Abduction as a methodological approach to the study of spoken interaction

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Studies of spoken interaction generally proceed in one of two ways: either an explicit theory of communication and interaction is used as the basis for analyzing actual instances of talk, or conversational data are taken as the point of departure for formulating new theoretical concepts and rules. The first "deductive" model is mostly found in various pragmatic approaches within linguistics, whereas the second "inductive" model is predominant in ethnomethodological conversation analysis. However, practical scientific programs cannot be based on either pure deduction or pure induction. Central to any scientific process is the inferential step from some initial puzzling fact to some theoretical hypothesis which can explain it. This inferential process is called *abduction* by the pragmatist philosopher Charles S. Peirce. The aim of this article is to describe how abduction enters into the methodologies of two approaches to spoken interaction, Gricean pragmatics and Conversation Analysis, and thereby contrast their respective conceptions of theory and data.

The article[1] first gives a brief outline of Peirce's theory of abduction and discusses its place in a pragmatically oriented philosophy of science. Second, the methodologies of Conversation Analysis and Gricean pragmatics are presented and evaluated on the basis of an abductory approach to scientific practice.

Peirce's theory of abduction

Although the concept of *abduction* was originally introduced by Aristotle, it is the American philosopher Charles Sanders Peirce (1839-1914) who developed it into an explicit theory of inference. He proposed that the traditional modes of inference – induction and deduction – should be complemented by a third mode – abduction – which he claimed was qualitatively different from the two others.

Let us first consider an example of deduction.

Deduction
Rule: All the beans from this bag are white
Case: These beans are from this bag
Result: These beans are white

This amounts to inferring a result, given a general rule and a given case[2].

Now consider induction:

Induction
Case: These beans are from this bag

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[1] This article is a slightly revised version of my trial lecture for the *doctor artium* degree, originally presented at the University of Oslo in October 1997. This also explains why it is written in English.

[2] Peirce here seems to use the word "rule" to refer to an abstract, general claim, "case" to refer to a particular abstract relationship, and "result" to refer to a single empirical observation.

Result: These beans are white
Rule: All the beans from this bag are white

Induction consists in the inference of a rule, given a specific case (a precondition) and a result (an observation). This inference involves generalization, that is, reasoning from particular instances to a general law, rule or pattern.

In deduction the conclusion follows from the premises with necessity, whereas in induction it does not. We might find out that only the beans at the top of the bag were white, whereas those at the bottom were brown.

Finally, this is an example of abduction:

Abduction
Rule: All the beans from this bag are white
Result: These beans are white
Case: These beans are from this bag

Abduction is thus inferring a case from a rule and a result. Like induction this inference is also more or less probable, and not sure. The beans could in fact have come from the bag of mixed beans or from a bag that is no longer there.

Pierce goes on to develop abduction as a process of gaining new knowledge. The basic elements of this process are outlined in the following quotes:

> "All our knowledge may be said to rest upon *observed facts*.
> […]
> But observed facts relate exclusively to the particular circumstances that happened to exist when they were observed. […] They, therefore, do not, in themselves, contain any practical knowledge.
> […]
> Any proposition added to observed facts, tending to make them applicable in any way to other circumstances than those under which they were observed, may be called a hypothesis.
> […]
> The first starting of a hypothesis and the entertaining of it, whether as a simple interrogation or with any degree of confidence, is an inferential step which I propose to call *abduction*. […] This will include a preference for any one hypothesis over others which would equally explain the facts, so long as this preference is not based upon any previous knowledge bearing upon the truth of the hypotheses, nor on any testing of any of the hypotheses, after having admitted them on probation."
>   (Peirce 1955:150–151)

As we see, abduction starts with consideration of facts, that is, particular observations. These observations then give rise to a hypothesis which relates them to some other fact or rule which will account for them. This involves correlating and integrating the facts into a more general description, that is, relating them to a wider context (Givón 1989).

The process of abduction is described as follows:

> "The surprising fact, C, is observed;
> But if A were true, C would be a matter of course,
> Hence, there is reason to suspect that A is true."
>   (Peirce 1955:151)

Here are a couple of examples Peirce gives of this process:

Practical reasoning:
One observes a man with a special dress, expression of countenance, and bearing.
One knows that such features are characteristic of Catholic priests.
One infers that this man is a Catholic priest.
        (Peirce 1955:151)

Scientific inquiry:
"Fossils are found, say, remains like those of fishes, but far in the interior of the country.
To explain the phenomenon, we suppose the sea once washed over this land."
      (Peirce 1994:155, Fann 1970:21)

As can be seen, Peirce's theory of abduction is meant to cover both practical reasoning and scientific inquiry. In fact, the philosopher seems to consider science as a just a special case of human sense-making.

What makes us opt for a specific hypothesis? Here Peirce admits that it is to a great extent a matter of guessing. But for any intriguing observation there is an infinite set of possible explanations. And as we have seen, abduction is not just choosing *any* hypothesis, but selecting one as more plausible than the others. Peirce presents a set of criteria for choosing the best hypothesis. And here his theory is more explicitly concerned with the methodology of scientific inquiry.

He mentions three criteria for favoring one hypothesis over others, namely:

1 The hypothesis should explain the facts
2 It should be economical
3 It should be capable of being subjected to experimental testing
      (Fann 1970:43)

The first criterion is that a hypothesis should *explain* the surprising facts. That the hypothesis should be explanatory means that it should account for the concrete, observable phenomena by invoking facts or rules from some other domain, for instance some abstract law or non-observable process. In this respect he opposes the positivists of his time, who claimed that science should be a mere *description* of observable phenomena (Fann 1970:45).

The second criterion for favoring a specific hypothesis over others is *economy*. One should favor those hypotheses which minimize the costs in terms of testing and which maximize the effects in terms of encompassing claims. Some citations will illustrate this point:

Cost: "Try the theory of fewest elements first"
        "[…]the simplest hypotheses are those of which the consequences are most readily deduced and compared with observation; so that, if they are wrong, they can be eliminated at less expense than any other"
          (Collected Papers 4.35 and 6.532, cited in Fann 1970:48-9)

Effect: "[…] other things being equal, that theory best fulfills its function which brings the most facts under a single formula."
          (Collected Papers 7.410, cited in Fann 1970:30)

The third criterion is that the hypothesis should be capable of being subjected to experimental testing. This testing process includes deduction and induction. First, the scientist deduces that if the hypothesis is true, creating a certain set of conditions will yield certain observable results. He may then undertake a series of experiments, and if the results are favorable, extend a certain confidence to the hypothesis. This is an inductive process (Pierce 1955:152).

Peirce's reference to *experimental* testing and *predictions* seems to point towards *causal* explanations. However, in an essay called "The law of mind" he acknowledges that human behavior cannot be treated in the same way as dead matter:

> […] the mind is not subject to "law" in the same rigid sense that matter is. It only experiences gentle forces which merely render it more likely to act in a given way than it otherwise would be. There always remains a certain amount of arbitrary spontaneity in its action, without which it would be dead. (Peirce 1955:348)

The existence of these "gentle forces" clearly makes it impossible to formulate predictive theories that may be experimentally tested. Rather, it seems to warrant explanations in terms of *functions, principles* or the like rather than *causes*.

As we have seen, Peirce identifies the three modes of inference with three different stages in the process of scientific inquiry:

First stage:	Abduction	(adopt a hypothesis on probation)
Second stage:	Deduction	(spell out the necessary and probable experimental consequences)
Third stage:	Induction	(assess the plausibility of the hypothesis on the basis of observed results of predictions)

Peirce notes that the process of induction involved in hypothesis testing is not a mere generalization of particulars. Rather, it involves interpretation of the results, involving estimation of their significance. This means that the process is not pure induction, but involves an element of hypothesis, that is, some element of guesswork which unites the observations. This is an insight which was presented already by Aristotle:

> "The particular facts are not merely brought together, but there is a new element added to the combination by the very act of thought by which they are combined […] The pearls are there, but they will not hang together until someone provides the string."
> (Aristotle, Posterior Analytic, vol. II, p. 19, cited in Givón 1989:286)

This inductive process is therefore referred to by Peirce as *abductory induction* (Peirce 1955:152).

Any inference which involves contextual judgments of relevance and significance has an abductive element. This makes abduction an essentially *pragmatic* mode of reasoning (Givón 1989:242f). The only mode of inference which does not make any appeal to aspects of context is deduction. This makes abduction and deduction polar opposites.

Other points of contrast between these inference types are *certainty* and *productivity*. We have already seen that deduction is the only mode which guarantees the truth of the conclusion given the truth of the premises. But, unlike traditional logicians, Pierce is not just interested in the certainty of conclusions. He claims that logic should also be concerned with the informational *productivity* of inferences, that is, whether or not they contribute *new ideas*, exceeding the information which is implied by the premises. Deduction does not produce any

such new ideas, whereas abduction does. This gives us the following oppositions: Deduction gives certainty for the conclusions, but is not productive and does not involve considerations of context. Abduction is a less than certain mode of inference, but is productive and relies on contextual judgments:

	Certainty	Productivity	Context-sensitivity
Deduction	+	−	−
Abduction	−	+	+

Contrasts between abduction and deduction

Induction is falls somewhere in between these poles. It provides greater certainty than abduction, but less productivity. Induction only becomes productive in combination with abduction, as in abductory induction.

Abduction and the philosophy of science

In the philosophy of science, the process of creating a hypothesis has often been dismissed as an "irrational element" of scientific inquiry. For instance, Popper notes that:

> "The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man […] may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge."
> (Popper 1959:31)

Instead, various philosophical traditions have recommended either an inductive or a (hypothetico-)deductive methodology. Givón (1989) claims that the preference for the one or the other is correlated with traditions of epistemology. In his words,

> "[…] the discrete cleavage between the two extreme reductionist schools in epistemology – rationalism and empiricism – is echoed faithfully in the split between rigid **deductivism** and rigid **inductivism**, respectively, in the philosophy of science."
> (Givón 1989:269)

Epistemology:	rationalism	empiricism
Methodology:	deductivism	inductivism

The combination of inductivism and empiricism can be found in pure form in the teaching of the logical positivists, such as the early Wittgenstein. In psychology it has given rise to behaviorism and in linguistics to the American structuralism associated with Bloomfield.

Deductivism and rationalism have roots back to Plato and Descartes, but in linguistics they are today associated with Chomskyan generative grammar.

As we have seen, Pierce rejects the idea that induction in itself can contribute to new ideas. Empirical observations will always require an abductive "leap of faith" to be included under a hypothesis.

Givón (1989) uses Peirce's theory to criticize also the deductivist approach. The problem is that it misrepresents the *directionality* of explanation and scientific discovery. Explanation is the process of hypothesizing the general rule, not of spelling out its consequences.

Deduction is thus rather the *testing stage* of an explanatory hypothesis, not its discovery (Givón 1989:303).

As an alternative, Givón presents Peircean abduction as a pragmatic account of scientific method. Some elements of this account are summarized in these points:

1   The status of observable facts: The objective 'givenness' of facts is considered a problematic matter. These are "conceptualized within the **context** ('framework') of a particular theory, on whose vocabulary they are at least partially dependent." (Givón 1989:289)

2   The relevance of data: "[…] the choice of *puzzling facts* is inseparable from the choice of *significant questions* concerning those puzzling facts. The puzzlement is indeed, itself, the question. […] And one's surprise depends […] on one's **presuppositions** about the domain, i.e. on one's **prior context**" (p. 292–3)

3   The nature of explanation: "Explanation [is] the viewing of the facts to be explained in **a wider context**; it fits facts in a **larger pattern**" (p.301) Causal connection is just one species of connection. In addition there are *functional* explanations, which differ from causal ones merely in that the context to which a phenomenon is linked is some biological or behavioral **function**. *Correlation* is merely the *heuristic* from which the scientist proceeds to abduct an explanatory connection (p. 309)

Givón's conclusion is that "data not defined by theory is empty, and theory not driven by data is blind" (p.321)

Methodology in the study of spoken interaction

If one accepts the pragmatic account of the scientific method, abduction is an inherent feature of all scientific discovery; and the study of spoken interaction is no exception. However, to my knowledge, none of the existing approaches are based on an explicitly formulated abductive methodology. Rather, many of the approaches are characterized by a lack of methodological reflection, and even characterizations in terms of inductive and deductive modes of reasoning are very hard to find.

In the following, two approaches to spoken interaction will be investigated – Grice's theory of conversational cooperation and the ethnomethodological program of Conversation Analysis. Some of their explicit and implicit methodological stances will be scrutinized in order to see whether their methods conform to the abductory philosophy of science outlined above.

The philosopher Paul Grice has developed a theory of conversational cooperation which is rooted in the epistemological tradition of rationalism and has been considered as involving conceptual rather than empirical analysis (Levinson 1983:285). The practitioners of Conversation Analysis, on the other hand, take a pronounced empirical stance, seeking to carry out in practice the program of Harold Garfinkel's ethnomethodology and Alfred Schutz's phenomenology.

It was mentioned above that there has been an historical affinity between a rationalistic epistemology and deductivism on the one hand, and between empiricism and inductivism on the other. It is therefore of special interest to investigate whether this holds for the two approaches concerned here. In that case, it would be expectable to find the Gricean approach to be associated with deductivism and Conversation Analysis with inductivism.

Conversation Analysis

One way of studying a tradition of research is to see what its practitioners say they do. Some of the programmatic statements found in this tradition propose an analytic procedure in which the analysts bracket their theoretical presuppositions and seek to approach data completely unbiased:

> If […] we figure or guess or decide that whatever humans do, they are just another animal after all […] then whatever humans do can be examined to discover some way they do it, and that way will be stably describable. That is, we may […] take it that there is order at all points.
> 　　[…] given the possibility that there is overwhelming order, it would be extremely hard *not* to find it, no matter how or where we looked.
> 　　(Sacks 1984:22, 23)

> When we start out with a piece of data, the question of what we are going to end up with, what kind of findings it will give, should not be a consideration. We sit down with a piece of data, make a bunch of observations, and see where they will go.
> 　　(Sacks 1984:27)

These statements seem to indicate a program of rigid inductivism, where the data so to say "speak for themselves". This seems to me a rather naive belief in the possibility of doing away with the researcher's presuppositions. If the researcher did not have any expectations of what the data *ought* to look like, there would be no puzzling facts, nothing to explain. Thus, it is not a pragmatic, abductory approach to scientific inquiry, since it does not consider the context-embeddedness of the research process, and the expectations and presuppositions of the researcher. Rather, this looks like reductionism towards the ideals of positivism.

In other statements Sacks presents conversation as a totally mechanistic activity:

> What we would be doing, then, is developing another grammar. And grammar, of course, is the model of routinely observable, closely ordered social activities.
> 　　(Sacks 1984:25)

> Our aim is to transform, in an almost literal, physical sense, our view of "what happened," from a matter of particular interaction done by particular people, to a matter of interactions as products of a machinery. We are trying to find the machinery.
> 　　(Sacks 1984:26)

The rules of grammar operate in an "all-or-nothing" way and do not leave any room for negotiation. Similarly, a machinery is a rigid mechanism which operates independently of intentions, emotions or obligations. The metaphors Sacks here uses are susceptible of evoking a picture of conversation as a behavioristic stimulus-response process, where the interactants spit out their lines as if they were preprogrammed robots. This is far from Peirce's "law of mind" with its "gentle forces" guiding human behavior.

Furthermore, the talk of "machinery" and "grammar" seems to propone a purely *descriptivist* account. When the mechanisms of the machinery are identified, or when the rules of the grammar are written, the job is done for the analyst. No further explanation is required. Also this aspect is characteristic of a positivist and not a pragmatic philosophy of science.

This was a review of what the father of Conversation Analysis, Harvey Sacks, claimed to be doing. However, instead of considering what the practitioners *say* they do, another way of approaching the field may be to study what they actually *do* in their empirical studies. Although we do not have access to their actual process of investigation, we may glean at least some things from their reportings. As an example, let us consider a classic paper that is basic to much of the subsequent research in this field, namely Sacks, Schegloff & Jefferson's 1974 article, "A simplest systematics for turn-taking in conversation".

Here the authors *do* in fact seem to be puzzled by some phenomena they have observed. Some of the facts they set out to describe and explain are presented in the following points. In conversation,

1   Speaker-change recurs, or at least occurs.
2   Overwhelmingly, one party talks at a time.
3   Occurrences of more than one speaker at a time are common, but brief.
4   Transitions (from one turn to the next) with no gap and no overlap are common. Together with transitions characterized by slight gap or slight overlap, they make up the vast majority of transitions.
5   Turn size and order are not fixed, but vary.
6   The distribution of turns is not specified in advance.
7   The number of parties can vary.
        (Sacks, Schegloff & Jefferson 1974:700–701)

The system they devise to account for these phenomena, has the character of an explanatory hypothesis. They posit a set of "turn-taking rules" that the interactants presumably orient to in talking together. The system can be summarized as follows:

1   *Turns* are made up by *turn-constructional units*, which may be sentential, clausal, phrasal, or lexical constructions. Instances of these constructions allow projection of a possible completion of the unit. A speaker is initially entitled to one such unit. The first possible completion of a first such unit constitutes an initial *transition-relevance place*, that is, an opportunity space for speaker shift.

2   *Turn allocation* may be effectuated in two ways:
    a) The current speaker may select a next speaker (by means of address terms, gaze direction etc.). In that case, the selected person has the right and obligation to take the next turn.
    b) If no next speaker is selected the interlocutors may self-select. The first starter acquires right to the turn. Also the current speaker may self-select, and thus continue to speak.
        (Based on Sacks, Schegloff & Jefferson 1973:702–704)

This amounts to more than a descriptive generalization of what happens in conversation. The system does not only describe what interactants do, but what they *should* do at various points in a conversation. The rules are normative phenomena and are not themselves observable in the data. In this way, the theory of turn-taking is not arrived at by induction; it represents an abductory leap into another domain; from a set of empirical observations to a set of norms. An inductive generalization would be like a statistical presentation of what *usually* happens. However, the set of norms operating in the turn-taking system is claimed to be operative also when the norms are not observed: A person who does not wait until a transition-relevance place before taking the turn will be a potential target of reproach or sanctions by his interlocutors. The system is thus not a formulation of regularities in conversation, but a claim

about an underlying set of norms shared by the interactants. These norms are formulated *partly* on the basis of observed regularities in conversation, but just as much on the basis of what happens in "deviant cases". In this way, the account seems to involve an abductory, and not an inductive methodology.

The turn-taking system is explanatory in the sense that it accounts for the actual facts, and is testable against other empirical materials, that is, other instances of conversation. However, the explanation is clearly not a *causal* one. This would have made the account a behaviorist-like model, with the speaker's turn-allocation as the stimulus and the hearer's taking the next turn as some more or less automatic response. No, the authors note that interactants may, on occasions, deviate from the rules, such as is the case with interruptions. However, a deviation will be treated by the interactants *as* a deviation, and the "transgressor" will be accountable for it.

So, instead of claiming a *causal* connection between rule and behavior the system may be interpreted as a *functional* explanation. The authors do not themselves use such characterizations, but they say that the rule system should coordinate speaker transfer "so as to minimize gap and overlap" (p. 704). This seems to indicate that the system is constructed so as to maximize the *efficiency* of exchange. This interpretation is furthermore supported by another claim by the authors that the system should be expected to be "shaped as an economy" (p. 701). Without being very explicit about this point, the authors allude to an explanation in terms of more general principles of human action and cooperation, and thus relate the turn-taking behavior to behavior in other domains.

The proposed systen amounts to a functionalist explanation in that it motivates the rules by reference to the requirements of the activity. It does not relate them to the actors, their intentions or motivations, but to the functioning of the very exchange process it is set to organize. It is not an *actor-explanation* but a *process-explanation*.

The speech act philosopher John Searle has criticized this approach for not being explanatory at all, but a mere description of regularities. Here are his objections:

> "Now what exactly does the [turn-taking] rule say when it is stated in plain English? It seems to me they are saying the following: In a conversation a speaker can select who is going to be the next speaker, for example, by asking him a question. Or he can just shut up and let somebody else talk. Or he can keep on talking. […] Now, as a description of what actually happens in a normal conversation, that is, a conversation where not everybody talks at once, the rule could hardly fail to describe what goes on.
> […] The objection to this kind of "rule" is that it is not really a rule and therefore has no explanatory power."
> (Searle 1992:15–16)

Instead, Searle claims that a rule should play a *causal* role in bringing about the behavior:

> "the purpose of the rule is to influence people's behavior in a certain way so that the behavior matches the content of the rule, and the rule functions as part of the cause of bringing that match about.
> […]
> There can be extensionally equivalent descriptions of my rule-governed behavior not all of which state the rules that I am following.
> […]
> The so-called rule for conversational turn-taking, like much similar research I have seen in this area […] describes the phenomenon of turn-taking as if it were a rule; but it couldn't be a rule because no one actually follows that rule."

(Searle 1992:17)

Searle thus requires that a rule should explain behavioral phenomena by relating them to speakers' intentions. Here we touch upon one basic difference between Speech Act Theory and Conversation Analysis. Conversation Analysts have a declared policy of not using mental entities in their explanations. As noted above, their explanations are process-explanations rather than actor-explanations. However, this does not mean that they do not require participant orientation to the rules. The central requirement of a rule in Conversation Analysis-tradition is that it can be shown that the participants design their contributions by reference to it. This does not mean that people always *follow* the rules, but that they treat deviations from them *as* deviations. For instance, when someone breaks the turn-taking rules by interrupting someone, they may either be sanctioned by the others, or they may themselves account for their deviation by saying such things as "Sorry for interrupting, but...". Therefore such deviant phenomena are not claimed to invalidate, but rather to *confirm* the rules of conversation.

This type of evidence should make it clear that the turn-taking rules are not just descriptive generalizations. They make specific claims about when participants should or should not speak. Transition-relevance places occur at points of possible completion of a turn. These points are opportunity spaces where participants may start talking and thus legitimately acquire a turn in a conversation. Other points do not provide this opportunity.

In his response to Searle's criticism, Schegloff presents several sorts of evidence that participants orient to such possible completion points. Here is some of it:

> "[…] decisions to shut up or keep talking have a very different character (and very different likelihood of occurrence) at different points in the talk. Once launched into a turn-constructional unit a speaker is under some onus to talk to possible completion; once arrived a such a point, the speaker encounters a structurally provided occasion for other participants' opportunities to take over.
>
> Similarly, starting up by an interlocutor is of differing import and differing frequency […] depending on the point a current speaker's turn has reached. Talk which overlaps a current speaker's talk may be recognized as "interruptive" if initiated nowhere near a possible completion, and as enthusiastic if overlapping what has already been recognized as its incipient possible completion."
> (Schegloff 1992:117-118)

To bring this back to the perspective of Peirce's abductory methodology, Schegloff's claims about interruption here may be considered as deductions from the hypothesis. These may be tested empirically to strengthen or weaken the theory of turn-taking.

To conclude, then, Conversation Analysis in actual practice has the characteristics of an abductory and explanatory method of science. This in spite of some programmatic formulations that seem to convey a commitment to pure inductivism and descriptivism.

Grice's theory of conversational cooperation

Grice's theory of conversational cooperation has its outspring in ordinary language philosophy. It accounts for certain aspects of utterance-meaning in conversation by positing a set of principles and maxims. These are:

Grice's Cooperative Principle
Make your conversational contribution such as is required at the stage at which it occurs, by the accepted purpose or direction of the talk exchange in which you are engaged.

The maxims of conversation
Quantity:
> Make your contribution as informative as is required (for the purposes of the exchange).
> Do not make your contribution more informative than is required.

Quality: Try to make your contribution one that is true.
> Do not say what you believe to be false.
> Do not say that for which you lack adequate evidence.

Relation: Be relevant.
Manner: Be perspicuous.
> Avoid obscurity of expression.
> Avoid ambiguity.
> Be brief (avoid unnecessary prolixity).
> Be orderly.

(Grice [1975] 1989:26-27)

The formulation of the cooperative principle and the maxims is not based on observation of data. Rather, they are motivated by inherent properties of conversation and communication *as such*. Grice motivates his theory in this way:

> I would like to be able to show that observance of the CP [Cooperative Principle] and maxims is reasonable (rational) along the following lines: that any one who cares about the goals that are central to conversation/communication (e.g., giving and receiving information, influencing and being influenced by others) must be expected to have an interest, given suitable circumstances, in participation in talk exchanges that will be profitable only on the assumption that they are conducted in general accordance with the CP and the maxims.
>   (Grice 1975:49)

As we see here, the maxims are motivated by human rationality and inherent goals of conversation and communication. The validity of such claims depend in the end on the meaning of concepts such as "rationality" and "communication". And this is a matter of conceptual analysis rather than empirical investigation.

This is thus a *theory-driven* rather than a *data-driven* approach. The claims about spoken interaction are based on theoretical primitives (such as *meaning, rationality* and *communication*) rather than on observation of data. It thus seems to fit nicely into Givón's category of rationalist and deductivist theories.

The puzzlement which initially motivates Grice's search for a theory of conversation is the inadequacy of the current semantic theory to account for certain diverging uses of some connectors, such as *and, if* and *or*; and quantifiers, such as *all* and *some* (Grice 1975:41). Instead of positing two distinct meanings for these expressions, Grice proposes to posit only one word meaning and account for the other meanings as implicatures that are created by the maxims in certain contexts. Thus, the puzzling facts that trigger the process of inquiry are not the uses of the expressions themselves, but the theoretical problems they pose.

The positing of a cooperative principle and a set of maxims is clearly an abductive process. It is a hypothesis which relates processes of interpretation of utterances to a wider

context, namely inherent characteristics of human communication and cognition. Let us see how it agrees with Peirce's criteria for accepting a hypothesis.

First, the hypothesis is explanatory. It accounts for facts concerning actual instances of interpretation, and it does so by invoking principles from another domain. Second, it is economical. With a limited number of maxims it purports to account for a range of different discourse phenomena, such as:

word/sentence meaning vs. speaker's meaning
disambiguation
reference assignment
indirect speech acts
irony
metaphor
tautologies
discourse coherence

The third criterion is that the hypothesis should be amenable to experimental testing. Here, it is quite clear that the theory is not able to predict actual conversational behavior or specific interpretations (Sperber & Wilson 1986). However, this is even in principle an unrealistic expectation, at least if one accepts Peirce's "law of mind" and the "gentle forces" guiding human behavior. In fact, the maxims may be seen as an attempt to formulate just those "gentle forces" – or at least some of them. For instance, people will generally be guided by the quantity maxim and produce maximally informative utterances, but there might be motivations that overrule this orientation, such as the need to manage interpersonal relationships (Brown & Levinson 1987). And which circumstances may come into play on a given occasion is impossible to predict.

However, the theory is to some extent capable of accounting for actual interpretations in retrospect. The problem here is that the phenomena the theory is to account for, namely speakers' and hearers' meaning, is itself not an observable entity. Researchers wishing to do empirical investigations may choose two different solutions.

One solution may be to use a post-recording interview to ask participants what they meant or understood at various points in the conversation. This method is practiced in interactional sociolinguistics by discourse analysts such as Tannen (1984) and Schiffrin (1987). The drawbacks associated with this procedure are several:

- First, one only gets access to the level of conscious meaning.
- Second, the participants may not remember what they meant or understood on a given occasion.
- Third, they may not want to tell the researcher what they actually thought.
- Fourth, in reporting on what happened in one part of the conversation, they may be influenced by what happened subsequently and therefore not give a true picture of what they experienced at the moment.
- Finally, the participants' reportings in a post-recording interview are not "transparent" either. They are themselves in need of interpretation, and may thus require a new post-recording interview (and so on in an infinite regress).

Another solution to the problem of assessing actual speaker's meanings or hearer's interpretations in conversation may be to investigate how the participants display their intentions and interpretations in their actual contributions to the conversation. This procedure is suggested by Bilmes (1993). Here, the key to understanding the import of a contribution is

to investigate how it is taken up and construed by the participants in their subsequent turns at talk. This method has the advantage of approaching more faithfully the actual treatment of a contribution by the participants. However, what is grasped is the participants' *joint construal* (Clark 1996) of an utterance and not their individual intentions and interpretations. And these two things are not necessarily the same.

To sum up, Grice's theory is also an abductive approach to conversation. However, it is primarily based on predefined theories of communication and rationality and not on observation of actual data. In this sense it is more dependent on deductive modes of reasoning than on inductive ones.

Conclusion

A pragmatic, abductory theory of science involves functional rather than causal explanations, such that behavior is *accounted for* rather than *predicted*. This study corroborates the claim that this sort of scientific approach is crucial to the humanities and the social sciences, where the data are constituted by behavior which in itself is meaningful and context-dependent.

The two approaches studied here in more detail, Conversation Analysis and Gricean pragmatics, both involve an abductory approach. However, they are yet radically different scientific enterprises. The claims of Conversation Analysis are primarily based on observations of data, whereas Grice's claims are primarily based on general conceptions of communication and rational agency. The conversation analytic account of turn-taking is possible to falsify by reference to actual data. For instance, it would be falsified if it was shown that overlap is just as frequent or more frequent in the middle of turn constructional units than at their borders. Grice's theory is only refutable by theoretical argument, for instance by claiming that the human mind is not rational after all or that communication is not goal oriented. And such claims are not themselves empirically testable.

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