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BAD ARGUMENTS AGAINST SEMANTIC PRIMITIVES

Semantic primitives have fallen on hard times. Though their existence was once widely accepted in linguistics, a variety of counter-arguments have since engendered widespread scepticism. This paper examines a selection of anti-primitives arguments with the aim of showing that they fail to apply to the most resilient and well-developed theory of semantic primitives, namely, Anna Wierzbicka's 'natural semantic metalanguage' theory. The most serious of the faulty arguments invalidly link semantic primitives with 'objectivism', or with abstractness and non-verifiability, or with implausible views about language acquisition or language processing. Others rely on misanalyzed linguistic 'facts', or simply fail to come to grips with the most credible pro-primitives position. The anti-primitives arguments are drawn from a broad range of sources, including the philosophy of language, psycholinguistics, language acquisition studies, and cognitive linguistics.

1. INTRODUCTION

Like Wilks (1977), whose title it partly echoes, this paper is heterogeneous in nature and polemical in purpose. My aim is to collect in one place a number of arguments which have been levelled against the existence of semantic primitives, and to show that these arguments are faulty. The faults to be identified are of various kinds. Perhaps the most serious is the sophism Aristotle called *ignoratio elenchi*, that is, ignorance of what must be proved. Opponents of semantic primitives fall prey to this fault in reasoning when they gratuitously link the notion of semantic primitives with some other doctrine or belief, such as 'objectivism' or 'abstractness', and spend their time refuting this other doctrine or belief even though one may perfectly well believe in semantic primitives while disavowing it.

A related fault is what may be called 'appeal to a bad precedent'. Instead of considering a range of possible or attested views about semantic primitives, the critic singles out a particularly vulnerable or discredited

version of the semantic primitives view, and parades this as proof of the foolishness of the whole idea. Other faulty arguments appeal to language 'facts' which are not facts at all, either because they can be shown to be blatantly false, or because they are the products of inadequate or questionable linguistic analyses. Still others amount to little more than blustery declarations to the effect that the critic finds the concept of semantic primitives implausible or can't imagine how it could be made to work.

I personally adhere to the view of semantic primitives which has been expounded for some 25 years by Anna Wierzbicka (1972, 1980, 1992, 1996), and which in recent times has come to be known as the 'natural semantic metalanguage' approach to semantic analysis. This approach can be seen as continuing the tradition of conceptual analysis associated with seventeenth century rationalist philosophers such as Descartes, Pascal, Arnauld, and, above all, Leibniz—with one important difference. The work of Wierzbicka and colleagues is not primarily speculative or philosophical, but is based on intensive empirical and descriptive work on the semantics of a wide range of languages. This work has led to a set of a highly concrete proposals about a hypothesised irreducible core of all human languages. This universal core is believed to have a fully 'language-like' character in the sense that it consists of a lexicon of semantic primitives together with a syntax governing how the primitives can be combined. About 60 firm candidates for semantic primitives have been identified so far (Wierzbicka 1996; Goddard and Wierzbicka Eds, To appear); for example, the primary meanings of the English words I, YOU, SOMEONE, SOMETHING, THIS, THE SAME, THINK, WANT, KNOW, SAY, DO, HAPPEN, GOOD, BAD, WHEN/TIME, WHERE/PLACE, BECAUSE, NOT, MAYBE, LIKE, KIND OF, and PART OF.

The NSM system can claim to be one of the most comprehensive and widely applied approaches to descriptive semantics in modern linguistics. The NSM bibliography is extensive: over a dozen books and several hundred scholarly articles, mostly by Anna Wierzbicka, but also by Felix Ameka, Hilary Chappell, Nick Enfield, Cliff Goddard, Jean Harkins, Rie Hasada, Masayuki Onishi, Catherine Travis, Bert Peeters, David Wilkins, and others (for references, see Wierzbicka 1992, 1996). The areas covered include speech-act verbs, ethnopsychological and ethnophilosophical terms, colour words, words for artefacts and natural kinds, discourse particles and other illocutionary devices, passives, causatives, oblique case-marking, and various other grammatical constructions. Languages involved include English, Polish, Russian, French, Japanese, Chinese, Ewe, Yankunytjatjara, Malay, and Arrernte.

The aim of the present exercise is, then, to defend the conception of the semantic primitives which is integral to 'natural semantic metalanguage' approach. In the process I hope also to clarify to some extent how the NSM position relates to other current approaches to semantics, and how it bears on certain issues such as prototype effects, language acquisition, and sentence processing. With this in mind I have selected 'anti-primitives' arguments from a fairly wide range of sources, including studies in cognitive linguistics, psycholinguistics, language acquisition, and the philosophy of language.

The NSM view of semantic primitives

The NSM approach starts with the premise that semantic analysis must be conducted in natural language, rather than in terms of technical formalisms (abstract features, logical symbols, etc.), if only because technical formalisms are not clear until and unless they are explained in ordinary language. A second premise is that the full meaning of any semantically complex expression can be stated in terms of a reductive paraphrase in ordinary language, i.e. an equivalent expression composed exclusively of simpler meanings than the original. It follows from these premises that every language must have an irreducible semantic core consisting of a mini-lexicon of indefinable expressions (semantic primitives) and a mini-syntax governing how they can be combined. Such a system of primitive meanings and grammar is termed a 'natural semantic metalanguage' (NSM). A third assumption, supported by the available evidence, is that the NSMs of all human languages are essentially isomorphic, i.e. that all languages possess concrete linguistic exponents (words, bound morphemes, or phrasemes) of the same set of semantic primes, and that these exponents share a common set of combinatorial properties, cf. Goddard and Wierzbicka Eds (1994), Goddard Ed. (1997).

In some respects the premises of the NSM approach resemble those of Ray Jackendoff (1983, 1990), who also believes in a single set of universal conceptual primitives which manifests itself in both lexical and grammatical domains, except for the fact that in Jackendoff's view the primitives are 'abstract' and not to be identified with ordinary word-meanings. Another point of divergence is that Wierzbicka believes that semantic analysis can (and should) be exhaustive, whereas Jackendoff allows for a residue of unstatable meaning to remain even after analysis into conceptual primitives

has gone as far as it can go. On account of these differences, it can fairly be said that Wierzbicka's NSM theory is unique.

After thirty years of trial-and-error experimentation in different semantic domains, and taking into account a number of careful cross-linguistic studies, the current inventory of proposed semantic primes numbers nearly 60. These elements are held to designate meanings which are impervious to (non-circular) definition and universal in the sense of having equivalents in all languages. It is not necessary here to discuss their syntax in any detail, except to say that it is quite rich and 'language-like', cf. Wierzbicka (1996: Ch. 3), Goddard Ed. (1997), Goddard and Wierzbicka Eds (To appear). It bears little resemblance to the syntax of logical systems such as the predicate calculus, and no real resemblance to the crude operations of 'bundling' or 'concatenation' employed in many systems of 'feature analysis'.

On the NSM approach, the meaning of a semantically complex expression (be it a word or a grammatical construction) is described by means of an explanatory reductive paraphrase (an 'explication') framed entirely within the semantic metalanguage. By relying on a specified, minimal metalanguage the approach aims to maximise explicitness, clarity, and translatability. NSM explications can, however, readily accommodate vagueness and reflect elements of subjectivity.

These various points can be illustrated by the following three explications. Explication (A) shows a semantic description for the word *lie* (Wierzbicka 1990), which, it can argued, is fully compatible with the prototypicality effect described by Coleman and Kay (1981). Notice in particular that the final component of this explication is a reference to a social evaluation. From a syntactic point of view, the explication exemplifies the use of certain complement and argument structures, e.g. the 'addressee' argument of SAY, the sentential complements of WANT and KNOW.

A. X *lied* to Y =
   X said something to person Y
   X knew it was not true
   X said it because X wanted Y to think it was true
   people think it is bad if someone does this

Explication (B) is for the causative verb *break* in one of its meanings (perhaps the most central one). The explication depicts a chain of causation, beginning with the agent doing something to the object (Y), which brings about an immediate effect on Y, which results in a consequent state ('Y

was not one thing any more'). Notice that this explication is rather more sophisticated than the conventional story about 'causative-inchoatives'; in particular, the final component conveys the idea of a disruption to or interference with the previous state of the object (cf. Goddard 1998).

B. X *broke* Y =
X did something to thing Y
because of this, something happened to Y at this time
because of this, after this Y was not one thing any more

Explication (C), for the emotion term *happy*, shows how a prototypical cognitive scenario can be incorporated into an explication. The feeling experienced by X is not described directly; rather it is described as LIKE the good feeling experienced by a person who thinks certain prototypical thoughts ('something good happened to me, I wanted this, I don't want anything else now'). This approach to emotion semantics allows a great deal of subtle differentiation between closely related emotions (e.g. *happy, joyful, pleased, content*, *related, jubilant,* and so on); cf. Wierzbicka 1996, Bardzokas and Dirven (forthcoming).

C. X was *happy* =
X felt something because X thought something
Sometimes a person thinks something like this:
some very good things happened to me
I wanted things like this to happen
I don't want other things now
When this person thinks this, this person feels something good
X felt something like this
because X thought something like this

Of course, explications (A)–(C) could bear much further discussion and justification; we do not have the space for that here. For our purposes, their role is just to give some idea of what NSM explications look like. And, above all, to underline the point that although they are composed exclusively of proposed semantic primitives, they are unlike other types of semantic representation which employ semantic primitives precisely in being thoroughly 'language-like'. NSM explications are essentially 'texts' composed in a specified subset of ordinary language. They are not 'lists of necessary and sufficient conditions', or 'bundles of features', or 'annotated

tree diagrams of features and categories', etc.

The full listing of currently proposed primes is given in Table 1. In addition, it should be mentioned that there are several other elements under consideration, notably TOUCH, BODY, HAVE and TRUE.

Table 1: Proposed semantic primes (after Wierzbicka 1996)

Substantives:	I, YOU, SOMEONE, PEOPLE/PERSON; SOMETHING/THING
Determiners:	THIS, THE SAME, OTHER/ELSE
Quantifiers:	ONE, TWO, ALL, MANY/MUCH, SOME
Mental predicates:	WANT, FEEL, THINK, KNOW, SEE, HEAR
Speech:	SAY, WORD
Actions, events, movement:	DO, HAPPEN, MOVE
Existence:	THERE IS
Life:	LIVE, DIE
Evaluators and descriptors:	GOOD, BAD, BIG, SMALL
Time:	WHEN/TIME, NOW, AFTER, BEFORE, A LONG TIME, A SHORT TIME, FOR SOME TIME
Space:	WHERE/PLACE, HERE, UNDER, ABOVE; FAR, NEAR; SIDE; INSIDE
Logical concepts:	NOT, MAYBE, IF, CAN, BECAUSE
Intensifier, augmentor:	VERY, MORE
Taxonomy, partonomy:	KIND OF, PART OF
Similarity:	LIKE

To conclude this brief exposition, a number of points about the NSM conception of semantic primitives deserve to be reiterated. First, the NSM concept of meaning is what philosophers know as a purely intensional one. Semantic primes can be thought of as linguistically embodied conceptual primes; but they are emphatically not supposed to correspond to any entities or facts in a postulated 'objective reality'.

Second, the NSM primitives are not held to be 'abstract'. They correspond to word-meanings in ordinary language.

Third, Wierzbicka's advocacy of semantic primitives is not purely speculative. At all times since 1972 (the publication date of her book *Semantic Primitives*), she has worked with a specific set of primitives. Admittedly, the set has expanded in size markedly over the years (starting from an austere 13 in its 1972 version, up to 30 in 1992, and now at about 60), but this does not detract from the point that there has always been a clearly identified set of posited primitives.

Fourth, although it is hard to see how many of the primitives could be derived solely from sensory experience, the NSM position is not bound to any view about how people acquire semantic primes (other than to opposition to crude empiricism).

Fifth, there is no claim that people, in the normal course of linguistic thinking, compose their thoughts directly in terms of semantic primitives; or, conversely, that normal processes of comprehension involve real-time decomposition down to the level of semantic primitives.

Plan of the remainder of the paper

In §2 I run through some examples of appeal to false or misanalysed 'facts' about language, then in §3 consider some objections which amount to mere assertion, often accompanied by an apparent ignorance of the relevant literature. In absolute terms, arguments (or non-arguments) of these two kinds do not carry much weight, but if presented with sufficient conviction and appropriate rhetoric they can seem quite powerful, especially when they are found in textbooks, or in works addressed to non-linguistic audiences (such as philosophers). My main targets in §2 and §3 will be Rom Harré and Michael Krausz, Roy Harris, and Jean Aitchison. In §4, I will take on the more serious *ignoratio elenchi* arguments, which invalidly link advocacy of semantic primitives with 'objectivism', logical atomism, positivism, etc., or with abstractness and non-verifiability, or with implausible views about language acquisition or language processing. This section also deals briefly with the related 'appeal to bad precedent' arguments. My focus in §4 will be on work by George Lakoff, Jean Aitchison, John Taylor, Susan Carey, and Melissa Bowerman. Concluding remarks form §5.

Before beginning, I would like to say that my selection of these authors for criticism does not mean that I have a low opinion of their work. Generally speaking, the opposite is the case. However, it is all the more necessary to refute bad arguments when they come from scholars whose work is widely respected, because their bad arguments are more likely to be taken seriously than are bad arguments which come from people of lesser credibility.

§2. FALSE 'FACTS' AND MISANALYSED DATA

An earlier tradition of anthropology, which concerned itself with elucidating the 'primitive thinking' of indigenous peoples, leaned heavily

on misinformation about indigenous languages. As we will see shortly, this tradition is not altogether dead in anthropology. It is, however, unnerving to find sophisticated modern philosophers making elementary mistakes about language data. My example in this respect comes from the recent book by Rom Harré and Michael Krausz (1996), titled *Varieties of Relativism*. In their chapter on 'Semantic relativism' the authors explicitly address Wierzbicka's (1992) claim that 'there is a small number of words for which exact equivalents exist in all languages and in every culture'. Since this situation would be incompatible with their own radical relativism, Harré and Krausz (pp. 41-48) set out to refute Wierzbicka's claim.

The authors begin by saying that Wierzbicka postulates 'about ten universal concepts'—an obvious indication that they haven't read Wierzbicka (1992), which claims that there are over 30 of them. Harré and Krausz do not bother to let the reader know the identity of any of the claimed primitives except for I and THIS, which according to them are the 'two most plausible candidates'. They also distort the situation when they say that 'Wierzbicka does not present an argument for her claim. She simply asserts it'. This ignores the argumentation in Wierzbicka (1976), Wierzbicka (1980), and Goddard and Wierzbicka Eds (1994); but Harré and Krausz do not see fit to mention these works, or any others in Wierzbicka's long period of distinguished scholarship. Rather, they propose to undo her by refuting just one example: 'We shall now try to show that the English pronoun 'I' is not fully translatable into all languages, nor can the first person devices of many languages be fully translated as 'I'' (p.43).

Their argument begins with the proposition that 'I' indexes 'the bodily location of the speaker'; a first person pronoun, according to them, 'is effective only for those persons who are familiar with the location of the speaker in the array of embodied persons' (p.44, 45). This is a dubious claim in its own right; but let's stay with Harré and Krausz, and see where they lead us. On the assumption that the role of pronouns is locative, they argue that on any occasion of use a first person pronoun is 'multiply indexical', because it indexes the location of the speaker not only in the spatial 'manifold', but in other 'manifolds', such as the temporal, the moral, and the social. Languages may differ, they say, in the way in ways in which their first person references index the speaker in terms of these multiple manifolds, thus leading to non-translatability.

Their first example is from Wintu. Citing Lee (1950), they say that the 'Wintu first person singular indexical', *-da*, does not necessarily demonstrate a one-to-one correspondence 'between person as speaker and his or her

body', but:

> ... in some circumstances can index the content of an utterance with the location of more than one body. Thus *lime-da* is rendered as 'I am ill' but *tuhutum-lim-tca-da*, which we perforce must render as 'My mother is ill', should run more like 'The compound body of mummy and me is where illness resides'. The infix *-tca-* indicates the spatial diffusion of the *-da* index... We conclude that the Wierzbicka thesis as regards the spatial indexicality of the first person is false'. (Harré and Krausz 1996: p47, 48)

The reality is rather different. Harré and Krausz fail to mention that the bizarre-sounding 'compound body' translation and the interpretation of *-tca-* as a 'spatial diffusion' suffix are their own inventions, not Lee's. In fact Lee (1950: 540) says that this use of the suffix *-tca-* implies 'involvement', and offers 'My mother got sick on me' as probably the closest English translation. But more importantly, Harré and Krausz omit the crucial information that Wintu has a set of independent pronominal words, including *ni* 'I' and *mi* 'you'! The full set is found in Pitkin (1984); *ni* 'I' is even mentioned in Harré and Krausz's source article, Lee (1950), which one can only assume Harré and Krausz's have not read with any great attention.

Harré and Krausz's next example can be used to demonstrate what I mean by reliance on a questionable analysis. It concerns the case of pronouns in Kawi, which, according to Becker and Oka (1974), are supposed to be built out of three elements: a locative, spatial and/or temporal element *i-* and/or *nga-*, a deictic formative (*-k-*), and a person index (*-i*, *-a*, *-u*). Harré and Krausz declare: 'Since English pronouns do not index utterances with times, Kawi first person forms cannot be translated without remainder by English 'I''. Harré and Krausz do not acknowledge that Becker and Oka's analysis of the 1sg *aku* into three single-segment morphemes (*a-k-u*) would be regarded by most linguists as highly implausible.

Perhaps not surprisingly, false or questionable language data tends to concern 'out of the way' languages, which are difficult for the sceptical reader to check up on. Wintu, for example, is an extinct language of northern California. Kawi is Old Javanese, an ancient language known only through texts. (Notably, Harré and Krausz do not disclose these facts.)

To round out this section, I will give two further examples of questionable analyses. First, various anthropologists writing in the 'primitive thought' tradition, including Hallpike (1979), have claimed that some indigenous languages do not draw any distinction between mental predicates like THINK and KNOW (both claimed by Wierzbicka to be semantic primitives) and perceptual ones like 'hear' or 'listen'. But, where data is available, this

claim turns out to be a confusion based on failure to understand polysemy. For example, in English one can say *I see what you mean,* but no-one concludes from this that the English word *see* is semantically general over visual perception and understanding; the correct conclusion, which can be supported by a range of syntactic as well as semantic evidence, is that *see* has two distinct meanings (just as the French verb *entendre* has two distinct meanings 'hear' and 'understand'). What holds for English and French holds also for 'exotic' languages such as Pitjantjatjara, despite the claims of Bain (1992: 86) that: 'There is no way to differentiate the concept of thinking, listening, and heeding in Pitjantjatjara. The same verb *kulini* does duty for all'. When looked at closely it turns out that it is not difficult to differentiate the three senses ('think', 'listen', 'heed') on language-internal grounds, since each of the senses just mentioned has a distinct syntactic frame, from which the others are excluded (cf. Goddard 1991).

Second, various authors, for example Reilly (1986), have claimed that particular languages do not distinguish between IF and WHEN (both claimed to be semantic primitives by Wierzbicka), having only one morpheme for both concepts. Taking German as an example, Reilly says that the same word *wenn* is used both for WHEN, as in *When Clare comes home, we'll have lunch*, and for English IF, as in *If Clare comes home, we'll have lunch.* But as Wierzbicka (1996 191) points out, German does lexically distinguish between IF (*wenn*) and WHEN (*als, wann*)—except in subordinate clauses referring to future events, such as those quoted by Reilly (in which *wenn* is polysemous, and means either IF or WHEN). A slightly different situation obtains in Japanese, a language which has also been claimed not to distinguish IF from WHEN. It is true that Japanese constructions employing conjunctive *-ba* can be (and commonly are) used in both temporal and conditional contexts. As Hasada (1997) points out, however, if the particle *moshi* in employed, the *-ba* construction becomes unambiguously conditional; thus, the pattern *moshi* + *-ba* can be regarded as the Japanese exponent of the IF construction.

Over the years, NSM researchers have often been challenged with the statement that 'primitive X is not found in language Y'. And rightly so. The NSM theory's claim that all languages possess discrete linguistic exponents for the posited primitives is a strong one, and it is wide open to empirical disconfirmation. As a matter of fact, many meanings which might strike one on pre-theoretical grounds as plausible candidates as lexical universals can be shown not to have equivalents in some languages—words

like ‘sun’, ‘hand’, and ‘break’, for instance (cf. Nida 1947; Goddard In press). Some earlier candidates for primitivehood, such as ‘imagine’ and ‘world’, were struck off the inventory of NSM primitives for this reason. The current inventory, however, appears to be cross-linguistically quite robust (cf. Goddard and Wierzbicka Eds 1994, Goddard Ed. 1997)—provided that the analytical standards which are usual in modern linguistics are observed.

§3. PREJUDICIAL LANGUAGE AND MERE ASSERTION

In this section, I will adduce a few examples of what, properly speaking, are not arguments at all, because they do not proceed from premises to conclusions. They simply amount to the author asserting the foolishness or unworkability of semantic primitives, usually in conjunction with an emotional appeal conveyed by the use of prejudicial language. Offenders on this score also often seem to be unaware of relevant literature (or, on a less charitable interpretation, to have deliberately decided not to acknowledge it).

An excellent example is furnished by a passage in Roy Harris’s (1981) book *The Language Myth*. Harris’s main concern in this book is to argue that conventional linguistics is founded on two erroneous theories which he calls the ‘telementation fallacy’ and the ‘fixed code fallacy’. Along the way, he finds it necessary to argue against the whole concept of verbal definition, which he labels the ‘substitution theory’. To dismiss the possibility of verbal definition, this is what he has to say:

> The substitution theory of the word has, however, its Achilles’ heel. ... For what are the words which occur in the definition substitutes? If these words are in turn substitutes for longer descriptions, the same question may then be raised concerning the words in those definitions. And so on. Behind the definition of a single word there thus appears to lie a regress of further definitions, which has no clearly discernible end point. For we cannot envisage what kind of verbal substitution it could be which would render any further substitution superfluous. (Harris 1981: 140)

This is an extraordinary passage to come from someone who writes as a philosopher of language. It conveys the impression that the question of where definitions come to an end has never been thought of before. In fact, of course, this was one of the main questions which preoccupied thinkers such Arnauld, Descartes, Pascal, and Leibniz, who all enunciated the obvious conclusion that interverbal definition presupposes the existence of some primitive words. To make this point more concrete I will adduce the following

quotation from Arnauld and Nicole's famous text *The Art of Thinking* (which, I might add, is included among the references of Harris's own book):

> ... I say it would be impossible to define every word. For in order to define a word it is necessary to use other words designating the idea we want to connect to the idea being defined. And if we wished to define the words used to explain that word, we would need still others and so on to infinity. Consequently, we necessarily have to stop at primitive terms which are undefined. (Arnauld and Nicole 1996[1662]: 64)

Perhaps the burden of Harris's argument is intended to fall upon his final sentence: 'We cannot envisage what kind of verbal substitution it could be which would render any further substitution superfluous'. In so saying, Harris chooses not to mention the notion, so important to thinkers like Arnauld, Descartes, and Leibniz, that some words designate 'simple ideas', i.e. meanings which are so simple that no further explanation is either possible or necessary—in modern terminology, conceptual primitives. (For example, in relation to the Cartesian proposition 'I think, therefore I am', Arnauld (1996: 29) says that the terms *to be* and *to think* are self-explanatory and indefinable: 'No one needs to ask for an explanation of these terms because they are among those everyone understands so well that trying to explain them only obscures them'.)

To be fair to Harris, a few pages later he does admit that could be a 'possibility of discovering "prime categories" common to all conceptual systems, in spite of the acknowledged differences between the systems' (p.144). If this were true, it would be possible to use these prime categories as a kind of bridge between otherwise different conceptual systems. Speaking against this idea, Harris simply voices his own incredulity:

> In short, it [i.e. this approach] would need to fall back on postulating in some form or other a basic set of invariant concepts common to all mankind. But the plausible candidates for this basic set are so few that one fails to see how it would be possible to 'generate' from them, by whatever universal rules, semantic systems of the complexity and diversity found in the vocabulary of the world's languages. (Harris 1981: 146)

It is hardly surprising that Harris 'fails to see' how a system of conceptual primitives could generate a great diversity of complex concepts, or that he can't imagine there being more than a few plausible candidates, since he is apparently unaware of the relevant literature. Harris' tactic of ignoring ideas he prefers not to deal with is accompanied by another hallmark of faulty argumentation, namely, use of prejudicial language. Harris's penchant is for coining disagreeable terms to label the views he is against ('telementation', 'fixed code', 'substitution theory', and so on). We can see

a more extreme example of this practice in Jean Aitchison's (1987) textbook *Words in the Mind.* In her introductory account of semantic issues, Aitchison sets up a 'bad guy–good guy' dichotomy between the idea that words have 'fixed meanings' (bad guy) vs. the idea that they have 'vague boundaries and fuzzy edges' (good guy). On this latter view, she continues: 'Word meanings cannot be pinned down, as if they were dead insects. Instead they flutter around elusively like live butterflies' (p.40). Well, goodness me—who *wouldn't* prefer to think of meanings as 'live butterflies', rather than as 'dead insects'?

To the alert reader (though not, perhaps, to introductory level students) Aitchison's bias is obvious. It is no surprise that to find that her chapter on semantic primitives (pp.62-71) bears the ridiculous title 'The Primordial Atomic Globule Hunt'. After a woefully inadequate survey of arguments against semantic primitives, Aitchison closes the chapter by speaking of people who have 'atomic globule faith' as if they have a psychological problem. What could explain such an (irrational) view, she asks, aside from 'descriptive convenience and wishful thinking' (p.70).

I will be coming back to Aitchison in §4, but this is probably the best place to observe that, like Harris, she seems quite unaware of relevant linguistic literature. *Words in the Mind* appeared in 1987, yet, with the exception of a passing reference to Jackendoff (1983), her most recent reference on semantic primitives dates from 1976. One of her most telling points is:

> The most obvious way of promoting the atomic globule viewpoint would be to identify the semantic primitives. Unfortunately, most of those who believe in their existence have been quite vague and evasive as to what they might actually be. (Aitchison 1987: 65)

It is hardly surprising therefore that Aitchison doesn't mention Wierzbicka, who has since 1972 argued for the existence of a specific, concrete set of primitives. If Aitchison acknowledged such work, she would have to argue against it. Why bother, when you can just ignore it?

§4. *Ignoratio Elenchi*

The sophism traditionally known as *ignoratio elenchi* is explained by the Oxford Dictionary as follows: 'argument that appears to refute an opponent while actually disproving something not asserted by him'. In this section I will discuss three commonly found examples of this style of fallacious reasoning in discussions of semantic primitives. The first wrongly links semantic primitives with 'objectivism'. The second wrongly assumes

that a commitment to semantic primitives is a commitment to the view that meaning is processed 'bottom up' from the level of primitives in every utterance. The third wrongly assumes that primitives are necessarily abstract or 'theory laden'.

We can begin with George Lakoff's (1987) widely cited book *Women, Fire and Dangerous Things*, which has as one of its main themes a critique of 'objectivism', that is, the attempt to define meaning in terms of correspondence with facts or entities in a presumed, objective 'real world'. This approach to meaning was adopted in the first half of this century by the philosophical movements known as positivism, verificationism, and logical atomism (associated with names such as Ayer and Russell). In linguistics it survives, in various guises, in systems of truth-conditional and 'formal' semantics.

Lakoff insists that such approaches are incompatible with what we know about human cognition as reflected in language. In particular, objectivist approaches are incompatible with the fact that languages enable people to effect multiply different construals of the same situation; with the fact that many semantic categories are culturally and linguistically constituted and do not correspond with any real-world objects; and with the fact that different languages can embody radically different conceptualisations. I must say that I have no quarrel with any of this. Indeed, broadly speaking, Lakoff and Wierzbicka can both be counted as members of the 'cognitive linguistics' movement, in virtue of their shared emphasis on a conceptual account of meaning and their shared opposition to 'formal' generative linguistics.

However, Lakoff's critique of 'objectivist semantics' goes astray on the question of its relationship to conceptual primitives. He argues that objectivist semantics is wedded to 'the doctrine of atomism, according to which concepts are either complex or primitive; primitives have no internal structure and are cognitively simpler than complex concepts'. This doctrine, he says, is integral to objectivist semantics: it is 'a consequence of real-world atomism and the assumption that meaning is based on truth (Lakoff 1987: 203).

All well and good—provided that by 'doctrine of conceptual atomism' one is talking about logical atomism in the style of Russell, or something similar. For, obviously, although objectivist semantics may be committed to a doctrine of objectivist semantic primitives, it doesn't follow that this is the only possible theory of semantic primitives. As we know, there is a vibrant contemporary school of semantics based on a strictly conceptual theory of semantic primitives, under which they are purely semiotic elements

(i.e. not based on or reducible to any external reality)[1]. Lakoff simply ignores this work, and the long tradition of philosophical work on conceptual primitives.

Incidentally, a similar mistake is made by Grace (1987), in his book *The Linguistic Construction of Reality*. Though Grace doesn't mention the term 'semantic primitive' he too paints a false dichotomy. In the bad corner, there is the 'reality mapping' approach which holds that meanings are determinate and translatable (and which, by implication, is committed to the existence of objectivist semantic primitives). In the good corner, there is the 'reality construction' approach, which believes that meanings are essentially indeterminate and non-translatable (and which therefore must reject the idea of semantic primitives). This false dichotomy leaves no room for the notion of primitives which are purely creatures of the mind (i.e. genuinely conceptual, non-objectivist).

But to return to Lakoff (1987): When he settles down to a dedicated discussion of 'The Issue of Primitives' (pp.279-280) the picture he paints is decidedly peculiar. Without reference to any of the literature, modern or earlier, he states:

> In objectivist semantic systems, the following criteria converge to characterize what a conceptual primitive is. When we say that a conceptual system has primitives, we usually mean that all of the following conditions hold:
>
> 1. There are fully productive principles of semantic composition, building up more complex concepts from less complex ones. Those concepts not put together by fully productive principles of semantic composition are primitive.
>
> 2. Every concept either has an internal structure or it does not. The ones with internal structure are complex. The concepts with no internal structure are primitive.
>
> 3. Some concepts get their meaning directly. These are the primitive concepts. Other concepts—the complex ones—get their meaning indirectly via the principles of composition that relate them to primitive concepts. (Lakoff 1987: 279)

Lakoff argues that these three conditions of his are inconsistent with one another, in view of what we know about conceptual structure. Personally, I would say that conditions (1) and (2) are a valid characterisation of the semantic primitives position (though I am not so sure that they are really distinct conditions). But condition (3) is phrased in a peculiar and vague

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[1] A clear statement to this effect can be found in Wierzbicka (1991: 16-17): 'In natural language, meaning consists in human interpretation of the world. It is subjective, it is anthropocentric, it reflects predominant cultural concerns and culture-specific modes of social interaction as much as any objective features of the world "as such"'.

way. What does it mean to say that concepts 'get their meaning' directly (or indirectly)? This is certainly not a formulation that I have ever heard from any supporter of semantic primitives.

The reason for this formulation becomes clearer when we see what use Lakoff makes of it. Essentially, he argues that certain types of concept are 'directly meaningful', and so would be primitive under condition (3); but that these same concepts do have 'internal structure', so violating condition (2). One type of concept Lakoff has in mind are so-called 'basic-level concepts', as identified by Eleanor Rosch and others. These are 'experience-near' concepts represented by common concrete words like *man, chair, dog*, etc. Research has shown that concepts like these are psychologically 'basic' when compared both with more abstract terms (like *human, furniture, dog*), on the one hand, and with more specific terms (like *policeman, stool, poodle*), on the other. Basic-level concepts are often said to be associated with an experiential gestalt.

Now, says Lakoff, consider a basic-level concept like MAN.

> It comes with a rich mental image, characterising overall shape. But the mental image also comes with an image schematic structure. The image of a man is structured as having an UP-DOWN organisation; it is structured as having an INSIDE and an OUTSIDE; it is also structured as a WHOLE with PARTS; and so on. In general, rich mental images are structured by image-schemas, but they are not exclusively structured by them. (Lakoff 1987: 280).

By 'image schemas' (such as UP-DOWN, INSIDE-OUTSIDE, and WHOLE-PART) Lakoff is referring to a semantic construct promoted by Mark Johnson (1987) and himself. Roughly speaking, image schemas are supposed to be pre-conceptual, embodied experiential schemas. As far as I can follow him, Lakoff is saying that *man* would have to be a 'conceptual primitive' because, as a basic-level concept, it 'gets its meaning directly'.[2] But *man* also contains various image-schemas, so that gives it internal structure, so it cannot be primitive. Lakoff's (1987: 280) conclusion: 'The only sensible recourse is to give up on the traditional concept of a primitive'.

It is not difficult to see that this argument depends on vague and shifting use of terms like 'directly (meaningful)', 'primitive', and 'basic'. Even more importantly, perhaps, it misrepresents what it means to be a semantic or comceptual 'primitive'.There is no reason at all why one cannot regard a term like *man* as corresponding to an experiential gestalt, and

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[2] Actually, as far as I know, no-one (aside from Lakoff) has ever proposed MAN as a semantic primitive. Scholars working in the Componential Analysis framework have often treated MALE as a primitive term, but not MAN.

being ‘directly meaningful’ in some kind of phenomenological sense, while at the same time maintaining that the concept *man* is conceptually complex, and therefore not a conceptual primitive. Actually, Lakoff himself had earlier admitted as much:

> “basic” does not mean “primitive”: that is, basic level concepts are not atomic building blocks without internal structure. The basic level is an intermediate level; it is neither the highest or the lowest level of conceptual organisation. (Lakoff 1987: 270)

This does not exhaust Lakoff’s arguments against conceptual primitives. He also makes an argument based on sentence processing, which one also can find represented in Aitchison (1987), in Carey (1982), and in various other works. This argument refers not only to Rosch’s work, but also to psycholinguistic experiments (e.g. Fodor *et al.* 1975, Cutler 1983) which supposedly show that it does not take people any longer to process semantically complex words (e.g. *bachelor*, *break*) than to process the definitions of the same words (e.g. ‘unmarried man’, ‘cause to break’). Aitchison (1987) places great weight on such experiments. ‘[I]f small components of meaning exist, then words might be stored in the human mind in a disassembled state’, she says (p.67f). But ‘experiments have not revealed any trace of assemblage procedures when people comprehend words’ (p.69).

Carey (1982) says that a key assumption of what she calls the ‘classical view’ of semantics is that: ‘in production, a message is formulated in conceptual primitives and then packaged in words of the language’, and conversely for comprehension (p.350); and she too notes that this assumption is at odds with the Fodor *et al.* study. However, Carey is discerning enough to acknowledge two alternative interpretations: first, the possibility that ‘the particular definitional analyses they chose were wrong’[3]; and second,

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[3] Reliance on faulty definitions is a major fault with another paper which is widely cited in the secondary literature, namely Fodor *et al* (1980) ‘Against definitions’. This study investigated respondents’ intuitions about the closeness of the relationship between the surface subject and surface object in sentences with causative verbs like *break* and *kill*. The hypothesis was that if such verbs were semantically biclausal (e.g. *X broke Y= X caused Y to break*), as was widely assumed in generative semantic literature of the time, then in semantic representation Y was not a patient, on a par with the direct object/patient in a sentence like *X bit Y*. Respondents were asked to compare the closeness of the relationship between X and Y in sentences with verbs like *break* as opposed to verbs like *bite*. When no significant differences were found, Fodor *et al* concluded that semantic structures have no psychological reality. However, the entire line of argument folds in the face of independent evidence that the semantic structure of causatives like *break* includes the component *X did something to Y* (see explication (B) in §1), which means that Y is equally a direct object/patient in both *X broke Y* and *X bit Y*.

the possibility that 'concepts are *definitionally* and *developmentally* compositional and yet are not decomposed during comprehension' (p.356); in other words, that 'definitionally primitive' does not necessarily equal 'computationally primitive' (p.351). Indeed, Carey observes that many supporters of the classical view have explicitly assumed that normal speech production and comprehension involves large-scale semantic 'chunking' and routinisation.

In other words, when opponents of semantic primitives refute the assumption that 'word meanings are stored in a disassembled state', as Aitchison puts it, they are refuting an assumption which is not held by most upholders of semantic primitives, and one which is certainly not entailed by the semantic primitives position.[4] Wierzbicka, in particular, has consistently pointed to the cognitive significance of language-specific lexicalisations. To make this point more concrete, consider the following quotation from a recent paper:

> *Inye vesc̆i na inom jazyke ne mysljatsja* 'there are some things which cannot be thought in another language', wrote the poet Marina Tsvetaeva (1972: 151). In a theoretical sense, this statement may be somewhat of an exaggeration, if, as the NSM theory contends, any culture-specific concept can be decomposed into a translatable configuration of semantic primes. ... But in an important sense, Tsvetaeva's statement remains true, because in practice it is impossible to formulate and manipulate thoughts of any sophistication without resort to the kind of conceptual 'chunking' enabled by the use of complex lexical items. Thoughts related to [Russian] *dusa*, for example, can be formulated in English only with great difficulty and at the cost of cognitive fluency, whereas in Russian they can be formulated more or less effortlessly. (Goddard and Wierzbicka 1995: 56-9)

I don't want to imply that the NSM position on semantic primitives makes no predictions whatever about language processing. It does predict,

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[4] Admittedly, supporters of the ancient idea of a *lingua mentalis* 'mental language' (in modern terms, 'mentalese') did sometimes express themselves in this way. For example, William of Ockham wrote: 'Whenever someone utters a vocal sentence he forms earlier, innerly, a mental sentence, which does not belong to any specific language' (Boehner 1957: 39, cited Wierzbicka 1980: 33, her translation). This seems to imply that to express thoughts in a particular 'vocal language' would involve a process of mental translation. On the other hand, Leibniz saw that learning a vocal language is learning, virtually as second nature, a complex conceptual skill; a skill which, once learnt, puts its stamp on habitual thinking: 'Most of our reasonings ... are performed by playing with characters, as we play the piano partly by habit, without the mind (soul) being quite conscious of it... Otherwise we would speak too slowly' (Dascal 1987: frontispiece).

or so it seems to me, that sentences composed of NSM semantic primitives should be very easy to comprehend. As far as I know, however, no experimental work has been done on this question.[5]

The third misguided objection to semantic primitives I will consider was, as far as I know, first voiced by Carey (1982) in the same article I referred to a moment ago. It has recently been repeated, and endorsed, by Bowerman (1996). Carey points out that most proposed semantic primitives, especially those which have fed into research on language acquisition, are highly abstract and 'theory-laden'. For example, in his analysis of dimensional adjectives like *long*, *tall*, *short*, *wide*, *narrow*, *thick*, and *thin*, Bierwisch (1967) posited the components /primary/, /secondary/, and /tertiary/, to capture the order of relative salience of length/height, width, and thickness. 'Elegant and abstract' features like these, Carey observes, represent a sophisticated schematization of knowledge by the linguistic community, and therefore are not likely candidates for what she calls 'developmental primitives', i.e. 'innate ... or at least very early-acquired concepts, out of which all other concepts are built' (p351).

> The semantic components revealed by semantic analysis of the adult lexicon cannot be expected to be the primitives over which the child forms his hypotheses about the meanings of words. Often those components are theoretical terms in theories the child has not yet encountered. (Carey 1982: 374)

In my opinion, Carey's argument that abstract features are not plausible as developmental primitives is convincing. If we assume that there is developmental continuity between the semantic system of the young child and that of an adult, we could conclude that abstract features are not viable as definitional primitives in the adult system either. The point I want to make, however, is that even for child language Carey's argument is not a refutation of semantic primitives. It is a refutation of abstract, theory-laden primitives—of terms such as /tertiary/, /polarity/, /dimension/, and so on. It simply does not apply to the kind of simple and intuitively clear meanings which Wierzbicka and colleagues have proposed—terms like BIG, SMALL, GOOD, BAD, WANT, DO, SAY, and so on. In fact, generally speaking it appears that the proposed lexical primitives appear quite early, and with relatively high frequencies, in the speech of young children (cf. Goddard 1998: Ch. 12).

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[5] Lakoff (1987: 199) implies that conceptual primitives are *not* among the 'categories that are easiest to process', but since there is no experimental evidence comparing the comprehensibility of "basic level terms" like *man*, *chair*, and *dog* with that of proposed primitives like I, WANT, DO, THIS, GOOD, and so on, there is no empirical basis for Lakoff's claim.

In endorsing Carey's critique of 'theory-laden' primitives, Bowerman (1996) identifies a further, related problem.

> Proposed primitives are usually designated with words of a particular language, often English. Although authors may insist that they do not intend their primitives to be identical with the meanings of words in any actual language, it is not clear what they *do* in fact intend them to mean. (Bowerman 1996: 425)

Though Bowerman does not identify anyone who holds this view, it is true that various proponents of abstract primitives (including Bierwisch, Katz, and McCawley, among others) have indeed insisted on it. Even scholars who use less technical language can be charged with not making themselves clear. Bowerman's example of a 'candidate primitive' is the term 'support', which is often used to describe the spatial meaning of the English preposition *on* (just as 'containment' and 'contiguity' are used to describe *in* and *at*, respectively). Bowerman points out that the English word 'support' includes support from all directions, whereas the nearest German word *stützen* only includes support from below. Interestingly, she notes that the 'two notions of support' are closely aligned with the range of use of 'on'-morphemes in the two languages: for example, English *on* can be used in contexts such as *on the wall*, where German *auf* would not be possible.

The trouble with the term 'support' is, then, (a) that it is language-specific, and (b) that it not clear enough to be regarded as self-explanatory. We are still left with the problem of 'figuring out how "support" is conceptualised in one's language'. Bowerman's (1996: 425) conclusion:

> Invoking semantic primitives... [has] a lulling effect—it makes us think we understand the acquisition process better than we do. To the extent that languages differ in what counts as 'support', as 'containment' (or 'inclusion'), as a 'plane', a 'point' or a 'volume', and so on, these concepts cannot serve as the ultimate building blocks out of which children construct their meanings.

One can agree with Bowerman's argument in relation to suggested primitives such as 'support', 'containment', 'plane', and the other examples she mentions. But the argument becomes faulty when the conclusion is generalised to take in semantic primitives of any kind whatsoever. Wierzbicka's conception of semantic primitives escapes Bowerman's criticisms because, first, the NSM primitives are claimed to be identical in meaning with specific meanings of ordinary English words, such as INSIDE, NEAR, and ABOVE; and second, because it is claimed that the same meanings have concrete linguistic exponents in all languages.

To conclude this section, I want to comment briefly on another strategy which is frequently used by opponents of semantic primitives, a strategy I

have dubbed the 'appeal to a bad precedent' argument. This works by presenting as representative of the semantic primitives position a discredited or implausible version, without acknowledgment of the existence of more viable pro-primitives positions. For example, in her exposition of the primitives position, Carey (1982) presents as an exemplar Eve Clark's (1973) 'Semantic Feature Hypothesis'. This was the hypothesis that the order in which young children acquired the correct meanings of adult words could be predicted from the semantic complexity of the adult words. On the face of it, this seems like a reasonable hypothesis, on the grounds that the more complex the learner's task, the longer it ought to take to master it.

However, to test this hypothesis we need to have reliable methods of establishing the semantic complexity of adult word-meanings. Unfortunately, the feature analyses used in the 1970s did not meet this requirement. Furthermore, Clark's early work was burdened down by a further, implausible assumption, namely, the theory of 'component-by-component acquisition', which Carey summarises as follows: 'the child's incorrect lexical entries are expected to differ from the adult lexical entries by just one or a few features, and the meanings of words within a domain are expected to differ from one another by just one or a few features' (Carey 1992: 354). Not surprisingly, a series of studies disconfirmed predictions made on the basis of these assumptions, and eventually Clark herself abandoned the component-by-component view. But it would wrong to conclude from this story that the semantic primitives position itself is faulty, or that other models which use semantic primitives have nothing to offer language acquisition studies.

To some extent, Carey can be forgiven for offering Clark's 'Semantic Feature Hypothesis' as a representative of semantic primitives theories, firstly, because her article appeared back in 1982; secondly, because the focus of the article was primarily on language acquisition studies; and thirdly, because Carey acknowledges the possibility that the failure of Clark's hypothesis was due to faulty semantic analyses and to the unfounded 'component by component' assumption (rather than the very idea of semantic primitives). Unfortunately, the same defences cannot be raised in relation to John Taylor's (1989: 22-39) treatment of semantic primitives in his popular book *Linguistic Categorization*.

Adopting a rhetorical device which can possibly be traced to Labov (1973), Taylor's expository strategy is to describe a 'classical view' of semantics which is supposed to go back to Aristotle. This so-called classical view is, of course, duly shown to be wrong-headed and outdated, and unable to hold a candle to the sophisticated, modern view which Taylor

himself supports. The Aristotelian theory of categorization, according to Taylor, saw categories as definable by means of 'a conjunction of necessary and sufficient conditions'. This tradition, according to Taylor, was taken up and augmented by twentieth century phonologists, such as Jakobson and Chomsky and Halle, who posited the existence of primitive, abstract, binary phonological features as a device for capturing the necessary and sufficient conditions. The idea of semantic primitives, according to Taylor, arose by analogy from these developments in phonological theory.

Now I would not deny that this account has some veracity in relation to 20th century componentialists and generativists such as Nida, Leech, Bierwisch, Katz, and Postal. But Taylor serves up an edited history which (except for a passing reference to Leibniz) ignores the older rationalist theory of semantic primitives, which had nothing to do with phonology but was based on purely conceptual arguments. He also wrongly employs the label 'classical' to designate a thoroughly modern theory (i.e. abstract binary semantic features).

But the lack of historical veracity is not my primary objection to Taylor's story about semantic primitives. My primary objection is that his account ignores the most plausible and well worked out theory of semantic primitives (i.e. Wierzbicka's NSM theory), leaving the 'pro-primitives' position to be represented by outdated work such as Katz and Postal (1964) and Bierwisch (1967), which can easily be criticised; for example, on account of Katz and Postal's suspect distinction between 'markers' and 'distinguishers', or the counter-intuitive abstractness of Bierwisch's features. Using this kind of reasoning one could argue that Lavoisier must have been wrong about the existence of oxygen because Priestley had been wrong about phlogiston!

§5. CONCLUDING REMARKS

Many readers will be left with two questions. First, what explains the sorry state of argumentation about semantic issues? And second, if all these arguments are bad ones, what would count as a good argument against semantic primitives? I will conclude by taking these questions in turn.

The explanation for the generally poor and ill-informed standard of argumentation in semantics is not too difficult to find. It relates to the generally marginalised and neglected status of semantics as a field of modern linguistics, which in turn can be put down to the fact that 20th century linguistics has been dominated by the twin figures of Leonard Bloomfield and Noam Chomsky: Bloomfield, who thought that semantics lay outside

the scope of scientific linguistics, and Chomsky, whose focus has always been primarily syntactic. The result is that the typical linguistics curriculum is terribly lopsided. Most linguists receive a relatively thorough grounding in formal analysis of language—phonetics, phonology, morphology, and, above all, syntax—but many have only a rudimentary background in semantics. Of course, in time many linguists get interested in semantic questions but often their grounding in semantics remains relatively narrow, tied to whatever their larger interests may be (for example, language acquisition, psycholinguistics, categorization, quantification). They tend not to be interested in semantic description for its own sake. If linguists, and other researchers in the language sciences, had a greater breadth and depth of knowledge and experience in semantics, we would be safer from faulty semantic argumentation.

Now for the second question: What counts as a good argument against semantic primitives? The account of universal semantic primitives given by Wierzbicka and colleagues is extremely vulnerable to empirical disconfirmation, at least in its specifics. First and foremost, primitives are supposed to be indefinable, so any proposed primitive can be undone by a demonstration that it can be defined in a non-circular fashion.

For example, the meaning expressed by the English preposition *on* can, I believe, can be ruled out as a semantic primitive for this reason. Consider the following explication, which is intended to apply to on where it designates a positional relationship betweeen two physical objects. The explication has two components which can be labelled: (a) 'contact' and (b) 'resultant stability of position'.[6]

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[6] In relation to component (a), note that the element TOUCH has only recently been proposed as an addition to the inventory of semantic primitives, and, as such, its cross-linguistic viability is relatively untested. The combinatorial grammar of TOUCH has yet to be fully explored, but it seems clear that it involves a 'locus' slot; that is, if X is touching Y, then X is necessarily touching Y *somewhere*. Notice also that the expression 'X is touching Y somewhere' inherently implies an 'external' locus of contact, a property which helps account for the incompatability of *on* and *in* (or *inside*). In relation to component (b), the phrasing given is preferable to a more literal spelling out of the notion of 'support', e.g. by way of a component such as 'if X were not touching Y like this, X would move (down)', because such a component would not fit contexts such as *The rug is on the floor, He puts his hand on hers, You've got mud on your shoes,* and *Don't put your dirty finger on my clean suit.* Also, *on* is not absolutely incompatible with motion of the object; for example, *The billiard balls were rolling aroung on the table*. An extended treatment of these and other issues, including the range of polysemic extensions of *on,* is undertaken in Goddard (To appear).

thing-X is *on* thing-Y =

(a) X is touching Y somewhere

(b) because X is touching Y like this,
X can be in the same place for some time

As required in view of the very broad range of use of English *on* (even in its primary, positional sense), this explication is equally applicable to sentences as varied as: *The cup is on the table, The spider is on the wall, The fly is on the ceiling, The rug is on the floor, The towel is on the rack* and *The key is on the chain.* It hardly needs saying, of course, that an adequate treatment of the semantics of *on* would require a journal article (or more) in its own right, cf. Goddard (to appear).

Second, NSM primitives are supposed to be universals, in the sense of having concrete linguistic exponents in all languages. Therefore, a proposed primitive can be disconfirmed by a convincing demonstration that there are languages which lack any lexeme, bound morpheme, or phraseme with the required meaning. Notice that it is not enough merely to show that language X has an expression which is seemingly vague as to the contrast between a pair of primitives. For example, it is not a counter-example to the proposed primitive INSIDE to observe that the Spanish preposition *en* can be used in both 'in'-situations and 'on'-situations (cf. Bowerman 1996), because there is a clear Spanish equivalent of INSIDE in the expression *dentro de* 'inside of'. But it is a counter-argument to a proposed primitive ON to show that in Cantonese there is apparently no term whatever which can be used to convey unambiguously the same idea as expressed in a phrase like English *on the table* (Tong *et al*. 1997).[7]

A good argument against the status of any particular element as a semantic primitive is, then, an argument that the element in question is either definable, or language-specific, or both. It is more difficult to characterise what would count as a good argument against the entire concept of a 'natural semantic metalanguage', because as with any principled system, the details are open to revision without the viability of the entire system being called into question. It is necessary to argue, as some writers have (e.g. Cattelain 1995), that there are serious logical or conceptual problems

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[7] Other studies which have contested the universality of proposed primitives are Myhill (1996), who argues that there is no exponent of BAD in Biblical Hebrew, and Bohnemeyer (In press), who argues that there are no exponents of BEFORE and AFTER in Yucatec Maya. In my view, these studies can be met by convincing counter-arguments , but this is no the place to undertake this exercise.

with the architecture of the system as a whole. The viability of the overall system would also be called into question if there turned out to be persistent problems with cross-linguistic identification of a substantial number of primitives; or if it seemed that there were a number of equally viable sets of primitives, with no grounds for choosing between them; or if the system proved incapable of making progress on a substantial range of widely recognised semantic problems.

For the moment, however, none of these possible 'system-wide' problems is in evidence. On the contrary, the NSM system appears to be flourishing. From a descriptive point of view, it is being fruitfully applied to semantic topics in a widening range of languages, and from a theoretical point of view there are exciting developments in the NSM approach to syntax, on the one hand, and to cross-cultural pragmatics, on the other. It would be a great shame if the general linguistic community were to be alienated from this work on account of general scepticism about semantic primitives—scepticism which, as I have tried to show this paper, is generally founded on misunderstanding and on faulty argumentation.

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