[Conference presentation at the Twentieth Annual Convention of the Association for Behavior Analysis, Atlanta, Georgia, May 29th 1994]

CONCEPTUAL ANALYSIS AS THE EMPIRICAL STUDY OF LINGUISTIC CONVENTION: SOME IMPLICATIONS FOR BEHAVIOR ANALYSIS.

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Abstract

Conceptual analysis is the empirical study of the linguistic conventions to which a speaker must conform if what she says is to effectively control the behavior of any competent listener. Demonstrating the existence of a social convention requires an *ethnomethodological experiment* in which the convention is deliberately flouted so as to observe the social consequences of so doing. More common is the *thought experiment* in which the reader is asked to *imagine* the consequences of flouting the convention. In conceptual analysis the existence of a *linguistic* convention is demonstrated by constructing a sentence which flouts the convention, and inviting the reader to consider how she would react if confronted by such a sentence in the course of ordinary conversation. Conceptual analysis provides valuable insights into the way language is used to control the behavior of the listener (*pragmatics*), the way sentences are used to depict environmental reality (*semantics*) and the nature of the reality thereby depicted (*metaphysics*). It has important implications for the study of verbal behavior, for an understanding of the relation between our ordinary psychological language and the technical languages of behavior analysis and physiology, and for an understanding of such fundamental scientific concepts as 'cause' and 'effect.'

1. Conceptual Analysis and Ordinary Language Philosophy

'Conceptual analysis,' as I shall be using the term in this paper, is a method of analyzing the meanings of words in natural language invented by Ludwig Wittgenstein while he was Professor of Philosophy at the University of Cambridge during the 1930's. It was developed and popularized in the 1940's and 1950's by the so-called Oxford ordinary language philosophers of whom the most important names are Gilbert Ryle, John Austin, my own philosophy tutor, Paul Grice and, the only major figure from the movement still alive, Sir Peter Strawson, as he now is. In this paper I shall argue

(1) that conceptual analysis in this sense has an important contribution to make to the analysis of behavior in general, and

(2) that, despite its having been invented by philosophers who are usually very reluctant to soil their hands with anything remotely empirical, conceptual analysis is itself a form of behavior analysis, an empirical study of the social conventions to which a speaker must conform, if what he or she

says is to be understood by any competent interpreter of the language or code which the speaker is speaking.

I shall not say much about the first of these two theses. The reason for this is that an article by my old friend and now colleague at the University of Wales Bangor, Tim Miles, arguing the case for thinking that conceptual analysis as advocated by Ryle and Austin has an important contribution to make to the scientific analysis of behavior, has appeared in the latest number of *The Behavior Analyst* (Miles 1994). I shall therefore confine my remarks on this point to two quotations which providing contrasting, but not, as I shall argue, conflicting views of what is variously referred to as "our ordinary psychological language," "common sense psychology" or, more disparagingly, as "folk psychology."

The first quotation comes from Daniel Dennett's (1978) book *Brainstorms*, the chapter entitled ‘Skinner skinned’:

> The first step in Skinner's argument is to characterize his enemy, ‘mentalism’. He has a strong gut intuition that the *traditional* way of talking about and explaining human behavior - in ‘mentalistic’ terms of a person's beliefs, desires, ideas, hopes, fears, feelings, emotions - is somehow utterly disqualified. This way of talking, he believes, is disqualified in the sense that not only is it not science as it stands; it could not be turned into science or used in science; it is inimical to science, would *have* to be in conflict with any genuine science of human behavior. (Dennett 1978, p. 54)

While a behavior analyst may take exception to the somewhat patronising manner in which Dennett presents Skinner's position in this passage, I don't think he can be accused of seriously misrepresenting it. Nevertheless, there is no inconsistency, I believe, nor, on the evidence of his (Skinner 1945) ‘Operational analysis of psychological terms,’ would Skinner have seen any inconsistency between his repudiation of our ordinary psychological language for the purpose of constructing a science of behavior and the claim made by John Austin in my second quotation which I have borrowed from Tim Miles' recent article. It comes from one of Austin's (1961) *Philosophical Papers*, a paper entitled ‘The meaning of a word’:

> Our common stock of words embodies all the distinctions men have found worth drawing, and the connexions they have found worth marking, in the lifetimes of many generations: these surely are likely to be more numerous, more sound, since they have stood up to the long test of the survival of the fittest, and more subtle, at least in all ordinary and reasonably practical matters, than any you or I are likely to think up in our arm-chairs. (Austin, 1961, p. 182, quoted by Miles, 1994, p. 29)

But if we are to combine this claim of Austin's with Skinner's rejection of such language for the purposes of science, it is obvious that we are going to need some way of extracting from our ordinary psychological language "all the distinctions men [and women] have found worth drawing, and the connexions they have found worth marking, in the lifetimes of many generations" so that we can incorporate them into the

language of behavior analysis which is to replace it for the purposes of science. Conceptual analysis, I maintain, is the only technique that can give us the kind of understanding of how ordinary language works which will enable us to do this.

2. Conceptual Analysis as the Empirical Study of Linguistic Convention

In a well known paper entitled 'Epistemology naturalized,' Quine (1969) has argued that epistemology is a part of and not apart from the scientific enterprise as a whole. The same, so it seems to me can be said with even greater confidence of conceptual analysis. But whereas Quine thinks, in my view mistakenly, that epistemology has its scientific abode within sensory physiology, conceptual analysis is a branch of empirical sociolinguistics. But sociolinguistics, on this view, is construed, not as it currently tends to be, as peripheral to mainstream linguistics, a kind of halfway house between linguistics and sociology, but as the very heart of an empirical science of linguistics. This is a science which hardly exists today in which language is construed, not as an outgrowth of the brain's computer language, Fodor's (1975) "language of thought," but as a process of social interaction between two living organisms, a speaker and a listener. This view makes linguistics a branch of sociology, and sociology that branch of the general science of the behavior of living organisms which concerns itself with the linguistically controlled interactions of human beings.

Considered as the methodology for linguistics so conceived, conceptual analysis is the study of the social conventions to which the speaker of a particular natural language must conform, if he or she is to be understood by a listener who is a competent interpreter of that language. These conventions are not innately inscribed in the brain as Chomsky (1965) supposes. They are contingency-shaped by the selective reinforcement practices of the listener who in this respect represents the verbal community constituted by the speakers and interpreters of the language in question.

3. The Ethnomethodological Experiment in the Empirical Study of Social Convention

In an article which I published in the journal *Philosophy of the Social Sciences* in 1992, I argued that statistical studies of the incidence of a particular kind of behavior in a population tell us very little about the social norms or conventions governing that behavior. The reason for this is that such studies are unable to distinguish between a behavior with a high natural probability of occurrence where the probability is

reduced by the aversive social consequences that ensure when it is emitted and a behavior whose low natural probability of occurrence is enhanced by positive social reinforcement, combined with negative social reinforcement of the failure to produce it. Both circumstances are capable of yielding the same frequency of occurrence. It is for this reason that the only way to demonstrate the existence of a social norm or convention is to flout what you take to be the convention and see what happens. If the effect is that all hell breaks loose and powerful social sanctions are mobilized so as to bring the offender's behavior into line, you have convincing evidence that an important social convention has been violated. That, I take it, is the rationale behind Garfinkel's (1964/1967) much reviled "ethnomethodological experiment." Garfinkel's example of such experiment is one in which he asked his students

> to spend from fifteen minutes to an hour in their [own] homes imagining that they were boarders and acting out this assumption. They were instructed to conduct themselves in a circumspect and polite fashion. They were to avoid getting personal, to use formal address, to speak only when spoken to. (Garfinkel 1967:47)

Typical reactions to this behavior on the part of the student are described as follows:

> family members were stupefied. They vigorously sought to make the strange actions intelligible and to restore the situation to normal appearances. Reports were filled with accounts of astonishment, bewilderment, shock, anxiety, embarrassment, and anger, and with charges by various family members that the student was mean, inconsiderate, selfish, nasty or impolite. (Garfinkel 1967:47)

It is hardly surprising to find that this kind of *in vivo* ethnomethodological experiment has not become a widespread practice even among ethnomethodological sociologists, let alone among sociologists in general. Indeed the only other example I have come across is an experiment by Bill Verplanck (1955) which will doubtless be familiar to many of you. This experiment was conceived and conducted within the behavior analytic conceptual framework long before ethnomethodology was ever heard of. Its purpose was to demonstrate the effect of the listener's expressions of agreement on the incidence of opinion-stating utterances in ordinary conversation. It became an ethnomethodological experiment during the extinction phase when the expressions of agreement that had followed every opinion-stating utterance during the experimental phase were suddenly withheld during what was construed as the extinction phase, with results very similar to those described by Garfinkel. The effect of treating Verplanck's experiment as an *in vivo* ethnomethodological experiment are set out on Table 1.

I. Verplanck, W.S. (1955) construed as an orthodox ABA design:

A
Baseline
No reinforcement of opinions

B
Experimental
Condition
Opinions reinforced

A
Return to
Baseline
No reinforcement of opinions

II. Verplanck, W.S. (1955) construed as an ethnomethodological experiment:

Convention
(conformity unconsciously contingency-shaped by avoiding the aversive consequences of failing to do so)
Reinforce each complete sentence uttered by the speaker with an appropriate reinforcer (question-answer, request-comply, invitation-accept, news-express surprise, opinion-agree, instruction-acknowledge understanding, troubles-express sympathy, joke-laugh, greeting-greet, etc.)

Baseline 1
Contingency-shaped conformity to convention

Baseline 2
Contingency-shaped conformity to convention
Rule-governed reinforcement of opinions

Experimental Condition
Rule-governed flouting of convention to reinforce opinions by agreeing

Result
"No *S* ever gave any evidence that he was 'aware' that he was serving as a subject in an experiment, that his behavior was being deliberately manipulated and recorded, or that there was anything peculiar about the conversation. The only qualification that must be made is this: during extinction some Ss got angry at E and commented on his disagreeableness, or noted his 'lack of interest.'" (Verplanck, 1955, p. 671)

Table 1. Verplanck (1955) as an Ethnomethodological Experiment

4. The ethnomethodogical thought experiment

Much more common than ethnomethodological experiments ***in vivo*** is the practice of constructing ethnomethodological thought experiments. In this case the listener or reader is invited to imagine what would happen or to recollect from their own experience the consternation and social disapproval that is

provoked when certain social conventions are contravened. In my *Philosophy of the Social Sciences* paper, I quote an example of such a thought experiment taken from a book by my sister, the Canadian feminist sociologist Dorothy Smith (1987). She writes:

> When I take my dog for a walk in the morning, I observe a number of what we might call 'conventions.' I myself walk on the sidewalk; I do not walk on the neighbor's lawns. My dog, however, freely runs over the lawns. *My dog also, if I am not careful, may shit on a neighbor's lawn, and there are certainly some neighbors who do not like this*. (my italics) (Smith 1987: 154-5)

It is in the form of this kind of thought experiment that we find the ethnomethodological experiment in the writings of conceptual analysts. Ryle (1949:105-6) for example writes:

> it would be absurd to speak of someone having a sensation, or a feeling, on purpose; or to ask someone what he had a twinge *for*.

Evidently what Ryle is doing here is inviting the reader to experience the consternation which is provoked in his or her own case by such deviant sentences, as a way of revealing the existence and nature of the linguistic conventions they flout.

5. A Behavior Analytic Rationale for the Ethnomethodological Experiment

To someone trained in the kind of experimental methodology described by Murray Sidman (1960) in his *Tactics of Scientific Research* the ethnomethodological experiment, particularly when presented as a thought experiment, will appear strangely lacking in the kind of rigor and objectivity that they come to associate with a scientific experiment. Nevertheless, I would like to persuade you not only that the ethnomethodological experiment is the only methodology which is capable of revealing the existence and nature of the conventions on which the maintenance of the fabric of human society depends, but also that the rationale for its use and the reason why it can and does provide novel information about social institutions, such as the practice of linguistic communication, can only be understood in the light of behavior analytic principles.

The important point to notice in this connection is that the kind of behavior that is controlled by social conventions such as those to which a speaker must conform if his or her sentences are to be understood within the relevant verbal community is, in Skinner's (1966/1969/1986) terms, contingency-shaped rather than rule-governed. In other words, although we all know the conventions in the sense that

our behavior is shaped by the relevant antecedent-behavior-consequence relations, we don't ordinarily formulate or "specify," to use Skinner's term, those contingencies in the form of what he calls "a rule."

This explains three things:

1. the fact that we don't ordinarily know what the conventions are in the sense of being able to say what they are and that consequently, when they are explained to us, we have the curious sense of being told both something we hadn't realized before, but yet something that in another sense we knew all along.
2. the fact that, in order to formulate or specify the contingencies which have shaped these aspects of our social behavior, we have to rely on our own and other people's intuitions (it is no accident that Skinner's (1966/1969/1986) 'Operant analysis of problem solving' paper contains the only plausible naturalistic explanation of intuitive knowledge that has ever been given).
3. the fact that, in order to specify the normally unspecified contingencies which shape social behavior, what we have to do and what the ethnomethodological experiment allows us to do, even when it takes the form of a thought experiment, is to reveal the hidden relation between the failure to conform to a social convention and the aversive consequences of so doing, the avoidance of which negatively reinforces conformity to it.

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