evidence that these uses are regular. We shall see (Sect. 11.6) that the rival PEp badly lacks the evidence it needs. Semantic polysemy is the best explanation.

11.2.3   Regular Polysemy

"Regular", or "systematic", polysemy is a striking example of metonymy, yielding many instances of semantic polysemy (SEp). In "Polysemy: An Overview", Yael Ravin and Claudia Leacock describe this phenomenon as follows:

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[2] Here I side with Szabó (2006: 34) against Cappelen and Lepore (2006: 61).

[3] Romero and Soria think I should not treat this as a pragmatic modification but rather as like a weather report: "what is regularly delivered by the semantics of this N + N construction is the meaning of 'nightmare [in some relation with] burglar [to be determined]'" (2019: 414). This amounts to taking such a construction to be covered by a convention like that I proposed for a genitive: it "demands saturation in context by the relation that the speaker has in mind" (Sect. 10.3). Maybe this is the right approach to compound nouns, but I'm dubious.

> Regular polysemy is governed by processes which are productive, rule-governed, and pre-
> dictable, very much like processes of word formation. One such process…is the systematic
> relation between words denoting vessels and the quantity that the vessel holds, such as
> spoon, the utensil and spoon meaning spoonful, as in a spoon of sugar. (2000: 10).

This "systematic relation" is exemplified by any word for a vessel and so is exempli-
fied often. Indeed, suppose that we manufactured a new vessel and named it, a
"grugru", that word *automatically* becomes a word in our language for the quantity
of stuff that this vessel holds.

There are countless examples of regular polysemy in our language. We have
come across one sort with the popular case of 'the ham sandwich' (Sect. 8.1), and
will return to it (Sect. 11.5). It illustrates that in restaurants, at least, there is a rule
that the word for what is ordered has a meaning referring to the orderer.[4] Here's a
list of some widely accepted examples of regular polysemy:

A count noun for an organism (at least), like 'rabbit', 'cabbage', or 'oak' yields a
   mass noun for the stuff of which it is made: "The road was covered with rabbit"
   (item (8) from our original Pragmatist examples in Sect. 1.1); "Let's have cab-
   bage for dinner"; "That table is made of oak". (This rule is often called "Universal
   Grinder".)[5]
A count noun for an organism (at least) yields a mass noun for its skin/fur ("fur
   grinder"): "The model wore rabbit".[6]
A name for a person yields a name for his/her/work; 'Jane Austen'; 'Picasso'.
A noun for a process/action yields a word for its product; 'interpretation'.
A word for the producer of some item yields a word for the item produced (or vice
   versa); 'Honda', 'newspaper'.[7]
A word for an event yields a word for an object that features in the event (or vice
   versa); 'lecture'; 'lunch'.
Any word yields a word for itself, particularly when spoken; 'rabbit' refers to 'rab-
   bit', 'Picasso' to 'Picasso'.
A word for a kind of object yields a word for a representation of that kind of object;
   'lion' refers to lion statues, pictures, toys.

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[4] Romero and Soria (2019: 411–12) wrongly take me to have a pragmatic view of such examples;
also, of 'lion' referring to a statue (below) and of 'cut' (Sect. 11.2.5; though I did earlier find "some
plausibility" in a pragmatic account of 'cut' (2013b: 96)).

[5] Universal Grinder is famously "blocked" in a few cases where there is a special mass term for a
popular food stuff: e.g., 'beef', 'pork'. See Nunberg and Zaenen (1992) for a subtle discussion of
grinding. Perhaps grinding extends to some other count nouns.

[6] It is common in lexical semantics to think that a count noun for an animal, for example 'rabbit'.
yields a mass noun that is essentially *meat* ("*meat grinder*"); see, e.g., Copestake and Briscoe
(1995). This strikes me as a mistake. Any count noun for an animal, or indeed for a plant ('cab-
bage'), yields a mass noun referring to stuff that is a *potential* food, but whether or not any organ-
ism actually eats the stuff is a fact about the world not something required by the noun. My view
here exemplifies a resistance to turning worldly facts into lexical information, a resistance that is a
theme in this chapter.

[7] John Oliver on the Daily Show 8/14/2013, reflecting on a recent series of newspaper acquisitions
by billionaires, remarked: "There are more people buying newspapers than are buying newspapers".

A word for a gradable perceptually salient color property yields a word for a non-
    gradable "classificatory" property (or vice versa); a painted green leaf vs a natu-
    rally green one; a red apple (surface) vs red grapefruit (inside).[8]
A word for a token yields a word for a type (or vice versa); 'word'; 'newspaper',
    'flower' "How many flowers are in your garden?" is probably about types but
    could be about tokens.
A word for an aperture yields a word for what goes in the aperture; 'window', 'door'.

The literature classifies many other linguistic phenomena as regular/systematic
polysemies. In the next subsection, I shall argue that these phenomena include some
that should *not* be so classified.

But first, what to say about regular/systematic polysemy? Ingrid Falkum and
Agustin Vicente sum up the received opinion:

> In formal semantic and computational approaches, regular polysemy of this kind is typi-
> cally analysed as being generated by lexical rules, in this way accounting for the productiv-
> ity and cross-linguistic availability of the patterns of sense extension, while avoiding a
> listing of all senses for the words in question (2015: 2)

Devorah Klein and Gregory Murphy nicely demonstrate the generative aspect of
regular polysemy:

> If we were to hear of a new species of plant called a *delgar*, we could say both "There is a
> delgar growing in my yard" (individual plant) and "This pen is made out of delgar" (the
> substance derived from that plant). Thus, these forms of polysemy are highly productive,
> and they are used quite easily when new words enter the lexicon (Klein and Murphy
> 2001: 260)

What we see exemplified here is a *meta*-convention, a process for generating lexical
conventions, of the following form: whenever a conventional meaning is established
according to which an expression refers to things of type *X*, that expression will
thereby also have another conventional meaning according to which it refers to
things of related type *Y*. Thus, when we introduce 'delgar' for a plant, we *thereby*
introduce that word into our language as a mass noun for certain stuff that we may
use to make pens or perhaps even eat ("Let's have delgar for dinner"). Perhaps this
calls for a modification of my dispositional account of the lexicon (Sect. 5.1). For,
consider someone who uses 'delgar' as a count noun for the plant but has never
thought of using it as a mass noun for the stuff. She certainly has the count-noun
disposition, but does she also have the mass-noun disposition? Perhaps we can say
that she does, simply on the strength of her participation in the meta-convention. So
no modification is called for. But perhaps, on the strength of that participation we
should only say that she has a *disposition to form* the mass-noun disposition. That is
enough for 'delgar' to have the mass-noun meaning in her idiolect and hence be in
her lexicon with that meaning. So a minor modification of the dispositional account
would be needed. Either way, SEp applies.

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[8] I'm guided by Kennedy and McNally (2010) here.

Not surprisingly, the rules for regular polysemy are much the same across languages (Srinivasan and Rabagliati 2015); but there are some interesting differences (Nunberg 2004).

How does a meta-convention get established? Typically, the same sort of way that a convention does. Just as a convention typically comes from the regular use in a community of an expression to convey certain parts of messages (Sect. 5.2), a meta-convention typically comes from the regular creation of conventions of using an expression to refer to things of type *Y* on the basis of conventions of using it to refer to things of type *X*. Thus, conventions of referring to organisms, like those for 'rabbit', 'cabbage', and 'oak' led to conventions of referring to stuffs and this led to our meta-convention that gave us 'delgar' referring to a stuff.

Finally, just as there can be indeterminacy about whether there is a convention because of a sorites problem (Sect. 5.2), so too can there be about whether there is for a meta-convention. Perhaps the denominal verbs 'xerox' and 'google' exemplify a meta-convention, perhaps not.

11.2.4 Mistaken Polysemy Classifications

Among the many phenomena that the literature classifies as regular polysemies are some that, on close inspection, should not be so classified. Thus, consider:

> Nouns like *city* alternate their meaning between an administrative entity or unit, the group of people living within the unit's borders and the people who govern it. Similarly, nouns like *book* alternate between the physical object and its content. (Ravin and Leacock 2000: 22).

Let us start with 'book'. This is indeed a very popular example of alleged polysemy.[9] 'Book' certainly exemplifies a type/token polysemy, one of the regular polysemies just listed. Thus, in "I'm going to burn all your books", 'books' likely refers to tokens; in "I've read all the books in your library", it likely refers to types.[10] But 'book' is standardly thought to be polysemous in another way: it is thought to have a "physical" sense in "This book is heavy" and an "informational" sense in "This book is real fun". I think that this is quite mistaken.[11] Why? Because it yields a mistaken metaphysics. Indeed, discussions of the meaning of 'book' seem to reflect an

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[9] See Pustejovsky (1995), for example. The title of an influential paper (to be discussed later (Sect. 11.6.4) is supposed to illustrate the two meanings: "About bound and scary books: The processing of *book* polysemies" (Frisson 2015).

[10] And it likely refers to types distinguished by *author-and-title* – Tolstoys' "War and Peace", Heller's "Catch 22", etc. But book tokens, like word tokens (Sect. 1.1), can be typed in other ways; for example, as novels, or biographies. So, in some contexts, 'book' may refer to types distinguished in some way other than by titles.

[11] John Collins does not (2017: 681–2). The discussion of 'book' that follows owes a lot to his generous attempt to persuade me to his way of thinking.

infirm grip on the relevant metaphysics or, worse, a metaphysics derived from a mistaken semantics.

Consider Nicholas Asher's (2011) detailed discussion. He calls nouns like 'book' "dual aspect" nouns and claims that in using one "we can predicate properties of two different aspects of the same thing". He argues that "this kind of predication", *copredication*, "requires a special metaphysical conception of the objects whose aspects are the bearers of the properties predicated". These aspects yield "a shifting of emphasis or a reconceptualization of the very same object" (pp. 23–4).

> The intuition is that objects like books or lunches have two distinct aspects. Books, for instance, appear to have both a "physical" aspect and an "informational" aspect; which is selected in a predication depends on the type restrictions imposed by the predicate on its arguments. Books have a "dual" nature—two conceptualizations, if you will, that are equally "true of" or "faithful to" the object. (pp. 130–1)

Many are led to "postulate two senses of book: the basic physical book and 'informational books'…*book* is ambiguous between i-book and p-book" (p. 134).

"Books" and "lunches" should not be talked about in the same breath when doing metaphysics. 'Lunch' was in my list of regular polysemies (Sect. 11.2.3): it can refer to *objects* eaten or *events* of eating. These are two very different categories of entities. They are not "aspects" or "conceptualizations" of the one entity. (We shall return to "lunches" when discussing copredication in Sect. 11.4.6.) "Books" are quite unlike "lunches" in this respect.

Set aside the semantics of 'book' for a moment and just consider the metaphysics of books. A book is a certain sort of physical object with a certain sort of function, a function that requires it to have a certain sort of semantic content. A physical object that lacks that sort of content – for example, a wad of paper – is not a book.[12] And something with that sort of content but which is not of an appropriate physical form – for example, an oral legend or a mental state – is not a book. (But, note, the appropriate forms include those of e-books and books-on-tape.) For an object to be a book *is* for it to have *both* that form *and* that content; that's the nature/essence of the kind *book*. So, whatever i-books and p-books may be, *they are not books*. In virtue of its somewhat complex nature, a book, *the one object*, can be both heavy and real fun. There is nothing puzzling about this: it can be heavy in virtue of its physical form, one of the properties *of that very book*, and fun in virtue of its content, another of the properties of *that very book*. One can call these form and content properties "aspects" of the book if one likes, perhaps even "conceptualizations", but one mustn't lose sight of the fact that *all we have here is the one object with various properties*, some of which it has in virtue of its having others. These are the key metaphysical facts. And what one must *not* do is conceptualize a property of the book as a special kind of book.

What goes for books goes for many similar objects like CDs and musical scores. Furthermore, something similar goes for other so-called "artifacts". Thus, coins are

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[12] What about a notebook? But is a notebook a book? I rather doubt it but, if so, we will have to modify the account: a book is a physical object made to have a certain sort of content.

physical objects with a certain sort of function; that's their nature/essence. So, a coin, *the one object*, can be both scratched in virtue of its form and be a quarter in virtue of its function. And a smart phone, *the one object*, can be both black and intuitive; an inscription, *the one object*, can be both one-inch long and refer to Aristotle. Indeed, even a *person* can be both heavy and fun; e.g. W. C. Fields. The nature of a kind can allow one of its members to have properties of many different sorts.

What is the basis for these claims about the nature of kinds? *Nothing semantic!* Indeed, it is a grievous error to derive a metaphysics from a semantics; or so I have argued (1997a, 2010a). For, we know *much* more about the way the world is than we do about how language relates to the world. So my metaphysical claims are not based on any view of the meanings of words. And though I think these claims are intuitive, they are not based on intuitions; c.f. Chap. 2. So what are they based on? I have argued elsewhere that the key to knowing about the nature of a kind, whether a biological kind like *tiger* (2008d, 2018c) or an "artifactual" kind like *book* (2020b: 444–7) lies in the fact that these natures are *explanatory*: in brief, the nature of kind *F* explains the properties that *F*s have simply because they are *F*s:

> Why are paperweights useful weapons? Because the essential function of a paperweight requires it to have intrinsic properties (which we could spell out) that make it a good weapon. Why is it easier to erase writing from a pencil than from a pen? Because of the essential intrinsic difference between pencils and pens (a difference we could spell out). (2020b: 445)

So we can identify the nature of a kind by looking for what plays the explanatory role. Books, coins, smart phones, and inscriptions have their places in the causal nexus of the world in virtue of having the sorts of part-physical-part-functional natures I have briefly indicated.

That's quite a lot about the metaphysics of books but not a word yet about the semantics of 'book'. Set aside the obvious type/token polysemy and consider only cases where 'book' refers to tokens. For example, on one occasion I pick a token physical object off my desk and say, "This book is heavy". On another occasion I pick that very same token object off my desk and say, "This book is real fun". On both occasions 'This book' (semantically) refers to that one token physical object and the two predications are thus of *one and the same object*, the one picked off my desk. I did not "predicate properties of two different aspects" of that object, for I didn't predicate properties of an aspect of the book at all; I predicated properties *of the book, that very object picked off my desk. So these predications do not demonstrate any polysemy at all*. One can say, if one likes, that the object has the property of being heavy in virtue of having a certain "aspect", and the property of being real fun in virtue of having another "aspect". But then these aspects are not different *objects* of my predications – in particular, they are not p-books or i-books. Rather, they are *properties* of the object of my predications. That object is the token physical object picked off my desk and it is the *only* object that might be the referent of my expression 'this book'. We might predicate indefinitely many properties of this token object, including its informational "aspect", but none of those predicated

properties is the object of those predications. Thinking otherwise is a real "category mistake".

I emphasize that polysemy claims like Asher's *have metaphysical consequences*. So if the metaphysical consequence is false, so is the polysemy claim. This puts me at odds with John Collins (2017). Here is his polysemy claim: "one may use *book* to speak just about certain concrete tokens or bodies of information (stories, say) to the exclusion of the others" (681). He goes on to deny that this claim has a metaphysical consequence:

> there are no books (*punkt*) as the referents of *books* that are such as to provide an invariant value for *books* that makes tokens of 'Ψ(books)' true or false. This is not to deny that there are books (or cities and so on), but only that there are no such things without a specification of the kind of properties they may support such as to make true or false some given claim involving a predicate. The point here bears emphasis. The copredication argument does not militate for a skepticism towards 'ordinary' objects, or propound any other metaphysical thesis. (681–2)

To say that *books* does not have "an invariant value" – later, does "not express invariant referents" (p. 682) – just *is* to say that sometimes the referent is to one sort of entity and sometimes to another, a *different* sort of entity (the "exclusion" comes from this difference). From my perspective, *that is what having a variant referent amounts to*. So that variance *does* yield a "metaphysical thesis", the thesis that there are these two different sorts of entities, perhaps p-books and i-books. That thesis is false; there aren't p-books and i-books as referents, just books (*punkt*).[13] That's the way the world is and semantics must adapt to it. And what are we to make of "there are no such things without a specification of the kind of properties they may support such as to make true or false some given claim involving a predicate"? What I can make of it is the view that it is not books (*punkt*) that make *book*-claims true or false but sometimes one sort of entity, perhaps p-books, sometimes a different sort of entity, perhaps i-books. Once again we have commitment to a false metaphysical thesis. So the polysemy claim is false. In contrast, claims about *lunch* and lunches that are analogous to Collins' ones about *book* and books yield a true metaphysical thesis: there are indeed no lunches (*punkt*) but rather lunch-objects and lunch-events, two very different sorts of entities.[14]

None of this is to say that I *cannot* predicate a property of an "aspect" of the book. Of course I can: I can say, "The content of this book is real fun". It is to say that when I assert "This book is real fun" I *am not* predicting a property of an "aspect" of the book; I am predicating a property of *the book*, a property it has in virtue of having that content. It is a fact *about the world* that a certain physical

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[13] This is not to deny that there are book-like physical objects, some of which are books some not; nor to deny that there are informational contents, some of which are the contents of books, some not.

[14] Some fundamental differences probably underlie this disagreement with Collins. I assume that the reference of a word is the core of its meaning; that reference is a relation to certain worldly entities; so, finally, the word's meaning is largely determined by the nature of those entities. Collins probably disagrees with some or all of this.

object on my desk is both a book and real fun. No theory of words, a mere *semantic* theory, can gainsay that worldly fact. So when one turns to semantics, there should be no resisting the obvious view that "This book is real fun" attributes being real fun to that particular physical object.

How do we know all this about the meaning of 'book'? We follow the method of Sect. 3.6. 'Book' is regularly used to speaker-mean token objects like the one on my desk, a certain sort of physical object with a certain sort of content. For, it is regularly used to speaker-mean *books* and that's what books *are*; see metaphysics above! And, as already accepted, it is regularly used to speaker-mean types of such tokens. But it is not regularly used to speaker-mean anything else, including the contents of such physical items. ("But to have a certain book planned is to have a certain content planned." No, it is to have a plan to produce a physical token of an appropriate form *with that content*, though one may have no *particular* form in mind, of course. Producing the idea for a book is not producing a book!)

So 'book' has the uncontroversial type/token polysemy but none other. Similarly, 'CD', 'musical score', 'coin', 'smart phone', and 'inscription'. And an important moral is: just because a term refers to things that have different sorts of properties, including physical properties and functional ones, does not alone show that the word has two senses.

What about 'city'? A city is another artifact, a very large one, and like any artifact it is a physical object with a function. Its function is to house a large number of people. So it must have a large number of dwellings and a location (though it can change its dwellings, even its location). That function, with dwellings and a location, is roughly the nature/essence of a city. But a city will of course have other properties that it might not have had. These typically include having a population and a government of some sort (though a city can lose both by being abandoned; for example, Ostia Antica). It is likely to include some other properties that come from its having such parts as a cathedral, an opera house, or a sporting team. In light of this, we may be able to truly predicate 'in the Midwest', 'multi-ethnic', 'cultured', 'prosperous', 'poorly run', and 'banned Airbnb' *of one entity that is a city*. Some of these predicates may also be true of a part of the city; for example, 'multi-ethnic' may be true of its population. And we can say as much: "The population of the city is multi-ethnic." But this does not show that 'multi-ethnic' is not true of the city. Quite the contrary: it is true of the city *because* it is true of a population which is part of the city. And, once again, no semantic theory could gainsay the worldly fact that this one entity, the city, is in the Midwest, multi-ethnic, etc. So the variety of possible predications of the city gives no basis for thinking that 'city' is polysemous. And, what goes for 'city' goes for nouns like 'town' referring to locations for housing a smaller number of people than cities and for 'country' referring to ones for a bigger number.

Here is Steven Frisson on another interesting count noun:

A quick search for a word like 'school' reveals that it can refer to many different things:

> (1) Jocelyn walked to the school.
> (2) The concerned mother talked to the school.

   (3) Eve's little brother is at the school.
   (4) The school won the match in the last minute.
   (5) The school took a trip to the lakes.
   (6) School's out!
   (7) 'The school was that rare achievement –; a family within an institution.' (2009: 112)

Now (1) to (7) certainly reveal that some very different *predicates* can be true of a school but, as we have seen, *it does not follow* from this that 'school' "can refer to many different things". And 'school' doesn't in (1) to (7). 'School' refers to an institution with the function of educating children, an institution that typically has many parts: a building, an administration, sporting teams, and so on. Such an institution is an entity that can be "walked to" because it is in a building that can be walked to. (But the institution can change buildings whilst remaining the same entity.) The entity can be "talked to" by talking to the administrators who are part of the institution. Any pupil of the institution is "at" the entity. The entity "won the match" in virtue of having a part that is a team than won. The entity "took a trip" in virtue of some of its children taking it. The entity is "out" in virtue of the school's vacation having begun. The entity is "a family" in virtue of some aspect of its social life. In sum, one and the same entity can have any of these various properties. And what goes for 'school' goes for the nouns for other sorts of institution like sporting teams. No evidence of polysemy here.

   What we have said about count nouns like 'city' and 'school' carries over to the proper names of the entities counted. So, the examples that Collins (2017: 680) offers to demonstrate the polysemy of names do not do so.[15] The name 'London' refers to one entity that both "tends to vote Conservative" and is "the largest urban area in the U.K."; 'Germany', to the one entity that is both "central European" and a "democracy", 'Manchester United', to the one entity that has been both spending a lot of money and yet not achieving victories. And then there is Collins' interesting example of 'The Nile':

   (8) The Nile runs the length of Egypt and it serves as the most important trade route in the
       region as well as the source of irrigation for nigh-on all of Egypt's crop production.
   A river can be a geographical feature, an abstract relation, such as a trade route, or a body of
   water (*inter alia*), and these notions are independent; for example, if the Nile were to freeze in
   perpetuity and be used as a road, it would cease to be a body of water or even be a geographical
   feature in the relevant sense, but would remain a trade route. (680)

Indeed, a river can have the variety of properties mentioned. And these properties may be independent of each other in that having one does not imply having another. Indeed, it is common for an entity to have a variety of independent properties. But this difference among properties does not of course show that one and the same entity cannot have all of them. There is not a Nile that is a trade route and a different

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[15] Collins offers these examples in discussing copredication (to be considered later: Sect. 11.4.6). If I'm right and these names are not (in this respect) polysemous then Collins' examples do not illustrate copredication but just ordinary old predication.

Nile that is a body of water; there is just the one and only Nile with all the properties mentioned. That's the way the world is, not a matter of semantics. This variety of The Nile's properties does not show that 'The Nile' is polysemous.

Still proper names do yield some polysemies. Consider 'Washington'. It is commonly used to refer to a certain inhabited city with its own government. But 'Washington' certainly has another meaning in which it refers to the government of the United States. Similarly, 'Downing Street' has a meaning that refers not to a short street in London but to the government of Britain. This exemplifies a regular polysemy: a name for an inhabited location can yield a name for a government of any larger inhabited location centered in that location.

This is just the tip of the iceberg. Katarzyna Kijania-Placek has argued very persuasively that "proper names exhibit systematically polysemous meanings" (2018: 977). A proper name has an obvious role of designating a particular object that is the bearer of that name; see the last two paragraphs. This yielded the traditional "referentialist" theory that this role was the one and only meaning of the name. But, as Tyler Burge (1973) emphasized, a name is often used as a general term applying to objects that have been so-named; thus, "I have known several Fionas". This led him and others to "predicativism", the view that this general-term role was a name's one and only meaning. But then there are examples of the "descriptive" use of proper names that did not fit this picture. We have already mentioned one in our list of widely accepted examples of regular/systematic polysemy; thus, Picasso's name yields a term for his work. Then consider, "Westmoreland was no Napoleon". Kijania-Placek draws attention to a many similarly "descriptive" uses of names; for example, "Two Obamas came to the Halloween party" (p. 974). She also argues (pp. 975–6) that names have anaphoric, bound, and deferred uses ("France plays Germany tonight"). The way to accommodate this range of interesting data is surely to abandon both one-and-only theories, "referentialism" and "predicativism", in favor of the view that proper names exhibit *several sorts of regular polysemies*.[16]

In the present discussion of the likes of 'book' and 'The Nile', I am arguing that Pragmatists *over*estimate the amount of polysemous phenomena. In my ongoing case for SEM over PRAG, I am arguing that Pragmatists *under*estimate the amount of polysemous phenomena that is conventional and semantic.

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[16] I have emphasized (Sects. 3.2.1, 6.5) that referential meanings are the properties of representations in virtue of which representations play certain causal roles, particularly that of causing behavior and guiding us to reality. These dual roles led me (1996. 2012d, 2015c) to talk of proper names, particularly tokens, as having more than one meaning, hence being in effect ambiguous: first, the property of referring to *x* by a certain causal mode (c.f. "direct reference", Sect. 9.2, n. 1), which explains behavior; second, the property of simply referring to *x*, which guides us to reality. This was infelicitous, at least. It would have been better to describe the name as having just the one meaning, the causal mode, whilst adding that only *part of* that meaning, the property of referring to *x*. is needed to explain the role of the representation in guiding us to reality.

11.2.5   *Polysemy vs "Underspecified" Monosemy*

I quoted the following claim earlier (Sect. 8.3): "questions concerning polysemy and monosemy are some of the most fundamental in lexical semantics" (Cruse 1992: 577). In the same vein: "the literature shows that distinguishing polysemy from monosemy is far from a trivial matter" (Falkum and Vicente 2015: 1). A particularly troublesome problem has been that of determining, in many cases, whether a certain popular version of PEp applies rather than SEp. According to the popular version, in these cases, a word is said to have just one "abstract", "indeterminate", "underspecified", "general", "core" linguistic meaning and so to be monosemous. It is then pragmatically modified in context into a more precise meaning. (We introduced this "abstract-core" version of PEp in Sect. 8.4.) Ravin and Leacocke nicely sum up the significance of this:

> A[n]…important distinction is one between polysemy and indeterminacy, sometimes referred to as vagueness. The distinction is between those aspects of meaning that correspond to multiple senses of a word versus those aspects that are manifestations of a single sense….The distinction between polysemy and indeterminacy is at the core of semantic theory as it defines the relation between the semantics of linguistic expressions and the extralinguistic entities to which these expressions refer. (2000: 2–3)

We have already met an extreme example on the underspecified-abstract-core side – the PRAG side – in Ruhl and his "MONOSEMANTIC BIAS" (Sect. 8.1): "the hypothesis that a form…has a single meaning, and that all the complicating factors that make it appear polysemic have their sources in contextual contributions to meaning" (1989: viii). Jay Atlas provides another:

> In interpreting utterances are we *selecting* from the linguistically given readings of a syntactically or lexically *ambiguous* sentence, or are we *constructing* from a meaningful but radically *sense-general* sentence a contextually determined interpretation of an utterance, an interpretation is far more specific than the literal meaning of the sentence?
>   *My answer has been*: In interpreting utterances, we are more often than philosophers and even linguists have recognized, doing the latter. (1989: 28–9)

My own view, SEM, of course takes the opposite view of the cases in question. Let us consider some examples. Geoffrey Nunberg points to a nice one:

> The word 'cell' can be used to refer to a number of different kinds of things: to cells of the body, prison cells, battery cells, Communist cells, photocells, the cells of a matrix, and so on. (1979: 171)

Next, consider the verb 'run': The action described differs greatly in the following: 'running a marathon'; 'running some water'; 'running a shop; 'running on gasoline'. "The verb run…has 29 senses in Webster's, further divided into nearly 125 subsenses" (Ravin and Leacock 2000: 1). Dictionaries are indeed helpful because we can count on their descriptions to be reliable reports of *usage* (which is not to say of *meanings*, of course; see Sect. 3.6). Thus we note that OED lists 73 senses for the verb 'get' and 63 for the verb 'cut'.

Take 'cut': there is a vivid difference between the cutting-action in cutting the grass, cutting a cake and cutting one's nails. The popular PRAG view is that 'cut'

has one conventional abstract-core meaning that gives an imprecise characterization of an action, something along the lines of *produce linear separation in the material integrity of something by a sharp edge coming in contact with it* (Hale and Keyser 1985).[17] This generic meaning is then pragmatically modulated in every utterance to get a more particular characterization. According to SEM, in contrast, 'cut' has several conventional meanings one for the cutting exemplified with grass, one for that with a cake, one for the one with nails, and so on. Just how many? Probably not as many as 63!

How do we tell? A constant refrain of this book, from Sect. 3.6 onwards, is that we look to regularities of speaker meanings to identify conventional meanings and hence ambiguities. And, as noted in Sect. 8.4, we do not appeal to the various diagnostic tests,

Consider 'cut' again. Speakers regularly use 'cut' to mean the particular action exemplified in cutting grass but *not* in cutting a cake; similarly, in cutting a cake but *not* in cutting grass. The best explanation of this is that 'cut' has a linguistic meaning that specifies the one action, and another linguistic meaning that specifies the other (SEp); it is semantically polysemous. But how do we know that a speaker means just one of those particular actions and not a generic action exemplified by any way of cutting? Consider the analogous question: How do we know that a speaker using 'bank' on an occasion means only *financial institution* and not the more general *financial institution or side of the river*? And the short answer to such questions, indicated in Sect. 3.6, is that it is likely to be obvious what speakers mean. This obviousness is evidenced in the typical agreement among theorists on what speakers mean. But if there is any doubt, we can ask a speaker to paraphrase, perhaps even demonstrate, what she meant and did not mean. If there is still doubt we can look for evidence in the speaker's behavior; we can see, for example, what actions she clearly counts as following her instructions, "Cut the grass", "Cut the cake", and so on.[18]

Suppose that SEM is right and 'cut' does have these various linguistic meanings and so is polysemous. That is, of course, quite compatible with 'cut' *also* having a generic "core" meaning. But the fact that *theorists* can describe such a meaning by abstracting what is common to all these particular cutting actions – see "linear separation" account above – does not show that 'cut' has that meaning. The regular use of the word with that meaning would, of course, be persuasive evidence that it has. But there is no such evidence. Indeed, I wonder if 'cut' is ever used generically outside the occasional "There are many ways to cut something", which seem better explained without positing an abstract-core meaning. I shall return to this issue in presenting the Abstract-Core Objection in Sect. 11.5.

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[17] Cf: "So what does *bear* mean?...this question cannot be answered in words" (Ruhl 1989: 63)

[18] This is my response, in effect, to a query of Carlo Penco and Massimiliano Vignolo (2019: 393–4): in the course of a helpful discussion of an incomplete draft of my book, they wonder how my methodology could answer the relevant questions about the meaning of 'cut'. Note that the evidence from usage is that 'cut' is polysemous not that it can be saturated by indefinitely many actions in context.

Finally, we must mention "co-composition". This an abstract-core view of poly-semous phenomena that nonetheless provides an SEp explanation and so supports SEM. In the impressively comprehensive book, *The Generative Lexicon* (1995), James Pustejovsky describes the view as follows:

> multiple elements within a phrase behave as functors, generating new non-lexicalized senses for the words in composition. (pp. 61–2)

Thus the sense that 'cut' contributes to a sentence depends on what it is combined with: combined with 'the grass', 'cut' means a cutting-grass action; with 'the cake', a cutting-cake action. Noting that baking a potato is a different action from baking a cake, Pustejovsky claims that "we can derive both word senses of verbs like *bake* by putting some of the semantic weight on the NP…[a] sense of *bake* is contributed in part by the meaning of *a cake*" (p. 124). As Vicente puts it, "verb meanings are, actually, dependent on whole VPs" (2018: 961). So, in the case of 'cut', a *linguistic rule* generates the specific cutting-grass meaning of 'cut' from an abstract-core meaning when 'cut' is combined with 'the grass'. So, the meaning of 'cut the grass' in context is determined by the conventions of English; the meaning is the result of *semantic generation* not pragmatic modulation. So this view is consistent with SEM.

Still, co-composition is not part of the SEM that I wish to urge. I take my case against co-composition largely from Jerry Fodor and Ernest Lepore (1998) in their review of Pustejovsky's book. So I shall be brief. The problem with co-composition is this. You can cut the grass in just the same way that you would cut a cake or cut your nails; you can bake a cake in just the same way that you would bake a potato. I could give the order "Cut the grass" intending that you cut it with scissors *without offending any rule of English*. As Fodor and Lepore say, in considering an allegedly ill-formed sentence we need to distinguish the situation of "*what it means*" being "defective" from the situation of it just being "'normally' or 'generally' hard to 'readily' interpret without contextual support" (1998: 275); and we need to distin-guish "what the language tells one from what one knows about the world" (p. 274). "What makes…*bake a knife* sound funny is a thing about the world, not a thing about the words: everybody knows that you can't make…a knife by baking [it]" (p. 280). Indeed, we should resist the tendency in lexical semantics to turn facts about the world into facts about meaning.[19] (This resistance is a recurring theme in what follows.) Fodor and Lepore conclude: "There is no evidence that the meaning of governing expressions is ever modulated by the semantics of the expressions that they govern" (p. 283). I agree. We should see the NP attached to 'cut' not as partly *constituting* 'cut''s meaning in context but as *evidence* of its meaning in context. The meaning is constituted by the speaker's choice of one lexical item for 'cut' rather than another.

The idea that a polyseme has an *abstract* core meaning from which others are derived, may have initial appeal to the Pragmatist with words like 'cut'. But, as we

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[19] It is even worse, of course, to turn a *non*-fact about the world into a *non*-fact about meaning. That's one way of seeing the mistaken idea, discussed in Sect. 11.2.4, that 'book' has a "physical" sense and an "informational" sense.

shall see with the "Abstract-Core Objection", it is especially problematic for PRAG (Sect. 11.5).

Furthermore, it is worth noting that with words like 'foot' and 'suit', the appealing Pragmatist idea should be that the word has a quite *un*-abstract core. The same issue of distinguishing polysemy from underspecified monosemy can arise with alleged cores that may not be particularly abstract:

> consider *paint a portrait* (depict someone using paint) vs. *paint the bathroom door* (cover the surface with paint) – are these distinct senses or different specifications of a single underspecified sense? (Geeraerts (2015: 236–8) gives a useful overview and a critical assessment of the diagnostic tests that have been used to distinguish accidental homonymy (ambiguity) and polysemy from semantic underspecification…. (Depraetere and Salkie 2017b: 23)

I propose an SEp explanation, just as with 'cut'. Speakers regularly use 'paint' with the depicting-sense but not the covering-surface-sense and vice versa. We know this because we have evidence of what they speaker-mean. 'Paint' is polysemous. And again one should resist the co-composition version of SEp according to which the required senses of 'paint' are linguistically determined by the NP it is combined with. For, as Bach aptly remarks in commenting on the above passage, "both *paint a portrait* and *paint the bathroom door* have additional readings – one can cover a portrait with paint and use paint to depict a bathroom door" (Bach 2017: 48). This provides ammunition for a Fodor and Lepore objection to a co-composition proposal.

11.2.6 *Polysemy vs Generalized Conversational Implicature*

As Stephen Neale points out:

> Semantical claims…about the linguistic counterparts to some of the formal devices of quantification theory (e.g., 'and', 'or', if…then…', 'the', and 'a') were some of Grice's philosophically important targets. (1990: 79)

The targets were, of course, claims that these counterparts were not simply synonymous with the formal devices but were ambiguous. In response, Grice (1989) insisted that the uses of these expressions that suggested ambiguities were better handled as generalized conversational implicatures.

I gave examples in Sect. 8.4 of the sort of uses that Grice had in mind:

> First,…Jones says [to Neale] "Smith turned up and got drunk", implying that he *first* turned up and *then* got drunk. Second,… Jones has lots of people in his seminar and responds to Neale's question by saying "Some people in my seminar turned up", implying that some in his seminar did not turn up.

Consider 'some'. Grice's target is the claim that this use of 'some' exemplifies the conventional meaning *some but not all*. His appealing alternative account is that this use is a "scalar" implicature". This alternative applies Grice's conversational maxim of "Quantity": "Make your contribution as informative as is required (for the current

purposes of the exchange)" (1989: 26). Saying "Some people in my seminar turned up" is "weaker" and less informative than saying "All the people in my seminar turned up". So if Jones *thinks* that all turned up, the maxim enjoins him to *say* that they did. So his failure to say this implicates that all did *not* turn up.[20] There is no call to say that 'some' not only has the uncontroversial meaning of the existential quantifier but also the meaning *some but not all*. As Kent Bach puts it nicely, "it is the failure to use a stronger expression, not the meaning of the weaker one actually used, that conveys the additional information" (2013: 239). Indeed, the Gricean story is a very convincing account of how an expression with the quantifier meaning *could be* used to convey the message *some but not all*. But it is not a convincing story of how it was *actually* used to do so in Jones' exchange with Neale. It is not convincing because, as noted (Sect. 8.4), it seems implausible that the psychological-reality requirement for an implicature is met in exchanges like this using 'some'; and we might add, looking ahead to Sect. 11.6, psycholinguists have not even attempted to show that it is met. Rather, there is now a convention of using 'some' to mean *some but not all*, a convention that may well have been created by a past regularity of so using it in particularized conversational implicatures. So Jones and Neale did not go through the mind-reading processes demanded by implicatures but rather they simply exploited that convention. In sum, implicatures yield a plausible story of how 'some' came to mean *some but not all* but not a plausible story of its current use to mean that.[21]

What about the temporal order meaning of 'and'? It is not so easy to come up with an appealing implicature account of this (as Neale's cautious "might be explicable" indicates; 1990: 80). In any case, such an account will fall foul of the psychological-reality requirement. Furthermore, it is plausible that 'and' does have a conventional meaning that conveys a temporal order because that provides a good explanation of the obvious fact that 'and' is regularly used to convey temporal order.[22]

Perhaps the treatment for 'some' will work for many other alleged scalar implicatures, for example 'looks' in Bach's, "That dress looks (but *is* not) expensive" (2013: 238). Perhaps the treatment for 'and' will yield another meaning of 'and' that implies a causal connection illustrated in Neale's, "The President walked in and the troops jumped to attention" (1990: 79). I shall not explore this.

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[20] See Neale 1990, pp. 82–3 for a full account of such an implicature.

[21] Furthermore, the Gricean view faces the "symmetry problem" (Block 2008). We might just as well claim that what Jones said is "weaker" and less informative than saying "Some but not all the people in my seminar turned up". So, if Jones *thinks* that some but not all turned up, the maxim of "Quantity" enjoins him to *say* that they did. So Jones' failure to say this implicates that all turned up.

[22] (a) However, see Lepore and Stone (2015) for what may be a better way to explain the temporal interpretation of utterances with 'and': it is "a matter of disambiguating the logical form of discourse, in a way that satisfies the interpretive constraints conventionally associated with the linguistic expressions the speaker has used" (125). (b) Eliot Michelson (2016) has used a "Lying Test" to argue that 'and' has just the one meaning. I agree with Penco and Vignolo that the "argument fails because it conflates the metaphysics of meaning with the epistemology of understanding" (2019: 397). (See Sect. 7.1 on this distinction.)

As can be seen, I am dubious that any alleged conversational implicatures meet the psychological-reality requirement. I have not of course demonstrated that none will.

This concludes my presentation of examples of semantic polysemy (SEp). It is not comprehensive, of course, and doubtless contains mistakes. Still, I hope it presents a persuasive case for SEM. But what about the *objections* to SEM? We have already rejected some: Modified Occam's Razor (as usually construed) and Ruhl's Monosemantic Bias (Sect. 8.2); Bach's "standardization" (Sect. 8.6). We will consider more objections in Sect. 11.4.

But first, in the next section, we must introduce the recent psycholinguistic discussion of polysemy. For, some of the objections to SEM are to be found there. Following discussion of the objections, I shall identify, in Sect. 11.5, the psycholinguistic evidence we *need* to cast light on the SEM-PRAG issue. Finally, in Sect. 11.6, I shall consider the bearing of the psycholinguistic evidence we *have* on that issue.

11.3   Polysemy in Psycholinguistics[23]

11.3.1   Introduction

We can sum up the issue that concerns us as follows. Suppose that a speaker means *M* by a polysemous expression *E* in a certain utterance. Our concern is with whether *E*'s meaning *M* is typically fully constituted by the conventional linguistic meaning of *E* in this context, an SEp explanation, or partly constituted by a pragmatic modification, a PEp explanation. SEM claims the former, PRAG, the latter.

The focus of the rest of this chapter will be on cognitive science outside philosophy, particularly on psycholinguistics, for light on the SEM-PRAG issue. Interest in polysemy has recently surged in psycholinguistics, as Falkum and Vicente (2015) bring out in a helpful survey. In the present section, I shall do preliminary work to bring the psycholinguistic literature to bear on SEM-PRAG. This is not easy because this literature is "a different world". Its issues are not SEM-PRAG, though they are certainly related to SEM-PRAG. And it includes background assumptions that differ crucially from any I think should be made. In particular, the literature takes over views from the linguistics field of "lexical semantics" that SEM should not embrace.

Pustejovsky describes one view that plays a prominent role in the psycholinguistic discussion of polysemy as follows: it allows "the lexicon to have multiple listings of words, each annotated with a separate meaning or lexical sense". He calls this view, "*Sense Enumeration Lexicon*" (1995: 34). This view, SEL, is a version of

SEM (applied to polysemy in particular), but has two important commitments that SEM lacks, as we shall see (Sects. 11.3.2, 11.3.4). Pustejovsky and many others reject SEL, urging instead either "underspecification" or "overspecification" views. Thus, in an interesting critical assessment of the state of play, Vicente claims that "recent psycholinguistics tends to favor underspecification and overspecification approaches" (2018: 952). How do these two approaches relate to SEM-PRAG? More importantly, how does *the experimental evidence* offered for these approaches bear on SEM-PRAG? These are my main concerns but I shall also make some passing critical comments on the approaches themselves.

Vicente describes "underspecification" as follows: "the meaning of a polysemous term is an underspecified, abstract, and summary representation that encompasses and gives access to its different senses" (2018: 952). Frisson describes this sort of meaning, which he takes to be activated initially in understanding a polyseme, as "semantically underspecified" and "abstract", "the same for all established senses of a word", encompassing "all semantically related interpretations" (2009: 116). So, underspecification requires a process by which the abstract-core meaning of a polyseme is enriched in context to yield a specific sense. It is natural to take the proposed enrichment process to be a pragmatic modification of some sort; so, underspecification is the "abstract-core" version of PRAG that we have just been discussing (Sect. 11.2.5). But psycholinguists may not take underspecification to be committed to a *pragmatic* enrichment process rather than to some sort of *linguistically determined* process. One wonders, of course, what that linguistic process could be (that does not collapse underspecification into overspecification below). In any case, we must see how the psycholinguistic evidence bears on the *pragmatic* version of underspecification and the abstract-core view, because that view is sharply at odds with SEM.

Vicente describes "overspecification" as follows: "the meaning of a polysemous term includes all its different senses, which are stored in a single representation. Senses are selections of the total meaning of the word" (2018: 952). Pustejovsky's "generative lexicon" (1995), of which co-composition is an instance (Sect. 11.2.5), is perhaps the best-known example. Overspecification, like SEL, is a version of SEM (applied to polysemy) with additional commitments: it has SEL's two and adds another (Sect. 11.3.3).

11.3.2 "Represented and Stored"

At the end of their survey, Falkum and Vicente describe one of the "two main concerns in the contemporary discussion" that they have identified: "the question of how the different senses of a polysemous expression are represented and stored" (2015, p. 14). Talk of what is "represented and stored" is ubiquitous in the literature. So too is talk of different senses of polysemous words, unlike of homonymous ones, being perhaps stored "under a single entry" in the lexicon rather than having "distinct lexical representations". What should be made of all this? I take these

researchers to be concerned with the ways in which the senses/meanings of words are in the mind. So their concern should be very relevant to the SEM-PRAG issue. I shall consider "represented and stored" first, and then stored "under a single entry" in the next subsection.

On the minimal, least theory-laden, view of the mental lexicon I presumed in Sect. 5.1, lexical entries are dispositions to associate words with certain meanings in production and understanding. So one way for a person to "store" a word E with a sense M is for the person to have a disposition to associate E (a physical form) with M in production and understanding. If E is ambiguous, then it will feature in at least two such stored entries. According to SEM, this is the typical situation with polysemy. So polysemous phenomena are typically semantic not pragmatic.

In contrast, M might be in the mind of a speaker as a meaning of E only in virtue of her novel pragmatic modification of the lexical meaning of E "on the fly"; and in the mind of a hearer only in virtue of her novel interpretation of E on the assumption that its lexical meaning has been modified. Everyone agrees that there are such events. And I assume that we would all agree that, at that point, M is not "stored" as a meaning of E at all, but is spontaneously created. Such polysemous phenomena are pragmatic.

Now, suppose that these processes of modifying and interpreting E to mean M are repeated a few times in successful communications. The SEM picture is that this is likely to cause a *convention* of E meaning M, and hence bring about a new *linguistic* meaning and *semantic* polysemy (Sect. 5.1). And that, like all other linguistic meanings, will come to be "stored" in the lexicon of speakers and hearers. But suppose that this did not happen: suppose rather that the modifying and interpreting events became regular *without establishing a new lexical entry*; the processes are no longer novel but somehow still have the nature of those initial on-the-fly spur-of-the-moment processes. So the polysemous phenomena remain pragmatic. That, I take it, would amount to "storing" M in a different way, not in the lexicon but in the mechanisms of modification and interpretation. PRAG is committed to this account of polysemous regularities being the norm. I have doubted that account and insisted that we badly need evidence of these regular processes in speakers and hearers that differ from the standard processes of disambiguation (Sect. 8.4; and see Sect. 11.5 below). So, if the psycholinguistic discussion of storage throws light on *this* issue – storage in the lexicon versus storage in the mechanisms of modification and interpretation – it is very relevant to our concern.

I have not talked of the "representation" of senses (or words) in my account of storage. 'Representation' and cognates are very widely used in cognitive science but we need to be very careful with them because they do not have one clear and steady meaning there.[24] The idea that the linguistic meaning M of E is "represented" in the mind of a competent speaker might be the uncontroversial "low-key" view that M is *present somehow in the mind*. And then the question of how M is "represented" is

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[24] For discussion, see Devitt 2006b: 5–7. For an indication of the trouble that 'represent' can bring, see the fascinating exchange: Rey 2003a, 2003b; Chomsky 2003.

just a question about the nature of that presence: What can we say about that presence beyond the already-known fact that speakers are disposed to associate *M* with *E*? But 'represent' has a widespread meaning, particularly in philosophy, that is much more robust. On this meaning, representing is a *referential* relation between something that represents and something that is purportedly represented; for example, between a portrait of Winston Churchill and the man himself; between a sound 'The President of the United States' and Biden; between an inscription 'rabbit' and rabbits. This meaning of 'represent' and cognates is exemplified in the popular "Representational Theory of Mind" according to which a thought about Biden is a mental state including something that represents Biden. This meaning yields the "high-key" view that a sense of, say, 'rabbit' is represented in the mind in that there is a lexical entry that *designates* 'rabbit'- just as that Biden thought designates Biden – and goes on to *describe* its sense; so the lexical entry provides *propositional information* about that sense of the word 'rabbit', the sort of information provided by a *semantic theory*. The evidence suggests that it is this high-key view that features in the literature that concerns us.[25] Yet this high-key view of the presence of senses in the mind is dubious. We know that speakers have a lexical entry that disposes them to associate 'rabbit' with its sense but we lack any persuasive reason for thinking that the entry does this by providing *semantic information about* the sense of 'rabbit'; or so I have in effect argued (2006b: 195–243). Rather than *designate 'rabbit' and describe its sense*, the lexical entry disposes speakers to produce and understand tokens of 'rabbit' that *have* a sense in virtue of which *the tokens apply to rabbits*; the lexicon represents *rabbits* not the *sense of 'rabbit'*. SEM, unlike SEL, is not committed to the high-key representation of senses.

11.3.3 Stored "Under a Single Entry"

Turn now to the talk of different senses being stored "under a single entry" rather than having "distinct lexical representations". Consider the following, for example:

> It is widely accepted that the different meanings have distinct lexical representations in the brain (Beretta et al. 2005; Pylkkänen et al. 2006). By contrast, the nature of polysemous representations is far more controversial and less well understood….Do polysemous senses, like homonymous meanings, have distinct lexical representations in the brain or do they share a single common representation? (MacGregor et al. 2015: 126–7)

Perhaps polysemes are stored as "unified lexical representations" (Li and Slevc: 1). The same idea is to be found in talk of a single mental structure with many possible meanings: "many psycholinguists believe that the semantics of a word-type can be a structure that offers different possibilities of meaning" (Vicente 2018: 951).

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[25] "It is commonly assumed that when we encounter a word in a text, we automatically and immediately activate specific, detailed semantic information associated with that word and instantly integrate this information in the unfolding interpretation of the text" (Frisson 2009: 111).

This idea that polysemes might be unified in the lexicon is the key idea in psycholinguistic discussions of polysemy. What to make of it? One answer is very pertinent to the SEM-PRAG issue. Suppose that the polysemy in question is pragmatic (PEp) not semantic (SEp): *E* means *M* in virtue of a pragmatic modification. This amounts to saying that the lexicon does *not* include a distinct entry pairing *E* with *M*. So there could be a single lexical entry pairing *E* with some meaning other than *M*, a "core" meaning whether abstract or not, from which many meanings, including *M*, are derived by pragmatic enrichments. Only the core meaning is stored in the lexicon. Here is a description of such a theory (based on Nunberg 1979):

> all that is represented is a core meaning of a word. The different polysemous extensions are generated on the fly, using pragmatics and plausible reasoning. Thus, on this view, different senses are not prestored but are rather computed from contextual features. (Klein and Murphy 2001: 261)

This "single-entry" PRAG view of senses unified under a core meaning – our earlier version of "underspecification" (Sect. 11.3.1) – stands in sharp contrast with SEM's view that *E* is *semantically* polysemous, hence *ambiguous* and, one would think, with each sense stored *separately* in the lexicon, a "multiple-entry" view.

But this is far from the only way that psycholinguists think of the unification. For, it is common to suppose that a word might be *both* ambiguous *and* yet still have its senses stored together. Here are three examples of this supposition. (i) Homonymous senses are taken to have distinct storage, so the issue is whether polysemous ones do too. Nonetheless, researchers once entertained the idea that even homonyms, paradigms of ambiguity, might have "a single representation" until experimental results on the effects of meaning frequency on processing led the researchers away from that idea (Frisson 2015: 18). (ii) Lucy MacGregor, Jennifer Bouwsema, and Ekaterini Klepousniotou's earlier-quoted question about whether "polysemous senses, like homonymous meanings, have distinct lexical representations" (2015: 127) is in a context that *assumes* "homonymy and polysemy" are "two types of ambiguity" (2015: 126). (iii) Leon Li and Robert Slevc, talking of "the multiple senses of an individual word represented in the mental lexicon", hence of an ambiguous word, consider whether the senses "are represented together…or separately as distinct lexical representations" (2016: 2). Yet, how *could E both* be ambiguous *and* have its senses stored under a single lexical entry? For *E* to be ambiguous *is* for it to have more than one conventional linguistic meaning and this seems to *require* that a competent speaker have distinct dispositions associating it with each of its conventional meanings. What then could talk of the ambiguous *E*'s meanings being stored "under a single entry" or in one "structure" be telling us about *what is actually going on in the mind/brain*?

Our earlier discussion of regular (systematic) polysemy (Sect. 11.2.3) suggests one answer. In some cases of polysemy there is a "meta-convention" of the following form: whenever a conventional meaning is established according to which an expression refers to things of type *X*, that expression will *thereby* also have another conventional meaning according to which it refers to things of related type *Y*. Thus, when we introduce 'delgar' for a plant – Klein and Murphy's example (2001) – we

thereby introduce that word into our language as a mass noun for certain stuff. Suppose that, in its early days, 'delgar' is never used as a mass noun. Then it may be appropriate to say that what is stored in the lexicon is just the one, count-noun, sense of 'delgar' together with the meta-convention that generates the other sense. But should people start to use 'delgar' as a mass noun – perhaps because they start eating delgar or making something out of it – then this basis for saying that the senses are stored together disappears.

Nonetheless the idea that regular polysemy provides a case for a much more widespread unification of senses in a single lexical representation is present in the literature. Thus MacGregor et al., after noting the "systematic patterns" of "semantic connection[s] between the different meanings" of words like 'rabbit' and 'fish', draw attention to the argument "that such linguistic rules make it possible that there is a single basic or core lexical sense from which others are derived online during comprehension" (2015: 127). Presumably, this idea counts as a case of "overspecification" of meaning. It differs from my 'delgar' idea in two important respects. First, the core meaning from which others are semantically generated seems to be the sort of abstract-core meaning we have been discussing, in contrast to the quite concrete meaning of 'delgar' referring to plants. The very existence of such an abstract meaning is problematic, as we shall see (Sect. 11.5). Second, my 'delgar' idea unified 'delgar' in one lexical item *in its early days*. The unification in the core-meaning idea is *forever*: in comprehending a regular polyseme like 'rabbit', a core meaning is always activated which then activates other meanings from which hearers select one in the context. Why suppose that? Here's a simpler alternative which cuts out the core: the polyseme activates several different senses and the hearer selects one. One wonders about the likelihood of any evidence favoring the core idea over this simple alternative. And what about production? Presumably there must be an analogous story there. So when a speaker says "Let's have rabbit", she activates an abstract core-meaning and moves from that to the rabbit-stuff meaning. I think it is a safe bet that we will not find evidence of that.

So I do not buy this way of unifying representations of polysemes. And I do not buy others, including co-composition (Sect. 11.2.5) and Pustejovsky's "dot objects" (Sect. 11.4.6). These presumably also count as "overspecifications". So the unified single-entry view is compatible not only with PRAG and underspecification but also with overspecification. And the important thing for us to note is that overspecifications are semantic not pragmatic explanations of polysemy, SEp not PEp. Note, for example, the crucial difference between the just-discussed idea for regular polysemy and the earlier PRAG view. That regular-polysemy idea derives a meaning from a core meaning by a *linguistic* rule not a pragmatic modification; it is a language-internal *semantic* generation.

So, these alleged single-entry meanings for regular polysemy, co-composition, and dot objects, presumed to be overspecifications, are compatible with SEM as stated (Sect. 3.5). *But the SEM I am urging should be taken henceforth as excluding such overspecifications.*

Finally, it must be acknowledged that nothing that I have said makes sense of the idea, no longer embraced but once apparently entertained, of *homonyms* being

stored under a single representation. I suspect that this can only be understood on the dubious "high-key" interpretation of "representation". On this view, a representation is a body of semantic propositional information or knowledge (Sect. 11.3.2). So the idea must be of the information store for one meaning of the homynym being somehow unified with that for another. One wonders how, of course.

Beyond all this, one wonders whether the talk of single storage could sometimes be just a way of capturing a close *causal* relation between the dispositions that constitute distinct lexical entries for polysemes. But that would be more aptly described as "*causally-related*" storages rather than as a "*single*" storage. In any case, *unless any alleged evidence for the single storage of E's meanings includes evidence that E's meaning M is the result of a pragmatic modification, the evidence is not for PRAG*; see earlier (Sect. 8.4) on the psychological-reality requirement. So that is the evidence we should look for.

11.3.4   An "Information-Rich" Lexicon?

Falkum herself, in a very helpful paper promoting a "radical pragmatic account" of polysemy (2015: 85), a "relevance-theoretic" version of PRAG, describes "two approaches to polysemy": rule- or code-based vs pragmatic-inference-based:

> A fundamental difference between rule-based and pragmatic approaches to polysemy lies in their radically different conceptions of what a language is. Underlying rule-based approaches is the view that language provides an information-rich code that enables speakers and hearers to encode and decode their thoughts in much detail, with pragmatics as a useful add-on to this linguistic capacity, operating primarily when some interpretation other than the linguistic default was intended. By contrast, radical pragmatic accounts see the role of the linguistic system as being that of providing a minimal input or clue -- a 'sketch', or 'blueprint' of the speaker's meaning -- which the pragmatic inferential system uses as evidence to yield hypotheses about occasion-specific, speaker-intended meanings. In this sense, we may call the first a code-based approach, and the second an inference-based approach. (2015: 85; see also 90, 97)

With two crucial qualifications, Falkum's account of the "code-" or "rule-based" view – the earlier SEL is an example (Sect. 3.1) – captures SEM's view of polysemy. And she has captured well the view of language that underlies SEM and my approach in this book. She has also summarized well the Pragmatist approach and its underlying view of language, *as typically presented*. But, for the reasons set out in Chap. 7, this way of presenting the Pragmatist alternative (PRAG) involves a serious confusion of the metaphysics of meaning with the epistemology of interpretation, "The Second Methodological Flaw of Linguistic Pragmatism". If the linguistic system does indeed provide only a minimal part of the speaker's message, as the Pragmatists suppose, the extra is not provided by any pragmatic inferences in the hearer but by what the speaker does: it is provided by the enrichments and impoverishments that the speaker has in mind.

My crucial qualifications are with the contrast that Falkum has in mind in saying that, on the code-based view, language provides an "information-rich" meaning which contrasts with the information-poor "blueprint" of a meaning provided on PRAG. The first qualification is with the talk of "information", reflecting Falkum's view of the lexicon:

> By 'mental lexicon', I refer to individual speakers' stable mental representations of words, which include information regarding their semantic properties (in the form of 'meanings' or 'senses'), as well as phonological and syntactic properties, which are accessed when a word is encountered in discourse. (p. 84, n. 3)

This is the "high-key" view that lexical entries are not mere dispositions but representations of *semantic information*, a view that is no part of SEM (Sect. 11.3.2). Still, let us go along with this view for a moment. The second qualification concerns something even more problematic: the view, popular in lexical semantics, that the information provided is *rich*. That view goes back at least to a classic paper by Jerrold Katz and Jerry Fodor (1963). A lexical entry is thought to be rich in that it describes a meaning that is constituted by a set of inferential relations to other words that determine its reference[26]; for example, one lexical entry for the ambiguous word 'bank' relates it to 'financial institution', another, to 'shore' (Pustejovsky 1995: 34). This view that the meanings allegedly represented are rich is controversial in philosophy, for reasons that I can only indicate briefly here. And it is important for our purposes to note that though such a view, like the "high-key" view of representations, is part of SEL, it is not part of the SEM I am urging.

So what, briefly, is wrong with the idea that lexical meanings are rich, whether represented (high-key) or not (low-key)? A good place to start an answer is with Fodor and Lepore's (1998) review of Pustejovsky (1995). Pustejovsky argues for a novel generative version of an information-rich lexicon. In Section 1 of their review (pp. 271–8), before getting to Pustejovsky's novel proposal, and partly inspired by Quine, Fodor and Lepore look very critically at the motivation for rich lexicons *in general*, the sort of lexicons that are so dominant in the field of lexical semantics. The key motivation comes from the view "that word meanings are constituted by inferences, hence that knowing what a word means involves knowing (some of) the inferences in which it participates" (p. 271). Relatedly, in Pustejosky's words, "lexical semantics is the study of how words are semantically related to one another" by relations such as "synonymy" and "entailment" (1995: 23). As Fodor and Lepore point out, this leads to

> constraints on semantic theories that a purely denotational lexicon clearly could not meet, hence that are supposed to motivate a richer notion of lexical semantic representation than a denotational lexicon could provide. (1998: 271–2)

According to the "purely denotational lexicon" which is thus ruled out, "satisfaction conditions, and properties defined in terms of them, are the only semantic features of linguistic expressions that lexical entries specify" (p. 271). This yields a lexicon

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[26]What about the view that the lexical descriptions are rich but do *not* determine reference? This departure from Fregean orthodoxy is quite mistaken, in my view (1996).

that is as poor as a church mouse, which is what Fodor and Lepore favor (with a minor qualification). So what is wrong with this poverty? What is the motivation for the popular information-rich view? Why suppose that meanings are constituted by inferential relations which then constitute *semantic* relations between words? The motivation is, as Fodor and Lepore put it delicately, "notoriously problematic" (p. 272).

The information-rich view has another, related, motivation that is just as problematic. This motivation concerns the hearer's process of interpreting language. It is thought that we need the richness to drive "the inferences that a language reasoning system must perform in order to understand a sentence" (Pustejovsky 1995: 19); we need it to "'suggest' interpretations of words in context" (p. 87)? But why do we need a rich lexicon for that?

I would put the problem with these motivations briefly as follows: *we do not need to posit rich meanings to do the explanatory work here because we have already posited a rich world that will do the job*. Thus, consider a person with lexical entries for 'bachelor' and 'unmarried' that dispose her to associate them with the "poor" meanings of a purely denotational semantics, *refers to bachelors* and *refers to unmarrieds*, respectively. Someone with those dispositions and with the familiar *worldly* knowledge that bachelors are unmarried will of course be disposed to infer '*x* is unmarried' from '*x* is a bachelor'. Nothing more is required to explain the inference.

"But how do we explain the *necessity* of the inference?" Well, it is a necessary feature of the world that bachelors are unmarried; indeed, being unmarried is part of the *nature* or *essence* of being a bachelor; it's part of *what makes* a person a bachelor. As Fodor and Lepore say. "the *necessity* of an inference is not, per se, sufficient for its meaning constitutivity" (1998: 273). One might follow Quine, of course, in resisting such essentialist claims (as I do not). But then one should also resist the necessity of the inference.[27]

This is not to say that a purely denotational semantics is correct. It is just to say that the standard motivations for a rich lexical semantics are no good. So what would be a good motivation for some richness? We would need to show, I've argued (1996), that *the reference of, for example, 'bachelor', is determined by its inferential relations to other words like 'unmarried'*. If 'bachelor' refers to bachelors *in virtue of* its relations to other words which jointly refer to bachelors, *then* we have our needed motivation to enrich the lexical entry for 'bachelor' with relations to those words. In philosophy, theories of a word's reference of this sort are called "description" theories and were once ubiquitous. And that brings us to a devastating problem for the information-rich view of the lexicon.

There has been a revolution in the theory of reference in the last 50 years, initiated by Saul Kripke (1980) and Hilary Putnam (1975). As a result, it has been at

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[27] SEM and PRAG yield different views of the role of worldly knowledge in interpreting polysemous phenomena. Whereas SEM holds that this knowledge is typically used to *disambiguate* these phenomena, PRAG holds, or should hold, that the knowledge is typically used to *discover pragmatic modifications*.

least controversial in philosophy that a description theory is true of *any particular* word. Indeed, "atomists" like Fodor and Lepore think that it is not true of any word *at all*. Furthermore, we should all think that it *could not* be true of *all* words. Perhaps the reference of 'bachelor' does indeed depend on the reference of 'unmarried', but then what does the reference of 'unmarried' depend on?

> This process cannot…go on for ever: there must be some terms whose referential properties are not dependent on others. Otherwise, language as a whole is cut loose from the world. Description theories, which explain one part of language in terms of another, can give no clue as to how, ultimately, language is referentially linked to reality. These theories pass the referential buck. But the buck must stop somewhere. (Devitt and Sterelny 1999: 60)

We have previously indicated the lack of evidence *for* a rich lexicon. The revolution provides a powerful reason *against* any general assumption of lexical richness.

The main problems for description theories are "ignorance and error" arguments: competent speakers often do not know enough about the referent to meet the demands of the theories. (My discussion of incomplete descriptions in Sect. 9.5 featured an ignorance and error argument.) But what's the alternative to a description theory? How could the meanings of words *not* be constituted by Mentalese descriptions carrying "information regarding their semantic properties" that determine reference? Putnam's slogan for the revolution, "Meanings Just Ain't in the Head", points to the answer. A word's meaning can be constituted by some direct, probably causal, relation that the word bears to its referent. This relation is likely to involve epistemically undemanding "reference borrowing", Kripke's brilliant idea that was central to his non-descriptivist "better picture" of the reference of proper names (1980). Plausible examples in the literature of terms covered by non-description theories are 'Cicero', 'elm', and 'water'. Earlier, I briefly described causal theories for singular terms that are examples of what the revolution has in mind (Sect. 4.1.2). The literature suggests many other direct relations to explain meanings, including indicator and teleological ones.[28]

In light of the revolution, we should not commit to an information-rich lexicon. And we saw earlier that we should not commit to the lexicon having "high-key" representations of meaning. So we should think of the lexicon as follows. An entry for expression E in a person's lexicon is a disposition to associate E with meaning M in language use. That disposition involves M in particular because M is the meaning (content) of the concept C that the person is disposed to use E to express (Sect. 5.1). If E is covered by a description theory of reference, then its having meaning M is constituted by certain inferential relations between C and other concepts. If E is covered by a causal theory of reference, then its having M is constituted by a certain direct relation between C and some part of the world (Sect. 4.2.2). And the issue of "how the sense of a word is represented" in the person's mind, understood in the

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[28] The textbook, Devitt and Sterelny 1999 is an account of the revolution and summary of the theories that it has prompted. More detailed arguments for the views that I favor are to be found in Devitt 1981a, 1996, 2015c.

preferred "low-key" way (Sect. 11.3.2), is simply the issue of what inferential and/ or direct relations constitute that sense.

So how should we *decide* whether a term is covered by a description or causal theory of reference? The tradition has been to rely on intuitions about reference. That is what gave us the ubiquitous description theories in the first place and it is, in effect, what gave us information-rich lexical semantics. It also gave us Kripke's rival "better picture". As already argued (Sects. 2.8, 3.6), theories of language should become empirically well-based by moving away from reliance on linguistic intuitions toward evidence from usage. At this point we have hardly any well-based theories: we know very little about reference; we should be fairly agnostic. And we should certainly not presume description theories. So we should not presume a rich lexicon.

These are complicated issues which cannot be explored further here. But, given that the revolution in the theory of reference has played such a dominant role in recent philosophy of language, and is so very relevant to lexical semantics, it is striking that it seems to have had *no impact at all* on that field. Thus, there is no mention of the revolution, nor any citation of Kripke or Putnam, in Pustejovsky (1995), Falkum and Vicente (2015), Falkum (2015), or Vicente (2018). Pustejovsky simply assumes that an associated description determines reference (1995: 180–1). Vicente even suggests that proper names have "rich meanings" (2018: 962) without noting Kripke's contrary view (1980) that is the centerpiece of the revolution. Kripke's view has recently received powerful empirical support (Domaneschi et al. 2017; Devitt and Porot 2018).

In sum, intuitions about meaning relations and about the inferences involved in language understanding should not be the basis for theories of meanings and the lexicon. Intuitions about reference may provide some basis for these theories but a better basis should be found in empirical studies of usage.

What has led people astray? The first mistake has been alluded to before (Sect. 11.2.5): the ill-supported turning of facts about the world into facts about meaning; for example, facts about baking into facts about the meaning of 'bake'; facts about Cicero, water, and elms into facts about the meanings of 'Cicero', 'water', and 'elm'; perhaps even, facts about bachelors into facts about 'bachelor' The second mistake is captured by Fodor and Lepore:

> lexical semantics simply takes for granted that semantic facts are basically epistemological—that they are grounded in facts about what speaker/hearers know about their language. (1998: 273 n. 9)

What is thus taken for granted is the Cartesianism about meaning criticized in Chap. 2, a Cartesianism that the revolution should have made untenable. Somebody competent in a language could be totally ignorant about it.

In light of this, Falkum's nice summary of the "two approaches" should be modified by simply dropping "information-rich". It would then give the following apt and accurate description of SEM:

> Underlying rule-based approaches is the view that language provides a code that enables speakers and hearers to encode and decode their thoughts in much detail, with pragmatics

as a useful add-on to this linguistic capacity, operating primarily when some interpretation other than the linguistic default was intended.

On this view, according to SEM, the lexicon is rich in the range of thought contents that it can be used to express *not in information about* those contents that it provides. In contrast, according to PRAG, use of the lexicon is seldom if ever enough to express thoughts.

To conclude this preliminary discussion, the relation between the various psycholinguistic positions on polysemy and SEM-PRAG is complicated and a bit uncertain. My main takeaways on this are:

1. The multiple-entry view is consistent with the version of SEM that I am urging (= SEL without commitment to "high-key" representations of "rich" meanings) but is inconsistent with underspecification, overspecification, and of course PRAG.
2. PRAG, underspecification, and overspecification each entail the single-entry view.
3. Underspecification seems to require PRAG, but maybe not.

As promised, I turn now to objections to semantic polysemy and SEM, some to be found in the psycholinguistic literature that we have just been introducing. Following that I will consider what psycholinguistic evidence we *need* to cast light on the SEM-PRAG issue (Sect. 11.5) before addressing the experimental evidence we actually *have* (Sect. 11.6).

11.4 Objections to Semantic Polysemy

In Chap. 8 I responded to two important objections in the literature to treating polysemous phenomena semantically, two ways of promoting the conservative strategy of meaning denialism: first, Modified Occam's Razor (as usually construed) and the related Ruhl's Monosemantic Bias (Sect. 8.2); second, Bach's "standardization" (Sect. 8.6). This section considers several more objections, starting with an old one from Geoffrey Nunberg.

11.4.1 Not Arbitrary

If an expression in one language is polysemous, the corresponding expression in other languages will typically be so too, and in a similar way. Thus consider the English 'crawl' and the French 'ramper':

both verbs may be used to describe the primary motion of insects and invertebrates, and the deliberate crouching movement of humans; both may be applied to the growth of plants gradually extending over a surface; and both may be used pejoratively to refer to the way in

which someone shows a servile attitude towards someone else. (Fillmore and Atkins 2000: 104)

This is no surprise, of course, because polysemy is a matter of *conceptually related* meanings. But Nunberg sees a problem in this relationship for the SEM-view that a polyseme has its various meanings conventionally. For, if the expression did, those meanings would not be "governed by separate conventions" and so, in a sense, could not have been otherwise (1979: 148):

> I know of no language in which the same form is *not* used to refer to newspaper companies and newspaper publishers, or window-holes and window-glass, or game-activities and game-rules, and so on. And this should make us wary of saying that these regularities could be otherwise, which is a prerequisite for saying they are conventional … some other practice should have been equally as rational (1979: 148)

It has been a truism, at least since Saussure, that languages are "arbitrary" in that they could have been otherwise: the French word 'chat' might have meant dogs. Yet a language's conventional meanings are not *all* arbitrary *to the extent that Nunberg requires*. Nor are they to the extent that Bach requires in thinking that the arbitrariness of a conventional meaning makes it, unlike a Gricean conversational implicature, "not calculable" (2013: 238). Consider, once again, a dead metaphor: its conventional meaning arises from an already established conventional meaning. Thus, very likely, the first conventional meanings of 'crawl' and 'ramper' applied to "the primary motion of insects and invertebrates, and the deliberate crouching movement of humans". And those conventions could indeed have been otherwise: they were really arbitrary. But once those meanings were established, they restricted the conventional meanings that can develop out of them to what is conceptually related. So the only conventional meanings of those words that are really arbitrary are the first ones. Indeed, conventions in general often have explanations that show them to be far from arbitrary and quite "calculable". (I note that the relevant entry in my dictionary for 'convention' reads: "A practice or procedure widely observed in a group, esp. to facilitate social intercourse, custom"; nothing about arbitrary.)

Here is a brief argument for my position. Consider these two standard scenarios:

Polysemous scenario: expression *E* has a conventional meaning *M1* but is also regularly used as if it had a conceptually related conventional meaning *M2*.

Diachronic scenario: *E* once conventionally meant *M1* but, over time that meaning disappeared and *E* now has a conceptually related conventional meaning *M2* and so is regularly used to mean *M2*.

The only difference between *Polysemous* and *Diachronic* is that *M1* has disappeared in *Diachronic*. That *E* is regularly used to mean *M2* in each scenario is *equally* open to a diachronic explanation and those explanations may be *identical*. So this regular use in *Polysemous* is no more arbitrary than it is in *Diachronic*: the extent to which the regularities could have been otherwise is the same in the two cases. Yet in *Diachronic*, *E* does conventionally mean *M2*, as everyone would agree. So such non-arbitrariness as *M2* has in *Polysemous* cannot be a good reason for denying that *E* conventionally means *M2*.

Because of his requirements for a convention, Nunberg thinks "that only one of the uses of the word can be conventional". Yet, he claims, we mostly "have no empirical grounds for saying which use it is, since exactly the same pattern of use would be generated under any of several analyses" (1979: 174). This leads him to the radical conclusion that "the semantics/ pragmatics distinction cannot be validated even in principle" (1979: 143). Clearly if I am right that all the uses of a polysemous word can be conventional, we are not led to this conclusion.

A final thought. Suppose Nunberg were right about what is required for a convention as usually understood, so that some of the meanings of a polysemous word like 'crawl' could not be conventions. This would not really matter to SEM-PRAG! It would simply show that this usual notion of *convention* was not what we want for our theoretical purposes. What matters in applying the distinction to, say, 'crawl', is whether its various regularities of meaning are to be explained by *linguistic rules* (Sect. 5.1). If they are then those meanings are semantic. If Nunberg were right that the rules could not be conventional then we would need some less demanding notion to explain the establishment of the rules.[29]

11.4.2   Too Psychologically Demanding

Background assumptions always play a role in assessing which of two explanations is the better (Sect. 3.6). It often seems as if the following background assumption prejudices people in favor of the pragmatic PEp over the semantic SEp as an explanation of polysemous phenomena. SEp's requirement that hearers choose between a multitude of lexical entries for all the alleged conventional meanings is *psychologically demanding*. So we should prefer a version of PEp according to which, the hearer, in processing a polyseme, goes to the one lexical entry for an abstract-core meaning which is then modified in context. On this supposition

> the processor does not have to make a selection between competing interpretations but can immediately proceed with the more abstract interpretation and flesh it out on the basis of further information in the text or when necessary (e.g., at choice points in a text). (Frisson 2009: 118)

> by not immediately selecting a specific sense, processing can progress more smoothly as language users will not be faced with possibly extensive ambiguity, will not often assign the wrong sense of a word, and can use context to "home-in" on the intended sense. (Frisson 2015: 18)

We have already looked critically at such an idea in rejecting Modified Occam's Razor, as commonly construed (Sect. 8.2). Notice, now, how odd Frisson's view is. Consider the demands put on a hearer by PEp. Ruhl gives a nice indication of this in his drive for semantic minimalism (Sect. 8.1). He offers several pragmatic

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[29] See also Sects. 5.3 and 8.6

explanations by appealing to "pragmatic rules" to explain the polysemous 'bear'. He then aptly remarks:

> By now the reader may have the uneasy feeling that pragmatic rules are beginning to proliferate without restraint….Listing pragmatic rules may be an infinite task: all knowledge of the world can be included. (1989: 36)

Consider, for example, the earlier PEp explanation of a use of the word 'cut': the word has one abstract-core meaning, along the lines of *produce linear separation in the material integrity of something by a sharp edge coming in contact with it*. This generic meaning is then said to be pragmatically modified in every utterance to get a more particular characterization. So, the hearer always has to start from scratch with the general meaning and, using nothing but contextual clues, figure out by pragmatic rules the more precise action that the speaker has in mind – this action for grass, that for a cake, etc. That seems *more* psychologically demanding than using those same contextual clues to select one of a set of more precise conventional meanings, even if that set is quite large. It is hard to see how using the context "to 'home-in' on the intended sense" in detecting a modified sense is easier than making "a selection between competing interpretations" in disambiguating.[30]

The Pragmatist may respond that the hearer mostly does not have to start from scratch because she will remember previous uses of 'cut' in the context of grass, cakes, etc. But if these memories reflect the fact that such uses are regular in their respective contexts, why not suppose that the regularities reflect conventions?

11.4.3 Overgeneration

I noted (Sect. 11.3.4) that Ingrid Falkum has urged a "radical pragmatic account" of polysemy (2015: 85), a "relevance-theoretic" version of PRAG. Over the next three subsections, I shall consider three objections that she, and sometimes others, make to what she calls the "code-based approach", in effect, to SEM.[31]

The first objection is that a code-based view *overgenerates* (pp. 88, 91). But this is aimed at a "modern, sophisticated" "code-model" initiated by Pustejovsky (1995)

> in which a considerable amount of the context-sensitivity of lexical meanings is built into the linguistic system. More specifically, polysemy is seen as being generated by a set of lexicon-internal generative rules, which operate over information-rich semantic representations to yield default interpretations (Falkum 2015: 86)

My earlier (Sect. 11.3.4) "crucial qualifications" to Falkum's presentation of the code-based approach provides the response to this objection. My promotion of SEM

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[30] Furthermore, detecting modified senses is likely to lead to more misunderstandings. For, that detection process must be influenced by the hearer's background associations with the polysemous term and these may well differ from the speaker's and may indeed be eccentric. Yet, as has often been noted (e.g. Falkum 2015: 84), the understanding of polysemes typically proceeds effortlessly with few misunderstandings. (Thanks to Rayaz Khan for this point.)

[31] I considered two of Falkum's other objections earlier: a Modified Occam's Razor objection in Sect. 8.2 and an inflexibility objection in Sect. 10.3.

is accompanied by a view of the lexicon that is much more austere than one Falkum describes: it is not "high-key" or "information rich" and does not include co-composition or dot objects. Lexical meanings *may* be, though I do not say *always will* be (cf. Fodor and Lepore), constituted by direct relations to the world, relations of which the speaker is likely to know nothing. A word is semantically polysemous simply in virtue of its having more than one related lexical meanings. One meaning may be "generated" from another only in that it was *caused by* that other, only in that it was *because* the new meaning of a word is related to an old meaning that the word comes to have that new meaning. This austere "code-model" cannot "overgenerate" because it does not generate at all (in the way specified).

11.4.4 Proliferation

The second Falkum objection to be considered concerns the *proliferation* of senses (2015: 84). Recanati makes this objection too (2004: 134). Falkum puts it as follows in her survey with Vicente:

> First, many words have a large number of different senses. Postulating that the full range of senses for each word is stored entails a (potentially) indefinite proliferation of mentally stored senses in order to cover the range of uses of words… this place an enormous demand on the storage capacity of the language user… (2015: 3)

Set aside for a moment the talk of the proliferation being "indefinite". Then there can surely be no serious objection to the idea that the mind could store "a pre-established list of discrete senses for 'get'", to take Recanati's example, (2004: 134), even if that list is long. The mind is, after all, capable of much more impressive feats, like speaking many languages and learning the Bible. So if there is a problem here, it must arise from the possibility of *indefinite* proliferation. Recanati takes the problem to be that "modulated senses result from a process of 'generation' or 'creation' rather than selection…generation is productive…words can take on an indefinite variety of possible senses" (2004: 134; see also 2010: 18; 2017: 384). But the creative use of expressions is *not* at odds with SEM and the tradition. At any time, there is indeed a (somewhat indeterminate; Sect. 5.2) list of discrete senses for an expression. *But the list can change*. An expression can have a speaker meaning other than its conventional meaning – in a metaphor, for example. And, in time, such a speaker meaning can become a new conventional meaning of the expression. Indeed, this is the story of polysemy (as Recanati notes: 2010: 70).[32] The tradition does not suppose that languages never change!

Falkum and Vicente conclude their proliferation objection with the claim that a view like SEM

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[32] Relatedly, it is not the case "that the proliferation of polysemy appears to have a stronger motivation on the pragmatic inferential account, where it arises as a natural consequence of lexical meanings not being able to fully encode speaker intended senses" (Falkum 2015: 97).

> fails to distinguish between those aspects of meaning that are part of the word meaning proper and those that result from its interaction with the context, a problem sometimes referred to as the 'polysemy fallacy' (Sandra 1998). (2015: 3)

This is not so. A fundamental part of the argument for SEM has been to provide a principled basis for distinguishing the semantic from the pragmatic properties of an utterance (Chap. 3).

11.4.5 Eternal Sentences

The third Falkum objection concerns *eternal sentences*. Citing Robyn Carston (2002: 29), Falkum ties SEM to a certain view of "linguistic underdeterminacy" according to which "it is a form of 'convenient abbreviation'" and not "essential":

> it is a matter of effort-saving convenience for the hearer…Although sentence meaning more often than not underdetermines the proposition expressed by it, a sentence that fully encodes the speaker's meaning could in principle be supplied. (2015: 94)

As she notes, "this lies close to the view…that every proposition expressed by a natural language sentence [is] describable in terms of a context-independent 'eternal' sentence. (p. 94n). Now, I do indeed think that the underdeterminacy in question is often a matter of convenient abbreviation but it is no part of SEM that the dependence of reference fixing on context can be eliminated.

11.4.6 Copredication

I turn finally to an interesting objection made by Pustejovsky. He notes that SEL (Sense Enumeration Lexicon) is "the most direct way to account for" the many polysemies he describes. It "allow[s] the lexicon to have multiple listings of words, each annotated with a separate meaning or lexical sense" (1995: 34). SEL includes an information-rich Katz-Fodor lexicon but is otherwise like my version of SEM (applied to polysemy). Pustejovsky's objection to SEL is a central motivation for his own proposal, including the idea of "dot objects".

The objection arises from the phenomenon of "copredication", mentioned in Sect. 8.3 as an "interesting difference" between polysemy and homonymy. John Collins explains copredication nicely as follow:

> Copredication occurs where two or more predicates (verbs, adjectives) take a single argument (or modify a head nominal) that is differentially construed relative to each predicate, but the host construction is acceptable, non-zeugmatic, and so truth-apt. (2017: 678)

Now, as Collins points out, failure of copredication for the homonymous *bank* "indicates that *bank*, as we knew, is ambiguous" (p. 678). But this does not, of course, show that success of copredication shows the *absence* of ambiguity.

In our discussion of regular polysemy (Sect. 11.2.3), we noted that words like 'window' can refer to a hole or to the glass that goes into the hole. But then consider:

(1) John crawled through the broken window. (Pustejovsky 1995: 48)

Here 'window' seems to have both senses at once, for what is crawled through is a hole and what is broken is the glass in the hole; one cannot crawl through the glass or break the hole. We have an example of copredication. 'Lunch' provides another example: it can refer to an event or to food, and yet both senses feature in:

(2) The lunch was delicious but took forever. (Vicente 2018: 955)[33]

There is a related anaphora phenomenon; for example, "The lunch was so delicious that it took forever".

How should we explain (1) and (2)? We can say simply that the displayed sentences containing them show that the one token of such a word can indeed express both of its conventional senses in the one sentence. So, they illustrate a phenomenon akin to pronouns of laziness and the "sloppy reading" of verb phrase anaphora in the following dialogue:

A: Mary loves her husband.
B: So does Jill (Neale 1997: 209)

(On the "sloppy reading" Jill is alleged to love her own husband not Mary's.) We might say that (2) is elliptical for:

The lunch-*qua*-food was delicious but the lunch-*qua*-event took forever.

But that is a clumsy way to get the message across. So it is not surprising that we have come up with a syntactic convention that enables us to get the message across by (2), exploiting both meanings of 'lunch' at once. All in all, it is hard to see why this simple explanation of (1) and (2) is not adequate. (And consider puns where the humor comes from both meanings being entertained at once.)

So, on this view, the two senses of the polysemous 'lunch' yield two entries in the lexicon, just as does the homonymous 'bank', but with 'lunch' but not 'bank' both senses can be exploited in the one use of the word.[34] What is Pustejovsky's objection? He criticizes such SEL "models of lexical knowledge" for their "inability to adequately express the logical relation between senses" (1995: 48) that is

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[33] In this discussion of Pustejovsky, Vicente also offers the example: "That heavy book is real fun". 'Book' is indeed often thought to provide examples of copredication but, if am right that the only ambiguity with 'book' is a type-token one (Sect. 11.2.3), Vicente's example is not a copredication. The reference in this utterance is to *the one entity* that is both heavy and real fun. Similarly, Collins' example, "London tends to vote Conservative, despite being the largest urban area in the U.K." (2017: 680), is not an example of copredication. The reference of 'London' is to *the one entity* that has some properties in virtue of its people and others in virtue of its location.

[34] But this does not seem to be true of all polysemes. Consider, for example, "The ham sandwich left without paying and I have eaten it" (Penco and Vignolo 2019: 379). Yet the following seems to be in order: "The ham sandwich is angry because the kitchen is taking so long to make it"? Whatever linguists discover about these interesting syntactic matters, 'window' and 'ham sandwich' are still polysemes.

characteristic of this sort of regular polysemy. What the models are alleged to miss is that "words such as…*lunch*…'denote' contradictory types…the senses of the nominals are related to each other in a specific and nonarbitrary way" (1998: 298). In response, I call again on my "crucial qualification" to Falkum's account of "code-based" views of which SEL and SEM are examples. There should be no call for an "information-rich" lexicon to "express" that relation between the senses of 'lunch'. The lexicon makes its contribution to the explanation of that relation by determining that in one sense 'lunch' refers to lunches-*qua*-events and in the other, to lunches-*qua*-food. Then a worldly relation completes the explanation of the relation between those senses. For, it is a worldly fact that lunches-*qua*-events consist in consuming lunches-*qua*-food. The lexicon's role in the explanation is to provide the minimal referential facts, that's all. *Facts about lunches should not be turned into facts about the meanings of 'lunch'*; c.f. the "first mistake" of lexical semantics (Sect. 11.3.4).[35]

Pustejovsky own handling of these polysemous phenomena involves his curious idea of a "dot object":

> The apparently contradictory nature of the two senses for each pair actually reveals a deeper structure relating these senses, called a *complex type* (or *dot object*…). For each sense pair, there is a relation that "connects" the senses in a well-defined way….This relation must be seen as part of the definition of the semantics for the dot object. Intuitively, the dot object can be thought of as an abstraction of the relation between the types, where the types are abstracted together (1998: 298)

So, on this view, there is "a single lexical representation" in dot-object cases into which are "pack[ed] many meanings" (1995: 224–5). And nominals like 'window' "are logically ambiguous…referring to either the object or to the aperture, or to both" (1995: 91). This is an example of the "overspecification" view described earlier (Sect. 11.3.1): "the meaning of a polysemous term includes all its different senses, which are stored in a single representation. Senses are selections of the total meaning of the word" (Vicente 2018: 952). Language users then "have to select only a part of or an aspect of the whole informational content provided by lexical meanings" (Falkum and Vicente 2015: 7).

Now, for the reason indicated, I don't buy this alleged example of unification under a single representation any more than I bought the earlier ones arising from regular polysemy and co-composition (Sect. 11.3.3). But, it needs emphasizing, this

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[35] I have a similar response to the central assumption in Esther Romero and Belén Soria's (2019) interesting criticisms. They assume that "the semantic frame of 'waiting'" demands a "sentient participant" as subject, that of 'telling', a "sentient speaker", and so on (p. 405). I don't see a sound theoretical basis for this (nor for Nicholas Asher's similar view, based on intuitions about "category mistakes" and "semantic anomalies", that certain predicates require certain types of arguments (2011: 4–6)). In my view, the oddity of "the rock…responds" in their example (p. 415) comes from the way the world is not from the rules of English. They think that I owe "some principled way to accept the demand of the provision of a location in the frame of 'raining' [see 10.3] and reject the demand of a sentient participant in the frame of 'waiting'" (405). But I don't, because I don't put any demand on what could be a "location". So far as the conventions of English are concerned it could be anything. Facts about the world make it odd to say, "It is raining", meaning, say, that it's raining in my toaster just as they do to say, "the rock responds".

sort of unification by overspecification is importantly different from the one that yields a version of PRAG. On the PRAG unification, a polysemous word has a meaning *M* in virtue of a pragmatic modification. This entails that the lexicon does not include a distinct entry pairing the word with *M*. So there could be a single lexical entry pairing the word with some sense other than *M* from which many senses, including *M*, are derived by pragmatic modification. In contrast, on the alleged overspecification unification, exemplified by dot objects, regular polysemy, and co-composition, the pairing of a word with a sense is by a linguistic rule not a pragmatic modification; it is a language-internal *semantic* generation of a meaning. So this unification is in accord with SEM (though not, of course, a version of it I embrace).

This concludes my response to some objections to SEM that have arisen outside philosophy, particularly in psycholinguistics. I shall now turn to the experimental evidence. In the next section, I shall outline the experimental evidence PRAG needs and, in Sect. 11.6, I shall assess SEM and PRAG in the light of the experimental evidence we have.

11.5   The Onus on Pragmatic Polysemy (2)

Issues of the onus on PRAG, and the PE-explanations that it urges, have been raised several time. In Sect. 9.4 the concern was with the handling of definite descriptions. In Sect. 10.4, it was with the handling of saturation. But an onus issue was first raised in Sect. 8.4 when the concern was with the handling of polysemy. I now return to that issue. For, the experimental evidence that PRAG needs is what this onus demands.

Those earlier discussions emphasized the psychological-reality requirement on PRAG. For pragmatic explanations (PEp) to be adequate, they must place the pragmatic derivation of one meaning from another actively within the cognitive lives of speakers and hearers. Here is how this works out in the handling of polysemy.

If *E*, having a conventional meaning *M1*, is regularly used with a meaning *M2* and this is to be handled pragmatically and not as a case of ambiguity, then there have to be psychological processes in speakers and hearers that are appropriately different from the standard convention-exploiting ones, whatever they may be, involved in the use of ambiguous terms. In hearers there have to be regular processes that differ from selecting *E* meaning *M2* from the lexicon in understanding the message as what-is-said, a WIS-meaning, processes that differ from that of participating in a convention for *E*: there have to be regular *mind-reading processes of detecting pragmatic modifications in order to infer from the utterance that E, in the circumstance, means M2 not its conventional meaning M1, and hence to infer the message, a PM-meaning, from what-is-said, from a WIS-meaning*. In speakers there have to be regular processes that differ from expressing a concept (part of a thought) with the content *M2* by selecting *E* meaning *M2* from the lexicon to yield a WIS-meaning: there have to be *partly mind-reading processes of selecting E to express*

that concept, even though E means M1 not M2 in the lexicon, partly because the speaker expects that the hearer will go through the above mind-reading process. These processes could be conscious in speakers and hearers but since they clearly are not in the cases that concern us, they have to be subconscious. And the problem for PRAG is finding any evidence that there are such subconscious processes.

Evidence that either speaker or hearer meets this requirement would be evidence for PRAG. Given how difficult it is to find out about psychological processes in general, we should expect problems in getting evidence of these non-convention-exploiting processes even where they exist. And, I emphasize, evidence of these processes in a speaker is particularly important, for two reasons. First, because what the speaker does would *constitute* any implied meaning (Sect. 7.1). Second, evidence of the required non-convention-exploiting process should be easier to find in the speaker than in the hearer. For, in the hearer it is likely to be hard to distinguish the required mind-reading process of detecting modifications from the mind-reading process of disambiguation. I take Leon Li and Robert Slevc to be getting at something like this problem:

> Given that the comprehension of a word as it appears in any sense entails a simultaneous activation of the word's other, semantically unrelated senses, it may be in principle not possible to discern, using comprehension-based measures, whether a word's multiple senses are truly unified in representation, or whether the word's multiple senses are separate but consistently co-activated. (2016, p. 4)

There is no such problem in the speaker because there is no analogue of the hearer's mind-reading process of disambiguation: the concept a speaker is intentionally expressing selects *E* meaning *M2* from the lexicon and *thereby* disambiguates *E*. The contrast between that "simple" convention-exploiting process and a speaker's partly mind-reading one that involves an expectation about the *hearer's* mind-reading should be stark. Yet, as Li and Slevc remark, "most cognitive research on polysemes has focused on comprehension, so little is known about the representations of polysemous words in the production system" (2016: 4). They themselves focus on production but cite no examples of *any* previous researchers doing so. From the perspective I have been presenting, the research has had the wrong focus.

I shall argue that what has been discovered about processes in speakers and hearers is far from sufficient to meet the psychological-reality requirement. This is very bad news for the PRAG idea that polysemies are typically pragmatic. For, in the absence of independent evidence for the processes PRAG requires, there are powerful Occamist and Developmental objections to it. These objections were made in Sect. 8.4 and analogous ones were made in Sect. 10.4 to the idea that *saturations* are typically pragmatic.

Occamist Objection We already know that there must be the largely subconscious convention-exploiting processes of disambiguation in speakers and hearers, even if we do not know much about the details (despite the ingenious efforts of psycholinguists). For, those processes are the standard ones for handling homonyms. So we already know that there must be the sort of processes required by SEp, the semantic rival to PEp. This is an important part of our background knowl-

edge in assessing SEp and PEp. SEp is committed to mechanisms we already know to exist, even though we are short on the details. *If we really do lack independent evidence of the subconscious processes that PEp requires, then we should prefer SEp over PEp on Occamist grounds.*

As I noted (Sect. 8.4) in response to Robyn Carston (2021), the Occamist objection arises because the processes that PEp requires with the regular uses of *E* to mean *M2* are clearly not conscious. We all believe in the existence of the processes required for a pragmatic explanation of Carston's "novel ad hoc uses" of *E* to mean *M2* because we are conscious of them. But we are typically not aware of any pragmatic mind-reading processes in regular uses.

Developmental Objection   In an influential article that I shall discuss in Sect. 11.6.2, two psycholinguists, Devorah Klein and Gregory Murphy, make a pertinent remark. They are addressing what is, in effect, the abstract-core version of PRAG that we have described before (Sects. 8.4, 11.3.1): a polysemous word has just one abstract conventional meaning from which its other senses are pragmatically derived. They point out that

> the sole representation of a core meaning seems psychologically implausible if specific senses are frequent. That is, it would be very surprising if people often used the word *paper* to refer to newspapers and to published articles, yet did not represent this fact in the lexicon, but derived it from general principles on every occurrence. (2001: 276–7)

It would indeed be very surprising. As I noted in Sect. 8.4, attention to the formation of conventions for monosemous and homonymous words, indeed for meeting at O'Reilly's, rowing, and countless other activities, makes it a priori plausible that polysemes have their meanings by convention and so make it implausible that there are the subconscious processes that PEp requires. When we are faced with a co-ordination task, as we are in communication, conventions pay off: *conventions enable us to avoid the demanding mind-reading that brought them about.* The benefit we get from having a language is a dramatic example. In sum, PEp, hence PRAG, *really* needs evidence of the subconscious processes for it is a priori so unlikely that they exist.

To illustrate the bearing of this discussion on SEM-PRAG, consider Falkum's paper (2015). As noted, her purpose is not only to criticize SEM but to argue for a relevance-theoretic version of PRAG. She offers two "prime examples of pragmatic processes on the relevance-theoretic account":

(20) John is a lion.
(21) The ham sandwich is getting impatient. (2015: 92)

((20) is our Pragmatist example (6) in Sect. 1.1.) 'Lion' has an encoded meaning that refers to a charismatic feline but it is used in (20) to convey the message that John is "strong, courageous, take risks, etc" (p. 92). 'The ham sandwich' has an encoded meaning that refers to a piece of food but is used in (21) to convey a message about "the person who ordered a ham sandwich" (p. 93). Falkum gives a nice account of how a hearer might infer these intended meanings from the stated encoded ones and hence of how the PEp explanation might go (pp. 92–3). But what

about the fact that these expressions are *regularly* used with those intended meanings? As Falkum remarks about 'the ham sandwich':

> the development of a convention of referring to customers via their food orders among the employees of a café, provides an important motivation for many rule-based analyses of the phenomenon. From a relevance-theoretic pragmatic point of view, this can be seen as cases where a repeated use of a linguistic metonymy that links different concepts together has set up a pattern of conceptual activation, or a "pragmatic routine", which gives rise to a sense of regularity… (p. 93)

Earlier, in discussing another example, she has described this process of "initial uses" becoming

> stabilised or conventional over time within a language community as a result of frequent adjustment of the lexical meaning of the word in a specific direction. In such a case, the construction of the ad hoc concept may become progressively more routinised, and a 'pragmatic routine' or inferential shortcut develop …Such routinised inference patterns might be useful procedures in comprehension, by increasing the accessibility of certain interpretations and thereby contributing to a reduction of hearers' processing effort and thus to the overall relevance of the utterance. However, rather than being part of the linguistic system, these inferential short-cuts have a pragmatic basis and can easily be cancelled out by contextual information (linguistic or otherwise) pointing to a different interpretation. (92)

All this prompts several comments. First, briefly but importantly, there is no mention in Falkum's discussion of the processes in speakers that in fact determine the nature of meanings. Second, provided that the "pragmatic routine" still exists, even if only as "inferential short-cuts", then Falkum has described a case covered by PEp. But, as noted in discussing the role of "streamlining" in Bach's account of "standardization" (Sect. 8.6), *we need evidence that the routine does still exist*. Third, absent that evidence, and supposing that our "sense of regularity" reflects a genuine regularity, there is no sound basis for denying that the term with the intended meaning *has* become part of "the linguistic system"; thus, we should assume that the use of 'the ham sandwich' to refer to a customer has become conventional in the language of the café employees.[36] Such a term has become ambiguous in their idiolects and is covered by SEp. So when an employee uses 'the ham sandwich' to refer to a customer, she simply participates in one semantic convention for the expression, without any pragmatic modification of meaning. And, when another employee responds to that use, he simply goes through the standard process of disambiguation not the inferential process of figuring out a modification.[37] Fourth, there is indeed "a pragmatic basis" for a word's gaining a new linguistic meaning in this way, thus

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[36] This is not to say, of course, that it has become conventional in the idiolects of the community at large. This leads Penco and Vignolo to call such conventions "'restricted' or 'weak'" (2019: 380). But this is not apt given that conventions are *essentially* conventions in a group and so they are *essentially* restricted (Sect. 5.2). Penco and Vignolo suggest that Bach's notion of "standardization", that I discussed in Sect. 8.6, can be explained and motivated in terms of these "weak" convention (pp. 380–81). I don't think so. Indeed, my line of argument against Falkum here is similar to that aimed at Bach's use of standardization.

[37] "But if 'ham sandwich' has really become a conventional way of referring to customers who order ham sandwiches, why is 'Ham sandwiches rarely wear ties' anomalous?" It doesn't strike me

becoming a new lexical entry. For, the new meaning was created by regular pragmatic modifications by speakers, as probably just about all new meanings are (Sect. 5.2). But this *creation* story does not undermine the *nature* story: the meaning *is now* linguistically encoded, whatever the story of its origin. Fifth, contextual information can indeed lead to a hearer's interpretation of a pragmatically modified meaning being "cancelled out". But it can also lead to her choice of one linguistic meaning rather than another of an ambiguous term being "cancelled out". Cancellability is no help in choosing between SEp and PEp (Devitt 2007a: 12).

Falkum's argument is for an account of polysemy where the alleged pragmatic routine derives a meaning from a conventional meaning that a word indubitably has and that is quite "*un*-abstract": from 'lion' meaning a certain feline and from 'the ham sandwich' meaning a certain food item. But a popular version of PRAG, that I called "abstract-core" and that psycholinguists have partly in mind by "underspecification" (Sects. 8.4, 11.3.1), does not have this encouraging feature. According to that version the pragmatic derivations are from an *abstract, underspecified, core meaning*. As I pointed out (Sect. 11.2.5), this may seem appealing for the likes of 'cut' and 'bear' but it is not for the likes of 'foot' and 'suit'; and not, we might add, for the likes of 'lion' and 'the ham sandwich'. In any case, the abstract-core version of PRAG is particularly problematic.

Abstract-Core Objection There are three reasons to doubt that words have such abstract meanings. First, as Frisson points out, "some linguists have questioned the existence of abstract representations as it is often impossible to find a definition that covers all possible senses of a word" (Frisson 2009: 121). The heroic failure of Charles Ruhl to find such a "definition" for 'bear' is salutary: he concludes his 29-page discussion, "So what does *bear* mean?...this question cannot be answered in words" (1989: 63).[38] And note this nice point:

> polysemous senses that belong to a regular pattern of polysemy do not share features that could build a core meaning, a summary representation, or an underspecific representation that covers all of them. (Vicente 2018: 959)

But suppose that we do come up with a needed description of an abstract meaning. A likely example might be the one we gave earlier for 'cut' (Sect. 11.2.5): its abstract core meaning picks out actions that "produce linear separation in the material integrity of something by a sharp edge coming in contact with it". Then according to this version of PRAG, this meaning of 'cut' is modified in context to pick out more precise actions: grass-cutting; cake-cutting; and so on. This brings us to the second reason for doubt. That description for 'cut' shows that *we theorists* may be

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as any more anomalous in the employee's language than 'People who order ham sandwiches rarely wear ties'. (Thanks to Kent Bach for this nice example.)

[38] This exemplifies what Cruse sees as a major shortcoming of Ruhl's account: "he does not at any point spell out what the meaning of a primary word is like, or how it is to be represented: it cannot be entertained consciously, it cannot be paraphrased, it has a totally mysterious essence." (1992: 591)

able to abstract a common *part* to the meanings of this polyseme. But this does not show, what this version of PRAG requires, that this part is *the*, or even *a*, conventional linguistic meaning of 'cut'. It does not show that there is a psychologically real entry in the lexicon of speakers that pairs 'cut' with just that meaning. Persuasive evidence of this *would* be provided, as noted (Sect. 11.2.5), by the regular use of 'cut' with just that speaker meaning (Sect. 3.6). But, of course, there is no such evidence. Indeed, I predict that speakers near-enough always use 'cut' to refer to a precise action not the abstractly described one. Similarly, 'bear', and many other examples. If these predictions are right, that would provide quite persuasive evidence that these words do not have the required abstract meaning. For, if a word had the abstract meaning why would speakers not be using it regularly to mean that? Co-composition is an answer, but I have rejected that (Sect. 11.2.5).

Third, not only is it not the case that the words *are* regularly used with an abstract meaning, it is not plausible to suppose that they *ever were*. Thus it is not plausible to suppose that 'cut' was ever used to pick out the vague action described above. Rather it seems likely that it was always used for one or other of the precise actions. This is important because, absent those past regularities, the very existence of these abstract meanings is inexplicable. Where could they come from but from regularities? Consider this plausible story for 'dog'. "In the beginning", it was regularly used to refer to members of a species of *Canis*, creating a convention of so using it. This far-from-abstract meaning led to 'dog' being regularly used also with a more precise meaning to refer to just the males. The problem for PRAG is that the analogous creation story for words like 'cut' that are alleged to have abstract meanings is not plausible. At least we need evidence that these words were once regularly used with those meanings.

Now, of course, these doubts that words have such abstract meanings could be allayed if we could find persuasive psycholinguistic evidence that language users, in processing the likes of 'cut' and 'bear', really do go through the non-convention-exploiting processes involving those meanings that PRAG requires: that, in the production of such words, speakers really do enrich an abstract meaning in the expectation that hearers will go through the mind-reading process of detecting this modification; and that, in understanding, hearers really do go through that mind-reading process. But we shall find no such persuasive evidence.

In the next section we shall turn to psycholinguists to see if it provides the evidence that PEp and PRAG need of the specified non-convention-exploiting processes. But before that, let us sum up the importance to PEp and PRAG of this evidence. Without it, PEp will not qualify as even a *good* explanation of the regular uses of polysemous words, let alone *the best* that would sustain an abduction. For PEp to be good, it needs to be plausible relative to background knowledge (Sect. 3.6). Absent the evidence, we have no reason to believe in the subconscious processes that it posits: the Occamist Objection. Worse, our background knowledge makes it unlikely that those processes exist: the Developmental Objection. Finally, the popular abstract-core version of PRAG is committed to abstract meanings that we have reasons to doubt: the Abstract-Core Objection. In contrast, without the evidence that PEp needs, SEp will be not just good and better than its *actual* alternative, PEp, but better than any alternative that is

likely, given what we already know about monosemous and homonymous words, and about the origins of language in general: the Developmental Objection again. So SEp will meet the criteria for a good abduction (Sect. 3.6).

So much for the experimental evidence PEp and PRAG need. I turn now to the experimental evidence we have.

11.6   Psycholinguistics and the Experimental Evidence

11.6.1   *Introduction*

In considering this evidence, we need to keep in mind the main takeaways from our discussion of the somewhat complicated relations between psycholinguistic positions and SEM-PRAG:

1. The multiple-entry view is consistent with the version of SEM that I am urging (= SEL without commitment to "high-key" representations of "rich" meanings) but is inconsistent with underspecification, overspecification, and of course PRAG.
2. PRAG, underspecification, and overspecification each entail the single-entry view.
3. Underspecification seems to require PRAG, but maybe not.

In their opening remarks, Klein and Murphy (2001) sum up the state of the evidence at that time as follows: "there is very little experimental evidence to support either the core or a multiple-sense theory" (p. 262). Since then, experimental evidence has been thought to show quite a lot about the various psycholinguistic positions on polysemy. Thus Falkum and Vicente report: "The lack of priming effects found for the senses of their [Klein and Murphy (2001)] polysemous words led them to conclude that these were represented in the same way as homonyms" (2015: 4). This was thought to support SEL, the sense enumeration view, which is a "high-key" information-rich version of SEM (applied to polysemy). But Vicente reports, citing MacGregor et al. (2015) in particular, "recent psycholinguistic studies question this view [SEL]" (2018: 958). "The psycholinguistic evidence related to the lack of frequency effects and to co-priming constitutes evidence against…sense enumeration" (p. 960). And, as noted earlier (Sect. 11.3.1), "recent psycholinguistics tends to favor underspecification and overspecification approaches" (p. 952).

It is obvious that we should look for the evidence PRAG needs by comparing the processing of polysemes with that of homonyms. Homonyms are paradigms of ambiguity and so, if polysemes are processed differently from homonyms *in the relevant respect*, then we have evidence against SEM. And the evidential problem lies in the demand that the difference be *relevant*. For, it is no part of SEM that polysemes are processed in production and understanding just like homonyms. Indeed, since the senses of a polyseme, unlike those of a homonym, are related, we already

know that the polyseme's lexical entries are *causally* related. For, the polyseme came to have its related senses *because its having one sense partly caused it to have others*; 'suit' came to mean an executive *because* it meant what an executive wears. The lexical entries for a polyseme were causally related from the start; there is an "association of ideas". We might expect this to affect processing. And, given that the way the senses of metaphors are related to each other differs from the way the senses of metonyms are, we might expect processing differences among polysemes, as indeed there seem to be (Klepousniotou and Baum 2007; Bambini et al. 2013). Finally, suppose that we had evidence that a hearer in interpreting a polyseme to mean *M2* always first "accesses" meaning *M1*. This would be a surprise but it would not count decisively against SEM. For, though that process might indeed be the hearer's detection of a pragmatic modification, hence PEp, it might not be: it might be just her causal route to the lexical item for *M2*, hence SEp.

The only processing differences between homonyms and polysemes that are relevant to the SEM-PRAG disagreement are the ones demanded by the psychological-reality requirement on PEp: if a speaker's use of the polysemous *E* means *M* not simply in virtue of her selection of *E* from her lexicon but in virtue of her modification of its lexical meaning, then its meaning *M is* pragmatic and *E* is not (in this respect) ambiguous: if a hearer understands a use of *E* to mean *M* not simply in virtue of his selection of *E* from his lexicon but in virtue of his taking its lexical meaning to have been modified, then that is *evidence* that its meaning *M* is pragmatic and *E* is not ambiguous.

The problem for PRAG is that though the psycholinguistic experiments provide evidence of all sorts of interesting processing differences, they do not provide evidence of these *relevant* processing differences. And that is evidence that PRAG so badly needs, given that it looks so unpromising in the face of the Occamist, Developmental, and Abstract-Core Objections. This lack of evidence is no surprise because it is very difficult to get evidence of such fine-grained mental processes. The experimental results are not at odds with any firm prediction of either SEM or PRAG.

Obviously I cannot discuss *all* the experiments to show that they really do not bear on the SEM-PRAG issue. I will consider in some detail the experiments presented in four papers, hoping that the argument can be seen to generalize. The three papers discussed first are on comprehension, the one discussed last, on production.

11.6.2 Klein and Murphy (2001)

Klein and Murphy describe their aim as follows: "Five experiments investigated whether different senses are represented distinctly in the lexicon or if there is a common, core meaning" 2001, p. 259). They conclude: "These experiments provide evidence that polysemous words have separate representations for each sense and that any core meaning is minimal" (p. 259). So they are alleged to provide evidence for the multiple sense view, which looks like good news for SEL and my version of SEM, a "low-key" "poor" SEL. But the evidence is not convincing.

Experiment 1 is a word recollection study. In the learning phase subjects see a polysemous word like 'paper' which can mean a newspaper or the kind of stuff used for many purposes including newspapers. In the test phase subjects are presented with the same word and asked if they had previously seen it. Sometimes that later word has the same sense as the earlier one, sometimes, a different sense. The senses are indicated by such modifiers as 'liberal' and 'shredded', Klein and Murphy think that the multiple-entry view, for example SEL, makes the following prediction: "If polysemous words are represented with separate sense representations, memory should be better if a word is used in the same sense than if it is in a different sense" (p. 264). So SEL is supposed to predict that the results for polysemous words will be similar to those for homonymous words, as revealed in previous experiments. And that is what they found, seemingly confirming SEL. But why would SEL make that prediction rather than the opposite one? Because the senses of polysemes are related, unlike those of homonyms, memory will be just as good when the word in the test phase is different from that in the learning phase as when it is the same. In brief, SEL (hence my version of SEM) makes no clear prediction here.

The prediction problem with the single-entry view is more serious. Klein and Murphy's prediction is: "If the core meaning view is correct, there should be no such difference, as subjects will access the single sense of a polysemous word every time it is encountered" (p. 264). Yet, as noted, they discovered a difference. But the prediction is dubious. On the single-entry core-meaning view, subjects in the learning phase arrive at an interpretation, say *newspaper*, by a pragmatic or linguistic derivation from a core meaning. In the test phase, they arrive at *newspaper* if the sense of the word is the same, *or* say *paper stuff* if the sense is different, again a derivation from a core meaning. (One wonders what that core meaning could be, of course; see Abstract-Core Objection in Sect. 11.5.) Now, on the core-meaning view, both the *newspaper*-process and the *stuff*-process do indeed require derivation *from the one core meaning*. But why would the view not predict that the differences *in those processes of derivation* might make a difference to memory? In one test phase, the subject is interpreting a word by a process that is *much the same* as in the learning phase, including the same sort of clues, for example, 'shredded' and 'wrapping'. In the other test phase, the subject is interpreting a word by a process that is *very different* from in the learning phase, including different sorts of clues, for example, 'wrapping' and 'liberal'. Why wouldn't the single-entry core-meaning view predict that it will be easier to remember the word in the former same-processing case than in the latter different-processing case? In sum, the results do not count against the single-entry view. So they do not count against PRAG.

Experiment 2 tested reaction time and accuracy on a sensicality test:

> the task was to make a sense/nonsense judgment on phrases similar to those used in the first memory experiment….The question, then, is whether the difficulty in deciding the sensicality of *liberal paper* depends on the consistency in sense of the prior use of *paper*. … Again, if there is only a core meaning, consistency should not make a difference, since all phrases would require access to the core concept of *paper*. (p. 266)

Yet, once again, they discovered a difference. And, once again, there is a problem with the prediction. On the core-meaning view, discovering the sensicality of 'liberal paper' requires a derivation from the core meaning. Why predict that it would make no difference to reaction times and accuracy whether that derivation in the test phase was much the same as, or quite different from, that in the learning phase? Again, the results do not count against the single-entry view, hence against PRAG.

There is a strong "consistency effect" with homonyms: performance in the test phase is much affected by whether the word has a sense that is the same as or different from that in the learning phase. Experiment 3 compared the consistency effects of homonyms with that of polysemes in a sensicality test.

> One assumption was that homonyms and polysemous words would differ in the size of the consistency effect, with consistency being stronger for homonyms, due to the completely separate representations. Surprisingly, this was not found…There was no difference overall between the homonyms and polysemous words. (p. 269)

They were so surprised that they replicated in Experiment 3A with the same result: "no important difference between the homonym and polysemy conditions" (p. 269). These results are perhaps a bit surprising from the SEL perspective. Still one wonders about the basis for supposing that the relation between polysemous senses would lessen the consistency effect. In any case, of course, the results do not count against SEL: the more polysemes are like homonyms in processing the better for SEL. Do the results count against single-entry view (and hence PRAG)? Klein and Murphy think so: "The stronger such an effect, the less significant a core meaning could be" (p. 268). "Such results rule out not only a full single-sense view but also any separate representation account in which a core meaning plays a significant role" (p. 270). Maybe the results do show that a core meaning could not be playing a "significant" role in explaining the consistency effect and so maybe count a bit against the single-entry view. But I see nothing decisive here. That view can explain the strong consistency effect for polysemes by pointing simply to the very different derivations involved in arriving at the different senses; see discussion of Experiment 2. It is hard to see a firm basis for predicting that this *inferential* difference would not yield a consistency effect similar to that yielded by the *sense* differences of a homonym.

Experiment 4 was a control. Experiment 5, another sensicality test, "was conducted to examine whether the priming of polysemous word senses is due to inhibitory or facilitory processes" (p. 272). Klein and Murphy assume that

> if senses inhibit one another, they must have separate representations. The newspaper sense of *paper* cannot be suppressed while the sheets of material sense is spared unless the two senses are functionally distinct. (p. 273)

Why? Why couldn't the derivation of a modified sense *paper-stuff* from 'wrapping paper' suppress a derivation of a modified sense *newspaper* from 'liberal paper' but spare a derivation of a modified sense *paper-stuff* from 'shredded paper'? I don't see the basis for Klein and Murphy's assumption. Hence, I doubt their conclusion:

> These results give even stronger evidence about the separate representation of polysemous senses. Clearly, the notion that the word has a single core meaning that is the basis for every use cannot accommodate such results. (p. 275)

In sum, Klein and Murphy's results do give some comfort to the multiple-sense view, hence to SEL and my version of SEM, but not much because, for all we know about the workings of the mind, they are quite consistent with the single-sense view and PRAG.

11.6.3   MacGregor et al. (2015)

Lucy MacGregor, Jennifer Bouwsema, and Ekaterini Klepousniotou (2015) conducted experiments which they see as pushing us in the opposite direction. They found some interesting processing differences between polysemes and homonyms which they take to provide evidence for a single-entry view, hence against SEL and my version of SEM. As noted, Vicente agrees (2018: 958). Once again, I'm skeptical.

The experiment tested the priming effect of ambiguous words using EEG. The words were of four types:

> (1) unbalanced homonymous words (e.g., "coach"); (2) balanced homonymous words (e.g., "match"); (3) metaphorically polysemous words (e.g., "mouth"); and (4) metonymically polysemous words (e.g., "rabbit"). (2015: 130)

These primes were paired with three types of targets:

> (1) words related to the dominant meaning of the prime; (2) words related to the subordinate meaning of the prime; (3) control words unrelated to the prime. (p. 130)

The experiment was a lexical decision task: having been given the prime, participants had to decide whether or not a target word "was a real word in English" (p. 131). The results showed a striking difference in the priming effect of polysemes and homonyms:

> semantic priming effects, as indexed by a reduction in the amplitude of the N400 brain response relative to unrelated targets, were observed for target words related to both meanings of polysemous prime words (both metaphors and metonyms) but not for targets related to homonymous prime words (both balanced and unbalanced). (p. 135)

They conclude:

> The observed processing differences between homonymy and polysemy provide evidence for differential neuro-cognitive representations for the two types of ambiguity, … Sustained activation of both meanings of polysemous words supports an account of representation in which the multiple senses are stored together. (pp. 136–7)

So MacGregor et al. take their results to support a single-entry view. They then cautiously entertain both the underspecification and overspecification version of this (p. 137).

But do the results really support the single-entry view? I think not. They seem quite compatible with the multiple-entry SEM view. The results show that the

meaning and causal relations that exist between lexical entries for polysemes but not homonyms affect processing. But, as already indicated, SEM has no problem with that (Sect. 11.6.1). The results do not count against my SEM view that the mind has a distinct lexical entry for each sense of a polyseme; that it has a disposition to associate the word with *M1*, another disposition to associate it with *M2*, and so on. And these dispositions are causally related.

More important to my purpose, these results certainly do not support, and were doubtless not intended to support, *the PRAG version* of the single-entry view in particular. On this version, derivations from the core meaning are by *pragmatic modifications* not by linguistic rules (like those for regular polysemies). As I have been emphasizing, PRAG badly needs evidence of these modifications: evidence that, in understanding the polyseme, a hearer does not simply select *M2* from her lexicon but rather infers a modification of its lexical meaning *M1*.

MacGregor et al. present persuasive evidence that "polysemous senses act collaboratively", "are complementary and can co-exist and co-activate quite easily" (p. 137). But this is not evidence that bears on SEM-PRAG.

11.6.4   Frisson (2015)

Steven Frisson describes his project as follows:

> The main focus of the present paper is how polysemous words are processed and what can be inferred from their processing profile with respect to their representation. (2015: 18)

His inferences from processing to representation, like the others we have considered, amount to inferences about meaning. And once again I think that the inferences are not justified.

Experiment 1 "was based on Klein and Murphy's (2001) sensicality judgement experiments" (p. 19) that we discussed in Sect. 11.6.2. Frisson

> wanted to investigate whether sense dominance played a role in the consistency effect…A large literature on homonym processing during reading has shown that subordinate meanings are in general more difficult to process than dominant meanings (p. 20)

This leads to the key prediction about the effect of dominance in the sensicality task for polysemes. Frisson claims that the multiple-entry SEL, hence in effect my SEM, predicts "that the subordinate sense should be harder to process than the dominant sense" (p. 22). But this was not what Frisson found: "whether the target expressed the dominant or subordinate sense did not affect reaction times" (p. 21).

Frisson wisely remarks earlier that "the underspecification view does not readily lend itself to specific predictions" (pp. 20–1). But he is not similarly cautious about predictions for SEL. According to SEL, polysemous words are like homonymous words in being ambiguous and so having a lexical entry for each sense. But, as we have been emphasizing, these entries for a polyseme differ from those for a homonym in both meaning relations and causal relations. We might expect that those

relations would be significant in processing, and we have discovered that they are. Why predict that the sense dominance that is significant for homonyms would also be significant for polysemes? Though the mind's path for the efficient processing of homonyms utilizes dominance, perhaps that for polysemes utilizes only those relations. Perhaps the fact that the meanings of a polyseme are related yields much better clues to its interpretation in a context than the crude fact that one meaning is dominant. I see no basis for predicting otherwise.

Experiment 2 was an eye movement study. Frisson concludes that this study also counts against SEL. But this conclusion rests on a similar prediction of the role of sense dominance in processing to the one I have just rejected. So I shall say not more about it.

Frisson adds to the evidence that polysemes are processed differently from homonyms and concludes that "it is unlikely that [they]…are represented in the same way" (p. 30). So he concludes that the multiple-entry SEL is unlikely and, by implication, a single-entry view is likely. It rather looks as if he has the PRAG version of that in mind. I don't think his results support these conclusions.

In sum, I have argued that the experiments on comprehension discussed do not support conclusions about whether polysemes have single or multiple lexical entries. So they do not support conclusions about PRAG or SEM/SEL. They do not because the theories in question make no firm predictions about such processing matters. In particular, there is no basis for supposing that SEM predicts anything at odds with these results. Most importantly, the experiments do not provide the evidence for PRAG demanded by the psychological-reality requirement, evidence that a hearer understands a polyseme by inferring a modification of meaning rather than by disambiguating.

But the evidence we most need, and which might be easier to find, is on production not comprehension (Sect. 11.5). I turn now to Li and Slevc (2016) who have made what seems to be the only attempt to find such evidence.

11.6.5   *Li and Slevc (2016)*

Li and Slevc (2016) test production using an "error elicitation paradigm". They claim that their results "suggest that polysemes, unlike homophones, share lexical (lemma-level) representations in the production lexicon" and are thus "unified". So they claim that their results suggest the "core-lexical" single-entry view (p. 15). I don't think so.

In Li and Slevc's experiment,

> speakers named pictures after reading sentence fragments that primed polysemes and homophones either as direct competitors to pictures (i.e., semantic-competitors), or as indirect-competitors to pictures (e.g., polysemous senses of semantic competitors, or homophonous meanings of semantic competitors). (abstract)

Thus, with homophones, a prime of a ballpoint pen is a direct competitor to a picture of a crayon, a prime of a pig pen, an indirect competitor. With polysemes, a prime

of printer paper is a direct competitor to a picture of cardboard, a prime of a term paper, an indirect competitor. Control primes are unrelated to what is pictured. Li and Slevc measured "how often the speakers accidentally produced the primed words (termed intrusions) instead of the picture names" (p 5).

Their key assumption is: "If polysemes' senses map onto separately stored lemmas, then the pattern of intrusions for primed polysemes should resemble the pattern of intrusions…for homophones" (p. 6). They did not find this resemblance:

> Polysemes (e.g., *paper*) elicited equal numbers of intrusions to picture names (e.g., *cardboard*) compared to in control conditions whether primed as direct competitors (*printer paper*) or as indirect-competitors (*term paper*). This contrasted with the finding that homophones (e.g., *pen*) elicited more intrusions to picture names (e.g., *crayon*) compared to in control conditions when primed as direct competitors (*ballpoint pen*) than when primed as indirect-competitors (*pig pen*). (abstract)

So with polysemes but not homophones, indirect-competitors are just as intrusive as direct competitors. Why the difference?

We should start with a definitive difference. With the polyseme 'paper', the meaning *term paper*, that is the indirect-competitor, is related to the meaning, *printer paper*, that is the direct competitor: term papers are (traditionally) written on printer paper (or the like). There is no such relation between the meanings of the homophone 'pen', *pig pen* and *ballpoint pen*. The multiple-entry SEM view then suggests a simple and modest explanation of the intrusion difference. With polysemes, the subject's activation of the indirect-competitor meaning *causes* the activation of its related direct-competitor meaning thus causing the intrusion; thought about a term paper stimulates thought about the paper on which it is written. There is no such process with homophones because their meanings are unrelated.

Late in their discussion, Li and Slevc entertain the "possibility" of something like this modest explanation (p. 14). But first they consider a much bolder one:

> The findings from this study are compatible with multiple core-representation theories of polysemy that were derived from comprehension-based evidence. If a polyseme is stored as an underspecified core-meaning that encompasses multiple senses (e.g., Frisson, 2009), then retrieval of a polyseme that is activated in any given sense may entail retrieval of an underspecified meaning that encompasses both the contextually relevant and contextually irrelevant senses. (p. 13)

The cautious "compatible with" and "may entail" are appropriate. The single-entry underspecification view *could* indeed explain the results but there is no evidence here to suggest that they *do* explain them. Most importantly, there is no evidence at all of the modifications of meanings by speakers that the PRAG version needs to fulfil its explanatory onus (Sect. 11.5).

Li and Slevc next consider a related (overspecification?) explanation:

> Alternately, if polysemes are stored as clusters of specified senses centered around generative core-meanings (e.g., Klepousniotou, 2002), then it is possible that the retrieval of any sense, especially the most generative sense, would pull the entire cluster along with it during lexical access. In short, lexical access in production may involve the retrieval of a polyseme "all at once," with both the senses that are relevant to the context and the contextually irrelevant senses being retrieved. (p. 13)

The idea of any sense pulling along the entire cluster of senses is just what our modest explanation proposes. But the cautiously proposed idea that speakers generate these senses from a core seems to be a gratuitous addition.

Li and Slevc give no reason for preferring either of their two bold explanations to the modest one they entertain as a "possibility".

In sum, the four experiments discussed do not support any conclusions about the nature of polysemy. I would argue the same of the other experiments in this literature. So, as predicted, psycholinguistic experiments do not throw significant light on the SEM-PRAG dispute. Most importantly, they do not provide the evidence of modified meanings that PRAG so badly needs in face of the Occamist, Developmental, and Abstract-Core Objections. So PRAG does not meet the psychological-reality requirement and SEM should be preferred.

11.7   Conclusion

According to SEM the regular use of a polysemous expression with a certain meaning is typically a semantic phenomenon not a pragmatic one; it is to be explained by SEp not PEp. Absent novel spur-of-the-moment modifications or implicatures, such a polyseme typically contributes one of its encoded meanings, selected in context, to the message of an utterance. I discussed many examples of polysemy, but there are of course many that I do not discuss. I hope that I treated enough examples to make it plausible that SEM can meet the Pragmatist challenge posed by polysemous phenomena (Sect. 11.2).

Why believe SEp applies to an example? Because SEp provides the best explanation of the regularity that the example illustrates, the regular use of a certain word with a certain speaker meaning.

I aimed to bring the psycholinguistic literature on polysemy to bear on the SEM-PRAG. This required some preliminary work discussing some expressions that feature prominently in that literature: "Sense Enumeration Lexicon"; "underspecification"; "overspecification"; "represented and stored"; "information-rich" (Sect. 11.3). I then defended SEM from objections, including some from psycholinguistics and one from the phenomenon of "copredication" (Sect. 11.4).

The contrary view of polysemous phenomena, PRAG, is that a pragmatic modification of meaning typically plays a role in constituting messages. This generates a "psychological-reality requirement": PEp, the PRAG explanation of a polysement phenomenon, is committed to there being regular subconscious psychological processes in speakers and hearers that are appropriately different from the standard convention-exploiting ones, whatever they may be, involved in the use of ambiguous terms. I offered Occamist, Developmental, and Abstract-Core Objections to PRAG, which place a heavy evidential burden on PEp (Sect. 11.5). I argued that the evidence on linguistic processes provided by psycholinguistics does not come close to fulfilling that burden. At this point there seems to be no evidence from the study

of the mind that counts significantly against the evidence from behavioral regularities for the SEM view of polysemy (Sect. 11.6).

In Chap. 3 I argued that the "semantic" properties that constitute "what is said" – the properties that symbols have in virtue of being uses of a language - arise from (i) convention, (ii) disambiguation, and (iii) linguistically demanded saturation (slot filling). I promised to defend SEM by showing that Pragmatist examples of context relativity, thought by them to challenge the tradition that they seek to overthrow, typically exemplify properties of sorts (i) to (iii). There are more of such properties than we have previously acknowledged: much more of the content of messages should be put into the convention-governed what-is-said – into semantics - than has been customary. I started the defense of SEM in Chap. 9 by considering definite descriptions. They illustrate previously unacknowledged semantic properties of all three sorts. But generally the context-relative phenomena of the Pragmatist challenge fall into two groups. In Chap. 10, I argued that one group can be accommodated within the tradition because they are of sorts (i) and (iii): they exemplify linguistic conventions of saturation in context. In the present Chapter, I have argued that the other group can be accommodated because they are of sorts (i) and (ii): they exemplify semantic polysemy that is disambiguated in context.

Finally, in the next chapter, I turn to sub-sententials, a special case of saturation in context. I shall argue that SEM applies to sub-sententials too.

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Chapter 12
Sub-Sententials: Pragmatics or Semantics?

12.1   Introduction[1]

Rob Stainton has an interesting discussion of sub-sententials in a number of works, particularly in a long article, "In Defense of Non-Sentential Assertion" (2005), and a book, *Words and Thoughts* (2006).[2] He points out that speakers "can make assertions while speaking sub-sententially" (2005: 384). Among his many nice examples is the following, that I shall number "(1)":

> (1) it seems that someone could hold up a letter and say 'From Spain', thereby claiming, about the displayed letter, that it comes from Spain. (2005: 384–5)

Stainton argues for a "pragmatics-oriented approach" to these phenomena and against a "semantics-oriented approach". In contrast, I shall argue for a largely semantics-oriented approach: setting aside novel uses as usual (Sects. 3.5, 10.1, and 11.1), sub-sentential utterances typically assert a proposition, something with truth conditions, in virtue of exploiting a semantic convention.

I have a three-way distinction among the possible "meaning" properties of an utterance (Sect. 3.5): an encoded linguistic (largely conventional) meaning of its sentence type (EC); a what-is-said (WIS), arising from (i) encoding, (ii) disambiguation, (iii) linguistically demanded saturation or slot filling; a pragmatic modification (PM), perhaps a modulated what-is-said or an implicature. EC and WIS are semantic properties; PM are at least partly pragmatic. I claimed that the Pragmatists have made the wrong response to their examples, putting far too many into PM

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[1] This chapter draws heavily on Devitt 2018a but differs from that paper in one important respect. I regret to say that the paper had a serious misrepresentation of Stainton's views. It attributed to him the assumption that the semantics-oriented approach *must* claim that much, or even all sub-sentential speech is actually *syntactically* elliptical (2018a: 49). In fact, Stainton allows that the approach *might* be justified by positing *semantic* ellipses. However, he argues, this justification does not succeed.

[2] Stainton has many citations of the pioneering work of Ellen Barton (1990) on sub-sententials.

instead of WIS. A paradigm example of this mistake is treating referential uses of descriptions as pragmatic phenomena (Chap. 9). Stainton's treatment of sub-sententials is another paradigm.

We must attend to a terminology matter. My methodology talks of "what is said" where Stainton talks of "what is asserted". Now there is a distinction to be made between sayings that are statings or assertions and sayings that are mere rehearsings. But if we set that distinction aside (as I have; see Note 15 in Sect. 3.2.4), then, from my perspective, there is just one theoretically motivated *semantic* notion here, whether expressed by 'what is said' or 'what is asserted'. Given Stainton's terminology, it is better, for the purposes of this chapter, to talk of 'what is asserted'. So I shall. So, on my usage, what is asserted, like what-is-said, is a WIS meaning and semantically constituted.

I shall, of course, be arguing for SEM: typically, what is asserted (=said) by a sub-sentential of a form that is regularly saturated in context is a proposition, a truth-conditional semantic property of an utterance. This is my "semantics-oriented approach" to these cases, at odds with Stainton's "pragmatics-oriented approach". But sometimes a sub-sentential utterances does not exemplify a regular use of a form: it is a novel on-the-fly spur-of-the-moment saturation. In such a case the saturation is not linguistically demanded and what is asserted by a sub-sentential may be only a fragment of a proposition. That fragment is pragmatically enriched to yield a propositional message. (This exemplifies, once again, the extended use of 'saturate', introduced in Sect. 9.2, that does not reserve the term for linguistically demanded slot fillings but leaves it as an open question whether saturations are semantic or pragmatic.) To the extent of these novel uses, I am in accord with a pragmatics-oriented approach. But this is not an interesting extent. As noted, the tradition acknowledged, or at least should have acknowledged, that language can be used in novel ways to convey messages: there can be novel modifications of conventional meanings that must of course be treated pragmatically (Sect. 3.5).

In Sect. 10.3, I pointed out the demand that SEM places on expressions of a form that is regularly used with speaker meanings that are saturated in context: they must be explained semantically by SEs; similarly, PRAG requires that they be explained pragmatically by PEs. This is the model for the demands placed on sub-sententials of a form that is regularly used with speaker meanings that are saturated in context to convey truth-conditional meanings. Suppose that, on a particular occasion, a speaker uses such a sub-sentential E to mean M. SEM demands that the following explanation be typical:

> **SEsub:** E's property of speaker-meaning M after saturation is semantic (in my sense). So M constitutes what-is-asserted, my "WIS"-meaning (Sect. 3.5). On other occasions, the speaker meaning of E after saturation may be different semantic meanings.

PRAG demands the following rival explanation be typical:

> **PEsub**: E's property of speaker-meaning M after saturation is a pragmatic modification of E's semantic meaning and so is at least partly pragmatic (in my senses). So M partly constitutes a pragmatic modification of what-is-asserted, my "PM"-meaning (Sect. 3.5). On other occasions, the speaker meaning of E may be different modification of E's semantic meaning,

I shall argue for SEM in Sects. 12.2, 12.3 and 12.4 by discussing five of Stainton's key examples of sub-sentential speech that he thinks, wrongly in my view, support his pragmatics-oriented approach. I shall then consider his objections to the semantics-oriented approach, starting with one aimed at the semantics-oriented approach of Jason Stanley (2000) in particular. Stanley claims, in Stainton's words, "that much, or even all, of such speech is actually syntactically elliptical – and hence should be treated semantically, rather than pragmatically" (2005: 383–4). Let us call this claim, "the syntactic-ellipsis claim". "The syntactic-ellipsis objection" then is that there is no such ellipsis with the sub-sententials in question. My semantics-oriented approach does not make the Stanley claim and so is not open to the objection. Still, the objection is theoretically interesting. I wonder about it in Sect. 12.5. I shall consider Stainton's other objections in Sect. 12.6. In Sect. 12.7, I will consider pragmatically explained novel uses asserting propositional fragments.

12.2 Implicit Demonstratives

I start my argument for thesis SEM with (1), Stainton's example of the letter from Spain. In introducing his examples, Stainton is careful to talk only of it *appearing* to be the case that what is used to make an assertion is a mere phrase. His pragmatics-oriented approach "takes the appearances at face value": "pragmatics provides the real-world object". In contrast, according to Stainton, "the semantics-oriented approach denies the appearances, and maintains that syntax, and with it semantics, are doing more than meets the eye" (p. 385). Stainton has in mind here Stanley's syntactic-ellipsis claim, of course.

We should all agree with Stainton that the utterance of "From Spain" in (1) conveyed the propositional message that *this*, the displayed letter, is from Spain. The issue is how much of this message is constituted by semantic properties of the utterance and hence, on my usage, asserted; and how much of the message is constituted by a pragmatic enrichment and hence, on my usage, not asserted. My thesis is that the utterance's message is entirely semantic: the proposition that this is from Spain is semantically asserted by "From Spain"; SEsub applies, PEsub does not.

To argue the matter I turn, as usual, to conventions. The speaker of "From Spain" is simply exploiting the conventions of English to assert that *this is* from Spain, where my 'this' refers to the displayed letter. When one has a demonstrative thought that a particular sighted object is from *a* there is, uncontroversially, a convention of expressing this thought, "This/that is from *a*". My point is that there is *also* one of expressing that thought simply, "From *a*"; this is *another conventional way* of speaker-meaning that the object in question is from Spain. Similarly, there is a convention that dispenses with the plural demonstratives, 'these/those'. And there is, of course, nothing special about the preposition 'from'. Let '*PP*' be any prepositional phrase. There is a convention of expressing the thought that one might express, "This/that is *PP*" or "These/those are *PP*", simply, "*PP*". We might say that these sub-sentential utterances contain *implicit* demonstratives and exemplify an

"implicit-demonstrative convention".[3] The reference of an implicit demonstrative is determined in just the same way as an explicit one. That way is, on the folk view, by what the speaker had in mind, and, on what seems to me the best theoretical view, by a certain sort of causal-perceptual link between the speaker and an object or objects (Sects. 3.2.3 and 4.1.2).

Robyn Carston provides an example that suggests that the convention covers not only demonstratives, strictly so-called, but also pronouns: a speaker, realizing that a hearer is looking for the marmalade for breakfast, utters, "On the top shelf" (2002: 17). This is a conventional way of asserting that *it*, the object in mind, is on the top shelf. Indeed, 'From Spain' is a conventional way of asserting that he is from Spain, she is from Spain, and so on.

What is the evidence for this implicit-demonstrative convention? Well, expression of the thought in the abbreviated way is indubitably a regular occurrence: there is nothing at all odd in what Stainton's speaker did. This regular way of expressing a demonstrative thought seems to be accepted in the community. So we have evidence that this regularity reflects a linguistic convention. Supposing that there is a convention offers *the best explanation* of the regularity. What could be the basis for denying this? The regularity certainly *could* arise from a convention: we could do the Kripke trick (1979) and *specify* a language, English* in which it was a convention. (Are there any actual languages where such a convention is *even more obviously* present than in English?) I suggest that English does not differ from English*. There seems to be no principled basis for denying this. Since "From Spain" conveys the message that the letter is from Spain simply by exploiting the conventions of the language, that message is semantically constituted. So I am prepared to say, as does Stainton, that "From Spain" "actually asserts" (p. 387) that the letter is from Spain. But when I say it, unlike when Stainton does, I am committed to what is asserted being semantic.

There is nothing special about sub-sententials that are prepositional phrases. Suppose that the person held up the letter and said, 'A letter'. That is a conventional way of asserting that *this is* a letter; similarly, 'Mail', *this is* mail;[4] 'Pink', *this is* pink; and so on. There are several conventions that enable sub-sententials to express demonstrative thoughts (and some other thoughts, for that matter).

Unaccompanied vocatives provide some interesting examples.[5] Thus, there is quite clearly a general convention of a speaker using a proper name, '*N*!', to

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[3] The psychological phenomenon of "joint attention" encourages the idea that the rule for expressing a thought with a sub-sentential is not conventional but innate. Perhaps we don't *learn* to express the thought that *this*, a particular object in mind, is *PP* by simply using a symbol that expresses *PP*; rather this rule for expressing thoughts is an innate rule of language (just as, according to Chomsky, many syntactic rules are). If so, we should describe it as an implicit-demonstrative "rule" not "convention". Nothing hinges on this for me but I shall continue to describe the rule as a convention. (This note was prompted by a criticism of my 2018a in a paper that I blind-reviewed for *Mind and Language*, "Articulating a framework for unarticulated constituents".)

[4] And, one might add, with the right intonation, it is a conventional way of *asking* if this is mail, *ordering* someone to mail this, and perhaps other illocutionary acts. (Thanks to Rob Stainton.)

[5] I am indebted to Tomasz Zyglewicz for drawing these to my attention.

command the attention of *N*, the person addressed. In certain groups there are conventions of using '*N*!' to convey richer meanings; for example, among those playing various sports "*N*!" means *catch it N*; in a roll call group, it means *Are you present N?*

12.3   Demonstratives and Demonstrations

Our story of (1) does not do it full justice: the speaker of "From Spain" exploits *another* convention to say that the letter is from Spain. He exploits the convention for demonstra*tions* by holding up the letter. There is a convention of using a gesture toward an object in mind as part of an expression of a thought about that object.[6] Demonstrations, like demonstratives, are conventional devices for referring to objects in mind.

It is common, of course, for demonstratives to be accompanied by demonstrations. This has misled many into thinking that a demonstrative *demands* a demonstration which then determines its reference. That was the view of David Kaplan in his classic "Demonstratives" (1989a: 489–91); and, according to Jason Stanley and Zoltan Szabó, the view is "uncontroversial" (2000a: 220–1). Yet the view has many problems and Kaplan rightly came to abandon it in "Afterthoughts" (1989b: 582).[7] I have summarized the problems as follows:

> (i) A demonstration is often so vague that it alone would not distinguish one object from many others in the environment. (ii) If an object is sufficiently salient in an environment, a demonstrative that refers to it may well not be accompanied by a demonstration. (iii) Reference is often to an object that is not around to be demonstrated; e.g., "That drunk at the party was boring". (2004: 290–1)

Just as there can be demonstratives without demonstrations, there can be demonstrations without demonstratives. Consider a woman confronted by a line-up and asked the question, "Who mugged you?" She points wordlessly to a man, Harry. She has designated Harry, and not simply *speaker*-designated him. Her demonstration in these circumstances is a conventional expression of the thought that Harry mugged her, just as much as would be her responding "Harry" when asked that same question at the scene of the crime. She is relying on the same convention that plays a role in many referential cases to *semantically* designate him.

How is the reference of a demonstration determined? In much the same way as the reference of a demonstrative. Take a person's deictic use of the pronoun 'he' as our specimen demonstrative. According to the best theory, in my view (Sects. 3.2.3 and 4.1.2), 'he' refers to a male that stands in a certain sort of causal-perceptual

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[6] Perhaps this role for a gesture is not conventional but innate. No matter; see note before last. Other gestures arguably have a conventional meaning too. Thus one can assert that the Yankees will reach the play offs by responding to "Will the Yankees reach the play offs?" with a nod. The nod conveys that message by convention and there is nothing interestingly pragmatic about it.

[7] He now thinks that a demonstration "is an aid to communication, like speaking more slowly and loudly, but is of no semantic significance" (1989b: 582). Clearly I disagree.

relation to the speaker. Similarly, a demonstration refers to an object in the gestured area that stands in a certain sort of causal-perceptual relation to the speaker. Perhaps we should say that demonstratives are, but demonstrations are not, *linguistic* referential devices. Whatever, they are distinct referential devices.

Just as it is common for demonstrations to accompany explicit demonstratives, so is it for them to accompany implicit ones; indeed, it may be more common. This might prompt an objection along the following lines to my treatment of (1). "On your view, the demonstration that accompanied "From Spain" referred. So, we can take the whole utterance to convey the message that *this*, the demonstrated letter, is from Spain, *without supposing that (1) includes an implicit demonstrative that refers to the letter*. So, your line on demonstrations undermines the case for an implicit-demonstrative convention." The objection fails because just as an *explicit* demonstrative need not be, and often is not, accompanied by a demonstration, so too, an *implicit* demonstrative need not be. Thus, compare these two assertions that Stainton might have made whilst looking straight at the letter but without holding it up or otherwise demonstrating it:[8] first, he might have said, "This is from Spain"; second, he might have said simply, "From Spain". Both these demonstration-less utterances would have asserted that *this*, the perceived letter, is from Spain. And each would have done so by exploiting a convention: in the first case the convention for explicit demonstratives, in the second, for implicit ones.

Where a demonstrative, whether explicit or implicit, is accompanied by a demonstration and all goes well, the demonstrative and the demonstration will each semantically refer, in their own right, to the one object. All did go well in (1). So, "From Spain" together with the gesture *doubly* refers to the letter in asserting that it is from Spain, once by implicit demonstrative and once by demonstration. And what is asserted is semantically determined. It is no more pragmatically determined than is what would be asserted by "This is from Spain".

All might not go well, as illustrated in Kaplan's famous example of an explicit demonstrative accompanied by a demonstration:

> Suppose that without turning and looking I point to the place on my wall which has long been occupied by a picture of Rudolph Carnap and I say:
>
> (27) Dthat…is a picture of one of the greatest philosophers of the twentieth century.
>
> But unbeknownst to me, someone has replaced my picture of Carnap with one of Spiro Agnew. (1979: 396)

Suppose that Kaplan's 'dthat' functions like a normal demonstrative 'that' (which is probably not what Kaplan intends).[9] In thinking about this example, it is vital to distinguish the semantics of the demonstrative from that of the demonstration. For, the demonstrative in (27) straightforwardly semantically designates Carnap's picture: in using 'dthat' Kaplan had that picture in mind in virtue of his thought being causally grounded in it via many earlier perceptions. The trouble comes from the

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[8] One *can* demonstrate an object by meaningfully moving one's eyes, of course, but merely looking at an object is not demonstrating it.

[9] Precisely what he does intend by 'dthat' is a difficult scholarly matter; see 1989a and 1989b, particularly pp. 580–2.

demonstration: even though Kaplan had that picture in mind in pointing, his gesture was not toward it but toward Agnew's picture. So though Kaplan *speaker*-designated Carnap's picture with his gesture, he did not *semantically* designate it: for, the convention requires that he gestures toward the object in mind.[10] Suppose, now, that Kaplan had not bothered with 'dthat' and had simply used the sub-sentential, 'A picture of one of the greatest philosophers of the twentieth century', pointing as before toward Agnew's picture. His implicit demonstrative would have semantically designated Carnap's picture but his gesture would not.

Marga Reimer provides another nice example of things not going well: "I…spot my keys, sitting there on the desk, alongside my officemate's keys. I then make a grab for my keys, saying *just as I mistakenly grab my officemate's keys*, 'These are mine'" (1991b: 190). I say, though Reimer does not, that her demonstrative semantically designated her keys. Her grabbing gesture is irrelevant to that but has its own semantics. Like Kaplan's gesture it fails to designate the object in mind because of a mistake. Had she simply used the sub-sentential, 'Mine', her implicit demonstrative would have semantically designated her keys just as her explicit one did.

Consider now a simple case of misidentification. Suppose that the only keys on the desk are her officemate's. Reimer mistakes them for her own and makes a grab for them, saying "These are mine." This is, of course, simply false. I explain this by saying that both her demonstrative and demonstration are grounded in her housemate's keys. But this misidentification reveals an interesting fact: her demonstrative and demonstration were also grounded in her own keys by earlier perceptions, but *those past groundings are trumped by the present one*. (This is the position on demonstratives taken implicitly in my discussion of analogous mistakes with names; 1981a: 143–144.) Of course, those earlier perceptions could determine the reference of a demonstrative in the *absence* of the later grounding – for example, "Those keys don't work", referring to keys acquired yesterday – but the latest grounding removes the semantic significance of earlier ones.

12.4   Other Examples

So much for Stainton's letter example (1). I continue the defense of SEM – the thesis that typically, the truth-conditional meaning conveyed using a sub-sentential is semantic (SEsub) – by considering another four of Stainton's examples. In Sects. 12.5 and 12.6 I shall turn to his criticisms of a semantics-oriented approach to such examples.

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[10] Of course, the gesture makes Agnew's picture *salient* and hence the audience is likely to take that picture to be the referent of both the demonstrative and the gesture. This would be a *mis*understanding arising from Kaplan's failure to follow the convention for demonstrations. It is a matter for "the epistemology of interpretation". What makes an object the referent is its causal relation to the thought expressed. This is a matter of "the metaphysics of meaning" and salience has nothing to do with it; or so I have argued (Sect. 7.4); see also Sect. 12.6.3 below.

(2) At one point on the Nixon tapes, Nixon, Halderman, and Dean discuss the reporting of an event involving Maurice Stans and money from Vesco. Halderman points out that it was not until "the back section" that the *Post* reported that the money had been returned. Dean responds: "Typical" (2005: 389). Dean is simply exploiting the implicit-demonstrative convention for an utterance of a sub-sentential adjective to assert that *that*, the event referred to by Halderman, is typical. Dean's message is semantically constituted; SEsub applies.

(3) Stainton discusses the following example presented by Jason Stanley (2000: 404):

> Suppose Bill walks into a room in which a woman in the corner is attracting an undue amount of attention. Turning quizzically to John, he arches his eyebrow and gestures towards the woman. John replies…"a world famous topologist". (Stainton 2005: 396)

Stanley wants to treat this case semantically by showing that it exemplifies syntactic ellipsis; see his syntactic-ellipsis claim above (Sect. 12.1). But he has a problem. According to the syntactic theory that he, and Stainton, favor, syntactic ellipsis cannot appear in "discourse initial" utterances (2005: 395). So Stanley's problem is that John's utterance, "a world famous topologist", seems to *be* discourse initial. In response, Stanley argues that because of the quizzical look, arched eyebrow, and gesture in (3), John's utterance *is not really* discourse initial and so can, after all, be treated as a case of syntactic ellipsis. So it can be treated semantically. Stainton rejects this view, insisting that the utterance must be treated pragmatically.
I shall take no stand on whether or not John's utterance is discourse initial. In my view, arising from my earlier resistance to "the tyranny of syntax" (Sect. 10.5), whether or not the utterance can be treated as syntactic ellipsis, as understood by the favored syntactic theory, is not crucial to the semantics-oriented approach. What *is* crucial is that John is exploiting the implicit-demonstrative convention to convey the message that *she*, referring to the woman in the corner that he has in mind, is a world famous topologist. That's why that message is explained by SEsub not PEsub. That's why that message is what John (semantically) *asserts*. If, and how, this implicit demonstrative is to be handled by the grammar is, of course, a matter for linguists, but the convention can't be gainsaid.

(4) The next example I shall consider is another one that has played a role in the exchange between Stainton and Stanley. Suppose that someone passes a woman in the street and says, "Nice dress" (Stainton 2005: 397). This is a paradigm exploitation of the implicit-demonstrative convention. The person has a particular dress in mind as a result of a causal-perceptual link to it and is asserting that *that*, the dress in mind, is a nice dress. This is semantic through and through: SEsub.

Although Stanley likes to treat sub-sentential assertions as cases of syntactic ellipsis, he has a different view of this one:

> it is intuitively plausible to suppose, in this case, that the speaker simply intended her utterance to be shorthand for "that is a nice dress". It is difficult to see how any of the resources of linguistic theory could be used to show that intuition misleads in cases of this sort. (2000: 409)

As Stainton notes (2005: 394), this in effect treats the case as one of "semantic" rather than "syntactic" ellipsis. Reinaldo Elugardo and Stainton wring their hands about what this might possibly mean through several pages before remarking: "We are not entirely sure what philosophers have in mind, when they appeal to things like 'shorthand'" (2004: 453). The implicit-demonstrative convention supplies a simple answer that they don't consider: there is a semantic convention in English of expressing the thought that that is a nice dress by the short "nice dress" as well as by the long "that is a nice dress".

(5) The final example I shall consider in support of SEM is drawn from that article by Elugardo and Stainton:

> Suppose that Fritz and Rob are walking past a misbehaving teenage boy. Fritz utters, "From Brazil". Fritz here says, about the boy, that he is from Brazil. (2004: 459)

I agree, of course, that Fritz did indeed say that. That is a semantic matter for me, SEsub, but not for Elugardo and Stainton. They see a problem for the semantics-oriented approach that they are discussing because there is "no good reason for saying the salient label was 'he' versus 'that boy' versus 'that teenager'" (p. 459). This objection is aimed at Stanley's syntactic-ellipsis claim. But my semantic-oriented approach is not committed to this claim. Whether or not the objection is effective against Stanley, it is not against me. I appeal to conventions not syntactic ellipsis. And the convention being exploited here provides only an implicit demonstrative or pronoun. So what is semantically expressed, what-is-said, could be explicitly expressed by 'He is from Brazil'.

But might it not be the case that Fritz intended to convey not just that a certain *male* was from Brazil but that a certain *teenager* was from Brazil? It surely might but, given that the male in question obviously is a teenager, it may be very hard to tell whether Fritz did in fact intend this richer message. Still, suppose he did. Then had he made his message explicit he would have used a complex demonstrative, "That teenager is from Brazil". There is surely no convention for conveying such a message implicitly by simply saying "From Brazil"; this is a novel use. So Fritz's message would be a small pragmatic enrichment of the semantic propositional what-is-said; PEsub would apply. There will be more on novel uses and pragmatic enrichments in Sect. 12.7.

It is time to turn to Stainton's objections to the semantics-oriented approach.

12.5   The Syntactic Ellipsis Objection

My semantics-oriented approach to examples (1) to (5) is what Stainton aptly calls a "slot-filling" one. He rejects such approaches unequivocally: "sub-sentential speech cannot be reduced to reference assignment to 'slots'" (2005: 389n). Perhaps his main reason for this rejection is that these examples cannot be treated as cases of syntactic ellipsis. Now, as I have emphasized with my resistance to "the tyranny of syntax" (Sect. 10.5), my semantics-oriented approach is not committed to the claim that they can be so treated. Still, it would be theoretically interesting if the

examples could be treated as syntactic ellipsis. So I wonder about Stainton's claim that they cannot. I am no syntactician and so must tread very lightly here.

We should start by considering what syntactic ellipsis is. We came across a paradigm example in Sect. 6.5.1: 'Mary went to visit the zoo and John, the museum' is elliptical for 'Mary went to visit the zoo and John went to visit the museum. Stainton gives some others involving sub-sententials, including the following:

A: Who lives in Madrid
   B: Juan doesn't

A: Juan will soon move to Madrid
   B: I wonder why

The idea is that underlying the B-utterances are the whole sentences, 'Juan doesn't live in Madrid' and 'I wonder why Juan will soon move to Madrid', respectively; parts of these sentences go unpronounced in the utterance. So though the B-utterances *appear to be* sub-sentential on the surface, at a deeper level they are not (2005: 395).

Here is an example of Stainton denying that a sub-sentential assertion is a case of syntactic ellipsis. Talking about Dean's utterance of "Typical" in (2), he says:

> Crucially, what makes the former filling-in-to-arrive-at-what-is-asserted pragmatics is that the saturation is not a matter of linguistic derivation, but is instead a matter of all-purpose inference triggered by the pragmatic unfitness of the sub-propositional content – where, moreover, the inference is based on both linguistic context and other kinds of knowledge. (2005: 389).

My main concern is with what Stainton denies, but first, a word about what he asserts. His view is that the saturation that yields the propositional content is *constituted by pragmatic inferences made by the hearer*. This is an example of the conflation discussed in Chap. 7: the conflation of "the metaphysics of meaning" – for example, the semantics of what a speaker asserts – with "the epistemology of interpretation", the pragmatics of how a hearer interprets what is asserted. The content of the utterance is constituted by the speaker; inferences made by the hearer to understand the utterance are no part of it.[11]

Turn now to Stainton's denial. Why isn't the saturation of Dean's utterance "a matter of linguistic derivation"? Why isn't the utterance the result of a derivation involving what we might call "demonstrative deletion"? Elugardo and Stainton claim, about a similar example, that "the only thing which syntactic structure contributes to the content here, even relative to a context, is a propositional function" (2004: 445). One wonders about the basis of these claims. Why is the structure not contributing an implicit demonstrative referring to the event described by Halderman?

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[11] Stainton is much more on the right track three pages earlier: "the speaker's intentions play a key role in determining the content and illocutionary force of the utterance" (p. 386). This exemplifies what we noted earlier (Sect. 7.3): Pragmatists who mostly take epistemic processes in hearers to be constitutive of content, do not always do so.

Consider this example (based on one of Kripke's 1979: 14). *S* and *H* see Smith in the distance and *S* says:

(a) What is Jones doing?

H responds:

(b) Raking the leaves.

Now, if Neale (2007) is right, in a discussion already cited in Sect. 10.6, it is an open empirical question whether the underlying syntactic structure of the sub-sentential in (b) might not be something like that of

(c) α raking the leaves

in which α is aphonic. Now suppose that, instead of asking (a), *S* had simply asserted (b) himself. If it is an open question whether the sub-sentential in *H*'s assertion (b) has a syntactic structure like (c), shouldn't it be just as much an open question whether the sub-sentential in *S*'s assertion (b) has? Perhaps not. A received principle of syntactic ellipsis is that the deleted material be "recoverable" (Neale 2004: 137–43). Thus, when *H* asserts (b), a deleted 'Jones' could be recovered from *S*'s (a). But had S asserted (b) out of the blue there would have been no prior discourse from which to recover deleted material. Still, why can't that deleted material be recovered from (b) itself? If there really is an implicit-demonstrative convention in English, – if English is English* – then the structure of the sub-sentential (b) alone could provide the missing demonstrative. If this is not accommodated by our current syntactic theory of English, wouldn't it be a simple matter to adjust that theory by taking '*F*' to be derived by deletion from 'This is *F*'? It is, after all, the responsibility of a syntactic theory to be in accord with the conventions of the language.

Stainton objects to positing any phonologically null elements where there is "no compelling syntactic evidence" for the posit, where it is "merely to account for what the sound *was used to say*" (2005: 424; see also 2006: 94–144). But why is that not enough evidence *if* there is a *convention* of using the sound for that purpose. And if the syntactic theory does not accommodate this, why is that not so much the worse for the theory?

Still, Stainton may well be right: perhaps these cases of sub-sentential assertions cannot be treated as syntactic ellipsis. But, as I have emphasized, the semantics-oriented approach does not depend on their being so treated.

12.6 Stainton's Other Objections

12.6.1 *Too Much Ambiguity*

Stainton objects that a semantics-oriented approach

> requires the introduction of a new *expression* in each case where semantic ellipsis is held to apply: things that can be used to perform speech acts, are syntactically non-sentential, but

nevertheless are *not* ordinary word and phrases…there would be a very large class of one-word and one-phrase sentences… (2006: 84)

When "nice dress" appears in a sentence like, "Fiona came to the party in a nice dress", it has a sub-propositional, meaning but when it appears as a sub-sentential in (4), it has a "new" propositional, meaning. On the semantics-oriented approach such ambiguity would be rampant. Stainton thinks that this "is not a good thing" though "not ultimately damning" (p. 84).

But this is the wrong way to look at a proposal like mine. The proposal is that the utterance of "nice dress" in (4) participates in one of *just a few* implicit-demonstrative conventions, each of which is a convention of using an expression *of a certain syntactic type with a certain sub-propositional meaning* to make propositional assertions. In (4), the convention is of expressing the thought that one might express, "This is *NP*", simply, "*NP*", where '*NP*' is a noun phrase. As noted in Sect. 12.2, there are similar conventions for prepositional phrases and others (and doubtless there are other conventions for sub-sententials that are not implicit-demonstrative conventions). Why suppose that there are these conventions? Because, to repeat, that best explains the regularities in the phenomena.

I emphasize that I am saying that there is a convention of using a sub-sentential, which of course *has* a sub-propositional meaning, to *express* a propositional meaning. That is not to say that the sub-sentential *has* a conventional propositional meaning. Here is an analogy: X says to Y: "Who were you talking to?" *Y* responds "*Z*". According to convention, *Y* thereby *expresses* the proposition that *Y* was talking to *Z*. But this is not to say that '*Z*' *has* a conventional propositional meaning.[12]

Stainton himself imagines a "rebuttal" of his too-much-ambiguity objection that is a bit along these lines:

we do not have to worry about the ambiguity of the sound-pattern, because there can be a single semantically productive mechanism that yields the propositional meaning of the word/phrase-in-the-non-sentence-construction. (p. 89)

Stainton gives this rebuttal short shrift:

the problems with it are legion. First, it predicts that all unembedded uses of subsentences will be propositional and force-bearing, which is not the case…Worse, this introduces a further kind of construction that simply cannot occur embedded in larger expressions,… Finally, it would require the postulation of many different constructions…what needs to be added to arrive at a propositional content varies: sometimes an object is added, sometimes a property, sometime a generalized quantifier, etc. (p. 92)

But the trouble with the rebuttal is that it presumes that there would be a *single* "productive mechanism" which seems to apply to *any* word/phrase. My proposal is of *several* conventions each applying to a *single* syntactic type of word/phrase. This deals with Stainton's first problem. And it pretty much deals with the final one. There is indeed good evidence for several implicit-demonstrative conventions, but these conventions restrict the additions that arrive at a propositional content to what

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[12] Thanks to Michael Greer for prompting this paragraph.

can be the referent of an implicit demonstrative. There may be other additions that are pragmatic not semantic, as we shall see in next section.

What about Stainton's second problem? It raises two questions. (a) Has my proposal really introduced a construction that cannot occur embedded in larger expressions? (b) If it has, does this amount to an objection? Does it count against there being a convention of using a sub-sentential to express a full propositional meaning? The answer to (a) seems to be "Mostly yes"; thus, "Mary hopes that nice dress" is ill-formed. But what about "If nice dress, then expensive"? And here are some other suggestions: "That's not right, but interesting idea"; "I wouldn't wear it, but cute hat".[13] Concerning (b), it seems that an objection must rest on a principle that conflicts with what our discussion has revealed about the conventions of English. That discussion shows that the meaning "nice dress" expresses when used in (4) according to the implicit-demonstrative convention is different from the meaning it expresses when used in "Fiona came to the party in a nice dress". One wonders about the empirical basis of any principle that could count against this.

12.6.2   *No Explanatory Work*

Stainton objects that semantics-oriented proposals posit extra "machinery" that "does no explanatory work" (2006: 84). For, the semantic machinery already in place, together with familiar pragmatic abilities, will do the trick:

> given only knowledge of the meaning of ordinary words and phrases, and a limited range of pragmatic abilities, a speaker could make non-sentential assertions; and given only knowledge of the meaning of ordinary words and phrases, and a limited range of pragmatic abilities, a hearer could interpret utterances of ordinary words and phrases as assertions....Hence there is no reason to introduce, as an extra competence, knowledge of one-word and one-phrase [non-sententials]. (85)

(Stainton wrote this before he considered the "rebuttal" and so thought that semantic proposals require knowledge of an "enormous class" of novel "form-meaning" pairs (pp. 84–5). As we have just seen, they actually require knowledge of only a few implicit-demonstrative conventions.)

This objection is an example of common thinking that is strikingly exemplified in the usual construal of Grice's Modified Occam's Razor: "Senses are not to be multiplied beyond necessity" (1989: 47). On that construal, as I have pointed out earlier (Sect. 8.1), the Razor advises against positing a sense *wherever there is a pragmatic derivation of the message*. Thus, in the circumstances of (4), there is a pragmatic derivation of the propositional message, *that is a nice dress*, from the sub-propositional meaning, *nice dress*, a derivation that the speaker and hearer *could* make. So, the Razor advises, there is no explanatory need to suppose that

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[13] Thanks to Richard Stillman for these suggestions.

"Nice dress" in (4) semantically expresses the propositional meaning rather than its familiar sub-propositional meaning. So, we should not suppose this.

I argued (Sect. 8.2) that this advice is seriously flawed: it would make all metaphors immortal. When a metaphor "dies", an expression comes to mean conventionally what it once meant metaphorically. *Yet there is still a pragmatic derivation of the new meaning from the old.* We should not prefer a pragmatic to a semantic explanation of linguistic regularities simply because *there is* such a pragmatic derivation but only because *the pragmatic explanation involving that derivation is better*. And for that explanation to be *even good*, as I have emphasized repeatedly (Sects. 8.4, 9.4, 10.4 and 11.5), it is not enough that speakers and hearers *could* make the derivation, the derivation has to be *actually present*, in the appropriate way, in their cognitive lives: there is a "psychological-reality requirement" that is particularly pressing given the Occamist and Developmental Objections to PRAG.

Consider, for example, the appropriate way for the derivation in (4) to be present. For a pragmatic explanation of the sort that Stainton is contemplating to be good, it would have to be plausible that, in those circumstances, there would be mental processes in the typical speaker and hearer that differed from the standard convention-exploiting ones. In speakers there would have to be thoughts about hearers and their expected non-convention-exploiting derivations; in hearers there would have to *be* those derivations or inferences. These mental processes need to be *psychologically real*. And it is plausible to think that they are real in Gricean particularized conversational implicatures. But it is not plausible to think this with "Nice dress" in (4). We should prefer the semantic explanation.

12.6.3 Fails a Kripkean Test

The final objection I shall consider draws inspiration from Kripke (1979). Stainton stipulates a language, Linglish, that lacks the phonologically null elements that, on one understanding, would have to be posited by my implicit demonstrative proposal. However, as I have emphasized (Sects. 12.4, 12.5), that is not my way of understanding the proposal. So, to make Stainton's objection bear on my way, let us broaden his stipulation: Linglish lacks implicit-demonstrative conventions *altogether*. Stainton goes on to consider a Linglish speaker, Angelika, who performs as in the Spanish letter case, (1). Stainton claims that Angelika could "be understood to have communicated the proposition that the displayed letter was from Spain" (2005: 424; 2006: 129). And so she could be. And the message conveyed would be partly constituted by a pragmatic enrichment by the speaker. But if Angelika's practice *became conventional* in the linguistic community then Linglish would have *changed* to now have an implicit-demonstrative convention; it would have become English. Languages change when new conventions are adopted.

This concludes my argument for SEM. Let *E* be a sub-sentential expression of a form that is regularly used with a speaker meaning that is saturated to yield a truth-conditional speaker meaning. Then, typically, what is meant by an utterance of *E* is

semantic: the utterance has a semantically constituted propositional content; typically SEsub applies. This counts against Stainton's pragmatics-oriented approach to sub-sententials.

12.7   The Pragmatic Explanation of Novel Uses

I turn now to some novel uses of sub-sententials. A sub-sentential of a certain form may be saturated in context in a way that does not exemplify a regular use of that form: it is a novel on-the-fly spur-of-the-moment saturation that is not linguistically demanded. What is asserted by the sub-sentential may be only a fragment of a proposition, which needs to be pragmatically enriched to yield a propositional message. Even if what is asserted is fully truth-conditional, the novelty may come from its pragmatic enrichment. To this extent I am in accord with a pragmatics-oriented approach. But it is not an interesting extent because novel nonconventional uses of language must obviously be explained pragmatically (Sects. 3.5, 10.1 and 11.1).

Stainton provides the following two examples, into which I have inserted numerals:

> [(6)] Meera was spooning out strawberry jam onto her toast, and produced (or, more safely, appeared to produce) the phrase 'Chunks of strawberries'. [(7)] Anita nodded, and (seemingly) added 'Rob's mom'. It appears that Meera asserted something like *This jam contains chunks of strawberries*, while Anita asserted something like *Rob's mom made it*. In both cases, they appear to have made true statements while using something sub-sentential. (2005: 384)

I start with (7). This seems a clear case of only a propositional fragment being asserted. Even with an implicit demonstrative, all that is conventionally conveyed is the propositional fragment that Rob's mom…*this*…. There is no linguistic convention available that could make this sub-sentential the assertion of the intended message that Rob's mom made this. The intended message is obtained by the speaker's pragmatic enrichment of what is literally asserted.

I'm inclined to say the same about another of Stainton's examples:

(8) A: The White House staff doesn't visit Tip O'Neill in his Congressional office.
    B: An old grudge (p. 418).

This conveys the message that the White House staff doesn't visit Tip O'Neill in his Congressional Office *because of* an old grudge. Had B conveyed that message by saying "That's because of an old grudge" he would have literally asserted that message. But B's actual remark has no "because of" and I don't suppose that there is a convention of conveying the message that *S because of N* by responding to 'S' with 'N'. (In contrast, there is a convention of conveying that message by 'N' in response to a why-question about *S*.)

What about (6)? This as another exploitation of the implicit-demonstrative convention. So Meera has asserted the full proposition that *these are chunks of strawberry*. That is the utterance's *semantic* property. Perhaps the intended message is a

bit richer: that this jam contains chunks of strawberries. Any such extra richness is not something that the utterance has by convention. It would be as a result of pragmatic properties added by the speaker to the proposition she literally asserted.

The pattern to my responses is to look for properties that the utterance has simply as a result of the exploitation of linguistic conventions. We have a theoretical basis for distinguishing these properties that make the utterance an instance of a linguistic system from any others that may constitute the message, whether we call them "what is said", "what is asserted", or whatever.

12.8   Conclusion

Stainton points out that speakers "can make assertions while speaking sub-sententially". He argues for a "pragmatics-oriented approach" to these phenomena and against a "semantics-oriented approach". In contrast, I have argued for SEM, a semantics-oriented approach: typically, what is asserted by a sub-sentential of a form that is regularly saturated in context is a proposition, a truth-conditional semantic property of the utterance; typically, the utterance is to be explained by SEsub. Thus, there is an "implicit-demonstrative convention" in English of expressing a thought that a particular object in mind is *F* by saying simply '*F*' (Sects. 12.2 and 12.4). I noted also that some sub-sentential assertions include demonstrations and argue that these exploit another semantic convention for expressing a thought with a particular object in mind (Sect. 12.3).

I considered four objections that Stainton has to a semantics-oriented approach. One is aimed at Stanley's semantics-oriented approach in particular. Stanley claims, in Stainton's words, "that much, or even all, of such speech is actually syntactically elliptical – and hence should be treated semantically, rather than pragmatically" (2005: 383–4). Stainton objects that there is no such ellipsis with the sub-sententials in question. My semantics-oriented approach does not make the syntactic-ellipsis claim and so is not open to the objection. Still, the objection is theoretically interesting. I have wondered about its appropriateness (Sect. 12.5). I went on to reject the other three objections: "too much ambiguity"; "no explanatory work"; and "fails a Kripkean test" (Sect. 12.6).

Nonetheless, the message of *novel* sub-sentential utterances, perhaps asserting only a fragment of a proposition, must come at least partly from pragmatic enrichment. To this extent I am in accord with a pragmatics-oriented approach. But it is not an interesting extent because novel nonconventional uses of language must obviously be explained pragmatically (Sect. 12.7).

In Chapter 3 I promised to defend SEM by arguing that Pragmatist examples of context relativity, thought by them to challenge the tradition that they seek to overthrow, typically exemplify properties arising from (i) convention, (ii) disambiguation, and (iii) linguistically demanded saturation (slot filling). I started the defense

in Chap. 9 by considering definite descriptions. They illustrate properties of all three sorts. But generally the context-relative phenomena of the Pragmatist challenge fall into two groups. In Chap. 11, I argued that one group can be accommodated because they exemplify semantic polysemy that is disambiguated in context. In Chap. 10, I argued that the other group can be accommodated within the tradition because they exemplify linguistic conventions of saturation in context. In this final chapter, I have argued that sub-sententials are a special case of saturation in context. That concludes my case for SEM.

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