THE SEMANTIC ILLUSION

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Abstract. Language, as it presents itself to observation, is a physical, in fact a biological, phenomenon. The facts of the matter seem to be that organisms produce differentiated sound streams and inscriptions, which have physical effects upon other organisms. It is therefore a curious matter that we seek understanding of language in semantic rather than in explanatory theories. After all, if the meaning of a linguistic element is its significance, then there is a place for a theoretical understanding of meanings as physical types. The justification for the dominance of semantics in philosophical treatments of language seems to be that the effects of language are conventional in character. By this is not meant that the causal significance of utterance has been established by convention, but merely that the physical significance of language is not dependent upon the individuating physical characteristics of any particular sound streams or inscriptions, a fact to which the multiplicity of human languages attests. However, that fact does not obviate explanatory theory; it merely displaces its role. Language is an evolved phenomenon, so although it therefore can be expected to have some global structural characteristics, locally its causal role itself has a discoverable causal history; that is what an explanatory theory must explain, and explain in some useful detail. If meanings are physical types, they are species, in one ordinary understanding of the word, that is, a union of populations temporally ordered by an engendering relation. Earlier populations of effects engender later ones. In this paper, this method is brought to bear upon the problem of understanding the connective vocabulary of natural language. It is argued that for much of this vocabulary, no semantic theory can fully explain its role in discourse or give an adequate understanding of its function in argument, since semantics is forced to ignore truth-conditionally inaccessible features. By way of illustration, some semantically puzzling features of particular connectives are revealed and explained. Lessons are drawn for the semantic enterprise more generally.

1. Introduction

On any plausible use of the word phenomenon, language must be regarded as a physical or more specifically a biological phenomenon. We emit sequences of sounds or we commit inscriptions, and those sounds and those inscriptions have biological and other physical effects. I say, in the course of a lecture, ‘Would someone please be good enough to open a window?’ and with sufficient luck and patient repetition, someone will rise to a semi-upright position, move to the transparent side of the room, whichever side that is, fumble with a catch and push out a casement, or throw up a sash, or change the position of a slider, all depending upon physical circumstances. The semanticist will say ‘You mean that someone opens a window. Astonishing.’, but I want to draw attention to the fact that the stream of sound has produced a physical effect, and not just that one, since it also produced physical responses in virtually everyone in the room. If we had a good functional account

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of Wernicke’s and Broca’s areas, we could give a more detailed general account of
the effect of the request. If we had a comprehensive functional understanding of
the human brain, we could perhaps give an account of why it was that that student
alone performed the task. The production of the sound stream was in effect like
tripping a low-energy relay that partially tripped a lot of other relays, and thereby
in one case triggered a relatively high-energy response. A sociopathic lecturer might
describe himself as having opened the window, as it were, by remote control.

The semanticist will reply that independently of meaning there is nothing that
physically fits that stream of sound to the opening of a window. In other places,
perhaps in the same place, depending upon the linguistic mix of students, different,
physically quite unrelated, streams of sounds might have had similar or better
success. What range of sound streams work is, as it were, entirely incidental: it
is the meaning that counts. To put the same point differently, the connection
between the sound streams that work and the work that they do is what is called
conventional. Now what is meant by this characterization is unclear. It seems to
point to something beyond the mere arbitrariness of the connection between the
physical type of what is the product of speech, and the family of physical types of its
effects. Yet there has been no convention in the ordinary understanding of the word,
no coming together of the linguistic family or some elected set of its representatives
to settle the terms of agreement between, say, the particular combinations of sounds
available in English speech and the details of their regulation and coordination
of other physical activities of English speakers. What can be the force of the
claim that the connection between speech and other activities, states, and so on is
conventional?

The physical theorist (at least the one in the pay of this writer) will answer
that the character of language that prompts us to say that its connection with the
rest of nature is conventional is this: that the causal relationships that constitute the
connection of language with the world themselves have a causal history. This is only
what we ought to have noticed all along: we produce the sound and inscriptional
complexes that we produce with the effects that they have as the combined result of
two factors: (a) because our linguistic ancestors (that is, everyone whose linguistic
productions have affected our own) produced the sound and inscriptional complexes
that they produced with the effects to which they gave rise, and (b) because of the
facts of engendering. The effects of present speech are engendered by the effects
of earlier speech. An explanatory physical theory of language must therefore be
one that tells us how the effects of earlier speech and inscription have engendered
the effects of present speech and inscription. A central explanatory item in such
a theory will be the engendering relation; a central topic of research in such a
theoretical framework will be the nature of that relation. An important body of
data for such a theorist will be the ancestry of vocabulary, and the ancestry of
syntax; for in the larger story, the causal role of items of vocabulary is, through
engendering, the product of the causal roles of their ancestral items, and those
roles were and are played within larger vocalized and inscribed entities, namely
sentences.

To sum up, an explanatory theory of language will interest itself in the causal
significance of items of spoken and written language, but the character of its subject
matter is such that its explanatory force will derive from its success in telling us
how that causal significance was engendered by the character of ancestral causal
connections. It can of course adopt the language of meaning, but in such a theory a meaning will be a physical type, or more precisely a type of physical effect, and in the early stages of the theory’s development, the exact character of the effect-types that constitute particular meanings will be unsettled. A suitable language for characterizing them must await a suitable functional language for explaining the workings of the human brain, not just Wernicke’s and Broca’s areas, but all of the regions implicated in the productions and apprehensions of the wider range of physical activities within which linguistic activities are integrated and find their biological fit. The most that can be said of these effect-types at the outset is that the effects in question include neural effects that are available to the motor control structures that regulate speech production. As we shall see in the sequel, these effects, however they may eventually be sub-categorized in whatever functional language, must be regarded as giving us the core effect-types of an explanatory theory of language. Fortunately, we can make substantial progress in the absence of any such detailed functional characterizations. We need only assume that however such neural effects are eventually typed, the following is true: in general an English (or L-) sentence addressed to group of English (or L-) speakers will produce in each of them about the same complex of neural effects as such effects are categorized by the functional neural account. That is, we must assume that whatever functional neural account is given of the relevant areas of the brain, it will enable us to speak non-trivially of a population of effects, of a type licensed by that functional account.

2. The Biological Model of Meaning

Just from what we have said so far, it follows that within the theoretical framework proposed, a meaning is a species, where by species is meant: the union, \( \cup (P) \) of a set \( P \) of populations, \( P \) being temporally ordered by an engendering relation. What we have not yet said is that a species figuring as subject matter of such an explanatory theory bears three important similarities to biological species as they are ordinarily understood. First, with few exceptions, such a species is a non-classical set, a set whose characteristic function is not 2-valued; that is, there must be items whose status as members of the set cannot non-arbitrarily be assigned a 1 or a 0. Second, a related fact, with few exceptions, every member of any such species of neural effects has ancestors that are not members of that species of neural effects. (We may add, in passing and for future reference, that what holds for such species holds also for the corresponding genera. Every species of neural effects that is a meaning has ancestral species which are non-meanings. This follows from the evolutionary fact that all language speakers have non-linguistic ancestors. So within this framework, the question as to language origins becomes the question: what was the engendering relation that, in sufficiently many generations, produced linguistic effects from non-linguistic ancestral effects?) The third similarity is that members of the same species have similar morphological profiles. In organic species this amounts to their having corresponding morphologies at corresponding ages of development, and the comparisons are to be made modulo sexual differences. For effects that are elements of meanings, they are the effects of vocal sound streams or inscriptions within which there are many variations, all of which are comprehended by the idea of a morphological profile. Modulo those distinctions, elements of the same meaning are the effects of items of speech having the same morphological profile.
Let me stress that in what follows the use of the language of species is not metaphorical. The term species is to be understood only according to the definition given. Should there be a conflict, it is the definition that must be allowed to get on with the work unmixed with competing intuitive understandings of the term. There is some point to the warning. Having come this far, it is tempting to look to biology for concrete and specific as well as formal correspondences; however, when we do, although we do in fact find individually striking parallels, they are biologically heterogeneous. Here I list a few such observations, claiming neither completeness nor biological consistency. Of course once the fancy has our lapels in its grip, our attention is forcibly drawn to numerous parallels even at the most fundamental level: first, (a) the prolific serial composition of words from a limited vocabulary of phonemes, and (b) the more prolific serial composition of sentences from that vocabulary of words are suggestive of (a’) the serial composition of codons from triples of amino acids, and (b’) the composition of chains of codons in a reading frame. Here it is the molecular biologists whose lapels were first gripped. Second, it is tempting to think of parts of speech as requiring, or at least supporting, a distinction akin to the distinction between genes as DNA molecules on the one hand, and their expression in protein synthesis on the other; for we must distinguish the phonemic compositions that constitute the basic ingredients of speech from their neural effects in the cerebral cortex of their recipient. Third, it is striking that, as the expression of genes is regulated at numerous levels (their transcription, the manner of their processing, their transport to site, and so on) so in speech the recipient’s neural expression of a speaker’s phonemic string is regulated at numerous levels. I mention only two here: First, the effect of the utterance of a sentence is altered by the circumstances of its utterance and by surrounding matter of speech. The point is obvious when the roles of pronouns and anaphora in general are considered, but equally, very general contextual and background matter plays a role in regulating the effects of speech. Consider the sentence

2.1 I’ll have breakfast only if there is insufficient food.

Secondly, the effects of the phonemic matter of speech are regulated by variations within the three prosodic dimensions of its presentation: stress, pitch contour, and lengthenings of component sounds. To illustrate, contrast the pair

2.2 What is this thing called love?

and

2.3 What is this thing called, Love? BBC talk (with the previous)

There is, too, a rare element of indeterminacy in which different effects in distinct recipients are not the results of discerned differences. Consider the two readings of

2.4 No trees have fallen over here.

William Calvin [3] has hypothesized that the production of speech involves a Darwinian process in which alternative sequencings compete for a limited workspace. There is no reason to suppose that, in general, speech reception is any less a Darwinian process than speech production. If it is, then we may look upon the cues that serve to promote one set of effects and to forestall others as something like environmental pressures that select for and so preserve one lineage while selecting against and so extinguishing others.
One other point of contact between speech and organic biology will assume importance later in this story. This is that both speech and organisms are similarly modular. As nature does not produce larger creatures by constructing them of larger cells rather than more, so novels are not distinguished from short stories in virtue of their having longer rather than more sentences. In neither case is this an accidental feature. The synthesis of proteins by ribosomes and polyribosomes depends upon bonds and weak forces that are effective only at the small molecular distances within the endoplasmic reticulum and other cell substructures. The apprehension of syntactic structure requires that sentences not exceed certain variable limits of length, that is, that its parts not be distant from one another beyond the capacity of the prosodic and contextual cueing of structure at various levels. In both cases, however, there is a non-zero error rate. In both cases, as we shall see, such errors have more than momentary consequences.

Now as I have already hinted, we are rightly suspicious of such pairings of features. From such observations in themselves, nothing of an explanatory nature can be inferred, since we have not claimed, let alone shown, that there is any physical connection between the features of the organic structures and what we have selected as corresponding characteristics of speech. And even if there is, it may be only that familiar recurrence of structural patterns that one finds at different levels of complexity in organic nature: even if there is some deep understanding to be had of it, the understanding may still yield little of immediate practical explanatory value. If there is theoretical utility in the observations, it must derive from the linguistic features themselves, independently of any supposed parallels with organic biochemical phenomena. Above all, there must exist linguistic data that they give us the correct means of understanding.

3. Some Observable Facts: Polysemy and Logicalization

3.1. Some English connectives. My own main application of the biological approach I have been describing has been in the study of English connectives, and it is the success of the method there that emboldens me to suppose that under its auspices more general conclusions about language can be drawn. To give a thorough account of that study will certainly take us far from the main purpose of this essay, but for various reasons that will become apparent, it will be worthwhile to pause over that topic long enough to make some specific remarks about the apparent place of semantics in linguistic understanding, and to see the need for an explanatory theory of the sort envisaged here.

English-speaking philosophers and logicians, at least the authors of just about all of the introductory logic texts in that mons copiosus that I have been able to examine, have, as it seems, settled and confident opinions on the subject of the so-called ‘logical’ vocabulary of English. No doubt in most cases they have inherited the opinions of their own teachers, but it is as well perhaps a common attitude among them that since they can accurately use connective vocabulary in speech, their professional opinions have the warrant of whatever semantic understanding enables them to do so. So it is not surprising that so many philosophers, and such a high proportion of textbook authors content themselves with illustrating the correctness of their assumptions rather than actually studying the English language. My intention is by no means to disoblige the authors of introductions to logic. But though their works taken collectively must constitute a prodigious contribution to
the reform of philosophy, this is so for reasons that can give the authors little satisfaction, though, I hope, no self-reproach. After all, natural language semanticists since the early Wittgenstein have looked to the successful logical treatment of the connectives as a source of inspiration. In the quest for a semantic theory of natural language, the connective part is supposed, on that reckoning, to have been the easy beginning. And if anyone can be supposed to have such a theory, formalists can. So if even logic text authors almost universally get even these matters wrong, there is some deep-seated error in some very common philosophical assumptions. These two subjects: the errors on the one hand, their significance on the other, provide the subject matter for the remainder of this essay.

Now it is no argument against the textbook views that they conflict with the views of Paul Grice, but the conflict is perhaps worth pointing out, (though not at length, since it is discussed so fully elsewhere.[6]) I content myself with one instance, that of the English word or, that being Grice’s central example, and a recurrent textbook theme. According to too many of the textbooks for it to be a coincidence, English has two or’s. The point is variously put: in English or has two meanings, two senses, two uses, and so on. One corresponds to the ∨ function; the other to the ⊻ function, that is, to xor. In their 1971 paper [2] Barrett and Stenner set out the requirements for establishing the existence of a ∨-like use of or. Since the table of ⊻ differs from that of ∨ only in the first row, the demonstration of the existence of such an or requires that there should be an or-sentence which is false, but both of the clauses of which are true. No one has produced such an example. Reichenbach1, twenty-four years earlier, had pointed out, that iterated xor behaves like a quantifier: it outputs 1 if any odd number of 1’s is input, and outputs 0 else. So a sentence composed with xor of five simple sentences will be true iff exactly 1 or exactly 3 or all of the sentences are true. (Notice that iterated material biconditional (↔) also behaves like a quantifier, outputting 1 if any even number of 0’s is input, outputting 0 else. So, for example, the five-argument case with ⊻ is equivalent to the corresponding five-argument case with ↔.) Linguists agree that there is no such naturally occurring connective vocabulary in any known natural language, and a fortiori no such sense or meaning or use of or in English. (One might add that for corresponding reasons, there is no use of if and only if in English corresponding to ↔.2 That we don’t in general know such facts as these, even after we have studied or taught propositional logic, would tell us something about the general nature of human linguistic competence, if we would but attend.)

Again, briefly, since it is so fully considered elsewhere, consider the or of such constructions as

3.1 You may have pie or you may have cake

variations on which appear in many introductory texts as examples of xor uses of English or3 Since from such a remark the addressee correctly infers that he may have pie, this must be a sort of conjunction, and so neither an inclusive nor an

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1 Apparently the first textbook author to do so.
2 There is a more fundamental reason: in English, neither or nor if and only if is binary. So neither the sentence Abe ambles or Mary marches or Ted tittups nor the sentence Abe ambles if and only if Mary marches if and only if Ted tittups is syntactically ambiguous. In a formal language, except by an abbreviative convention that also subsumes the properties of associativity, neither p ∨ q ∨ r nor p ⊻ q ⊻ r nor p ↔ q ↔ r is well-formed.
3 For their ubiquity and uniformity of content I have labelled it the argument from confection.
exclusive disjunction. Why is this not noticed by textbook authors? The reason is partly that the language of disjunction and conjunction is not a part of the language of folk-semantics; nor is that distinction the salient distinction between and or for English speakers. But why then, when we consciously set out to codify the semantics of natural language or, do we get the matter so wildly wrong even on so simple a case?

I know of no empirical study that purports to answer that question. Most philosophical logicians have not noticed the fact. It is fair to say that the philosophical logicians who write the texts make quite contrary assumptions, assumptions for which no case has ever yet been made out. One is that truth-conditions do play a central role in folk-semantics, as the abstract counterpart of valuations and compositions of functions does in formal semantics. Another is that a folk-semantical theory plays a role in the compositions of speech. That the same philosophical logicians, who are also speakers of natural language, get the matter so wrong when they try to articulate fragments of the folk-semantics is evidence that if such a semantical theory exists, it is not accessible to trained experts. On the other hand, if we consult the illusive common man, say by reading what he says in press interviews, we tend to find such references as to “either-or situations” which unreasonably exclude our having our cake and eating it. The evidence is anecdotal, but in my experience exclusivity seems to form a natural component of spontaneous explanations of or; truth-conditions do not. Moreover exclusivity is taken to apply, even by the textbook-authors, even to cases in which or is truth-conditionally conjunctive.

A separate point urges an early mention here: truth is itself a folk-theoretic notion of which we have little useful semantic understanding. In formal settings, it gives a convenient reading of formalist artefacts, but not a mathematically informative interpretation of them. We know as much as we need to know about 1 and 0 if (a) we know that they are distinct, and (b) we know which of them is designated. Outside of such settings truth remains a subject of philosophical puzzle. In introductory philosophy courses it is invariably a topic, but the discussion dwindles after mention of one or two inadequate and incomprehensible theories. That the notion is then allowed to play any role at all in later courses, that regard for it is extolled as a philosophical virtue, that its applicability or inapplicability, for example, to moral ascriptions, is contested as though the contesters actually knew what they were contesting: none of these facts speaks eloquently to the credit of philosophical instruction or method. In fact rather the reverse. I return to the subject under a more general heading later.

3.2. The Gricean urn: Did the hand of the potter shake? The acceptance of an evolutionary rather than a creationist view (however scientific), in language as elsewhere in biology, would condition us to certain expectations. In the first place we would expect a great and messy prolificity of meaning arising from comparatively simple origins, by a few simple principles acting in diverse environments that the earlier meanings have themselves partly produced. We would expect the theoretical constituents of its most fundamental explanations to be the same constituents as those of other sciences. Biology is fundamentally chemistry (or, if you like, fundamentally physics), though the nature of the chemicals is such that they sponsor a large number of specifically biological types. Polynucleotide-types are cheap, and no one invokes Occam’s razor, neat or modified, to compel biologists to limit the number of them. Biologists, by contrast, would themselves readily
invoke the name of Occam in defence of limiting fundamental theoretical types.
Occam, though he was himself no doubt a devout creationist, would be an eligible
patron for evolutionary biologists. Theoretical foundations are expensive; life-forms
are cheap, provided their existence is explained by the theory. Much the same Oc-
camist sentiment would shape a biological approach to language. One wants to keep
the number of new fundamental theoretical constructs at a minimum but accept
whatever diversity of meaning they dictate. Theoretical foundations are expensive,
meanings cheap if their existence is explained by the theory.

I do not know how to understand the word sense as various authors apply it
to words. So I do not know whether or not I am in conflict with Paul Grice over
what he calls ‘Modified Occam’s Razor’: Senses are not to be multiplied beyond
necessity.\[5\] The two points of difficulty with the principle are, of course, at the
occurrence of the word sense and at the occurrence of the word necessity. He is
frustratingly vague about what senses are, though there are clues suggesting that
on his understanding, if words have two distinct senses, then there is at least one
sentence that (a) has an occurrence of the word and (b) has two distinct sets of truth-
conditions, accordingly as the word has one or the other of the two senses. And
again, as he confesses, there is some difficulty in sorting out what would constitute
an overriding necessity.

The exegetical task is further knotted by Grice’s inexplicable preoccupation with
his putative strong sense of or, the sense in which it implies non-truth-functional
grounds for its use. Here at least the difficulty is easily cut through, for even if he
is successful in demonstrating that or has no such sense, this would not prove that
it has only one. As the Yiddish saying has it, a for-instance is not an argument. In
fact it is very easily demonstrated that all natural language connectives have more
than one truth-conditionally distinguishable use. Even but, which for Grice is a
model case of a word that carries a conventional implicature, has both conjunctive
and disjunctive uses. Contrast

3.2 It never rains, but we all own umbrellas anyway
with

3.3 It never rains but it pours.
In fact it has other non-connectival uses as well. Consider

3.4 My, but it’s a beauty
and

3.5 No one but his mother calls him Hulon.
Why, one wants to ask, should words have only one connectival sense but be allowed
many non-connectival ones. Or is another case in point. Simply contrast

3.6 He is thirty-nine or he is forty
with

3.7 He may be thirty-nine or he may be forty.
or

3.8 You may have tea or you may have coffee.
There is no reason to suppose that the correctness of the inference from this to
I may have tea is founded in anything other than a distinct, truth-conditionally
expressible use of or: the one in which it means alternatively. And then consider

3.9 He must have left early or I would have seen him
in which the or could be replaced by otherwise, but not alternatively. And contrast

3.10 Lou is taller than Mary or Nancy

from which we properly infer that Lou is taller than Mary, with

3.11 Lou is the sister of Mary or Nancy

from which we do not properly infer that Lou is Mary’s sister.

Two further points about or must be given their place in an assessment of Grice’s approach. The first is that, statistically, the disjunctive uses of or constitute a very small minority of its uses. The second is that, historically, it seems likely that a species of conjunctive uses is the ancestor of the species of disjunctive ones.

It is a commonplace of philosophical logic that there are, or appear to be, divergences in meaning between, on the one hand, at least some of what I shall call the formal devices—¬, ∧, ∨, ⊃, (∀x), (∃x), (x) (when these are given the standard two-valued interpretation)—and, on the other, what are taken to be their analogues or counterparts in natural language—such expressions as not, and, or, if, all, some (or at least one), the. Some logicians may at some time wanted to claim that there were in fact no such divergences; but such claims, if made at all, have been somewhat rashly made, and those suspected of making them have been subjected to some pretty rough handling.[5] p.22.

No such debate could have taken place before the advent of the truth-table. Equally, Grice’s own contribution to it,

... that the common assumptions of the contestants that the divergences do in fact exist is (broadly speaking) a common mistake, and that the mistake arises from inadequate attention to the nature and importance of the conditions governing conversation.[5] p.24,

as beguiling as it is, as influential as it has been, and fraught though it undoubtedly is with useful ideas, is, represents an oversimplification that could not have occurred to anyone before truth-tables were introduced. It is certainly a singular feature of Grice’s theory that the involvement of truth-tables should so precisely demarcate the authority of its dictates. The theory applies only to the vocabulary that gives us English readings of connectives. It does not apply to any of the other vocabulary of logic, the illative adverbs, for example. It is not insisted that the since of

3.12 I have been longing to meet you since I first read your book

must be dependent for its meaning upon the logical role of the since of

3.13 Since I’ve read your book, you must read mine

or that the so of

3.14 I knew his name and address, so I consulted the phone book

must apply for its meaning to the so of

3.15 I knew his name and address, so I knew his name.

Neither are any claims issued on behalf of the English word necessarily and the □ of $E^4$. When a colleague says Necessarily $\alpha$, and then Necessarily $\beta$, do I

\[\text{4} \text{The smallest classical modal logic, adding to the PL closure conditions, the rule [RE] } \vdash \alpha \leftrightarrow \beta \Rightarrow \vdash \Box \alpha \leftrightarrow \Box \beta.\]
infer \( \text{Necessarily } \alpha \) and \( \beta \) because of \( E \) or \( M \) plus a convention, or because of a principle of \( K \)\(^6\). The answer is, of course, not forthcoming. Only the English words that happen to be given as convenient readings of certain of the truth-functional connectives are subject to these stricures. That \textit{unless} might have been given as a reading of \( \lor \) seems not to enrol it in this club. That had Polish notation gained the ascendancy the word \textit{either} might have been given as the most convenient reading of \( A \) or \textit{given} for \( C \) does not impose this requirement upon \textit{either} or \textit{given}, though presumably it might have. Had \textit{as well as} been adopted for \( \land \), had reverse Polish notation become the vogue, . . . ; there seems hardly any point in jogging further on this spot.

There is but one other point to make: in his development of the notion of \textit{implicature}, Grice has undoubtedly altered the professional lives, certainly the professional vocabulary, of many linguists. But where it matters, they place no great weight upon its intentional components, taking the implicatures of acts of speech to be effects, perhaps (conversationally speaking) even intended ones, but not with any special theoretical reliance upon the notion of \textit{intention}. The difference between a Gricean account and a biological one is the former’s theoretical reliance upon the notions of \textit{truth-condition} and \textit{intention}, not its theoretical reliance upon implicature understood as linguists understand it. The implicatures are precisely what we learn when we learn language. In this Grice could not have got the matter righter. Moreover, the idea that acts of speech have effects that may incidentally invite the application of intentional and cognitive language is common to both approaches. \textit{The point of the explanatory theory is to explain conversation, not to put a stop to it.}

A word about that vocabulary. For a biological linguist, the uses of the words \textit{intend} and \textit{true} and their cognates are just more observational data. Uses of that vocabulary have effects; that they have those effects requires explanation; the theory presumes that they have the effects that they have because ancestral items of vocabulary had the effects that they had, and because of the particular character of the engendering of the later effects by the earlier. They have no special theoretical status. There is no guarantee or even presumption that they can be given a truth-conditional semantics. In fact all the comprehension (or indeed all the comprehensibility) that is guaranteed is what is required for the transmission of that part of the language from one generation to the next. Anyone who insists that there must be some method for finding out what intentions are or what truth is must overcome this challenge: to demonstrate that there being such a method is required for the transmission of the use of the vocabulary of \textit{intention} and \textit{truth} from one generation to another; for all we can be certain of is that we use the vocabulary, and that our linguistic ancestors used vocabulary somewhat like it in somewhat the same way.

3.3. \textbf{Where ‘logical’ connectives come from.} All the functional vocabulary of any natural language has descended from ancestral lexical vocabulary. Sometimes functionalized items retain their old lexical morphology; sometimes not: contrast,

\(^5\)The smallest monotonic modal logic, adding to the PL closure conditions, the rule \([\text{RM}]\) \(\vdash \alpha \rightarrow \beta \Rightarrow \vdash \Box \alpha \rightarrow \Box \beta \).

\(^6\)The smallest normal modal logic, adjoining to the axiomatization of \( M \), the axiom \([\text{K}]\) \(\vdash \Box p \land \Box q \rightarrow \Box (p \land q)\).
for example, the auxiliary uses of *have* and *go* in the formation of perfect and progressive future tenses in English with the vestigial *have* and *go* endings of Latin past and simple future verbs. All natural language logical connectives are functionalized (logicalized) versions of lexical vocabulary, usually vocabulary of physical relationship. So, for example, *if* is a descendent from *gyfan* (*give*), or from *other* (*second* as in *every other day*), and *but* from *butan* (*outside*). They are as often as not morphologically reduced by the exigencies of frequent use, which explains why so many of the logical connectives of so many languages are such short words. The explanation of their uses in modern English is the explanation of their descent from that lexical vocabulary. The supposition that, for example, *or* or *if* has, truth-conditionally speaking, exactly one meaning must entail that only one descendent of *other* has survived, that it only ever had one line of descendants or that all the others must have been extinguished before the present morphology was achieved. Any weaker hypothesis must accept more than one meaning in the past, and any such concession must render arbitrary any *a priori* insistence such as Grice’s on one meaning now (the truth-tabular one) for each connective word.

3.4. But. Consider the following list of sentences

3.16 I don’t fall asleep *but* I dream of Jeannie
3.17 I fall asleep, *but* I wake up almost immediately
3.18 My *but* Jeannie is beautiful

I make no claim that that list exhausts the distinguishable uses of *but*. It does not include such uses as that of

3.19 It is *but* Jeannie

or

3.20 It is not Jeannie, *but* Sarah.

However, it will do for the purposes of illustration.

Observe first that the *but* of examples 3.3 and 3.16 is (on a superficial examination, anyway) disjunctive (Either it doesn’t rain or it pours; Either I don’t fall asleep or I dream of Jeannie.) When it is recalled that the connective *but* descends from *butan* (*outside* as in current northern *but the house*), it will be almost immediately apparent that, from a purely historical point of view, as between this *but* and the allegedly implicature-fired conjunctive *but* of 3.2, this disjunctive use of *but* is primary. The descent of the disjunctive *but* is not difficult to trace. The main stages of the development see the extension of spatial *but* first to abstract categories of items (no reward but glory) then to circumstantial classes:

3.21 No course is forgivable but that he should relent.

The ellipsis of *that* gives us connectival *but*:

3.22 I will not be exalted but you shall have share in my glory.

Now, notice, if historical order counts for anything, it is the conjunctive uses that ought to seem anomalous. If Grice had placed his faith in history rather than the truth-table, the Gricean line we would be debating would be whether we are justified in regarding even the truth-conditionally conjunctive aspect of conjunctive *but* as no genuinely distinct meaning, but rather as itself a mere implicature, and its implicaturial aspect as an implicature inside an implicature.
3.5. **Mutations.** There seems to persist among some philosophers a vision of a large-scale semantical theory of language. It rests upon an assumption that the semantic theory of the logical portion of language is now nearly complete and will form the foundation upon which some larger semantical edifice will be built. But, having taken no account of linguistic change, they have ordered their building materials the wrong way round. Connectives are not at all suitable for a lasting foundation. Historically, the connective vocabulary has shown itself to be extremely fragile, while under similar conversational pressures, lexical vocabulary has proved itself comparatively resilient. And although compositionality is widely claimed as a key condition both of our capacity for novel speech production and of our capacity for speech comprehension, this represents only a synchronic point of view. Language as a phenomenon must also be viewed diachronically. And when we look at language in its temporal dimension, we see matters differently. First, for all our compositional potential, we actually compose very few sentences each day either in production or apprehension of speech, and this actual composition of speech is the main, brakeless vehicle (including the engine) of language change. But the compositional forces that can bring about compositionally significant changes of meaning in functionalized vocabulary have less dramatic effects upon lexical items. The point, curiously enough, finds an illustration in Jane Austen [1] 209–10.

> ‘Have you had any letter from Bath?’ [Henry Tilney to Catherine]
> ‘No, and I am very much surprized. Isabella promised so faithfully to write directly.’
> ‘Promised so faithfully!–A faithful promise!–That puzzles me.–I have heard of a faithful performance. But a faithful promise–the fidelity of promising! It is a power little worth knowing however, since it can pain and deceive you.’

What has Austen noticed? The construction, not an especially intelligent one, arises from a misconstrual of scope: in the word sequence promise faithfully to φ the adverb faithfully, in the ancestral uses, modifies the infinitive. The promise, on the ancestral construal, is the promise faithfully to write. The other construal (reserved by Austen for the likes of the naive Catherine and the feckless Lydia), which takes faithfully to modify the main verb, permits these intransitive constructions, and must therefore give to the element faithfully either a new meaning, or no meaning at all. Of course the language finds an idiomatic use for the construction as a whole, a use which is suggestive of earnest, hand-on-heart asseverations and undertakings, but it is not one that relies upon composition of autonomous meanings. The word faithfully has never migrated with any such meaning to other environments.

Negation-raising verbs such as believe, think and so on present a similar phenomenon. The common construal of I don’t believe that α as I believe that not-α has not spawned a new meaning of the verb believe; in biological terms, idiomatic uses do not generally propagate except artificially. A non-English speaker who says I don’t hope you slip on the ice by analogy with the negation-raising idiom, is more likely to be quoted than to be imitated. I mention in passing that the verb doubt may be an exception to this general claim. There is good reason to suppose that the present use of the verb is a mutation of an earlier weaker use, one that is exemplified frequently, for example, in Pepys’ Diary

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7I owe the example to Charles Travis.
3.23 There I found as I doubted Mr. Pembleton with my wife and probably represents the correct understanding of every occurrence in KJV. It may well be this earlier weaker use that persists in such constructions as

3.24 I do not doubt but that the Viet Cong will be defeated. Richard Nixon

Functional vocabulary is less impervious to the effects of such scope misconstruals. Of the many instances I now know about I will mention only two here. The first involves the word unless, which is a reduced form of a longer construction on [a condition] less (than that). In its earliest inter-clausal uses, such a construction would be conjunctive in character. \( \alpha \text{ on a condition less than that } \beta \) would be representable roughly as \( \alpha \land \neg \beta \). But now suppose (as seems to have been the case) that the construction is never used outside the scope of some prefixed negating item. Schematically we can represent this as \( \text{Not } \alpha \text{ unless } \beta \) (the underlining representing the original and . . . not reading. On that reading of unless the sentence as a whole will have its present-day reading if the scope of the Not is taken to be as in Not (\( \alpha \text{ unless } \beta \)). But suppose that an emerging portion of the linguistic population agrees with its complementary portion in the construal of such a sentence as a whole, that is, agrees on the occasions of use of all such sentences, but takes the scope arrangements as (Not \( \alpha \)) unless \( \beta \), then that emerging portion must give to the unless element of such sentences a new construal as or for purposes of new compositions, the new (succubinal) meaning that the overline is intended to express. Of course, since the two portions of the population never see (the ancestor of) unless unaccompanied by the preceding negation, the difference in their syntactic construals will never become apparent, and therefore the new construal never corrected. But under such conditions a natural bias in favour of short-scope construals of negatives or simpler syntax more generally will eventually tip the balance of construals in favour of the innovation, and, if unless migrates to other, un-negated environments demanding an or construal, a sufficient portion of the population of language-users will have already accepted the or construal of unless to ensure that it survives. Since the or reading will do for all instances, both the original (on the the new syntactic construal) and the new, un-negated one, the and . . . not construal is eventually extinguished.

I have expressed this in the language of construals, but the phenomenon clearly has a neural substrate involving some form of imperfect replication of structure. One element of the apprehension of speech (or written text) involves the neural rehearsal of the motor sequencing involved in its production. I assume that the rehearsal of a spoken sequence under the auspices of one syntactic scheme is different from the rehearsal of the same spoken sequence under the auspices of another, that, for instance, the rehearsal of the Shaw example is neurally distinct accordingly as over goes with fallen or here if one or the other is salient.

In the case of functionalized vocabulary, the principle governing such changes seems to be

\[(\text{identical occasions of use}) + (\text{novel syntax}) \rightarrow (\text{novel meaning})\]

For lexical vocabulary such novel construals produce idiomatic constructions, but seldom new independent meanings, if as is usual, we take it as a requirement of an independent meaning that it be stable through some range of distinct environments. We may of course speak of a meaning restricted to a single environment type if we wish, but within the theoretical framework I am presenting, this would just be a redescription of what we ordinarily call an idiomatic use. Even for much
functionalized vocabulary its use is triggered only within a narrow range of environment types.\(^8\) (Think of universal \textit{any} and its partiality for negative and modal environments.)

3.6. The descent of \textit{but}. It seems that (a) a development paralleling the one just mentioned explains the slow emergence of disjunctive \textit{but}, and that (b) then sometime over the sixteenth and seventeenth centuries, various conjunctive \textit{but}'s emerged through developments of the same general character but from an ancestral base broadened by the earlier development.

The first stage of this fragmentation more or less exactly parallels the emergence of disjunctive \textit{unless}. Syntax schematically of the structure \textit{Not (α but β)} is misconstrued as having the structure \textit{(Not α) but β}, forcing an \textit{or} or \textit{if not} reading of \textit{but}, which is initially concealed by coincidence of occasions of use, but which emerges when the new \textit{but} migrates to environments in which no governing negation masks the new construal. As an example, consider

3.25 Damne but she’s a beauty
Again, the \textit{if not} reading unifies such cases with those in which the preceding negation is present.\(^9\)

The emergence of conjunctive \textit{but} seems to have overlapped the emergence of the disjunctive one, and to have involved once again a novel apprehension of scope, now sometimes involving negations, but also sometimes involving other non-negated environments licensed by the emerging \textit{if not} reading, and abetted by the general trend toward ellipsis of \textit{that}. As an example, consider

3.26 I would have gone but [that] I was afraid
(that is, . . . \textit{if [that] I had not been afraid}).

Certainly we find transitional uses in Pepys’ diaries and elsewhere. By the early eighteenth century, conjunctive \textit{but} was established, with the result that one can, at this remove, be less confident, in many cases, as to which reading should be given. Consider the two \textit{but}'s of Dafoe’s 1722: [4] 118

3.27 . . . the man at the window said it had lain almost an hour, \textit{but} that they had not meddled with it, because they did not know \textit{but} the person who dropped it might come back to look for it.

Compare, as an answer to \textit{Where’s Bryson?}

3.28 I don’t know(,) but he may be in Melbourne.

With \textit{but} as with other items of functionalized vocabulary that have been subject to parallel or similar developments, the production of new meanings by mutation is dependent upon there persisting sufficient similarity of effect of whole constructions that in early stages the two meanings can coexist undiscovered. The \textit{satisfaction-conditions} need not be identical, nor even capable of articulation for the two readings; they need only be at most negligibly different. Mainly it is required that the \textit{occasions of use} of the construction under one construal should be approximately the same as the occasions of use of the construction under the other.

\(^8\)The American use of \textit{internecine} as meaning \textit{within a family or group} is a useful foil. Notice, however, that it is (so far) restricted to conflicts, evidence of its origins in misconstrual, though in this case a misconstrual of extension not of scope.

\(^9\)As a matter of interest, this explains a puzzle which has long (insufficiently) vexed linguists. The use of \textit{but} here persists, when the curse is generalized to \textit{My god} and the offending sacred reference expurgated, leaving only \textit{My}. 
If the explanation is correct, then doubts may be registered as to the importance of semantics, truth-conditionally understood, to linguistic practice. If we look for parallels only to the uses of formal languages, we might take the significance of these disclosures to be that, as natural deductive rules are simply rules for extending proofs and not rules of inference, so whatever rules govern our use of language, they are really rules for extending conversation, not for the expression of the content of thought. In fact, though there is not space to develop the point fully here, the constraints on language use seem to be hybrid in character. To be sure, speech produces sensory and distinctively linguistic neural effects, and the role of speech is often as an intervention that modifies the effects of other speech. So speech is constrained by previous or concurrent speech, as lines of proofs are constrained by previous lines. But many features of our surroundings, and not just the linguistic features, produce sensory effects, effects that include distinctively linguistic neural ones, since they can independently prompt us to speak, and do in fact constrain what we say. So linguistic acts should be understood as interventions within an interconnected field of stimuli, some of which are linguistic, some of which are not. That said, it can still be denied that the non-linguistic portion of the constraints on speech must be understood as semantic in character. All of these constraints, both linguistic and non-linguistic, are physical ones. But they are physical constraints that have engendered by earlier sets of constraints upon ancestral neural structures. There is still room for understanding that goes beyond the systematic understanding of the neurophysiology of speech production and apprehension, but that is nevertheless not semantic.

In this general connection it is a point insufficiently appreciated in philosophy that language is transmitted mainly through its being learned by children. In the early stages of language development, there need be no articulable supporting semantic or syntactic theory for connectives, no account of satisfaction-conditions for the sentences in which they occur. And when a habit of speech has been learned, there occurs no semantical audit through which the inexplicable habits of speech are rejected and the explicable ones retained. There is only the comfortable familiarity of adult speech. That is to say that the transmission of language from one generation to the next does not depend upon understanding, only upon a growth of comprehension, and the cultivation of a capacity for the production of comprehensible speech. I do not immediately see how, but would patiently hear it argued that a shared, detailed, and articulable semantic theory might help if we were all born into linguistic adulthood, or acquired a first language in some fashion as we do a second. But there is no evidence that any such theory is required for the acquisition of language by a child, and therefore none that one is required for the continued possession of linguistic ability once it is acquired. There is certainly no evidence that such a theory is consulted in speech; in fact the automaticity of speech is all against the notion.

Now all this might be taken to excuse us from the necessity of being able to give an account of but in its conjunctive uses. The development of conjunctive but does not depend upon any child’s understanding either the ancestral, disjunctive use, or the new conjunctive one. As in the case of idiomatic faithfully, we are not required not to be puzzled, though neither need we assume that no account can be given for the role that conjunctive but plays. I know of no satisfactory such account in the literature. Grice seems to say only that the word carries a conventional implicature,
but does not venture a guess as to what in general that implicature is. He does
give but not β as a formula for the cancellation of an implicature that β. So if
his doctrines of implicature are intended as explanatory or even descriptive of the
practices of speech, then it might be concluded that the cancellation of implicatures
is a standard function of but in speech and written language. On such an account,
but is used to implicate that there was an implicature to be cancelled or at least
that the speaker or writer supposes that there was. But even if this is correct,
there remains the question as to why but plays this role, and how it does it. The
following attempts an account.

To begin, it should be said that as to the ancestral but, a crude schematic account
of its construal is easy enough to make out, for it corresponds roughly to relative
complementation. The semantic representation of It does not rain but it pours
would be

\[ \| \alpha \| - \| \beta \| = \emptyset \]

where \( \| \alpha \| \) is the interpretation of It rains and \( \| \beta \| \) is the interpretation of It
pours. The trail from the earlier use as outside at that historical juncture is still
warm. So, as with faithfully in the earlier example, we can expect the descendent
use to be regulated in some dimension or other and in some degree by that feature
of the ancestral use.

Now any account of conjunctive but must of course cover such cases as

3.29 I loved her, but she didn’t love me back

that is, it must cover the cases fitting the roughly Gricean mould. And we can
also admit on Grice’s behalf that a word (such as but or or) whose use we do not
fully understand must be susceptible to many loosenings of semantic or pragmatic
control. Nevertheless, any general account of conjunctive but must also take into
account those cases in which one of the two clauses entails the other, and it must
account for the conversational non-commutativity of such constructions, even of
these cases. We can certainly form either of

3.30 He got here, but he got here late

and

3.31 He got here late, but he got here.
The two are clearly distinct. And in particular, there is nothing queer about the
second case, even though the first clause does not implicate the negation of the
second. (Nor is there aught queer about that observation, in spite of its content.)
However, it must be admitted that uses of but can fail of queerness even where the
two clauses are equivalent, as for instance in a sentence of the general shape

3.32 If α, then β, but if not-β then not-α.
even though

3.33 α but α

without prosodic distinction is decidedly queer. Queerness in these matters is prob-
ably triggered by the failure of some neural anticipation, by some automatically
initiated connection or arrangement requiring disconnection or dissolution. And
these anticipations are probably schematic and syntactic rather than semantic in

\[\text{Remark in passing that the idea would play mildly merry Ned with his account of the putative strong sense of or, since the construction ‘α or β, but I know which’ is decidedly queer whereas ‘α or β, but I don’t know which’ is not queer in the least.}\]
character. The presence of negation or of the element $\beta$ or the combination of the two may satisfy the anticipation. And I accept that in this case the judgement is a delicate one. As between trusting our automatic responses and trusting our settled judgements in such matters we must simply decide. In any case the following remarks may not account for all such examples.

How then do we account for the conjunctive use of but in these cases, and how do we distinguish the one case from its commutation, _He got here, but he got here late_ from _He got here late, but he got here_? On this account the element of relative complementation applies to conversational effects of the main clause, and the _but_ is subtractive. Schematically the proposal presents _but_ (as it would any connective) as having associated with it an instruction set to be acted upon by the recipient. As the instructions of the set apply to the $\alpha$ and $\beta$ of an utterance of $\alpha$ _but_ $\beta$, they require the following:

Subtract from the effects of $\alpha$, those effects that it shares with the negation of or salient alternative to $\beta$.

So, as the rule applies to

3.34 I went home, but I _went_ home

it subtracts from the effects of the main disclosure, some effects that that disclosure shares with the alternative to _going_ home. Without a fuller account of the imagined situation, we cannot say what the alternatives are. However, the story can be filled in in such a way as to tell us: being dragged or carried there, or staying where trouble was brewing. In another case, say

3.35 I went home, but I went _home_

the salient alternative might be having gone to another bar or having wandered about in the wet. But again, having said this, we cannot give _a priori_ an account of either minuend or subtrahend effects.

3.7. **In the end is the beginning.** Whatever one may think residually about the place of semantics in the understanding of language, I trust that I have given sufficient reason to suppose that language can be studied from a physical, more particularly a biological point of view without ignoring its essential features. I hope that I have illustrated that this point of view also reveals features of language that, to its cost, semantic theory has neglected. I admit that in the course of this piecemeal and inadequate discussion I have made free heuristic use of conversational semantic vocabulary. I don’t see how to do otherwise, nor the necessity of avoiding the use of useful language for the latest moments of linguistic evolution. I do insist that the more neutral language of biological linguistics is essential for framing hypotheses about the earliest pre-linguistic developments that can be regarded usefully as ancestors of linguistic ones, and I claim for the idiom that we need not abandon it in framing hypotheses about later primitive stages.

I would claim also, that the study of logicalization can usefully recolour our aesthetic view of language. The earliest precursors of language represented relatively crude novel exploitations of natural effects by brains capable of such exploitations. The facts of logicalization suggest that what has centrally changed in the course of

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11Some such account may explain the effects of surprise endings as, in the right context and with the right prosodic presentation, that of _It smells awful, but it may taste terrible_.

language-brain co-evolution is the diminished scale of the crudity, matched by an increased resolving power in the brain’s capacity to exploit effects in novel ways. The scale changes, but the crudity remains. A universal daily intoning of this central fact of linguistic crudity would have a salutary, not to mention steadying, effect upon philosophical outpourings.

For more specialized philosophical interest groups, miniaturists in the style of Grice, there is also a useful new perspective; for it can be seen that this increased resolving power has a cost: linguistic transactions become liable to the kind of replicative error that we have sampled here, errors akin to the replicative errors of microbiology. Inevitably, items long removed from their ancestral lexical uses and morphologically reduced beyond recognition have little resistance to mutations that result from errors of syntactic replication. We cannot cling to the simple Gricean view. If I am right, then it is, in the words of one of my earlier respondents\textsuperscript{12}, no more than an imaginative fantasy.

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