The doctor and the semantician

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Suppose you have a patient who has been suffering for a number of days from a muscular ache in the upper legs, regularly accompanied by a gnawing pain in the right groin. The night before you see him, he was kept awake by sharp pains in the lower back, in the region of the sacral spine. Several interpretations suggest themselves: an abdominal or inguinal rupture, a spinal hernia, an inflammation – each time accompanied by a radiating of the pain from the initial location to other parts of the body. The possibility of an abdominal or inguinal rupture is easily discarded: there is no visible swelling, and neither palpation nor the valsalva maneuver reveals any. Given the fact that the patient's movements are unimpeded, and that the lasègue test is negative, a spinal hernia or similar is not very likely either, but to make sure, you have a number of x-rays of the pelvis and the lower spine taken. They confirm that the option of a spinal hernia may be rejected. Remains the possibility of an inflammation, but a blood test does not reveal any heightened values of C-reactive protein. However, a CT-scan of the lumbal spinal column displays a diffuse picture for the sacroiliac joints, so that you can now safely conclude to bilateral sacroiliitis. As a therapy for the inflammation of the sacroiliac joints, you suggest anti-inflammatory drugs.

What you do here, as a medical doctor, is putting into practice an empirical method of investigation: instead of just relying on your intuition, you think about the symptoms that would correlate with one or the other interpretation that you come up with, and then check whether those correlates indeed obtain. Your overall interpretative hypothesis is analyzed into separate, objectively testable correlates, and the best correlation yields the best interpretation. You look for convergence among different types of evidence (physical tests, blood tests, medical imaging), and the evidence that you take into consideration is not chosen randomly: the fact that a certain type of evidence correlates with a certain disease or disorder is itself based on a large number of observations, taken from numerous patients throughout the history of medicine.

Compare this to what you traditionally do as a semanticist: the procedures you follow are much more intuitive, and while both the doctor and the semantician may start from hunches, going beyond those hunches in a systematic and controllable way is much less common in semantics than in medicine – notwithstanding the recent upsurge of quantitative studies of the kind collected in this volume. Why is that ? What are the factors that prevent linguistic semantics from embracing the empirical method more frequently and enthusiastically than it actually does ? Why is a volume like the present one still relatively exceptional in the study of natural language meaning ?

One alternative to consider could be that it is *impossible* to treat semantics in this way. Semantics, of all disciplines, would seem to be most resistent to quantification: how can meaning, the most qualitative of all linguistic features, be expressed in numbers, and more broadly, how could meaning, the most ephemeral and subjective of all linguistic phenomena, be tackled with methods that aim at objectivity ? The suggestion of an impossibility may however be rejected on two grounds.

First, with a rhetorical repartee, if psychology can do it, why couldn't linguistics ? If we believe that the methods of experimental psychology – controlled experimentation and quantitative

analysis of the data – constitute a legitimate and fruitful way of investigating cognitive phenomena at large, then surely linguistic meaning may be subjected to the same kind of approach – as psycholinguists already do, of course.

Second, if we think of the traditional method of lexical semantics, we may definitely discern the basics of the empirical approach as outlined by our medical example. Think, for instance, of a historical linguist determining the reading of a passage in a historical text. The interpretative process, like the physician's interpretative effort, is guided by contextual clues (textual ones, and extra-textual, historical or cultural ones), and in general – disregarding deliberately recalcitrant hermeneutic theories like the deconstructivist approach – those interpretations will be favoured that make sense of most of the clues. From Boeckh (1877) to Joos's brilliant 'The best meaning is the least meaning' (1972), the principles of such a coherence-based method of interpretation have been spelled out fairly explicitly. Similarly, looking for generalizations has just as well been part and parcel of semantics. What else, for instance, is the classical interest in metaphor and metonymy but an attempt to establish regularities of semantic change ? If we think of these well-established facets of semantics, the transition to a quantitative methodology would seem to be a gradual step, a continuation rather than a rupture – an observation that makes it even less probable that the adoption of an empirical methodology is an impossibility.

So, given that there is little credibility to the claim that semantics is *impossible* to treat in this way, maybe there is a widespread assumption that it is *not necessary* to resort to empirical methods of the type described above. Perhaps the extra trouble of moving beyond a largely intuitive approach is not worth it: perhaps the more intuitive approach is *sufficient*, in the sense that it allows us to reach the same conclusions as a more empirical one.

Given this assumption, how can we argue for the inevitability of adopting the scientific method in linguistics? A rhetorical appeal to the 'universally accepted' nature of the scientific method will not do, for two closely related reasons. First, applying the very principle of falsifiability makes it easy to see that the statement about the universal acceptedness of the scientific method is in fact not universally valid: the method is *not* universally accepted, at least not in the humanities. Competing methods like hermeneutics and phenomenology, to name only these, *do* exist, and their existence suffices to show that the appeal to the 'universally accepted' nature of the scientific method is merely a prescriptive strategem. Second, it could be argued that the object of study in linguistics is sufficiently different from that of the natural sciences to warrant a radically different method. This, of course, is a time-honoured line of argumentation, dating back to Dilthey (1910): if there is an essential distinction between the natural sciences and the human sciences, then linguistics too will be seen as a Geisteswissenschaft rather than a Naturwissenschaft, and it will be pursued with appropriate interpretative methods.

Let us therefore rather take an analytic approach, and consider the question under which conditions the introspective method would be sufficient. To bring some system into the discussion, let us note that studying a phenomenon involves demarcating the object of enquiry, plus observing that object from a certain perspective. If you study the social monogamy of coyotes, you need to distinguish coyotes from jackals, and you need to observe whether coyotes have a life-time social bond with one partner or not (regardless of the presence of extra-pair copulation). Similarly, if you intend to study linguistic meaning, you need to distinguish what expressions belong to the investigated language(s), and you will need to make observations about the meaning of those expressions. For both aspects, the demarcational one and the observational one, we may now ask the question why the scientific method is superior

over introspection – or perhaps more cautiously, why the introspective method is not sufficient. If we try to identify the conditions that would make the intuitive method sufficient, what evidence do we have to claim that those conditions are met with or not ?

The argument pro intuition would seem to hinge on two assumptions: the assumption of a native speaker's complete command of the language, and the assumption of direct access to meaning. Let us consider both assumptions in turn.

1 The demarcation of a language boils down to the identification of the expressions of the language. Introspection will do the job if, as a competent speaker of the language, we have complete command of the language. This is Chomsky's 'ideal speaker/hearer', where he said that

> Linguistic theory is primarily about the language of an ideal speaker-hearer, in a completely homogeneous speech community, who knows its language perfectly and is unaffected by grammatically irrelevant conditions, such as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance (1965:3).

If you are an ideal speaker-hearer of this type, introspection is sufficient, but of course, the very notion of such an ideal speaker-hearer is too much of an idealization to be valid as the methodological basis of linguistics. Languages are not homogeneous, and competent speaker-hearers of the language do not have a full command of the language, not even if we restrict the perspective to passive, receptive knowledge: no individual speaker has a complete command of the language. Consider vocabulary knowledge: who could claim to know all the words of the language? If we demarcate the lexicon of the language, for instance by using a reference dictionary, we would have difficulty finding speakers with an active (or even passive) knowledge of the whole vocabulary – perhaps not even the lexicographers compiling the dictionary would pass the test. The argument against homogeneity has been spelled out many times, as have the methodological consequences: see Labov (1971) for a cogent refutation of the Chomskyan stance, and compare Geeraerts (2005) for a discussion of Itkonen's (2003) non-Chomskyan variant of the argument in favour of introspection. (On good grounds, Itkonen distinguishes 'intuition' from 'introspection', but the methodological thrust of Itkonen's reasoning is the same.)

However, we need to take a further step by considering the possibility of a 'local' interpretation of homogeneity. We may not know the language – the full spectrum of linguistic behaviour exhibited by different subgroups of a linguistic community in different contexts – as such, but we may still be able to demarcate our own language perfectly well. We may not know for sure what others say, the reasoning goes, but we definitely know what *we* would say in specific circumstances. The methodological value of introspection would be salvaged then: if each of us separately can give a reliable, first-hand report on his or her language behaviour, we can generalize over those reports to obtain an adequate image of the overall heterogeneity, without recourse to objective data-gathering methods of the type that are usual in sociolinguistics.

But self-reports are known to be unreliable. Although they may not reject the approach in all respects, classic studies like Nisbett & Wilson (1977), Ericsson & Simon (1980), Webb, Campbell, Schwartz & Seechrist (1981) indicate the limits on the validity of self-reports in the cognitive sciences, and even in disciplines that use self-report extensively, like some branches of social science and medical studies, the value of the method is a cause of constant concern: studies comparing subjective reports and objective measurements are conducted to check the

validity of the method. In linguistics, one need only consider the quandaries of grammaticality judgements to appreciate the restricted value of an introspective demarcation of one's own idiolect. We have probably all had the experience that in assigning grammaticality judgements – on the basis of our own idiolect – we would soon wind up with numerous question marks: even if we stay within our own idiolect, we are not entirely certain what actual linguistic behaviour we exhibit, and our intuitions about grammaticality tend to evaporate under our own eyes. With regard to this point too, the argument has been made forcefully elsewhere: see Schütze (2006) and Sampson (2007) for a very thorough recent discussion. Overall, we may conclude with Sampson that there is no reason to assume that patterns in a speaker's intuitive grammaticality judgments reflect realities of his language.

2 With regard to the observational part of the methodology, introspection will be vindicated if we assume that meaning is a directly observable property of linguistic expressions. Note that this argument in favour of introspection is independent of the previous, demarcational point. Even if we accept that we need to have recourse to objective data-gathering to get an idea of what actually occurs in the language (rather than what we think occurs), we could still maintain that our understanding of each of the expressions so encountered is immediate and indubitable. In general, the empirical method ss based on the notion that every descriptive term in the language of science needs to be connected with terms designating observable properties of things. Further, this connection should be of such a kind that statements applying such terms can be intersubjectively confirmed. Given such a description, meaning would seem to be an observable property of linguistic expressions. After all, when we read *The glass is on the table*, we observe something more than a mere succession of sounds: we observe something conceptual, notional, cognitive; we know that something is meant, and we think we know what is meant.

Now surely, it makes no sense denying that we understand language, and that that understanding can be part of our methodology. But how far does that understanding go, and does it go far enough to halt the incorporation of the scientific method into semantics? The problems we may identify mirror the ones we mentioned with regard to the demarcational use of introspection.

To begin with, consider the issue of homogeneity. We cannot simply assume that our own reading of an utterance, however clear it may seem to us, reflects a common interpretation: we would need to go beyond our own observations to reach certainty on this point. But is this really a problem ? As with regard to the demarcational issue, the global problem would not be decisive if a local transparancy is maintained: individually, we may have no direct access to other people's interpretations, but as long as we have direct access to our own meanings, a picture of the global situation may be obtained indirectly, through comparison and discussion. If subjectively we have direct access to our own interpretations, we can intersubjectively compare interpretations to see if we understand language in the same way.

One philosophical problem with this position is the danger of an infinite regress. To compare our individual interpretations, we would need a language, and the language would have to be a common one, i.e. one that we know is the same for all of us. How else could we compare our individual interpretations in a safe way ? So, in order to establish whether we are speaking a given language in the same way, we need to establish whether we are speaking our language of intersubjective comparison in the same way – and so on ad infinitum. It is not very likely that we could get out of this regress if we don't find a way of methodologically transcending our

individual intuitions, by correlating them with more objectifiable observations.

Further, whether we have indubitable direct access to our own interpretations is as doubtful as the idea that we have a complete introspective overview of our own speech habits. Take the simple example we mentioned above: *The glass is on the table.* What would it mean to say that we clearly and distinctly understand such an utterance ? When we say the glass is on the table, we understand, among other things, that there is an object that belongs to the category 'glass'. If we claim that our understanding of the utterance is perfectly transparent to ourselves, that would mean that in the process of understanding the utterance, we realize a clear and distinct idea of the category 'glass'. Such a clear and distinct idea of 'glass' would include an answer to a number of issues that typically relate to natural language categorization: what are the boundaries of the category, both intensionally and extensionally ? Is shape the defining characteristic, so that plastic or earthenware glass-shaped objects would still be glasses ? Or is it function ? Or is it material ? Or specific combinations of these features ? If our direct access to meaning would be as transparent as the intuitionists claim, we would have an immediate, mentally accessible answer to those questions when we understand an utterance like *The glass is on the table*. But clearly, we don't. (This point has been made before: see the discussion in Geeraerts 1999, Kristiansen and Geeraerts 2007 of Wierzbicka's Natrual Semantic Metalanguage.)

The point may be further illustrated with an example from recent research in Cognitive Linguistics. Kemmer & Verhagen (1994) have argued that the choice between the causative auxiliaries *doen* and *laten* in Dutch is determined by (in)direct causation. Indirect causation refers to 'a situation that is conceptualized in such a way that it is recognized that some other force besides the initiator is the most immediate source of energy in the effected event' (Verhagen & Kemmer 1997: 6). Typically, in a sentence like *De agent liet de studenten passeren* 'The police man let the students pass', the students ultimately do the passing: the policeman only creates the conditions for the students to perform the action. Now, if the idealist view of direct access to meaning were correct, the presence of (in)direct causation would be easy to establish: we would just have to inspect our own interpretation of utterances like *De agent liet de studenten passeren.* If we want to avoid circularity, that would mean being able to determine whether sentences containing the causative construction exhibit direct or indirect causation regardless of whether the causative verb is filled out by *doen* or *laten.* So how, in a sentence like *The teacher CAUSE the students finish their book*, could (in)directness be established? As we probably have no direct intuition of (in)direct causation, we will look for ways of operationalizing the concepts in question. Like our doctor, we will look for indices of (in)direct causation that are easier to establish than (in)direct causation as such. Following d'Andrade (1987), Kemmer and Verhagen for instance assume that we conceptualize situations with animate causers and causees in such a way that animate beings are not normally thought of as acting directly upon other human beings. While physical entities and forces are taken to exert a direct action on other things, animate beings exert an influence on others only indirectly, through the intervening medium of the physical world. It follows that direct causation is considered typical for physical, inanimate contexts, whereas animate contexts prime for indirect causation. But we will probably have to take a few more steps (Speelman & Geeraerts, *In press*), and the operational criteria may well turn out to be fairly complex, semantically speaking. For instance, while the description provided by Kemmer and Verhagen suggests that the distinction between physical and immaterial causation plays a role, a further analysis would have to refine that distinction. Difficult cases are likely to occur: if *the lightning made the alarm*

go off is material causation, should we then also classify *the lightning made the children tremble* as physical? The latter sentence probably features a less material type of causation than the former, but at the same time, *the lightning made the children tremble* would seem to be more material than *the idea of having to stay alone at home made the children tremble*. So where would we draw the line? The methodological point, however, would not be to enforce a binary categorial decision in every possible case, but rather to find an operationally applicable set of diagnostic features that would make it possible to chart all possible borderline cases and nuances. Such a componential analysis of the relevant contexts of use would indeed almost inevitably imply that the concepts 'direct' and 'indirect causation' stop being categorial variables, but rather reveal themselves as prototypical reference points on a continuum (or perhaps even in a multidimensional semantic space).

In general, then, the demarcational use of introspection rests on the assumption of homogeneity in a linguistic community, and the observational use of introspection rests on the assumption of direct access to meaning in the individual. These are both assumptions that have to be taken seriously, as both have a considerable pedigree in the history of the linguistics and the human sciences: the former has its origins in the structuralist conception of the linguistic system, and the latter in an idealist philosophy of mind. That is to say, an indignant reaction of the type 'How is it possible that at the beginning of the 21st century we still have to argue for the adoption of empirical methods in linguistics ?' is unnecessarily naive. It is true that compared to psychology, linguistic semantics lags behind roughly a century in the large-scale adoption of an empirical methodology, but the retardation is not due to ignorance or inflexibility, but rather to deeply held assumptions that deserve to be examined in their own right. However, as both assumptions appear to be questionable upon such an examination, we may conclude that the intuitive method is not sufficient.

But does that mean, conversely, that intuition can be dispensed with ? We've given a negative answer to the question whether an empirical methodology might be unnecessary, but should we now conclude, turning the question round, that intuition is not only not sufficient, but also not necessary ? In order to answer that question, we should first try to summarize the central aspects of research as meant by the scientific method, where the 'scientific method' - as in our good doctor's endeavours - is the approach to scientific investigation in which the empirical testing of hypotheses is paramount: systematic data gathering on the basis of observation or experimentation yields material that may be used to falsify predictions derived from a theoretical hypothesis. (This passage is an elaboration of a number of remarks made in Geeraerts 2006.) What are the main features of empirical research?

First, empirical research is *data-driven*. You cannot easily draw conclusions from single cases and isolated observations, and the more data you can collect to study a particular phenomenon, the better your conclusions will get.

Second, empirical research in linguistics may be *observational or experimental;* there is a complementarity between both approaches. The research data may come from different sources: they may be collected as they exist (as is the case in corpus research), but they may also be elicited by doing experimental research, or by doing survey research. As applied to language, the mutual advantages of observational versus experimental research are clear: observational research (viz. corpus research) allows you to study language in a natural and spontaneous state; but experimental research, by contrast, may give you a better control over specific variables, as when they are underrepresented in the corpus.

Third, empirical research involves *quantitative methods*. In order to get a good grip on the broad observational basis of elicited and/or non-elicited data, investigators need techniques to come to terms with the amount of material involved. Specifically, they will need statistical tests to determine whether specific observations might be due to chance or not. It needs to be emphasized that quantification is not the essence of empirical research, but simply follows in a natural way of what an empirical methodology tries to achieve: quantification in empirical research is not about quantification, but about data management and hypothesis testing.

Fourth, empirical research crucially hinges on asking the right questions, or in other words, on the *formulation of hypotheses*. No perception could be more misguided than to think that once you have your database of elicited or non-elicited observations, the conclusions will arise automatically and purely inductively from the data. On the contrary, the only conclusions you will be able to draw are the ones that relate to hypotheses you have formulated and tested – so that will be the investigator's first task. Another way of saying this is that empirical research necessarily combines inductive and deductive reasoning: on the one hand, you work in a bottom-up way from data to hypotheses, but on the other hand, those hypotheses will also be derived top-down from the theoretical perspective you adopt in thinking about your data.

Fifth, empirical research requires the *operationalization of hypotheses*. It is not sufficient to think up a plausible and intriguing hypothesis: you also have to formulate it in such a way that it can be put to the test. That is what is meant by 'operationalization': turning a hypothesis into concrete predictions that can be tested against the data. In most empirical research in linguistics, it is questions of operationalization that require all the ingenuity of the researcher – and most of his or her time, because getting the relevant data and measurements is not an automatic process.

And sixth, empirical research involves an *empirical cycle* in which several rounds of data gathering, testing of hypotheses, and interpretation of the results follow each other. Just like it is misguided to think that empirical, data-driven research automatically gives one all the answers, it is misguided to think that it immediately gives one the final answer. The empirical cycle as such, in fact, does not constitute a straightforward march towards the truth, because negative results may be interpreted in different ways. If a prediction is not borne out, at least two kinds of interpretation suggest themselves: the original hypothesis (or the broader frame-work in which it is couched) may be wrong, but in principle, it could also be the case that our operationalization of the hypothesis was not adequate. The assumption may be wrong, or our way of testing the assumption may be inappropriate - but the consequences in either case are largely different. Empirical research seeks maximal objectivity, but it is in no way a mechanical procedure that inevitably leads to a single possible result. That is not the way it happens in the hard sciences, and it is not the way it happens in the study of language either.

This is a point that cannot be sufficiently emphasized, because the way in which the empirical cycle is often represented in introductory treatises on scientific method is often overly optimistic. The stereotyped view, so to speak, is represented in Figure 1. A theory leads to predictions in the form of specific hypotheses, which are experimentally (or more broadly, operationally) tested. The analysis leads back to the theory: the experimental results may or may not contradict the theoretical expectations.
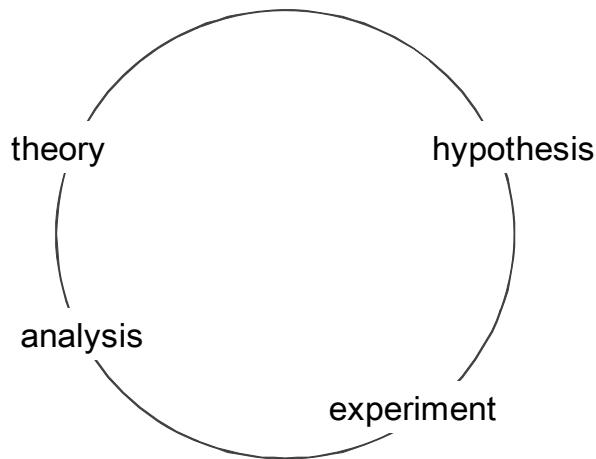
Figure 1

In actual practice, however, negative experimental results are not necessarily seen as straightforward falsifications of theories. For one thing, negative results may be attributed to inappropriate experimental designs rather than to theoretical mistakes. The famous Michelson and Morley experiments are a case in point: instead of rejecting the assumption that there is an ether (which would seem obvious from our contemporary point of view), their failure to measure an ether wind only stimulated them to find ever more sophisticated experimental designs to observe the ether wind. Further, rather than to a wholesale rejection of a theory, negative results may lead to modifications in the theory and refinements of the hypotheses. This, after all, is the basis of the Kuhnian theory of science: as against the strict falsificationalist model of scientific enquiry proposed by Popper (1963), Kuhn (1970) and his followers (Lakatos 1974) stress the fact that scientific paradigms have a tendency towards inertia. Rather than rejecting theories on the basis of single negative results, theories are adapted to deal with the critical findings; and (apart from the specific dynamics of the sociology of science) it is basically only when the adaptive capacity of a theory runs that it may be abandoned as a whole.
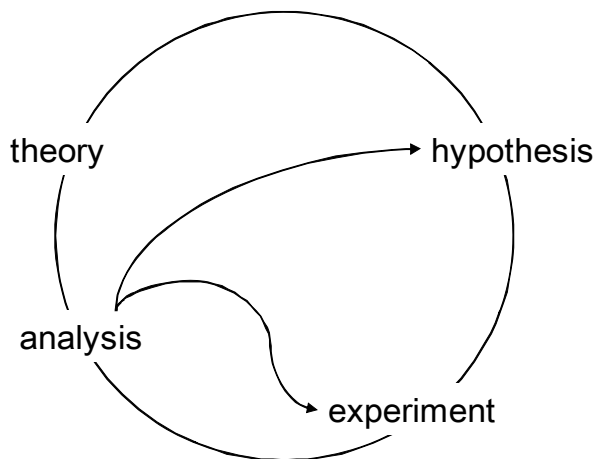

Figure 2

In Figure 2, a more sophisticated version of the empirical cycle is drawn: upon analysis, experimental results may lead (in decreasing order of theoretical radicality) to a change of theory, a change of theoretical assumptions and hypotheses within a theory, or a change of experimental design. From the point of view of the present discussion, the non-mechanical nature of scientific progress again testifies to the importance of subjective elements in empirical research: results need to be interpreted and decisions need to be made on the basis of those interpretations.

What then about the role of intuition in empirical research ? On the basis of this nutshelled presentation of an empirical approach, we may conclude that empirical research does not rule out creativity and intuition. To the undiscerning eye, the ideal of scientific objectivity would seem to banish the investigator as a subject from the investigation, but a closer look makes clear that ingenuity and interpretative insight are indispensable features of the empirical cycle. Hypotheses translate an intuitive understanding into operational predictions; finding the right operationalization rests on inventiveness as much as on expertise; and processing the results of the empirical cycle requires creative imagination. Empirical research does not lower the demands on the subjective skills of the researchers; it only raises the criteria for the objective validity of their claims.

So will the doctor save the semantician ? Or is the adoption of an empirical method a failproof way of making the human scientist lose his soul ? It should be clear from the foregoing discussion that an empirical method should not be treated fetishistically - neither as a positive nor a negative fetish. It is not a mechanical march to absolute truth, but an inevitable and indispensable next step for anyone dealing with the intricacy and elusiveness of natural language meaning: in a systematic attempt to overcome the insufficiencies of an intuitive method, it broadens the basis of the hermeneutic process with maximally objectifiable data and it supports the interpretative efforts with quantitative techniques. Rather than being the opposite of a more traditional hermeneutic approach, an empirical approach to semantics is the completion and consummation of it.

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