

REFLECTIONS ON A Q DISSERTATION AND ITS OPPOSITION

Stuart H. Schwartz
Marquette University

It would have been easier to choose another methodology for my dissertation, given that a number of the faculty at my alma mater had 'grave doubts' about the validity of Q. (A Q dissertation 'is rot' was one comment.) But Q methodology was appropriate to my theoretical framework, and Q sorting seemed the perfect way quantitatively to model the value systems of newspaper reporters, editors, and photographers (Schwartz, 1978).

My introduction to the world of William Stephenson came in a graduate research course offered by Edward J. Traves, who had been schooled by Malcolm MacLean at the University of Iowa. Traves displayed a missionary zeal for Q, and I was an eager convert. Like MacLean--who told the First International Symposium on Communication Theory and Research that 'I find William Stephenson's Q methodology at least a partial solution for practically every research problem that comes into my hands' (Thayer, 1967)--I saw applications everywhere I turned. Imagine my surprise, therefore, when one member of the faculty vowed that a Q dissertation would never come out of the University, and imagine his surprise when he was di-

Operant Subjectivity, 1978(Apr), 1(3), 78-84.

rected to Brown's bibliography on Q (1968: 609) which showed that another department had produced such a dissertation twelve years earlier. Another professor said Q is a 'quick and dirty methodology, and a dissertation should be rigorous and protracted.' What does that mean? 'It's too easily and quickly done,' came the reply, 'and can only approximate reality.'

I shrugged, wondering how many, if any, of our social scientific techniques produce, and not approximate, reality. Is the attitude scale the attitude? Does the semantic differential generate semantic space, or a simulation of semantic space? Q merely models or approximates the subjective processes of the individual, and that, I explained, is part of its strength. In addition, to say that Q methodology can *never* be rigorous in application--and such was the implication--seemed to betray an ignorance of the methodology and technique, a lack of knowledge about the nature of science and research. Q has its limitations; all who use it recognize this, or should. And certainly the methodology has been abused, but what behavioral and scientific technique has not? For example, Rosenthal and Rosnow (1969) have outlined the problems of the laboratory setting, while the abuse of statistical tests of significance is decried by Bakan (1969). Abuse is human, and academics, even members of communications faculties, are human.

There are few 'pure' methodologies; to criticize Q as 'impure and unscientific,' as did a number of faculty during my proposal and dissertation defenses, seems to be a bit of methodological snobbery that communications academics can ill afford. As Bakan (1969: 94-95) noted in his perceptive volume on behavioral methodology:

There is no investigatory method which is 'pure' and which provides an absolute guarantee against the commission of error. It may indeed be, as one of my students once aptly put it, that the correlation between purity and fertil-

ity must at least be negative. If errors be committed, we look to the future for their correctives. In the meantime, and perhaps ultimately, we accept a pragmatic criterion.

In a sense, Q is as fertile as it is 'impure'. But to insist upon a methodology uncriticized, even if such a methodology exists, seems utter folly in a division of a discipline (the journalism portion of communications) criticized by one of its more prominent researchers as being particularly devoid of significant scholarly research (Tunstall, 1971: 5). Those of us interested in communication behavior need the hypotheses and theory generated by as many methodologies as possible; to concentrate instead on a search for purity in technique to the exclusion of all else--I was surprised to hear that a dissertation in my field should be '99 per cent method and one per cent thought'--is to flirt needlessly with sterility.

One term often popping to the surface of our discussions of the dissertation was 'error rate.' As it was explained, the conventional definition of error does not apply to dissertations. An error rate is divided between the copy of a dissertation (such things as the accuracy of footnotes, the reliability of the typist, the number of typos, etc.) and the methodology. The claim by at least one member of the faculty was that the error rate of Q technique was extraordinarily low. Q, whether we talk about the methodological philosophy or its application, simply cannot be trusted. In other words, Q methodology claims to provide a technique within its statistical-philosophical system for measuring subjectivity but actually does not. (Error rate, as might have been noticed, seems synonymous with validity.)

Much of some faculty criticism of Q along this line was based upon a paper delivered to the Association for Education in Journalism convention in 1974 (Freeman, 1974). The paper was designed to tho-

roughly debunk Q. As a university staff member familiar with the project told me 'The intent was to discredit a popular research method.' However, the would-be debunkers were themselves debunked by Sanders (1974) who pointed out some obvious and embarrassing flaws in the paper. Nevertheless, Freeman was used occasionally as witness to the misuse of Q in communications research, and that which has been misused, or lends itself to misuse, therefore is invalid as a methodology for dissertation research. 'It may be good for a quick journal article or two,' a non-Q type commented, 'but it lacks the rigor necessitated for pure scientific research.'

How do you answer? Do you point out that very little outside of the physical sciences is classified as 'pure' scientific research, and even the physical science philosophers have doubts about their own? Or, perhaps, note that even the 'pure' stuff to which one detractor referred--the experimental studies of the social psychologists--is under attack as an outmoded paradigm (McGuire, 1973)?

It seems to me that communications research cannot afford such arrogance. Time and again during my dissertation research I was told that a young program in a growing discipline should produce dissertations that rely on statistical tests of significance for decisions. The particular Q technique used in my work relied upon factor analysis, which has a large number of criteria for decision-making but few with the authority of the 'traditional' significance tests. Much to my amazement, one person on my committee confided, 'Let me see some tests of significance, use a few *real* statistics, and maybe I can support you.' No methodologist, he: At the bottom of his statement lies a gross misunderstanding of the nature of statistics and the purpose of statistical tests of significance. Such tests are merely *tools* for inferring causal relationships, not *rules* which automatically decide the meaningfulness of a relationship. Based upon the opposition to Q, it appeared that some of the opponents of the methodology had

forgotten the true functions of analyses of data, that of generating theory and acquiring support or non-support of the theory. This is the purpose, and statistics are merely means to an end. It is a mistake to place the responsibility for a decision about data relationships solely upon the test statistic, asking it to do things for which it was not intended. As Hays (1973: 353) admonished in his highly regarded statistics textbook, '*There is no God-given rule about when and how to make up your mind in general.*' Does a statistically meaningful result automatically establish the worth of a study? Is the academic or social significance of a project positively correlated with the strength of a statistical relationship between portions of the data? I think not. A statistically meaningful result in a research situation is *one of many* reasons for believing that some variable X is a condition for the occurrence of Y.

At one point in the proposal defense the following exchange took place between my dissertation advisor and another member of the communications faculty:

Advisor: No method is perfect. No matter what you choose there are certain things you'll just have to live with.

Faculty Member: I'm familiar with the method. Don't give me a lecture. I'm familiar with the method, and you don't use it to find out what people think because better techniques are available.

Advisor: Why do you object?

Faculty Member: Q has outlived its use. It was only experimental; you don't use it for serious studies. If you're a real methodologist, you use something else.

My advisor, his face flushed, said nothing. Had he sorted the persons in the room from 'most like' to 'most dislike,' there was little doubt as to who would have occupied the bottom place in the distribution. This faculty member, with his single-minded

emphasis on 'official' and 'correct' methodologies, had written off more than 40 years of Q-related work. A correlate assumption was this: A researcher is judged on the worth of his or her methodology according to the prevailing wisdom of the discipline, or portion of the discipline, and never mind the thought or theory generated by the project. There seemed to be a peculiar sort of mindlessness at work; the predominant methodologies of whole disciplines--anthropology, history, sociology, and political science--were dismissed in an attempt to emulate the physical scientists and clinical psychologists. And this was being done with little or no awareness of the crises of confidence in the experimental paradigm experienced by psychologists and physical scientists over the past few years (Rosenthal & Rosnow, 1969; Rosnow & Rosenthal, 1976).

In this scheme of things, Q is too messy, too liable to misinterpretation, too imprecise. Never mind that we are searching for basic structures, looking for new directions. No. Confirm what we know, tell us exactly how to measure the familiar, count that which has been counted before, and we'll sanction your study--but without Q.

However, I 'fought the good fight,' wrote a Q dissertation, and graduated. Since then I have served on the committee of another Q dissertation, taught a course which included Q methodology, and supervised a number of student projects. And now and then I think of those who opposed my dissertation on the grounds that *all* Q dissertations are invalid. I think and laugh because they, with their methodological blinders, will never know the peculiar joy and thrill of which philosopher John Dewey wrote, of 'the very act of exploration of expedition, the process of enterprising adventure into the remote' (Dewey, 1950: 54).

Stuart H. Schwartz, College of Journalism, Marquette University, 1131 West Wisconsin Avenue, Milwaukee, Wisconsin 53233

REFERENCES

- Bakan, D. *On method: Toward a reconstruction of psychological investigation*. San Francisco: Jossey-Bass, 1969.
- Brown, S.R. Bibliography on Q technique and its methodology. *Perceptual and Motor Skills*, 1968, 26, 587-613.
- Dewey, J. *Reconstruction in philosophy*. New York: Mentor, 1950.
- Freeman, L. Q-method 20 years later: Its uses and abuses in communications research. Association for Education in Journalism, San Diego, 1974.
- Hays, W.L. *Statistics for the social sciences*. 2nd ed. New York: Holt, Rinehart & Winston, 1973.
- McGuire, W.J. The yin and yang of progress in social psychology: Seven koan. *Journal of Personality and Social Psychology*, 1973, 26, 446-456.
- Rosenthal, R. & R. Rosnow (Eds.). *Artifact in behavioral research*. New York: Academic, 1969.
- Rosnow, R. & R. Rosenthal. The volunteer subject revisited. *Australian Journal of Psychology*, 1976, 28, 98-111.
- Sanders, K.P. William Stephenson: *The study of (his) behavior*. *Mass Communication Review*, 1974, 2, 9-16.
- Schwartz, S.H. A Q-study of the value structures and professionalism of 35 reporters, editors, and photographers from five daily newspapers in New York, Connecticut, and Massachusetts. Doctoral dissertation, Temple University, 1978.
- Thayer, L. (Ed.). *Communication theory and research: Proceedings of the first international symposium*. Springfield, IL: Charles C Thomas, 1967.
- Tunstall, J. *Journalists at work--specialist correspondents: Their news organizations, news sources, and competitor-colleagues*. Beverly Hills, CA: Sage, 1971.