

## **A Participatory Conversation with Gerald Berreman (Edited and Expanded)**

*Gerald Berreman, with Mark Pedelty, Interlocutor*

**A Suggestion from Tom Rosin:** “I do hope that some of Gerry’s humorous responses in answering questions at the end of that long day of presentations will be included in the festschrift. I believe this material was all taped at the time. How could there be a Gerry Berreman, without his sparkling humor?”

Berreman, when asked about publishing this, replied: “To the extent that the humor was ‘sparkling,’ I think you probably had to be there!”

---

**Mark Pedelty (Interlocutor):** I remember, as a new graduate student and young activist, going to Gerry’s office with considerable angst, to ask: “What’s it all about? Why be an anthropologist?” Deborah Pruitt says she had asked the same thing but got a different answer. He probably always gave a different answer, because he’s an excellent teacher and mentor who treats students as individuals and establishes human relationships with them. So, he listened patiently—I got the impression he had heard it all before. The answer he gave me was, “It beats working for a living.” And, having worked for a living as a driver for Super Shuttle, I have learned as an anthropologist that he was right—it *does* beat working for a living. I know that Gerry has worked hard as anthropologist, as teacher, but he has infused that work with a sense of effortless joy and humanity that is so different from the humorlessness that often passes for commitment. He has provided an excellent role model of how to keep that humanity, that commitment, that fire, going. With that I will open the floor for the “participatory conversation.”

**Question:** Why the conference title, “Behind Many Masks?”

**Gerald Berreman:** I was totally surprised when I learned that the Conference was to be given that title; I didn’t even expect that it would *have* a title. It’s the title of a very short monograph I wrote shortly after doing my dissertation research. It was my part of a pact among six or eight grad students at Cornell to each write a paper—a chapter—for a book on what we should have been taught about field research before undertaking it, but weren’t. The book never materialized.

Why that title? I've always thought it's a mistake to write an article and then try to think up a title for it. Much better is to think of an interesting title; then think up an article to go under it. Titles with rhymes and alliteration are excellent bases for articles—so are puns; ask Alan Dundes. For example: In the Himalayan village where I did my dissertation research I knew Hindu priests and shamans. I liked their designations, so I wrote an article to justify the title *Brahmins and Shamans*. Ten years later I had to spend a year in India doing a study in the old bazaar of Dehra Dun city, in order to get an article I could title *Bazaar Behavior*. A title that still awaits a book, forty years after my last research in the Aleutians, is the candid ethnography, *Aleutians without Illusions*. Another, inspired by Margaret Mead's *Coming of Age in Samoa*, was to have been *Attaining Adolescence in the Aleutians*.

**Question:** So, what about "Behind Many Masks?"

**Gerald Berreman:** Oh yes, sorry, good question. Actually, "Behind Many Masks" doesn't fit very well into the pattern I've just been talking about, so this is a transition rather than an explication.

The first thing you have to know is that the subtitle of "Behind Many Masks" is "Ethnography and Impression Management in a Himalayan Hill Village." Here—as everywhere—context is everything. I was inspired early on by Erving Goffman's now classic book, *The Presentation of Self in Everyday Life*. "Impression management" was a key concept in Goffman's sociology, and became central to my monograph, which was an account of the facts and foibles of my field work experience. "Behind Many Masks" seemed to me an apt metaphor for the impression management it entailed—and it had a nice alliterative ring to it.

Speaking of Goffman brings to mind an anecdote appropriate to this audience. He was an exemplar of what came to be known as the "West Coast School of Sociology," which grew out of George Herbert Mead's work, Herbert Blumer's symbolic interactionism, Alfred Schutz's phenomenological sociology, Harry Stack Sullivan, and related philosophical traditions of hermeneutic social science, etc. And here comes the anecdote: Eric Wolf used to tell the story of his undergraduate days at City College, New York where he felt intimidated by professors' erudition and fellow students' knowledge. In lectures he kept hearing mention of "hermeneutic," which seemed to be familiar to everyone but him. So he tried looking in biographies of famous people—looking for Herman Nudik. Eric thought he must be some nice Jewish boy who had become famous. And that reminds me that in high school I thought *Ibid* must be a widely cited Latin reference work.

But back to the question, why "Behind Many Masks?" When I first arrived here there was a grad student named Theron Nunez. (Some will remember him. He was mainly George Foster's student, except that he had a great sense of humor.) Theron was intrigued by the title "Behind Many Masks," and often ribbed me about it.

After he had left to teach at University of Florida we occasionally exchanged letters (no e-mail then) in which we vied with one another by inventing similar titles for hypothetical anthropological projects.

Some were very simple, like “The Professor’s Lament: ‘Behind Many Tasks’” and, on the then rapid “Global Expansion of Fast Food Outlets: ‘Behind Many Macs’” or, since McDonald’s had copyrighted “Mac,” the generic, “Behind Many Snacks.” A local California project was to be “A Study of the Christian Brothers Vintners in the Napa Valley: ‘Behind Many Casks.’” “A Comparative Study of Outhouse Architecture” would obviously be “Behind Many Shacks.” The titles we invented ranged from the mundane to the ridiculous, as you can see, but three bordered on the sublime: “Mapping the Distribution of Bordellos in Istanbul” was to be titled “Behind Many Mosques.” Another with merit was “An Ethnography of Sheep Herding in the Pyrenees: ‘Behind Many Basques.’” And, to my mind the topper: “Missionary Projects among Tribals in the Tropics: ‘Mask Many Behinds.’”

Does that answer your question??

**Question:** How did you get into anthropology?

**Gerald Berreman:** Maybe better; how did anthropology get into me?

When I was a freshman at the University of Oregon, my brother Dwight advised me never to take a freshman course—“invariably a waste of time,” he warned. Thinking I might want to be a psychologist or, God forbid, a psychiatrist, I went to enroll in a sophomore psychology course. But they said “no way—you have to start with the freshman course.” So I went to anthropology. They said “yes, yes by all means, take an anthro. course, take the second year course, any course!” So I did. (They were apparently hard up for enrollment.)

The course I took was a year-long cultural anthropology course taught by Homer Barnett. (I still had to pay my dues by taking the freshman course in my sophomore year—a quarter each on cultural, archaeological, and physical anthropology.) Barnett had been a student of Kroeber’s at Berkeley, and had special interests in culture change, psychological anthropology, the Pacific, and applied anthropology. I have never encountered the equal of that course. His lectures weren’t funny, he wasn’t very approachable, and I found out only later that he didn’t think much of the culture concept—except as a useful teaching device—but it was just a great course. One quarter was on ethnography (focussed on the Pacific), one on theory (focused on cultural dynamics), and one on topics (focused on religion and economy). I looked forward to each of his lectures, read all the assignments, was enthusiastic about the papers—I liked everything in the course. Maybe I was naïve, either that or cynicism-challenged. After that course I had no doubt that anthropology would be my major.

There were very few anthro. majors, and only four professors: Barnett; Luther Cressman (archaeologist and Margaret Mead's first husband); Theodore Stern (Linguist and ethnographer of Burmese hill tribes and Klamath Indians); and William Laughlin (fresh out of Harvard as its first Ph.D. in modern physical anthropology—i.e. genetics, etc.). When, after a post-graduate year at Oregon for an M.A., and two years in the Air Force, I turned up at Cornell for my Ph.D., I was told I had taken more anthropology than they had to offer so I could take what I wanted. And I did. Among others, I took the anthropological theory seminar (equivalent to our 240), given by the entire anthropology staff of five (it was a joint department with sociology), and through which the department head routinely slept.

Returning to undergraduate days at the University of Oregon: Among the anthro. majors were Steve Talbot, who did his Ph.D. here at Berkeley (and has taught at U.C. Davis, Oregon State, Delta College, and elsewhere), and Philip Newman, who got his Ph.D. at the University of Washington and is recently retired from UCLA. Both are my lifelong friends. In fact, Phil is sitting right over there. (Raise your hand Phil!) Steve and Phil and I were among about eight others on a 1949 summer's salvage archaeology dig on the Columbia River, sponsored by the Smithsonian Institution, which paid us a princely dollar an hour. Undeterred, most of us became anthropologists, in practice if not in name. Among those you may know are my fellow UC *emeriti* Fred Dunn, epidemiologist at UCSF, and Octavio Romano of our School of Public Health. The latter, by the way, deserves recognition as a key figure in the history of the Kroeber Anthropological Society. As a grad student in the late '50s, enroute to his Ph.D., he revived the then moribund *KAS Papers* and initiated the KAS Annual Meetings. (What ever became of those meetings?)

To my mind, Octavio Romano holds another claim to fame in his grad school years. He had been offended when George Foster gave the pseudonymous name "Romano" to a character in an article on Mexican villagers published in the *American Anthropologist*. So, in an article in a following issue of the same journal, Romano gave the name "Foster" to a gullible, perhaps arrogant, North American rancher who fell for a humiliating practical joke perpetrated by his Mexican employees. They convinced "Foster" that his bunions could be cured by filling his boots with stewed tomatoes and wearing them that way for a week. He followed that prescription, to everyone's scarcely concealed amusement as he sloshed uncomfortably around the ranch. At least that's my memory of the event—you could look the articles up.

Two years after our summer on the Columbia River, in the summer following my senior year and Phil's junior year, he and I were recruited to a six person Aleutian expedition led by William Laughlin. That experience resulted in my M.A. thesis (which was an ethnography of Nikolski village on Umnak Island), and Phil's research report on Aleut material culture there. Both of us also helped excavate the site that was Laughlin's main project. The two summer field experiences confirmed my commitment to anthropology.

Hang on, this story is just beginning! The appeal anthropology holds for me has roots even deeper than those I have described so far.

But first, an aside—*another* aside—actually, an aside *about* asides: There used to be an elderly professor at UC Santa Cruz named Page Smith, who wrote an occasional column for the *San Francisco Chronicle*. I don't know whether he was in the Philosophy or Psychology department—maybe both. He often wrote on aging, and in one of his pieces he said that one thing that happens as you grow old is that everything begins to remind you of something else. That's what's going on here.

Now to the *real* beginning:

I got an early start in anthropology by participating in an ethnographic project during the summer of 1934 when I was 20. . . no, actually when I was four.

Our family spent that summer on the small Grand Ronde Indian reservation in Oregon's Coast Mountains, while my father did ethno-historical research. I have few memories of that summer, among them: an old man with long braids sitting silently on a bench under a tree near the general store; the sixteen-year-old twin girls who baby sat my brother and me while our mother was otherwise occupied. My most vivid memory—aside from the nauseating smell of the hop yards where many of the women worked—is accompanying my mother when she joined them in picking hops on a nearby farm for a few dollars a day. I amused myself by collecting the yellow ladybug-type insects that infested the hop vines. The Indian women would save a few for me, which I kept in a glass milk bottle . . . where most of them died a horrible death.

A year later—1935—I decided to broaden my anthropological training by accompanying my parents on an archaeological expedition to the southern Oregon Coast. My father had earlier made a survey of shell mounds and other occupation sites along the Oregon coast, locating about 40 that seemed archaeologically worth investigating. He then selected one, the Lone Ranch site, to excavate. For many years now the site has been designated a Scenic Wayside on Highway 101, about fifteen miles north of the California state line—five miles north of Brookings.

We camped there for the summer. My brother and I collected then plentiful "arrow heads," scrapers, and stone fishing sinkers on the nearby sand dunes—artifacts remaining from habitation sites that had eroded away—and generally had a carefree time. I remember little of the excavation beyond the excitement that ranch hands displayed when they saw a skeleton *in situ*, and later a rusted-out six-shooter that had been uncovered. Mostly, I remember being told to keep out of the way.

Back to my story: between summers during the Depression, my dad taught grade school and high school in lumber company towns, making \$500 a year while simultaneously pursuing an M.A., as best he could, in anthropology at the University of Oregon. There he one day saw ads on a bulletin board announcing two TA-ships, one at Stanford, in anthropology, and one at Cal, in sociology, each paying about \$850 a year. He applied for both. Homer Barnett got the one at Cal (which my dad would have preferred) and my dad got the one at Stanford, which didn't have anthropology, but beggars couldn't be choosers, so he took it. To finance the move, they sold our cow and my mother took my brother and me to eastern Washington where she picked apples while my dad went to Palo Alto to enroll in his new position and find us a place to live. It turned out that Stanford had only one sociologist, and he was usually at his cabin up at Tahoe, but it had a major psychology department, so my dad took a lot of their seminars and ended up with a Ph.D. in sociology, with a social psychological bent. My mother, with an M.A. from Washington State, enrolled in Stanford's Ph.D. program, where, as a T.A., she shared an office with Sherna Vinograd, who (Berkeley connection coming up!) was destined to be the mother of Telegraph Avenue street poet and "Bubble Lady" Julia Vinograd (who, I might add, has an M.A. from University of Iowa's famed creative writing program). So, my brother and I were grad students' kids, while most of our peers in the Stanford public elementary school were professors' and coaches' kids.

These rarely evoked memories remind me (cf. Page Smith) of another anecdote I can't resist rendering to this academic audience. In grade school a friend, upon being told that my father was a sociologist, asked, logically enough, "what's that?" After I had given my version of an answer, he responded, "I get it, he's sort of a non-religious preacher." My father loved that definition—thought it hilariously apt—and often quoted it in classroom lectures.

My dad's thesis for his University of Oregon M.A. in Anthropology was titled *Tribal Distribution in Oregon*, and was published in the series, *Memoirs of the AAA* (1937). His report on the excavation at Lone Ranch became a short monograph, *Chetco Archaeology*, in the AAA's *General Series in Anthropology* (1944).

I mention these two publications because they entail another Berkeley connection, and an implicit commentary on academic mentorship, or its absence. My father has said that, as a mere grad student, it would never have occurred to him to try to publish either of them. His adviser at Oregon, Luther Cressman, had simply accepted them virtually without comment. But Alfred Kroeber, who was at Berkeley, saw them, liked them, and arranged their publication.

I first met Kroeber at a reception following a lecture he gave at Cornell in 1956, when I was a graduate student. His response when I was introduced to him was to ask whether I was related to Joel Berreman, what was he doing now, why hadn't he stayed in anthropology, and "please give him my regards." Kroeber, who was then

around 80, clearly had an impressive memory and concern for those he had mentored—even if briefly and at a distance. When I met him four years later at the mailbox in Kroeber Hall, he asked me the very same questions. Well, his memory wasn't perfect, but then, as Joe E. Brown says at the end of the film, *Some Like it Hot*, nobody's perfect.

Let's see—where was I? Yes . . . nostalgia . . . how I became an anthropologist. Should I tell you about when I was a Boy Scout? You laugh, but why not? . . . OK, this is actually relevant! When I was a 13-year-old Boy Scout, I was the Den Chief for a group of six or eight Cub Scouts, one of whom was Roger Keesing, age nine. His mother was Den Mother for the group, which met weekly at their house. His father was Felix Keesing, Stanford's first anthropologist and probably the premier anthropologist of the Pacific in his generation. He had no official role in the Cub Den, but he did teach us Maori songs and ritual dances to perform at Cub Pack meetings! When I arrived back in Berkeley sixteen years later, Felix was a regular member of the short-lived Bay Area Anthropological Association until, one morning following its annual dinner meeting featuring copious quantities of wine and food, he dropped dead on a tennis court. Roger was then about to finish a Ph.D. in anthropology at Harvard—or perhaps had recently done so.

Now, if you don't know who Roger was, it only goes to show how transitory fame is. He became a well-known anthropologist and southwest Pacific specialist in his own right. At one point he turned our department down to join the faculty at UC Santa Cruz, which was then scheduled to be the UC Center for Pacific Studies. Not long thereafter, when that Center didn't materialize, he left for Australian National University, where he made his career for many years, before moving to Canada and ultimately dropping dead on a dance floor during an anthropological conference in Toronto, at 58—the same age, almost to the day, as his father had met his fate . . . but that's a different story. *Sic transit gloria.*

So, the Keesings were another early and continuing anthropological contact of mine, though so far as I can tell, they had little if any impact on my career. I describe it here because, despite its minimal relevance, I think it's interesting history.

Wait, here's another Keesing footnote. Bear with me—I'm on a roll. In grade school (1940), Roger and his elder brother John (about 7 and 9) approached me (11) on the Stanford elementary school ground and asked: "are you for Roosevelt or Wilkie?" (Don't tell me you don't know who Wilkie was!) "Roosevelt," I replied. "Good," said John, "that makes three of us; everybody else here's a Republican!" That must have impressed me, because it's the only non-Cub memory I have of them from that era.

**Question:** You have been an activist as well as a scholar of inequality, especially in India. Will you reflect on where things are going in that area? Are you pessimistic or optimistic?

**Gerald Berreman:** Anyone who has taken a course with me probably knows that I often quote Antonio Gramsci to the effect that pessimism and optimism are not mutually exclusive. Specifically, he contrasted “pessimism of the intellect” and “optimism of the will [or spirit].” Actually, I don’t know *where* he wrote it—I’ve never seen it—but I’m confident he wrote it! Someone, likely Bob Murphy, told me he did. I once mentioned this to my friend, Leon Wofsy (bacteriologist/immunologist and political activist professor who was here earlier today), and he told me he knew exactly where it’s written—but he has never turned up with it. The same with Henry Rosenfeld (American anthropologist, classmate of Eric Wolf, long-time courageous opposition activist in Israel, who has worked for many years with Palestinians in Nazareth). He told me he’d send me the reference . . . never did. Even so, I remain confident that he *did* say it. It’s probably somewhere deep in Gramsci’s *Prison Notebooks*. I’ve seen pages where the general idea comes up, but no quotable passage. So, I often quote him—but not in print!

How did Gramsci—or more accurately, given my lack of a quote—how do I describe these concepts, you may ask. It’s like this: “pessimism of the intellect” is the pessimism that comes with understanding the ominous nature of how things work and where things are going in the world. That is, in the words of C. Wright Mills, being in possession of an “adequate definition of reality.” “Optimism of the will [or spirit]” is the belief, fueled by hope, that things just might turn out better than your intellect tells you they will. That is, you see what’s happening, hope something else will happen, have at least a glimmer of belief that it *could* happen, and therefore try to make it happen.

I have occasionally cited, as a non-political example, the case from my college days of a news report of a woman who fell off a cruise liner somewhere between San Francisco and Hawaii. Hours later, when her absence was noticed, the captain retraced his route on that vast, trackless ocean and finally, there she was, dog-paddling around. When asked why, it being such a hopeless situation, she answered, in effect, “Sure, but why not? I had nothing to lose and I thought I *might* be found.” And she was. That’s optimism of the will in the face of pessimism of the intellect. The captain evidently shared that optimism—or maybe feared a lawsuit if he didn’t give it a try.

And I think that’s about the chance we have that things will get better in today’s world! I *do* hold hope; otherwise I couldn’t have persevered in my teaching, my research, my activism. That’s me: intellectual pessimism, and a will to optimism; tentative optimism, but optimism nonetheless.



What will happen in India, you ask? I have no idea. I'm definitely pessimistic, given the current political climate in India and South Asia as a whole. But I also think things may get better . . . not that it's in any way evident! We in the U.S. are certainly not in any position to lecture India—or any other country—on this topic, given the political climate here.

With regard to my activism, I think that if I've had any significant impact it has been mostly through my students, by teaching them to be socially responsible, outspoken, skeptical of conventional wisdom—basically, to practice what C. Wright Mills called “the politics of truth.” As he wrote, “in a world of widely disseminated nonsense any statement of fact is a political act.” I have encouraged them to cultivate what he described as “the sociological imagination”—the “quality of mind” that recognizes the relationship between private troubles and social problems, between individual experience and its social context. I have tried to teach students to apply that critical, skeptical imagination as they view what passes for “news” on television and in the press, and as they listen to words of “authority” and power. Only in that way, I tell them, can they achieve an adequate definition of reality. Postmodernism notwithstanding, I *do* think there is reality in the world and that it can be discerned.

One relatively early example of my activist predilections: When I was in the Air Force (1953-55), I was assigned to a small social science research unit (Human Resources Research Institute, later dubbed Officer Education Research Laboratory) at Maxwell Air Force Base, home of the Air University, in then totally segregated Montgomery, Alabama—totally, that is, except for the recently and putatively desegregated military. I was introduced to my duties by a militant young Black officer, Alfonso Pinkney, who had already been in the unit for a year and was, like me, a Lieutenant with a social science M.A., his in sociology from N.Y.U. We soon became friends and, having little to do in our official duties, and being appalled by the social environment in Alabama, we undertook a variety of investigations of discrimination against Blacks on the base. We regularly prepared “Research Reports” detailing our findings and mailed them off to President Eisenhower. Just as regularly we received curt letters of acknowledgement from a Major Green in Washington, D.C.—we never did know what color he was! After a few months we got no more responses. Then, our commanding officer, one Colonel Barlow, (who had a Ph.D. in Social Psychology from Michigan State) called me in and sternly announced that he had been instructed to tell us this had to stop. No more research; no more reports. Thinking that the Colonel-cum-social psychologist must be experiencing role conflict, I asked, “Are you telling me I can't do it?” After a nervous pause, he blurted, “I'm telling you that I've *told* you, you can't do it!” Pinkney wasn't called in—I guess I was supposed to relay the message—but but he was harassed in more direct and damaging ways.

We had also become involved in the defense of a Black Master Sergeant, who had been court martialed on charges of embezzlement preferred by his commanding

officer, a White Captain. The Sergeant was eventually exonerated after I had solicited the services of a prominent and courageous anti-racist southern White lawyer, Clifford Durr, who defended him *pro bono*. Through careful pre-trial questioning of the defense witnesses, Durr had ascertained that the Captain had framed the Sergeant and was himself the embezzler. The Captain was then court martialed, convicted, and sentenced to 11 years at hard labor in Fort Leavenworth Federal Penitentiary. Talk about poetic justice!

Meanwhile, long before the captain's trial, and clearly as a result of these and other incidents of racial conflict and discrimination, Pinkney had been punitively transferred to the Air Force hospital at Wright Patterson Air Force Base in Ohio, as a psychologically disturbed "racial agitator." I won't take the time to tell you about his subsequent transfer and his passive resistance in his new post, during which he worked on a book-length exposé of the Air Force. I *will* tell you that he eventually, and to his amazement, received a telephone call in his quarters from a secretary who announced that he was to leave. When he asked, "You mean I'm discharged?" The answer was, "Just go!" Taking no chances, he packed his bag, picked up his manuscript and went. Where he went was to his beloved Greenwich Village. There he relaxed in familiar haunts with old friends and completed his exposé. Then one day, carrying his manuscript, he took a walk along the East River, sat on a bench, gave it a final look-over, and threw it in the river. He then retired to the bucolic environs of Ithaca, N.Y., enrolled in Cornell's Ph.D. program in Sociology, and ultimately became a well-known, much-published, and now Emeritus, Professor at Hunter College, and frequent Visiting Professor at the University of Chicago. He never heard from the Air Force again.

Having had experience of the racism of civilian (as well as military) Montgomery via both White and Black friends, I had become intensely concerned with issues of racism and social inequality in general. I had also followed India's Independence movement, the role and non-violent methods of Mahatma Gandhi, and the struggles of untouchables and other minorities for emancipation. I had read the work of sociologist Oliver Cromwell Cox, whose message was that Blacks in America resent and resist their status because the U.S. is avowedly egalitarian, while Blacks are exploited and excluded—in short, denied the rights, privileges, and protections supposedly guaranteed by the Constitution. As a Black American, Cox knew well whereof he wrote. He went on, however, to compare the plight of Blacks with that of India's untouchables. He wrote that untouchables accept their status because Indian society is traditionally and pervasively inegalitarian. Untouchables accept their status, he said, because it is enshrined in the scriptures. So, they wait patiently and willingly for their reward in their next incarnation. He had no first hand knowledge of India, so far as I know. The nearest he came was to read the descriptions and explanations of India's literate elite—the high, powerful, and privileged castes, and their friends.

I thought that he must surely be wrong. Based on what I had experienced with Blacks in Alabama, and what I had read of the circumstances of Untouchables in India, I did not believe that any people would endorse, and cooperate in, their own oppression. I wanted to see for myself.

So, I got a dissertation grant and went to India and lived in a village much of the time for a year. I got to know a number of Untouchables well and many more casually, as well as far more people of the middle and high castes. And, lo and behold, I found exactly what I expected to find. Untouchables' responses to their status were down-the-line similar to those of Blacks in the U.S. Responses of higher castes were down-the-line similar to those of racist Whites in the U.S.

I had gone to India quite sure of what I would find, and I did!

I know what you may be thinking, but the fact is I had evidence—data rather than simply ideology or conjecture. In those days Anthropology was built on evidence: on field research, on participant observation, on empathy. Postmodernism was not even on the horizon.

I like to think I have made a contribution, however small, to the understanding of caste oppression in India and birth-ascribed inequalities and iniquities everywhere. I believe that such understanding has the potential for contributing to their mitigation and perhaps, eventually, elimination. I must say, though, that while there did follow a turnabout in interpretations of caste and its consequences among foreign Indianists and at least some Indian scholars—not by any means as a result of my work alone—there has been a subsequent reversion on the part of many to a view of caste as having been overemphasized. According to this revisionist view, the caste system as we know it was largely a product of the colonial administration and in any case has been largely eliminated. These cannot be scholars who have first-hand experience of the rural 75% of India's population. That view is of a piece with the view that social and economic inequality are in general necessary, inevitable, benign, and even desirable in any society. This is clearly part of the move in India and worldwide, to the political, economic, ideological, and moral right. Power, privilege, greed, and brutality are formidable adversaries. The struggle for equality and justice never ends.

I have described just now some of the early, formative events in my socialization to activism. Now I want to simply list examples of the main sorts of activism in which I have participated, together with many other faculty members and students at Berkeley and elsewhere, from the 1960s to the present. In some cases I will briefly indicate the role I played.<sup>1</sup>

- Cuban Missile Crisis: member of committee organized in opposition to U.S. policy.

- American Federation of Teachers (AFL-CIO): faculty union organizing committee and member.
- Civil Rights: Bay Area and campus activities against racism.
- Free Speech Movement: faculty activist.
- Anti-Vietnam War activism: organized first U.C. anti-war teach-in; member of Faculty Peace Committee steering committee & sometime chair; active throughout.
- Anti-apartheid and divestment from S. Africa: activism until it ended.
- Association for Asian Studies: anti-military complicity and funding activist.
- U.C. Berkeley's Himalayan Border Countries Research Project: Opposed military funding. Succeeded, with help of Senator Fulbright and Government of India, in termination of the project.
- American Anthropological Association, Committee on Ethics: Key role in drafting Principles of Professional Responsibility (i.e., code of ethics), and, with Eric Wolf and others, exposed some anthropologists' complicity in the war in Southeast Asia.
- American Anthropological Association: Nominated as the "Anti-war" candidate for AAA President, 1970 (over opposition of then AAA President George Foster—an inveterate adversary of the Committee on Ethics in that era—who convinced two of the other three candidates to withdraw (in violation of AAA rules) to avoid a split in the "responsible" vote and a "radical take-over" (i.e., reducing the election to a contest between myself and the much senior, more prominent and conservative A. F. C. Wallace—who who won by a margin of two to one). But the nomination and the substantial vote it achieved constituted a moral victory in that it afforded the "radical" minority a degree of legitimacy that others had been loath to grant it!<sup>2</sup>
- U.C. Academic Senate Ad Hoc Committee on ROTC: Member of this diverse committee which unanimously concluded that ROTC lacked both intellectual content and legitimacy as a university department, and therefore had no place on this campus. (Note: While in the Air Force, a major project to which I was assigned was an evaluation of the Air Force ROTC program. In that role, among other things I interviewed ROTC "faculty" and students at University of Washington, Auburn University, and Tuskegee Institute, and participated in a study of ROTC summer programs.)

- U.C. Academic Senate Committee on Academic Freedom: Member active in defense of academic freedom on many specific issues.
- International Commission on the *Tasaday* Controversy (Philippine Anthropological Society) 1986: Member of the commission and a continuing voice in the successful five-year effort to demonstrate that the allegedly uniquely “primitive” *Tasaday* “tribe” comprised a crude (but heavily funded) politically motivated hoax.<sup>3</sup>
- AAA Committee on Ethics member (1969-71) and AAA Executive Board member (1971-74): Active opposition to military advisory roles and similar complicity by anthropologists, through both of these AAA offices throughout these years.
- Anti-Iraq War: activism throughout that war’s duration.
- Peace and Conflict Studies program, U.C. Berkeley: Active in the struggle for a department or program on the U.C. Berkeley campus (ultimately achieved an Undergraduate Group Major, with one Lecturer faculty member and a number of faculty members in various departments teaching courses therein).
- Department of Ethnic Studies: Active in the long fight for establishment and retention of this department, and service on (and sometime chair of) its “oversight committee,” preceding its eventual independent departmental status.
- American Cultures Requirement: Active in implementation of this requirement, which assures that every undergraduate will take at least one course in their Berkeley career which exposes them to a variety of American ethnic groups and cultures other than their own.

All of the above entailed confrontation with powerful forces in opposition on the campus, in the university, in the relevant professional associations, and/or throughout the society or internationally. Most of the efforts described have met with at least a degree of success, and many of the successes have come to be taken virtually for granted.

**Question:** Would you reflect on your career in terms of your fieldwork experience?

**Gerald Berreman:** OK, reflections on fieldwork: Fieldwork has meant a great deal to me. It is certainly the most rewarding part of being an anthropologist. I have learned more from it than I could have in any other way—not just facts and

experiences, but understanding of people and the worlds they live in. I, and I am sure many others, have been rewarded immeasurably by the fact that our research has been “long-term” and “multi-sited,” long before those terms became fashionable. Such research has provided me with lifelong friends: Aleuts, Himalayan villagers, urbanites of Dehra Dun (the Indian city I lived in and studied), urbanites of Kathmandu (and villagers who were their relatives), academic colleagues and students in those cities and elsewhere in those countries, and colleagues and students in many other countries I have visited as researcher, professor, or lecturer. I would not trade my ethnographic fieldwork experiences for any other experience.

Some thoughts specific to my fieldwork: When I had arrived in India and was searching near the foot of the Himalayas for villages suitable for my dissertation research plan to compare culture change in two villages, one near to and the other relatively isolated from Dehra Dun, I encountered a teashop keeper who took an interest in what I wanted to do. He said, “There’s nothing very interesting here. It’s just like the rest of north India. You ought to visit some mountain villages. Those people are Hindus, but their way of life is different from ours.” Intrigued, I followed his suggestion. When I saw the beauty of the environment and discovered the uniqueness of the people and their villages, I knew that was where I wanted to work. Having heard from classmates of the rigors of fieldwork in the dusty plains of north India, I thought it almost a cop-out to work in a place so attractive. Wasn’t misery in the field part of the initiation ritual for an anthropologist? But my decision was made. I took up residence in Sirkanda village which, together with its region, became my long-time research site. I have never regretted it. (Not that the Himalayan physical and social environments don’t have rigors of their own, I hasten to say!)

I return frequently—most recently, together with my wife and colleague Keiko Yamanaka, six months ago. The return is always rewarding. It’s the special reward that long-term ethnographic research holds for the anthropologist: to experience the life histories of one’s friends and informants, and the cultural, economic, political, and material changes that any people, any community, any environment undergoes over time. In another dimension, it is both rewarding and enlightening to recognize, understand, and empathize with people who are superficially very different from those with whom one is already familiar, yet prove always to be fundamentally very similar to those one has known elsewhere and under other circumstances. In short, in fieldwork, one experiences common humanity and unique individuality—the same range that one finds in one’s home community, among one’s relatives, in one’s academic department!

Long-term research also guarantees that one will experience acutely and poignantly the facts of aging and mortality. I have been naively surprised that children I knew now have children—or grandchildren. A generation is emerging that recognizes me, if at all, only as part of local folklore. Always, I suppose, such expectable changes are mutually surprising—to both anthropologist and those who are

informants and friends. Dismay is likely to be concealed through humor. At a recent reunion in my Himalayan field site, my blacksmith friend shouted in greeting that I must have soap in my hair, and I responded by asking when he had become a “four-eyes.” Among my age-mates and older, of course, the numbers grow fewer. I am saddened at each return to discover the loss of friends. But my life has been immeasurably enriched by having known them.

Changes in the material environment and technology are of course conspicuous and to be expected, but these too may come as a shock: a road is approaching Sirkanda, some houses have electricity, a few have television. More children, including, impressively, many more girls than before, are in school; some have gone on to high school, several have become teachers, and others have migrated to cities. Development schemes are intruding in ways that are often damaging and resented—massive limestone and timber extraction, dam building, tourism intrusion—the list goes on.

In short, fieldwork has enriched me and, indirectly my students, in ways and to an extent that I could not have anticipated.

**Question:** What’s next for you?

**Gerald Berreman:** I have no very concrete plans. I look forward to visiting places and peoples that I know about, but have not known experientially. I will definitely continue to revisit those I have known, in South Asia, the Pacific, the Aleutians, Scandinavia, and elsewhere. I don’t intend to embark on major new research, but I will enjoy—in fact already have enjoyed—accompanying Keiko on her research trips to Brazil, Europe, and most of the countries of East and Southeast Asia.

As I contemplate the experience of retirement, the hopes and expectations that come immediately to mind tend toward escapes. Among these is the escape from uninteresting changes (and don’t misunderstand me, many of the changes are interesting) that are occurring in the definition and practice of anthropology. That is, I look forward to escaping responsibility for keeping up with those that depart from what attracted me to the discipline and sustained me in it. But that is no doubt to be expected in the evolution of the discipline, and mine must be the experience, more or less, of every generation.

More pragmatic is the relief I expect to experience at avoiding the direction of change I see in the university, in the academic profession, and in the daily practices and experiences of being a professor here. I’m thinking of the growing impersonality, the rampant bureaucratization, the vast increase in the number of administrators and their Bush-like arrogance of power, together with the drastic diminution of the role of faculty in university governance. The role of the Academic Senate epitomizes this: “lip service, yes; self governance, no” seems to be the administrative motto.

On a less profound note, I will be happy not to endlessly prepare lectures, attend department meetings, file bio-bibliographical reports, respond to every bureaucratic demand the administration can think up, invent letters of recommendation (and I *do* mean invent!). How I have dreaded letters of recommendation; how relieved I will be to escape them. Let me elaborate:

You know, students say: "I need 6 letters of recommendation. Just run them off on your computer from the last one you wrote." But guess what? One is for a job at Harvard, another at Laney College; one is for a grant to do research in London and another in Timbuktu, one is to work for an NGO and another for the CIA. Every student has to be touted a paragon. You have to mark the reference form with "top 2%," "5%," or, in the case of the total dunce, "top10%," if they're ever to get a job. Or how about, "best in 10 years," "5 years," heck, why not just a binary choice, "best ever" or "worst ever?" I've advocated an adjective wheel for use in the qualitative statement—something like those verb wheels you get to help you through French class. Maybe make one where you'd just spin the wheel and insert the adjective that turns up—like Wheel of Fortune. Stanley Brandes, gentle soul, confided to me a few years ago that he hated recommendation writing more than any other professorial task. I said, "I'm so glad to hear that, I thought I was the only one!" That's probably true for most of us.

**Comment from the audience:** I have news for you. Retirement is no escape. Letters of recommendation never go away!

**Gerald Berreman:** Yes, I suspected as much. Gramsci to the rescue! That's the optimism of my will kicking in, as I confront the pessimism of my intellect. I know intellectually that the chances that, post-retirement, recommendations will no longer be requested are vanishingly small, but I cling to that thin thread of hope.

On that hopeful note, I see that I have run out of time—actually, substantially over-run out of time. Thank you all for being here, for staying to the end, which has doubtless seemed to continuously recede toward infinity. I am truly overwhelmed by this whole event. I would say I'm "speechless," if that weren't so obviously not the case!

**Pedely:** We want to thank all of those who planned and executed this event, notably Katie MacKinnon and Kevin Bartoy of the Kroeber Anthropological Society, and Keiko Yamanaka, of the Department of Ethnic Studies, as well, of course, as all of the presenters and session chairs and those graduate students who helped in other capacities. Above all, we are grateful to all of you who have attended this wonderful event, some of whom have come from afar. Let's acknowledge those who have come the greatest distances. The one who has come the farthest is Rosiah Omar, Professor and Dean at the University of Malaya, in Kuala Lumpur. Probably next is Terry



Haynes from Fairbanks, where he is in the Division of Subsistence of the Alaska Department of Fish and Game. Any others contenders?

**From the audience:** How about Rhode Island?

**Gerald Berreman:** No, Lucile [Newman], Rhode Island doesn't qualify—it's too small!!

**Pedelty:** With that, our time is up and we will close this conference and retire to the Atrium in 2251 College, the building that houses the Archaeologists, where a reception with food and drink awaits us all.

---

<sup>1</sup> As those who were in attendance at the conference will recognize, this list has been inserted after the oral "conversation" occurred, have a number of other items and anecdotes in this text. They are included here as a result of subsequent requests, suggestions, and inspirations.

<sup>2</sup> For accounts of the saga of that election and its context see my chapter, "Ethics versus 'Realism' in Anthropology Redux," in C. Fluehr-Lobban's edited *Ethics and the Profession of Anthropology: Dialog for Ethically Conscious Practice*. (2nd edition, Altamira Press, 2003), Pp. 51- 83, and Eric Wakin's *Anthropology Goes to War: Professional Ethics and Counterinsurgency in Thailand*, (Monograph no. 7, Center for Southeast Asian Studies, University of Wisconsin, 1992).

<sup>3</sup> See my "The Incredible 'Tasaday': Deconstructing the Myth of a 'Stone-Age' People." *Cultural Survival Quarterly*, (1991) 15(1): 2-45, and Thomas N. Headland (ed.) *The Tasaday Hoax Controversy: Assessing the Evidence*. Washington D.C.: American Anthropological Assn.