

## M. Brewster Smith

- Born June 26, 1919; died August 4, 2012
- Spouse: Deborah Smith
- B.A. (1939) and M.A. (1940) Stanford University, Ph.D. in Social Psychology (1947) Harvard University



### Major Employment

- Professor of Psychology, University of California, Berkeley: 1959-1968
- Vice Chancellor for Social Sciences, University of California, Santa Cruz: 1970-1975
- Professor (then Emeritus Professor) of Psychology, University of California, Santa Cruz: 1970-2012

### Major Areas of Work

- Social psychology, personality

## SRCD ORAL HISTORY INTERVIEW

### M. Brewster Smith

Interviewed by Barbara Rogoff  
At the University of California, Santa Cruz  
May 5, 2000

**Rogoff:** I'm Barbara Rogoff, and Brewster Smith is here. We're going to be talking about his oral history in relation to human development. Also present, we have about eight folks who are interested in human development from UCSC and University of Michigan, and we are standing in for the audience of the future.

**So Brewster,** maybe you want to tell us a little bit of personal background. I know SRCD is interested in your educational history, where you grew up, and things like that.

**Smith:** Well, I'll try to be fairly brief about this, but of course, being now a senior citizen, I'm increasingly fascinated by some aspects of my early past. I was an only child, and my parents were the first college-educated people in their families. My mother was out of an old New England farming background family. My father had—as his father was the person who became the oldest bank teller in the city of New York, dying on the job at the age of 89, having worked 70 years in the same bank.

My father was a brilliant graduate student at Harvard. He took a degree in comparative philology and I think was Chair of the Modern Language Colloquium when he was there. He went back to his alma mater at Syracuse, and then took a job as Dean of Basic Arts and Sciences at a cow college, Oregon Agricultural College, now Oregon State, where he was in charge of all the various science and liberal arts departments that didn't offer major degrees. The major degrees were agriculture and mechanical arts, home economics and so on, and I think he always felt that he had not lived up to his initial promise in that career.

I was an only child, and I think my parents' relationship was sort of a C-minus marriage of a stable sort, and I was my mother's joy of life, the center of her concern. My parents were Republican liberals in the 1920s. They subscribed to the *Nation* and became enthusiastic New Dealers later. Well, I was the apple of their eye, but I think my mother had much more extrinsic notions, that it was important to be a straight A and so on, and I breezed through school, never having yet really to work at all.

I was started two years early in a neighborhood sort of preschool run by a faculty wife, entered school in the third grade at the age of six. I already knew how to read and write perfectly well. Happily, I was not skipped after that, but I was always two years younger than my classmates in grade school and, as a result of that, never learned the basic athletic skills of the boys, so I felt in many ways marginal and deprived, even though I was an excellent student and teacher's pet and so on. And then, of course, by the time I got to high school, I was still pre-adolescent at the time when being an effective male adolescent was the important thing at high school, so again I felt deprived.

Well, eventually I did go to an excellent school, Reed College in Portland, where the important thing for me was not only the high quality of education, but the fact that I got away from my parents—who were suffocating—and had a chance to go through an identity crisis as an adolescent, which was really formative of whom I became later. Also, it involved some big risks in establishing my own independent identity, that both meant identifying with a Bohemian, pre-hippy clique, getting a foot-hold in downtown Portland with a group of people that were hack writers and would-be artists and involved with a good deal of alcohol, and then also in radical politics on campus, becoming a Young Communist. I never joined the party, but became part of this covert Young Communist League. And I think my adolescent orientation at that point is symbolized by the name on my YCL card. We took pseudonyms, so I put down Stephen Daedalus, which suggests a certain callow-romanticism about all these things.

So anyhow, I began as an excellent student at Reed and ended up flunking over half of my junior year, and also becoming interested in psychology because of some excellent teaching and also because of an older girlfriend who was a psych major. So I busted out of Reed and, thanks to my father's intervention, I'm quite sure, I was provisionally admitted to Stanford if I did all right in the summer session. I did all right in the summer session, more than all right, and I decided that I was really going to do okay at Stanford, so I did okay, and took 18 units all through my senior year and graduated. And since Stanford only took its own record seriously, I became Phi Beta Kappa, probably one of the few Phi Beta Kappas in the country who flunked over half a year's work.

Well, at Stanford I got launched in psychology with important experiences with Jack Hilgard in learning theory—published a paper with him in *Journal of Experimental* on pursuit rotor learning, massed versus spaced practice, et cetera—and a rat study with Calvin Stone on sensory control of maze habits. I thought of myself at that time as particularly interested in social psychology because of my, at that time, fading Marxist background, but was really getting much more interested in personality, I think, largely because of my own adolescent self preoccupations.

Well, after a Stanford master's, I went back on a good scholarship to Harvard and I managed to finish all of my doctoral exam requirements except the thesis before I got drafted in December 1941. I had major experience there with Henry Murray and Gordon Allport as being people who were really inspiring and had quite different approaches to personality theory. Then I entered the Army, and in the Army I had great good fortune in, after basic training, getting into psychological work as an enlisted man in the Air Force doing test development on selection of aviation cadet pilots, bombardiers, and navigators, then going to OCS and eventually getting very, very fortunately assigned to a unit that was doing survey research and mass communication research for the Army. There, eventually, I went overseas to the North African and Italian theatre and had just a very, very important professional experience and felt that I was also being useful in a war, which we—I think none of us were patriotic flag-wavers, but we did take very seriously the importance of trying to win the damn thing.

There I had mentors—people like Carl Hovland and Sam Stouffer—in research I was involved in there, and I got a particularly important informal education from my colleague in work in the field there, Arnold Rose, who was an enlisted man; he'd been drafted the year after I was. I was an officer. He was much more experienced than I. He had been junior author of the Myrdal study, *An American Dilemma*, on the American race problem. He was an all-but-the-dissertation sociology product from Chicago, sort of the central place of modern sociology. So over a year and a half, mainly in Italy, evening bull sessions and so on, I really got quite a thorough education in symbolic interactionism and race relations and Chicago sociology.

I came back after the war to Harvard, had only the dissertation, was very, very fortunate and, well, I spent half a year in writing up stuff on the Army research contributing to volumes on the *American Soldier*. Then I was lucky in working with Jerry Bruner in social psychology and Bob White, the personality psychologist, in close collaboration, being project director on a study of the relationship between individual political opinions and attitudes and the personality context in which a person has formed them. So we were studying the personal basis of public opinion and that eventually resulted in a book, which we brought out considerably later in 1956. Well, that got me launched. I also took advantage of the G.I. Bill, which was providing higher education for veterans, to finance a year of training in psychoanalysis—so I had analysis paid for by the federal government on a G.I. Bill—with Helen Tartekoff, who was one of the training analysts of the Boston Psychoanalytic Institute. I was assistant professor there at the new Department of Social Relations, which was a juncture of social and clinical personality psychology, with sociology and anthropology.

**Rogoff:** So you went from being a graduate student at Harvard to being an assistant professor, right?

Smith: Yes. When I had completed my thesis I had already informally been offered a job as assistant professor for the fall. I did the thesis in six weeks, I think, writing, and it was also my honeymoon. If I'd had more time it would have been shorter. My oral, after I had submitted the thesis, was the scariest thing imaginable, because I think I was the first candidate in the new Social Relations Department to come up for an oral to get the degree, because I'd done all my previous work before the war except for my thesis. So everybody in the department came out for it and the visiting professors—Alfred Kroeber, the anthropologist, was there as a visitor. I knew that I wasn't in danger of failing, but I felt since they had given me the appointment, in fact, I was very much under pressure not to disgrace myself, to do well. Well, I went in there, and I think it came out all right. I was certainly anxious under the surface. I must have learned how to control my anxiety better than I did for my preliminary exams, the written ones at the end of my first year at Harvard, before the war, because at that time the day before the exam I was physically ill from the tension. I remember lying in bed with a wastebasket bucket by my side and retching and cramming notes. So in any case, things were better by the end of the war.

I did get launched at Harvard. I was certainly very anxious about getting established in the academic world. I think I much over-valued Harvard because of my father's relationship to it and felt really quite anxious about getting launched in a research program there. I was stuck with the duty of being Chair of the Board of Tutors and Advisors, which meant running the tutorial program, especially the assignment of people for honors theses to different faculty, and had some interesting experiences there, but I did not really get launched in a research career at Harvard, because, I think, of my anxieties and, therefore, took an avenue of escape as chair of the department and full professor at Vassar.

This step, being Full Professor at Vassar, could have been an absolutely permanent isolation in a backwater, but it didn't work out that way. I moved out of that career, eventually, to a staff position with the Social Science Research Council, which I had been related to, both because they had supported my work on the *American Soldier* research—that had been a SSRC enterprise, they knew about me—and I'd also been on the SSRC Committee on Political Behavior, as a result particularly of my doctoral research, so they knew about me and they invited me to be a staff person.

At that point things at Vassar—I had done my bit there and I was happy to move back into a different kind of mainstream. I was with the Council for four years, in which I continued to get a very strong interdisciplinary education in cultural anthropology and sociology and political science, not economics—perhaps the human relations kind of core of the social sciences. There I was directing a project that involved people at several universities considering the impact of American study on foreign students and what they did when they got back home afterwards.

From there I went to New York University where I was brought in by a person that had been my supervisor and my friend in the Army when I was an enlisted man, Stuart Cook, who was a really very great applied social psychologist, whom I continue to have enormous respect for. And after three years there I was invited out to Berkeley. In the meantime I had earned professional status, I guess, more heavily by editorial roles. I had edited for the Society for Psychological Study of Social Issues their publication *The Journal of Social Issues*, and then after having done that I became editor of the APA's prime journal in the soft areas, which was then *The Journal of Abnormal and Social Psychology*, and sort of the parent of the current *Journal of Personality and Social Psychology* and *Journal of Abnormal Psychology*, so I was doing that. That was a very, very educational experience being a journal editor, but also a rather confining one; you cannot go off for a long holiday without the manuscripts piling up.

At that point, after some time at NYU, I got an invitation to go out to UC Berkeley as Full Professor of Social Psychology and that seemed extremely attractive because I have West Coast origins; it seemed nice to get back. How I got involved with child development is curious, because at Berkeley the Institute of Human Development there had been an organization started by Herbert Stolz way back in the '20s, and Harold Jones became Director several years before I'd joined. He'd been the Director for quite a long time. His wife was Mary Cover-Jones. Mary Jones had done one of the classic studies of conditioning of fears and experimental extinction of such conditioned fears. And she later on did distinguished work of all kinds while she was a research associate there at the Institute.

But the important thing about the Institute had been the conduct of longitudinal studies, two major ones. One, the so-called Guidance Study, directed by Jean Macfarlane, began with children in infancy and was originally to have a guidance group and a control group. In fact, that comparison petered out; being member of the study gave everybody a kind of special kind of status in development and the distinction between experimentals and controls lost importance. It was one of the very few studies taking people from infancy up to adulthood. Harold Jones had started the Adolescent Growth Study some years later, picking up kids in Oakland Schools at the beginning high school level and following them through high school and into adulthood. These studies had been heavily criticized because they seemed to be black holes of money and data collection and nothing issuing out the other side. Shortly before I had arrived there, there had been major investment in reviving these studies, doing follow-ups in adulthood and beginning to do significant analysis and publication.

When I arrived at Berkeley, the Director of the Institute of Human Development was not a developmental psychologist; it was a sociologist, John Clausen. John Clausen was somebody I had known, not very well, but I had known him during the survey research work I was doing in the Army; he was part of the same unit. I gathered, both from talking with members of the staff and just the scuttlebutt going around, that there were really quite major problems in the relationship between John and his staff. He was a very intelligent, very able, scrupulous, decent person, but very obsessive-compulsive in his general orientation and had a very difficult time keeping open communication with his staff. After I had been two or three years in the Psychology Department at Berkeley, John approached me and asked would I be willing to be Associate Director of the Institute. At that point, I don't know—I can't reconstruct just how it was that I saw that as an interesting option, I think it may partly have been because of the difficulty I'd had with my colleagues to get them to agree to strong appointments in social psychology. I remember one particular case, a person who subsequently has been a national star in social psychology, whom I was recommending, and they were unwilling to appoint at a high enough level to bring him because they themselves had not been appointed high, they had to go through this long, steady, slow process of earning their stars. Well, I was unable to get people to make the appointments that I wanted to have made, so this seemed like a route that would be an interesting, different role for me at Berkeley.

Well, in due course, John decided not to continue to be director and I found myself in the role of Director of the Institute, not having been a developmental psychologist at all, and being charged with trying to make things go well there. Now, things were difficult to go well there because Jean Macfarlane, among others, was a fairly tough old gal and a rather manipulative tough old gal, and she, I think, regarded her Guidance Study as being a rather closed personal preserve and not readily coopted

into a coordination of the follow-ups on the several longitudinal studies. There was a third study I had not mentioned—I should have before—Nancy Bailey’s Growth Study, which had been focused initially on development of intelligence in infancy, but again had been followed all the way through with longitudinal data.

There was a good deal of tension among the old gang and some newer people, and it was not easy. For my own role there I had gotten involved in a joint enterprise, which cut across some of the major lines of antagonism. I was collaborating with Jeanne Block, Jack Block’s wife, and Norma Hahn, who had been Jack Block’s collaborator, in a study using Kohlberg’s moral development measures as applied to the student protestors in the FSM (Free Speech Movement) controversies of 1964-65, and also to activists in several groups of trainees for Peace Corps assignment, looking to see whether political activism of one or another sort related to the various stages in the Kohlberg scheme of things. Well, Norma Hahn and Jack Block were in deep, deep conflict about the revisions of what later became *Lives through Time* that Jack published with a little sort of subsidiary note, “With the collaboration of Norma Hahn,” and there had been extreme difficulty in relationship between Jack on the one hand and John Clausen on the other. I don’t think I resolved all these conflicts, but I managed to steer somewhat of an even course in the midst of considerable tension, and the follow-up studies did continue and some important publications came out of them.

The other thing I was doing under the sponsorship of the Institute of Human Development was a second major episode in my own life. Namely, I had the opportunity to follow up the first Peace Corps group to go overseas who were a group that had been trained in Berkeley to go over as teachers in the secondary schools of Ghana. I had been asked to help out during the selection of these people during training and I had a graduate student develop a procedure that we were both very much interested in, asking each of the volunteers in training to write little mock-autobiographies of their lives five years after they got back from the Peace Corps and when they were 40 years old—at that dim distant future of age 40. Rafe Ezekiel, my student, developed a way of rating them in terms of degree of differentiation of the future and degree to which the future was something that was actively brought about by the person, or was it something that a person slipped into, and how much effort was involved, active involvement. And, as it turned out, these measures were moderately predictive of effective performance in their two years in Ghana, whereas psychiatric ratings by Langley-Porter psychiatrists that had interviewed each of them at some length had no predictive value whatsoever; that the measures of readiness to engage with a challenging opportunity seemed to be what these autobiographies gave and was a predictive thing, especially for men, especially for Protestants and Jews. For women it was less predictive, because their autobiographies tended so much to depend upon their expectations in regard to marriage at that point, so it was less of a predictor for Peace Corps purposes.

**Rogoff: They were recruited in 1964?**

Smith: They were recruited in 1961, I think, at the very beginning. They were people who had signed up when there was no existing Peace Corps that they could look at to see what it was. In fact, they got telegrams saying, “Would you be willing to come to Berkeley for training to be teachers in Chana?” It was a misprint in the telegram, so they couldn’t tell whether it was China or Ghana.

**Rogoff: And they still signed up!**

Smith: And they came. I think that in itself was a—

**Rogoff: A predictor.**

Smith: It was a screener for people that were willing to take a chance. Well, that experience with the Peace Corps study was awfully important for my own education, and one thing, it gave me some real contact with a developing country through the eyes of each of the volunteers that I’d interviewed twice. Once at the end of the first year, and once at the end of the second year, and going over with

the graduate student, again the same one, each of us interviewed about half of them for, you know, two, three, four hours apiece, tape recording the interviews and getting around to all the places where the schools were, seeing, realizing that Africans aren't Negroes, Africans are just people. A *Negro* is the American social construction. If you're a liberal as I was, or a racist as I didn't think I was, *Negro* nevertheless is something that has the whole aftermath of slavery involved in it. What we see in Africa, West Africa, was not the aftermath of slavery, but the aftermath of Western colonialism, which is a very different story. Black is simply a people color, and that was something that I think one has to be immersed in a Black society to realize—the extent to which *Negro*, as we used the term back in those times, carried with it a freight of the whole American experience. I, therefore, readily understood why people wanted to change *Negro* to *Black*, or *African-American*, and to try and unhinge this particular unavoidable way of thinking that went with the term *Negro*. Well, it was that experience on my part. Also, it gave me an opportunity to think further about an idea that I had gotten especially from my mentor, Bob White, at Harvard, who wrote a classic article about the concept of competence, "Motivation Reconsidered: The Concept of Competence." It was a paper he published in the *Psych Review* back sometime in the—

**Rogoff: 1959.**

Smith: 1959. Thank you.

**Rogoff: I had to read it for my general exam at Harvard.**

Smith: It was an important paper that challenged the then-existing consensus about drive theories, and links in with more recent concerns with infancy, motivation, et cetera. So as I was looking at what distinguished Peace Corps volunteers who really did a job versus ones who were time servers, I think Bob White's idea of competence, effectance motivation and so on were terribly relevant.

**Rogoff: That was the first effort to turn around the idea that everybody just worked for reinforcement, right?**

Smith: Yes. Well, there were the people that disagreed with that before, I mean the whole Gestalt psychology tradition, I think, didn't like that view, but it was a major reconceptualization that I think had quite considerable influence.

So, that is my launching at Berkeley and my first contact with the developmental people. Let me just plug in a couple of other places where—one example, where I got to know John Modell, whose invitation brought us here. I have a long time association with the Social Science Research Council, which is a private organization dependent upon foundation funding and some government contracts and so on, but is based on members of its Board of Directors who are elected by various social science societies: psychological, sociological, anthropological, historical, economic. I had been on their staff. I have been on several of their committees, and at the time that I was getting involved with developmental psychology at the Institute of Human Development I was appointed to a Council committee on lifespan development in which the key characters were John Clausen again, Paul Baltes, a very, very brilliant chairman who was at that time in the U.S. at West Virginia very heavily involved in gerontological studies, but was helping to define a conception of lifespan development which would integrate gerontology and child development and make it into a lifespan, life course in development. Eleanor Maccoby, whom I have known since her undergraduate days at Reed—we were a year apart in our class. I've known her for over 70 years. Not over 70, over 60 years, not over 70. An awful long time. Ron Lippitt was involved early, he was a social psychologist. Richard Lerner, who is undoubtedly known to you in one capacity or another, and Orville G. Brim, Bert Brim, who is now in sort of a kingpin of a MacArthur Network having to do with aging and development and maturity, but under one title or another there was a succession of committees, which were trying to launch a field of lifespan or life course development. The psychologists tend to call it *lifespan development*. The sociologists like to use the term *life course development* because they saw socially contrived pathways being the thing that was most interesting to them, rather than things happening over different age locations.

Well, anyhow, this committee held conferences and invited papers, educated themselves, and there was a book that I think John Clausen was editor of, *Socialization in Society*, in which I did a chapter on competence and socialization, looking at the research literature, insofar as I could master it, having to do with socialization backgrounds of emerging competence. I organized the conference at San Juan, Puerto Rico, on competence and socialization which fed into that chapter. So there I was really getting into developmental psychology, although not child psychology specifically.

I think now is a good time for you to come up with a question.

**Rogoff: To come up with the next question.**

Smith: I've spieled—

**Rogoff: That was wonderful. You covered a tremendous amount of questions here already. You brought us up to Berkeley. You're at Berkeley, heading the Institute of Human Development, and it's the mid '60s?**

Smith: It is the mid '60s. That's why I've got my CV here, I can look at it. Yes. I became Associate Director in '62-'65, and I was Director '65 to '68, so I was associated with the Institute for around six years.

**Rogoff: Well, I was curious, I don't know whether SRCD will be, but I was curious about all the activity happening in Berkeley in the late '60s and whether you had some role to play in that.**

Smith: I was at the think tank, the Center for Advanced Study in the Behavioral Sciences, at Stanford during that hot year of '64-'65, so I was out, but very much interested. I came back for a couple of faculty meetings I remember during the year and it was—I was much interested in the resistance against the Vietnam War as it was developing and I was—because of considerable personal involvement in the value issues that Jeanne Block and Norma Hahn and I were interested in—studying the people that had been participants in the student uprising at Berkeley. We were sympathetic with what they were trying to do, and I remember also the marching down the streets with the Blacks in some anti-Vietnam protests.

Now I had left Berkeley for Chicago in 1968, where I was Chair of the Psych Department before the worst events happened at Berkeley, where the helicopter spread tear gas over the mobs and so on—that was happening while I was at Chicago, not when I was at Berkeley. But in Chicago we had our own events and they are—there was a lecturer in the Committee on Human Development, Marlene Dixon, who was discharged because, so the committee said, of inadequate research publication, and she became a cause célèbre on the part of the Students for Democratic Society, SDS, who occupied the administration building in a sit-in. And when I was Chair of the Psych Department there in '68-'69, I was appointed to being a member of a sort of blue-ribbon group to look at the problems in the Committee on Human Development in the Dixon case to see whether something could be done. The administration there had taken, I think, a remarkably cold-headed position. They were not going to negotiate, they were not going to bring in the police, they were just going to sit tight.

**Rogoff: Hold that thought.**

Smith: They were going to sit tight and not bring in the police and just wait until the occupying students got tired. Well, meanwhile, our committee was chaired by Hanna Gray, she was later president of the university, historian, a most impressive woman. Our committee was meeting very, very frequently and talking with all kinds of sources, and everybody was at various times exploding in exasperation, but we didn't solve anything and the students eventually gave up and they were immediately fired, kicked out as soon as they left the building.

Well, so I was exposed to that aspect of student activism at that time, and I was at that time highly respectful of the firmness with which the University of Chicago dealt with things, but I felt it was just not the way in which I wanted myself to be lined up. John Wilson, formerly from the Office of Naval Research, was the provost at that time, and he told me if the students make any more fuss there at the college or the University of Chicago, it was his intent to convert the University into something like Rockefeller University, having it solely for the benefit of the faculty and forget about undergraduates. It was at that time that Dean McHenry sought me out and asked would I be interested in coming to University of California at Santa Cruz.

**Rogoff: That's an amazing connection. And you were very interested in helping develop an undergraduate—**

Smith: I said, "If I'm going to be giving—" Well, I also had problems in my own area of social psychology. These two senior social psychologists, Milt Rosenberg and Fred Strodbeck, were much more interested in destroying each other than they were in advancing anything concerned with their program in the University. One was a thickheaded rhinoceros, Fred Strodbeck. The other was a mongoose. They couldn't destroy each other, but they totally made social psychology a disaster. That was my field, again, so there were reasons why I was glad enough to consider coming out here. I thought if I'm going to devote myself to administrative headaches, I'd much rather do them for headaches in a new institution trying to do something new and different than in trying to hold the fort in a Rockefeller-institute-like University of Chicago.

**Rogoff: So I see your reasons for wanting to be away from Chicago. Can you say a little bit more about what attracted you to Santa Cruz, and the sorts of things you were involved in in the early days?**

Smith: Well, when I was at the think tank at Stanford in '64-'65, which was very, very early in the building of the campus, I was invited to a picnic on campus and I didn't realize at that time that it was a recruitment thing. I didn't realize it until afterwards that it was. So I had seen their early foundations, I think, of buildings being started and I think students were already there living in trailers; anyhow, it was very close to the beginning. I remember telling myself at that time, before I ever considered coming here that I had thought it would be a very good place for students to go for the first five years, because it would be an experimental college and new and exciting and different. I think my prediction was that after five years it would probably settle down to becoming something not very different from anywhere else. Having that expectation—since I've come here, I've felt that the campus has done much better than I expected, that its early experimental commitment left a mark of a rather enduring sort, ways of doing things, such that even though we're no longer an experimental college, we are significantly different from other campuses of the University, and other institutions of higher education, and in good ways.

A funny thing—this is of no relevance to SRCD, but they can bear with us for a minute. I knew that when I came I should be associated with a college, not just simply be an administrator with a part-time appointment in psychology, and the college that was being started at that time was Kresge. Kresge was already controversial in that it was started on sort of T-group principles of consensuality and a little bit touchy-feely and so on, and I had been a participant in the humanistic psychology movement, mainly as a critic. I got myself elected President of the Division of Humanistic Psychology, to my great surprise. I've been mainly a critic and I had found myself really quite not at home with most of my colleagues and its leadership. I'd labeled myself carefully a secular humanist, not a touchy-feely humanist. Well, anyhow, I thought that maybe I could be a useful participant in Kresge and that I would learn something from it too. Well, they wouldn't have me. I'm too square, and so I fell back on Stevenson College. I was happy as a clam in Stevenson. I felt greatly pleased that I was not allowed to participate in the Kresge experiment. I'm not myself a very good undergraduate teacher, and I know it, but the closest I have come to the UCSC kinds of teaching commitment was co-teaching with another person. Well, I taught a core course segment in Stevenson and I also taught a course in the Modern Society and Social Thought Program there, which was sort of general social theory for undergraduates, a seminar



that was something I really enjoyed, but I've not really had a great deal to do with undergraduate education.

**Rogoff:** What was your administrative post that you came here for?

Smith: I was Vice-Chancellor for Social Sciences, and that is funny too because it's the same job as Dean, but we couldn't have Deans then because our Chancellor was Dean McHenry, so you had Vice Chancellors. It sounded a little more glorious, but it was Dean.

**Rogoff:** And at that time you helped build the psychology department here?

Smith: Yes.

**Rogoff:** Anything relevant to human development about that history—

Smith: Well, the big things that have happened here, of course, were Catherine Cooper's coming and then your coming, and I can't claim any special hand and these were things that a lot of people had a lot to do with, not me in particular, but I think the thing that I have tried to do, thinking about the graduate programs, is to see human development as being the critical cohesive ingredient in an overall set of programs in psychology, because it includes the social-personal-cognitive span, the whole range of the department in a way, which no other special program does. I have over the years—I think if I were to characterize my focal interests they have been personality and social in the old sort of sense that Division 8 of the APA used to mean. And that includes my role models—Gardner Murphy and Gordon Allport, Lewin—these are all people that saw personality and social as an overall area of human psychology with holistic approaches, who did not let themselves fall into a totally narrow specialization.

Well, I'm wandering again. You were asking—

**Rogoff:** I was wondering if some of what you were doing related to bringing human development on board, and you said that you saw human development as sort of the center of—

Smith: Yes. And in that regard, I also felt strongly that social psychology has a much more natural relationship with—that personality has a much more natural relationship with human development than it does with social psychology as it was developing here. And in fact the experimental social traditions in social psychology featured an idea that came from Lee Ross at Stanford, talking about any attribution that people make to internal dispositions as being the fundamental attribution error, that if you talk about personality you're engaged in the fundamental attribution error. So then, in effect, the ideology going with experimental social psychology is saying that people who are interested in personality are dealing with a non-existent. Well, I think developmental psychologists don't feel that way about it, so I was strongly supportive of that particular re-shuffling of things.

**Rogoff:** Let me broaden that a bit, because I think that the people listening to the tape from SRCD will be real interested in the idea that human development can be the center of psychology, since in many parts of the history it's been treated as something more peripheral. You were President of APA for a while—

Smith: '78. Yes.

**Rogoff:** Did any of this thinking come into your work in APA or in other organizations in relation to human development? We were talking about it in the context of UC Santa Cruz—

Smith: Yes.

**Rogoff:** —but I think nationally it's an interesting question.

Smith: Well, I certainly, in viewing psychology as a whole recently—for example, when one gets involved in talking about perspectives for the new millennium et cetera, and I've, I think, publicly on more than one occasion, said that I think that in terms of the various sub-areas of psychology that I think developmental is one of the healthiest. I think there are problems. Well, I think that as we look at cognitive science, as we look at bio-psychology, as we look at each of the various areas around, we know that we're going to need to have a psychology of a more holistic sort that deals with people, persons in their interrelationships and their group affiliations in ways that can be informative to the practical things that have to be done in human services, education, therapies. There has to be a science, basic and applied, that deals with people as people, not just simply as organisms or as parts of people. I think developmental psychology has that character and that, as one looks at the changes in the field over years—when I began in psychology, developmental psychology was child psychology and it was dull as dishwater. You had Gesell and you had age norms, and it was descriptive, sometimes prescriptive, it gave you no interesting theoretical issues, so I remember avoiding child psychology at the time I was an undergraduate. I think Piaget brought a sense of excitement into the field that only really hit us after the war, although in my senior year and first graduate year at Stanford I remember I had Louis Terman in his last years of teaching, and he was introducing us to Piaget and taking Piaget seriously. I thought that was—

**Rogoff: This was in the '30s?**

Smith: This was in the '30s, yes. Well, it was '38 to '40. So Piaget was known, but the excitement came after the war and it did really revive the field and, of course, since that time I think the other big thing that hit the field is Vygotsky and the social-historical approach that Barbara (Rogoff) is so much involved with. And while there isn't—happily, I think, there isn't a gospel now of what is received truth about these matters, I think there has been genuine empirical progress in what we think we know, and the theoretical issues are interesting ones that make the field an intellectually engaging one to be a part of. So this is, in a sense, a conversion testimonial from me, but I do think that I am a developmentally-oriented person and that a developmental perspective can be extended over to the evolutionary developmental context, it can be extended into a historically developmental context and it can incorporate what I think is sound about the contextualists' emphasis. I think context is terribly important, but I don't like people that refer to themselves as contextualists, because I think they are wanting to discard the search for principles, lawful relationships that cut across contextual specifications. I think contextualists are fine in criticizing attempts at getting at positivistic laws, which ignore context; we've got to look at it first, but if you look at context, one hopes to get beyond just simply groveling in particular contexts, to look at things that kind of cut across and are more general.

I'm very much interested in cross-cultural research. I've not done it, but I'm a consumer of it, and I think it's very important. I also do think, however, that one is always trying to get beyond just simply—there are all these different ways of doing things, all these different values, everything's equally good, yet everything's equally trivial. I'm not a relativist in that sense.

**Rogoff: I was going to ask you about your interest in culture, because I've noticed in some of the things that you've written in recent years that you read that material broadly. Is this something from your Peace Corps years in Ghana? Is this something that's a theme of your work, or would you like to comment on that?**

Smith: I think I've become increasingly educated about the relevance of culture, not just from the Peace Corps work in Ghana, but through my kids also—my oldest son was a war resister during Vietnam. Because he had a Berkeley draft board he was allowed to go into an alternative service project with a group that was putting in potable water supplies in mountain Indian villages in Chiapas. So my wife Debbie and I went down a couple of summers visiting where he was working in Chiapas, going into some of the—they were based in San Cristobal, but the place where he was working that one summer that we went down was in a village where you had to walk—you took a bus to the end of the line and then you

walked about five miles up a ridge and down into a valley, which had no roads into it, and there was the little hamlet where he was helping people pipe in water from a spring so they didn't have to carry it by jug five kilometers, something like that. And the odd thing was, which again gave me a different sense of what anthropologists do and what development is about, there was a school near the farmstead where we were staying, even though there were no roads at all there was a school and they were playing a volleyball game, and there was a PA system announcing it in the Mayan language and not even in Spanish. Also, there was apparently a cantina, perhaps there just above us where they were playing Ranchero music, you know, the corny Tex-Mex music. We had seen a 12-year-old boy on the trail going down the hill with a truck storage battery on his back, carried from a headband, and we learned that they bring the truck storage battery by truck from San Cristobal all charged, the boy carries it over the hill five miles, and then—on the PA system there's a bamboo pole up 20 feet high, and the broadcaster—that's where they get the electricity. The next farmstead, also, we saw a guy with a big bag of—whatever it was, peanuts or whatever, and you know, my son was talking with him. It turns out he had just spent two weeks in Berkeley; he was an informant for the Anthropology Department. And in San Cristobal E.Z. Vogt from Harvard had a base there and there are captive Indians in that base typing out the Ph.D. theses of the Harvard students who were doing their theses.

**Rogoff: Wow!**

Smith: So there's a mixture of—

**Rogoff: A mixture of influences there.**

Smith: Yes. Well, this is again not [relevant for] SRCD.

**Rogoff: Well, the part about you growing interested in culture, I think, is consistent with what I see is one of the major challenges for SRCD for the coming years—**

Smith: I would agree.

**Rogoff: —is to figure out how to broaden our research base and an understanding beyond the populations that had been studied before.**

Smith: I don't think it's a matter of going indigenous, I think it's a matter of respecting the indigenous and then trying to find ways of cutting through that will help us have general understanding. I think Geertz puts it rather well, that one has to approach the etic, or the general, via a careful look at the emic, or contextual or particular, that if one tries to rush into it immediately without looking at context closely you're likely to be kidding yourself and not doing it well.

**Rogoff: Does anyone else have a question or ask for elaboration, or should we turn this off for a moment and just look over our notes and see if there's anything else that—**

Smith: I think that covers most of the things that were asked here that I had anything to say about.

**Rogoff: Anything [the rest of you are] curious about? Yes, Ruth has something. Do you want to turn the tape off for a minute?**

**We turned off the tape for a moment and I asked if there's anything in the more recent years that we might not have touched on.**

Smith: I was realizing that there are some major intellectual concerns that I have, which do touch upon child development too that I have not mentioned. I've been concerned with research ethics for a long time. I was on a task force of the APA that drew up an original document on the ethics of research with human participants. And I've been just involved recently in an attempt to redraft a book no longer prescriptive but educational to try to sensitize the field and our ethical review panels, et cetera, as to

things they need to be concerned about in this time when we have much more, I think, real ethical challenges dealing with AIDS research or drug related research or work with disadvantaged minorities, work involved in genetic issues and so on, than I think it used to be when we were concerned about the ethical violations involved in experimental social psychology with fictitious stories and so on. Now I think it's a lot more challenging and serious, and in that regard I did, back in 1967, write a paper that came out in *Children* and I think also in *The American Psychologist* on ethical issues in behavioral research for children. I think these issues are still important. Celia Fisher at Fordham had a book on applied child psychology and asked me to write a contribution to that on ethical issues, which I did a few years back.

On the more philosophical side, one of my other roles was being President of the Division of Philosophical and Theoretical Psychology of the APA. I have been trying hard over the years to advance a conception of psychology that is humanistic and scientific and socially concerned, and I think the tendency in the field has been to split those things apart. If you're humanistic, you're anti-scientific. If you're scientific, you're anti-humanistic. If you're concerned with advocacy for social issues, again, you're unscientific, and I'm wanting to see that package held together. So in social psychology my heroes would be people like Kurt Lewin and Muzafer Sherif, and I think in developmental psychology I'm still in touch with Lois Murphy, whom I greatly admire. I don't think I heard from her this last Christmas, so—I did hear from her in some length the Christmas before, and she must be in her late 90s.

**Rogoff: Wow!**

Smith: Yes. Well, and more recently, here is a thing that I think does touch upon child and developmental psychology. I have wanted to be responsive to the critiques of positivistic psychology by people that regard themselves as post-modern or social constructionists and so on, but I certainly do not want to throw the baby out with the bath, and I do not want to give up the concerns with evidential empiricism that I think has been traditional with the kinds of psychology that I respect and enjoy doing, so that I've gotten in published argument with Ken Gergen, arguing that the post-modernists of his stripe in effect are taking a nihilistic position that almost anything goes. It's all rhetoric; it's all simply linguistic contrivance.

I think that our scientific conclusions in the human area, like in the physical area, are indeed social constructions. Science is a human enterprise, it's fallible, and we—it's socially constructed, but it's socially constructed under strong constraints, by the way things are. The trick of doing good research, good theorizing is to so develop one's inquiries that these constraints become identified and we therefore increasingly approximate an account of how things are in the physical world, and I think it's possible also in the personal and social world. There are some things that would be historically limited truths, but one can arrive at them not simply by social agreement, social construction; one arrives at them evidentially under the constraint of how things are. The challenge is to be clever at how one identifies the constraints of how things are and shows themselves. I think there is a risk, which I think is a demoralizing impact of post-modernism on humanities, leading to kind of intellectual chaos spreading into psychology by, again, marginal groups, but I think one can be critical of the traditional neo-behavioristic versions of positivism and at the same time preserve the concern about evidential empiricism that we've had in the past.

**Rogoff: That's great. Anybody have a last question?**

Smith: Are we done?

**Rogoff: I think so.**

Smith: Thank you very much, Barbara, and thank you all for being an audience.

[End of interview]