

though not presenting very much in the way of new formulations. However, the main line of the argument in these essays deserves some comments of a more general nature.

It is a central contention of Lipset's that values play an important part in shaping social institutions as well as behavior within them. However, to move from this suggestive metaphor to more detailed substantiation of the existence of, say, "egalitarian values", is extremely difficult.

We tend to base our inferences of egalitarianism on the egalitarian practices, and the tautological danger is clear and present. Are people egalitarian because they have something inside ("values") which forbid them to treat each other differently, or do they abstain from differential treatment of others because this will not pay (and not just because other people hold this value)? It is a tricky matter indeed, and it is one which is difficult to solve by evoking evidence as macroscopic in nature and with as much sweep as Lipset does: it rather urges more penetrating analysis of concrete social situations. I would suggest a closer study of evaluation rituals, such as examination procedures and the situations in which people have to be told that their performance leaves a lot to be desired, but in any case a shift from the bird's-eye view perfected by Lipset to the more painstaking activities of the anthropologists seems a necessity.

This in turn leads us to a second problem linked to "class". While admitting that the market was a central concept both for Marx and for Weber, market conditions and changes in them are not really subjects of analysis for Lipset. The problems of the market are all too quickly allowed to change into the less interesting problem of income level: more amenable to survey research (or so some people believe) but essentially without that explanatory power inherent in a serious analysis of the different labor markets (as well as other markets). Lipset's earlier and more limited study, *Union Democracy* dealt much more effectively with the interplay between the market of printing and printers and the leeway of action of leaders in the printing unions. Obviously this type of analysis meets much greater difficulties when performed with the nation as the unit. But unless the explicit concern with markets is reinstated in political sociology, it will be one-sided indeed. "Social status" is frequently used to explain things which simple market considerations might possibly do better.

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*Readings in Reference Group Theory and Research.* By Herbert H. Hyman and Eleanor Singer (editors). London: Collier-Macmillan, 1968, xi, 509 pp. 105/—.

Some collections of articles have made important contributions to the scholarly literature in sociology. Thus, Lazarsfeld and Rosenberg's *Language of Social Research* was a major contribution to the methodology of social research. It was organized from the point of view of an explicit and articulate philosophy of evidence, and contains articles that illustrate the practice and pitfalls of that philosophy. The methodological doctrine set forth in the book is in the view of some sociologists one-sided or even unfortunate (it never, for instance, goes seriously into the process of theory construction or experimental research), but every sociologist, regardless of orientation, does well to confront or learn from the doctrine. Other readers constitute assessments of the theoretical and empirical status of an area. The editor attempts in his selections and critical introductions to identify the strengths, weaknesses and gaps in the available research and thinking in the field. Dell Hymes, for instance, performed this service for an important area in sociolinguistics through his reader *Language in Culture and Society*.

Most readers, however, are little more than haphazard collections of articles. The volume edited by Hyman and Singer belongs in this category. The introductory essay is discursive, not analytical, and the introductions to the different subsections merely tell the reader, in a vague way, what he can expect the various pieces to be about. Some of the selections appear to be concerned with reference group behavior only in a tangential way: Merton's paper on locals and cosmopolitans does show that people differ in their choices of reference groups, but is the demonstration of this truism a sufficient ground to include this previously reprinted, easily available paper? And in what sense does the study of clique formation by Festinger, Schachter and Back focus on reference groups?

Some of the papers included are methodologically weak. Thus, the Siegel and Siegel study attempted to show that attitude change depends on the norms held in groups that a person is a member of and on the norms upheld in groups the person desires to become a member of. A group that is critical in their analysis consists of women students who at the end of their freshman year expressed a desire to live in a former sorority house ("row house"), and who drew for the same type of residence at the end of their sophomore year after spending their sophomore year in a regular undergraduate dormitory. The membership groups of these

girls were supposedly *not* their reference groups. But since there is no control of social contacts introduced into the analysis, we do not know to what extent these students saw a great deal of people living in "row houses", although they themselves lived elsewhere.

A question that appears to have been on the minds of the editors concerns the extent to which reference group theory and research have been cumulative. In order to answer this question one must break it down into sub-questions. One must first address oneself to the question of *conceptual clarity*. The term "reference group behavior" has been used in connection with several different kinds of situations.

(1) In order to evaluate himself on some characteristic (e.g. ability or attitude) a person compares himself with a referent which may be another person or category of persons. Am I being adequately paid? I answer that question by comparing my earnings with those of "people like myself". Important theoretical problems are, among others, how do I decide who is "like me", and how do I react if the comparison turns out to be favorable or unfavorable to me?

(2) A person positively values membership in a group which he may or may not be a member of. If he is not a member he may be attempting to become a member, or anticipate that he will be a member at some future time. He "identifies" with the group, its norms and values. The behaviors of people who are members become "models" for his own behavior.

(3) A person interprets a sequence of behavior by putting it into the context of some social group or groups. The interpretation of, say, a political slogan depends partly on who is seen to come out and support or reject it. This third problem area seems to be the one that Shibutani has in mind in his paper "Reference Groups as Perspectives" in the volume.

We can now ask the question whether it is useful or fruitful to try to conceptualize these various phenomena under one heading, as "reference group" behavior. Problem area (1), the comparison process, is dealt with in a paper reprinted in the volume. The theory is so far incomplete and rather indeterminate, but a good start has been made. Area (2) exemplified by the findings in Newcomb's Bennington study (summarized in another paper in the book) can be dealt with using Heiderian balance theory or Newcomb's own ABX model. Area (3) would seem to require a theory of how semantic meaning gets ascribed to verbal and other behavior.

This reviewer believes that we are dealing not with a unitary process but with three separate areas of social psychological concern.

To lump them under the heading of "reference group behavior" would then tend to hamper theoretical progress. It does not help to specify different "functions" of reference groups. We should retain the term "reference group behavior" for one of the processes or dispense with it altogether.

A second question pertains to the *rigor* of the theoretical argument. Rigor has to do with the logical structure of the thinking. Can one tell what follows from "reference group theory" and what doesn't? Without a rigorous formulation research cannot be cumulative. For instance the interesting paper by Helen May Strauss seems, at least on the face of it, to contradict a prediction made from Festinger's theory of social comparisons. One would have expected a discussion of how Festinger's formulation should be changed to take the contradictory findings into account. Is it, for instance, the case that Festinger's propositions hold primarily in situations where comparison behavior is not influenced by institutional norms? To take another example, one wishes that Hyman and Singer had addressed themselves to the question of how the findings of Form and Geschwender on the effects of different comparisons on job satisfaction fit into the larger picture of "reference group theory and research".

In spite of its inadequacies the Hyman and Singer volume is useful in a limited sense. It does bring together several important articles, including a long extract from Hyman's own pioneering essay on the psychology of status, Patchen's papers on wage comparisons, an intriguing paper by Keller and Stern on group references in France and others. One misses two papers which have attempted to contribute toward serious theory construction in the area: James A. Davis' formal analysis of relative deprivation (*Sociometry*, December 1959), and B. P. Cohen's treatment of the choice of reference groups in the volume *Mathematical Methods in Small Group Processes* (edited by Criswell, Solomon and Suppes).

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*American Piety: The Nature of Religious Commitment.* By Rodney Stark and Charles Y. Glock. Berkeley and Los Angeles: University of California Press, 1968, x, 230 pp. No price indicated.

The book is the first of three volumes on "religious commitment" in the United States. The material is drawn from a questionnaire