Book Reviews

Sex in Fact and Theory

Human Sexual Behavior. Variations in the Ethnographic Spectrum. DONALD S. MARSHALL and ROBERT C. SUGGS, Eds. Basic Books, New York, 1971. xvii, 302 pp., illus. $10. Studies in Sex and Society.

The first sentence of the prologue of this book encapsulates its major weakness: "Human sexual behavior is certainly the most controversial aspect of man's fundamental activities." Emphasizing controversy rather than our ignorance gives this work a shadow far longer than its substance.

And what of the substance? It is not inconsiderable. There are six good essays about sexual behavior and attitudes: Messenger about an Irish island where sexual repression covers everything and hence sex is to be found in everything; Altschuler on the Cayapa Indians of Ecuador; Harold Schneider on the Turu of East Africa; Merriam on the Bala of the Congo; Marshall on Polynesian Mangaia, the most sexually knowledgeable people here discussed; and Rainwater on four "cultures of poverty" in the United States, Mexico, Puerto Rico, and England. There is also a valuable analysis by Suggs (who has published elsewhere on Marquesan sexuality) of the inadequacies of Linton's data and hence of Kardiner's theory about Marquesan sexuality; the article is well reasoned and documented, without venom, and convincing. So far, so good.

But then comes the "program." The editors lecture us for puritanism and neglect, and they provide the first trait list guide for fieldworkers to have been published since "Notes and Queries." But they have not provided any incentive for us to do anything about their program.

The book brings us face to face with two serious anthropological problems: first, the collection and particularly the banking of data; and second, the search for a theoretical basis for an extended study of human sexuality.

Clearly, anthropologists have never collected as much information about human sexuality as students of that subject would like (a statement that can be made about law, economics, potmaking, and the *Nilotenstellung*). But the reason is not that they are puritanical so much as that students of comparative sexual customs have not—and still have not in this book—provided the profession with any good problems. Malinowski did not do so in *Sexual Life of Savages*, published in 1929. Margaret Mead's studies of the 1920's and 1930's took their problems from our own society and illuminated them ethnographically. Forty years after Malinowski and early Mead there still aren't any good problems, except those of our own culture.

The editors point out that anthropologists, in conversation, obviously know a great deal, but do not publish it. That is correct, and for two reasons. Anthropologists, like other professed scientists, publish only those of their data that are directly relevant to the topics they analyze. But, because of the nature of the data-gathering endeavor, anthropologists are apt to know a very great deal beyond the areas they analyze. And those data are never made generally available. We badly need an organized data bank and a rewarding reason and sanctioned means to get anthropologists to put ethnographic data into it. If we had that, then the legitimate complaints of Marshall and Suggs could not be made. It is not just sex about which anthropologists know a great deal but publish little!

The theoretical difficulty is even more trying. To study human sexuality, anthropologists must study both sexual behavior and sexual experience. Sexual behavior is infracultural behavior, and ethology and the Kinsey studies have given us a fine beginning in this direction. But, like all human "animal" behavior, it cannot be behaved without being experienced. To some degree the experience is culturally communicated and even culturally formed.

The only theory that has ever begun to deal with sexual experience (as against sexual behavior) is psychoanalytic theory. Unfortunately, it has too often been rejected or embraced without being understood. Psychoanalysis primarily studies experience, and when its vocabulary is applied to behavior it becomes absurd to the uninitiated. Anthropology studies—or should study—the cultural formulations of both experience and behavior. The two disciplines badly need each other.

The courtship of psychoanalysis and anthropology is several decades old. Within psychoanalysis there is a subdiscipline called "anthropology" that goes back at least to *Totem and Tabu* (1911). But it has almost nothing to do with what anthropologists call anthropology. It utilizes ethnographic data, but not for anthropological ends. Similarly, there is within anthropology a subdiscipline, called "culture and personality" or some alias, that relates to psychoanalysis but has little or nothing to do with that discipline as it is practiced and theorized. There is a small but not insignificant number of anthropologists with full and sound psychoanalytic training, and there has been for some decades (unfortunately, the same cannot be said in reverse—few psychoanalysts have worked as hard at learning anthropology as anthropologists have worked at learning psychoanalysis). They have come to grief—or to naught—because they have asked whether the discoveries of psychoanalysis hold true among the Bongo. It is no more sensible a question than whether the principles of Marshallian economics hold true among the Bongo. The answer is yes—and no. Both psychoanalysis and economics have been developed in complex and introspective societies, to explain specific institutions and situations. Fine. But when students go beyond these particular "laboratory" situations (and any specific culture is a laboratory, not the real world), they must broaden the question. The difficulty is rampant in two or three essays of this book that suggest that the "Oedipus complex" may be the "cause" of some of the behavior reported. Obviously, it is a theoretical formulation and cannot "cause" anything. The classic Viennese Oedipus complex (and the 20th-century American one) involves at least two basic points that can be separated in other cultures: (i) the determination and acceptance by an individual of the social roles and limitations that go with his being one sex and not the other, and (ii) the capacity of the individual to deal with the social triad. In our society, these two "social skills" are intertwined, and if we learn them at all we learn them together.

Anthropologists more often get

tripped up on their own sophistication than on any other form of ethnocentrism. To hold what one knows in abeyance as one learns is difficult, but it is the anthropological task. It can be accomplished only by searching for simpler questions, questions that "what one knows" provides answers for. Psychological anthropologists are still asking questions that are too sophisticated, too highly evolved, and hence too culturally laden. They must go back to the simple queries: who produces what, who eats what, who copulates with whom and under what conditions, and what do they say about their experiences of it? Some profound thinking about sexuality, at the basic question level, would provide the platform, as it were, for a more profound anthropology. After all, look what it did for psychiatry.

In spite of some good data and interesting essays, this book does not provide enough "bait" to encourage most anthropologists; it does not provide a way in which the comparative study of human sexuality can be better organized. The subject will not get going so long as we claim that sex is controversial, are embroiled (even peripherally) in traditional psychological anthropology, and have only letterpress (and that beginning of an indexing system called the Human Relations Area Files) in which to store, and from which to struggle to retrieve, our data.

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Typology

Cluster Analysis. ROBERT C. TRYON and DANIEL E. BAILEY. McGraw-Hill, New York, 1970. xx, 348 pp., illus. $13.50.

Throughout a long academic career in psychology spent almost entirely at the University of California at Berkeley (and ended by his death in 1967), Robert C. Tryon devoted major effort to developing the methods that are documented and illustrated in this book. An incomplete and preliminary edition was his 1939 monograph of the same title. Tryon had expected that the present work would be completed in the late 1950's. Inevitably, it became dependent upon specific computer programs and thus upon specific computer hardware. Each change in computing equipment at Berkeley, from the IBM 701, 704, 7090, and 7094 to the CDC 6400, led to major software revisions (and elaborations) in the programs for cluster analysis and contributed to delay in the completion of the project.

The prototypical problem to which the methods of this book apply is as follows: Observations are made on each of n variables for each of N objects or persons. It is desired to define $K < n$ factors, whereby all variables are represented as linear combinations of the factors. Then objects (or persons) are clustered into types as a function of the profiles of their "scores" on the K factors.

The book emphasizes throughout the dependence upon computer programs, with frequent reference to the BC TRY package of programs, which has been used to produce all substantive results reported. The final 70 pages are devoted explicitly to describing the 30 component programs in BC TRY. Included are routines to compute correlations and covariances, to display scatter diagrams, to perform key-cluster analyses or principal-axes factor analyses (with varimax or quartimax rotation) of the variables, and to display graphically in spherical representation the results of the cluster or factor solution. An additional family of programs is available for the typological classification of objects or persons in terms of their scores on the several factor dimensions, where these scores are specified linear combinations of the variables. Finally programs are included for differential prediction by object type of scores on one or more predicted variables. A source of possible confusion is the authors' use of the term cluster analysis to mean both the reduction in dimensionality of the variables and the subsequent determination of distinct clusters of objects.

The methods are exemplified by their application to three separate bodies of data: the responses of 301 individuals to 24 tests of intellectual ability, the responses of 310 individuals to the 556 items of the Minnesota Multiphasic Personality Inventory, and the values that characterize 225 census tracts in San Francisco and Oakland (for both 1940 and 1950) on 33 census variables.

Unfortunately, the authors present no justification for striving to determine types of individuals in the problems that involve intellectual or personality inventories. For these examples one might expect that scores for individuals are continuously distributed, perhaps approximately normal in distribution; consequently, "sharp concentrations or lacunae may not appear, in which case the analyst may segment the configuration into any arbitrary classes of core O-types [object or person types] that are convenient for his purposes of taxonomic analysis" (p. 268). A reader wonders why two of the three substantive examples chosen to illustrate the clustering methods are of this kind, where the authors' solution may not be appreciably better than a categorization into arbitrary classes.

Application of the clustering methods to the problem of determining homogeneous social areas, distinct one from another, is considerably more convincing. The relations among neighborhoods as characterized by 33 census variables are largely "explained" by three dimensions (factors) on which neighborhoods differ: socioeconomic independence, family life, and assimilation (the proportion of white, Protestant, native-born). Twelve neighborhood types or clusters are defined on the basis of these three dimensions. Separate analyses of Oakland and San Francisco show much the same solution in terms of community type. Separate analyses of 1940 and 1950 census data also produce much the same findings, despite high rates of family mobility during the decade. Finally, the 1940 community typology is shown to be a powerful predictor of community characteristics in 1950 and of voting results both in 1947 and in 1954.

One may fault the Tryon and Bailey book in several respects: Tryon developed procedures for "clustering" variables in the 1930's, motivated primarily by a desire to avoid the heavy computation of more refined dimensional analysis procedures; that the authors persist in favoring a "key cluster" method for the computer analysis of variables today is puzzling, particularly since they include in the BC TRY system more suitable computational factoring procedures. Basic problems of assessing the goodness of fit of model to data, of determining the number of dimensions to retain, and so on, all are "left up to the investigator"; no statistical bases for such decisions are provided. No effort is made to comparatively evaluate the proposed methods against alternative approaches to clustering that have been developed in recent years; the investigator who refers to this book hoping to discover the most appropriate of the several available clustering procedures to apply to his data will be disappointed. The failure to