**Can we really replace semantic parsimony with psychological parsimony? Burge and Millikan against Grice and Sperber & Wilson**

**Abstract:** In this paper, I defend higher-order-thought (HOT) theories of meaning against recent criticisms proposed both by Burge (1993) and Millikan (1993, 2004, 2005), which have urged a psychologically parsimonious view of linguistic communication. Concentrating on Millikan's account, I show that empirical data contradicts two major predictions of Millikan's account: first, a full continuity with animal communication systems; second, the epiphenomenal character of mental states attribution in linguistic communication. Additionally, I show that Millikan's view of linguistic conventions leads to a widespread ambiguity which can only be resolved by an appeal to speaker's intention. Finally, I quickly describe children's linguistic communication, showing that it is consistent with Sperber & Wilson's view that pragmatics is modular.

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*1. Introduction*

As is well known, Grice's (1989) view of meaning, and more generally of linguistic communication, rested on two major tenets: a principle of semantic parsimony, dubbed *Modified Occam's razor*, which enjoins not to multiply senses more than is strictly necessary; a psychologically-based view of meaning, in which the kind of meaning involved in linguistic communication — though not only in linguistic communication — involves a double intention on the part of the speaker, as well as the recognition of these intentions on the part of the hearer (cf. § 2, below). In other words, one can see Grice's view of meaning and communication as proposing a trade-off between semantic parsimony and psychological inflation. Though different in many ways, Sperber & Wilson's (1986/1995) theory basically follows the same path. By contrast, recent views (e.g. Burge 1993,

Millikan 1993, 2004, 2005) have advocated views which seem entirely opposite, i.e. views in which a semantic inflation is balanced by a psychological parsimony (cf. § 3). This paper is devoted to the defense of the semantic parsimony-psychological inflation trade-off.

Its line of defense will rely on three main arguments: a) Millikan's view[1] makes linguistic communication continuous with animal communication systems and it can be shown that such a continuity cannot account for known facts about both linguistic communication and animal communication systems; b) Millikan's account supposes that one can have a communicative behavior indistinguishable from that of other humans without having the ability to attribute psychological states to others and, again, facts contradict this consequence of her views; c) Millikan's view supposes widespread ambiguity in linguistic communication but does not give us any account of how such ambiguities are solved in actual linguistic communication, and notably what criterion for correctness of interpretation should be used; I will argue that the only valid criterion is speaker intention and that thus one cannot trade off semantic inflation against psychological parsimony because semantic inflation entails psychological inflation.

However, there are aspects of both Burge's and Millikan's views which are appealing, and there are also some legitimate doubts as to whether young children are able of the metarepresentational feats which Gricean and post-Gricean views seem to impose on communicators (cf. Breheny 2006). So I will end up with developmental questions regarding linguistic communication.

## *2. HOT theories of meaning*

HOT theories of meaning are theories which, like that of Grice (1989) and that of Sperber & Wilson (1986/1995), attribute to speakers and hearers higher-order thoughts (HOTs) in the production and/or interpretation of utterances. I will mainly concentrate in the present section on Grice's notion of non-natural meaning (or meaning$_{NN}$: cf. Grice 1989, chapters 14 and 18). Grice based his argument on the following examples:

(1) Those spots mean/meant measles.

(2) The recent budget means that we shall have a hard year.

(3) Those three rings on the bell (of the bus) means that 'the bus is full'.

(4) That remark, 'Smith couldn't get on without his trouble and strife', meant that Smith found his wife indispensable.

Grice considered that the first two examples are instances of natural meaning and that the last two examples are instances of meaning$_{NN}$. The main differences between natural meaning and meaning$_{NN}$ concern factivity and being under voluntary control[2] as shown in the table below:

INSERT TABLE 1 HERE

These distinguishing features can be developed as follows: *factivity* can be defined as the fact that, in natural meaning, if the sentence "Those spots mean/meant measles" is true, then it is true that the spot bearer *has* measles; by contrast, in meaning$_{NN}$, the truth of the sentence "Those three rings on the bell (of the bus) means that 'the bus is full'" does not in any way entail that the bus *is* full. Being under voluntary control is more transparent that factivity, meaning only that the production of the utterance is intentional in the vernacular sense. This led Grice to the following informal definition for meaning$_{NN}$: ""A means$_{NN}$ something by x" is (roughly) equivalent to "A intended the utterance of x to produce some effect in an audience by means of the recognition of this intention"" (Grice 1989, 219). Basically, what this implies is that meaning$_{NN}$, as for instance it can be found in linguistic communication, entails that linguistic communication involves entertaining fourth-order thoughts of the following type: *The speaker intends [$_1$that the hearer believes [$_2$that the speaker intends [$_3$that he believes [$_4$that p]]]]*. Sperber & Wilson (1986/1995) have a less controversial view in that they concentrate on human communication (which they call *ostensive-inferential communication*) and suppose that the speaker has a double intention: an informative intention, whereby the speaker intends to make manifest or more manifest to the hearer a set of assumptions *I*; a communicative intention, whereby the speaker intends to make it mutually manifest to hearer and speaker that the speaker has this informative intention. Again — and this is why Sperber & Wilson's theory

is, just as Grice's, a HOT theory of meaning —, this entails that the speaker and the hearer must entertain the following HOT thought: *The speaker intends [$_1$that it is mutually manifest to the hearer and the speaker [$_2$that the speaker has the informative intention [$_3$that it is manifest or more manifest to the hearer [$_4$that I]]]]*. Another point of agreement between these two theories is the fact that they both accept the principle, initially proposed by Grice (1989, 47), of Modified Occam's Razor, according to which: *Senses are not to be multiplied without necessity*. This leads to the notion of semantic underdeterminacy, i.e. to the idea that speaker's meaning is underdetermined by sentence meaning, a gap which is bridged by pragmatic processes in which HOT thoughts of the type indicated above play a role either as a result of the interpretation process (Sperber & Wilson) or as premises as well as result in the interpretation process (Grice). In other words, it can be said that for both Grice and Sperber & Wilson, there is a trade-off between semantic parsimony (leading to semantic underdetermination of speaker meaning) and psychological inflation (bridging between sentence meaning and speaker meaning).

*3. Psychologically parsimonious theories: Burge and Millikan*

Burge's concern in his 1993 paper was with a comparison between the acquisition of information through linguistic communication (which he calls *interlocution*) and the acquisition of information through perception. According to him, both processes are equally transparent, i.e. neither relies on psychological attribution of any kind. They do differ, however, in as much as linguistic acquisition of information is a priori (it is not in need of any empirical justification), while perceptual acquisition of information, being in essence empirical, is not. The transparency hypothesis is linked through an assumption of rationality to an *Acceptance principle*, which Burge (1993: 467, his emphasis) defines as follows: "*A person is entitled to accept as true something that is presented as true and that is intelligible to him, unless there are stronger reasons not to do so*". Thus, according to him, the speaker is not taken as "an object of interpretation, but rather as the source of informa-

tion presumed to be understood without interpretation" (Burge 1993: 487). However, Gricean implicatures are taken not to fall under this view of linguistic communication and are supposed to be in need of empirical justification.

Millikan's (1993, 2004, 2005) view is much richer than Burge's, including hypotheses on conventions as well as acknowledging more fully the variety of speech acts (Burge's view seems restricted to *saying that*). She rejects Lewis' (1969) view of conventions as involving mental states attribution and replace it with a view according to which conventions are not psychologically based prescriptive rules but lineages on a par with the biological lineages leading to species: thus, just as identifying an animal is placing it in the correct biological lineage, identifying a word is placing it in the correct linguistic lineage. Linguistic conventions have coordinating functions (which explains why they are reproduced generation after generation) and can concern syntactico-semantic phrases as well as individual words. Up to this point, Millikan's account is entirely compatible with Burge's. However, where, *prima facie*, they differ is in that Millikan sees linguistic conventions as leading to widespread ambiguity, which seems to raise a problem regarding the immediate intelligibility that, to Burge, is the mark of linguistic communication. This can be seen in the account given by Millikan (2005: 35) of lexical ambiguity: "Tokens of the same word that have taken on different senses are words with a common lineage some distance back, but whose lineages have now separated". A second point of departure is that Millikan thinks that indeed Gricean implicatures can be accounted to in exactly the same way:

(5) Anne has four children.

(6) Anne has at least four children.

(7) Anne has exactly four children.

As is well-known, the semantic (sentence) meaning of (5) is (6), though, more often than not, such a utterance is interpreted as in (7), which is, precisely, the implicature of (5). Millikan's suggestion is that originally, the linguistic lineage linked (5) to (6). This original lineage then split in two different directions, the first linking (5) and (6) and the second linking (5) and (7). Basically, what this

means is that utterances, which can trigger implicatures (e.g. (5)) are ambiguous between the logical interpretation (e.g. (6)) and the implicature interpretation (e.g. (7)). In the Gricean account, by contrast, (5) is not ambiguous in the least: it has only one meaning (i.e. (6)) and the implicature is inferred on the basis of the mental states the speaker attributes to the hearer to explain the speaker's choice of utterance (see Grice 1989, chapters 2 and 3). Thus, Millikan's account is semantically inflationist (it clearly does not follow Modified Occam's Razor), but psychologically parsimonious (it does not posit any mental state attribution in the interpretation process). This is clearly a trade-off, but one which is exactly the reverse of the Gricean one:

INSERT TABLE 2 HERE

Indeed, one of the main thrust of both Millikan's and Burge's views, whatever their differences, goes against the Gricean distinction between natural meaning and meaning$_{NN}$: in other words, if they are right, there should not be any difference between human linguistic communication and animal communication systems. Additionally, not being able to attribute mental states to others should not lead to a distinguishable communicative behavior (both on the production and on the interpretation side) in humans. Finally, the disambiguation which Millikan's system presupposes, should not, *in any way*, appeal to the attribution of mental states. We will now examine each of these claims in detail.

*4. Comparing animal and human communication systems*

I should begin by pointing out that not any animal communication system will do. Given the prominence of the comparison between linguistic acquisition of information and perceptual acquisition of information, it is clear that animal communication systems of interest are those that are geared to information transmission, i.e. that can be seen as referential, as least from a functional point of view. This excludes most of animal communication, i.e. everything to do with reproduction or territory. It leaves us however with two possibilities: transmission of information about food and

about predators. I will concentrate here on what has to be the best studied such system, i.e. vervet monkeys' alarm calls (see for a detailed description, Cheney & Seyfarth 1990). As is well-known, vervet monkeys (which are Old World — i.e. African — monkeys about the size of a cat) have three major alarm calls, alerting conspecifics to the approach of a predator, which can be either a leopard, a bird of prey (mostly eagles) or a snake (mostly pythons and mambas). The three calls are acoustically distinctive and they trigger quite different reactions, each appropriate for escape from the type of predator whose presence has been signaled: leopard calls lead the monkeys to climb into the canopy, eagle calls to climb down, while snake calls lead to a mobbing behavior in which monkeys collectively force the snake to go away. These behaviors are identical with those that the direct perception of the corresponding predators would cause, which indeed goes well with Millikan's and Burge's view, in that perception of the predators and the different alarm calls are causally similar, given that the behavioral reactions are themselves similar. Thus vervet alarm calls seem to be the very epitome of what Millikan and Burge have in mind in their comparison between perception and (informative) communication. The question is whether or not human linguistic communication can be approached in the same way.

There are two main paths to an answer to that question: the first one is to see in which way, if any, vervet alarm calls can be seen as an instance of meaning$_{NN}$; the second one is to see in which way, if any, human linguistic communication can be seen as an instance of natural meaning. We will turn to the second one in the next section but will stay on the first one now. Vervet alarm calls, as many (if not most) animal signals, are genetically determined, not acquired: vervet infants produce them in an acoustic form identical to that found in adults from birth on. What is more, they produce them in roughly adequate situations: i.e., vervet infants will produce leopard calls when a four-legged animal of more or less the right size approaches, but not when a bird hovers above and *vice versa*; they do not produce leopard alarm calls when snakes are in view, etc. Granted, they tend to overgeneralize and will produce a leopard calls when a warthog approaches and there is some fine-tuning to do, but the calls can be said to be roughly in place at birth. Thus, here is a major departure

between human and non-human communication systems: with the possible exceptions of human infants cries (see Owings & Zeifman 2004), human signals (i.e. the lexicon) are acquired[3].

Another point is whether or not vervet alarm calls are under voluntary control, which is, apart from being non-factive, one necessary — though not sufficient — feature of $meaning_{NN}$. Here, the general consensus among specialists is that they are indeed under voluntary control, because the very presence of a predator is not enough to cause the production of the relevant call: there is what is called an *audience effect*, i.e. the presence of conspecifics with particular properties (mainly consanguinity) is needed. Thus, vervet alarm calls are under voluntary control, a necessary feature of $meaning_{NN}$. This, however, is not sufficient for them to be $meaning_{NN}$. It would be necessary for them to be produced not only in the presence of an audience, but for their production to depend on the state of mind which can reasonably be attributed to the audience. In other words, the necessity of the presence of the audience may be enough to show that vervet calls are under voluntary control, but not to show that they are an instance of $meaning_{NN}$; it must be shown that the presence of the audience (as well as that of the predator of course) is not sufficient. Thus, the production of calls should depend on a) the presence of an audience of an appropriate kind; b) the fact that the audience ignores the presence of the predator. From all accounts, it seems clear that this second condition is not met in vervet alarm calls. Hence, vervet alarm calls are not $meaning_{NN}$, though they are not either, given the fact that they are under voluntary control, an instance of natural meaning[4]. They are an intermediate type of communication or meaning, being — barring mistakes, such as those made by the infants — factive, but also being under voluntary control.

The fact that they are transparent in exactly the sense that Millikan and Burge want means that if human linguistic communication does answer to Millikan's and Burge's description, one would expect human communicative behavior to be like vervet communicative behavior: under voluntary control, but transparent, or at least as transparent as is perception. The question is: is it?

*5. Human communication without mental state attribution*

If human linguistic communication is on a par with vervet alarm calls, the main feature it should share with vervet communication is that neither the production nor the interpretation process should make any appeal whatsoever to mental states attribution: in short, it should be just as direct, which is to say, as codic, as vervet alarm communication. This makes a strong prediction: human beings who, for one reason or another, are not able of mental state attributions should be indistinguishable, as far as their communicative behavior, and notably their *linguistic* communicative behavior is concerned, from other humans. This point has been made, from a different point of view by Glüer & Pagin (2003). Just as Millikan and Burge do, Glüer & Pagin reject the Gricean view of human linguistic communication. Their argument is not based, however, on a comparison between perceptual acquisition of information and linguistic acquisition of information, but on the idea that if some humans, who are unable of mental states attribution, are nevertheless able of human linguistic communication, then mental states attribution is merely epiphenomenal to human linguistic communication. Basically, this is the form of many arguments in philosophy, and mostly in philosophy of mind. For instance, it is very similar in shape to Chalmers' (1996) philosophical zombies argument. Briefly, Chalmers' argument — which is a modal argument — rests on a thought experiment: according to him, there is a possible world, microphysically —molecule per molecule — identical with ours, on which live counterparts of us, themselves microphysically identical with us, who, given that behavior is constrained and guided by neurophysiological facts, have exactly the same behavior as we do. However — this is part of the definition of the thought experiment —, though microphysiologically and behaviorally identical with us, they have absolutely no feelings or sensations whatsoever. Chalmers's claim is that such creatures (called *philosophical zombies*) are (metaphysically) possible and that their very possibility shows that feelings and sensations (i.e. *qualia* in the philosophical terminology) are epiphenomenal to behaviour. Obviously, discussions of Chalmers' argument have focused on the very possibility of philosophical zombies. What is of interest here however is that no argument to the effect that this or that ability is epiphenomenal to this or

that behavior can escape a strong constraint: it has to establish that the behavior is identical (in the sense of being indistinguishable) in creatures with the ability and creatures without the ability. Chalmers' way of securing it is *via* a thought experiment. This, however, is not the path followed by Glüer & Pagin. Instead, they invoke empirical facts, i.e. the fact that there are autistic speakers. Now, autistic people have been shown to be unable of mental states attribution, or at least to manifest a deficit in such an attribution. Yet, about 50% of them do speak. This, according to Glüer & Pagin, is enough to show that mental states attribution is epiphenomenal to linguistic communication. However, by parity of reasoning with philosophical arguments — which, incidentally, is what the authors claim their argument to be — their argument can only be successful if they can show that autistic patients' communicative behavior is identical with (i.e. indistinguishable from) non-autistic humans' communicative behavior. So the question (which they do not address) is whether it, actually, is.

The answer, on all accounts, is resolutely negative: indeed, one of the most noted symptoms of autistic (speaking) patients' is the persisting anomaly of their linguistic communicative behavior. They do not seem able to adapt their communicative strategies to their hearers, either in the choice of topics, in the choice of referential expressions, or in their interpretation of others' utterances. Misunderstandings are more common than is usually believed in human linguistic communication, but they are fairly often seamlessly resolved. This, however, is far from being the case when one of the speakers is an autistic patient. Additionally, autistic speakers have difficulty as soon as utterances are non-literal, i.e. in metaphors or, more severely, in sarcasm or irony. The very fact that they have difficulties with both literal and non-literal linguistic communication should be proof enough that they are not behaviorally indistinguishable from normal speakers.

There is more however. A recent study (cf. Howlin 2003) shows that, despite commonly held convictions, both Asperger (who are supposed not to show the language acquisition delays which are one of the main symptoms of autism, but who are also impaired in mental states attribution) and high functioning autistic patients with language are rather deficient in their linguistic abilities, both

in production and in interpretation. Thus, Howlin tested adult (> 18 year olds) high functioning autistic and Aperger patients with current scales of linguistic abilities designed to evaluate school children's linguistic progression (i.e. the ceiling performance is at 18 year of age) relative to expression (production) and comprehension (interpretation). She sums up her results as follows (Howlin 2003: 10):

> 57% of the Asperger group (…) scored below ceiling on the BPVS (a measure of receptive vocabulary) and 62% scored below ceiling on the EOWPVT (a measure of expressive vocabulary). In the autism group 76% were below the age ceiling on the expressive test and 69% on the comprehension test (X2 non-significant for each comparison between groups). Among the 21 individuals with Asperger syndrome whose scores were below the ceiling age of the test (i.e., those for whom an exact language score rather than an extrapolation was available), the average gap between chronological age and language comprehension age was 12.6 years (SD 6.29 years) and the gap between chronological age and expressive language age was 10.20 years (SD 6.10 years). In the autism group the gap between chronological age and comprehension and expressive ages for those scoring below the age ceiling (n = 22) was similar (difference between comprehension age and CA = 13.33 years, SD 9.53 years; difference between expressive age and CA = 11.67, SD 9.34 years).

In other words, Glüer & Pagin's argument fails dismally: not only is the communicative behavior of autistic and Asperger patients not indistinguishable from that of normal speakers, their linguistic abilities themselves are clearly different, which, given that their IQs are normal (between 80 and 120), seems to suggest that mental states attribution *does* play a role in normal linguistic communication.

Thus, empirical data does not verify Glüer & Pagin's position on the epiphenomenal character of mental states attribution in linguistic communication (for a more detailed argument, see Reboul

forthcoming a). The question is: does it destroy Millikan's or Burge's view of linguistic communication? On the face of it, it contradicts Burge's account in its entirety. However, it might leave some of Millikan's account standing, notably her hypothesis of widespread ambiguity, though it does seem to contradict her transparency view. We will now turn to why this should be so.

*6. The impossibility of trading off semantic inflation with psychological parsimony*

This leads us back to vervet alarm calls. Clearly vervet alarm communication does not involve mental states attribution either in production or in interpretation. What the differences between autistic and normal speakers' communicative behavior suggest, however, is that linguistic communicative behavior cannot proceed normally in the absence of mental states attribution. This makes sense under the hypothesis of semantic underdetermination which Grice and Sperber & Wilson share. Does it make sense under Millikan's view, which is not a semantic underdetermination view, but rather a semantic overdetermination view? Strangely enough, it does: clearly, linguistic communication can be transparent if linguistic communication were describable as are vervet alarm calls, that is as a set of bi-univocal pairs of meanings and linguistic types, where "bi-univocal" is meant to set aside any kind of ambiguity. In other words, any system with ambiguity, let alone the widespread ambiguity which is the price Millikan has to pay for her conventionalist view of linguistic communication, will hardly be transparent. This is why one may be doubtful as to whether a trade-off between semantic inflation and psychological parsimony is possible.

Let me recapitulate the two positions, with the two formulas below:

**Grice**: Semantic parsimony ⇒ Semantic underdetermination ⇒ Psychological inflation

**Millikan**: Semantic inflation ⇒ Semantic overdetermination ⇒ Psychological parsimony

It clearly is possible to hold the Gricean position, which entails that there is a gap between sentence meaning and speaker meaning and that mental states attribution can bridge the gap. Basically, Mil-

likan's claim is that semantic overdetermination means that there is no gap between sentence meaning and speaker meaning, and thus no need for any kind of bridge. In other words:

**Grice**: sentence meaning ≠ speaker meaning

**Millikan**: sentence meaning = speaker meaning

Given Grice's position, this is entirely coherent. But I will argue that Millikan's position does not support the equivalence of sentence meaning and speaker meaning. The problem is that given the kind of widespread ambiguity which results from Millikan's view, the linguistic decoding processes cannot deliver a *single* sentence meaning: rather, they will deliver several different sentence meanings, all, if one relies only on linguistic conventions, equally probable. This is foreseen by Millikan, who does acknowledge that though linguistic interpretation is similar to, if not identical with, perception, both perception and linguistic interpretation can and frequently do appeal to inferential processes, though she insists on the fact that, in linguistic interpretation, these inferential processes do not use as premises, nor do they produce as conclusions, mental states attribution. She is rather cautious about the inferential processes concerned, but quite forceful on that point. Thus, her view is that one can combine widespread ambiguity *and* psychological parsimony, given that one can rely on (psychology-free) inferences.

Is that tenable? There remains an important problem, which is that a criterion of correct interpretation has to be provided, otherwise misunderstandings (or at least misunderstandings not due to background noise) would be impossible and it is clear that they are not. The problem is that of the criterion, which Millikan could propose, given her overall theoretical framework. It should rest on non-psychological factors, or psychological parsimony dismally fails. What is that criterion? Millikan does not say and it is, indeed, doubtful that any such non-psychological criterion is possible in her system. In other words, whether one is confronted with semantic underdetermination (as in Grice or Sperber & Wilson), or with semantic overdetermination (as in Millikan), there can only be one criterion of correct interpretation for any utterance and that criterion is speaker meaning, i.e. what the speaker intended to convey to his hearer. Thus, neither semantic underdetermination nor

semantic overdetermination can ensure psychological parsimony: in both cases, psychological inflation is mandatory. What we get is a (more realistic) reformulation of Millikan's position, yielding:

**Grice**: Semantic parsimony ⇒ Semantic underdetermination ⇒ Psychological inflation

**Millikan**: Semantic inflation ⇒ Semantic overdetermination ⇒ Psychological inflation

Basically, what this means is that there is no possible trade-off between semantic inflation and psychological inflation, because semantic inflation — or, at least, the sort of semantic inflation advocated by Millikan — leads inevitably to psychological inflation. Thus, Grice's position and its variant proposed by Sperber & Wilson are more economical from a theoretical point of view than is Millikan's, and, hence, should be preferred (for a more detailed argument, see Reboul forthcoming b). The next important question is how inflationary has psychological attribution to be under a semantically parsimonious view.

*7. How inflationary should psychological attribution be, given semantic parsimony?*

This question has been tackled by Breheny (2006), who approaches it through linguistic communication in young children. Breheny notes that HOT accounts, such as Grice's or Sperber & Wilson's, are generally supposed to make linguistic communication rests on theory of mind abilities. This, as he points out raises the question of how to account for young children's communication, given that the rather late age at which they actually pass the false belief test (taken to be *the* test of a fully functioning theory of mind), i.e. ≈ 4 year, does not coincide with their beginings at linguistic communication (first words ≈ 11 months, advanced linguistic communication around 30 to 36 months). As Breheny (2006: 75) points out, this discrepancy can be addressed by three different strategies:

(a) Argue that two- and three-year old children are not really competent with regards to basic communication.

(b) Argue that children under four possess the required abilities.

(c) Argue that these abilities are not required for agents to engage in basic language use and communication, while maintaining that communication is as the elaborate analysis suggests.

It is clear that Grice's account and Sperber & Wilson's do not meet this difficulty in the same way: Grice's account supposes that attribution of mental states to the speaker occurs in the inferential interpretation process as well as as a result of this process; Sperber & Wilson only suppose that the result of the inferential interpretation process is a mental state attribution, but not that mental states attributions enter the process as premises on which the inference schemata operate. This is an important difference between the two accounts, despite their *prima facie* similarities. Leaving aside Grice's account, how far is the discrepancy between the age of full theory of mind abilities and the age of linguistic communication destructive for Sperber & Wilson? And would any of the strategies outlined by Breheny be preferable?

One possible way out of this difficulty is to invoke recent non-verbal experiments aiming at showing false belief attribution in 15 months old (see Onishi & Baillargeon 2005). If such experiments can be shown to have reached their aims, the problem obviously disappears. However, it seems that this experiment meets with some difficulties, minor ones being invoked by Perner & Ruffman (2005), major ones invoked by Povinelli & Vonk (2004). The minor ones rest on previous experiments, using standard false belief tests, but monitoring children's gaze as well as explicit answers (see Clements & Perner 1994), which show that despite the facts that correct answers are not given before 4 year of age, the children gaze at the correct location by 3 year of age. However, this implicit and correct answer is not detectable in younger children who both look and answer incorrectly. Thus Perner & Ruffman objects to the interpretation Onishi & Baillargeon put on their results (as showing that 15 month-olds are able of false belief attributions), asking where theory of mind abilities go between 15 month and 3 year. This is a valid objection, but it is weakened by the fact that Perner & Ruffman mention that alternative interpretations of success are available both for Onishi & Baillargeon's non-verbal version and for more standard versions of the false belief test: for instance, success could be explained by simple associations or by (more plausibly) mere behav-

ior reading without any kind of mental attribution. However, it should be obvious that the same objection as above can be made against these alternative accounts: where have associative or behavior reading abilities gone between 15 month and 3 year (for a more detailed account, see Reboul forthcoming c)? In other words, the basic objection is that that gap is present, whatever the account. So is there any reason to discard the mentalist explanation of Onishi & Baillargeon's results?

It seems that there is: Onishi & Baillargeon meet with a more serious objection, which was raised by Povinelli & Vonk (2004), not against their experiment — which it predated — but against all versions of the false belief test. Povinelli & Vonk point out that in fact the false belief test cannot discriminate between behavior reading and mind reading (theory of mind) because success at it could be explained by behavior reading alone. However, given that mind reading presupposes the existence of behavior reading (it does not replace it, but complements it, see Povinelli & Vonk's paper for their — very convincing — argumentation), one could have mind reading, in addition to behavior reading, though the false belief test cannot establish that.

A general conclusion, which can be derived from this short discussion is that the only certainty is that children have false belief attribution ability at 4 year (not because they can pass the false belief test, but because independent data establishes it). They may have it before, but that has not been irrefutably shown. So the gap between linguistic communication and full theory of mind abilities stands and the second strategy described by Breheny does not solve the problem. However, as Breheny points out, the fact that children below 4 year may well not have full theory of mind abilities does not mean that they have no theory of mind abilities. As Baron-Cohen (1995) has shown, though children before 18 month have abilities which seem only to resort to behavior reading (gaze direction and intention detection abilities), at around 18 months, a new ability emerges, the ability to share attention with others — otherwise called *joint attention* —, which is, arguably, a mentalistic ability, and, by the way, an ability which autistic children seem to lack[5]. That still seems, nevertheless, far from the metarepresentational abilities which HOT accounts, to which Sperber & Wilson's belong, suppose. So what can be the solution?

The other two strategies proposed by Breheny (who goes for a fourth one) are refusing that children have basic communicative abilities or accepting that they have basic communicative abilities, but claiming that adult communicative abilities go farther than those basic communicative abilities. I will adopt the last possibility and will now defend it.

Basically, what Sperber & Wilson's account needs is the ability of detecting the necessity for inferential interpretation and the ability to produce, as the end result of that inferential process, a metarepresentation to the effect that the speaker means so-and-so. Is there any reason to think that children below 4 year cannot do that? Indeed, I think that the main problem is not the metarepresentational ability (the ability for joint attention may well be enough to show early metarepresentational ability). The main problem is rather why children should trigger an inferential process of the nature adduced by Sperber & Wilson. Sperber & Wilson have in recent papers (see e.g. Sperber & Wilson 2002) advocated a view according to which there is an innate module which has evolved in the human species specifically for the interpretation of communication, as a sub-module of theory of mind (also conceived as an innate module). Now this makes sense only if no alternative is available, because an adaptation, as such, must answer a need. One such alternative (which is actually very near, if not equivalent, to Sperber & Wilson's (1986) original version of Relevance Theory) would be the fact that any act of communication is, given that it is ostensive-inferential, enough to trigger a presumption of optimal relevance, which would, itself trigger an inferential interpretation process, constrained by the relevance principle and possibly relying on general theory of mind mechanism. One central question is whether, as Sperber & Wilson claim, the fact that communication is ostensive is enough, in and off itself, to guarantee optimal relevance, and mandate an inferential interpretation process. Part of the problem is that any act of communication is bound to be ostensive (for instance, vervet alarm calls clearly are ostensive) and hence could be considered as guaranteeing optimal relevance (arguably, vervet alarm calls do) but this clearly does not mandate any inferential process (vervet alarm calls are *both* ostensive *and* transparent).

Thus, taking stock of the situation, young children are not (as far as can be known with any certainty) in full possession of a theory of mind, though they may have some metarepresentational ability. Additionally, ostension does not trigger, in and off itself, inference. This, then, is the missing ingredient, and this is just where Sperber & Wilson's (2002) hypothesis of a pragmatic module, triggered by any form of ostensive communication, steps in: ostensive communication is enough to trigger inferences constrained by relevance, not because all ostensive communication needs inference for its interpretation, but because, in humans, ostensive communication automatically triggers such inferential processes. In other words, young children, though they are indeed unable to use theory of mind abilities in their own linguistic production and interpretation (as evidenced in psycho-linguistic experiments on the production and interpretation of referential expressions by preschool children: see, e.g., Mitchell et al. 1999), can and do use relevance-constrained inferential processes in their interpretation of others' discourse and presumably access as a result of these processes, metarepresentations of the form *x means that p* (where, possibly, "mean" is not interpreted in fully mentalistic terms, but rather on a par with speech verbs, such as "say").

Another way of saying the same thing is in terms of Sperber's (1994) tripartite distinction between different communicative or interpretive strategies:

(a) *naive optimism*: in naive optimism, the hearer takes it for granted that the speaker is both competent — i.e., he tailors his/her utterance to his/her hearer's interpretive abilities — and sincere — he/she is not trying to mislead his/her hearer.

(b) *cautious optimism*: in cautious optimism, the hearer takes it for granted that the speaker is sincere, but not that he/she is competent.

(c) *sophisticated understanding*: in sophisticated understanding, the hearer does not take for granted either the competence or the sincerity of the speaker.

It should be clear that these three strategies will differ in the amount and degree of metarepresentation they entail. Naïve optimism does not need anything over first-order metarepresentation (i.e., *the speaker means that p*) and speaker meaning is supposed to coincide with the result of the relevance-

constrained inferential process, following a least-effort heuristics. In cautious optimism, speaker meaning may not coincide with the first interpretation available because the speaker may not be competent enough to ensure that coincidence. Hence, there may be a discrepancy and more metarepresentation may be involved. Finally, in sophisticated understanding, where neither competence nor sincerity is taken for granted, it seems clear that yet more metarepresentation of a higher order is necessary. What this seems to mean is that preschool children might be able only of naive optimism, graduating to cautious optimism and even more so to sophisticated understanding only by the time they have mastered false belief and a full theory of mind.

*8. Conclusion*

If this is so, then young children's linguistic communication does indeed comply with Burge's Acceptance principle (*A person is entitled to accept as true something that is presented as true and that is intelligible to him, unless there are stronger reasons not to do so*), as indeed, up to a point does adults' linguistic communication. Presumably, we, most of the time, unthinkingly apply naive optimism and only take the trouble of using cautious optimism or sophisticated understanding when these last strategies are imposed by what we know of the situation (e.g., a job interview) or of the speaker (e.g., politicians). Hence, there is some truth in the Burge-Millikan view, but that does not mean that no psychological attributions are needed, or that semantic inflation does allow a trade-off with psychological parsimony. In other words, we need psychological inflation, whatever the strategy we apply and however rich our semantics is.

Thus, HOT theories of meaning are here to stay and it makes sense to explore them rather than to devise alternative theories which, in the end, can be shown not to solve the problem. Granted, there is a developmental story, which still has to be told in full, but that is where research should go and where psychological experiments are needed. And there, the very fact that Sperber & Wilson's po-

sition can be experimentally investigated — it is not clear that Burge's or Millikan's positions can — is an advantage.

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[1] I will concentrate on Millikan's view as it is more precisely articulated than Burge's and has been described in a number of more accessible publications.

[2] In his original paper ("Meaning"), Grice gave other distinguishing features, but those are the main ones as shown by the fact that they are the ones singled out in his later paper, "Meaning revisited".

[3] This does not contradict the hypothesis that syntax might be innate. But clearly, the lexicon is acquired.

[4] For more on this point, see Reboul (in revision).

[5] Indeed, Peacock (2005) defends the view that joint attention *is* the basis of meaning$_{NN}$, a position which is very near to Breheny's. I will not discuss this view here (but see Reboul in preparation).

| | Natural meaning | Meaning$_{NN}$ |
|---|---|---|
| Factivity | + | — |
| Under voluntary control | — | + |

*Table 1: Natural vs. non-natural meaning*

| | Grice | Millikan |
|---|---|---|
| Psychological parsimony | — | + |
| Semantic parsimony | + | — |

*Table 2: Grice's trade-off vs. Millikan's trade-off*