Culture and Personality Studies, 1918–1960: Myth and History

Robert A. LeVine
Harvard University

ABSTRACT The field known as “culture and personality studies” in the middle decades of the 20th century was a precursor of contemporary cross-cultural research on personality. Its rejection by anthropologists and sociologists after 1950 was accompanied by stereotypes that have hardened into myth and obscured its character and relevance for contemporary investigators. This article dispels some prevalent misconceptions (concerning its chronology, its theoretical unity, its positions on individual differences and its relationship to Freudian psychoanalysis) and proposes a tentative explanation of its decline.

The recognition of intimate connections between the psychology of the individual and the social and cultural properties of human groups has a long history and is much older than the institutionalization of psychology, anthropology, and sociology as separate academic disciplines about 100 years ago (Jahoda, 1993). No sooner were disciplinary boundaries established, however, than visionary theorists and researchers began to cross them in search of a more satisfactory understanding of human behavior and institutions. In the second and third decades of the 20th century, a largely American movement of anthropologists, psychologists, and psychiatrists began formulating the connections in terms of “culture and personality”; their work was soon known as “culture and personality studies,” and it was a vital part of the social science scene during the
1930s and 1940s. Its vitality at the middle of the century and its virtual abandonment thereafter is the subject of this article.

Contemporary investigators of culture and personality can hardly ignore the fact that such research was once considered an exciting frontier in the social and behavioral sciences and then fell from grace after 1950. Disciplinary boundaries have hardened since then, and mythical representations of the past have clouded the history of this field. Paradoxically, however, a wealth of newly available historical and biographical evidence provides unprecedented means of dispelling the myths, permitting us to understand the historical antecedents of contemporary research. This article is intended to help clarify what culture and personality studies were and the conditions under which they were virtually abandoned some 50 years ago.

Anthropologists who are antipsychological but otherwise diverse in their theoretical inclinations have found some unity in their rejection of the culture and personality studies that were popular before 1950. Graduate students in many anthropology departments have been brought up on cautionary tales derived from anthropology’s public dalliance with psychology and the lapses from scholarly standards that resulted. Culture and personality studies have been represented as undisciplined speculation parading as scholarship. However useful it may have been as a *bête noire*, this representation is mythical, and the discrepancy between myth and history has become increasingly obvious as relevant historical material has become available.

During World War II and in the following decade, culture and personality studies received a good deal of positive attention within the disciplines of anthropology and psychology. The American Anthropological Association elected as president between 1946 and 1949 four leading figures in culture and personality research (Ralph Linton, Ruth Benedict, Clyde Kluckhohn, and A. Irving Hallowell). The quasi-official handbook of the discipline, *Anthropology Today: An Encyclopedic Inventory*, edited by A. L. Kroeber (1953), contained five chapters on culture and personality topics. Psychology was dominated by the Hullian behaviorists (“learning theorists”), and this group (centered at Yale) was closely connected to culture and personality studies by John Dollard (Miller & Dollard, 1941), John Whiting, and Irvin Child (Whiting & Child, 1953). Personality psychology was on the rise, and the major syntheses by Gardner Murphy (1947) and David McClelland (1951) incorporated anthropological perspectives and evidence, as did *Readings in Social*

In 1948, Clyde Kluckhohn and Henry A. Murray published their widely used book of readings, Personality in Nature, Society, and Culture, which represented the richness and variety of the culture and personality field at its moment of greatest interest, not only to anthropologists but to psychiatrists and psychologists as well. The first two chapters of the book, by Kluckhohn and Murray themselves, still seem extraordinarily sane and sophisticated as an approach to the field, quite inconsistent with the image that has been propagated in the critical literature since then. For example, they begin chapter 2 with their frequently quoted dictum: “Every man is in certain respects (a) like all other men, (b) like some other men, and (c) like no other man” (1948, p. 35). They develop this theme in terms of the determinants of personality—constitutional, group-membership, role, and situational determinants. Then, they discuss the interdependence of the determinants:

“Culture and personality” is one of the fashionable slogans of contemporary social science and, by present usage, denotes a range of problems on the borderline between anthropology and sociology, on the one hand, and psychology and psychiatry, on the other. However, the phrase has unfortunate implications. A dualism is implied, whereas “culture in personality” and “personality in culture” would suggest conceptual models more in accord with the facts. Moreover, the slogan favors a dangerous simplification of the problems of personality formation. Recognition of culture as one of the determinants of personality is a great gain, but there are some indications that this theoretical advance has tended to obscure the significance of other types of determinants. “Culture and personality” is as lopsided as “biology and personality.” To avoid perpetuation of an over-emphasis upon culture, the writers have treated cultural forces as but one variety of the press to which personalities are subjected as a consequence of their membership in an organized group.

A balanced consideration of “personality in nature, society and culture” must be carried on within the framework of a complex conceptual scheme which explicitly recognizes, instead of tacitly excluding, a number of types of determinants. But it must also not
be forgotten that any classification of personality determinants is, at best, a convenient abstraction. (p. 44; italics in original)

The virtues of this sensible approach were soon overshadowed by controversy concerning the more speculative examples of culture and personality research. Eventually the field as a whole was stigmatized as unscholarly and sank in the estimation of anthropologists and other social scientists. By 1960 some of its remaining practitioners proposed changing the name of the field to “psychological anthropology” so as to avoid the stigma, and “culture and personality studies” faded from memory and became the stuff of myth.

Four misconceptions can provide reasonable starting points for clarifying the early history of culture and personality studies: (1) that the theoretical basis of culture and personality studies was developed during the 1940s and 1950s; (2) that there was a culture and personality “school” in the sense of a school of thought; (3) that culture and personality theory and research ignored individual variation in favor of an homogenized psychology of culture; and (4) that culture and personality studies were Freudian in orientation. Dispelling these misconceptions is the necessary first step toward an understanding of “culture and personality” as it was and how it might be related to current work.

1. Culture and personality theories were developed during the 1940s and 1950s. This has been asserted by Shweder (1979a, b, 1991), but it misses the actual history of the field. The major period of theory development began in 1918 with the publication of the first two volumes of *The Polish Peasant in Europe and America* by W. I. Thomas and Florian Znaniecki (1927) and ended in 1939 with Abram Kardiner’s *The Individual and His Society*. During those two decades, all the major theoretical positions in the field were laid out—not only by the students of Franz Boas (Ruth Benedict, Margaret Mead, and Edward Sapir) but also by Ralph Linton, Gregory Bateson, A. Irving Hallowell, John Dollard, and a number of others. This was an extremely active period of theory construction and publication of seminal books and articles, particularly from 1926 onwards. Developments after 1939 must be understood as resting on foundations laid during the interwar years.

The concept of interaction between culture and personality was already developed by 1918, when W. I. Thomas and Florian Znaniecki published the following statements:
Elsewhere we have outlined the standpoint that a nomothetic social science is possible only if all social becoming is viewed as the product of a continual interaction of individual consciousness and objective social reality. In this connection the human personality is both a continually producing factor and continually produced result of social evolution, and this double relation expresses itself in every elementary social fact; there can be for social science no change of social reality which is not the common effect of pre-existing social values and individual attitudes acting upon them. When viewed as a factor of social evolution the human personality is a ground of the causal explanation of social happenings; when viewed as a product of social evolution it is causally explicable by social happenings. (Thomas and Znaniecki, 1927, 2nd ed., Vol. II, p. 1831)

Here is the historical overview provided by Kluckhohn and Murray in 1948:

In 1925, Dr. Leslie A. White published an article called “Personality and Culture” in The Open Court. Dr. White was then a student of the linguist-anthropologist, Professor Edward Sapir, and in 1932–33 Professor Sapir, with Dr. John Dollard, conducted a seminar in culture and personality at Yale University for a group of fellows of the Rockefeller Foundation chosen from various European countries. . . . During the same period, Dr. W. I. Thomas, also much influenced by the ideas of Edward Sapir, was surveying this field for the Social Science Research Council. In April 1933, Dr. Thomas submitted his report, “On the Organization of a Program in the Field of Personality and Culture.” This document, which was supported by ninety-seven appendices (many of which have since been published), forms a major landmark in the growth of organized research on the relationship between individual development and the biological, social and cultural matrix in which it occurs. Thomas’s insistence upon the necessity for a multi-dimensional attack is worth recalling at a time when so many publications are set in a framework that in fact is purely biological, or social, or cultural—however much verbal hat-tipping there may be to the other dimensions. Since the early thirties, when Dr. Sapir initiated formal instruction in this field and Dr. Thomas first systematically mapped the research territory, interest has steadily grown. The editors [i.e., Kluckhohn and Murray] have examined more than a thousand pertinent articles
and discovered several hundred which it was necessary to consider very seriously for inclusion in this volume. . . . (1948, xii–xiii)

In the 1940s, the field became highly visible, not only within anthropology and the social sciences but also among the educated public. Major works on culture and personality continued to be published after that point—notably *The Lonely Crowd* (1950) by David Riesman, *Childhood and Society* (1950) by Erik Erikson, *Child Training and Personality* (1953) by John W. M. Whiting and Irvin L. Child and *Culture and Experience* (1955) by A. Irving Hallowell—but the tide of academic opinion within anthropology and the social sciences had turned decisively against the interdisciplinary field. However significant and influential these books proved to be, and however much they represented relatively mature forms of theory and research that had been developing for decades, they could not restore to the field of culture and personality studies the esteem it was rapidly losing in the course of the 1950s.

2. There was a culture and personality “school.” The field was deeply divided from the start, far more than, say, the structural-functional school of A.R. Radcliffe-Brown within anthropology or the behavior theory school of Clark R. Hull in psychology, and it had no orthodox viewpoint, centralized leadership, or coherent training program or center. It should be thought of not as a school of thought but rather as a field of inquiry in which scholars seeking to bring together psychological and psychiatric with sociological or cultural perspectives experimented in devising a variety of new theoretical models, field methods, and research programs.

Most participants in the culture and personality field would probably have agreed with the three following statements: (a) All adult behavior is “culturally patterned,” just as all adult speech is patterned by a particular language; (b) childhood experience, also culturally patterned, has a long-term influence on adult personality; (c) adult personality characteristics prevalent in a community have an influence on its culture, institutions, patterns of social change, and forms of psychopathology. This might represent the core consensus within the field, but there were major points of division about which participants felt just as strongly. Some of these were points that divided (and continue to divide) contemporary social anthropologists (i.e., materialism vs. idealism, relativism vs. universalism, reductionism vs. holism, and “humanistic” vs. “scientific” approaches). The Yale group of John Dollard and George Peter
Murdock were the most concerned with being scientific in the logical positivist mode that was becoming dominant in academic psychology, and that included materialism, a certain measure of universalism (in the sense of universal categories that permitted cross-cultural comparison), and the reductionism of stimulus-response psychology. Some of the others (Ruth Benedict, Dorothy D. Lee, and in some respects, Edward Sapir) saw themselves as humanistic, holistic, relativistic, and focused on the ideational aspects of culture. Some of the major figures like Margaret Mead, Gregory Bateson, and A. Irving Hallowell, were located between these poles, and others like Ralph Linton, Clyde Kluckhohn, and John W. M. Whiting were concerned with uniting these diverse tendencies within a single framework. But even this attempt at classification is an oversimplification of a fragmented and individualistic research community.

There were also divisions specific to the culture and personality field concerning the particular concepts of personality and of culture that were to be brought together and the forms of their integration. Two major points of controversy, regarding individual variation within a cultural group and Freudian psychoanalysis, are described below.

Thus, culture and personality studies did not constitute a school of thought or even a unified movement. If they seemed to be one, it was partly because efforts were made to bring scholars of diverse viewpoints together in seminars under the rubric of “Personality and Culture.” They influenced each other but also sharpened their differences through these meetings. More definitively, the books about culture and personality that came out in the late 1940s may have helped to create the image of a school or movement, at least for those who were outside the field. For those who participated in the field, however, the divisions were highly significant.

The culture and personality field in its first two decades, roughly 1918 to 1939, was arguably one of the most exciting intellectual explorations launched by American social science in the 20th century. Its failure was due not to a lack of sound and promising theoretical ideas, which were offered in profusion and often in contradiction with one another, but to an inability to translate the ideas into a strong, coherent, and generally accepted research program.

3. Culture and personality research ignored individual variation in favor of a homogenized psychology of culture. This is one of the most frequently repeated and widely believed charges against culture and
personality studies. However, far from being generally characteristic of the field, this was a contested issue within it, particularly after the publication of Ruth Benedict’s *Patterns of Culture* (1934) with its view that culture was personality writ large, with which Edward Sapir strongly disagreed (Darnell, 1990). In Sapir’s view, culture had to be seen as realized differently in each individual; for him, the study of culture and personality should be focused on individuals rather than customs, borrowing methods from clinical psychiatry, as argued in his article, “Why Cultural Anthropology Needs the Psychiatrist.” (See also Sapir’s *The Psychology of Culture: A Course of Lectures*, edited by Judith Irvine, 1994, particularly chapter 7.) This article was published in 1938, when A. I. Hallowell also published an article entitled, “Fear and Anxiety as Cultural and Individual Variables in a Primitive Society,” arguing that

[any comparison . . . between the fears and defenses of such individuals (neurotics) and the culturally constituted fears and institutionalized defenses of whole human societies is not only superficial, it is actually misleading. (Hallowell, 1955 [1938], p. 259)

That same year, 1938, Clyde Kluckhohn published an article on Navaho ceremonial participation in which he argued that ethnographic description of the generalized feelings and reactions of a group must be supported by data on the (quantitative) distribution of individual behavior. This was a position taken by numerous other investigators in the culture and personality field who were critical of Benedict’s position in *Patterns of Culture* (e.g., DuBois, 1944; Hanks, 1949; Linton, 1945; Kluckhohn & Murray, 1948). But Kardiner’s (1939, 1945) books on culture and personality, although written with Linton (who in other writings took a statistical view of personality distributions within a cultural group), complicated matters beyond the Benedict-Sapir debate. In the Kardiner model, cultural institutions were distinguished from individual personality, but a typological concept—the “basic personality type”—was introduced as characterizing the members of a particular culture. Culture was not personality writ large (as in Benedict’s model), but it was assumed that personality patterns were shared to a degree that those influenced by Sapir (Hallowell, Kluckhohn) found unacceptable.

Thus, although the “uniformitarian” position, as Wallace (1961) termed it, was characteristic of some in the field (Benedict and Kardiner), there were always others who maintained alternative positions in which individual differences were in sharp focus. Beginning with Sapir, they
were as sensitive to the dangers of “totalizing” and “essentializing” in ethnographic description as contemporary “critical” anthropologists are today, and they were determined to find solutions to this problem. Indeed, for Sapir, the purpose of culture and personality studies was to solve anthropology’s problem of generalized cultural description through research on individual personalities.

4. Culture and personality research was Freudian. This is often assumed in latter-day discussions of culture and personality studies (e.g., Goldschmidt & Price-Williams, 1973, iii–iv). The founders of the culture and personality field were influenced by Freud, but they were not Freudians, and most were critical of Freudian theory. Their positions ranged from the explicitly anti-Freudian social phenomenology of W. I. Thomas to the interpersonal psychiatry of Harry Stack Sullivan, who was a friend of Sapir and probably the strongest influence on the field. Ruth Benedict (1934) drew on Gestalt psychology for her configurational theory of culture. Abram Kardiner (1939, 1945) was, of course, a psychoanalyst who had been analyzed by Freud, but in his theory-building work with Linton, he simply threw out Freudian drive theory. Margaret Mead was a friend of Karen Horney, the renegade neo-Freudian analyst, and her work with Gregory Bateson (e.g., *Balinese Childhood*, 1942) shows a view of childhood experience focused on interpersonal relationships, reflecting the influence of Horney and Sullivan rather than Freud. John Dollard had been psychoanalyzed by Hanns Sachs at the Berlin Psychoanalytic Institute, but he translated Freudian concepts into the language of Hullian stimulus-response psychology in order to test them and evaluate their validity (Miller & Dollard, 1941). None of these developments was even remotely acceptable to the Freudians. Whatever the mainstream of the culture and personality field was, it was not Freudian in any sense that would be recognized as such by Freud’s followers.

This was confirmed from the Freudian side by the psychoanalysts Hartmann, Kris, and Loewenstein (1951) in a critical review of culture and personality studies, which they treated as a body of work entirely distinct from psychoanalysis. Their mildly disdainful tone and their selective citations leave no doubt that they could not assume culture and personality studies would be familiar or acceptable to the readers of a volume honoring the Freudian folklorist-anthropologist Geza Roheim. (Erik Erikson goes unmentioned, possibly because they were not certain
which side he was on.) Thus, culture and personality was not Freudian social science and was not thought to be by those in the field or by those in Freudian circles. Although Freud published *Totem and Taboo* in 1913, it was the late 1960s before anthropologists began to build a psychoanalytic anthropology as we know it today.

**CONCLUSIONS**

Culture and personality studies as they developed in the 1920s and 1930s and during their brief efflorescence after World War II, encompassed a variety of theories and approaches to the relationship between culture and the individual. The 1930s, in particular, best approximated a true “experimental moment” in this part of the social sciences, with an extraordinary, if unruly, creativity that has yet to be fully appreciated. The recognition that individual personalities were not replicas of a shared culture and required separate assessment (emphasized by Sapir and his followers) was at least as widespread among leading anthropological students of culture and personality as support for positions emphasizing shared personality characteristics (Benedict’s configurationalism and Kardiner’s basic personality type).

The central problem, as I see it, was how to translate the position launched by Sapir, and the sophisticated theoretical models it generated (Hallowell’s for example), into a program for comparative field research using acceptable methods. What methods were possible? One option was to collect life histories, and that was extensively tried, but only a few life histories could be collected in sufficient depth to permit serious analysis at the personality level, raising questions concerning the representativeness of the few persons studied. An alternative, permitting larger samples, was the use of personality tests like the Rorschach, but insofar as anthropologists engaged test experts to do “blind scoring” of the protocols, using categories devised for Western populations, they committed the fallacy of the “psychological x-ray”, that is, the assumption that a test can permit depth interpretations of underlying dynamics without knowledge of the intervening cultural contexts (LeVine, 1982; Lindzey, 1961). The problem of devising assessment methods that are both contextual and applicable to a substantial sample of persons is not insoluble, and in fact has been solved numerous times by investigators willing to relinquish the goal of assessing personality in the field at a depth approximating that of clinical psychoanalysis. But these solutions
came later (in the 1950s and 1960s), when anthropological interest in personality and its assessment had waned.

What happened to culture and personality research, which seemed to have reached its peak of interest and acceptance in the late 1940s but was already in serious decline a decade later? I do not claim to have a definitive answer, but I propose that any explanation of this decline must include the controversy surrounding *The People of Great Russia* (1949) by Geoffrey Gorer and John Rickman. The book advanced the hypothesis that Russians’ preference for authoritarian leadership under Stalin as well as the Czars could be linked to their experience of having been swaddled as infants. The “swaddling hypothesis,” as Gorer called it, aroused a storm of criticism. The book review in *American Anthropologist* stated, “Gorer’s hypothesis is too often supported by loose analogy, unwarranted assumption and not a few errors of fact” (Golden, 1952, pp. 415–416).

The Gorer and Rickman work had its origins in a study of Japanese national character conducted by Ruth Benedict for the U.S. Office of War Information during World War II in order to assess Japanese reactions to an American invasion. Because Benedict and her colleagues, including Gorer, could not do fieldwork in Japan, they used documentary evidence and the interviewing of Japanese informants available in the United States—a method that became known as “the study of culture at a distance” but which was never widely accepted by cultural anthropologists. The book about Japanese national character resulting from the study and published after the war, *The Chrysanthemum and the Sword* (Benedict, 1946), was viewed with skepticism, not only because of its lack of a basis in fieldwork but also because of its attempt to describe the culture and personality of a large complex society rather than a small face-to-face community. (More recent evaluations of the book by specialists on Japan indicate its accuracy as an account of Japanese normative ideals prevalent in the past.) As the Cold War between the Soviet Union and the Western allies began, the United States military asked Benedict to do additional studies at a distance, this time of countries in the Soviet sphere; the project was called the Research on Contemporary Cultures at Columbia University. Geoffrey Gorer and Margaret Mead were involved, and Mead took over as director when Benedict died in 1948.

Gorer collaborated with the British psychoanalyst John Rickman in using evidence from the Columbia project for a book-length psychocultural interpretation of Russian national character, *The People of Great*
Russia, published in Britain in 1949 and in the United States the following year. In it, Gorer and Rickman claimed that Russians—historically under the Czars and contemporaneously under Stalin—had a need to be controlled by an authoritarian government that was connected with (though not necessarily caused by) their being swaddled as infants. This claim occasioned what Kluckhohn (1955) later called “a blaze of controversy.” In those early days of the Cold War, the book received a substantial amount of attention, including severe criticism by social scientists and historians of the Soviet Union.

It will take more historical information than is currently available to reconstruct the atmosphere in which the swaddling hypothesis was received in anthropology and the extent to which it was involved in the decline of culture and personality studies, but some of the elements are obvious: First, Benedict and Mead, though held in ambivalent regard by other anthropologists, were the most widely read anthropological authors of their time; to most educated Americans, they were anthropology. Thus, anthropology as a discipline was bound to receive some of the ridicule (“diaperology”) heaped on the book, and it angered those anthropologists who were not psychologically oriented that they would be tarred with a brush intended for the excesses of culture and personality research (Lindholm, 2001, pp. 130–132, 141). Second, the Columbia studies were funded by the U.S. military to investigate Eastern European peoples under Communist regimes; anthropology was thus exposed as a contributor to Cold War military intelligence—a role repugnant to many anthropologists. Third, the Columbia project involved studies of culture “at a distance,” without fieldwork; only the interviewing of refugee informants living in New York was utilized to reconstruct the cultures of complex societies. For American anthropologists—who at the time were moving in the opposite direction to emulate the lengthier and more intensive overseas fieldwork of their British colleagues—this was a flimsy base for ethnographic description, even without wild psychological interpretations. The swaddling affair thus threatened the public credibility and probity of anthropological scholarship and of the culture and personality field in which Benedict and Mead were leading figures. For many anthropologists and sociologists, it confirmed what they had previously suspected, that culture and personality studies were susceptible to crude psychological reductionism and overgeneralization.

Furthermore, Mead (1953a, 1953b, 1954) vigorously defended the swaddling hypothesis, together with national character research and the
study of culture at a distance, at great length, in publications reaching all American anthropologists. Defending what others considered indefensible did not make matters better and may well have made them worse. The swaddling hypothesis came to be seen—quite inaccurately—as a prime example of what was wrong with culture and personality research in general. Many anthropologists felt that the ship of anthropology could only continue on course by jettisoning culture and personality altogether, and among those throwing it overboard were some previously enthusiastic participants. In 1960, when Francis Hsu was completing a planned handbook of culture and personality, he decided, partly on the advice of John Whiting and Anthony Wallace, to call the book *Psychological Anthropology* (Hsu, 1961) in order to avoid association with the already stigmatized “culture and personality studies.”

Thus—according to my hypothesis—the mythical stereotype of culture and personality research prevalent in anthropology for the last 40 years is based heavily on the stigmatized national character studies, the most vulnerable and easily dismissed part of a broad and diverse field of exploratory studies. The sophisticated perspectives represented by the Kluckhohn and Murray book in 1948 and by other publications afterwards could not overcome the impact of an event that left stigma permanently in its wake within the discipline of anthropology. Most of the small number of anthropologists who continued to work on culture and personality after the swaddling controversy found their primary support in interdisciplinary units of psychiatry, human development, education, and social work rather than in anthropology departments, at least until the university expansion of the middle 1960s. Without a foothold outside of anthropology, and research support from the National Institute of Mental Health, culture and personality research might have disappeared altogether. But it did survive, as “psychological anthropology,” now the official term for the unit of the American Anthropological Association that publishes the journal *Ethos*. That journal and the monographs published by Cambridge University Press for the Society for Psychological Anthropology show that, though this small community of scholars is remarkably fragmented in its interests and approaches, there is a good deal of research on problems first identified in the period prior to 1960, and much of it is based on intensive ethnographic fieldwork and/or sound linguistic and psychological investigation.

The case for culture and personality studies was strong in the 1920s and is strong now. The several disciplines involved share a common
concern with problems like psychological similarity and diversity in humans, the child’s acquisition of culture, the motivation of social conformity and change, and the relation between normal and deviant behavior. It must be acknowledged, however, that the fields of anthropology and psychology have developed in divergent directions since 1950 and have also grown enormously and divided internally during that same half century. They have a great deal to learn from each other, but the chances they will are mixed at best.

Speaking more personally, I believe that each of the fields in which I have participated, including developmental and personality psychology, psychoanalysis, and anthropology, has standards and evidence worth respecting and incorporating into a new field of psychosocial studies that speak to major problems concerning human behavior. This new field must involve a biosocial or biocultural approach, comparative analysis, the use of quantitative as well as qualitative evidence, a concern with public issues as well as theoretical problems, and a willingness on the part of its participants to move into any relevant discipline, from hermeneutics to demography, and learn it thoroughly in order to solve a significant problem. The present prospects for such a project within any one discipline are remote, and this calls for reviving the interdisciplinary collaboration of the past to realize the potential of the social sciences for understanding human behavior.

REFERENCES


