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THE LIFE OF AN ACADEMIC: A Personal Record of a Teacher, Administrator, and Anthropologist

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ABSTRACT

An account, spanning 50 years, of how I became an anthropologist, my graduate education at Columbia University, and my academic positions at Brooklyn and Queens College and at Duke University. I discuss my fieldwork among the Chippewa of Wisconsin and among modern Greeks in Boetia and Athens. I comment on the new ethnography as it applies to modern Greek studies and discuss how and why I turned to gender studies. I comment on teaching, university administration, and trends in contemporary anthropology and make a recommendation for a future thrust of the field. Reconnecting biology and cultural anthropology is, I believe, a necessary step if anthropology is to continue to be useful for ameliorating the human condition.

CHOOSING ANTHROPOLOGY

My introduction to anthropology in the fall of 1937 was a course in physical anthropology at Hunter College in New York City. I enrolled because I had met an anthropologist, Dorothy Keur, on a faculty-student social committee, admired and respected her, and followed her advice when she suggested I take physical anthropology to fulfill the college's science requirement. At the time, the second half of my freshman year, I was searching for a major that would

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help me develop a career. My parents, in particular my mother, felt strongly that a profession enabled a woman to earn her own living.

When I started college we were still in the midst of the Great Depression. To supplement my father's meager and diminishing income as a salesman, my mother had gone to work as a seamstress in the garment industry, earning very little at hand sewing. Among my parents' friends in the Hungarian Jewish immigrant community, several widows were struggling to support themselves and their children as sales clerks, milliners, or dressmakers. Other women contributed to the family income by sewing at home. They were paid by the piece and often worked far into the night after their children were in bed. I needed no convincing that marriage did not guarantee a woman lifetime financial support. It was therefore natural for me to think of college as preparation for a career. Perhaps I would teach, I thought, but I did not know what subject or at what level. In the meantime, I opted for a pre-social work major, which had few required courses, thus allowing me freedom to explore.

It was in this context that I took an anthropology course from Alice Galligan James, an excellent teacher. I became fascinated by the concept of human evolution, then taught in a laboratory largely with hominid fossil reproductions and skeletal materials. This course inspired me to take other courses in physical anthropology with Elsie Steedman, with whom we discussed racial taxonomies. I learned social organization and linguistics from Dorothy Cross Jensen, an archaeologist who, like many others, was obliged to teach outside her specialty. To supplement our understanding of culture areas and physical anthropology, several courses required visits to the American Museum of Natural History. There, armed with question sheets, we searched for answers from the museum displays. An implicit Boasian eclecticism and the four-field approach were taken for granted.

Hunter was then a women's college supported by the citizens of New York City. There was no tuition; even textbooks were free. Most of our instructors were women, and their mission was to teach. They all had done advanced graduate work, but a PhD had only recently become a requirement for a faculty position. The normal teaching load was 5 classes a semester, with a total of 125 students and no readers to help. What was striking among the anthropologists was their intellectual and emotional commitment to the discipline. They taught that all humans were worthy of respect, that racial variation in appearance and structure was only skin deep, and that all the world's languages were successful communicative and expressive devices. They believed passionately that such knowledge liberated the human spirit and made for social and political civility. Their commitment and enthusiasm was infectious; I had discovered what I wanted to teach! Their views set the stage for my lifelong conviction that anthropology was a discipline related to the real world and that its findings could help ameliorate the ills of the human condition.

I was shocked to discover that anthropology was taught only at the college or university level, for which a PhD was required. I did not know what a PhD entailed, and when I learned it meant years of graduate study, financing became a pressing problem. I applied to Columbia University, which had a fine anthropology program. Although admission was easy, the department exercised no responsibility for student financial support. With help from Hunter advisors, however, I learned of two independent foundations that helped women finance advanced studies, and I received a fellowship from one and a loan from the other. By living at home, I felt I could manage.

GRADUATE EDUCATION

In the spring of 1941, I began studying at Columbia. Ralph Linton had recently been brought in as Chair, a position that Ruth Benedict had every reason to believe would be hers. The tension between them was severe, putting us graduate students in an awkward position. Linton had published *The Study of Man*; Benedict's *Patterns of Culture* was a best-seller. Neither Linton nor Benedict ever mentioned the work of the other. Benedict was still an associate professor and was not promoted until just before she died seven years later. Among the other faculty, Harry Shapiro taught physical anthropology, George Herzog was the linguist, and William Duncan Strong, the archaeologist. Gordon Willey, then still a graduate student, taught theory of anthropology. Gene Weltfish, Ruth Bunzel, and Marian Smith (all cultural anthropologists) were there as adjunct instructors. That women anthropologists were being treated unfairly was not something I noticed fifty years ago. At least there were women teaching and doing research just as there had been at Hunter. I knew little about faculty ranks, so for me gender equity or any gender issue went unremarked.

We were encouraged to take the normal complement of four courses and to audit as many others as we could. It was very much a sink-or-swim system, geared toward a list of required readings, written and oral examinations on all four fields, and a dissertation. We developed no informal seminars for teaching each other, but our small contingent of cultural anthropology graduate students ate lunch regularly at a local diner and talked anthropology and anthropological gossip. I felt socially peripheral to the other students because I was about to be married to Harry L. Levy, a classicist who already had a PhD and was almost 14 years older than I. We went to parties and evening lectures, but we did not join in the informal social life of the other students.

At Columbia, Linton and Benedict were the most important influences on my understanding of anthropology. Linton, the only non-Boasian, incorporated and refined sociological ideas mostly in the form of Radcliffe-Brown's structural-functionalism. What endured for me was his concept of status and

role: the distinction between status as culturally expected norms in a structural position, and role as the range and variety of behavior by which individuals expressed statuses. I also absorbed some of Linton's love of world ethnography and historical diffusion studies of material culture.

Benedict focused on culture and psychology. She believed that an individual's personality and character were derived from cultural conditioning and that the sharing of that personal character led to a coherence of cultures taken as wholes. She hoped to demonstrate the rationality implicit in other cultures, even when practitioners of those cultures engaged in what, in the contemporary United States, would be bizarre behavior. The contrast between behaviors in United States culture and "Other" cultures was used to demonstrate some of the absurdity of "Us" and so enable us to develop a perspective on our own ways. That the study of the Other is inevitably a simultaneous study of Us is an idea that remains a basic tenet of my conception of the anthropological project. Benedict and Linton's teaching styles were very different from each others. Linton was a master at organized, informative lectures with touches of humor. In his teaching as in his writings, he was a synthesizer, par excellence. Benedict, by contrast, seemed to be ruminating during her lectures as she turned the prism of an idea to highlight one facet and then another. She asked herself and us questions as her lectures jerked along with stops and starts and turns from one subject to another. One did not take notes in Benedict's class. One listened, strained to follow her thought, and therefore participated actively in learning. She was the most intellectually stimulating of all the faculty.

Our anthropological heroes were Alfred Kroeber, Robert Lowie, Leonard Bloomfield, and Edward Sapir. According to Sapir, linguistics was in part psychological. Language was viewed as a distinguishing characteristic of *Homo sapiens* and a template for culture: regularity in behavior and thought often outside the awareness of speakers. Whether grammatical structures could influence thought was being debated through the Sapir-Whorf hypothesis. Nevertheless, much of what we read and were taught—historical linguistics, archaeology, physical and sociocultural anthropology—exemplified the Boasian project of reconstructing the particular histories of so-called people without history. This project led to reconstructions of human evolution and racial history and produced evidence to deconstruct the concept of race. Most important, the absence of congruence among race, language, and culture was demonstrated conclusively. At Columbia, anthropology viewed humanity as a whole, to be understood in terms of our placement in the natural world, our psychology, and our culture history.

From 1941 until 1943, when I was taking graduate courses, the United States was trying to climb out of a severe economic depression, and World War II was being fought against enemies that were trying to exterminate a people they thought were racially inferior. The broad sweep of Columbia

anthropology gave promise of a knowledge base for understanding the strengths and weaknesses of humankind. That cultural practices and beliefs needed to be studied in their own terms first and not valued or disvalued a priori (a strategic but not a moral relativity) was not only an intellectual contribution to social analysis but, I believed, also a practical guide to harmonious human relations. I was, and remain, a child of the Enlightenment.

TEACHING

In the spring of 1942, after only a year and a half of full-time course work at Columbia, I began teaching at Brooklyn College, replacing a man who obtained a war-related job in Washington, DC. In this way, World War II created opportunities for many women. I taught full schedules thereafter and studied for exams and wrote a dissertation with little supervision from Columbia professors.

At Brooklyn, we taught five three-hour classes each semester, requiring two or sometimes three separate course preparations. The department was joint with sociology, and I was expected to give courses in introductory and urban sociology. I was also asked to develop a course in what we then called “The Negro in the Old World and the New.” For the Old World I used texts based on Melville Herskovits’s work on African culture areas. Among the issues was whether contemporary African-American culture in the United States still manifested the African provenience of the original slaves or whether it was primarily a New World development. For the situation of contemporary African-Americans, I used Gunnar Myrdal’s *An American Dilemma*. A young sociologist colleague, Elizabeth Johns, was invaluable in helping develop the course. She was white, and early in our discussions I was unaware that her husband, St. Clair Drake, was African American. He was then collaborating with another African-American, Horace Cayton, on a field study of an urban African-American community in Chicago. Their book, *Black Metropolis*, a landmark work, was published in 1945. Because at that time reactions to African-American–white marriages were unpredictable, Johns kept her husband’s race a secret from the rest of the department.

The impetus for adding such a course to the curriculum in 1944 and the reasons students registered for it confound some of the expectations and assumptions of the mid-1990s. There were few African-Americans in the college, and I remember none in the first class of the new course. With Herskovits and Myrdal, the writings and ideas used included those by a Jew, a Swede, African-American sociologists, and an African-American novelist, Richard Wright, as well as materials from African-American newspapers. The 20 students were enthusiastic and spoke well of the class to their friends.

Why was there a demand in Brooklyn for such a course at that time? By 1943, news of the horror of the vicious racism in Hitler's Europe was slowly seeping through to the world. The US Armed Forces segregated white and black soldiers, schools were still segregated in the South, housing was separate, and social and economic discrimination was the practice throughout the country. African-Americans, in particular, had also suffered disproportionately from the Depression. The dilemma Myrdal articulated was that of a nation professing democracy and equality of opportunity while permitting and even encouraging discrimination and brutality toward some of its citizens. The discrepancy between ideals and behavior was becoming more noticeable to the general population. The mostly Jewish and Irish students in Brooklyn were conscious of and appalled by anti-semitism and American black racism. An anthropology course on black African historical provenience and contemporary culture and society in the western hemisphere promised some understanding of the genesis and nature of current human traumas. Blacks and whites participated in the scholarly enterprise.

In 1944, on Linton's recommendation, I was offered an instructorship at Wellesley College. My husband was in the army, and it seemed like a good opportunity to experience an environment outside of New York City. The two years I spent at Wellesley, a private women's college in a lovely suburban setting with mostly upper-middle-class white students, a student population quite different from Brooklyn's, were enormously enriching. Florence Kluckhohn was in the department, and I helped her with sociology classes. I also studied for my oral and written PhD exams during the summers and took them in 1945.

When the war ended I needed to return to New York City, where my husband was a professor at Hunter College. In 1946 I obtained a regular instructorship at Brooklyn College, where I stayed for a year before leaving to become an instructor at Queens College. I had coveted the post at Queens because Hortense Powdermaker was on the faculty. I admired her work on "Negro" culture and society in Mississippi and was looking forward to being her colleague. Powdermaker's influence helped to strengthen our then small cohort of anthropologists. She helped us to maintain a strong sense of professionalism as anthropological scholars in an undergraduate environment. Through all this time I continued writing my dissertation, mostly during summers; I received my PhD in 1950.

I stayed at Queens for 27 years. During that time, Powdermaker, Mariam Slater, and I assembled a department of predominantly cultural anthropologists, with some archaeologists, physical anthropologists, and linguists. The teaching load gradually diminished from five to three classes a semester. Soon after the unification of New York's municipal colleges into the City University of New York (CUNY) in 1969–1970, a PhD program in anthropology was

approved. Classes were held at the Graduate Center in Manhattan, taught by faculty from all the municipal colleges.

The years at Queens set the pattern for my professional life; teaching undergraduates as the base, with an accompanying alternation between administrative tasks and research. Beginning in the 1960s, I held some administrative post almost continually. I chaired first the joint anthropology-sociology department and then the new anthropology department before becoming Executive Officer of the CUNY PhD program. In 1973, just after my husband had retired and we were free to leave New York, I retired from Queens to join and chair the newly independent anthropology department at Duke University, at that time a four-field department. Eventually I served as Dean of Arts and Sciences at Trinity College, the undergraduate arts and sciences college at Duke, for five and a half years before I retired in 1986. My husband, who had been a staunch supporter and helper in my career, died in 1981. The support of his children and grandchildren, my friends, and the rigors of a deanship sustained me after the loss. I remarried in 1990.

REFLECTIONS ON TEACHING

I have taught anthropology for almost 50 years. At Brooklyn and Queens Colleges, the undergraduates were bright, articulate, and hardworking. They were mostly Irish, Italian, and Jewish, often the children of immigrants and, like me, the first generation in their families to go to college. Many of them worked to support themselves but still completed a heavy load of readings, including full-scale monographs. They even challenged their teachers. Class discussions would send me to the library to search for more information or to verify materials a student had questioned. Anthropological ideas were meaningful to these students, who were eager for the perspective on themselves that the study of different people and cultures in different times and places could give. In contrast, the Wellesley women in the 1940s and the Duke students in the 1970s and 1980s were not predominantly ethnic nor the children of immigrants. They were highly selected and were striving not to surpass the achievements of their parents but to prepare themselves to reach the same levels.

In the earlier years, I thought I understood my students' interests and ambitions and was familiar with the main outlines of young adult culture. It made possible teaching of anthropological concepts by analogies with their own experiences. As one decade succeeded another, I saw a shift in student assumptions (folk theories, really) about what they believed governed and determined their life chances. In the 1940s it was economic power in the hands of a few (the capitalist system) that they thought limited all their options; individuals were powerless. The shift to belief in the individual's ability to control his or her destiny began in the 1950s. At first it was by individuals

conforming to cultural expectations; by the 1960s it was by not conforming to expectations and instead by building new contexts for individual independence from social and cultural constraints. By the 1970s and 1980s it became the belief that individual work and striving within the system were all that was necessary to gain material success without regard to social, cultural, economic, and political context and constraints. In the 1980s and 1990s, ethnicity has emerged as a strong base for identity. In the United States, African-Americans, Hispanics, Asians, and Native Americans strive for better opportunities and civility of treatment. Their stance is like that of the young adults of the 1940s. They think that economic, social, and political forces constrain opportunity so that the individual is relatively helpless. Striving and hard work will not by themselves, they believe, further their careers, and unified, ethnically based political pressure is the answer. In the meantime, the students whose progenitors are, for example, white British, Irish, Italian, Greek, Scandinavian, German, or Jewish see themselves as less ethnic than their grandparents, for whom ethnicity may have been a larger issue. Their elders' battles for power and acceptance are forgotten or thought to be over. For these students, the patterns of the 1970s and 1980s prevail. Individual effort is proclaimed as paramount. They think social context is irrelevant for others, just as they mistakenly believe it is for themselves. These different stances make for conflicts among students and faculty, many of whom divide themselves in the same way.

The consequences of these shifts for my teaching were that by the late 1960s and throughout the 1970s I grew less able to know enough about my students to devise courses and materials that would be intellectually engaging and that would offer a perspective on their folk theories. As the gap in our ages widened, I understood less and less about their artistic tastes, their politics, their social and sexual culture, or even their standards of integrity and responsibility. I still, however, enjoyed young people enough to try teaching in a different setting. After a year at the National Humanities Center and subsequent retirement from Duke, I accepted an invitation to teach for the anthropology department at Princeton and taught graduate and undergraduate students there intermittently for four years. It was a fine experience because I had time to explore student opinions and values anew. The gap in age was so obvious that the need for student and teacher alike to discover each other's suppositions was self-evident, resulting in a fruitful exchange.

UNIVERSITY ADMINISTRATION

Obviously I like administration, or I would not have done it for as long as I did. As a department chair at Queens, the City University, and Duke, I came to each post at a time of change and growth, with a need for restructuring. We were constantly recruiting, which enabled us to monitor the intellectual cur-

rents of anthropology as they ebbed and flowed, and to look for fruitful new directions.

The deanship at Duke was a new institutional arrangement, combining the deanship of students at Trinity College with the deanship of the faculty of arts and sciences. (The two deanships are now separate once more.) In 1980, Duke, a private university, had less developed faculty governance than I had been used to at a public university. Merit-based salaries were negotiated every year. The basketful of vice presidents, chancellors, and provosts was very small. Fiscal management was sound. These combined conditions gave administrators flexibility that made it possible to achieve goals in a reasonable time.

My colleagues and I added faculty and supported promising departments and units such as the Marine Lab, the Primate Center, and the Art Museum. We encouraged appropriate and fair standards for promotion and tenure. There were so few women and minorities on the faculty that my advent as chair of a department had caused shock waves. I directed and encouraged departments to include women and minorities in their searches. To attract fine faculty we worked hard on creative arrangements to accommodate two careers, more of a necessity in the early 1980s than it had been earlier. For students, the Associate Dean of Trinity and I tried to improve advising and teaching and to improve the feasibility of study abroad.

Between 1974 and 1982 my horizons were broadened by election to the presidency of the American Anthropological Association and by appointment to the National Science Board, which oversees the activities of the National Science Foundation. The Association then as now was trying to find a proper niche for the *American Anthropologist*, which was dedicated to the four fields and was trying, unsuccessfully, to publish articles of general rather than specialized interest. Then as now we were also thinking about the responsibilities of anthropologists to the general public. In 1974 anthropologists still needed to be persuaded that research on homosexuality was a legitimate anthropological subject, and efforts to have the Association endorse that position were under way. On the National Science Board, I became familiar with all the current issues in science research and discovered how pervasively politics influenced science policy and funding.

REFLECTIONS ON UNIVERSITY ADMINISTRATION

In each post, I discovered new facets of my own personality, in particular how I reacted to stress, uncertainty, and ambiguity. I also verified my anthropological expectation that scholars in different disciplines would have different cultures needing various kinds of care and attention. I was most impressed, however, by the force of academic politics. Value in academic communities is measured in reputation and prestige, both of which have to be accorded by

others. Academics rarely believe in their heart of hearts that they are doing great work. Nor can colleagues always tell. Disciplines change and what was once considered a great contribution or approach becomes obsolete. Within the university, wherever we think we stand in this amorphous reputation ranking, all of us are highly sensitive to manifestations of where our reference groups are placing us. Place is tested by signs and symbols like space, equipment, and student quality. Rank and salary are important in themselves but also as signs of status. With the tensions and frustrations of scholarly writing, teaching, trying for tenure and promotion, and the competition for research funds, it is clear that the levels of sensitivity are very high indeed. This atmosphere is conducive to political maneuvering. The entire game is played by all sides in the language of high academic principles. The well-known saying that academic politics are the more vicious because the material stakes are so small is wrong. Indeed, the stakes in reputation and esteem are very high and they are, along with basic curiosity, the significant driving forces for a faculty's dedication to teaching and new knowledge.

The satisfactions of administration have an ephemeral quality. My nine years of retirement from Duke and over twenty year's distance from Queens have enabled me to see middle-range consequences of decisions made in my active years. Many of these decisions have had important long-lasting consequences; some have faded without a trace. Nor do I know whether or how being a woman influenced my decisions or efficacy. I never had overt manifestations of any problem and always acted as if my gender was not a consideration.

FIELDWORK AND RESEARCH: ANTHROPOLOGY AS A CHANGING DISCIPLINE

The Chippewa of Wisconsin

As part of my graduate education, I spent the summer of 1941 among the Pomo Indians in Ukiah, California, at a field school run by Burt and Ethel Aginsky and at which Elizabeth Colson was among the student instructors. In 1942, Linton, who had become interested in culture and personality, suggested a summer in the field among the Chippewa Indians of Wisconsin. He was intrigued by Ruth Landes's depiction of Chippewa extreme individualism as constituting "atomism" and sent me to find more information. The Milwaukee Public Museum had sent Joseph Casagrande and Bob Riltzenthaller to northern Wisconsin the previous year; Bob and his wife, Pat, were to be there with me. I took some life histories of women and inquired about the contemporary political and social structure on the Courtes Oreilles Reservation. The next summer my husband and I went alone to continue my investigations. He had

some interest in linguistics and had been pleased to accept Leonard Bloomfield's invitation to collect texts for Bloomfield's major work on Algonquian grammar.

My Chippewa fieldwork was undertaken in the context of culture and personality studies, with culture defined vaguely as a coherent system of practices and beliefs. Individual personalities would be expected to be congruent with the culture and even might have a determining influence on practice. Whether atomism existed and, if so, what could account for it, was the problem. On my return to Columbia, I gave a series of lectures on Chippewa culture and society for the seminars Linton and Abram Kardiner were conducting to test Kardiner's ideas. He believed that individuals, by virtue of adapting to the same primary care institutions (kinship, child care, sexuality, and subsistence), developed the same basic personality structure, and that as institutions changed so would basic personality. A. Irving Hallowell of the University of Pennsylvania, a specialist on the related Sauteaux, attended the lectures and always made insightful comments. I thought Kardiner overemphasized the influence of childhood settings for adult personalities. I believed, perhaps because I had lived through changes in setting and institutions, that explanations for personality or national character, as it came to be called during wartime studies of other peoples, had to start with understanding ecological, economic, political, and social conditions. Then one could ask how these conditions might be congruent with adult individual character. These conditions were not in my view necessarily determining but, as a research strategy, had to be understood before sense could be made of childhood experiences. (Totalizing, undifferentiated categories such as Chippewa or US character and personality were based on inadequate research and soon became obsolete. These categories are being revived in the 1980s and 1990s in the guise of multiculturalism.)

The Chippewa fieldwork inspired my doctoral dissertation. The subject of leadership on the reservation in what I had also concluded was an atomistic and thus an acephalous society had interested me. I did a historical study (later to be labeled ethnohistory) on the patterns of leadership among the Chippewa, primarily in their relations with Europeans from the first arrival of the Jesuits in the seventeenth century to 1948, when I finished reading the files in the Bureau of Indian Affairs archives. I tried to demonstrate that styles of human relationships were at least congruent with ecological settings and the economic and social relations associated with subsistence. In the Chippewa case, their economic and political history led them from one short-term situation to another; none of these situations were conducive to strong hierarchal leadership or bureaucratic organization. The tenacity of atomism was a consequence of interaction with a situation and not an immutable trait of Chippewa character.

Linton had left Columbia for Yale University, and Julian Steward became my supervisor. Although my work was congenial to Steward's cultural ecology position, he and I never discussed my thesis. I turned to Hallowell for advice. My dissertation, completed in 1950, was not published because at that time I would have had to pay personally for its publication in the *American Ethnological Society* series, and I did not have the funds to do so. In 1956, an article I wrote embodying its major themes appeared in the *American Anthropologist*.

In the late 1940s I had tried to publish a paper that purported to show that the economic and political situation on the reservation and the poverty of its population was a consequence of local conditions both inside and outside the reservation and not of some inevitable persistence of earlier culture. I sent the paper to the *American Anthropologist*. It was turned down for reasons I do not now recall. I mention the paper because I had neither the sophistication nor the self-confidence to argue with as eminent an editor as Melville Herskovits, nor did I have the confidence to send it elsewhere.

In the 1950s and 1960s, however, the Chippewa and other hunting Native Americans of northeastern North America became significant sources of evidence for Marxist-inspired debates about the influence of colonialism on a postulated pristine state of communal hunting territories. Individualism and individual hunting territories were thought to be consequences of the fur trade's requirements for individual ownership. Whichever side may have won this debate, its importance lies in the relatively early recognition by American anthropologists of European impingement on the ways of indigenous peoples and in warning against assumptions that early observers were seeing and describing some undisturbed and unchanging traditional culture. That particular issue is still very much with us—witness the debates on the !Kung.

Greece and Europe

Having been granted tenure at Queens in 1954, I became free to apply for fieldwork funds. I wanted to study post-World War II changes in rural communities. If my husband and I were to go into the field together (we did not even consider spending a year apart), we had to find a setting appropriate for him. Italy and Greece had the advantages of interesting a classicist but also created an opportunity for me both to pursue my project and to use British and American sociocultural anthropological approaches in Europe. We settled on Greece because no such research had been published on modern Greece (I did not know that John Campbell was in the field at the time) and competition for Fulbright funds was less keen for Greece than for Italy.

The choice of a European site had some of the quality of finding an undiscovered people. In all the vast scholarship on Europe, studies with an anthropological eye were largely lacking. (Conrad Arensberg and Solon Kim-

ball's work on Ireland was a prominent exception.) I wanted to do a community study inspired by Robert Redfield's concept of the "little community," although I was not convinced of his sharp contrast between urban and folk culture. The effects of postwar change in Greece also required attention to national influences on farm life.

In addition to much written history primarily on ancient Greece and Byzantium, Greek nationals and other Europeans had done physical anthropology and cultural studies in folklore. The latter were aimed at showing historical continuity between ancient and modern Greeks, usually as part of an effort to place Greek identity and polity in Christian European culture rather than in the Islamic Middle East. It was part of a strategy, not always conscious, to gain political protection and help from European nations. Greek rural populations were romanticized as morally better than their urban counterparts. For my interests in change I wanted to find a rural community at least geographically within easy reach of urban and national influences. Before leaving for Greece, I had some excellent advice from Margaret Mead and Dorothy Demetrocou-poulou Lee. After arriving in Athens, with the help of some classicists and the librarian of the Gennadion, we found a suitable farming village in the Boeotian plain. With the consent of the nomarch (the main government official in Boeotia) and the community leaders, we settled into the village of Vasilika.

Because there was little pertinent local or national history, for the fieldwork among 200 cotton and tobacco farmers it seemed best to try to discover the economic, political, and social organization of the village and its culture as it existed in 1955–1956. I limited myself to the history the villagers thought relevant to their present situation.

Once in the village, we were surprised by an unexpected kind of culture shock. The shock was a sense of disorientation because distinguishing what was Other about the villagers was so difficult. To be sure the language was different, but Caucasian faces, clothes, houses, churches, fields, farm animals, crops, and similar features were all within the range of rural settings with which I was familiar. Even the absence of running water, electricity, and indoor plumbing was not alien to my experience. Nor were the verbal eloquence and dramatic emotional reaction to even trivial situations so unusual for a man and woman who grew up in the Bronx, New York. Yet in those early days we were disoriented by the way in which the familiar features were combined and recombined. We were doing fieldwork in "our" European culture and yet we were not. Distance between anthropologist and people was perforce abandoned in favor of a dialogue in which we discussed their lives and they questioned us about ours. In any case, my village monograph stressed those aspects of village life to which I was led by the villagers' own preoccupations: family, and economic advancement of the family through dowry, education, and the acquisition of urban housing and jobs. Religion appeared

instrumental and practical, not infused by searches for spiritual or moral fulfillment. I also did fieldwork in the 1960s on the villagers who had moved to Athens in search of education, husbands, and jobs. They were successful because the Greek economy was expanding, and through their contact with urban relatives, the villagers' behavior and values had become essentially urban.

The anthropological writings of the 1980s and 1990s have stressed the importance of reflexivity in doing fieldwork, and through that they have highlighted the difficulties of writing objective ethnographies. There is even despair at the possibility of writing ethnography at all. In my view, ruminations about the new ethnography have been an effective antidote to expectations of some pristine scientific objectivity and have helped usher anthropology into the postmodern world. But those of us trained in the anthropology of the 1940s did not expect pure objectivity in what we read and wrote. If nothing else, we knew that the theoretical suppositions that underlined the problems and places we chose to study influenced the outcome. Before I went into the field, Robert Redfield in 1930 and Oscar Lewis in 1951 had published two different interpretations of Tepoztlan in Mexico. Some of the difference could be attributed to changes in the community over time, but Redfield's rural nostalgia and Radcliffe-Brownian stance, and Lewis's Marxist position surely influenced what each observed and wrote about.

The ethnography of modern Greece provides illustrations of the imprint of the observer on what is observed and on what is stressed in the panoply of possibilities. I have already mentioned my political economy, pragmatic view, and my sense of exchanging information with nonexotic people. British anthropologists who worked in Greece seemed able, I believe, to maintain a more distanced view. The Mediterranean was for them, as visitors and tourists, a place to enjoy escape from the rigidities and formalities of British society and to wonder at what seemed to them like excessive exuberance or tragedy. The Greeks were felt by them to be different.

But any postmodernist would ask whether I saw the villagers' preoccupations or my own. Was my underplaying of religion a consequence of the absence of formal religious ritual in my own life? Were my underplaying the limits on women's lives and overestimating their strength reflections of my blindness in the 1950s to limitations placed on women in the United States? Some British-trained anthropologists, coming from a more delineated class society and a strong patronage system in their academic communities, found competition for rank intriguing and so foregrounded honor, shame, and patronage among the Greeks they studied. The patterns are there, but observers with different sensibilities do not highlight them in the same way. Some American women anthropologists studying Greece have put less stress on the phenomenon: They recognize honor and shame but see it in lesser intensities. There is

evidence that in prosperous communities, people compete more in terms of economic standing than in honor. In any case, there is great variation in the behavior associated with honor and shame among Greeks and in the Mediterranean as a whole.

Since the 1950s, when John Campbell and I were in Greece, and after the publication of our books in the early 1960s, cultural and social anthropological studies of Greece have burgeoned as have those of other European countries. The Greek work has been excellent overall, and some of it has been truly distinguished. Fieldwork has been conducted by Greek nationals and overseas Greeks; as well as by Australians; French, Dutch, and other European nationals; and more British and Americans. Greek studies, now embedded in Mediterranean studies, have participated in the revival of early twentieth-century concerns with the definition of and heuristic value of culture areas. The questions asked and the approaches taken have reflected developments in anthropological theory and topical interests. Structuralism, political economy, semiotics and symbolism, feminist theory, and cultural studies of literature and dance have also contributed. Folklore has had a renaissance. Scholars are no longer in search of antiquity but see texts of poetry and song as contemporary performance. The fieldwork sites have been urban and rural. Has each of these lenses produced different images of Greek nationals? Yes, but postmodernism or not, it remains possible to judge some fieldwork and some analyses as better grounded in data and more sophisticated than others. Furthermore, different facets of Greek culture and society are visible in each picture and contribute to a composite portrait of the Greeks, albeit more like Picasso than Whistler. And this is despite recognition that a search for an essence of "Greekness" is futile. I have since turned to other research, but I continued a peripheral interest in Greek studies, most recently by editing *The Modern Greek Studies Journal* for five years until 1990.

Gender

In 1966, in response to an invitation to participate in a symposium on women, I examined my field notes and wrote a paper substantiating my view that the Greek village women had more power and influence than the appearances of deference and spatial segregation would suggest. The paper was published in 1967. But even then I had no particular interest in gender issues until several years later when I served on the American Anthropological Association Committee on the Status of Women in Anthropology. To my astonishment, I learned that on average, women anthropologists were no more successful at getting academic positions than were women in other disciplines, in spite of visible stars such as Margaret Mead and Ruth Benedict. More important, women PhDs from the 14 oldest anthropology departments in the country were almost never appointed to the faculty of those universities but

rather were clustered in the small liberal arts colleges and state universities with fewer research opportunities. The men's rewards for receiving the same quality of education were superior to women's, as measured by appointment in institutions where intellectual excitement and opportunities for future research were more accessible and teaching loads were lower. Given the criteria for prestige and reputation in anthropology, women had fewer professional opportunities to achieve them than did men. Concerns about civil and fair treatment for all racial and ethnic groups led me into anthropology; now concern for the fair and civil treatment of women led me into gender studies.

In the early 1970s, for George and Louise Spindler's series, I undertook to write about what anthropologists knew about women. There was an appalling lack of information on women or gender hierarchies in the literature, although papers on women were being read at meetings. A collection had appeared in a 1974 landmark book edited by Michelle Rosaldo and Louise Lamphere. These papers were significant case studies of gender roles in specific societies and from different points of view. As a result of my scholarly roots in evolutionary theory and a sort of not totally Marxist materialism, I thought it useful to organize what we knew into an evolutionary sequence of hunting and gathering (foraging) and horticultural societies, and to ask the same questions for each type so as to make eventual comparison possible. (I had expected to add peasant and industrial societies, but space and time were insufficient.) Issues at that time were whether male dominance was universal and how gender roles were culturally constructed.

I also searched for necessary conditions that might influence the degrees of dominance men held over women, ranging from severe oppression to close-to-equal status. Among the foraging and horticultural societies, I found two conditions: the degree to which the gender division of labor and custom enabled women to control goods and services they could distribute outside of the domestic unit, and the structure of affinal relations. The former enabled women to gain power over those to whom they gave, because reciprocity or exchange in the larger polity had to ensue, and the latter regulated the degree of control a woman's own kin could exert on her behalf. The research also led me to suspect that women's activities were neither limited by nor entirely structured by the needs of pregnancy and child care, but that systems of child care accommodated the other tasks culturally expected of women.

Published in 1975, *Women and Men* exemplified my conviction that behavior on the ground—what people actually do or practice—is an essential beginning for investigations. Since the mid-1970s, anthropological and feminist theories have recognized the limitations inherent in thinking about men and women as essential, eternal types by virtue of basic biological differences. The variations in race, class, culture, and historical circumstance that differentiate men among men, women among women, and men and women from each other

make simple studies of so-called women's place obsolete. The political economy of gender is, in my view, still where the main thrust should be found, but the value of studying the richness of symbol, ritual, and literary texts as a source of nuanced understandings of gender and gender relations has become obvious. In the meantime, feminism and feminist theory, the fuel for gender studies, have become more complex. Nevertheless, once we have exhaustive knowledge of the variations, it will be time to examine what we know in search of patterns and regularities that can illuminate gender relations wherever they may be.

SOME OBSERVATIONS

The anthropology rooted in evolutionary theory, with the ultimate aim of comparison and generalization (the scientific stance), is still producing excellent research. Some gender and ecological problems are still suited to the application of techniques and theory from the United States's traditional four fields. On the humanist side, many anthropologists have been studying particular peoples and their symbolic structures of ritual and thought as ends in themselves. The trend has been noticeable among those who limit the concept of culture to structures of meaning in particular contexts. Ethnic and political groups are understood in terms of self-declared definitions of identity that have cultural meaning, but interestingly, anthropologists have made less effort at sophisticated psychological understanding of those identities. The meanings of commodities and commodification, and of texts in popular culture occupy us more than the phenomena underlying their development. Rubrics such as late capitalism, hegemony, and consumerism often suffice as explanation without adequate understanding of process.

By relegating the concept of culture to meaning and semantics, too many anthropologists have shifted attention from issues of people in their world to issues of people in their heads. Too often the worlds are not seen as connected to each other; both views are needed for a complete anthropology. The view of people in their heads permits a period of escape from anthropologists' despair at the state of the world. It is an escape into aesthetics and wonderfully nuanced analyses of ideas as a response to the inadequacy of twentieth-century political and social theory to account for contemporary history. The end of European colonialism by political control has been replaced by European business and economic control. The limitations of both political and economic resources in the old colonial areas are glaringly evident, and ethnic conflicts have been the unexpected result.

A concept of culture limited to meanings, designs, templates, and the like was ripe for the picking by literary scholars and other humanists who developed it with enthusiasm. When ethnic differences (which slip easily into racial

differences) began to be couched in terms of cultural differences, and understanding diverse peoples was phrased as multiculturalism, we anthropologists were surprised, distressed, and even occasionally pleased that the entire discourse was conducted with little contribution from us. By having torn the concept of culture from its roots as a pan-human condition to be understood in terms of practice and behavior situated in place and time, as well as in thought and meaning, our work resembled that of the literary world so closely that there appeared to be no difference. One can see why literary culture theorists thought that they knew all there was to know about culture.

Not all anthropologists have followed these trends. Those whose stance is in political economy have profited greatly from the delicate and intricate work of their textually inclined colleagues and have incorporated such analyses into their efforts to find a way to think about the upheavals of the 1990s. For them, issues of ethnic and national identity and the constructed cultures related to them are placed in a theoretical space that expects an interrelationship between people in the world and people in their heads.

Future Needs

If anthropological findings are to have any value for understanding the world of the twenty-first century, cultural and social anthropologists, in particular, need to develop another front. We have so far failed to face head-on the reintroduction into scholarly and popular discourse of the place of genes and biology in the political and social life of populations. Feminist and gender studies have had perforce to deal with the issues because biological difference has defined, rightly or wrongly, the categories of men and women. Now terms like ethnic, multicultural, and race are common parlance in and outside academe and are used in ways that can be interpreted as referring to biologically, genetically based distinctions. The impact of decades of scholarship that demonstrated the historical contingency of boundaries of ethnicity and the particular cultural ideas and practices associated with them has been forgotten. Essentialist qualities of character, personality, and intelligence (shades of the 1940s) are being imputed to so-called cultural differences among diverse populations, and too often they are construed as genetic differences.

Anthropological scholarship long ago demonstrated the distinctions among race, language, and culture. The matter needs to be revisited in terms that confront the enormous developments in biology and psychology—not just sociobiology, which has its own agenda, but new knowledge, particularly in neurology and cognitive psychology. We need to return to viewing humans as wholes and learning what characterizes all of us as humans. What is the neurological base for consciousness and cognition that interacts with experience to produce and continually effect culture and cultural reproduction? Unless we anthropologists know the results of new work in biology and

psychology, we cannot substantiate a conviction that situational factors and the nervous system are in constant interaction to create both similarity and diversity. Human psychology may be the next frontier for us, if we are to continue the tradition of a scholarly discipline whose results are available to improve the human condition.

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