

The What's New Problem Or Why Are Sociologists so Interested in Deviants?

'To illustrate, a recent Gallop Poll (Newsweek, January 25, 1971, p.55) indicated that approximately half of American college students have tried marijuana, and a large number of them take it fairly regularly. They do this at the risk of having their careers ruined and going to jail for several years. Why?

'Conventional research on the nature of marijuana intoxication tells us that the primary effects are a slight increase in heart rate, reddening of the eyes, some difficulty with memory, and small decrements in performance on complex psychomotor tasks. Would you risk going to jail to experience these?

'A young marijuana smoker who hears a scientist or physician talk about these findings as the basic nature of marijuana intoxication will simply sneer and have his anti-scientific attitude further reinforced. It is clear to him that the scientist has no real understanding of what marijuana intoxication is all about.'¹

The above quote epitomises a conflict which, in one sense, could be said to have spawned qualitative sociology. Apparently, in choosing to talk in the way they did doctors and scientists showed young people that they didn't know what marijuana was all about. Yet their descriptions were both scientific and correct. What then was wrong with their descriptions?

This is not an overly hard mystery to solve. There is a prevalent kind of folk reasoning that seems to be in back of this conflict. One way to evaluate an

account is to first ask whether the person about to give it 'knows what he is talking about.' One can use the kind of story someone tells to determine the social identity of the storyteller. This identity can then be used to assess and interpret the story. In the sense the scientists' descriptions of marijuana were 'wrong' because they were the talk of non-users—something which was exhibited by the very nature of the talk itself.

The dilemma here is by no means parochial. As sociologists we are a group of people whose business it is to describe the lives of other groups of people. Our accounts are therefore outsiders' accounts on *a priori* grounds, unless it should happen that we study ourselves. There are always ways of describing some area of social life, all of which are correct. These different descriptions convey different images of the phenomenon in question; they contain different inventories of what is present and what is absent; and they vary in what they emphasise and what they leave out. How should our choice be legitimated? In about such a question it is wise to speak of the 'authenticity' of an account rather than whether it is 'valid,' 'true,' or 'scientifically useful.' For the latter terms implicitly refer to standards of assessing accounts, and it is precisely which standards to choose that is at issue. Also the search for authenticity—as in authentic will, folk music, or paintings—tends to be concerned with social identities rather than metaphysics. It proceeds by tracing objects and activities to the people who produced them and using

¹ Charles T. Tart, 'States of Consciousness and State-Specific Sciences'. *Science*, vol. 176 (June, 1972), p. 1203.

these connections to determine the worth of the objects and activities.

There have been at least two pure paths taken by sociologists in seeking authenticity. Some have sought the legitimacy of their ways of describing people by reference to science and being scientists. Other sociologists, notably those of the qualitative school, sought the superiority of their accounts elsewhere. They sought to repair their status as members of an outside group. Both strategies are not without their precedents in the society at large. In some cases we both permit and encourage non-group members to become the authorities on a particular group and its way of life. In this connection, the experts on children are considered to be adults and experts on the mentally ill; are the mentally normal(?). Yet in other cases this convention is reversed. Blacks and many non-blacks insist that no white person can be an authority on black life, and, from a feminist perspective, only women can know what it is like to be a woman. It is clear that we are dealing with an almost political issue. How do societies and their constituent groups confer 'entitlement?' How does someone become entitled to know what (s)he is talking about—this being potentially independent of what (s)he really knows?

If one takes the view that only insiders are entitled to know, or speak about, the area of social life that they are 'inside,' then research would follow a relatively clear format:

1. Choose some phenomenon, e.g. motorcycle riding, delinquency, or heroin addiction.
2. Locate the collection of people which the society designates as the

insiders with respect to this phenomenon.

3. Either become a member of this group oneself, so that one's own reflections and knowledge can be taken as authentic, or—
4. Solicit accounts and theories about the phenomenon from group members.

So far I have merely been recounting ancient history, or perhaps recasting it a bit. This was all to lead up to a puzzle. Using the above procedure one is allowed to freely choose a target group of insiders. It might be imagined that these insiders would turn out to be ordinary people doing ordinary things. For one of qualitative sociology's strongest claims is that its methods are best for analysing everyday life and ordinary activities. Yet an astounding array of studies, theories, and findings in qualitative sociology revolve around pornography, sorcerers, transsexuals, street hustlers and every other conceivable kind of non-ordinary person and activity. In particular, deviance and deviants have become almost a substantive speciality among participant observers and phenomenologically orientated sociologists. Why? Why this choice of the unusual or bizarre?

Besides the obvious explanations—the relief of boredom, getting grants, making the best sellers list, etc.—there is one which is not very colourful but perhaps more important. Sociology's concept of knowledge is status orientated. The value of what one knows is, in part, proportional to the extent that other people do not know it. If someone studies ordinary people and situations this kind of knowledge can be hard to come by. When studying ordinary conversation or behaviour in

public places one can not find some group of ‘foreigners’ who own these phenomena and are uniquely qualified to describe their nature and significance. All of us, laymen and social scientists alike, become insiders who know, do, and talk about what the researcher is studying. Secondly, one’s analysis of the causes, consequences and implications of these activities may sound like news from nowhere for an interesting reason. Qualitative sociologists, by and large, have chosen to use the same nomenclature as those they study to report their findings, *i.e.*, the natural language. One advantage of this is that their monographs, in the very way they are written, can display access to insiders and the inside. They can employ the jargon and ways of talking of those who know what they are talking about, within their own research reports. But there is a price to pay for the narrative liberty which is granted to one by the natural language. The qualitative sociologist’s own analysis becomes directly comparable to the common sense explanations constructed by the ‘natives’ (in this case, most of us) which also use the natural language. In making such a comparison either his analysis will, or will not, make good common sense. If it does, it may come off as something:

1. everybody already knows;
2. could have been found out given a moment’s reflection or a look around the corner;
3. which is dull, obvious or trite.

In a way this is not fair since there are many stories one might tell about an event, all of which are plausible in terms of common sense. It takes research and observation to discover which of these are correct in terms of what people actually do. Yet, when written down it will not come off as a finding. On the other hand the analyst

may offer an explanation, couched in common sense language, which does not make good common sense. It may refer to matters that are new, unknown, or unnoticed by most of us. In this case (s)he has violated his precept of presenting members’ understandings of their world, having imposed foreign categories and new concepts on everyday life for the purpose of doing science, sociology, or both.

The reader might object that this is not fair. Perhaps one must talk like the natives talk when describing their world, but not when explaining it. Yet, descriptions and explanation are not that easily separable here. In numerous and interesting ways, one can not easily explain the events of everyday life differently or better than one’s fellows without simultaneously recharacterising the essential nature of those events themselves.

But the analyst’s troubles do not stop here. We may not believe him. Ironically, it may be that other experiments, numbers, his own professional ‘expertise,’ and other legitimating devices of science will authenticate a description of daily life that violates the common sense beliefs of those who live it.

In the light of all these difficulties it would seem that I have misformulated the problem facing someone who wishes to become and speak on behalf of insiders. She needs to pay close attention to the common sense understandings of two groups, not one—the group of people she will study, and the audience or consumers to whom she will present her findings. If she chooses these two groups so that their daily lives are substantially disjointed, she can have her cake and eat it too. She can describe the daily life of heroin addiction as seen and

known by the addicts themselves, and present these descriptions to sociologists. She thus neatly comes by insiders' accounts which are no news to the insiders themselves, but which have the status of findings for the group to which they are presented. In fact, in selecting what things about the life of addicts to put in her monograph this researcher may find herself implicitly drawing upon folk knowledge concerning what things are likely to be common knowledge among addicts, but not all that well known among sociologists.

Of course, there are many refinements on this idea. One can choose a target population who are a 'group' only in the sense that they are collected together in the researcher's mind. Such groups include suicide attempters, persons who become schizophrenic, and certain kinds of deviants. In no natural way do these people share a common social world. They need not be in any kind of effective communication, mutually affect one another's lives, or even be present in the same physical environments. Therefore they will probably not develop a folklore about each other's lives and the causes and consequences of these lives. By studying the individuals of such groups, one by one, the sociologist comes by knowledge which is not only news to other sociologists, but to group members as well. In fact, even when studying a group whose members are in effective communication the ethnographer can ask each individual about some topic which, for one reason or another, is not talked about within the group itself. While each individual may know what (s)he does or thinks about, with respect to such a topic, (s)he does not know what others are doing or thinking and/or what patterns, convergences, and divergences exist in the group

concerning the topic. Again, this allows the sociologist to come by knowledge which others do not possess.

The only trouble with this solution is that it bars sociologists from studying what may be the most interesting aspects of everyday life—those events, actions, and circumstances which are more or less common to us all. Of course there are those who study such phenomena. But they too, in their own way, deal with the 'what's new problem' by trying to escape from it. In choosing frameworks and vocabularies they redescribe everyday events so that they sound novel, interesting, important, new and so on. Paradoxically, sociologists become showmen and claims to knowledge become equivalent to the dramatic effect of a text. Also paradoxically, the most definitive phenomenological characteristics of everyday events get altered or destroyed in such presentations. Trivial, unimportant affairs become important, fascinating matter worthy of the attention of a professional.

Of the many rhetorical devices that accompany this transformation I will mention just one—one contributed by the discipline of ethnomethodology. In a recent paper on qualitative methods Davis distinguishes two ideal-typical strategies for understanding the life-world of others, the Martian and the convert. The former sought to estrange the researcher from those he studied and the latter sought to maximise empathy. Why then, asked Davis, do so many phenomenological sociologists and ethnomethodologists always want to see the world as Martians see it? In the light of the what's new problem it can be answered that, only through the eyes of a Martian are sociological 'findings'

observable when one studies situations common to us all. As a presentational style, one way to be a Martian is to technicalise some commonplace activity by redescribing it as a series of skilled accomplishments. So described, merely walking down the street becomes an intricate set of behaviours and recognitions which invite, if not demand, a sociological analysis. As ordinarily construed it is merely a matter of putting one foot in front of the other:

‘It can be observed that the transportation of our bodies is a commonplace feature of our everyday experience of the world. To be sure when such is not the case we have certain warrant for noting those details of our circumstances rendering bodily transportation unlikely. While we, of course, have a variety of devices to achieve our transportation—automobiles, tricycles, elevators, donkeys and so on—the body itself is regularly used for its own self-transportation. Using the body in this way can take many forms and some of these can be pointed to with readily understood glosses in our native discourse—crawling, hopping, running, cartwheeling, jumping, skipping, walking, and so on.

‘The substantive focus of this discussion shall be the phenomenon of doing walking. We use the verb ‘doing’ to underscore a conception of walking as the concerted accomplishment of members of the community involved as a matter of course in its production and recognition. We hope to indicate that these members rely upon an elaborate collection of methodic practices in the conduct of doing walking and we want to sketch out some sort of analytic technology to gain access to the details of these methodic practices. In treating this commonplace phenomenon as the problematic achievement of members, we hope to build towards a greater understanding of social phenomena as ongoing situated accomplishments. It is,

after all, these methodic practices that make the phenomenon of doing walking so utterly un-noteworthy at first glance to both lay and professional analysts alike.’²

In closing, let me speculate on my reader’s reaction. He or she may find in the ‘what’s new problem’ a somewhat folksy, definitely oversimplified, but not overly important issue. Let me caution such a reader that this issue in its general form is anything but unimportant: how does one see, know and describe everyday reality in new, interesting or scientific ways while keeping that reality phenomenologically intact as the selfsame world one has started out with?

² A. Lincoln Ryave and James N. Schenkein, ‘Notes on the Art of Walking’. In Roy Turner (ed.), *Ethnomethodology*. Baltimore, MD: Penguin Books, 1974, p. 265.