SOLICITED ARTICLE

Semantic polysemy and psycholinguistics

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The paper urges that polysemous phenomena are typically semantic not pragmatic. The part of a message sent by a polysemous expression is typically one of its meanings encoded in the speaker's language and not the result of pragmatic modification. The hearer receives that part of the message by a process of disambiguation, by detecting which item in the lexicon the speaker has selected. This is the best explanation of observed regularities. The paper argues that the experimental evidence from psycholinguistics, particularly that produced in discussions of "underspecification" and "overspecification," does not undermine this view nor support the pragmatic alternative.

KEYWORDS

conventional meaning, lexicon, overspecification, polysemy, pragmatic modification, sense enumeration lexicon, underspecification

1 | BACKGROUND

Linguistic pragmatism, or contextualism, has mounted a formidable challenge to the semantic tradition.[1] In "What makes a property 'semantic'?" (Devitt, 2013a), I argued for a certain approach to this semantics–pragmatics dispute with the aim of defending a position in the spirit of the tradition. One thing I had in mind, and suggested briefly elsewhere (Devitt, 2013b, p. 95), was that polysemy is typically a "semantic" not "pragmatic" phenomenon. I made this suggestion without attending to the burgeoning psycholinguistic work on polysemy. For, despite my admiration for the ingenuity of experimental work in psycholinguistics (Devitt, 2006a), I thought it unlikely that this work would cast much light on the semantics–pragmatics dispute.

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[1]The seminal work in the challenge is Sperber and Wilson (1986/1995).

My aim in this paper is to explain this prediction and to give it some support. But first I must summarize my approach to the dispute and my position on polysemy.[2]

We need a *theoretical basis* for distinctions that play a role in the semantics–pragmatics issue; for example, for the Gricean distinction between what is said and what is meant (Grice, 1989). I argued that the required basis is to be found by noting first that languages are representational systems that scientists attribute to species such as bees, prairie dogs, and humans to explain their communicative behaviors.[3] We then have a powerful theoretical interest in distinguishing two sorts of properties of any particular utterance: (a) the representational properties that it has simply in virtue of being a token-expression in a language, that it has simply as a result of the organism's exploitation of that language; (b) any other properties that may constitute the organism's "message." I call the (a) properties part of "what is said," and "semantic," and the (b) ones—for example, certain "modulations" and Gricean "implicatures"—part of "what is meant but not said," and "pragmatic." "Semantics" is the study of semantic properties, "pragmatics," pragmatic ones.[4] This theoretical basis then provides an argument for the view that what is said is constituted by properties arising from three sources: (i) from (largely)[5] conventional linguistic rules in the speaker's language, rules that determine what is *encoded* in the language; (ii) from disambiguations where more than one rule governs an expression in the language; and (iii) from "saturations," for example, the reference fixing of indexicals (and tenses), deictic demonstratives and pronouns. This is a common view for which I claim to have given a theoretical, not just intuitive, basis.

From this perspective, "semantics" is concerned with the representational properties that symbols have simply in virtue of being uses of a language, the properties that constitute what is said. These properties contribute to conveying the message of an utterance. Other properties may also contribute to this, but these are the concern of "pragmatics" not semantics. So the key semantic issue is the nature of those linguistic representational properties. The symbols have those properties in virtue of being part of a representational system of conventional rules. So the key issue comes down to: What are the system's conventional rules?[6]

What is a *linguistic* convention? It is a convention of using a certain physical form to express a certain part of a thought/message, a concept. Putting this in Gricean terms, it is a convention of using that form with a certain "speaker meaning."

How do we tell when there is such a convention if we are not to follow the custom of simply relying on our intuitions?[7] We should look for evidence mainly from regularities in behavior. Is this physical form regularly used to express a certain concept, used with a certain speaker

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[2]Both the approach and the position are developed in much more detail in *Overlooking conventions: The trouble with linguistic pragmatism* (forthcoming). The present paper is drawn from this work.

[3]This view of human languages is rejected by Chomskians. They see these languages as internal states not systems of external symbols that represent the world. I argue that they are wrong (2006a, 2006b, 2008a, 2008b, 2008c, 2020, Section 19.1.1).

[4]"Pragmatics" is also used for "the theory of interpretation," the study of the *processes* of interpreting utterances. So the term is ambiguous (Devitt, 2013a, pp. 103–105).

[5]I say "largely" because I accept the Chomskian view that some syntax is innate. The qualification should be taken as read in future.

[6]I think that conventions should loom very large in our view of human language. In stark contrast, Chomsky thinks that the "regularities in usage" needed for linguistic conventions "are few and scattered" (1996, p. 47; see also 1980, pp. 81–83). Furthermore, such conventions as there are do not have "any interesting bearing on the theory of meaning or knowledge of language" (1996, p. 48). I think these views are very mistaken 2006a, pp. 178–89; 2006b, pp. 581–582, 598–605; 2008a, pp. 217–229).

[7]I have labeled the custom of relying on intuitions, the "First Methodological Flaw of Linguistic Pragmatism" (2013c).

meaning? If so, it is likely, though not certain of course, that this regularity is *best explained* by supposing that there is a convention of so using the form. In principle, evidence could be found also in mental processes but in fact, I shall argue, we lack any such evidence.

What is the pragmatist challenge to the semantic tradition from this perspective? It is helpful to answer this in terms of a three-way distinction among the possible "meaning" properties of an utterance: (1) an encoded conventional (EC) meaning; (2) a what-is-said (WIS), arising from encoding, disambiguation, saturation; (3) a pragmatic modification (PM), perhaps a modulated WIS, an implicature, or whatever. EC and WIS are semantic properties; PM are at least partly pragmatic. We should all accept—and I assume the tradition did—that there are novel uses of language, "spur of the moment" uses "on the fly" that yield PM-meanings. Thus there can be implicatures like Grice's famous reference letter (1989, p. 33). And spontaneous ellipses are surely common, requiring WIS to be pragmatically enriched or impoverished to get the precise message. 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 pragmatist challenge to the tradition is posed by *the expressions that motivated linguistic pragmatism*, for these expressions have *regular* uses that are alleged to be pragmatic; for example, regular saturations of quantifiers with domain restriction, of weather reports with reference to a location, of definite descriptions with reference to a particular object in mind; and, turning to polysemy, regular uses of "foot" to refer to the bottom of a mountain, of "suit" to refer to business executives, of "rabbit" to refer not only to rabbits but also to rabbit stuff. This interesting challenge is to the view that the truth-conditional meaning communicated in such regular uses is typically constituted only by what-is-said; by WIS-meanings arising only from (a) conventional encodings, (b) disambiguations, and (c) saturations. The challenge is to the view that, absent novel spur-of-the-moment modulations and implicatures, the message of an utterance is typically its WIS-meaning. In challenging that view, pragmatism claims that, even setting aside the novel, pragmatic modifications of some sort 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.

The concern in this paper is with the pragmatist challenge on polysemy not saturation. Here is how I set up the challenge. 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 to M1.[8] On a particular occasion, a speaker uses E to mean M2. The view, in the spirit of the tradition, that is challenged is:

> **SEM**: Typically, E's property of speaker-meaning M2 is semantic and so is another encoded meaning. It partly constitutes what-is-said, a WIS-meaning. On other occasions, the speaker meaning of E may be a different semantic meaning, M3. So, typically, E is ambiguous and the polysemy is semantic.

The rival pragmatic view is:

> **PRAG**: Typically, E's property of speaker-meaning M2 is a pragmatic modification of E's semantic meaning M1 and partly constitutes a PM-meaning. On other

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[8]I set aside than an expression can be both homonymous and polysemous; for example, "bank."

occasions, the speaker meaning of E may be a different pragmatic modification, M3. So, typically, E is not thereby ambiguous and the polysemy is pragmatic.

On SEM, my view, the striking examples of polysemy that partly motivate linguistic pragmatism do not show the need for a new theoretical framework but rather that some ambiguities have been overlooked. Typically, what the speaker does in using a polyseme is not modifying a conventional meaning but select one meaning from her mental lexicon, just as she does in using a homonym.

Finally, a person's lexicon is constituted by "entries," each a competence matching a word with its meaning in her language. I presume only a minimal view of such a lexical entry: It is a disposition to associate a word (a physical form) with a certain meaning in production and understanding.

I turn now to my prediction that the psycholinguistic literature would be unlikely to cast much light on SEM–PRAG, the semantics–pragmatics issue as I have presented it.

2 | PREDICTION: NOT MUCH HELP FROM PSYCHOLINGUISTICS

In this section, I shall emphasize the heavy onus on PRAG, and hence how unlikely it is that psycholinguistics will provide the evidence that would fulfill the onus.

First, for reasons I have presented elsewhere (2007, pp. 11–16), the mere existence of a pragmatic derivation of one meaning from another that linguists can give, even one that speakers can give, is far from sufficient to show that a speaker meaning should be explained pragmatically.[9] Rather, for a pragmatic explanation to be adequate, it must place that pragmatic derivation actively within the cognitive lives of speakers and hearers: There is a "psychological-reality requirement" on PRAG. Here is how I now sees this.

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 speakers' 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*

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[9]The idea that the mere existence *is* sufficient accompanies the common construal of Grice's "Modified Occam's Razor": "Senses are not to be multiplied beyond necessity" (1989, p. 47). I label adherence to the Razor, so-construed, the "Third Methodological Flaw of Linguistic Pragmatism" (2013c).

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 *speakers* meet this psychological-reality requirement is particularly important for two reasons. First, because what speakers do *constitute* any implied meaning.[10] Second, evidence of the required nonconvention-exploiting process should be easier to find in speakers than in hearers. For, in hearers 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.[11] (Li & Slevc, 2016, p. 4)

There is no such problem in speakers because there is no analogue of the hearers' mind-reading processes 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 (2016) remark, "most cognitive research on polysemes has focused on comprehension, so little is known about the representations of polysemous words in the production system" (p. 4). They themselves focus on production but cite no examples of *any* previous researchers doing so. From the SEM–PRAG perspective, the research has had the wrong focus.

Now suppose that we lack independent evidence of processes in speakers and hearers that satisfy the psychological-reality requirement. This is very bad news for PRAG. For, in the absence of that independent evidence, there are powerful Occamist and developmental objections to PRAG.

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 SEM, the semantic rival to PRAG. This is a crucial part of our

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[10]I label the widespread practice of taking the *hearer's* pragmatic inferences to be constitutive, the "Second Methodological Flaw of Linguistic Pragmatism" (2013c).

[11]Psycholinguists tend to talk of the "meanings" of homonyms but the "senses" of polysemes. I treat those terms as synonyms.

background knowledge in assessing SEM and PRAG. SEM 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 PRAG requires, then we should prefer SEM over PRAG on Occamist grounds.*[12]

Developmental objection: There is good reason to suppose, a priori, that the different psychological processes required by PRAG do *not* exist. In an influential article that I shall discuss in Section 4, two psycholinguists, Devorah Klein and Gregory Murphy, make a pertinent remark. They are addressing, in effect, a version of PRAG according to which a polysemous word has just one core conventional meaning from which its other senses are pragmatically derived:

> 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. (Klein & Murphy, 2001, pp. 276–277)

It would indeed be very surprising. Consider a word E that conventionally meant M1 and was then used for the first time to convey a related meaning M2 in a successful communication. That success depended on the speaker and hearer consciously going through the mind-reading processes we have described. Suppose that this success led other speakers to use E to mean M2, thus establishing a pattern of communicative success in so doing. *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. Any such word has a meaning as a result of convention. How did this convention come about? Setting aside the occasional meaning stipulation, we can presume that mind-reading processes in speakers and hearers using the word led to successful communications and hence to the convention. Now, given that these processes for monosemes and homonyms developed into conventions why would the similar ones for polyseme E not do so? 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 monosemes and homonyms *and hence a language.* Why would we have denied ourselves that benefit with polysemes? To suppose that we have denied ourselves is like supposing that the graduate students who meet for drinks at O'Reilly's on Fridays 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 (1969) brought out with a range of examples (pp. 36–42), conventions enable us to co-ordinate our actions to our mutual benefit, whether in communication, meeting for drinks, or whatever, *without the complicated*

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[12]Robyn Carston (2021) is not convinced. She insists that we *do* know of the existence of the processes that PRAG requires: 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. And, we should not forget, it gets worse for PRAG: It faces the developmental objection.

mind-reading that brought them about. That, we might say, is the point of conventions. In sum, PRAG is developmentally implausible.

Given the Occamist and developmental objections, if psycholinguistics is to cast light on the SEM–PRAG issue, it will have to provide persuasive evidence of *processes that meet the psychological-reality requirement on PRAG.* Given how difficult it is to discover fine-grained information about the mind, I predicted that psycholinguistics would not be providing this evidence. Psycholinguistics is, of course, providing all sorts of evidence about linguistic processing but, I will argue, it is not providing the evidence that PRAG needs: My prediction was right.

To illustrate the bearing of this discussion on SEM–PRAG, consider Ingrid Falkum's (2015) nicely clear and provocative paper in which she argues for a "radical pragmatic" account of polysemy and against any "rule-based" account like SEM (p. 85). Her account is 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. (p. 92)

"Lion" has an encoded meaning that refers to a charismatic feline but is used in (20) to convey the message that John is "strong, courageous, takes 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 good account of how a hearer might infer these intended meanings from the stated encoded ones and hence of how the pragmatic explanation might go (pp. 92–93). 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. (Falkum, 2015, 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. (Falkum, 2015, p. 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 shortcuts," then Falkum has indeed described a case to be explained pragmatically. But, *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 encoded in the language of the café employees. Such a term has become ambiguous in their idiolects and is covered by SEM. 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.[13] Fourth, there is indeed "a pragmatic basis" for a word's *gaining* a new linguistic meaning in this way, thus *becoming* a new lexical entry.[14] For, the new meaning was created by regular pragmatic modifications by speakers, as probably just about all new meanings are. 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." Cancelability is no help in choosing between SEM and PRAG.

So much for the experimental evidence we *need*. I turn now to the experimental evidence we *have*.

3 | POLYSEMY IN PSYCHOLINGUISTICS

3.1 | Introduction

Interest in polysemy has recently surged in psycholinguistics, as Falkum and Agustin Vicente (2015) bring out in a helpful survey. I shall consider some of the experimental evidence that this has produced in Section 4. But first, I shall do preliminary work to bring the psycholinguistic literature to bear on our SEM–PRAG issue. 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 "lexical semantics" that SEM should not embrace.

James Pustejovsky describes one view that plays a prominent role in psycholinguistic discussions 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, p. 34). This view, SEL, is a version of SEM (as stated in Section 1) but has two important

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[13]The line of argument here is similar to one I aimed at Kent Bach's use of "standardization" (2013d, pp. 192–195).

[14]Carston (2021) also emphasizes this.

commitments that SEM lacks, as we shall see (Section 3.2, Section 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, p. 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 (2018) describes "underspecification" as follows: "[T]he meaning of a polysemous term is an underspecified, abstract, and summary representation that encompasses and gives access to its different senses" (p. 952). Steven Frisson (2009) 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" (p. 116; see also Ruhl, 1989, p. viii). 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 this enrichment process to be a pragmatic modification of some sort; so, underspecification is an "abstract-core" version of PRAG. 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).

The idea that a polyseme has an *abstract* core meaning may have initial pragmatist appeal with some polysemes—for example, "cut" and "bear"—but, I suggest, it should not have with others. Thus, consider Falkum's discussion above. Her plausible "pragmatic routine" derives meanings from conventional meanings that are quite *un*-abstract: from "lion" meaning a certain feline and from "the ham sandwich" meaning a certain food. Furthermore, and importantly, the derivations are from meanings the polysemes *indubitably have*. The abstract-core version of PRAG lacks this encouraging feature. This makes it particularly problematic and open to the "abstract-core objection" that there are good reasons to doubt that words have such abstract meanings. I have presented these reasons elsewhere (Devitt, Forthcoming, Section 11.5).

Vicente (2018) describes "overspecification" as follows: "[T]he 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" (p. 952). Pustejovsky's *The generative lexicon* (1995) urges many examples. Overspecification, like SEL, is a version of SEM with additional commitments: It has SEL's two and adds another (Section 3.3).

3.2 | "Represented and stored"

Falkum and Vicente describe one of the "two main concerns in the contemporary discussion" that they have identified: "[T]he 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 polysemes, unlike of homonyms, 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 view of the mental lexicon I presumed in Section 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 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 similarly novel interpretation of E on the assumption that its lexical meaning has been thus modified. Everyone agrees that there are such processes. 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. And that, like all other linguistic meanings, will come to be "stored" as a disposition in the lexicon of speakers and hearers. But suppose that this did not happen: Suppose rather that the modifying and interpreting events become 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. And, as I insisted in Section 2, given the Occamist and developmental objections, the account badly needs evidence of these regular processes in speakers and hearers that differ from the standard processes of disambiguation. 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.[15] 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 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 that is much more robust. On this meaning, representing is a *referential* relation between something that represents and something that is purportedly represented. This meaning yields the "high-key" view that a sense of, say, the word "rabbit" is represented in the mind in that there is a lexical entry that *designates* "rabbit" 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 psycholinguistic views that concern

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[15]For discussion, see Devitt (2006a, pp. 5–7). For an indication of the trouble that "represent" can bring, see the fascinating exchange: Rey (2003a), Chomsky (2003), and Rey (2003b).

us.[16] 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 (2006a, pp. 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.

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...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, Bouwsema & Klepousniotou, 2015, pp. 126–127)

Perhaps polysemes are stored as "unified lexical representations" (Li & Slevc, 2016, p. 1).

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 not semantic: 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):

> [A]ll 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 & Murphy, 2001, p. 261)

This "single-entry" PRAG view of senses unified under a core meaning—our earlier version of "underspecification" (Section 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

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[16]"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, p. 111).

together under a single lexical entry.[17] Yet, how *could* this be so? 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*?

One answer seems to be that where one meaning of a word is derived from another in context *by a linguistic rule*, that is seen as unifying the meanings in the one lexical entry. And this unification is thought to yield "overspecification" (Section 3.1). Alleged examples include regular polysemy, cocomposition, and "dot objects" (Pustejovsky, 1995). So the unified single-entry view is compatible not only with PRAG and underspecification but also with overspecification. And it is important for us to note that overspecifications are semantic not pragmatic explanations of polysemy. Their derivations in context differ crucially from those of the just-discussed PRAG view: They are derivations governed by linguistic rules not pragmatic modulations; they are language-internal *semantic* generations.

So, these alleged unifications by overspecification yield versions of SEM. But SEM has no commitment to any of these unifications. And I do not buy any (Devitt, Forthcoming).

Beyond all this, one wonders whether the talk of single storage could 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 above on the psychological-reality requirement. So that is the evidence we should look for.

3.4 | An "information-rich" lexicon?

Falkum sees "two approaches to polysemy": rule-based or code-based versus 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. (Falkum, 2015, p. 85; see also pp. 90, 97)

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[17]See for example: Pustejovsky (1995), Klepousniotou and Baum (2007), Frisson (2015), MacGregor et al. (2015), and Li and Slevc (2016).

With two crucial qualifications, Falkum's account of the "code-based" or "rule-based" view—the earlier SEL is an example (Section 3.1)— captures SEM's view of polysemy. And she has captured well the view of language that underlies SEM. She has also summarized well the pragmatist approach and its underlying view of language, *as typically presented*. But, for the reasons set out elsewhere (Devitt, 2013c), presenting PRAG as a *pragmatic-inference*-based approach conflates what *constitutes* the utterance's meaning with *how the hearer tells* what the meaning is. 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 inferences in the hearer but by what the speaker does: It is provided by the enrichments or 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. (Falkum, 2015, 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 (Section 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 the linguistic field of 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[18]; for example, one lexical entry for the ambiguous word "bank" relates it to "financial institution," another, to "shore" (Pustejovsky, 1995, p. 34). This view that the meanings allegedly represented are rich is controversial in philosophy for reasons that I can only indicate *very* 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.

The idea that lexical meanings are rich, whether represented or not, lacks a good motivation, as Fodor and Lepore (1998) demonstrate. We are supposed to need the richness to explain "the inferences that a language reasoning system must perform in order to understand a sentence" (Pustejovsky, 1995, p. 19) and "how words are semantically related to one another" (p. 23). But we do not need the richness for these purposes. That explanatory work can be done by combining a "*poor*" lexicon with *our knowledge of the world*. Thus, consider a person with lexical entries for "bachelor" and "unmarried" that dispose her to associate them with the "poor" meanings, *refers to bachelors* and *refers to unmarrieds*, respectively. The combination of these dispositions with the *worldly* knowledge that bachelors are unmarried readily explains the relation between "*x* is a bachelor" and "*x* is unmarried." This is not to say that the lexicon *is* so "poor." It is just to say that we do not need anything richer to explain the inferential relations. If we need anything richer, in my view, it is to explain reference (Devitt, 1996).

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[18]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).

Not only is the rich view of the lexicon unmotivated, but the revolution in the theory of reference that has swept philosophy in the last 50 years, initiated by Saul Kripke (1980) and Hilary Putnam (1975), shows that the rich view is probably false for many lexical entries. For, the rich view is committed to "description theories" of reference of just the sort that the revolution has shown to be often dubious (see Devitt & Sterelny, 1999 for details).[19]

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 she is disposed to use E to express. 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. And the issue of "how the sense of a word is represented" in the person's mind, understood in the preferred low-key way (Section 3.2), is simply the issue of what inferential and/or direct relations constitute that sense.

Falkum's nice summary of "rule-based approaches" should be modified by simply dropping "information-rich." 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 takeaway on this is:

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.

We need to keep (1) to (3) in mind as we consider the experimental evidence.

4 | PSYCHOLINGUISTICS AND THE EXPERIMENTAL EVIDENCE

4.1 | Introduction

In their opening remarks, Klein and Murphy (2001) sum up the state of the evidence at that time as follows: "[T]here 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

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[19]It is striking that this revolution seems to have had no impact at all on the literature that concerns us; thus, it goes unmentioned in Pustejovsky (1995), Falkum and Vicente (2015), Falkum (2015), and Vicente (2018). Kripke's refutation of the description theory of proper names, which is the centerpiece of the revolution, has recently received powerful empirical support (Domaneschi, Vignolo, & Di Paola, 2017; Devitt & Porot, 2018; cf., Vicente, 2018, p. 962).

Vicente (2015) report: "The lack of priming effects found for the senses of their [Klein & Murphy, 2001] polysemous words led them to conclude that these were represented in the same way as homonyms" (p. 4). This was thought to support SEL, the sense enumeration view, which is a "high-key" information-rich version of SEM. But Vicente (2018) reports, citing MacGregor et al. (2015) in particular, "recent psycholinguistic studies question [SEL]" (p. 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 (Section 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 by 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 (Bambini, Ghio, Moro & Schumacher, 2013; Klepousniotou & Baum, 2007). 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, it might not be: It might be just her causal route to the lexical item for M2.

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 PRAG: 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.

4.2 | Klein and Murphy (2001)

Klein and Murphy (2001) 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" (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 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. 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 would the single-entry core-meaning view not predict that it will be easier to remember the word in the former case than in the latter? 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. (Klein & Murphy, 2001, 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. (Klein &
> Murphy, 2001, p. 269)

These results are perhaps a bit surprising from the SEL perspective but they clearly do not count against SEL. Do they count against the 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. (Klein & Murphy, 2001, p. 273)

Why? Why could the derivation of a modified sense *paper-stuff* from "wrapping paper" not 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 do not 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. (Klein & Murphy, 2001, 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.

4.3 | MacGregor et al. (2015)

MacGregor et al. (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, p. 958). Once again, I am 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"). (MacGregor et al., 2015, p. 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. (MacGregor et al., 2015, 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). (MacGregor et al., 2015, 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. (MacGregor et al., 2015, pp. 136–137)

So MacGregor et al. take their results to support a single-entry view. They then cautiously enter-tain both the underspecification and overspecification versions of this (p. 137).

But do the results really support the single-entry view? I think not. They seem quite compati-ble 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 (Section 4.1). 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. 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 evi-dence that bears on SEM–PRAG.

4.4 | Frisson (2015)

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 representa-tion. (Frisson, 2015, p. 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) discussed in Section 4.2. 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" (Frisson, 2015, p. 20).

This leads to the key prediction about the effect of dominance in the sensicality task for poly-semes. 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. 20–21). 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 do not 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. I turn now to Li and Slevc (2015) who have made what seems to be the only attempt to find such evidence.

4.5 | Li and Slevc (2015)

Li and Slevc (2015) 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 do not 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). (Li and Slevc, 2015, p. 1)

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*). (Li & Slevc, 2015, p. 1)

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. (Li & Slevc, 2015, 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 fulfill its explanatory onus.

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. (Li & Slevc, 2015, 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.

5 | CONCLUSION

SEM is the view that 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 message of an utterance is typically one of its encoded conventional meanings, with references fixed in context. Why believe this? Because it is the best explanation of the regularities involved.

The contrary view, PRAG, is that a pragmatic modification of meaning typically plays a role in constituting messages. This generates a "psychological-reality requirement": PRAG 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 (briefly) abstract-core objections which place a heavy evidential burden on PRAG. 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 SEM.[20]

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[20]A longer version of this paper, and a defense of SEM, are part of *Overlooking conventions* (Forthcoming).

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