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Truth in Dreaming

MASSIMO PIATTELLI-PALMARINI

Introduction

The goal of the workshop, upon which the present volume is based, was to promote an earnest exchange of ideas between neuroscientists and researchers in other disciplines. The "Description of Goals" circulated by the organizers urged us "to explore how modern neuroscience has changed conceptions of what it means to be a human being." I will prudently leave this formidable task to the neuroscientists themselves, but there is something I do want to say about ways in which modern *cognitive science* has changed our conceptions of what it means to *explain* human behavior. Certain general considerations may be seen as relevant by neuroscientists and I will attempt to stress such relevance. The particular departure point I have chosen here is the *logical* problem represented by a scientific explanation of meanings in natural languages. Summarily stated, this is the problem of mapping linguistic sounds onto mental representations that have a definite content, this content (a proposition) having in turn the property of being true or false, or, more generally, of satisfying certain relations of adequacy with respect to real or possible "states of affairs." More on all this in a moment. Adopting a terminology that has become common in linguistics and in cognitive science, the expression "logical problem" suggests an analysis of the basic assumptions that any theory of meaning, reference, and truth is bound to share, and a reflection on the fundamental nature of the task, irrespective of the specific hypotheses one chooses to adopt. Let's look at these points in turn.

The Place of Meaning in a World of Causes

It is a powerful temptation to consider meanings, and our knowledge of them, as causal factors in the explanation of much of what we do, think, believe, and say. Very often, an explanation of our actions and our mental

states that is based on what we were *told* is not only legitimate (I mean epistemically legitimate), but constitutes the *only* explanation possessing adequate specificity, plausibility, relevance, and systematicity (Piattelli-Palmarini, 1990). For instance, we surely did not gather in Woods Hole for our symposium because we were driven there by physical forces, or because we were satisfying our primary biological needs. We went there because we were told to meet there. A certain message had reached us, kindly inviting us to be at a certain place, at a certain time, for a certain length of time, in order to responsibly engage in a series of scholarly conversations. The meaning of those messages, and our access to that meaning, given certain shared assumptions, norms, and utilities, is *the* explanation of why we gathered in Woods Hole. Yet, it would certainly be inaccurate to say that those messages were the *cause* of our being there. Nothing, properly speaking, was “the cause” of our being there. We went because we decided to go. Those messages persuaded us to go, they did not “cause” our going. The fundamental problem in the explanation of human action and thought is that a lot of it is not caused *at all*. This is a fundamental reflection that goes back, of course, to Descartes and which has been recently reemphasized over and over by, among others, Noam Chomsky (Chomsky, 1986, 1988b, 1988c).

The deep puzzle is that human behavior is *neither* caused by sensory inputs, *nor* unrelated to them. It is “appropriate to” those inputs and it is selectively “occasioned by” them. An important part of the logical problem of the explanation of behavior, notably including the lawful and systematic dependency of behavior on our mental states (our beliefs, desires, utilities, values, etc.), is one of constructing a scientifically rigorous and truly explanatory link from classes of specific stimuli to classes of specific mental representations and their contents, and via them, to classes of specific “appropriate” behaviors. Everything we know suggests that this link must be *neither* causal *nor* stochastic. This is the chief challenge we have to meet both in cognitive science and in the neurosciences. I must confess that, in spite of awesome recent progress in the understanding of subtle neuronal mechanisms, the volitional component (i.e., the class of properly *uncaused* events in the brain) seems to me to still constitute a total mystery for the neuroscientist. Any light, however dim and indirect, one can throw upon these highly elusive components of brain activity represents, almost by definition, a contribution to our “conceptions of what it means to be a human being.”

Intimations of a Science of Meaning

Let's take a brief look at certain important conclusions (I do not dare call them “discoveries”) reached at the “software” level, that is, in linguistics and

in cognitive science. This is the level of analysis of abstract *structures* (not to be equated with “behavior” as commonly defined) and of their *systematic* effects on meaning.

Some of these conclusions are, I believe, worthy of being singled out:

a. *Our routine segmentation of the strings of linguistic sounds into salient linguistic units does not match, in general, the purely physical segmentation of the signal.* It is not just the case that linguistically salient units (words, phrases, syntactic constituents, clauses, etc.) do not “exactly” or “exhaustively” match the acoustic intervals of speech, the point here is that these two orders of “parsing” often do not match *at all* (Halle, 1990). Let’s take the following simple sentence:

(1) Father sat in a chair

and let’s look at its sound-spectrogram, millisecond by millisecond. (This recording was made at the Max Planck Institute for Psycholinguistics in Nijmegen, but many similar recordings are routinely available in many laboratories for a variety of analogous sentences in a variety of languages [Zwitselood, 1990].) We see that the acoustic intervals do not correspond *at all* to the separation between words. In fact, it is a well-known phenomenon that we are unable to *hear* where a word ends and another word begins in a language we do not understand (Halle, 1985). The purely acoustic parsing would generate

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as words of English. It is obvious to any of us, as it is to any child acquiring the language, that they are not. *Why* this is so “obvious,” though, is far from obvious. Research in the domain of early language acquisition (as early as one week after birth) has shown that the newborn actively (though, of course, unconsciously) *projects* onto the stream of sounds a very restricted innate set of *highly abstract* hypotheses, rapidly converging on the right ones by a process of selection that bears intriguing resemblances to

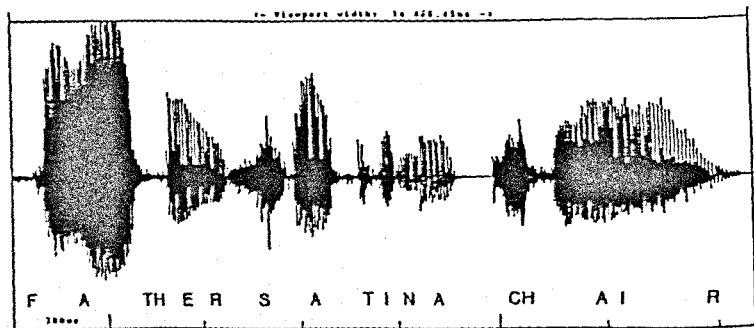


FIGURE 2.1. Father sat in a chair. From Zwitselood (1990).

how some birds acquire the song patterns typical of their species, subspecies, or local population (Mehler, 1990; Mehler et al., 1987).

Much the same *kind* of lesson is to be derived from the process of lexical acquisition and from the intimately related process of the acquisition of syntactic structures. Once again, the purely physical (acoustic) segmentation of the incoming stimuli is *as such* unreliable. The mismatches between “brute” physical segmentation and proper linguistic parsing are so pervasive that we have to conclude in favor of the hypothesis of highly specific, internally (presumably innately) determined sources of active projection onto the stimulus. In the light of overwhelming evidence, we have to reject the traditional instructivist hypotheses of “learning,” that is, a molding of the brain/mind by the environment, a transfer of structure *from* the environment *to* the mind (Piattelli-Palmarini, 1986, 1989).

Nobody is claiming that there is a magic process of direct “mind reading.” We do basically understand “what we hear.” The point to be stressed is that “what we hear,” the physically characterized linguistic stimulus, contains enough *cues* for us to construct a family of specific abstract mental representations. It constitutes sufficient *evidence* for us to select the right hypothesis, but it does not “determine” the hypothesis (in this case, a family of abstract structural descriptions of the incoming acoustical string). I want to stress that the notion of cue is quite different from the notion of cause. A cue constitutes relevant evidence only to the sufficiently structured ear. A cue, at variance with a proper causal determinant, does not itself do the work: it constrains and canalizes the work carried out by an active and “prepared” mind.

b. A lot can be said about the systematic effects of abstract linguistic structures on meanings that are independent of context (independent of storytelling, of appreciation of pragmatic uses, of the beliefs, desires, and utilities of the speaker and the hearer). Evidence for this conclusion is overwhelming and is to be found in a huge technical literature (for reviews, see Chierchia and McConnell-Ginet, 1990; Higginbotham, 1985, 1988, 1989; Jackendoff, 1990). A few selected examples will suffice here.

Lexical Effects

Let's examine the relations between these sentences:

- (2) I persuaded John to go to college
- (3) John (now) intends to go to college
- (4) I (now) intend to go to college

If (2) is true, then (3) must be also true. (4) might also be true, contingent upon (2) *and* some story we can concoct. But the *necessary* link between the truth values of (2) and (3) does not depend on *any* story. It depends only on the respective meanings of the verbs *persuade* and *intend*. It is an analytic dependence and we do not need to know anything at all about John, about

the speaker, or the college in question in order to fully appreciate the connection between (2) and (3). Plainly, this is not the case for the *contingent* link (if any) between (2) and (4). This link can *only* be constructed by mounting up a plausible story. Examples of this kind are abundant and quite straightforward for a variety of verb-meanings (Grimshaw, 1990; Tenny, 1988a, 1988c).

Effects of "Larger" Units

Possibly, the most striking examples are offered by cases of mandatory coreference, mandatory disjoint reference, and optional coreference between elements of a same sentence. "His" is preferentially (though not obligatorily) coreferent with "every man" in the sentence

- (5) Every man kisses his mother

but necessarily noncoreferent in the sentence

- (6) His mother kisses every man

"Himself" is obligatorily coreferential with "John" in the sentence

- (7) John expects to support himself

but obligatorily *not* coreferential with "John" in the "larger" sentence

- (8) I wonder who John expects to support himself

Notice that in these, as in myriads of other cases (Chomsky, 1981; Lasnik and Uriagereka, 1988; van Riemsdijk and Williams, 1986), the systematic effects of linguistic structures on meanings are, so to speak, intrinsic. We only have to look at (or listen to) the sentence *in isolation* in order to have access to these effects. We do not need to know anything at all about the speaker, the characters appearing in the sentence, nor about any story that might make these sentences more relevant to our interests and concerns. These facts about meaning are context independent. Data of great scientific relevance can be collected by examining identical, or analogous, context-independent meaning effects of linguistic constructions in other languages, or local dialects. Which leads me to the next point:

c. Significant insights into the systematic mapping of sounds onto meanings in our language often need comparative data from other languages. Data on "what we mean by what we say" that are simply derived from individual introspection are not sufficient for a scientific semantics. (A corollary, rightly stressed by J. T. Higginbotham, is that we do not have full spontaneous access even to the meanings of many terms in our own private idiolect [Higginbotham, 1988].)

This may appear so counter intuitive that at least a few telling examples are required.

In many cases the genitive *of* and the agentival *by* allow for a corresponding adjectival expression that is perfectly synonymous:

- (9) The speech of the president
- (9a) The presidential speech
- (10) The invasion by the Germans
- (10a) The German invasion

but this is not always the case:

- (11) The death of the president
- (11a) *The presidential death
- (12) The arrival of the Russians
- (12a) *The Russian arrival

These are, plainly, not synonyms any more (Giorgi and Longobardi, 1991). The adjectival construction (if at all acceptable) now conveys the idea of a *way of doing* something, it does not specify the agent as in the two previous examples. We have very clear introspective intuitions of which expressions allow this synonymy and which do not, but we would be utterly unable to tell *why*. Nothing in the introspectively accessible *meaning* of these terms allows us to generate the good and the bad examples. Properties of the *verb* from which some of these nominals derive offer a cue. As a first rough approximation, nominals derived from transitive verbs, in the main, tend to allow for this synonymy (*invade, attack, occupy*), while intransitive verbs, again in the main, tend to disallow it (*die, mature, arrive*). Plainly, this cannot be the whole story. Exceptions are too numerous (*insistence*, from the intransitive *insist*, and *allegation*, from the intransitive *allege*, allow it, while *knowledge*, from the transitive *know*, disallows it). Moreover, there are plenty of nominal expressions that derive from no verb at all (at least from no *contemporary* English verb); some allow it (*affluence, predicament, strategy, disease, superiority*), while others disallow it (*importance, defamation, inception*). Little wisdom do we derive from the transparently accessible meanings of these expressions. (*The predicament of the Germans* is the same as *the German predicament*, but we cannot say **the presidential importance* to mean *the importance of the president*. Why?) A better, but still insufficient, cue is to pay attention to what the agent has to do, in order to satisfy the adjectival expression. When the action, or property, so to speak, strongly “emanates” from the agent, then the construction is valid (*invade, reach affluence, be superior*). When the “agentival role” is less prominent, or the agent is the passive receptacle of the property (*death, maturation, arrival, being considered important by others*, etc.), then the construction is invalid (Giorgi and Longobardi, 1989; Grimshaw, 1990; Higginbotham, 1985). To cut a long story short, it appears that *none* of these *intuitive* criteria, which are accessible to us with a moment’s reflection, can *fully* explain the phenomenon.

The beginning of a satisfactory explanation has recently come from a comparison between the syntax of noun phrases in the Romance and in the German languages. Interesting asymmetries appear between different families of languages. This intriguing phenomenon has been found to be deeply related to other syntactic, heretofore distinct and *prima facie* unrelated, phenomena. Just to give a flavor of the explanatory strategy, one has to also take into account the reasons why **The my book* is ungrammatical in English, while *Il mio libro* is perfectly grammatical in Italian; why in English we do not say **the Callas* to refer to the famous soprano (or to anyone picked out by a family name), while in Italian we currently and quite correctly say *la Callas*, *la Navratilova*, etc. (although their masculine counterparts **il Pavarotti*, **il Gianni* are ungrammatical in standard Italian and are to be heard—but never written—only in certain dialects). A tentative explanation that commandeers all these superficially distinct phenomena under a single powerful principle relies on technical notions such as those of differently set syntactic parameters, of “specifiers” and “maximal projections,” of assigning reference at the level of logical form versus assigning reference at the level of D-structure and surface structure (Belletti and Rizzi, 1981; Burzio, 1986; Giorgi and Longobardi, 1989; Rizzi, 1982). Clearly, nothing of all this is introspectively accessible to us, and there was little hope of arriving at these general, far-reaching, and deep explanatory hypotheses by examining one language only.

The full scientific elucidation of “what we mean by what we say” often consists of unraveling our *tacit* knowledge of *our* language, and this *may* require the full and systematic study of *other* languages as well. We do not get enough semantic evidence just from the introspective analysis of what we mean by what we say, and from how we use certain expressions in our own language. This is a crucial fact about semantics that is often difficult to convey to the philosophers of “ordinary language” in the orthodox analytic tradition (for a thorough analysis, see Chomsky, 1986).

d. It is crucial to analyze what every speaker of a language would say (and not say) in certain imaginary and counterfactual situations. (There is an intimate and systematic connection not only between meaning and the state of affairs that make an expression true—or false—in the real world, but also between meaning and *hypothetical* states of affairs that *would* make the expression true—or false—in “possible worlds” [reviewed in Chierchia and McConnell-Ginet, 1990].)

All attempts to “reduce” meaning to objective statistical co-occurrences of linguistic sounds and physical (or at any rate “public”) arrangements of objects have failed (Fodor, 1987, 1990; Higginbotham, 1983). The demise of behavioristic semantics is probably the most exemplary case, but other attempts, for instance, those based on “informational content,” could be cited (Dretske, 1981, 1988). One major stumbling block for all these physicalistic reductions of meaning is constituted by the capacity of

meaningful expressions in all natural languages to refer to events, including events in the past and in the future, to possible states of affairs, and even to explicitly *impossible* states of affairs (Bennett, 1988; Schein, 1986). Plainly, many such referents cannot *in principle* be characterized physically.

Yet, we have perfectly straightforward understanding of counterfactual conditionals such as: *If you had left home on time, you would not have missed the train.* We also have no problem whatsoever in understanding pronominal and anaphoric reference to *explicitly* nonexistent persons and things: *If you had a sister I would have married her, though she would have been most unhappy to be my wife. Too bad you never wrote that book, it would have been a best-seller* (Fauconnier, 1985).

What makes these expressions *obviously* meaningful is our easy access to mental representations of states of affairs that are *both* relevantly like the real world *and* relevantly unlike it with regard to certain selectively chosen aspects. All serious accounts of how we manage to understand these expressions, and of how we often manage to derive from them important and *true* consequences for our life, must countenance the mental construction of, and reference to, "possible worlds" (also severally called, by different theorists: situations, schemes, scripts, frames, mental spaces, mental models, etc.) (Chierchia and McConnell-Ginet, 1990; Fauconnier, 1985; Jackendoff, 1990; Johnson-Laird, 1983). It appears that we also have to postulate our routine access to a *metric* measuring their greater or lesser "proximity" to the real world (Stalnaker, 1968, 1984). I will come back to this problem when dealing with truth in fiction (Lewis, 1983) and with the semantics of dreams.

How crucial our access to possible and counterfactual states of affairs is for semantics in general can already be seen from our understanding of the meaning of single items in the lexicon. Would it be accurate to say that I "persuaded" someone to do something if I had injected into his veins a psychotropic drug that determines passive subservience to the will of others? Would it be accurate to say that we have "intercepted" today a message sent during World War I and which had then been lost and forgotten? Our robust and quite systematic intuitions about what we would say, or not say, in these imaginary states of affairs is a *crucial* component of semantics. Someone (say, a child or a foreigner) who answers such questions incorrectly thereby shows that he has not understood the meaning of that word.

e. There are specific tools for a scientific semantic analysis, and there are standard problems that every semantic theory must be able to explain. (It is not satisfactory to construct ad hoc semantic theories that explain some phenomena—possibly generated by weird and infrequent expressions—but that do not even begin to account for other current, garden-variety problems of meaning and reference. It is not satisfactory either—for the reasons explained above—to construct a *mono-lingual* semantic theory, a

theory that only holds for, say, English, but that would not be equally applicable to Chinese or Italian) (Chierchia and McConnell-Ginet, 1990; Dowty, 1979; Higginbotham, 1985, 1989; Jackendoff, 1983, 1990.)

Some of the precious tools have been set up through an extensive study of synonymy, intertranslatability across languages, quantification, and the calculus of presuppositions and implications. The typical syntactic and semantic phenomena covered by these in-depth analyses are (the list being, of course, always open to new entries): passivization, relativization, extraction, questioning, nominalization, adverbialization, anaphora, verb tenses, progressives, aspectualization, counterfactuals, antonyms, and opposites. The essential information here is that there is a corpus of highly specific data and well-defined problems, some of which are extracted from a variety of languages the world over, and that every semantic theory these days *has to* be able to tackle. We have already met several examples. I will only offer a few more to show that many terms and expressions in our languages routinely make reference to *events* (Bennett, 1988; Davidson, 1980; Enç, 1990; Grimshaw, 1990; Higginbotham and Schein, 1989; Schein, 1986). This is another physicalistically *nonreducible* semantic element that will become important when examining the semantic content of dreams.

We can start from the following two couples of *superficially* analogous sentences:

- (13) It rains, but it does not snow
- (13a) It rains, but it does not matter

- (14) He bought a ticket, but it was too expensive
- (14a) He bought a ticket, but it was too late

Plainly, they are not *really* analogous. In fact, the pronoun *it* in the second member of each couple shows weird referential properties. It refers not to a "thing," but to a *fact* suitably described. As the sentences make it all clear, the pronoun refers to the fact *that* it rains, to the fact *that* he had bought the ticket. Plenty of expressions betray this referential attachment to events, not to persons or things. This includes also certain "silent" elements of speech (technically called "empty categories"), that have no acoustic expression, and therefore no physical "reality" (Chomsky, 1981; Giorgi and Longobardi, 1989; Lasnik and Uriagereka, 1988; MacDonald and Clark, 1987; van Riemsdijk and Williams, 1986). For instance, the Italian word-for-word translation of these sentences *has to* leave the various occurrences of *it* acoustically unexpressed:

- (13') *Piove, ma non nevica*
- (13a') *Piove, ma non importa*

- (14') *Comprò il biglietto, ma era troppo caro*
- (14a') *Comprò il biglietto, ma era troppo tardi*

These unexpressed (and, in the above Italian examples, strictly inexpressible) syntactic elements share with pronouns a number of crucial properties (and are thus technically called “pro”—lowercase). Both “pro” and an analogous, though syntactically distinct, empty category called “PRO” (uppercase) can refer to events. Striking as it may sound, reference to nonphysical entities (i.e., events) by physically inexpressed linguistic elements is what we *have to* appeal to, in order to fully account for certain routine syntactic and semantic phenomena (Dowty, 1990; Enç, 1990; Hornstein, 1990; Pustejovsky, 1988). For instance, let’s examine the differences in meaning between superficially similar sentences such as:

- (15) The boat was built to brave any storm
 (15a) The boat was sunk to collect the insurance

What happens in these cases can be rendered at an intuitive (i.e., nontechnical) level: in (15) the acoustically empty element PRO, which every speaker mentally—and of course unconsciously—inserts between “built” and “to,” is interpreted as referring to “the boat” (i.e., a thing), while in (15a) it is interpreted as referring to the sinking of the boat (an event, not a thing).

On the basis of many subtle syntactic and semantic data of this kind, it appears unwise to conclude that certain *public* and “concrete” strings of sounds or characters connect with other, equally public, strings of sounds and characters. This interpretation may appear, at first blush, more prudent and more realistic, because it would “only” acknowledge that overt expressions may connect to other overt expressions (for instance, descriptions). The fact is that this surface-to-surface interpretation would fail to do justice even to the simple data we have just encountered. The correct interpretation is vastly more radical: there are in our languages both overt (acoustically and graphically expressible) and “covert” (acoustically and graphically unexpressed—or even inexpressible) devices, suited to make reference to nonphysical individuals, namely to *events*. These are best conceptualized as *abstract* individuals possessing their own specific identity (raining-there-and-then, my buying a ticket at 8:05, his sinking the boat, etc.). They typically consist of real or *possible* actions, changes of state, happenings, and mental processes *under certain salient descriptions*. Cogent arguments based on considerations of economy, compactness, generality, and on *specific* explanatory hypotheses, militate in favor of the second choice: our “ontology” has to accommodate events and reference to events as perfectly routine devices exploited by natural languages.

Let’s briefly consider further kinds of evidence (Tenny, 1988b):

- (16) The paratroopers landed in an hour
 (16a) Paratroopers landed for an hour

In the first case, there is one single event, the landing of the paratroopers, and this event is encompassed within the time interval of an hour. In the

second case, there are multiple events, each constituted by the landing of a single paratrooper, and the series of all these separate events is contained within the time interval of an hour. Note that the *physical* characterization of the state of affairs is exactly the same. A quality camera and a reliable watch will give exactly the same data. Yet, the truth conditions of (16) and those of (16a) are different. The syntactic differences generate different descriptions, which successfully pick out different events. There is no other elegant and compact way of explaining how we understand those sentences. Reference to events and different quantifications over events, as routinely found in expressions such as "in an hour" and "for an hour," offer a simple, powerful, and easily generalizable explanation. For instance, verbs of achievement, those that represent actions with a "culmination" (Pustejovsky, 1988), freely admit the construction "in an hour," "in five minutes," etc. We can say *He built the house in an hour*, *She solved the problem in three minutes*, but it sounds weird to say **She solved the problem for three minutes*. On the contrary, if she solved many problems (there are many such events) we can say *She solved problems for an hour*. Verbs that do not express actions possessing a culmination behave differently: they freely admit the construction "for," but not the construction "in." *He sang for an hour*, *She walked for an hour*, but not **He sang in an hour*, **She slept in an hour*. In order to make this "in" construction acceptable we have to generate the culmination: *He sang the song in five minutes*, *She walked to the bridge in an hour*. Reference to events and quantification over events make all these phenomena (and many more) systematically explicable by elegant and general syntactico-semantic theories, applicable to all languages.

There are also cases that vindicate in a particularly compelling way reference to events and *not* to things or sets of things. These are constituted by sentences such as (May, 1985; Schein, 1986):

- (17) Ten boys ate ten pies
- (18) The eating of ten pies by ten boys
- (19) Ten men unloaded five trucks
- (20) The unloading of the five trucks by the five men
- (21) The bushes grow thicker in the middle of the forest
- (22) I resent the approval of all the budgets by some delegates

The gist of the strategy here is to analyze all the possible sets (of pies, trucks, budgets, etc.), and all the corresponding possible sets of boys, men, delegates, etc., exhausting all the combinations of these sets, and of the elements thereof, that make these sentences true or false. The result is that no construction of sets does justice to our syntactic and semantic knowledge. Only the construction of events (events of eating pies, unloading trucks, growing thicker, approving budgets) do manage to account for our syntactic and semantic judgments. What is more, all the obvious inferences and presuppositions of all these sentences come out *as* obvious, and the

uncertain or hard cases come out *as* uncertain or hard. As they should. This kind of "preservation of inferential ease" does *not* come out from *any* analysis of these cases in terms of reference to sets of objects and/or persons.

These are some of the reasons why the introduction of events, of reference to events, and of quantification over events, has recently contributed to a boom in the explanatory power of theories in syntax and semantics. From this follows the natural conclusion that, therefore, there *are* events in the ontology of linguistic theory, and that we "really" routinely build mental representations of events. This *kind* of conclusion is presently resisted by some philosophers (notably John Searle) but it appears unproblematic to many professional syntacticians and cognitive scientists.

It appears that, if all this is even approximately on the right track, neuroscientists may one day discover the neuronal correlates of these processes. If we really construct events in our mind, and if we really refer to them and quantify over them, then something in our brain allows us to do it. It will be fascinating to discover one day what are the neuronal bases of our mental event-construction and event-manipulation. Something that the neuroscientists would not have looked at, had the "software" scientists not discovered the existence of these phenomena.

A Methodological Caveat

To a hard (or rather "wetware") scientist, all this talk of sentences, intuitions about meaning, reference to nonmaterial entities, and so on, must appear as poor evidence indeed, if not as outright wizardry. It is in part to enlarge the domain of relevant evidence for semantic theories that I recommend also examining dreams and their contents. I said enlarge, not improve or refine. In fact, the kinds of evidence I have just summarily sketched are, and will be for some time, the *central* data for semantics, just as judgments about grammaticality are, and will be, the central data for syntactic theory. A short digression on this topic may prove useful (Chomsky, 1988a, 1988c).

If introspective evidence and judgments about meaning were indeed considered useless as data for science, then nothing I have to say about dreams and their contents would command any attention. Moreover, biologists are (in my experience at least) *particularly* reluctant to accept expressions in ordinary language and intuitions thereon as something even remotely deserving the status of scientific data. They tend to regard pieces of "behavior" as data, but not the abstract structures that underlie these pieces of behavior. Now, it would be quite easy to *superficially* transform the primary data for syntax and semantics into bona fide pieces of behavior. We could just ask the subjects to press a button, or to raise their right hand, if and only if the sentence we present to them satisfies certain criteria (is

grammatically well formed, acceptable as such in a standard conversation, synonymous with some other sentence, unambiguous in meaning, etc.). This is routinely done in psycholinguistics, because the *precise* measurement of *time delays* in the responses is often crucial (for a review, see Garrett, 1990). When, on the contrary, the subject is allowed to reflect as much as he or she wants to before issuing an answer, these scientific paraphernalia become totally unnecessary. We could also place electrodes on the scalp and measure how certain characteristic wave forms correlate with certain syntactic and semantic properties of the input. Once again, this is nowadays done in psycholinguistics (Garnsey et al., 1988; Kutas and Hillyard, 1980) because it is, for instance, important to monitor the timing of different computational *paths* in the parsing of a sentence, when this sentence is flashed word-for-word on a screen, under controlled rates of presentation. Magnetic resonance imaging (MRI) might soon offer still better techniques and it is not totally out of the question that one day these sophisticated recordings might become routine even in the departments of "pure" linguistics. It needs to be stressed, however, that *any* such device will have to be *calibrated*, if it is to be of any use. The initial (and crucial) calibration will *have to* rely on "typical instances" of the linguistic phenomenon we want to monitor, that is, on the standard *intuitions* of a standard speaker-hearer, about certain standard examples. And this, of course, can *only* mean that we have to trust intuitions about sentences and expressions in ordinary language. The initial baseline for any such study will have to be built on the present kind of data, and on the present kind of evidence. The new techniques might well, for all we know, improve the field dramatically and even allow to build, progressively, step by step, different baselines to calibrate ever more refined phenomena. This science-fictional scenario does *not* make the actual data for syntax and semantics irrelevant or obsolete. They will still constitute the proper *evidence* for these explanations and the ultimate *control* for all these findings. No biologist has ever claimed, in virtue of the modern techniques of gene-sequence analysis and polymerase chain-reactions, that it is meaningless to maintain the distinction between insects and mammals, or between neurons and glial cells. We do not expect this "dismissal" to happen in linguistics either.

Since *individual* intuitions about meaning, reference, truth, and well-formedness *are* scientifically relevant data for semantics, I urge that reports on dreams can also be relevant. I will now consecrate the rest of this chapter to show how, and why.

The Content of Dreams as Data for Semantics

In the light of overwhelming introspective evidence, of physiologically monitored sleep-states and well-controlled neuropharmacological effects on sleeping and dreaming, we can safely assert the following (for a truly comprehensive interdisciplinary review, see Hobson, 1988):

1. *Human beings, and many mammals, regularly have dreams*
2. *Many dreams have content* (Michel Jouvet has managed to prove, beyond reasonable doubt, that this is the case even in cats [Jouvet and Michel, 1959].)
3. *Many of these contents are "about" something specific* (persons, places, events, and actions)
4. *The dreaming subject has direct and vivid access to these contents*
5. *Many "characters" and places in the dreams have their full-blown identity.* (Surely identity enough to allow for specific and unambiguous subsequent recall, once the subject is awake.)

If all this is correct (and it *is*, for all we know) then these simple facts *already* have interesting consequences for some semantic theories. I will start by pointing out the semantic consequences that we must derive from the very *fact* that dreams have identifiable contents. In a second phase I will derive other interesting consequences from the *kind of content* that certain dreams can have.

Since dreams have content, then . . .

A widespread misconception of semantics is summarized by the motto "meaning is use." The core idea is that of an *identity* between meaning and the socially admissible ways for us to use linguistic expressions in ordinary life. A variant of this conception, which is notably associated with the later works of the philosopher Ludwig Wittgenstein, equates our understanding of meanings with the ability to successfully engage in communication, and meaning as such with procedures specifying a variety of "language games" (for a critique of this position, see Chomsky, 1986; Fodor, 1987, 1990). Plainly, if dreams have content *while they happen*, and do not *acquire* (pace Norman Malcolm) content *only* post facto, when the subject makes a *public* report of the dream, then this conception is utterly untenable. And, in fact, I think it is, also on many other grounds and for other compelling reasons (Higginbotham, 1983, 1985, 1989).

The contentfulness of dreams strikes me as a further definitive blow to this line of semantic theorizing. I always found very symptomatic that certain philosophers have wished to go out of their way (and out of their depth) in order to *deny* that dreams have content. The most representative piece of such obdurate denial is Norman Malcolm's short book *Dreaming* (Malcolm, 1959), aptly rebuffed by Hilary Putnam (Putnam, 1975a). The philosopher Daniel Dennett has also attempted to demonstrate that dreams may well receive their content in the reawakening phase, in the relatively few minutes during which the sleeper comes back to the real world and reconnects with public life (Dennett, 1969, 1978). There is no physiological plausibility whatsoever to these desperate attempts (Hobson, 1988; Mamelak and Hobson, 1989), but it is indeed symptomatic that philosophers of this meaning-pragmatic tendency are definitely *embarrassed* by the contentfulness of dreams.

I feel entitled to stress that the contentfulness of dreams is per se a further confirmation of the *individualistic*, *mental-representational*, and *intrinsic* nature of meaning. It discourages us once again from *explaining* meaning in terms of communication and *collective* causal links with the world. Dreams are a sort of semantic "experiment of nature." We observe a subject who lies motionless, isolated from any sensory and factual commerce with the world, *but* who can *still* generate *specific* meanings and have access to them. It will now repay us to examine what kinds of meanings these can be.

The Bizarreness of Certain Dreams

Table 2.1 is (an abridged and simplified version of) a classification of the bizarreness of dreams painstakingly developed, and routinely used in clinical practice, by A. N. Mamelak and J. Allan Hobson.

The following excerpts are glimpses from a collection of 233 dream reports, diligently written down in a special journal by a subject known as "The Engine Man." I have copied them from Professor Hobson's large corpus of data, to serve here as a quick reminder of the kind of dreaming experiences that each of us might easily summon from personal recollections. I took the liberty of underlining some passages that are particularly relevant to our present semantic revaluation of dream contents. It will be especially rewarding for us to remark how the *identity* of people and places is *maintained*, in the face of striking and flagrant denials of the most essential defining attributes.

Walking South on 14th St., just south of Pennsylvania Avenue. Street was very muddy. . . . A few blocks (about 3) south of the avenue (Pa. Ave.) I turned east, passing behind various buildings none of which seemed large. No one in sight except my companion, *a child of perhaps 6 to 8 years, who later turned into Jason but who, at first, seemed like a stranger.* [Hobson, 1988, p. 272.]

TABLE 2.1. Two-stage scoring system for dream bizarreness*

Stage I:	Mark as bizarre physically impossible or improbable items in:
	A. The plot, characters, objects, or actions
	B. The thoughts of the dreamer or dream character
	C. The feeling state of the dreamer or dream character
	Localize each item of bizarreness in the dream domain (as it is reported)
Stage II:	Characterize the item as exhibiting:
	A. Discontinuity (change of identity, time, plots, or features)
	B. Incongruity (mismatching features)
	C. Cognitive abnormalities (non sequiturs, ad hoc explanations, explicit vagueness)

*Adapted from A. N. Mamelak and J. A. Hobson, *Journal of Cognitive Neuroscience* 1(3):201-221 (1989).

From Hobson's characteristically insightful comments on this fragment of the report, we learn that, in spite of vast incongruences (muddy street, small buildings, etc.), of which the dreamer is *perfectly aware*, both during the dream and after, he *still* pictured himself in Washington, DC, *while the dream lasted*. Let's make a note that in the real Washington, DC, as the subject *knew full well*, such a walk would have brought him straight in front of the White House, but in the dream it does not. Let's also make a note that Jason, the dreamer's nephew, was in fact *an adult*.

The Engine Man then continues in his report:

It was at the Customs Building, where all animals (except small ones such as cats) *must be registered or declared, weighed, and the proper tax paid*. . . .

It was a 3-story building, of white stone with "ramps" on outside apparently to enable animals to reach the upper stories . . . We entered the building *somehow (not by means of ramps)* went to the upper stories and looked in several rooms . . . [Hobson, 1988, p. 272.]

For a final assault on the canons of identity, let's read the following:

Jason went from door to door, pushing each open. Usually we saw two persons in a room. . . . In each of two other rooms was a girl (young lady) in nurse's uniform. Each was talking to small persons, *evidently children in years but with aged and deeply lined faces*. [Hobson, 1988, p. 274.]

This is surely enough reporting for our present purposes. What is of interest to us, in view of a reassessment of semantic theories, is the almost unlimited *tolerance* vis-à-vis causal anomalies, contradictions, violations of identity, exceptions to necessary and sufficient conditions, lack of unity of place and time, general vagueness of properties and attributes. Yet, people and places and objects *possess* an identity in the dream. They *are*, in the dream, Jason and Washington, DC, in spite of their manifestly incompatible attributes. In dreams we have another semantic "experiment of nature": the best evidence so far of the ubiquity, and the centrality, in our mental life of what Kripke and Putnam have called "rigid designators" (Kripke, 1980; Putnam, 1975b).

The notion of these rigid designators is, in essence, the following: we have to account for our remarkable capacity to reason, and often to reason *cogently*, in counterfactual situations (Fodor, 1987; Kratzer, 1988). We must also account for our remarkable capacity to change (even drastically) our descriptions of, and beliefs about, a *same* object or person. A certain line of thinking, inaugurated by Aristotle and then refined by Bertrand Russell, had decreed that we pick the referents of our discourse through definite descriptions, by detecting properties that are individually necessary and jointly sufficient to single out the proper referents, and these only. Kripke and Putnam object to this theory of reference, pointing out that reference often *remains* fixed also when the description changes drastically, and when we do not have the faintest idea of what these "individually

necessary and jointly sufficient conditions" are. Proper names (Plato, Richard Nixon, etc.) and the terms for "natural kinds" (tiger, lemon, electron, water, etc.) turn out to be typical "rigid designators." We manage to *stably* refer to them in spite of changing descriptions and in spite of changes in their characteristic properties. We understand, in fact, without any problem, expressions such as "Richard Nixon might not have become President," "If Plato lived today, he would like truth-conditional semantics," "If electrons possessed twice the electric charge they possess, matter would be unstable," etc. These different descriptions maintain the same referent. They still are "about" Nixon, Plato, electrons. Yet they project them onto different, counterfactual states of affairs, or different "possible worlds."

In a nutshell, the main objection to a referential mechanism that would be *exhausted* by our competent use of necessary and sufficient criteria lies in the fact that we could not maintain the *same* referents when we drastically *change* these very criteria. Since we *do* often change them, and *still* manage to keep our referents constant, there must be also something else in the referential mechanism.

A rigid designator, unlike a description, *always* picks out the same referent in *every* possible world in which that referent is supposed to *exist*. Proper names, for instance, are the archetypical (though not the only) rigid designators. How they manage to be "rigid," according to Putnam and Kripke, is a complicated story. For us, it suffices to stress that, in their scheme, a crucial component is an *objective* causal link between the term and *actual* true exemplars, bona fide referents, in the real world. This "collective" and "objective" part of the story cannot possibly work for dreams. We will have to rejustify the referential powers of rigid designators *without* the collective-objective component. I think that this needs to be done not only for rigid designators in dreams, but for rigid designators in general, even in plain natural language. This is a most interesting consequence for semantic theory, further corroborated by data on the contents of dreams, on which I cannot delve here.

Another interesting consequence has to do with the *impossibility* to establish a "metric" of proximity between the real world and the worlds of dreams (again, while they last and while they display the meanings and the references they display) (Bennett, 1988; Chierchia and McConnell-Ginet, 1990; Davidson, 1967; Dowty, 1990; Higginbotham and Schein, 1989; Kratzer, 1988; Stalnaker, 1968, 1984). This ought to totally *disrupt* meaning and reference *in the dream*, but it does not. People and places are what they are, do what they do, and say what they say in the dream, *in spite* of the dreamer's utter impossibility of constructing *in the dream* a metric of proximity to the real world. Yet, many dreams *feel* exactly like real worlds themselves.

The worlds of dreams are *at least* possible worlds (I say "at least" because they look mighty *real* to the dreamer while they last) and they appear to be *filled* with rigid designators: persons and places and events that are what

they are in spite of drastic redescriptions, and even in spite of downright *contradictory* descriptions.

It is interesting, though, that we seem totally unable to reason cogently in (and about) the contents of our dreams. Unlike fictional contexts, in which we *are* able to reason cogently, by combining truths in the story with truths in the real life (Lewis, 1983), dreams carry all sorts of unredeemable vagueness and bizarreness. We are unable to combine truths in the dream and truths in real life in *any* way that bears consequences in either context. Hobson seems to have a good neurological explanation for this fact (Mamelak and Hobson, 1989). I am inclined to offer a convergent semantic explanation: the problem may well lie in the impossibility of constructing a "proximity metric." Since this metric, and our access to it in the real world, has been shown to be essential to our *reasoning* on counterfactuals (Kratzer, 1988; Stalnaker, 1968), to our understanding of progressives and tenses (Dowty, 1990; Enç, 1990; Hornstein, 1990), to our understanding fiction (Lewis, 1983), and to other subtle semantic inferences (Bennett, 1988), the case of dreams can be particularly illuminating. Rigid designators do preserve their power of reference, but the absence of a metric of proximity baffles the attempts of our dreaming mind to construct minimally consistent fictions. The bizarreness of dreams might be just the result one would expect, given the absence of a metric for degrees of proximity among "possible worlds."

In fact, the divorce between dream and reality does not seem to reside in a lack of meaningfulness, in a lack of referential power, or in a lack of believability. There *are* meanings in dreams, there *is* full-blown reference, and events *are* quite "believable" while they happen. The true divorce is in *any* availability of the content of dreams for *reasoning*. Pace the ancient dream interpreters, there is no truth "for us" to be found in dreams. Unlike even the craziest works of fiction, the content of dreams is *unusable* for lessons bearing on our real life. Pace the Freudians, there is, in dreams, no truth "about us" qua dreamers. The important truth to be found in dreams is *only* of a generic kind. We can derive truths about our mind/brain. Not individually, but as a species.

I will limit myself to offering these hints in order to suggest why and how data from the analysis of dreams *can* be relevant to semantic theory and to cognitive science.

I now conclude with considerations on the content of dreams as data for the modularity of the mind/brain (Fodor, 1983; Garfield, 1987).

The Peculiar "Realism" of Dreams

Dreams, as we have seen, possess a sort of extrinsic bizarreness: they do not match the probability distributions of real life, do not preserve causal inferences, and often do not preserve even logical inferences. But they also have, in my opinion, a sort of *intrinsic* bizarreness. What I find equally

stunning is that they, after all, resemble reality *so much*. They should not, according to a theory that makes us the "scriptwriters" of our dreams. How can it be that *in* our dreams, while they *happen*, we:

Encounter places and faces, that we *know* we have never seen, endowed
with great vividness and ample details
Suffer from the happening of events we desperately would like *not* to
happen
Are anguished from uncertainties and uncertain outcomes, or are greatly
disappointed by the final turn of events
Are afraid of events and people
Are in admiration of *superior* performances by characters we encounter
(perfect native speakers of languages we barely mumble, perfect dancers,
pianists, consummate lecturers, etc.)

How can this be? Plainly, nothing of the above *ever* happens to a scriptwriter. These things just *cannot* happen to someone who writes a novel. Yet they happen in our dreams. Therefore, I urge, we are *not* the authors of our dreams. Our brain/mind is, not "us."

It only takes a moment of reflection to realize that these are *some of the best data we can possibly collect on the profoundly modular nature of our brain-mind*. Only a modular brain-mind (Fodor, 1983) possessing a myriad of specialized, bullheaded, domain-driven and cognitively inaccessible (impenetrable) subcomponents can display such features. It displays these modular activities *even* when it is, in fact, *disconnected* from *any* external source of stimulation. Intense, ineliminable modularity, plus the fact that the system cannot tell the difference between externally and internally generated "data," plus the fact it is *bound* to try to make there-and-then the best sense it can of the internally generated data (Hobson, 1988; Mamelak et al., 1989) can explain all this. I do not see any other explanation that even begins to make sense.

Conclusion

There has always been, since time immemorial, a feeling that dreams convey *some* peculiar kind of truth. The long prehistory (and the anthropology) of the analysis of dreams shows that they have been often *interpreted* as being somehow true of *the real world*. True about the future, for instance. With Freud, the locus of truth switched from the external world to the internal world of the dreamer. To Freud, dreams were true of the inner, repressed tendencies of *the dreaming subject*. The modern scientific analysis of dreams (Aserinsky, Dement, Kleitman, Jouvet, Hobson, etc.) has shifted this perspective considerably. Dreams are held to be true of *our brain/mind*, not of "us" qua persons. What I have been hinting at here is a

refinement of this insight. I take dreams to be true of our *modular* brain/mind, and I take them to be able to offer precious data for semantic theory. Since the terrain has been cleared, in linguistics and in cognitive science, for the scientific legitimacy of data about our individual intuitions, the analysis of dream reports becomes also a valid source of evidence for (or against) *specific* semantic hypotheses. In this chapter I have tried to offer some very tentative guidelines for such a novel "modular" and "semantic" approach to the problem.

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