LETTERS

AMONGST COMPETENT SOCIOLOGISTS?

In a recent letter (*The American Sociologist*, November 1968) Irwin Deutscher was disquieted by Alex Carey's critique of the famous Hawthorne studies (*American Sociological Review*, June 1967, 403–416). Deutscher suggested that sociological findings are uncritically accepted within the discipline when they are in accord with moral or ideological positions and attacked when they are not, and that this explained why the methodologically weak Hawthorne studies went so long unchallenged. It is quite proper, I think, that scientific results which may have strong adverse effects on human welfare be given special scrutiny, whether it be in the field of sociology or of physics. What is equally important is that evidence be accorded respect whatever the implications. (I, for one, find it difficult to imagine a truth that mankind as a whole is better off not knowing. Those who hold an opposite view ought to consider whether it stems from their conception of truth or from their conception of mankind.)

Deutscher used the occasion to call into question the status of sociology as a science, simply because findings congenial to humanitarian preconceptions are less subject to challenge than those that are uncongenial. But isn't the status of sociology as a science based on the competence of the work done, rather than on the readiness of sociologists to challenge findings that do not accord with their intuitions, moral or scientific? Deutscher further asserts that sociologists will not be persuaded, no matter what the evidence, if findings do not accord with their moral and political preconceptions. I find this conclusion not in accord with my moral preconceptions. Specifically, he contends that Carey's criticism of the Hawthorne studies will not make any difference. Amongst competent sociologists?

What Deutscher overlooks is that there are many different sets of moral preconceptions, and criticism, whatever its motivation, may spring from many sources, thus providing countervailing opportunities. *The important thing is that scientific debates be judged competently on scientific grounds.* I therefore disagree emphatically with Deutscher's claim that "we are all ideologues rather than scientists." Furthermore, if we must push on into a completely relativistic view of knowledge, it is sufficient to point out that science has its own countervailing ideology, and this ideology its own adherents, even among sociologists.

If we are to investigate the sociological foundations of knowledge, I would prefer the phrase "sociology of ignorance" to the phrase "sociology of knowledge," because I feel something is either true or not; this would relieve truth from the relativistic burden implied by "sociology of knowledge." The whole point of linguistically distinguishing a set of activities known as "science" is to set them apart from more subjective behaviors. This distinction is an important one, and it muddies epistemological waters to have everything continuously up for grabs by reducing the acquisition of knowledge to the status of a subjective brawl. Scientific method is capable of settling scientific disputes, at least in the long run. Of course, this gives rise to a hierarchy of competence that contrasts with the equalitarianism implied by Deutscher's conception of sociologists as undifferentiated ideologues, varying only in the content, but not in the objective merit, of their ideas.

If one were to apply the sociology of "knowledge" to Deutscher's own letter, as he did to sociology, one might conclude that there is something about the discrediting of scientific method in sociology that does not displease him. Although the whole tenor of his letter is one of skepticism concerning the scientific basis of empirical research and of theory based upon that research, Deutscher displays a completely credulous attitude toward any research or criticism that casts doubt upon research itself. Lest his statements about the validity of research results be taken at face value, I would like to correct the impression he gives concerning several matters having to do with the status of research and theory.

Deutscher's assertion that "most" of the variance in certain instruments is due to an acquiescent response set is completely uninformed by sophisticated results from the other side of the controversy (see, for example, Leonard G. Rorer, "The great response-style myth," Psychological Bulletin 3 [1965]: 129–156; Alfred B. Heilbrun, Jr., "Social-learning theory, social desirability, and the MMPI," Psychological Bulletin 5 [1964]: 377–387; and, especially, R. Darrell Bock, Charles Dicken, and John Van Pelt, "Methodological implications of content-acquiescence correlation in the MMPI," Psychological Bulletin 2 [1969]: 127–139). I am aware that the last reference here postdates Deutscher's letter, but then I have not cited all the relevant literature either. Those who wish to discredit research by claiming everything is due to response set will find no comfort in a full reading of the record.

Herbert Blumer's attack on public opinion polling, cited by Deutscher, is far from "indisputable." In fact, it was ably answered by Theodore Newcomb in the same journal issue. Deutscher may lean toward Blumer in this exchange, but he has certainly given no reasons why we should do the same. The record, on the face of it, is by no stretch of imagination so clearly in favor of Blumer that Deutscher should have overlooked Newcomb's reply entirely.

The supporting studies for Robert Rosenthal's "experimenter effect," that is, that subjects' responses are biased by *conscientious* experimenters in *subtle* and *unintentional* ways, have been viewed with proper skepticism by experienced researchers for some time. The evidence they contain has just been painstakingly reviewed (Theodore Xenophon Barber and Maurice J. Silver, "Fact, fiction, and the experimenter bias effect," Psychological Bulletin Monograph, December 1968) with disastrous results for the credibility of the Rosenthal effect. Rosenthal's other major piece of work, *Pygmalion in the Classroom*, which purports to show IQ gains from students when teachers are tricked into expecting them, has just been the subject of a devastating review by Robert L. Thorndike (American Educational Research Journal [1968]: 708–711). The fate of this particular book is of special interest because of the extent it has been used by humanistically oriented intellectuals to impugn the humanity of objective scientists and educators. I understand there is still another review, due in *Contemporary Psychology*, that is also sharply critical on objective grounds. Friedman's book, mentioned by Deutscher together with

Rosenthal's, is included in the research on the experimenter bias effect reviewed by Barber and Silver.

The controversy to which Deutscher alludes between the Hodges, on the one hand, and the Taeubers and Cain, on the other, was conducted on the highest scientific level, and it is an example of a situation in which substantive relevance did lead to critical inspection. What is wrong with that?

Finally, let me point out that George Homans "need not despair" over Carey's criticism of the Hawthorne studies, not because today he can find more credible evidence over which to drape his theory, but because Carey's criticism does not touch upon any use that Homans made of the Bank Wiring Room data.

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ON ATTITUDE-BEHAVIOR CORRELATIONS

The differing conclusions drawn by Howard Ehrlich and Irwin Deutscher on attitude-behavior correlations (*The American Sociologist,* February 1969) may be in part a function of the research sources upon which each author rests his well-reasoned perspective. Ehrlich cites forty-six references and Deutscher twenty-four. Of this total there are only eight overlapping references, although both authors concentrate on studies of an interracial nature in discussing their general concern with the efficacy of attitude studies.

Aside from the possible reference citation influence upon his conclusions, the portents of Deutscher's case are consirerable since there is the suggestion to abandon attitude analyses. As he notes: "the problem of validity disappears when we have direct observation of the actual phenomenon we are attempting to approximate with our measuring instruments" (p. 40). If this view is accurate then, as LaPiere suggests in his supporting commentary (pp. 41–42), the consequences for the sociological enterprise *would be* considerable. Governmental research is not likely to continue to grow if sociologists begin taking the position that they can offer valid analyses—e.g. on the likelihood of interracial disorders—only after they occur. Consequences to support for research aside, this posture would certainly increase our prediction ratio over past performance. It would also be academically respectable, although I would think the new name of our enterprise would more appropriately be "contemporary history" rather than sociology.

Fortunately for our future growth and development, I believe that Ehrlich has the stronger case. When conceptual and methodological care is taken, attitude theory as a predictor of behavior is, I believe, valid. Merton observed two decades ago (R. M. MacIver, ed., *Discrimination and National Welfare,* 1949: 103–110) in his syntax of prejudice and discrimination that there are at least four logical prototype responses. Only two of these would appear on a standard scale such as Bogardus's (1926) or Adorno's (1944) designed to discern prejudicial attitudes. The two attitude-behavior consistency prototypes would be the "Prejudiced Discriminator" and the "Non-Prejudiced Non-Discriminator." The behavior of the other two prototypes Merton cites—the "Prejudiced Non-Discriminator" and the "Non-Prejudiced Discriminator"—would have to be explained by intervening variables, the point of Ehrlich's article. This is in fact what Robert Kahn and his associates found to be the case in their recent study, *The Chosen Few* (Ann Arbor, 1968). In a discussion of the impact of anti-Semitic attitudes upon exclusion of Jews from executive positions, the following observation is made about the data secured from the attitude interview instrument and behavior observed:

> That discrimination is directly predictable from anti-Semitic attitudes provides a misleadingly simple explanation of discrimination. It is misleading because it suggests that other explanations are of little consequence . . . data presented . . . indicated that third party pressures have an equally significant impact upon discrimination *independent* of anti-Semitism (p. 27).

The intervening variable of a "third party," such as the influence of clearly stated company hiring and promotional policy, reportedly had a measurably independent effect upon behavior of both prejudiced and non-prejudiced managers even when these policies were at odds with the managers' attitudes. Why not concentrate only on the behavior dimension in this case, as Deutscher urges? The empirical results of Kahn's study provide the reason for his and other attitude analyses. The third-party and other intervening-variable effects upon prejudiced and non-prejudiced managers were "largely additive" (p. 26). As reported in this study, third-party pressures—company policy, attitudes of superiors, peers, etc.—had an independent effect upon behavior *but* non-prejudiced managers, as measured by an attitude scale, were significantly less likely to discriminate than prejudiced managers even when third-party pressures were the same. Thus, measurable attitudes provided insights into actual behavior, even if not to the degree early attitude theorists may have assumed, e.g. Katz and Braly (1933) and Gilbert (1951).

That attitude analysis can prove to be an effective predictor of behavior patterns is evident also from one of the eight sources both Ehrlich and Deutscher cite. This source is the Ehrlich-Rinehart study (1965), which involved an examination of the Katz-Braly scale of 84 traits applied to 10 ethnic groups tested at Princeton in 1932. A careful reading of the reported original Katz-Braly project (1933) and the Ehrlich-Rinehart critique tends further to put into question Deutschers' position on the futility of attitude analysis and tends to support Ehrlich's thesis even more than he suggests. After an analysis of ethnic attitudes independent of the Katz-Braly scale, Ehrlich and Rinehart concluded that "we suspect that these scores [based upon the original scale] have been biased in the direction of displaying greater prejudice and intergroup hostility than may exist" (Social Forces 43, 1965:575). Yet, the substantive findings of the 1932 Katz-Braly study suggest a high correlation between attitudes and behavior in the years since. Katz and Braly found no overlapping traits between those selected for "Americans" (read WASP) and those selected for "Negroes." The traits identified for "Americans" were entirely of a positive nature, e.g. "industrious"—48 per cent, and "intelligent"—47 per cent. In contrast, the traits selected for "Negroes" were of a negative nature and more generally accepted, e.g. "superstitious"—84 per cent, and "lazy"—75 per cent. It does not take a chi-square test to realize that there is substantive significance to this variance of selections (a point well taken up by David Gold in the February issue of *The American Sociologist*). If Hartley (1946) and others are correct about the reliability of college-student samples, the Katz-Braly Princeton sample was reflective of college students' interracial perceptions in 1932. These students were to become the current business, professional, and societal leaders. Can there be a serious question about the general resisting behavior of this leadership to Negroes in all sectors of the society's social life?

For myself, I continue to believe that attitude analysis is a legitimate pursuit. Indeed, along with Professor John