(BY WAY OF AN) INTRODUCTION:
A FIRST DIALOGUE ON THE
SEMANTICS-PRAGMATICS INTERFACE

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> The subject is full of perplexities,
> but I think that our troubles arise from
> a failure to recognise that different parts
> of our language have meaning in different ways.
> (Kneale 1949:89)

P. Well, I think that Kneale is right, but I would suggest that what Cohen (1971, 1977) has called the Conversational Hypothesis be employed to confront some of these perplexities. The distinction between 'sense' and 'implicature' (both are terms of art, of course, but the scare quotes ought to keep us on our toes) is of enormous use. And the accompanying recommendations, that (a) senses not be multiplied beyond necessity and (b) 'it is more generally feasible to strengthen one's meaning by achieving a superimposed implicature, than to make a relaxed use of an expression' (Grice 1989:48), though not without their problems, both serve to define a potentially useful methodology.

S. Yes, I think I would agree, but your terms of art need to be firmed up a little. Stalnaker is helpful:

> Semantics, as contrasted with pragmatics, can mean either the study of *meaning* or the study of *content*. The contrast between semantic and pragmatic claims can be either of two things, depending on which notion of semantics one has in mind. First, it can be a contrast between claims about the particular conventional meaning of some word or phrase on the one hand, and claims about the general structure or strategy of conversation on the other. Grice's distinction between conventional implicatures and conversational implicatures is an instance of this contrast. Second, it can be a contrast between claims about the truth-conditions or *content* of what is said – the proposition expressed – on the one hand, and claims about the *context* in which a statement is made – the attitudes and interests of speaker and audience – on the other. (Stalnaker 1974:212).

I like this quote. It highlights an ambiguity in the word 'semantics' that too often goes unrecognised. It foreshadows some later work. And it also pushes attention to someone who seems to have fallen victim to that ambiguity. Grice, an early defender of the Conversational Hypothesis, seems to have, on several occasions, wanted to endorse an intention-based approach to conventional meaning, and, on other occasions – his defence of the propositional connectives, for example – a truth-functional approach to content. It is, I think, little wonder that the William James Lectures are so difficult to interpret (cf. Neale 1992; Cosenza 2001; Chapman 2005).

P.   Yes, Stalnaker was always ten years ahead of the pack. And you are right about Grice. But it is possible to go a little further. The notion of conventional meaning in his work, as well as in the work of others, is almost completely undefined. Let me return a quote:

> The claims for the explanatory power of Gricean principles of conversational inference rest upon a highly convincing if vague account of the RELATIONSHIP between the CONVENTIONAL MEANING of the IMPLICANS, its conversational role, and the resulting IMPLICATA. But for words and sentences the theory posits CONVENTIONAL MEANINGS that are controversial, while seeming to assume that the adequacy of such posits of conventional word/sentence-meaning can never be tested directly, but – and this is a truism in the theory – only by their contribution to the speaker's utterance-meaning of words and sentences uttered in contexts of actual use. The theory's great success is its convincing explanation of how and what speakers are understood to mean when patently they do NOT mean what their words conventionally do. ...the account is enlightening. But how much CAN this kind of theory tell us about the ACTUAL conventional meanings of English expressions? (Atlas 1979:270).

Atlas (1989, 2005) has subsequently developed a theory of semantic underspecification. And this bring us up to the present.

S.   How's that, exactly?

P.   Well, I'm thinking of the number of models that seem to be indebted both to Stalnaker's observation and to Atlas's efforts.

S.   Go on, explain a little more.

P.   OK. The current fashion seems to be for models that have a number of different kinds of semantics, each of them associated with their own pragmatics. Let's take a concrete example.

S.   OK.

P.   A template that seems to be common to a number of theories includes (a) some reference to linguistic meaning; (b) some account of truth-conditional meaning and (c) some calculation of contextual, or pragmatic, meaning. Different theories differ on the 'size' of (a), (b) and (c) and what they assume 'mediates' between these different domains.

S.   Does Relevance Theory employ this template?

P.   Well, yes, but I prefer to keep Relevance Theory out of the frame.

S.   Why's that?

P. Well, consider the following quote:

> The principle of relevance differs from every other principle, maxim, convention or presumption proposed in modern pragmatics in that it is not something that people have to know, let alone learn, in order to communicate effectively; it is not something that they obey or might disobey: it is an exceptionless generalisation about human communicative behaviour.

This quote appears in numerous places in the relevance theory literature (Sperber & Wilson 1986a:160; Wilson & Sperber 1988:140; Wilson & Sperber 1992:68). Something close to it appears in both editions of their book:

> The principle of relevance ... is a generalisation about ostensive-inferential communication. Communicators and audience need no more know the principle of relevance to communicate than they need to know the principles of genetics to reproduce. Communicators do not 'follow' the principle of relevance; and they could not violate it even if they wanted to. The principle of relevance applies without exception: every act of ostensive communication communicates a presumption of relevance. (Sperber & Wilson 1986b:162; Sperber & Wilson 1995:162).

S. I think I can see where this going.

P. Right. The principle of relevance, we are told, is an exceptionless generalisation. But being exceptionless suggests something stipulative, whereas being a generalisation suggests something empirical. Look at it this way: suppose you have seen one hundred swans and they all have been white. You can say 'All swans are white'. Then you see a black swan and your generalisation is now false. It has to be revised in some way. Compare that with another case: suppose you have seen one hundred swans and they all have been white. You can say 'All swans are white'. Then you see a 'black swan'. What do you do? Well, under the stipulation you point to the 'black swan' and you say 'That's not a swan'. Being a stipulation and being an empirical generalisation are two quite different things.

S. So Relevance Theory rests upon a category mistake.

P. I think so, yes.

S. It's almost a replay of Ryle versus Descartes.

P. It would appear so, yes.

S. Doesn't the 'Postface' (in Sperber & Wilson 1995:255–279) go some way to diminishing that objection. After all, in the 'Postface' they introduce some revisions – one of which is the distinction between (a) a cognitive principle of relevance and (b) a communicative principle of relevance. (a) states that human cognition tends to be geared to the maximization of relevance and (b) states that every act of communication communicates a presumption of its own optimal relevance. I think they call (a) the principle of relevance and (b) the presumption of relevance.

P. No, I don't think the revision helps. They go on to say that "[t]he change is, of course, expository and not substantive" (Sperber & Wilson 1995:261) so the original objection stands.

S. Yes, I think you are right, and we could add that a bit later they say something rather surprising.

P. What are you thinking of?

S. Well, later on they say:

> ...neither the principle nor the presumption of relevance is presented as a goal to be pursued or a rule to be followed by the communicator. The (Second) Principle of Relevance is a descriptive (as opposed to normative) claim about the content of a given act of ostensive communication. It claims that part of that content is a presumption that this very act of communication is relevant to the addressee. (Sperber & Wilson 1995:271).

P. Yes, that's puzzling. I conclude from this that the First Principle of Relevance (and I don't know why it has grown capital letters) is most probably, in their eyes at least, exceptionless and that the Second Principle (or Presumption) of Relevance is a generalization. They don't say that this is what they mean but this interpretation might help to get them out of a hole. The whole thing is far from clear, though.

S. But, if I am not mistaken, there is another, how shall we say, infelicity in the theory.

P. What are you thinking of here?

S. Well, as I understand it, Relevance Theory assumes that speakers and hearers are equipped with a grammar, a logic, an encyclopaedia or memory and the principle of relevance.

P. Yes, that's what I understand too.

S. Now, the logic part of this is a bit puzzling. The theory claims that the logic is deductive, but that it is restricted to elimination rules. You know, you can infer P or Q from $P \wedge Q$, by $\wedge$-elimination, but you can't infer $P \wedge Q$ from P and Q, because that step requires $\wedge$-introduction.

P. Yes, the theory requires that because with introduction rules, chains of inferences might never to come to an end. With $\vee$-introduction, for example, you can go from P to $P \vee Q$ to $(P \vee Q) \vee R$ and so on.

S. But what I don't understand, if the logic is deductive and eliminative, is how one can, using the principle of relevance, infer the propositional content from an underspecified linguistic meaning. Deductive rules operate on truth carrying propositions and they preserve truth. Deductive rules cannot operate on sub-propositional linguistic meanings because these things, whatever they are, are not true or false.

P. Yes, and to anticipate a bit, let me quote Levinson on this matter:

> ...owing to its explicit commitment to deductive reasoning, Relevance Theory appears especially incoherent: "no assumption is simply decoded [from an utterance], and ...the recovery of any assumption requires an element of inference" (Sperber & Wilson 1986b:182). But how are we to get from nonlogical forms to further contextual implications that will enrich them by a deductive process that can only handle logical forms? Theories that are abductive or inductive in character, or that are phrased in terms of an inference to the best explanation,

may not face this theoretical incoherence, but of course that does not erase the *computational* problem. (Levinson 2000:257).

S.   So my observation has already been anticipated?

P.   Yes, but the point deserves to be repeated.

S.   The overall Relevance Theory position has changed a little bit, though, recently, if I have been correctly informed.

P.   Well, you are right. The linear nature of the model – starting with logical form, then augmenting logical form into truth-conditional content with some application of the principle of relevance, then adding to content (or explicature, as we should now say), again with some application of the principle of relevance, to derive implicatures – has changed into a parallel model. Here's Carston:

> The mechanism that mediates the inferences from logical form to communicated propositions is one of 'mutual parallel adjustment' of explicatures and implicatures, constrained by the comprehension strategy. The result should consist of (sets of) premises and conclusions making up valid arguments, but *the important point is that the process need not progress strictly logically* from the accessing of premises to the drawing of conclusions. For instance, a particular conclusion, or type of conclusion, might be expected on the basis of considerations of relevance and, via a backwards inference process, premises constructed (explicatures and implicatures) which will make for a sound inference to the conclusion. The process may involve several backwards and forwards adjustments of content before an equilibrium is achieved which meets the system's current 'expectation' of relevance. (Carston 2002:139).

I've added a little emphasis.

S.   Is it really 'sound inference' that is meant here, or 'valid inference'?

P.   I don't really know – nor do I really know what a 'backward inference' is.

S.   OK. I can see why you want to keep Relevance out of the frame.

P.   OK.

S.   So what does fit the template that you mention?

P.   Well, let's try Truth Conditional Pragmatics.

S.   OK. What's that?

P.   Let me quote a version. (This is a bit long but it's as well to stick to the originals).

> . . . there are several **levels** of meaning. When an utterance is made, the sentence-type that is uttered possesses a linguistic meaning (level 1). More often than not, that meaning is not a complete content: to get a complete content, one must resolve indeterminacies, assign values to indexical expressions, etc. The richer meaning thus determined is the literal content of the occurrence which depends not merely upon the conventional significance of the expression-type, but also on features of the context of use (level 2). At level 3, we find aspects of meaning that

> are not part of the literal content of the utterance. Those aspects of meaning are not aspects of what is said. Rather, the speaker manages to communicate them indirectly, BY saying what she says. Conversational implicatures and indirect speech acts fall into that category. This division into three levels – linguistic meaning, literal content, and conveyed meaning – is incomplete and very rough, but it will do for my present purposes. (Recanati 2004b:457).

    I endorse that last sentence.

S.   This seems to be a very full statement of the template that you mentioned.

P.   Yes, what distinguishes, I think, Truth Conditional Pragmatics from, say, Relevance Theory, is that there are different kinds of pragmatics mediating between linguistic meaning and literal content, on the one hand, and literal content and conveyed meaning, on the other.

S.   Relevance Theory claims that the principle of relevance does both jobs.

P.   Yes.

S.   But we have seen that the principle of relevance is designed in a rather general fashion and it is surprising if it is able to do anything at all.

P.   Yes, Truth Conditional Pragmatics posits two kinds of pragmatic processes – primary and secondary.

S.   What's the difference?

P.   Well, and I have to preface any remarks with the words 'As I understand it...'

S.   I know what you mean.

P.   As I understand it...as I understand it ...erm ...as I understand ...oh dear, I've lost it again. Let me go back to the original.

S.   OK.

P.   Here goes:

> I distinguish between two sorts of pragmatic process. The contextual processes which are (subpersonally) involved in the determination of what is said I call *primary* pragmatic processes. In contrast, *secondary* pragmatic processes are ordinary inferential processes taking us from what is said, or rather from the speaker's saying of what is said, to something that (under standard assumptions of rationality and cooperativeness) follows from the fact that the speaker has said what she has said. To the extent that the speaker overtly intends the hearer to recognize such consequences as following from her speech act, they form an integral part of what the speaker means by her utterance. That is, roughly, Grice's theory of 'conversational implicature' (Grice 1989). An essential aspect of that theory is that the hearer must be able to recognize what is said and to work out the inferential connection between what is said and what is implied by saying it. Again, it follows that what is said must be consciously available to the interpreter. It must satisfy what I call the Availability constraint. (Recanati 2004c:51; cf Recanati 2004a:17).

Or again: (sorry to go on but I find this position rather puzzling and I'm hoping you will be able to infer the overall perspective from a number of quotes).

S.   OK. Let's have it.

P.   The determination of what is said takes place at a sub-personal level, much as the determination of what we see. But the determination of what the speaker implies takes place at the personal level, much like the determination of the consequences of what we see. (Seeing John's car, I infer that he did not leave). The crucial fact is that pragmatic, background-dependent processes may well take place at a sub-personal level in an automatic and non-reflective manner. Such processes are not 'inferential' in the strong sense in which secondary pragmatic processes are inferential. (Recanati 2002:114).

S.   OK. But make that a little more concrete for me. Hang some examples on it.

P.   Well, on the assumption that secondary pragmatic processes are not too distant from the familiar Gricean sort of things, I think we only need to talk about the primary pragmatic processes. Would that be fair?

S.   I think so, yes.

P.   So, these processes contribute to the constitution of literal meaning or 'what is said'. Some examples that are used to illustrate this constitution include (linguistic meaning) 'I have nothing to wear' which is transformed into (truth conditional/literal meaning) 'I have nothing suitable to wear for this evening' and (linguistic meaning) 'I have had breakfast' which is transformed into (truth conditional/literal meaning) 'I have had breakfast today'.

S.   I think I am getting the idea. But what does Truth Conditional Pragmatics 'take to the bank', as the Americans say?

P.   Hmm, that's none too clear. In Truth Conditional Pragmatics such terms as 'expansion', 'strengthening' and 'enrichment' are used to label the kinds of processes that have these kinds of effects, but such labels are only defined ostensively and I have to admit that I do not find such definitions entirely helpful. It's not always easy to tell these processes apart.

S.   And what about the Availability Constraint?

P.   Well, in Truth Conditional Pragmatics it is assumed that the content of truth conditional meaning, unlike linguistic meaning, is consciously available. Back to the originals:

> *Availability Principle*: In deciding whether a pragmatically determined aspect of utterance meaning is part of what is said, that is, in making a decision concerning what is said, we should always try to preserve our pre-theoretic intuitions on the matter. (Recanati 1989:310).

S.   Erm, wait a minute. Who does the 'our' refer to in that quote?

P.   Well, it had previously been remarked that this

> presupposes that what is said by an utterance is available or acceptable to the unsophisticated speaker-hearer. 'Available' must be understood here in a strong sense: what I mean is not that what is said by an utterance is tacitly identified at some sub-doxastic level, but that it is accessible to our ordinary, conscious intuitions. (Recanati 1989:310).

S. That sounds like the sort of claim that ought to be put under experimental examination. I've been browsing through Noveck & Sperber (2004), without, I should add, coming to any firm conclusions just yet.

P. And Truth Conditional Pragmatics is coming in for a more general examination in Frápolli (forthcoming). Until then, Bezuidenhout (2002) is not too bad a read.

S. Are there any other theories that posit three levels of meaning?

P. Well, it's funny that you should use that expression – three levels of meaning – as that is the title of a very interesting paper (Levinson 1995) that introduces the Theory of Generalized Conversational Implicature.

S. What's the idea here?

P. The Theory of Generalized Conversational Implicature, or TGCI from now on, is a theory of utterance-*type* meaning, as distinct from a theory of utterance-*token* meaning. The idea is that there are implicatures which are relatively steady across contexts. Take a couple of examples, both from Levinson (1995) (where PCI is short for 'particularized conversational implicature' and GCI for 'generalized conversational implicature'):

> **Context 1.** A: What time is it?
>   B: Some of the guests are already leaving.
>
>   PCI: It must be late.
>   GCI: Not all of the guests are leaving.
>
> **Context 2.** A: Where's John?
>   B: Some of the guests are already leaving.
>
>   PCI: Perhaps John is already leaving.
>   GCI: Not all of the guests are leaving.

S. OK. These are nice examples. I can see that certain inferences are relatively context free. But where does the theory say these inferences come from?

P. It's claimed there are three heuristics which license these readings. There is the Q-heuristic, which states that 'what isn't said to be the case is not the case'. The GCIs in Contexts 1 and 2 arise from this heuristic. Second, there is the I-heuristic, which states that 'what is said in a simple, or unmarked, way represents a stereotypical situation' and finally there is the M-heuristic which states that 'what is said in an abnormal, or marked, way represents an abnormal situation'.

S. OK, and just for concreteness, what would be illustrative examples of I and M?

P. Well, we can take some examples straight from Levinson again. (This time his 2000:141):

> 1. Larry stopped the car.
>   I-implicates that Larry caused the car to stop in the normal way, by using the foot pedal, whilst
> 2. Larry caused the car the stop.
>   M-implicates that Larry caused the car to stop in a nonstereotypical way, e.g., by using the emergency brake or by running it into a pillar box.

S. We'll be hearing from Larry a little later, I think.

P. Yes. He's the first up. Or:

> 3. The Spanish killed the Aztecs.
>   I-implicates that the Spaniards slaughtered the Aztecs directly, whereas
>
> 4. The Spanish caused the Aztecs to die.
>   M-implicates that the Spanish killed the Aztecs, by, for example, disease or hard labour.

S. This is all very interesting.

P. Yes, I think so. At least the TGCI is a lot more *explicit* than Truth Conditional Pragmatics. I'm still impressed by those few lines that one finds in the Preface to *Syntactic Structures*.

S. What are those?

P.
> Precisely constructed models for linguistic structure can play an important role, both negative and positive, in the process of discovery itself. By pushing a precise but inadequate formulation to an unacceptable conclusion, we can often expose the exact source of this inadequacy and, consequently, gain a deeper understanding of the linguistic data. (Chomsky 1957:5).

S. I agree entirely.

P. But there is something more in the TGCI that we should dwell upon.

S. What's that?

P. Well, something called 'pragmatic intrusion'.

S. I think I know about that. It's when, in cases like comparatives and conditionals, implicatures contribute to truth-conditions.

P. That's right.

S. Although the name is not particularly felicitous.

P. How do you mean?

S. Well, Levinson, for example, talks about intrusive constructions (e.g., Levinson 2000:198–217), but this name suggests that it is the constructions themselves which are intrusive but, in fact, the constructions are intruded upon. They are the recipients of intrusion, not the perpetrators of it.

P. Yes, this point has already been made:

> The label 'intrusive construction' seems an odd usage to me, since the point surely is, not that these constructions are themselves intrusive, but rather are 'intruded upon' by pragmatically inferred meaning, that is, they are 'pragmatically penetrable'. (Carston 2004:81 fn5).

S. So my observation, once again, has already been anticipated?

P. Yes, but the point merits repetition. As does the point about the facts of intrusion having far-reaching consequences for the architecture of a theory of the semantics/pragmatics interface. Levinson says:

> There is every reason then to try and reconstrue the interaction between semantics and pragmatics as the intimate interlocking of distinct processes, rather than, as traditionally, in terms of the output of the one being the input of the other. (Levinson 2000:242).

S.   But one can go back a little bit.

P.   Where to?

S.   Well, Levinson had earlier, in a discussion of discourse representation and theories designed to account for that, introduced an evocative metaphor:

> ...there is a common slate, a level of propositional representation, upon which both semantics and pragmatics can write – the contributions may be distinguished, let's suppose, by the color of the ink: semantics in black, pragmatics in red. Semantics and pragmatics remain modular "pens" as it were: they are separate devices making distinctively different contributions to a common level of representation. (Levinson 2000:193).

P.   So *that's* where semantics meets pragmatics – on the common slate.

S.   Maybe, yes.

P.   I like that metaphor.

S.   So do I. But it should not be forgotten that there is an alternative.

P.   I think I know what you are going to say.

S.   Well, I'm impressed by the following bit of honesty:

> There will always be doubts about whether a better semantic analysis of the relevant construction might not accommodate the apparent pragmatic intrusions in some other way. (Levinson 2000:214).

P.   Yes, that's what I thought you were going to say.

S.   And, in fact, there has recently been an attempt to deal with these facts in some other way.

P.   You mean King & Stanley (2005).

S.   Yes, there is no need to rehearse their arguments here, but they do provide a plausible account that gives a little more descriptive and analytical priority to semantics. Their argument is especially interesting because the semantic/pragmatic analysis is embedded in the context of a discussion of what *philosophers* should be understood to mean when they talk, in epistemology, for example, about what knowledge claims like 'I know' mean.

P.   There is a lot more that one could say, of course.

S.   Well, there is. We could go back to Stalnaker and discuss his project of a formal pragmatics.

P.   Yes, our discussion has been a little informal so far.

S. Yes, Stalnaker recommends that a formal pragmatics be based on possible worlds:

> Formal pragmatic theory begins, as do the semantic theories that have been most fruitfully applied to natural languages, with possible worlds. Possible worlds semantics is an appropriate framework for pragmatic theory, not just because it has proved to be an elegant, flexible and technically fruitful apparatus, but because it makes possible an explanation of content and context in terms of an essential feature of discourse, and more generally of rational activity. It is a common, and I think defining feature of rational activities ... that they involve agents distinguishing among alternative possibilities. (Stalnaker 1981:441).

P. Put me straight if I am wrong but possible worlds don't seem to have, how shall we say, big box office these days.

S. No, they don't. Some years ago the following minimal pair was noticed:

> 5. (a) Exactly one of the ten marbles is not in the bag.
>   (b) It is under the sofa.
> 6. (a) Exactly nine of the ten marbles are in the bag.
>   (b) It is under the sofa.

These examples were first noticed by Barbara Partee and first reported in (Heim 1982:21).

P. And what conclusions does this pair point to?

S. Well, the third person singular pronoun in 5(b) is understood as referring to the marble not in the bag but the pronoun in 6(b) is not so understood. Therefore, 5(a) and 6(a) must provide different kinds of contexts for 5(b) and 6(b). But the sentences 5(a) and 6(a) are compatible with exactly the same set of possible worlds. The conclusion is therefore that possible worlds are not sufficiently fine-grained for an adequate analysis of this kind of data. Kamp is very clear on the matter:

> We must conclude that no differences [between 5(a) and 6(a), S] can be predicted if contexts are identified with sets of possible worlds. Therefore, a theory of meaning and context dependent interpretation of English must, if it is to handle such examples successfully, adopt a representation of contexts that goes beyond what sets of possible worlds are able to reveal. (Kamp 1988:158).

P. I see. So, as a result of these observations about anaphora, possible worlds don't have the audience that they once enjoyed and semantics and pragmatics are not going to find a suitable meeting place here.

S. Not really, to both observations, although there is large amount of introductory and survey literature appearing at the moment – for example, Divers (2002); Girle (2003) and Melia (2003) – that might stimulate a renewed interest. Discourse representations are, at the moment, one of the preferred currencies.

P. So, possible worlds are waning. They *did* come with a lot of baggage, I suppose, rather too much intensionality.

S. Yes, Davidson spoke about this early on:

> There is ... a danger that the know-nothings and the experts will join forces: the former, hearing mutterings of possible worlds, transworld lines, counterparts, and the like, are apt to think, *now* semantics is getting somewhere – out of this world, anyway. (Davidson 1973:78).

P. It's funny that you should mention Davidson.

S. Why's that?

P. Well, I understand that, just as possible worlds have been waning, Davidsonian Semantics has been waxing, or maybe, re-waxing, if there is such a word.

S. Yes, that seems to be at least partly the case. I think you must be thinking of such recent publications as Borg (2004) and Cappelen & Lepore (2005).

P. Yes, I think so. And Lepore & Ludwig (2005).

S. I haven't read all of these properly yet so I'm not really in the best position to discuss them just yet. Perhaps if we do this again at some time in the future, perhaps we could do it then.

P. OK.

S. But, judging from Borg's and Cappelen and Lepore's titles, at least, the Davidsonian Program still endorses a very small semantics, and leaves many things that cannot be brought into extensional truth conditions to pragmatics.

P. So, on the surface at least, this Program is maybe conceptually and strategically compatible with Grice's.

S. Yes, on the surface at the least, and maybe not just on the surface. We'll return to it.

P. OK.

S. Erm, just to go back a bit, to something you said earlier.

P. Yes.

S. You said that the notion of conventional meaning in Grice's work is almost completely undefined.

P. Well, that was a bit of a rhetorical flourish. Grice (1989:87–88) has a number of suggestions to make on this matter, but he is careful to say that he is merely providing 'a sketch of direction, rather than a formulation of a thesis' and he adds that the formulation he comes up with contains a 'hideous simplification' (Grice 1989:87).

S. So the notion is not 'almost completely undefined'?

P. No. But take a look at what he says:

> I want to say that (1) "U (utterer) said that p" entails (2) "U did something x by which U meant that p." But ... many things are examples of the condition specified in statement (2) which are not cases of saying. For example, a man in a car, by refraining from turning on his lights, means that I should go first, and he will wait for me.
>   Let us try substituting, for (2), (2'):

> "U did something x (1) by which U meant that p
>
> > (2) which is of a type which means 'p'." (that is, has for some person or other an established standard or conventional meaning). (Grice 1989:87)

He goes on to say: 'There is a convenient laxity of formulation here' (Grice 1989:87). And his next attempt, running to five clauses, is the one that contains a 'hideous oversimplification'. Perhaps I will be forgiven if I don't repeat it here.

S.   Oh, I think I would like to see it.

P.   Now, why's that exactly?

S.   Well, I think I might have a useful observation to make, a little later.

P.   OK, then. Grice says:

> We want doing x to be a linguistic act; with hideous oversimplification we might try the formulation:
>
> "U did something x (1) by which U meant that p
>
> > (2) which is an occurrence of an utterance type S (sentence) such that
> >
> > (3) S means 'p'
> >
> > (4) S consists of a sequence of elements (such as words) ordered in a way licensed by a system of rules (syntactical rules)
> >
> > (5) S means 'p' in virtue of the particular meanings of the elements of S, their order, and their syntactical character. (Grice 1989:87).

He goes on:

> I abbreviate this to:
>
> "U did something x (1) by which U meant that p
>
> > (2) which is an occurrence of a type S which means 'p' in some linguistic system"

> This is still too wide. U's doing x might be his uttering the sentence "She was poor but she was honest." What U meant, and what the sentence means, will both contain something contributed by the word "but", and I do not want this contribution to appear in an account of what (in my favoured sense) U said (but rather as a conventional implicature). (Grice 1989:87–88).

There. That's it. You can probably see why I didn't want to go there.

S.   Yes. Thanks.

P.   You're very welcome.

S.   But what Grice outlines here as an analysis of what is said, in his favoured sense, is a long way from the direction that subsequent inquiry took.

P.   There is a problem, yes. Levinson says:

> Grice uses the phrase *what is said* as a technical term for the truth-conditional
> content of an expression, which may in fact be somewhat less than the full con-
> ventional content. (Levinson 1983:97, fn1).

Levinson squirrels this remark away in a footnote but it is reasonably clear that Grice had
something else entirely in mind about this.

S.   And the analysis of 'what is said' is something of an expanding market at the moment. Per-
haps I might employ the conversational equivalent of a footnote and just list, in alphabetical
order, some of the main contributions: Bach (2001); Hawley (2002); Recanati (1989, 2001,
2004c); Saul (2002a,b); and Ziff (1972).

P.   Sure.

S.   OK. Let's leave conventional meaning and move onto conventional implicature. This is
another way that our language can have meaning, isn't it? Grice doesn't say too much about
this kind of meaning.

P.   He says that 'the nature of conventional implicature needs to be examined before any free
use of it, for explanatory purposes, can be indulged in' (Grice 1989:46), though he doesn't
do that examination himself, no.

S.   That's my impression.

P.   But he does say that a difference between a conversational and a conventional implicature
is that the former must be capable of 'being worked out', and that 'it is all too easy to treat
a generalized conversational implicature as if it were a conventional implicature' (Grice
1989:37).

S.   OK. So where are we with conventional implicature. The classification looks unhelpful.

P.   Enter Horn:

> ... conventional implicature remains a controversial domain. While it continues
> to be invoked to handle non-truth-conditional aspects of lexical meaning, this
> tends to constitute an admission of analytic failure, a label rather than true expla-
> nation of the phenomenon in question. It has on occasion been maintained that
> conventional implicature is a myth (Bach 1999), and even for the true believers,
> the domain in which such implicatures have been posited continues to shrink,
> eaten away on one side by an increasingly fine-grained understanding of truth-
> conditional meaning and entailment ... and on the other by a more sophisticated
> employment of the tools of conversational implicature. While conventional im-
> plicature remains a plausible *faute de mieux* account of particles like *even* and
> *too*, whose contribution has not convincingly been shown to affect the truth con-
> ditions of a given utterance but is not derivable from general considerations of
> rationality or cooperation, the role played by conventional implicature within the
> general theory of meaning is increasingly shaky. (Horn 2004:6).

S.   Increasingly shaky, eh?

P.   Well, no. Or rather, not necessarily.

S. Why's that?

P. Some stability has been brought into the picture by Potts (2005). He presents a bold reanalysis of conventional implicatures that models them with a type-driven multi-dimensional semantic translation language, working out the classical idea of Karttunen & Peters (1979) on a three dimensional semantics.

S. Semantic?

P. Yes. His first sentence in Chapter 1 is: 'I hope readers of this book are struck by how little pragmatics it contains'.

S. But conventional implicatures as semantic? How does he get to that claim?

P. He takes some quotes from Grice. He takes this one:

> In some cases the conventional meaning of the words used will determine what is implicated, besides determining what is said. (Grice 1989:25).

Of this, he says:

> The phrase 'the conventional meaning of the words' is the crux of this statement, since it locates CIs [this is a Potts-abbreviation for conventional implicatures] in the grammar. The 'conventional' part of 'conventional implicature' stands for 'not calculable from the conversational maxims and the cooperative principle'. This is initial (and compelling) motivation for a dividing line between the phenomena that pragmatic principles should cover (conversational implicatures) and those that they cannot (CIs, among others). (Potts 2005:9).

S. But wait a minute. Potts is here conflating conventional meaning and conventional implicature. Grice is talking about conventional meaning – and he gives some idea of what he hopes, eventually, to be understood by this term in those formulations that you outlined just a minute ago. I'm glad I asked you to elaborate further. (I know you were reluctant.) But Potts is here slipping from this onto conventional implicature and stating that they are in the grammar. This is not, surely, a safe step.

P. Well, he goes on to say that 'the 'implicature' part of 'conventional implicature' is unfortunate' (Potts 2005:9). I'm not so inclined to agree with him on this. It has always seemed to me, and, I suspect, to others, that 'implicature' refers to those inferences that *are* cancellable, and conventional implicatures are cancellable. Take a well-worn example (in fact, it's already been used here): 'She is poor but honest and I don't mean to suggest that there is anything surprising in that conjunction'.

S. Is it possible to say that?

P. I think so, yes. Anyway, on the basis of the above quote and a few others, Potts extracts the following abstract properties of conventional implicatures:

    a. CIs are part of the conventional meaning of words.

    b. CIs are commitments, and thus give rise to entailments.

    c. These commitments are made by *the speaker of the utterance* 'by virtue of the meaning of' the words he chooses.

      d. CIs are logically and compositionally independent of what is *'said* (in the favored sense)', i.e. independent of the at-issue entailments. (Potts 2005:11).

Where 'at-issue entailments' is Potts-speak for 'regular asserted content ('what is said', in Grice's terms)' (Potts 2005:6).

S.   Hmmm. So there is quite a large number of changes being introduced.

P.   Yes. And it goes a little further. The empirical focus also changes:

      Suppose the textbook examples (*therefore, even, but* and its synonyms) disappeared. Where would conventional implicatures be then? This book's primary descriptive claim is that they would still enjoy widespread factual support. (Potts 2005:5).

S.   So what's the empirical focus now?

P.   On the one hand, supplemental expressions like appositives and parentheticals and, on the other hand, expressives like epithets and honorifics.

S.   Interesting data.

P.   Yes.

S.   But let's cut to the chase: does, on this data, the type-driven multi-dimensional semantic translation language bring some stability to the increasingly shaky role played by conventional implicature within the general theory of meaning?

P.   Well, that rather depends on what you mean by 'the general theory of meaning'

S.   Or, by what you take to be the locus of where semantics meets pragmatics.

P.   Yes. Potts provides a 'meaning graph' (Potts 2005:23). It goes a bit like this. 'Meanings' are taken to be 'entailments' or 'context dependent'. 'Entailments' are taken to be (a) 'conventional presuppositions' (not speaker oriented, backgrounded); (b) CIs; and (c) 'at issue entailments' (not invariably speaker oriented, vary under holes, plugs). 'Context dependent meanings' are taken to be (a) 'conversational implicatures' (not conventional, not speaker oriented, not backgrounded) and (b) 'conversationally-triggered presuppositions' (not speaker oriented, backgrounded).

S.   Holes and plugs?

P.   Yes, I'm not entirely convinced that conventional implicatures and presuppositions are being kept separate here.

S.   That is all quite dense. I'll not request that you repeat it for me, though. But what's the difference between the conversationally-triggered and the conventional presuppositions, both of which are said to be not speaker oriented and backgrounded?

P.   Well, presumably the first is, whilst the second isn't, cancellable.

S.   Of course.

P.   But that's not the most pressing concern.

S.   Oh?

P.   I'm more worried about what he says here:

... it is worth noting that Levinson (2000) proposes to distinguish generalized and particularized conversational implicatures from one another, in service of the more general goal of developing a robust notion of *default interpretation.* I do not explore this hypothesis, mainly because it seems clear that this is an issue that impacts the nature of the 'context-dependent' node in [the meaning graph] and the paths from it. (Potts 2005:25).

I would have thought, given Grice's previous remark about the difficulty of distinguishing generalized conversational implicatures from conventional implicatures, that Levinson's TGCI would have merited the closest scrutiny.

S. Well, as I said earlier, there is quite a large number of changes being introduced here.

P. Yes, and to answer your question about stabilizing the shakes – I think it is too early to tell. I'm still inclined to think that conventional implicatures *are* pragmatic and that a logic of them, whether type-driven, or multi-dimensional, or whatever, will have to contain a large defeasible character. But perhaps my thinking is just a bit slow at moving with the times.

S. Well, perhaps this is the moment to bring this dialogue to a close, and to make way for the main event.

P. Yes, I agree. The subject is, as Kneale remarked, full of perplexities, some of which we may not even have noticed, let alone addressed, but the papers to come in this collection will, I think, and I think you think too, show that there are many theoretically and descriptively profitable lines of inquiry that will help not only to demarcate but also to integrate meanings and uses.

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