Letters

Psychological Experiments without Subjects' Consent

The exchange between S. E. Miller and M. Rokeach (1 April, p. 15) opens an issue long overdue for examination. What are the permissible limits of covert manipulation and observation of human subjects in research?

Miller seemed shocked by the surreptitious experimentation with people who had been led to believe that they were merely taking part in a normal interview procedure connected with their applications for jobs. I wonder what he would think about the psychologist Karl Weick's descriptions of more thorough methods for obtaining unaware subjects ["Laboratory experimentation with organizations," in *Handbook of Organizations*, James G. March, Ed. (Rand McNally, Chicago, 1965), pp. 247–253]. Weick cites a number of studies in which investigators actually hired job applicants with the intent of involving them in experiments of which they would be unaware. If it is impractical for the investigator to "hire" subjects for experiments in organizational settings, the tables may be turned. W. Richard Scott reports on a variety of published studies based on the "disguised researcher method," in which the investigator is the one who is hired into a group to be observed ("Field methods in the study of organizations," *ibid.*, pp. 261–304). Scott reports that "some people believe that this approach raises serious ethical problems."

Another innovation in deceit is represented by what we might call the vectoring approach. This involves the analysis of a subject's behavior in one situation in the light of information obtained from or about the same subject in another situation. Investigators may, with this method, circumvent a subject's reluctance to divulge too much information to any one investigator at any one time.

The availability of electronic gadgetry makes covert observations of human subjects increasingly popular. The hidden tape-recorder microphone and the one-way mirror have become standard equipment in university and business research departments across the country. Many an enterprising researcher has found that the movie camera and the closed circuit TV or videotape camera can be easily concealed. Infrared photography provides special opportunities for covert recording of behavior in darkened areas. [See "Contrived observation: hidden hardware and control," in Eugene Webb *et al.*, *Unobtrusive Measures: Nonreactive Research in the Social Sciences* (Rand McNally, Chicago, 1966), pp. 142–170.] The increasing availability of psychophysiological sensing devices has even made it possible to wire-tap the human nervous system. If the researcher is concerned about spurious emotional-response readings which may arise from the subject's awareness of bulky "lie detection" equipment, he may obtain unobtrusive sensing electrodes which operate remotely from recording equipment and weigh but a few grams each. . . .

But what of the cases in which the subject's participation is voluntary? Is it all right to do anything to a human subject so long as he volunteers for the study (or "evaluation" or "training") and is given a prior briefing? Very often the situation is such that it is most embarrassing or inconvenient not to "volunteer," and the subject is often in no position to understand the real implications of the "briefing." Is it all right to subject a volunteer to conditions which may cause intense confusion or disorientation when he has no prior basis for knowing how disturbing the experience may be?

Is there any ethical problem involved when the psychologist is drawing private inferences about a subject's overt behavior? We all do this almost unconsciously in sizing up new acquaintances or friends. But do the rules change when a trained psychologist does this with preplanned leading questions and systematic observations of gestures, tones of voice, or facial expressions?

Are the subject's rights amply guaranteed if the investigator is careful not to report his individual responses? Some years ago I used an attitude-measuring device, disguised as a current-events test, to study the extent to which industrial foremen were "management-oriented" (rather than labor- or union-oriented). To reward the cooperation of employers, I reported to them the average scores of their foremen, withholding the individual foremen's scores. Did I adequately respect the rights of my subjects? I am not at all sure now that I did.

As Rokeach says, the issue is not a simple one. The success of many studies depends upon the naiveté of the subject, and many researchers, myself included, are confronted time and again with the need to balance the gain in scientific knowledge against chances of infringement upon the rights of subjects. The American Psychological Association's "Ethical Standards for Psychologists," mentioned in Rokeach's letter, is of little help on these matters. Section 4, paragraph b, remands the justifications for misleading research subjects to the psychologist's own judgment. Each year this particular crisis of conscience becomes more trying. Indeed, the pangs may be becoming so acute that we will turn away from them altogether. Important advances in the understanding of human behavior hinge upon a certain latitude for the investigator in these areas, but without some proper checks we may be well ahead of schedule in reaching Orwell's 1984.

One thing seems certain; the problem deserves an airing among scientists who can objectively examine both sides of the issue.

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I am so glad that Miller has protested against the practice, in the behavioral sciences, of experimenting on human subjects "without their permission or knowledge." I have felt distaste for this practice for many years; I wish that I had raised my voice earlier.

Rokeach tells us that he consults his

own conscience in such matters, and the consciences of some of his friends, and that he adheres to "the moral standards of his profession," as expressed in a booklet on ethical standards put out by the American Psychological Association, that is to say, by a group of scientists just as biased in favor of psychology as Rokeach himself.

Miller protests the experiments as "an invasion of fundamental human rights," and I agree with him that "one of the most fundamental aspects of a civilized culture is that the citizen may correctly assume that in ordinary day-to-day activity he will be treated with candor and dignity. . . ." I would add that the practice of deceit in science has a bad effect on the scientists who engage in it; their moral judgment has been corroded if they think that human freedom and individuality are to be weighed in a scale and balanced against anything at all—particularly when the weighing and balancing are done by psychologists themselves, who are interested parties!

I hope that we will never see government by, or relying too much on the advice of, behavioral scientists. I would not trust my civil liberties to such people.

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AAAS Election System

L. Cranberg (Letters, 8 April) objects to the defeat of a constitutional amendment under which officers of AAAS would be elected by fellows instead of by council. I take responsibility for defeating the amendment, as I was the first person and the last to protest it at the council meeting. My argument on the question centers on the restriction of voting to fellows.

Because the membership of AAAS is now so large, nomination and election to fellowship is capricious. The lists of qualifications for nomination to fellowship have become impracticable, and the organization itself circumvents them by permitting ordinary members appointed to the council to be raised automatically to fellows. Moreover, a division of the membership into aristocrats (qualified to vote) and proletarians (disenfranchised) is meaningless in the context of present-day sciences. It makes little sense for AAAS to consider a new voting procedure based on that division.

I believe that the entire concept of fellows and members should be reexamined. If this step is taken by the council, the question of voting can then also be reexamined. I do not see that voting for officers by representatives is any less democratic than direct voting. There are practical matters of cost and mechanics which also suggest that voting should be restricted to the council as it now is, but these are not in themselves germane to the issue.

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Soviet Genetics

In a recent letter to *Science* (25 February), H. J. Muller described two new Soviet journals, *Researches in Genetics*, and *Genetika. Researches in Genetics* is published by Leningrad University. Thus far only the two issues mentioned by Muller have been published. Future issues may be purchased through Universitets Knizhnii Magazin (University Book Store), Leningrad V-164.

Genetika was first published in July 1965. Six issues appeared in 1965; three issues, January through March, have appeared this year.

In the last year the government has given much attention to the development of genetics in the Soviet Union. The new Five-Year Plan for 1966–70 mentions the need for "further studies of processes, occurring in living material, of the genetic regularity of selection in microorganisms, plants, and animals, with the goal of creating new, more highly productive races of animals, and strains of plants producing a greater harvest," as well as "the exploration of the genetical problems of inherited diseases" (*Leningradskaya Pravda*, 10 April 1966).

This emphasis may be seen in the many new books now appearing on genetics and related subjects. One book of interest that has just been published is *Practical Genetics* by N. N. Meduedev, which includes an extensive review of work on *Drosophila* and mice. In addition, during the last two years many of the papers of N. I. Vavilov have been republished in five volumes (*Selected Works of N. I. Vavilov*, published by Nauka); and, according to a note in *Genetika* (1965, No. 4) the presidium of the Akademia Nauk has established the N. I. Vavilov prize of 2000 rubles to be awarded "to Soviet Scientists for outstanding work in the area of genetics, selection, and plant breeding." The prize will be given once every 3 years on the birth date of N. I. Vavilov (26 November) beginning in 1966.

The first issue of *Genetika* includes a plan developed by the presidium of the Akademia Nauk for the reorganization of all laboratories and calls for the formation of new laboratories, including a Laboratory of Population Genetics, the latter to be associated with the Zoological Institute in Leningrad.

One last matter of note is the award of a Lenin prize to N. P. Dubinin for his "works on the development of a chromosome theory of heredity and theory of mutations" (*Pravda*, 22 April 1966; the prize was actually announced earlier in the year and a lengthy article about Dubinin's work was published in *Pravda*).

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Same Old Yardstick

On rereading Greenberg's vivid account (4, 11, and 18 February) of the "rocky road to academic excellence" along which the University of Pittsburgh has traveled, I was stuck by his use of Harvard as the standard (for example, "instant Harvard"). This is justifiable for literary simplicity, but won't it cause the behavioral scientists among *Science* readers to take their pipes out of their mouths and say: "Shouldn't we question the assumption? Shouldn't we formulate it as a hypothesis and test it?"

Greenberg should undertake a series of articles to test the hypothesis, positively or negatively, that Harvard is still the measure of excellence. Think of the satisfaction among the Harvardians if he found that it is! Think of the excitement everywhere else if he found that somebody else had caught up or gone further!

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