

**RECOLLECTIONS OF EARLY ANTHROPOLOGY AT
COLORADO STATE UNIVERSITY
AND
REFLECTIONS ON ANTHROPOLOGY**

by

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Introduction

In this paper I will present an account of the early development of the anthropology program at Colorado State University (CSU). The report draws on my knowledge of events, not on records, and I have not consulted with colleagues. I must assume that viewpoints, interpretations, and judgments expressed here might not be shared by all others who experienced the early years.

This essay is developed from the experience of a cultural anthropologist, and most attention is given to cultural anthropology. Archaeology receives limited but careful attention, while physical anthropology is underemphasized.

The factual centerpiece of the report concerns the period from 1963 to 1974. Those were years of faculty growth from zero to nine, program development, and, during most years, increasing enrollments in anthropology courses. By 1974 anthropology was poised to achieve departmental standing, and later in the 1970s there were substantial changes in conditions affecting anthropology. This report will not concern such changes, but it will focus attention on the years when anthropology was in its primary growth phase and when it established major characteristics that have endured into the 1990s.

Cultural anthropology is a contextualizing field, so it is proper to foreground the growth years with discussion of conditions and trends that affected professional anthropology and that made anthropology interesting and important to the public. I will evoke post-war America at mid-century and review some international, national, and Colorado-specific issues. Anthropology was suited to

the climate of the times, and its intense growth here and elsewhere can be seen as a response to those times. Where it is appropriate I will mention some background items that affected anthropology at CSU in a definite manner, such as the composition of the faculty. In closing sections of the paper I will offer some opinions and judgments gained from the benefit of hindsight. I assume that readers will recognize names of professionally prominent anthropologists mentioned at several points in the essay.

National and International Contexts for Anthropology at Mid-Century

Anthropology benefited from the growth of American universities in the decades following World War II. The era was one of economic growth, higher incomes, and substantial social mobility. State governments expanded their university systems and the federal government increased its research budget while also subsidizing military veterans with the GI Bill.

If all fields gained from the rising tide, the gain for anthropology was disproportionate. The major surge in higher education was a necessary condition for anthropology to acquire a foothold at hundreds of universities and colleges where it was previously unknown. Without the surge those schools would have continued to employ economists and political scientists, but few would have gambled on the novelty presented by anthropology, which would have remained tethered to a handful of institutions.

Anthropology's role was enhanced by its post-war transformation from an Americanist field to one with global aspirations and multicontinental reach. Prior to World War II most anthropologists followed the flag in the sense that they did field work within the political boundaries set by their own nation or by their homeland's overseas empire. A young American visiting Asia, Africa, or South America was more likely to be a missionary or *National Geographic* photographer than an anthropologist. By the 1930s more Americans were doing research abroad, but American anthropology still had a strong North Americanist flavor. The four-fields structure of anthropology here was rooted in a tradition of comprehensive studies of American Indians, studies performed with the methods of ethnology, archaeology, linguistics, and physical anthropology.

With the dissolution of empires and the insertion of former imperial domains into the American sphere of influence during the early Cold War, most of the world not sequestered behind Iron and Bamboo Curtains became accessible to young Americans. The newcomers would study the development and modernization of Third World peoples, then return home to jobs in the growing universities that suddenly had positions for practitioners of anthropology, the exotic social science.

Some of the universities would promote area studies research programs, taking advantages of regional expertise gained by specialists who journeyed to parts of Asia, Africa, and Latin America. Anthropologists and rural sociologists were often welcomed to the programs because of their grounded local knowledge of lifeways and social systems beyond the reach of political scientists and

economists for whom Cold War issues probably loomed larger than they did for most anthropologists. Universities with area studies programs became magnets for young anthropologists who wished to visit distant lands. Cornell University, for instance, developed Asian programs.

There were also opportunities closer to home. Because of its openness to Americans and its accessibility at low cost, Mexico became a favorite research area. For 30 years after World War II, Mexico and Guatemala (loosely, Mesoamerica) provided the setting for an extraordinary number of research projects in anthropology, sometimes but not always associated with Latin American area programs. Mexico itself had the most advanced national anthropology of any Latin American country, but few Mexican researches published in Spanish were translated into English, and Mexico's cultural anthropologists had only minor influence on topic definition by American anthropologists who did field work there.

The Chiapas Highlands of Southeast Mexico were home to many Maya Indians still almost untouched by anthropology until the late 1950s, when teams of scholars and graduate students from Harvard and Chicago launched major programs, all of them based on funding from United States federal agencies. Stanford faculty members with close ties to Harvard and Chicago set up a more limited program, one that supported my own field work.

In the aggregate these projects generated a great deal of effort in ethnography, social anthropology, and linguistics, plus some archaeology. Chiapas became one of the most thoroughly researched areas in New World anthropology.

The Harvard project had the most long-term success. Several participants who began field work in the early 1960s have returned periodically and continue to do so in the days of the Zapatistas.

The domestic American climate of opinion through most of the period was favorable for anthropology. Political liberalism favored civil rights and the empowerment of minority groups, and it authorized government intervention in the social order. An expanded government promoted education and research on a vast array of subjects, and public consensus sometimes favored the use of the social sciences to help solve problems by an activist government.

If anthropology was not central to any of these trends, it nonetheless benefited from them. Its version of secular humanism could flourish in an atmosphere of intellectual freedom for doing research about human diversity, especially in the public universities that grew rapidly and employed evermore anthropologists. In a generally static university system, or in one governed by sectarian viewpoints, neither these anthropologists nor their research would have been welcome.

Anthropology profited from the establishment of the Peace Corps during the Kennedy administration, and from the Peace Corps' subsequent popularity. CSU faculty members had been instrumental during the founding stages of the Peace Corps, and many departments had people with technical and socioeconomic development expertise that guided their own projects and attracted students concerned with change in the Third World.

Colorado itself ranked high with respect to numbers of Peace Corps Volunteers per capita, and CSU attracted many out-of-state students who were internationally-minded. It helped that unspecialized liberal arts graduates were acceptable to Peace Corps administrators and to host countries. I do not think we kept records about the students who were attracted to anthropology directly because of Peace Corps opportunities, but I recall writing many recommendations in support of applicants, especially during our main growth period and its aftermath for well over a decade. Some applicants had been anthropology majors, but many others were not. I believe anthropology enjoyed a benefit-by-association in students' thinking because the positive response to the Peace Corps was widened to include anthropology, itself perceived as a supporting discipline.

Our host department's Ph.D. program was being defined as the "sociology of development," and members of the anthropology faculty began to serve on graduate committees of foreign students and of American students who would go abroad, both within and outside the department. Internationally-minded students were encouraged to enroll in anthropology courses. For much of the underdeveloped world we really did not offer directly relevant ethnology courses, but we profited from the assumption that the study of anthropology and its exotic cultures provided useful knowledge and insights, wherever the destination of an individual student might be.

In summary, the major issues and trends of the times favored anthropology by attributing more than academic importance to it, even in the academic setting.

Cultural anthropology was a primary source of knowledge about Third World peoples and their underdeveloped societies, knowledge derived from the experience of direct, human contact with such peoples. Anthropologists were in demand within the university setting as teachers, researchers, and counselors to those who had not "been there."

Anthropology's Relations with Sociology

In many institutions nascent anthropology programs started within sociology departments, and by mid-century there were numerous instances across the country. Several of us who came to CSU had experienced anthropology within its dependent status, or at least we were aware combined programs.

I cannot believe that archaeology and physical anthropology faculty were very comfortable in combined programs, or that they developed serious intellectual relations with sociology. The situation was more ambiguous for cultural (or, sociocultural) anthropologists who were aware of convergence between sociology and cultural anthropology.

In mid-century cultural anthropology and sociology shared some research perspectives, and some sociologists did field work. In fact the sociological publications analyzing the field work process were important, especially with respect to the participant observation method. The method is fundamental to the identity of cultural anthropology, but during the 1950s the analytical papers in

sociology were more thorough-going, and no bibliography on the subject could reasonably avoid cogent references to sociology.

The community study tradition drew substantially from sociology and anthropology both, and it would have been foolish for people in one field to ignore the other. Studies of marriage, family, and kinship involved both disciplines, although we in anthropology thought generalizations in sociology were ethnocentric and culture-bound. At least the sociology textbooks showed a deference to anthropology in the chapters on human fundamentals and exotic variations. It may be ironic that the mid-century leader of comparative kinship study in anthropology, G. P. Murdock, perhaps the most salient American scholar on the subject, had been trained originally by evolutionary sociologists.

Research on ethnic and "racial" groups was done in both fields, although anthropology claimed a near-monopoly on American Indians. Sociology dominated studies of American Negroes, if not to the point of monopoly, and as Hispanic Americans came into view most of the research about them was done within sociology. Each field had a major focus on the adjustment of immigrants and minorities to life in the United States. Acculturation was the main concept used in anthropology, while assimilation was utilized more in sociology.

Just as research interests pulled some anthropologists toward sociology, so did graduate departments create a professional atmosphere that was compatible. To illustrate, it is helpful to remark briefly on four outstanding anthropology

programs, and to delineate a fault line separating those that did promote some alignments between cultural anthropology and sociology, from those that did not.

Chicago's large department has supported four-fields training for many years, but since the time of Redfield and Radcliffe-Brown students in cultural anthropology often have been encouraged to position themselves broadly within the social sciences. Chicago's institutional strength in all social science fields accentuated the trend.

Harvard's rather traditional program was supplemented in mid-century by the Department of Social Relations, which drew faculty and students from across the social sciences. Anthropologists might have dual appointments, or Social Relations might provide their only position. The collaborative enterprise was formulated by eminent scholars including Clyde Kluckhohn and Talcott Parsons, who promoted a vision of unified social science.

Within an interdisciplinary social science cultural anthropology had the special task of dealing with culture, its properties and dynamics. Over time, faculty and students associated with Harvard and Chicago cultural anthropology came to dominate cultural studies of symbols and cognition.

In contrast, Columbia and California-Berkeley maintained a purer tradition of anthropology and kept social science at arm's length. I believe the academic climate at these schools has favored the retention, among their cultural anthropology graduates, of perspectives linked to general anthropology and with

few moorings in general social science and few linkages to sociology. Perhaps Boas and Kroeber have cast long shadows.

It also bears mention that during the mid-1950s the center for Advanced Study in the Behavioral Sciences was founded at Stanford University, with Ford Foundation sponsorship. From the outset cultural anthropology was a prominent discipline from which Fellows were selected. A year at the Center provided opportunity for research and writing, and for frequent exchanges across disciplinary lines between scholars who could position their interests within the behavioral sciences.

It is normal for scholars to look beyond their own fields some of the time, and it was never possible to imagine cultural anthropology, archaeology, linguistics, or physical anthropology as sealed off from outside discourse. But the situation creates tensions for anthropology because of the fabled four-fields tradition, one that connotes a "gang of four" joined as one impermeable entity. That may have been viable for an Americanist field practiced by a handful of mutually acquainted scholars. As time went on each subfield became more permeable, and cleavages developed that corresponded to directions of external discourse. But our growth at CSU depended on commitment to the unity of anthropology, and for some of us that entailed suppression of outward linkages, especially with sociology, the host discipline.

As individuals several of us in cultural anthropology had prior exposure to sociology. Robert J. Theodoratus took his bachelor's degree in a sociology

department at Washington State, and I believe Donald E. Crim's departments of origin, at Ohio State and Cornell, both were combined programs at an earlier time. Crim has spoken about sociological associations in his earlier years. My department at Stanford had split from sociology not long before I entered it. There was an emphasis on sociocultural change, and bibliographies cross-cut both areas. Within anthropology there were no methods courses, and as I was advised into psychology for statistics, so was I advised into sociology for methods. That is why I remember the sociological literature on participant observation.

In the late 1950s the Stanford department was really a program in cultural anthropology. Bert Gerow, a Berkeley graduate who had studied under Kroeber and Lowie, was responsible for archaeology, physical anthropology, linguistics, and folklore! We were told that if we wanted more than basic courses in these areas, we should apply to Berkeley. Stanford later developed a broader program, although it never developed as much range in general anthropology as Berkeley's. I came to understand that Gerow's sense of anthropology was firmly in the Berkeley tradition when he advised my trait-distributionalist M.A. thesis on Plateau oral literature. And I found it interesting that two figures from Gerow's graduate student era at Berkeley later developed the program at Colorado-Boulder. Omer Stewart preceded him, and Gordon Hewes was in the same cohort. I believe the kind of anthropology they installed at Boulder conformed to Berkeley-style anthropology reasonably well.

For several of us in cultural anthropology, sociology had familiar aspects and at least some research that needed to be read. If students could not differentiate us from sociologists, we emphasized how comparative we were, and that anthropologists went to remote places and sought out diverse peoples. It helped to call attention to anthropology's bases in language, biology, and the material world. We said that sociology had no museums.

These viewpoints were useful in aligning ourselves with colleagues in physical anthropology and archaeology, who represented biology and material things for us. Although cultural anthropology contributed most of the course enrollments necessary to anthropology's existence at CSU, the essential nature of the anthropology program depended on our minority partners in physical anthropology and archaeology. Without them we were, after all, perhaps only exotic sociologists.

In general, the archaeology and physical anthropology faculty themselves supported the four-fields principle, which may have been inculcated in graduate school. They tended to see cultural anthropology more as traditional ethnology than as social science. Their viewpoints and sense of personal interest merged with ours, so there were no disagreements about our collective goals. We must persuade CSU administrators that they could not claim CSU was a major university as long as anthropology was shackled to sociology. And as the 1960s progressed we and numerous other anthropology faculties across the country took steps that separated us from our host sociology departments.

Early Faculty Hiring and Growth within Sociology

According to hearsay I recall from years ago, the first course in anthropology was taught by a now-retired sociologist, Joe Sardo. I do not know what led to the decision to hire full-time faculty in anthropology, but the step was taken by 1963, when Donald E. Crim was hired. In graduate school at Cornell University, he had completed field work among the Yir Yoront of Australia's Cape York Peninsula.

Crim soon introduced courses spanning the discipline, including archaeology and physical anthropology. In cultural anthropology he would ultimately teach a wider range of subjects than anyone else, such as theory, ethnomusicology, and language and culture. And nobody else would ever teach two regional ethnology courses so effectively. His Oceania and North American Indians courses would draw large enrollments for many years.

A year later, in 1964, David McCurdy joined the faculty, also from Cornell. I had known McCurdy between 1957 and 1959, when we were in the same M.A. cohort at Stanford. He chose to return to his alma mater for the Ph.D., in part because of Cornell's foreign-area research opportunities, and he performed field work among a community of Bhils in India. Besides India ethnology he also taught culture and personality, and he was very effective in lower division introductory courses.

McCurdy had an engaging personality, and he interacted easily with a great range of people on campus and in the Fort Collins community. The CSU internal

structure was much simpler than it is now, and he quickly established roles that went beyond the department.

Two positions were opened for 1965. Creighton T. Shay filled the archaeology position; I do not know how he was selected, but I believe he was interviewed at a convention. Shay's field site was at Lake Itasca, Minnesota, and he had not completed the dissertation at the University of Minnesota. A specialist in pollen analysis, he had spent a year in Denmark studying the subject. Shay's hire was the first in archaeology.

I was recruited for the program's third cultural anthropology position by McCurdy, entirely on the basis of our prior acquaintance. We had not been in communication between 1959 and early 1965. I had returned to Stanford from Chiapas, Mexico, where I had done field work on the social organization of family and kin in a Ladino town, and I was writing my dissertation. We conducted the hiring process entirely by telephone and mail, without interviews.

My own graduate education process exemplifies some of the ways the federal government's activities affected student careers. As a military veteran I was eligible for GI Bill support and received a monthly stipend. Before going to Mexico I spent on year working strictly for income. The job, mostly performed in Alaska, was done for an academic psychologist whose funds came from a U.S. Air Force contract. And the Mexican period was supported by a federally-funded research grant to my thesis adviser, A. Kimball Romney, a product of Social Relations at Harvard.

At CSU I had a broad measure of freedom to define areas of teaching interest, which became Mexican ethnology, kinship, and cultural change. During the 1965-66 year my priorities were to teach the courses adequately and finish the dissertation. As a graduate student I had taught weekly section classes, but I had no prior experience with full class responsibility. In retrospect, I am glad no videotapes were made during the first year, which I found difficult. I did finish the dissertation by June of 1966.

Shay and I had joined the program while it was still housed in the Old Main Building, one of the original campus structures, but plans were underway for new facilities in the Social Sciences Building (later Clark). Some faculty were involved in planning. For instance, Shay planned a pollen analysis facility he expected to use in the archaeology laboratory.

There was every expectation of continued growth, and a new position was created for 1966. I do not recall how it was defined, but interviews took place at the 1965 meetings of the American Anthropological Association (AAA), conveniently held in Denver. The hiring of Theodora C. Kreps was affected by prior acquaintance patterns.

Kreps had belonged to the same student cohort as McCurdy and I. Her Stanford dissertation was a comparative study of Uto-Aztecan kinship terminology, and her interests included psychological anthropology, theory, and linguistics. She had taught several years at Sacramento State (now, Calif. State Univ. at Sacramento), and she welcomed the chance to return to Colorado, her native

state. She and I were friends, but it was McCurdy who persuaded her to accept CSU's offer. Before coming to Fort Collins, she would spend the summer of 1966 as an instructor in an anthropology teacher training institute held on the Colorado-Boulder campus and supported by the National Science Foundation.

Still during the 1965-66 academic year, McCurdy surprised and disappointed all of us by announcing that he would leave CSU in favor of Macalester College in St. Paul. This first loss of faculty was disconcerting both because of his teaching ability and because of his leadership qualities. I believe McCurdy preferred the size and structure of a private liberal arts college to the organization of a campus such as CSU, which was going to become very complex as it expanded. And I think he expected the rewards of superior teaching were more likely to be received at a college.

McCurdy has remained at Macalester for 30 years. Working with a colleague whom he hired, he co-edited an anthology of readings for use in introductory anthropology classes. The book is one of the most successful ever published for cultural anthropology and it has gone through several editions. McCurdy also became active in the General Anthropology Division of the AAA, and lately he has held elective offices in it, as Secretary-Treasurer and Chair.

To fill the gap created by McCurdy's impending departure, Kreps recommended that her Sacramento colleague, Robert J. Theodoratus, be considered. His Ph.D. was from the University of Washington, and his interests included immigrants, ethnographic bibliography, religion, Native Americans, Europe,

and the Middle East. His library knowledge and bibliographic skills were especially attractive, given the need to enlarge the anthropology holdings at the Morgan Library.

I believe Theodoratus shared Kreps' view that general opportunities were greater here than in Sacramento. His undergraduate degree was from Washington State, a land-grant school with structural parallels to CSU. He and Kreps would be the first anthropologists appointed at the Associate Professor level.

Between 1963 and 1966 five full-time anthropology positions had opened, and we entered the 1966-67 academic year in a fairly strong position despite losing a valued person. Crim was our veteran, Shay and I had one year's faculty experience, and our two newcomers would provide range and depth beyond what we had before.

Enrollments were increasing, and we used this asset as a basis for lobbying the administration for another faculty member, a physical anthropologist. Our initiative was resisted by the Academic Vice President, but eventually a position was authorized. The physical anthropologist professional identity of the new person somehow was camouflaged.

I have forgotten the details of J. Stanley Rhine's appointment as our first physical anthropology faculty member. We had no close associates at the Colorado-Boulder department where he was a graduate student, though Kreps had met him when she taught in the NSF program. He specialized in osteology and had interests in human evolution and primate behavior.

We anticipated that Rhine's arrival for 1967-68 would bring our faculty size up to the level of six members. A core structure had been established for cultural anthropology, archaeology, and physical anthropology. But within a short time we were to lose Kreps and Shay, and it would be several years before the instability in archaeology was corrected.

In 1967 Kreps moved to the State University of New York at Albany, attracted in part by the expectation of a large graduate program at SUNY. She had been disappointed when McCurdy left because she thought he would be the key figure in negotiating anthropology's growth and separation from sociology.

I do not remember if Shay left at the same time, or not until 1968, but I believe his departure was based on a combination of personal preferences and research considerations. He went to the University of Manitoba at Winnipeg, a location closer to this area of primary archaeological interest. Although the late 1960s was an era when many Americans went to Canada for political reasons, including some professional anthropologists, I do not think politics affected Shay's decision. He took advantage of a Canadian opportunity that brought him closer to his research. He stayed at Manitoba until retirement.

Considering all three of the early faculty losses, I believe McCurdy's was the most consequential. Kreps had not been here long enough to build influences, and Shay was replaced by 1968, if only temporarily. McCurdy was not only a good teacher and leader; he had acquired committee memberships and made friendships that gave him remarkable access to prominent figures, including the

President. This gave anthropology a measure of visibility on campus, and his departure reduced our collective image.

Beginnings of Segmentation from Sociology, and Program Development

1967-68 was an important transitional period. Faculty appointments, which I will discuss first, allowed us to replenish losses but not to augment numbers. We also accelerated the process of segmentation from sociology.

Although faculty loss had occurred, enrollment growth allowed us to hire replacements. The cultural anthropology position was filled for 1968 by **Esther J. Pressel**. I believe she was interviewed at an AAA meeting by Theodoratus, after field work in an Afro-Brazilian community. Her Ohio State University dissertation on trance behavior and folk religion had not been finished. Besides ethnographic teaching on Africa and African-derived cultures of the New World including the Caribbean, Pressel would specialize in psychological anthropology and, later, in medical anthropology.

For archaeology, we had already made use of one temporary part-time instructor and would soon add another. Orville Parsons was a soils expert retired from government employment who was also an amateur enthusiast for Colorado archaeology. He taught some courses on a stop-gap basis during the late 1960s.

We also hired Betty Bell, a UCLA-trained Mesoamerican archaeologist then living in Fort Collins only because her husband was briefly on the sociology faculty. After two or three years they departed for permanent residence in Mexico, where

Bell later had a staff position with a Mexican federally supported regional museum at Ajijic, Jalisco. One of our undergraduate majors, Michael Foster, probably continued in archaeology because of Bell's encouragement, tutelage, and professional contacts. His Colorado-Boulder Ph.D. was based on West Mexico, and I do not remember any other undergraduate whose Ph.D. field site was in Mexico.

W. James Judge was hired for Fall 1968 in the full-time archaeology position vacated by Shay. He had not finished his degree at the University of New Mexico, and I do not recall how he was selected; he was not expected to remain permanently. Judge attracted students and strengthened the professional character of the program, believed to have been tainted by Parsons' amateurism. Judge had a summer program at Fort Vasquez that gave students the chance to do hands-on research.

Physical facilities changes and new administrators both made a difference for us. The move out of the Old Main and into the Social Sciences Building dissolved our intimacy with sociologists. The new area was so much more spacious that mere contiguity no longer structured interactions so much. And the new classroom and laboratory space helped give us a sense of entitlement.

Glenn Dildine, Chairman of the Department of Sociology when Shay and I arrived, had experience in community development programs, and he liked to promote a co-operative ethos. I believe he preferred that anthropology and sociology integrate their programs, and that some of his sociology colleagues

shared the view. Some were rural sociologists, some had degrees from combined departments, and several had foreign-area field experience akin to that of cultural anthropology.

During the early period we shared faculty meetings and anthropology faculty served on sociology graduate committees. The internal committee structure was fairly simple and not burdensome. We were welcome at sociology parties. Anthropology faculty were involved with the selection process for Dildine's successor, Bert Ellenbogen, both in formal review meetings and at social entertainments for visiting candidates. Once Ellenbogen assumed the chairmanship, our association with sociology became attenuated and we were freer to take responsibility for our own affairs.

The new College Dean in 1968, Daniel Ogden, supported anthropology's goals. He and Ellenbogen recognized the professional differences between the fields, and the sociology program was growing so fast that sociology faculty had no reason to retain claims on anthropology or inhibit growth in anthropology. The "good times" produced by soaring enrollments and increasing budgets helped ease relationships.

It was in the late 1960s during Ogden's deanship and Ellenbogen's chairmanship that administrative processes were reorganized. **Theodoratus became Director of the Anthropology Division and functioned in many respects as a Chairman.** Important reports and documents still required sign-off from Ellenbogen, but most activities affecting faculty and students in sociology and

anthropology were effectively separated. The graduate programs would diverge with respect to requirements, but the M.A. degree would formally designate a joint program. Sociology kept most of the external committee obligations for itself, which left anthropology faculty more time to spend on our own concerns. So we became separate but not quite equal.

Especially in cultural anthropology, the undergraduate curriculum already in place in the late 1960s set the pattern for later decades. Our structure conformed to basic anthropology as we understood it, modified to suit local circumstances. We installed a foundationalist curriculum that avoided idiosyncrasy, and we did not over-proliferate regional or topical courses. On the whole, we established a fine curriculum in general anthropology. It drew occasional criticism from faculty in applied disciplines on campus, who judged the anthropology program to be "classical."

Students jammed our classes, and we looked to Colorado-Boulder's Ph.D. candidates for instructors to staff extra sections of 100-level courses. I have mentioned Peace Corps as an opportunity that put anthropology in the spotlight in student thinking, as well as anthropology's expertise on foreign cultures. There were other factors that made anthropology a popular student choice.

Middle class prosperity permitted many students to delay career decisions, and anthropology took its place among fields judged impractical but interesting. As a relatively new subject it attracted students who were willing to risk novelty, especially after experiencing dissatisfaction with initial major choices.

I believe we benefited a great deal from incoming transfers who selected the anthropology major, and from out-of-state students. I know the latter had come to count disproportionately among our major students by the early 1970s. It is flattering to speculate that transfer and non-resident categories can serve as proxy measures of greater willingness to go a bit beyond convention, because we in anthropology regard ourselves as less bound to habitual ways of thinking, and we take pride in attracting the less conventional students.

It is beyond the scope of this essay to address the meanings attributed to anthropology by members of the '60s generation of counter-culturalists and anti-war militants. I believe that the stresses of "youthquake" and unpopular war contributed to the 1960s surge in interest in anthropology, but also that the same events put moral demands on anthropology that it could not satisfy. The subject of the relation of anthropology to the counter-culture movement is one that is more often understood through stereotype than analysis, and I hope the issue someday receives dispassionate attention.

One undeniable aspect of student-faculty relations in the 1960s is that faculty were young, especially in programs such as anthropology. The ethos of the times diminished the importance attributed to status distinctions. So, both youth and relaxed manners promoted a good deal of social interaction between faculty and students, especially graduate students.

We anthropologists were perceived to be egalitarian, and some of us were especially hospitable. Parties at the Crims', and for a time at the Rhines', became

central events in the social lives of many students. A visible reminder of the "era of good feelings" between students and faculty is the Altamira bison painting that graces the hallway wall between rooms B-250 and B-251 Clark, an office area formerly assigned to anthropology. The picture was painted on her own initiative by Nancy Robinson, artist and anthropology student. Facilities management bureaucrats disapproved of the cave art's appearance on the blank wall, but they were dissuaded from removing it.

As the faculty aged and students did not, greater social distance developed, especially in cultural anthropology. Archaeology has the advantage that its training process is necessarily collaborative and interactional, so the student-faculty social gulf is lessened, and morale is improved.

Maximum Faculty Expansion in the 1970s.

Faculty turnover in archaeology and physical anthropology during the early 1970s was followed by expansion in archaeology and cultural anthropology. By 1974 the anthropology full-time faculty would climb to nine members.

Judge moved to New Mexico in 1970. For him the switch back to the Southwest represented an opportunity in the major archaeology program from which he had received his Ph.D. In retrospect, the timing of his departure seems fortunate both from the viewpoint of the anthropology program and from the perspective of his successor, Elizabeth A. Morris.

We badly needed professional strength in the only budgeted position allocated to archaeology, and we needed someone willing to commit to CSU. Morris met both needs. A Colorado native whose degree was from the University of Arizona, she had been away from the region for a number of years and was pleased to see an opportunity to return.

Morris had a lifelong background in archaeology, plus an abundance of educational and museum training experiences in archaeology and related fields. With field experience in the Southwest, the Middle East, Europe and the historical Eastern U.S., her professional background was substantially broader than Judge's. And she had recent teaching experience at Temple University. Once she arrived at CSU she developed a teaching and research structure that was of lasting importance for both undergraduate and graduate students, and she banished the traces of amateurism that may have persisted from Mr. Parsons.

In 1971 Rhine went to the University of New Mexico, where he has remained. From our perspective the principal loss was his teaching skill; he had developed a loyal following of devoted students. In the process of considering job applicants who wanted to succeed him, Rhine was influential in recommending Michael Charney, a friend from the time both had been at Colorado-Boulder.

Approaching age 60 in 1971, Charney had returned to anthropology graduate school in mid-life following a career in forensic-related activities and business ownership. He had left a position at Idaho State University because of

dissatisfaction with the administration of anthropology at the school, and was willing to return to Colorado.

Charney quickly allayed our concerns about the loss of Rhine by establishing a reputation as a compelling lecturer. By force of personality he became a respected figure in the program and, like Rhine, recruited students who were devoted to physical anthropology and to him as a person. During two decades of forensic and human identification activities, mostly performed after formal retirement, he became the most prominent CSU anthropologist in the eyes of the public.

There were parallels in the circumstances that brought Morris and Charney to CSU, and in the consequences that followed. In consecutive years, anthropology had lost able young persons with strong potential who occupied important solo positions in the program. In both cases we found successors with superior professional experience who were available for hiring and who wanted to come. In both cases the replacements were authorized in short order, so there was no hiatus, and the appointments were made at the Associate Professor level in recognition of experience. Morris and Charney both brought highly energetic commitments to the anthropology program, and I believe there was a net benefit in each of these instances of faculty turnover.

Primates were not part of Charney's repertoire, and during the early 1970s James Ellis was hired to teach a primatology course vacated by Rhine. A zoologist with an appointment elsewhere on campus, Ellis had spent a year at a primate

research laboratory in England. He would teach the course until a primate-qualified physical anthropologist was hired later in the 1970s.

Three new hires would culminate anthropology's program growth in the early 1970s, all of them made while anthropology was still linked formally with sociology. Faculty hiring was becoming more complex, partly because procedures became more elaborate and partly because of high numbers of applicants. As Director, Theodoratus was granted some discretion on administrative matters and much of the program's internal structure was informal, but on important issues such as hiring the faculty acted as a committee-of-the-whole.

An obvious gap in cultural anthropology was the lack of a specialist in Native American studies, an anomaly in any anthropology program and especially so at a Western land-grant school. The fifth position in cultural anthropology was defined for this topic. The job was offered to and taken by **John L. Schultz**, who was getting his degree at Washington State University.

Schultz had done field work among the Colville of the Northwest Plateau and among the Crow of the Northern Plains, and his research was contemporary, not classical. He was a protégé of Deward Walker, a well-known scholar of Native Americans who was at the University of Idaho, a few miles from WSU, but who was moving to Colorado-Boulder. At CU Walker would become nationally prominent both for Native American studies and for his role in the Society for Applied Anthropology. He would later edit the SfAA's journal.

We expected that Schultz would expand our curriculum in Native American classes and develop research projects that involved students. And he would become an applied anthropologist like his mentor, Walker, adding a course on the subject to the curriculum. During his first years here, Schultz began to fulfill these expectations. He was a very popular teacher who attracted students to anthropology, and he initiated summer programs involving students, the most important being at Ute Mountain in southwestern Colorado.

A new position was allocated to archaeology in 1972, and Calvin H. Jennings was selected to fill it. He had completed his Ph.D. at Colorado-Boulder on the basis of field excavation in northern Arizona and had been on the faculty at Fresno State; he wanted very much to leave the California academic system and welcomed the opportunity to return to Colorado.

At the time of the Jennings hire student interest in archaeology was on the rise, and there would be more opportunities for field participation. Federal laws mandated that public lands be surveyed for cultural resources prior to their exploitation for various industrial, commercial, and extractive purposes. The Colorado energy boom would support the growth of archaeology at CSU. The Laboratory of Public Archaeology (LOPA) headed by Jennings would provide students with research opportunities, salary support, and a built-in social structure. As a growth center in the middle and late 1970s, LOPA would give anthropology some compensating strength during a period of enrollment decline, faculty turnover, and diminished morale.

By 1973-74 the maximum enrollment peak had been reached and the anthropology faculty numbered eight. We were authorized one more position in cultural anthropology.

From a distance of 20 years it is difficult to recall why the second position in Native American studies was established. I think two issues were involved. Courses were popular, including new ones taught by Schultz. And I believe we acknowledged that the topic might generate more research and graduate student opportunities beyond those generated by Schultz. I do not recall his opinions, but archaeology students enjoyed more research opportunities than did cultural anthropology students, and we wanted to redress the imbalance.

During spring of 1974 we reviewed a great many applications, some from very promising persons. Our first choice was Janet Jordan, who accepted. A Colorado native and a graduate in anthropology at Colorado College, Jordan was glad to return. Her field work among Oklahoma Cherokees was for her degree at Yale University, and she had not finished the dissertation, as I recall.

Jordan's mentor at Yale was Leopold Pospisil, a legal anthropologist, and she also had experienced tutelage by Pertti Pelto, University of Connecticut applied/nutritional anthropologist. Her Cherokee research had contemporary and ethnohistorical features, and the legal issues set her application apart from others. Her recommenders provided "over-the-top" assessments of her research ability. We were dazzled by the high scholarly attributes and we may have overlooked

her lack of research grounding in the Rocky Mountain West. Like coaches who recruit the "best athlete," we selected the "best scholar."

Jordan's hire marked the end of an eleven-year period of faculty expansion. Anthropology now had **nine full-time faculty**, of whom six were in cultural anthropology, two in archaeology, and one in physical anthropology. This was to be an all-time high, not to be approached again for 20 years.

Credit for sustaining the growth through 1974 must be granted to Dean Ogden, who had confidence in the program. We had a friend in the Dean's office, one who accepted our assessments of need. High enrollments helped, Ogden himself acknowledging the importance of "bean-counting." We had plenty of beans, even taking account of staffing formulas that placed high numbers requirements on the social sciences.

It helped that archaeology and Schultz were viewed as actually or potentially robust financially; this would offset the effete financial character of most cultural anthropology faculty. And it helped that we had a thriving M.A. program with an adequate number of students; we were graduating students who found teaching jobs or enrolled in Ph.D. programs. These advantages were important in Ogden's assessment, and credit should be given to Theodoratus for keeping the Dean in our favor, and for negotiating to anthropology's advantage. I do not remember whether we were in direct competition with sociologists for new positions. They also were adding faculty, and in a growth era there were enough positions to go

around. I think Ellenbogen had a benign view of growth in anthropology and did not see it as a threat to sociology.

By 1974 all the basic groundwork for departmentalization had been built, and what remained was a process of formalities within the University and beyond to the Colorado Commission on Higher Education. Meanwhile, Theodoratus was awarded a sabbatical leave for 1974-75, the first for a CSU anthropologist. I was selected to serve in his absence as Acting Director of the Anthropology Division. Crim nominated me, the faculty agreed, and Ogden made the appointment.

A Note on Networks

Personal networks built at certain universities obviously affected hiring. Most obvious are the Stanford connection involving McCurdy, Kreps, and myself; the Sacramento State connection between Kreps and Theodoratus; and, the Colorado-Boulder connection between Rhine and Charney. I do not know details of the Cornell connection that included Crim, McCurdy, and, according to plausible hearsay from the early years, some members of the sociology faculty. After Rhine left for New Mexico, only a year after Judge's departure to the same place, we speculated about the role of a CSU connection between them.

Other linkages may have eluded me. Apart from hiring matters, it is tantalizing to recall an aspect of Ogden's goodwill toward anthropology. Ogden had once been at Washington State, Theodoratus's alma mater, and Theodoratus

enjoyed remarking to us about Ogden's alleged friendship with a neighbor, the Northwest archaeologist Richard Daugherty.

Remarks on the M.A. Program

The M.A. program had two main academic purposes. One goal was to qualify students for teaching anthropology at the community college or junior college level; during the late 1960s there were numerous teaching opportunities for holders of the M.A. degree. We wanted our graduates to be able to compete successfully for such jobs, and to perform well in settings where they might be the only anthropologists.

The second purpose was to prepare students for admission to Ph.D. programs, assuring that they would not be delayed by the need for remedial study once admitted. At the time a significant number of our students anticipated the Ph.D.

A modified four-fields approach supported both goals. Students taking comprehensive exams were tested in archaeology, physical anthropology, and cultural anthropology. We required them to take the language and culture course but usually did not test separately in the area; it was combined with cultural anthropology. Thesis students were advised into suitable courses as our means of educating them for breadth in anthropology.

These practices emphasized the view that anthropology is a single discipline, and also that three sub-fields are structurally equivalent. Cultural anthropology is

not linked to other social sciences, but to the rest of anthropology. We spent a great deal of effort trying to achieve program balance and overcoming the condition that most faculty and classes were in cultural anthropology. One consequence of the balancing act was that exam questions in cultural anthropology, refined from the submissions of several faculty, had an omnibus quality. Questions in archaeology and physical anthropology, offered by one or two persons, were more focussed and coherent. The collective process had the salutary result that all faculty were involved in the examination process. We believed this fostered faculty unity and that it was an explicit way of demonstrating to students that anthropology is a single discipline.

We had many fine students, and program self-evaluations in later years showed that our main goals were achieved reasonably well. Comparing ourselves with other faculty in similar circumstances, I have always thought our results were good. Students did get jobs and those who entered doctoral departments reported back that they had been well-schooled in the basics of anthropology.

Nonetheless, it is fair to acknowledge internal differences with respect to professional orientation of students toward the research process. Archaeology did better than cultural anthropology. **Once Morris and Jennings arrived, they established pre-professional processes that continued to characterize and partially distinguish archaeology.**

There were regular archaeology summer field sessions and site visits during the rest of the year, especially by LOPA. Faculty provided consistent supervision

of students' excavations, analyses, and write-ups, all on a predictable basis. These stable processes often led to participation by students in local or regional meetings, and they helped establish the normative expectation that a thesis would be written for the M.A. Thesis revisions sometimes were published.

Cultural anthropology as such had no parallel processes. We educated our students well, but we had no training program that professionalized them. The more able among them, the self-starters, adapted well and utilized our resources effectively. But most of the better students themselves had to await doctoral work before acquiring a professional orientation.

Concerning my own role, I would distinguish two aspects. As an adviser I was not strong, and most of my students took comprehensive exams. I was a member of at least my share of anthropology student committees, but had only one thesis student, fewer than some others in cultural anthropology.

I am more comfortable reviewing my instructional role because in general I taught at least as effectively as I had been taught in graduate school, and sometimes better. If I have regret it is that I did not utilize the methods course better. I am still satisfied that my major assumption was correct, i.e., cultural anthropology methods are most fundamentally ethnography and comparison, and students should learn about those, above all. Hindsight suggests my course could have been more useful had it emphasized analytical techniques more directly. Methods was just one course, and we in cultural anthropology did little to cross-reference our courses; each of us was a sovereign entity.

The Macalester College anthropology program developed by McCurdy and his colleagues has an unusually high focus on ethnographic analysis, and all undergraduates there must perform it. This policy requires a high level of faculty consensus and willingness to collaborate. I do not believe our cultural anthropology faculty had enough consensus that we could have implemented an agreed-upon process, one that entailed curricular co-operation. The positive "spin" on the matter suggests that by granting full instructor autonomy we assured that students would be exposed to several distinct perspectives on cultural anthropology, this seen as a strength.

The differences at CSU between cultural anthropology and archaeology reflect anthropology as a whole. Archaeology provides early field work and trains students in hands-on techniques. Every summer there are many archaeology field programs across the country; in cultural anthropology there may be a handful, and close attention is necessary to get a sense about what the research process will be. Moreover, excavation in archaeology is usually a social process; in cultural anthropology the "lone wolf" process is much more likely.

It may be mentioned that our preferred cultural anthropology field sites were remote from Colorado, and that our research colleagues were likely to be at distant schools. Few professional meetings of cultural anthropologists' interest occur within a thousand miles. In contrast, the number of nearby archaeologists is high by national and world standards; archaeology colleagues abound, professional meetings are accessible and frequently held.

I think the best defense cultural anthropology can make is that we made a genuine effort to redress the imbalance, especially with the Schultz hire, then with Jordan's. We hoped they would develop regional research projects that would generate research and financial support for graduate students. Approaching the mid-1970s we had reason for optimism that internal imbalances would be mitigated. In 1974 it was impossible to foresee that before the end of the decade Schultz and Jordan would be gone, the former to private business and the latter to law school. Neither would be replaced.

Some Thoughts on Global Anthropology, Culture, and Nature

The globalization of American anthropology had mixed consequences. It provided new generations of anthropologists with opportunities of extraordinary scope, far beyond the dreams of intellectual avarice. To appreciate the range available to American anthropologists one has only to contrast it with the range available to Mexican anthropologists, which will be limited to Mexico/Mesoamerica forevermore. And there is no question that the position anthropology has gained in the universities owes substantially to presumptions of cultural omniscience attributed to cultural anthropology.

There has been a downside. As cultural anthropology has expanded regionally and divided into a welter of topical specializations it has lost some of the authority formerly attributed to it, possibly because of doubts about its omniscience. Cultural anthropology in the 1990s does not make strong claims

about the subjects on which its authority should be recognized. Meanwhile, "back home" in North America, Native Americans often reject the authority of anthropology, and historians increasingly mark Amerind ethnohistory as their domain.

In the 1960s anthropology was confident of its teachings about culture and about humans as cultural beings. What linked the four fields was that all were concerned with knowledge about human cultural capacity and the variations in actual cultural behavior patterns across time and space. But to the general public the attraction of anthropology often was its concern with nature, or with the alleged part of nature that included the less civilized peoples who, contrary to cultural anthropology's doctrine, were thought to be closer to nature, less cultured and more natural than we Westerners.

This perspective reflects old Western conceits that are not easily dislodged from popular imagination, and that may be reflected in cultural anthropology's own language. Thus, 35 years ago the University of Chicago's Chiapas project was titled "Man in Nature" even as its field workers wrote accounts of Maya culture, and as the techniques of archaeology, linguistics, and ethnohistory were used to reconstruct linkages between modern and ancient Maya cultures. Perhaps the public view of anthropology is not wholly misguided; anthropology is the principal discipline that mediates between the poles of the nature-culture dichotomy in ancient Western civilization.

Anthropology continues to acknowledge its conformity to public perception, studies of people living closest to nature always ranking high in salience. I myself recently felt disappointed to learn that Richard Lee's Dobe !Kung San lifeways of 35 years ago might have been affected by historical change. Anthropology's view of the foraging Tasaday is affected by the alleged authenticity of the group as pristine, or natural.

Because of their isolation the horticultural Yanomamo can seem natural, and Napoleon Chagnon's heroic penetration of the heart of their warring darkness only enhances the image. His field work exemplifies the natural, man-to-man, human process that crosses divides of cultural difference and engages two people, field worker and informant, the latter teaching the former. Our sense that Chagnon is a skilled field worker is confirmed when we learn that he is accorded recognition as a kinsman, a natural relationship. A handful of other social scientists do field work and/or work with informants, but only cultural anthropology defines this process as canonical, as central to its professional identity.

Our habit of naturalizing peoples and their cultures is of course based on the fact that before it became a social science, anthropology was a special form of natural history, and therefore it has an ancestry very distinct from sociology's. And in modern times if one wants to find the best museum displays of anthropological cultures, one goes to natural history museums such as Denver's.

The naturalizing of our cultures has had mixed results for anthropology. Native Americans often resent it, and in some parts of the world anthropologists

are unwelcome because anthropology of course only concerns the primitive. Elite Mexicans patronize anthropology for reasons of nationalistic symbolism, but they know anthropology concerns Aztecs or modern Indians, not themselves. There is evidence that many Americans are ambivalent about being considered an anthropological population, and I believe naturalization provides some of the reason. An irony: living in a culture that exalts nature and science as the key to nature, anthropologists want to discuss culture but are resisted because of anthropology's associations with nature.

On the positive side, anthropology benefited when natural man and the symbols of natural living were appropriated by the ecology movement. We were seen as the best spokesmen within the social sciences for the viewpoints and interests of what were called "Fourth World" peoples. These were unprotected ethnic minorities whose resources were being ravaged by local elites, indifferent majorities, and multinational corporations.

I have used such perspective in teaching. The Lacandonese of the Chiapas rainforest provide an excellent contemporary instance of environmentally knowledgeable people being overrun by a whole plague of destructive forces. But I have found that students resist excessively realistic depictions of Mexican political economy and its consequences. As bearers of an enduring Maya culture adapted to tropical nature the Lacandonese are more acceptable than as a despoiled, opportunistic population of rural Mexicans.

Whither Cultural Anthropology?

Cultural anthropology's mid-century burst of growth in the American university occurred in conjunction with a unique set of historical conditions. Borne on the wings of technology and politics, cultural anthropology went to the far corners of the world just as the last of the natural peoples at the ends of the earth were being incorporated into modern states.

The *National Geographic* and cultural anthropology both took part over several decades in an attempt to use steadily improving photographic technologies to record diverse lifeways before they "vanished." If *The National Geographic* was often first and if it naturalized lifeways, then cultural anthropology's task was to culturalize them, depicting a family of mankind in which all peoples were equally human by virtue of their shared human nature and their diverse cultures. Cultural anthropology's doctrine was that people became fully human only by cultural means.

In most of Asia, Africa, and Latin America there were few natural peoples to study. But there were millions of farming people whose remote ancestors belonged to ancient civilizations known to archaeology and history. Peasant villagers still shared some of the ancient traditions that would be torn asunder as lifeways were transformed by new technologies and the nationalizing, modernizing governments of Western-educated elites. Over several decades thousands of cultural anthropologists and counterparts in cultural geography and rural sociology studied Third World modernization and transformation to material abundance and

political democracy. Cultural anthropology's special talent was its control of cultural theory, and the principal disciplinary marking that identified cultural anthropology studies was the elaboration of theories of cultural change.

The world that nourished cultural anthropology 40 years ago has changed radically. The last remnants of natural peoples have been swept into the global system. !Kung and other San are now deprived minorities in Botswana and Namibia, many of their men having been mercenary soldiers in a "dirty war." Yanomamo are marginalized Venezuelans or Brazilians, stricken by epidemic disease and displaced by members of the dominant society. Their current experience repeats that of most indigenous North Americans during the 17th, 18th, and 19th centuries.

In many areas of old civilization the transition to modernity, predicted so confidently by "emerging nations" theorists in the social sciences, has gone awry and the best hope for them, also predicted by theorists but this time mainly economists, is to be "emerging markets." Whatever the future may bring, in much of the former Third World as it can be perceived in 1996 political democracy is at best dimly visible, material abundance is limited to elites, and environments are being devastated.

Contemporary Mexico is a case in point. National policies are set primarily by economists, or by functionaries who are responsive to economic analysis and to the dictates of international economic institutions. Chiapas now provides a setting for witnessing how large-scale economic policies affect local populations. The

behavior of today's Maya, especially those who support the Zapatistas, is understandable only by taking account of the social ecology of an elite-dominated system of political economy, plus rapid population growth in a deteriorating environment. Contemporary political economy has intensified the extreme inequalities of income for which Mexico is notorious.

Tourists who visit Chiapas from Europe, North America, and Asia most often want to see culturally authentic Maya, as promised by Mexico's promotional advertising and as described by genuinely high-quality ethnography of a generation ago. But today's Maya are actors in a political and economic system they do not control but nonetheless attempt to contend with. In Chiapas as in so many other places in the 1990s, political economy has surpassed culture in salience.

In mid-century and for some decades beyond, cultural anthropology was confident that it could formulate cultural theory as the ultimate basis for unity of anthropology and for the position of cultural anthropology in a non-reductive social science. And anthropologists with training or faculty membership at Harvard's Social Relations and Chicago's anthropology programs were prominent in the effort; anthropologists such as Clifford Geertz attained general prominence for their cultural analyses. For the public at large, Margaret Mead's tireless lecturing and willing popularization of cultural anthropology kept the field and its notions about culture in the public eye.

Interdisciplinary social science has faded, and so has the luster of cultural anthropology's special role within it. Geertz's style of cultural analysis was

influential but also idiosyncratic and it did not become mainstream in cultural anthropology. After Mead's death in 1978 we realized that her role was unique, because nobody succeeded her as a public celebrity figure who used cultural anthropology to formulate viewpoints on a wide variety of subjects.

I think cultural anthropology has lost confidence that it can develop powerful, culturally grounded explanations. In 1981 I heard Kent Flannery use humor to lament the "loss of faith," on the part of many archaeologists, in the culture concept. Materialist, sociobiological, economic, and other reductionist explanatory principles have been invoked persuasively by various proponents, often within cultural anthropology.

Derek Freeman's unremitting attacks on Mead's Samoa field work and ethnographic writings have tarnished her reputation with the public at large, despite the efforts of many who came to her defense, and I believe that many people in the intellectual world lowered their estimate of cultural anthropology by a notch or two, because of the extraordinary visibility Mead had enjoyed as a symbol of cultural anthropology.

There has been other self-inflicted damage. By submitting classic, "objective" ethnographic writings to literary textual analysis, Geertz and others have undermined confidence in the putative scientific or comparative value of these works. Stephen Tyler, a one-time acquaintance who has become prominent for his critical stance on ethnography, once alluded to ethnographies as "turgid travelogues."

So, what should cultural anthropology do? I believe we should acknowledge that the culture concept has been oversold as an explanatory concept. Where noncultural reductionism has successful results, these should be recognized. Reductionism is the way of science.

The more descriptive, contextual and holistic uses of the concept remain necessary. In my view, much of the best ethnographic work in cultural anthropology is wholly consistent with conventional views on culture. I grant that loss of seriously shared interest in culture must weaken the bonds among the four fields. Today each of the four is driven by its own dynamic, one that is often nourished more by collaboration with specialists outside anthropology than by discourse within the "discipline."

Considering anthropology in the professional sense, I see so many centrifugal tendencies that artificial unifying attempts are unlikely to have permanent value. The Americanist focus that gave rise to the four-fields curriculum is overshadowed by a whole-world focus today. The mid-century constellation of conditions that supported a decades-long boom for anthropology has receded into the dustbin of history. Anthropology cannot retrieve that context.

I will hazard no guesses about the future of archaeology and physical anthropology. I think cultural anthropology has to find some new roles for itself, roles that can be at least partially institutionalized in a fast-changing world. My views on this are affected by concerns about the condition of cultural anthropology in the 1990s and its place in the future of American universities.

Any survey of journals that publish cultural anthropology articles and book reviews must conclude that American cultural anthropology today is a truly cosmopolitan field, one that accommodates an astonishing variety of subject matter and modes of analysis. If it is obvious to me that any seriously intended approach to cultural diversity must highlight cultural anthropology, and that any serious examination of the range of human conditions today must take account of cultural anthropology's research, university decision-makers seldom act as if they share my perceptions. Cultural anthropology has only a modest profile in most institutions, and its considerable strengths might not be enough to provide it with a secure place in tomorrow's universities.

Unless I am mistaken, anthropology is still the most academy-dependent of the social sciences, and cultural anthropology bears the highest rate of unemployment or inappropriate employment among new Ph.D.s in the social sciences. These difficulties seem unlikely to abate as pressures for cost-effective higher education continue. Administrators may question the value of a subject that is not manifestly useful and that is often perceived to fit awkwardly into the intellectual and moral comfort zones that define acceptability for American social sciences. With so many normal and safer choices available, it is not surprising that few students opt for majors in "risky business" such as cultural anthropology.

One response cultural anthropology makes is to develop vocational anthropology programs, and I have encouraged my former department to support applied anthropology. I have thought CSU might explore whether the program at

Northern Arizona University offers a useful model. I have acquaintances among the High Plains group of applied Anthropologists, and also some familiarity with the difficulties applied anthropology has had in institutionalizing roles for itself. This is an important effort, but I do not think applied anthropology is the answer for cultural anthropology as a whole. Taking the vocational turn would reduce the field to a service function and amount to complicity in its own marginalization. Cultural anthropology belongs in the academy and should take itself seriously enough to retain its primary role there.

I hope that cultural anthropology can draw on its links with the rest of anthropology as a means of restoring its role within the social sciences. It would help if cultural anthropology were to address the intellectual world at large and make a stronger case for its venerable holism, and if it were to examine its own practices to assure that it implements holism in fact as well as in rhetoric. It would help if cultural anthropology would raise its voice to an audible level. I think our quiescence accounts for such strange events as the recent publication, in a respected journal, of a learned essay suggesting the culture concept is obsolete in reference to modern Westerners such as today's technological elite (Clausen 1996).

It might help to revive and revise some issues from the 1950s, when culture change was prominent in cultural anthropology's agenda, and when studies of change were important for understanding development and also for providing critiques of ethnocentric modernization theories offered by other social sciences.

Technology and global integration have proceeded apace since the 1950s, and cultural anthropology should have a central role in examining the human conditions of the contemporary world as these are affected by powerful and perhaps ethnocentric trends in political economy. By identifying itself as a cogent, university-based social science, cultural anthropology can be true to its own past and to its character as part of the most synthesizing of the social sciences.

For those of us who think anthropology is the best means now available for humans to acquire informed and shared consciousness of ourselves as human, it is crucial that anthropology remain in the university curriculum, and that it not dissolve. Successful university departments provide a bulwark against disintegration. Considering cultural anthropology in its own right, by strengthening its presence in the American university it might be able to enlarge its minuscule role in national intellectual life. I think it is important for cultural anthropology to be critically engaged with the culture that is its own context, and that the university system provides the best institutional setting for that to occur.

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