SELECTIONS FROM
ASSESSING CULTURAL ANTHROPOLOGY
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FREDRIK BARTH *is currently Research Fellow under the Norwegian Ministry of Culture and Professor of Anthropology at Emory University. He has previously taught at the universities of Oslo and Bergen, and as a visitor at various American departments of anthropology. He has carried out research in a number of areas, starting in the Middle East with a focus on tribal politics and ecology. His best known works from this period are: Political Leadership among Swat Pathans (1959), Nomads of South Persia (1961), Models of Social Organization (1964), and the edited work Ethnic Groups and Boundaries (1969). Later, he has also done fieldwork in New Guinea and Southeast Asia, and among his publications are Ritual and Knowledge among the Baktaman of New Guinea (1975) and Cosmologies in the Making (1987). A monograph entitled Balinese Worlds will appear in 1993.*

"After a wartime childhood in Norway, I started at the University of Chicago with an interest in paleontology and human evolution. But the active and rich teaching program of Fred Eggan, Sol Tax, Robert Redfield and others broadened my intellectual horizon and led, after an interlude on a dig in Iraq with Bob Braidwood, to my choice of social anthropology as the focus of my work. My foundations derived indirectly from Radcliffe-Brown, who had taught my teachers during the 1930s.

"Like many of my Chicago cohort, I went on to further studies in England. I chose the L.S.E.

and developed a life-long association with Raymond Firth and, even more importantly, with Edmund Leach, whom I later followed to Cambridge for my Ph.D. In the structuralist ambience of the British school of social anthropology of the 1950s and the 1960s, this placed me in a somewhat oppositional role, aggravated by my admiration for Weber over Durkheim and Marx.

"Through Firth and Leach, the influence of their teacher Malinowski strengthened my natural inclinations towards fieldwork. Indeed, my intellectual biography has probably been shaped more by the places I have been than by the books I have read or even the formative teachers I have known. Middle Eastern tribals taught me the turbulence and pragmatics of politics and the powerful constraints of ecology; the diversity of their modes of livelihood challenged me to think about comparative economics; and their situation as embattled minorities on an enormous continent forced me to face the problematics of ethnicity and boundaries. Likewise, the issues of Third World development were impressed on me through my trying to cope with them as a U.N. consultant in the Sudan and elsewhere. At the same time, the practical and intellectual tasks of building up national institutions for social anthropology in my native Norway also affected my perspective on our discipline and made it broader and more critical than it might otherwise have become.

"A growing interest in religion, ritual, and the analysis of meaning took me to New Guinea, where my exposure to a singularly rich and evocative secret cult of fertility was formative in my understanding of how to analyze symbols. My ideas on cultural pluralism were changed by doing fieldwork in Oman. Lately, the challenges of Bali, where my wife and I started fieldwork in 1983, and most recently Bhutan, have provided the impetus again to rethink issues of culture and human action, and the ontology of the powerful continuities and contexts in which people

everywhere in the world are embedded as actors and thinkers."

H. RUSSELL BERNARD *is professor of anthropology at the University of Florida. He has done field research in Greece, Mexico, and the U.S. and on ships at sea. Bernard was editor of Human Organization (1976-81) and of the American Anthropologist (1981-1989). Recently Bernard has been working with Jesus Salinas and other Indian colleagues in Oaxaca, Mexico to establish a center where native peoples can publish books in their own languages. Bernard's best-know contributions are Research Methods in Cultural Anthropology (1988); Native Ethnography (with Jesus Salinas Pedraza, 1989); Technology and Social Change (edited with Pertti Pelto, 2nd edition, 1987); and a series of articles on social network analysis (with Peter Killworth and others).*

"In the summer of 1959, as a junior at Queens College, I went to Mexico to study Spanish and came back knowing that I wanted to be an anthropologist. As an undergraduate, I studied with Ernestine Friedl, Hortense Powdermaker, and Mariam Slater. Late in my senior year, Powdermaker told me about a new Ph.D. program just opening at the University of Illinois. Perhaps I could get in there, she said.

"Illinois in 1961 was an intense, intellectual environment. I studied with Kenneth Hale and Duane Metzger for my M.A. in anthropological linguistics, and then with Edward Bruner, Oscar Lewis, Julian Steward, Dimitri Shimkin, and Joseph Casagrande for the Ph.D.

"Metzger was part of the (then) new ethnoscience camp. The goal was to write the grammar of a culture - to learn what a native speaker of a language knows about, say, ordering

a drink and to lay that knowledge out clearly.

"Making cultural grammars turned out to be harder than anyone imagined. Metzger offered a hands-on seminar. With a few other students, I spent a semester working with one Japanese housewife, learning and mapping the implicit rules she used for deciding how to cut and arrange vegetables on a plate.

"It was an enormous effort just to keep track of the data. One of the other students got the computer to sort and print the whole corpus every time we learned a new rule. The people over at the computer center thought this was pretty quaint, but this systematic approach to data-gathering and the idea of suing computers to make light work of complex data-management tasks have stated with me ever since.

"Ken Hale was Carl Voegelin's student. Like Carl (and like Boas and his early students before him), Ken worked closely and collaboratively with Indian colleagues. The model was to help Indian colleagues produce their own texts, in their own languages, and then to use the texts for linguistic analysis and for cultural exegesis. Ken's example, and the tradition it represented, led to my lifelong collaboration with Jesus Salinas, a Nahnu Indian from the Mezquital Valley in Mexico.

"Jesus was my informant in 1962 when I did the research for my M.A. thesis on the tone patterns of Otomi (called Nahnu in those days). In 1971, I became Jesus's informant, teaching him to write in Nahnu and, in the '80s, to use a Nahnu word processor. I'm still working with Jesus who now heads the Native Literacy Center in Oaxaca, where Indians from around Latin America train in using computers to write and print books in their own languages.

"Much of my career, then, was shaped by my work at the M.A. level. I learned from that

experience how important it is for students to become involved in research projects early and often.

"During my Ph.D, studies, Julian Steward, Dimitri Shimkin, Joe Casagrande and Kris Lehman encouraged me to pursue my interests in quantitative data analysis. In Casagrande's seminar on cross-cultural research, I first learned to use the Human Relations Area Files and to test hypotheses using cultures as units of analysis.

"I don't recall anyone labeling all this 'positivism' in those days, or worrying about whether my interest in scientific, quantitative research was unhealthy. I read works by Tylor, Boas, Kroeber, Driver, Wissler, Murdock, and Roberts and noticed that all of them did quantitative work and published reams of ethnographic work as well. I found this mix of qualitative and quantitative methods to be very sensible.

"My major doctoral professor was Ed Bruner. Ed became identified with symbolic anthropology and I went in a different direction. But Ed taught me to write, and to understand that seeking knowledge was only half the battle. You have to be able to tell others what you have learned, to engage their attention, and to keep them from closing the book before you have finished your argument. This may be one of the few things thing that positivists and interpretivists fully agree on; but for my money, it's the most important thing of all.

"In 1972, I spent a year at the Scripps Institution of Oceanography, where I met Peter Killworth, an ocean physicist. We decided to study problems together that (a) neither of us could tackle alone, (b) both of use agreed were sheer fun, and (c) were not in the mainstream of research in either of our disciplines. We also agreed that we would not let our joint projects get in the way of our separate research careers. (He is in ocean modelling.)

"We did, in fact, have a great time doing a series of papers on informant accuracy and we are having just as much fun now testing a network model for estimating the size of populations that you cannot count (like the number of rape victims in a city). Peter has taught me a lot about data analysis.

"I have also benefited greatly from my association with Pertti Pelto. We began the NSF Summer Institute on Research Methods in Cultural Anthropology in 1987. Stephen Borgatti joined the teaching team of the summer institute in 1988, and I have learned a lot from him about new analytic methods.

"My intellectual biography is still being written. I can look back and see the influences of my professors clearly. But just as clearly, I see the influence of contemporaries, of junior colleagues, and of students. This is what makes anthropology so exciting for me. The learning never has to slow down."

MAURICE BLOCH *is Professor of Anthropology at the London School of Economics and Political Science, University of London. His publications include Placing the Dead: Tombs, Ancestral Villages, and Kinship Organization in Madagascar (1971), Marxism and Anthropology: The History of a Relationship (1983), From Blessing to Violence: History and Ideology in the Circumcision Ritual of the Merina of Madagascar (1986), Ritual, History and Power - Selected Papers in Anthropology (1989), and Prey into Hunter: The Politics of Religious Experience (1992). He has also edited Political Language, Oratory and Traditional Society (1975), Marxist Analyses and Social Anthropology (1975), Death and Regeneration of Life (1982), and, with J. Parry, Money and the Morality of Exchange (1989).*

"During my early years I was brought up under the contradictory influences of French catholicism and French communism. Both of these I rejected but they left me with the conviction, which I still have, that the dominated are more interesting and valuable than the dominators. This, and the influence of a French children's book: Deradji, Fils du Desert, by an author whose name I forgot, about the humiliation of a Muslim boy in the colonial environment, made me decide that I wanted to study the culture of those who had been colonized.

"This made me turn to anthropology. As a student, first at the London School of Economics and then at Cambridge, I was taught within the British anthropological tradition which then owed most to Radcliffe-Brown and Malinowski. I was particularly influence by Firth, Mayer, Leach, Fortes, and Tambiah. I kept reading French anthropology and became swayed by the structuralists and French Marxists.

"In reality, I have learnt much more from two other sources. The first are my friends and students (many of whom became my friends). Among my colleagues the ones who seem to have had the most effect on me are A. Strathern, J. Parry, D. Sperber, and E. Terray. Among my students I can see the influence of the following on my work: J. Kahn, D. Lan, J. Carsten, R. Astuti, and F. Cannell. The second major source of my ideas and opinions are several people in the Malagasy villages where I have worked, their names would mean nothing to readers of a book such as this, but I am well aware that they have taught me much of anthropology and much more."

ROBERT BOROFSKY *is Professor of Anthropology at Hawaii Pacific. He has*

carried out research on Pukapuka, Cook Islands from 1977 to 1981 resulting in the book Making History: Pukapukan and Anthropological Constructions of Knowledge (Cambridge 1987). Borofsky co-edited (with Alan Howard) Developments in Polynesian Ethnology (Hawaii 1989), a volume exploring the current state and future direction of Polynesian studies. He has also edited Assessing Cultural Anthropology (McGraw-Hill 1994),an effort to assess the state of the field through its leading figures perspectives; Remembrance of Pacific Pasts (Hawaii 2000), an attempt to convey the region's past through an integration of indigenous and Western overlapping perspectives; and Yanomami: The Fierce Controversy and What We Can Learn From It (California 2005), an assessment of the Yanomami controversy from the viewpoints of key players in it.

"Having grown up in a family of psychologists, my interest in anthropology only developed gradually. My first encounter with the discipline came as a junior year abroad student at University College, London where, partly by accident, I took courses in the anthropology department. Torn between social work and anthropology for graduate school, I got a master's degree in anthropology at Brandeis and then, because the experience proved unpleasant, went on to teach elementary school for some years and travel. It was only when I got to graduate school at Hawaii that I felt at home in anthropology.

"I have had a number of distinguished teachers. At London, I took courses

from Mary Douglas (who studied with Evans-Pritchard) and Daryll Forde (who, as head of the International African Institute, had strong ties with most of Britain's top anthropologists). In Hawaii, I worked particularly with Richard Lieban (a student of both Steward and Fried), Alan Howard (a student of Felix Keesing) and Douglas Oliver. Oliver, after being an undergraduate at Harvard (where Hooton and Cline influenced him), got his advanced degree from the University of Vienna. But he was most affected intellectually, he claims, by reading the ethnographies of British anthropologists such as Firth, Fortes, Evans-Pritchard, and Malinowski. Firth remains a hero for him (as he does for me) and he was personal friend of Fortes in later life.

"These teachers conveyed two important things to me. They passed on to me a deep appreciation for ethnographies. At London and at Hawaii (particularly with Oliver), ethnographies were treated as something special, as skilled productions that persisted through time despite the frailties of theoretical fashions. (When I returned from my own fieldwork and began sketching out my dissertation, I reread Firth's We The Tikopia and Evans-Pritchard's Witchcraft, Oracles and Magic Among the Azande for inspiration.) And they encouraged me to take pleasure in the crafting process of intellectual endeavors - to work slowly but well.

"At Hawaii, I read widely - not just the major theorists cited by anthropologists today but also other perspectives in French, British, and American anthropology that, while less in vogue, I felt had much to offer. Still, I seemed to be repeatedly

drawn back to the intersections among the works of Weber, Lévi-Strauss, and Marx. I was intrigued by how people constructed meanings about the world around them and how power, in interaction with a range of other variables, played a role in this process.

"Other experiences were also important in my development. Coming of age intellectually in the 1960's, I find myself very much concerned with issues of social activism, power, and democracy. Forty-one months of fieldwork on a small Polynesian atoll had a critical impact. I kept finding new questions to explore, new mistakes to learn from, and new insights to reflect on. It was in the personal interactions with Pukapukans over time, in my seeking to understand how they perceived their traditions and their pasts in relation to how I (and others) perceived them, that I found my love for the discipline. When I returned from fieldwork in 1981, Pacific history was "abuzz" with recent publications by Sahlins (1981) and Dening (1980). Sahlins's Historical Metaphors and Mythical Realities influenced me; Dening's Islands and Beaches inspired me. I have, in some small way, tried to follow in his footsteps with Remembrance of Pacific Pasts.

"Today, my interests remain eclectic. I am deeply committed to teaching as a part of the academic experience and am concerned about the state of graduate and undergraduate education in the United States. I continue to explore aspects of the anthropology of knowing as well as cross-cultural encounters in the Pacific, particularly how they illuminate subtle dynamics of knowing and power.

Pervading these interests and, to a degree, framing them is a continuing commitment to public anthropology and hoping against hope to help facilitate more public accountability for anthropology, especially since the public – through government and private foundations – funds most of its work."

JANE F. COLLIER *is a professor of anthropology at Stanford University, Stanford, California, where she has been teaching since 1972. She has done fieldwork among the Maya Indians of Zinacantan, Chiapas, Mexico and with Andalusian peasants who became urban wage-workers in Spain. Her publications include Law and Social Change in Zinacantan (1973) and Marriage and Inequality in Classless Societies (1988), and two co-edited volumes, Gender and Kinship: Essays Toward a Unified Analysis with Sylvia J. Yanagisako (1987) and History and Power in the Study of Law with June Starr (1989). She is interested in studying systems of social inequality. Her work focuses on how legal and customary norms organize the unequal distribution of wealth, power, and prestige, fostering differentiation by class, gender, and ethnicity/race.*

"I became an anthropologist because, as an undergraduate at Radcliffe, I had the opportunity to participate in a summer field work program in southern Mexico organized by Evon Z. Vogt of Harvard. The Zinacanteco families who hosted me in 1960, particularly the Vaskis family of Navenchauk, changed me from an aspiring archaeologist into an aspiring social anthropologist. Although I never mastered the weaving and tortilla-patting skills they tried to teach me, I learned that I wanted to know more about Maya culture. I returned to Zinacantan the following summer to study courtship customs, and wrote an undergraduate honors thesis

under Vogt's direction.

"It was a privilege to participate in the Harvard Chiapas Project. E.Z. (Vogtie) and Nan Vogt were outstanding leaders, creating an intellectual and social environment that promoted learning and cooperation. Vogtie shared his immense knowledge of Zinacanteco religion while encouraging each of his students to pursue an individual project, and Nan taught us to work together. Through sharing experiences and field notes with other project members -particularly my husband George Collier, Frank and Francesca Cancian, Robert and Miriam Laughlin, John Haviland, Leslie Devereaux, Victoria Bricker, and Stuart and Phyllis Plattner - I learned that knowledge and understanding grow by building upon each other's ideas.

"I trace my present interest in law and social inequality to the coming together of my Harvard training in cultural values with my later participation in the feminist movement. Although I never took a course from Clyde Kluckhohn, all my teachers studied with him. Vogt, my undergraduate advisor and sponsor during the five years I postponed schooling for motherhood, encouraged me to continue working in Zinacantan. When I decided to focus on customary law, he introduced me to Laura Nader at Berkeley who, through correspondence and later in person, generously instructed me in how to collect and analyze case materials. Her article and film on Zapotec judges who "make the balance" inspired me to search for a comparable norm underlying Zinacanteco legal procedures. B. N. Colby, who hired me to work part-time on a cross-cultural study of values inspired by Kluckhohn's classification scheme, taught me to extract norms from ethnographies. And Munro Edmonson, my advisor during graduate study at Tulane, helped me to appreciate the complex, and often contradictory, ideas encompassed within Mayan and Hispanic cultures.

"I became interested in the role of social processes in creating and perpetuating cultural values after reading Fredrik Barth's "Models of Social Organization," which convinced me to focus on litigants' strategies rather than on judges' decisions when writing my dissertation on Zinacanteco customary law. Later, at Stanford, Katherine Verdery introduced me to Ralf Dahrendorf's article on "The Origin of Inequality Among Men," which led me to ask how legal norms create and perpetuate the unequal distribution of prestige, power and privilege. And M. Bridget O'Laughlin introduced me to marxism, an intellectual tradition that had been significantly missing from my education. The excitement of discussing works by Marx and his 20th. century French followers with Bridget, Donald Donham, Michelle (Shelly) and Renato Rosaldo, and George Collier led me to experiment with developing ideal typic models connecting the role of power in producing knowledge with the role of knowledges and ignorances in distributing power. My 1988 book on Marriage and Inequality in Classless Societies proposes models for analyzing groups where age and gender organize obligations, and my current study of Andalusian peasants who became wage workers traces the effects of replacing amount of inherited land with labor market success as the apparent determinant of status.

"Although marxism gave me the conceptual tools to study the relationship between values and inequality, feminism provided the incentive. My growing awareness of gender asymmetry stimulated me to study the creation and perpetuation of cultural concepts of femininity and masculinity. In 1971, Shelly Rosaldo and I, as faculty wives, participated in a six-woman collective to develop and teach Stanford's first anthropology course on "Women in Cross-Cultural Perspective." The following year, Benjamin Paul, the department chair, obtained

affirmative action funds to hire us both as half-time assistant professors. Shelly and I continued to co-teach the course on women,

changing its name and content as our interests shifted from studying women to analyzing the social construction of gender differences. While Shelly wrote her book on Ilongot concepts of self and society, Knowledge and Passion, and I searched for cross-cultural correlations between legal procedures and forms of political organization, we built upon each other's ideas to develop an ideal-typic model linking cultural conceptions of gender to stratification processes in what we called "brideservice societies." Shelly's death in 1981 left a large hole in my life and in Stanford's Anthropology department. But her influence lives on. Sylvia Yanagisako and I organized the conference on "Kinship and Gender" that the three of us had planned. And Sylvia's and my article, reproduced in this volume, develops ideas we shared."

ELIZABETH COLSON *is an emeritus professor of anthropology at the University of California, Berkeley. She has carried out research among a number of different groups including Native Americans, Japanese Americans, and Australians. Her most well-known fieldwork is among the Plateau Tonga and Gwembe Tonga of Zambia and covers a forty year period - from 1946 to 1989. Her research has resulted in numerous publications, among them The Makah (1953), Marriage and the Family among the Plateau Tonga (1958), Social Organization of the Gwembe Tonga (1960), The Social Consequences of Resettlement (1971), Tradition and Contract (1975), Planned Change (1982), and with Thayer Scudder, Secondary Education and the Formation of an Elite (1980) and For Prayer and Profit (1988). She is a member of the National Academy of Science, the American Academy of Arts and Sciences, and an Honorary Fellow of the Royal*

Anthropological Institute.

"Initially, at the University of Minnesota, I studied under Wilson Wallis, who in turn had studied at Oxford University under Robert Marett who was a student of Edward Tylor. Wallis had also attended seminars by Franz Boas. Wallis encouraged a view that anthropology was about humanity at all times and all places (not simply a study of "primitive" groupings). As a graduate student at Minnesota, I also studied with David Mandelbaum, who had been a student of Edward Sapir and earlier of Melville Herskovits, and had been influenced by Clark Wissler. At Radcliffe, where I took my Ph.D., I worked with Clyde Kluckhohn. He had been a Rhodes Scholar at Oxford, under Robert Marett, but had gone on to Vienna where he had worked with Father Schmidt and others involved in the Kulturkreise School. On his return to the United States he moved closer to Edward Sapir as he developed his concern for the interplay between personality and culture and his interest in values. Kluckhohn fostered a recognition for the importance of long-term field studies. Kluckhohn also expected his students to read rather generally - including the work of psychologists, sociologists, and those who became termed "British social anthropologists". (He was then discussing with Talcott Parsons and Gordon Alport the formation of a department of Social Relations, to bring together cultural anthropology, sociology, and psychology.)

"As a member of the New York University Field Laboratory in the Social Sciences for three summers, I received field training from Burt and Ethel Aginsky. They were both students of Boas and of Ruth Benedict, and at that time were participating in the Kardener-Linton seminar at Columbia where the interplay between personality and culture was being explored. Through

the Laboratory's focus on present-day concerns, the Aginskys encouraged me to think of anthropology as a study of action. Another member of the Kardiner-Linton seminar was Alexander Leighton, under whom I worked as an assistant social science analyst at the Colorado War Relocation Camp, better known as Poston. By that time, however, I was moving away from the study of personality and culture to explore more carefully work on social structure, encouraged by Edward Spicer who had also worked with Leighton. Spicer had been a student of Radcliffe-Brown at Chicago and was then at the height of his interest in social structure and functionalism. Spicer gave me a strong impetus toward analyzing activities that people engage in, together with how they phrase these activities, as means of understanding the people themselves.

"In 1946 I joined the Rhodes-Livingstone Institute where Max Gluckman was then director. Gluckman was initially trained in anthropology by Winifred Hoernle (who herself was trained at Cambridge under Rivers) and had gone as a Rhodes Scholar to Oxford where Marett was still the dominant figure in anthropology. Like all of his generation studying in Britain at that time, Gluckman attended the Malinowski seminar at LSE, and when Radcliffe-Brown arrived at Oxford, Gluckman found him more intellectually congenial than Marett or Malinowski. Gluckman was interested in conflict resolution, especially the maintenance of some form of community in potentially disruptive situations, and stressed, as Kluckhohn did, the importance of collecting good demographic data. Revulsion against the situation in South Africa and Nazi activity in Germany and against the general misery of the Great Depression had led him, like many others of his generation, to read rather extensively in Marxist literature. The migration of German intellectuals also meant that the German sociological traditions, including the work of

Max Weber, became part of the intellectual ferment of the time, although Durkheim and his school continued to have more influence on most English anthropologists.

"I spent the year 1947-48 at Oxford University, along with fellow research officers of the Institute (John Barnes and Clyde Mitchell), where Max Gluckman had just become Senior Lecturer, Meyer Fortes was reader, and Edward Evans-Pritchard had replaced Radcliffe-Brown as Professor. It was the high noon of interest in social structure as the dominant concern of ethnography. Students at Oxford during that year included Paul Bohannan, Laura Bohannan, Mary Douglas, and John Middleton. Once a week, I went to London for the big joint seminar run by Raymond Firth of the London School of Economics and Daryll Forde of University College, London. This was considered the continuation of the old Malinowski seminar and was run in much the same fashion. Audrey Richards, Siegfried Nadel, Edmund Leach, John Persitiany, and Kenneth Little were regular and active participants.

"My teaching covers a range of topics, from political anthropology and comparative social organization to comparative religion, migration studies, and the history of anthropological theory. My research has likewise been many faceted, both geographically and in terms of theoretical concerns. But much of it has focused on the consequences of dislocation or the threat of dislocation. I have argued for the importance of longitudinal field studies that can provide a better basis for the anthropological study of process of change and continuity. I am currently at work on several articles examining responses to forced relation and, with Thayer Scudder, am preparing a book on religious responses among Tonga-speakers of Southern Zambia through a century of change."

VEENA DAS *is Professor of Sociology at the Delhi School of Economics, University of Delhi. She is the author of Structure and Cognition: Aspects of Hindu Caste and Ritual, Oxford University Press, Delhi, 1977; and editor of The Word and the World: Fantasy, Symbol and Record, Sage Publications, Delhi, 1986; and Mirrors of Violence: Communities, Riots and Survivors in South Asia, Oxford University Press, Delhi. She has taught in the University of Delhi since 1968. She has held visiting appointments at the University of Chicago, University of Harvard, Amherst College, and University of Heidelberg. She is currently one of the editors of the journal, Contributions to Indian Sociology. She has been awarded the Ghurye Award in 1977 and the VKRV Rao award in 1986.*

"I cannot find any systematic pattern in my development as an anthropologist. I was privileged to be a student of M. N. Srinivas at the University of Delhi. Srinivas had been a student of G.S.Ghurye at Bombay University and Radcliffe Brown at the University of Oxford. Although at Delhi University, Srinivas emphasized the 'field view' of Indian society and opposed it to the 'book view', he encouraged me to use my training in Sanskrit for the construction of sociological problems. That is how I began to work on the lesser known Sanskrit texts of 13th century Gujarat for my doctoral dissertation. Since I had a consuming interest in relocating Sanskrit texts in modern knowledge systems, I found the work of Louis Dumont absolutely fascinating. The structuralist method for the study of myth opened many doorways for me in my early twenties. I used to imagine that Claude Levi Strauss was perhaps a modern day avatar of an ancient or medieval Sanskrit scholar who had to risk his salvation when he chose one theory of language as against another! All this is to say that the ideas of structuralism and later

of narrative analysis were made available to me not only in the texts in which they were contained but also by creating a different genealogy for them in the Sanskritic tradition. In 1982 I explicitly tried this method by posing the mimamsa school of ancient Indian philosophy as interlocutor to contemporary anthropological theories of sacrifice in the Henry Myers Lecture that I was privileged to deliver. I am happy that categories of knowledge emanating in non-Western cultures are engaging the social anthropologists but regret the totalizing frames within which these are formulated.

"Now when I reflect back I can recognize in my intellectual preoccupations the desperate need to escape to a collective past that would overcome the violence all around me. But the immediacy began to press on me and I began to resent the role of anthropology as a purveyor of dreams. Slowly I learnt to engage the problems of my immediate environment. Since 1984 I have been engrossed with the understanding of violence and the way in which moral communities are created through suffering. These concerns stem from some aspects of my personal biography, some contingent events which threw me right in the middle of catastrophic violence, and the advice Srinivas once gave me, that a social anthropologist must feel her way into the environment as an animal feels it - not only as an intellectual preoccupation but as a way of being. How I have been able to translate this vision will be tested in my forthcoming book, Critical Events, where I have tried to show how social anthropology helps us to redescribe such critical events as communal riots during the partition of India, the emergence of sati in modern India, the chemical disaster in Bhopal, by recreating pain as an anthropological object. In this process I have learnt to read classical scholars like Durkheim and Nietzsche as intensely preoccupied with the problem of pain. Although I cannot claim descent from the long line of

venerable scholars who created social anthropology as a discipline, I think the kind of work being done in so called marginal spaces like India and Brazil will claim this tradition and shape its future.

"The community of scholars in Delhi, especially Andre Beteille, Ashis Nandy, J. P. S. Uberoi, Ritu Menon and Upendra Baxi; and elsewhere, Richard Burghart, Audrey Cantalie and Arthur Kleinman have acted as critical anchors in my development. I cannot emulate them in being able to define a central focus to my research for I seem to live intellectually as a completely contingent being. This too, as Kierkegaard said, is a reflective choice."

CLIFFORD GEERTZ *is Harold F. Linder Professor of Social Science at the Institute for Advanced Study, Princeton. He has carried out extensive field research over a forty year period in Indonesia and in Morocco and is the author (or co-author) of thirteen books and the editor of two others. Among his more prominent works are* <u>*The Religion of Java*</u> *(1960),* <u>*Agricultural Involution*</u> *(1963),* <u>*Islam Observed*</u> *(1968),* <u>*The Interpretation of Cultures*</u> *(1973),* <u>*Negara*</u> *(1980),* <u>*Local Knowledge*</u> *(1983), and* <u>*Works and Lives*</u> *(1988). He is a member of the National Academy of Sciences, the American Academy of Arts and Sciences, the American Philosophical Society, and is a Corresponding Member of the British Academy.*

"I had no anthropological training whatsoever as an undergraduate (it was not even taught at Antioch College when I attended it just after World War II), and, economics aside, very little social science of any kind. A philosophy and literature major, the field was suggested to me by

my advisor, a Deweyian philosopher. He knew Clyde Kluckhohn, who was just then getting the Social Relations Department at Harvard underway with the cooperation of the sociologist Talcott Parsons, the clinical psychologist Henry Murray, and the social psychologist Gordon Allport, and thought I might flourish there. I applied and did indeed flourish, studying not only with such anthropologists as Kluckhohn, Benjamin Paul, Evon Vogt, Douglas Oliver, David Schneider, and later on Cora DuBois (who became my thesis advisor), but with various sociologists and psychologists as well. The Social Relations Department was an experiment in interdisciplinary study which was, at least for a decade or two, quite successful.

"My then wife, Hildred Geertz, who was also a student in the Department, and I were offered the opportunity to participate in a group research project in Java, which we did from 1952-54, along with our colleagues Alice Dewey, Donald Fagg, Rufus Hendon, Robert Jay, and Edward Ryan. There was a general division of ethnographic labor in which I concentrated mainly on religion, but pursued a wide variety of other concerns as well. My main orientation at that time, due as much to Parsons as to Kluckhohn, was Weberian, and I was concerned to see whether his "Protestant Ethic" argument could be adapted for reformist Indonesian Muslims, the conclusion being, rather unsurprisingly, that it could and it couldn't.

"In any case, after a couple years back in Cambridge, during which I worked with some development economists at MIT and wrote an analytical history of the "involutional" development of Javanese agriculture, my wife and I returned to Indonesia, this time to Bali, where my main concerns were social organization and the development of the indigenous state.

"Upon returning from this second field trip, I spent the year at the Center for Advanced

Study in the Behavioral Sciences in Palo Alto. It was an "anthropological year" there, with perhaps the largest collection of anthropologists, all of them older and more eminent than I, ever in attendance at one time: Fred Eggan, Meyer Fortes, George Peter Murdock, Cora DuBois, Joseph Greenberg, Melford Spiro, Lloyd Fallers. I spent the following year teaching at Berkeley, but while I was at the Center a number of people from the University of Chicago invited me to join them in founding the Committee for the Comparative Study of New Nations there, and in 1960, I did so.

"The Chicago department, where I spent ten years, was an extraordinarily lively one, with in addition to Eggan, Sol Tax, Milton Singer, Robert Braidwood, Norman McQuown and other senior figures, a number of junior ones such as myself - Robert Adams, McKim Marriott, Melford Spiro, Manning Nash, David Schneider, Lloyd Fallers, Clark Howell - who banded together to revise the core curriculum. What later came to be known as "symbolic anthropology" perhaps first emerged there in full form, and the period was in any case an extremely vital one, critically important to me. While at Chicago I also began fieldwork in Morocco, making three or four trips there, and introducing a number of graduate students into field research.

In 1970 I was invited to be the first professor of social science at the Institute for Advanced Study, where I have been ever since, the only anthropologist on the faculty, though I have been able to invite a large number of anthropologists and other social scientists to come there for a year's research. I am presently in the process of trying to write a book recapitulating not only this rather unstandard career (I have spent, all told, perhaps only two or three years wholly involved in an anthropology department as such, and though I have taught a fair amount, as

much of it has been outside the confines of anthropology in the strict sense as within them), but what I think I have learned from pursuing it.

This has turned out to be a daunting task. Sorting out the "influences" upon one's work, trying to determine retrospectively its general direction, deciding what parts of it seem central, which less so is an invitation to self-deception. I have always been interested in "philosophical" issues, from my undergraduate days forward, but have always wished to pursue them not abstractly but in terms of concrete material, about Java, about Bali, about Morocco, or wherever. The present paper, though in the nature of the case more generally cast, is in that tradition: an attempt to look at critically important intellectual issues from the angle of a working anthropologist.

MAURICE GODELIER *is a professor of anthropology at the Ecole des Hautes Etudes en Sciences Sociales in Paris. In 1990 he was awarded the Von Humboldt International Prize for the Social Sciences. Former Vice-President of the Société des Océanistes, Scientific Director from 1982 to 1986 of the French National Center for Scientific Research (CNRS) and head of its Department of Humanities and Social Sciences, he has done intensive fieldwork in Papua New Guinea among the Baruya and other members of the Anga cultural group. Among his numerous publications:* <u>Rationality and Irrationality in Economics</u>*, New Left Books, 1972 (French edition, 1966);* <u>Perspectives in Marxist Anthropology</u>*, Cambridge University Press, 1977 (French edition, 1972);* <u>The Making of Great Men</u>*, Cambridge University Press, 1977 (French edition, 1972);* <u>The Mental and the Material</u>*, Verso, 1986 (French edition, 1984); Co-Editor with Marilyn Strathern of* <u>Big Men and Great Men: Personifications of Power in Melanesia</u>*, Cambridge University Press, 1991;*

and Editor of Transitions et Subordinations au Capitalisme, Maison des Sciences de l'Homme, 1991.

When I began my studies of Lille in 1951, I had no idea that I would one day become an anthropologists. Philosophy was my passion, but I was also interested in psychology. In 1952, Michel Foucault, fresh from the Ecole Normale Supérieure and having just passed his agrégation de philosophie, came to Lille as a professor of philosophy and psychology. We became friends, and it was he who advised me to go to Paris to continue my studies there.

I entered the Ecole Normale Supérieure in 1955 and began working on logic in Husserl, Kant and Hegel, and then set out to read Marx's Capital from cover to cover. In 1958, I passed my agrégation de philosophie. At this time a debate was raging over the death of philosophy, which many felt could not possibly withstand the development of the sciences and the revolutionary changes taking place in society. I took the stand that what had to die was the presumption that philosophy could by itself discover the foundations of the sciences and social practice. I came to the conclusion that I would need more than philosophy to philosophize about anything and decide to continue my studies. I was torn between medicine and economics, but because of my inter est in Marx and my political activities, I chose economics.

In 1960, the historian Fernand Braudel engaged my at the Ecole Pratique des Hautes Etudes, of which he was president. This was a great opportunity for me. Braudel gave me two years of complete freedom to do as I pleased, and I read copiously.

Ultimately three questions intrigued me: Under what conditions can economic systems be "compared"? What explains their appearance and disappearance at certain moments in

history? To what extent can the Western conception of "economic rationality" be used in comparing socio-economic systems? The last question seemed to be at the center of violent discussions that would break out between economists any time someone wanted to demonstrate the superiority of capitalism and market economy over socialism and planned economy, or vice versa. And it was in search of answers to these questions that I turned to anthropology. It seemed to me that it would be more productive to study economic systems in contemporary living societies organized according to social and cultural logics totally different from our Western models.

It was at that point that a second important opportunity arose. Having just published three articles on the notion of "structure" in Marx's Capital, I sent them to Lévi-Strauss whom I did not know personally. He responded with a note saying that these texts interested him, and all the more because, in his younger days, before his own agrégation de philosophie, he had written an essay on "the Logical Structure of Marx's Capital." He also invited me to call on him, and, when I told him I want to go into anthropology, he suggested I join him at the Collége de France.

In 1962, La Pensée Sauvage had just been published, and in it Lévi-Strauss adopted Marx's position that infrastructures prevail over superstructures, and he presented himself as the superstructure specialist. One day he jokingly suggested that I take infrastructures and research all the anthropological material on the economy of primitive societies. In 1963, I was appointed Senior Lecturer in his department, and I organized the first course in France on economic anthropology. The only French scholar working in the area at the time was C. Meillassoux, a student of G. Balandier. A lively debate was running between substantivists

(Polanyi and his disciplines) and formalists (Firth, Schneider, etc.). It seemed to me that the discussion was headed in the wrong direction. I felt that there was a need to look further and to try to understand why the economy of a society was embedded in kinship or political relations or, on the contrary, non-embedded, as in capitalist economies. It was at that time that I began to see the distinction between infrastructure and superstructure no longer as a distinction between institutions, but as one between functions which could be located in very different areas of social practice. I would develop this view some ten years later in Horizon, Trajets Marxistes en Anthropology (1977), for which I was immediately attacked by Meillassoux, Terray, Kahn and others, who claimed to be Marxists, but who in reality followed Althusser's interpretation of Marx.

I met George Dalton, Marshall Sahlins, and many others on trips to the United States and then return to France to complete Rationality and Irrationality in Economics, which came out after I had left for New Guinea (1966). In it , I concluded that there was no such thing as economic rationality, but that there existed various social rationalities and that the reasons for the succession of socio-economic systems down through the ages were to be sought in the largely unintentional [?] structural changes and conscious reorganizations that had occurred in social relations, which taken alone were not enough to bring about a changeover from one system to another.

One of Marx's ideas that seemed important to me was that the relations that organize the production and circulation of means of subsistence and wealth imply the development of conflicting interests and social contradictions. Another was that ideologies either pass over these contradictions in silence or disguise them as reciprocal exchanges, thereby masking the

domination and exploitation they imply. The Marxist outlook, which stresses material constraints and the evolution of systems, brought me closer to the new school of ecological anthropology which was emerging in the United States with R. Rappaport and P. Vayda and in France with J. Barrau, who had worked with H. Conklin at Yale. Nevertheless, I felt that the ecological approach did not always pay enough heed to the existence of social contradictions and forms of exploitation in the dynamics of systems.

At the end of 1966, I left for New Guinea, after having taken E. Leach's advice and gone to Cambridge to meet A. and M. Strathern, who had just returned from their first fieldwork with the Melpa. On my way to Australia, I stropped in New York to consult M. Meggitt and R. Rappaport and then in Canberra to see W. Stanner. My first stay lasted three years (1967-1969), as I wanted to combine a quantitative and qualitative approach, and the size of the Baruya group (approximately 1700 persons) seemed to allow this. In 1969, M. and A. Jablonko came to film the production of salt and other aspects of Baruya life; and Ian Dunlop came to film the big male initiation ceremonies which were held in October-November of that year. Analyzing these images with my Baruya friends, translating the dialogues and then presenting the Baruya with copies of the films were very important experiences in my life.

The time I spent with Baruya profoundly altered my theoretical views, not to speak of my own self. I was struck by the fact that the myths and rituals worked systematically to raise men and lower women in people's minds by a series of imaginary explanations that justified each gender's place in society. I was also struck by the fact that male domination always combined two forces: the use of physical, political and symbolic violence, of course, but also the women's own consent to this domination. Therefore, instead of writing the usual monograph dealing

with the economy, kinship, etc., I decided to describe Baruya society from the standpoint of male-female relations, which seemed to be the centerpiece - whence The Making of Great Men (1982). It was the Baruya, then, who got me interested in analyzing gender relations, or at least made me see and hear more clearly what was going on around me in my own society. Paradoxically, this also led me, beginning in 1974, to spend much more time reading about hunter-gatherer societies, as these were being tapped by feminist and anti-feminist movements in anthropology to prove either that male domination had always been the universal rule or, on the contrary, that it had only come about with the emergence of more complex types of society and social stratification. In 1978, I organized a meeting on "Hunters and Gatherers", the results of which appeared as Politics and History in Band Societies (edited by Eleanor Leacock and Richard Lee, Cambridge University Press, 1982). My acquaintance with E. Leacock influenced me all the more perhaps because I did not share a number of her views.

My work among the Baruya also had theoretical consequences for me as a specialist on Melanesia. Twenty years ago, many anthropologists looked to M. Sahlins's model of Melanesian Big Man vs. Polynesian Chief when analyzing the forms of power they encountered in New Guinea. Unfortunately, try as I might, I could not fit the Baruya into this mold. Nowhere did I find a Big Man amassing wives and wealth and trying to outdo others with gifts and counter-gifts in ceremonial exchanges. The prominent men in Baruya society were the masters of the male initiations, great warriors, shamans, in a word, persons whom I chose to call "Great Men", to distinguish them from the others. This led me to scour my colleagues' work on some 15 New Guinea societies to see if this distinction between Big-Men and Great-Men societies occurred elsewhere. I had a hunch that these were two poles of a set of social

and cultural logics whose variations were not simply the product of chance. In 1987, M. Strathern and I organized a meeting on "Big Men and Great Men: Personifications of Power in Melanesia."

But there was one problem that kept cropping up: while the power to dominate always combines violence and consent, for this consent to exist, in some way dominators and dominated, exploiters and exploited must share the same representations. This plus the conviction that there were no direct links between a given mode of production and any one kinship system or religion - Christianity, for instance, and capitalism, which appeared 15 centuries later - led me to attempt a synthesis in The Mental and the Material. It no longer seemed possible to make the economic sphere the general foundation of society and thus the primary key for analyzing forms of society, as Marx had tried to do. It now seemed to me that two force fields, economic relations and relations of power, entertained structural affinities which were more than simply the effect of reciprocal adaptations and which outweighed all other areas of human practice (art, kinship, etc) in the processes which not only induce societies to change but to change into another type of society.

Bearing all this in mind, I have, for the last ten years, been exploring two directions . One the one hand, I have been looking at the process of subordination and disintegration/reproduction in peripheral societies subjected to the expansion of Western capitalism. And, on the other hand, I have returned to the classic study of kinship. Why kinship? Once again the story begins with my work on the Baruya. The feeling that there must be some connection between the existence of a kinship system based on the direct exchange of women and the existence of a collective political and symbolic organization - male initiations -

uniting the men against the women incited me to compare the kinship systems found within New Guinea, first of all, and then further afield. For the last three years I have been going through numerous works on kinship with the intention of one day writing something on "Incest, Kinship and Power(s)," and I must admit that I find many of the widely discussed theories unconvincing.

WARD H. GOODENOUGH *is Emeritus University Professor of Anthropology at the University of Pennsylvania, where he has served on the faculty since 1949. His books are Property, Kin, and Community on Truk (1951), Cooperation in Change (1963), Description and Comparison in Cultural Anthropology (1970), Culture, Language, and Society (1971, 1981), Trukese-English Dictionary (with Hiroshi Sugita, 1980, supplementary volume 1990), and the edited volume Explorations in Cultural Anthropology (1964). He was editor of the American Anthropologist 1966-1970, president of the American Ethnological Society (1962), and President of the Society for Applied Anthropology (1963), and on the board of directors of American Association for the Advancement of Science (1972-75). He is a member of the National Academy of Sciences, the American Philosophical Society, and the American Academy of Arts and Sciences.*

"Up to my senior year at Cornell University, my studies were in Old Icelandic and the Icelandic saga literature, Germanic languages, Latin and Greek, and history. I turned to the social and behavioral sciences in my senior year, taking a course in cultural anthropology from Lauriston Sharp and in personality theory from Leonard S. Cotrell.

"From Cotrell's course, I learned that our sense of self - cognitively, kinesthetically, and

affectively - emerges from our experience of ourselves as objects in interaction with our environment, most importantly other people. It emerges, that is, out of the transactions that take place in "self-other" relationships. From Sharp, I learned that what anthropologists call culture is learned, not transmitted biologically. It, too, emerges from and is sustained by the transactions that take place in "self-other" relationships. Culture and an individual's sense of self, I saw, were in some way products of the same processes. It was also evident to me that while theory was able to say something about the processes by which a sense of self and culture come into being, we were still without any good procedures for describing the content of either one. Here was a challenge.

"In my first year of graduate study at Yale, I took courses with Bronislaw Malinowski, John Dollard, Irving Rouse, Clellan Ford, George P. Murdock, and Wendell Bennett, and worked under Murdock on the Cross-Cultural Survey. I got a good exposure to Malinowski's ideas and to the ways that behavioristic psychology and psychoanalytic theory might be applicable to anthropology's holistic approach to the study of human nature and human phenomena. That year, also, I had a course in phonetics and phonemics from George Trager. This course was an inspiration to me in that it seemed to offer an answer to the question that had emerged from my studies the year before at Cornell: how to get at the content of culture and of what is learned in the course of social interaction. I saw that linguists had developed fairly rigorous procedures for arriving at testable hypotheses about the content of what people had learned and knew subjectively in regard to the language they spoke. It seemed to me that if we could put the linguists' approach to content together with the social and behavioral psychologists' to process, we had the possibility of creating a productive theory of culture and also making a

significant contribution to the science of human behavior. I have been at work chipping away at trying to realize this possibility ever since.

"In Word War II, I was for three years with the field staff of the Research Branch of the Army's Information and Education Division. There I got a grounding in the methods of attitude and opinion research and learned Guttman scaling. (My first scientific publication in 1944 was on a technique for doing such scaling.)

"When I returned to Yale, I had a rewarding apprenticeship with Murdock as his research assistant when he was writing Social Structure. Ralph Linton, then also at Yale, had considerable influence on my thinking, as well, stimulating me to try to put together his formulation of "status" and "role" with what I had learned about Guttman scale analysis. His work with Kardiner on "culture and personality" added to my growing interest in the role of culture in the interactive processes leading to the construction of a sense of self. I was fortunately able to continue to develop my thinking on these matters after joining the faculty at the University of Pennsylvania in 1949, where I learned much from A. I. Hallowell and Anthony Wallace. My wife, Ruth Gallagher Goodenough, herself trained in social psychology, has also been an important continuing influence in my work. Pervasively influential has been my father, Erwin R. Goodenough, a noted historian of religion, who contributed greatly to theory and method in the study of religious symbols in his Jewish Symbols in the Greco-Roman Period.

"In my forty years at Penn, I have continued to work at how to describe the content of culture and at how we are to understand the human processes from which culture emerges and which affect its content in the course of time. In this, my methodological approach, following the example of linguistics, has been to develop data bases of specific cases sufficient to reveal

patterns in events and to suggest the criteria with which people in specific groups appear to judge whether or not behavior is acceptable and appropriate. My theoretical approach had been that human behavior is largely aimed at accomplishing purposes, taking care of wants and needs, not only in regard to physical survival but also to social relationships and, perhaps most important of all, to emotional well-being. My primary concern has been to contribute to our understanding of how particular human groups work as expectation-governed systems, whether the expectations involve speech or other kinds of activity. It has been my premise that the better we understand how such systems work, each in their own terms, the better we shall be able to understand what is common to or underlies all such systems and thereby what it means to be human. In all of this, I have, of course, been far from alone and owe much to more people than there is space to mention here."

JACK GOODY *is a Fellow of St. Johns College, Cambridge and held the William Wyse Chair in Social Anthropology at the University from 1972-1985. He has carried our fieldwork extensively among the LoDagaa and Gonja of northern Ghana, and briefly in Gujarat, as well as doing survey work or particular studies in other parts of the world (e.g. flowers in South China, 'may' in the Lot, and cemeteries in the U.S.A.). His best known publications are* Death, Property and the Ancestors *(1962),* Technology, Tradition and the State in Africa *(1971),* The Myth of the Bagre *(1972),* Production and Reproduction *(1976),* The Domestication of the Savage Mind *(1977),* Cooking. Cuisine and Class *(1982),* The Development of the Family and Marriage in Europe *(1983),* The Logic of Writing and the Organization of Society *(1986),* The Interface between the Written and the Oral *(1987),* The Oriental, The Ancient and the Primitive *(1990) and* The Culture

of Flowers (1993).

"What is my intellectual genealogy? It differs depending on the topic. Topics can rarely be encapsulated by the word 'anthropology' taken in its academic sense. My initial training at Cambridge was in English Literature, though I was also interested in history and in politics. Already before going to the University, I was attracted to the moral and cultural analyses of F. R. Leavis and his associates. At this time I read around the sociology of literature and took up references to the 'Cambridge anthropology' of Frazer from studies on medieval literature.

"Returning to Cambridge after the Second World War, I switched to reading anthropology, having been stimulated by the wider ranging historical studies of Gordon Childe and the social psycho-analytic work carried out by the Tavistock Institute of Human Relations. Once there, I was influenced by the lectures of Evans-Pritchard but more by my fellow students (especially G. Lienhardt and E. L. Peters, who followed him to Oxford).

"Later, when I joined them there, I was supervised by Fortes and got to know the other past and present members of the department - M. Gluckman, P. Bohannan, M. N. Srinivas, L. Dumont, and J. Peristiany, many of whom looked up to the work of Radcliffe-Brown and, to a lesser extent, Malinowski. Behind Radcliffe-Brown lay the nineteenth century tradition of jurisprudence, Maine and Maitland in England, as well as the remarkable school of the Scottish enlightenment, culminating in Robertson Smith. Above all, it was the works of Durkheim and other members of the Année Sociologique that constituted the core of our reading, though I also became familiar with the writings of Talcott Parsons (with whom I later took a seminar) and through him of Weber. Marx and Freud were not central to our teaching but they played a

large part in life as a whole. I have elsewhere expressed my debts to French sociology, to Parsons, Shils, and Homans, as well as those influence by the early "Cambridge anthropology" of Sir James Frazer, Jane Harrison, E. K. Chambers, and Jessie L. Weston.

"I did not set out to become an anthropologist in the usual sense of the term. A major interest has always been in trying to place the cultures and societies I know in the framework of the wider history and distribution of mankind. Essentially, I have been heir to the same set of interests in history and literature that moved my contemporaries, Raymond Williams, Eric Hobsbawm, E. P. Thompson, Ian Watt and a host of others, some of whom, like Godfrey Lienhardt, Kathleen Gough, and Peter Worsley, also made the move from "literature" to "anthropology" in an academic sense but always retained an interdisciplinary (i.e. human) concern with the world as a whole."

MARVIN HARRIS *was a member of the faculty of Columbia University in the Department of Anthropology from 1953 to 1980, and chairman there from 1963 to 1966. Since 1980 he has been Graduate Research Professor of Anthropology at the University of Florida. He has carried out field research in Brazil (most recently - 1992 - in Rio de Contas, Bahia, which he first visited in 1950); Mozambique; India; and East Harlem, New York City. He has also conducted field training programs in Brazil and Ecuador. Of his 17 books, the most influential are* <u>Patterns of Race in the Americas</u> *(1964);* <u>The Rise of Anthropological Theory</u> *(1968 - designated a "Social Science Citation Classic" in 1991);* <u>Culture, People, Nature</u>*:* <u>An Introduction to General Anthropology</u> *(6 editions since 1971);* <u>Cultural Anthropology</u> *(3 editions since 1983);* <u>Cows, Pigs, Wars, and Witches: Riddles of Culture</u> *(1974);* <u>Cannibals and Kings: The Origins of Cultures</u>

(1977); Cultural Materialism: The Struggle for A Science of Culture (1979); America Now: The Anthropology of A Changing Culture (1981); Good to Eat (1985); and Our Kind (1989). These books have been translated into a total of 16 languages. Harris is past chair of the General Anthropology Division of the American Anthropological Association and Distinguished Lecturer for 1991.

"I committed myself to anthropology after taking Charles Wagley's four-field introductory course at Columbia College. Wagley was my fieldwork supervisor, chair of my doctoral committee, and life-long friend. Under his tutelage, I became a good eclectic Boasian particularist-relativist and shared his rejection of Steward's cultural ecology approach. Although I took a course with Steward, it had little effect on me until several years later (perhaps because he was ill and absent most of the time). Being a Boasian in those days however, meant being a positivist - the image of Boas as the consummate scientist was an article of faith among his intellectual heirs at Columbia such as Bunzel, Lesser, and Mead. Courses in Skinnerian psychology and independent readings in logical positivism also helped to inspire my interest in convincing others that anthropology was or should be a science. After 1953 when I began to teach and shared an office with Morton Fried, I was attracted by Leslie White's critique of the Boasians and by White's emphasis on energetics. What bothered me about White, however, was that he had not actually measured energy inputs and outputs, a fault I intended to remedy when I next went to the field, but never did.

"Alfred Kroeber was and continues to be a very important role model for me. He twice taught at Columbia, first when I was an undergraduate, and again when I was in graduate

school. As an undergraduate I wrote a paper for him that showed why neither he nor Leslie White understood the logico-empirical operations that validate the abstractions we call "culture" and related "superorganic" entities. (The paper foreshadowed The Nature of Cultural Things, 1964. Most anthropologists still remain incapable of supplying an epistemologically sound operational model of "culture".) I admired Kroeber for his erudition, especially his four-field approach as embodied in his masterpiece, Anthropology (1948) which was the inspiration for my own (1971) foray into four-field textbook writing.

"The big change in my intellectual outlook came about as a result of the field trip to Mozambique. While I had hoped to carry out research on energetics in Bathonga households, as well as to trace the history of changes since the beginning of the century using Henri Junod's ethnography as a base line, my most important objective was to compare race relations in Brazil with race relations in another part of the past and present Portuguese empire. All my intentions were overwhelmed by the reality of Portugal's system of colonial apartheid and labor exploitation. The discrepancy between what people were doing and what they were saying made the problem of subjectivity and objectivity, of mental and behavioral events, and of emic and etic perspectives (words which I did not yet possess) altogether inescapable. Furthermore, the more I learned about the difference between race relations in Mozambique and Brazil, the clearer it became that different systems of production and of exploitation of labor accounted for much of the divergent evolution of Africa and the Americas. Portuguese or Anglo-Saxon values and traditions had little to do with it but were themselves largely dependent on what I later called the demo-techno-econo-environmental infrastructure. Finally, the Mozambique experience made it clear to me that, as an anthropologist, I could not escape the moral

consequences of a feigned relativist neutrality.

"Returning to Columbia, I wrote a pamphlet for the American Committee on Africa, Portugal's African Wards (1958), describing the operation of the Portuguese Indigenato (plus other articles). At the same time, I read Marx, Plekhanov, and Kautsky with new insight. I realized that Marx was the common ground under Steward and White although both had shied away from public acknowledgements in deference to the anti-Marxist terror that reigned in academia during the late 1930s and early 1950s. Another crucial figure for me in the early 1960s was Karl Wittfogel, who despite his rabid and treacherous anti-communism, was responsible for Steward's treatment of ecological factors in the rise of early agro-managerial states.

"Before Mozambique, my scientism had existed apart from the special epistemological and theoretical principles necessary for establishing a positivist materialist (non-dialectical) paradigm in the social sciences; after Mozambique, all the components came together. The basic materialism came from Marx and B. F. Skinner; the importance of economic factors also from Marx; the overall evolutionism from White; and the environmental and demographic foci from Steward and Wittfogel (but ultimately of course from Darwin and Malthus).

"In 1963 I presented my first paper on the material conditions that select for Hindu India's beliefs about cattle. And in 1964 Patterns of Race in the Americas appeared along with The Nature of Cultural Things. While teaching the "History of Anthropology" course at Columbia, I began to assemble the materials for The Rise of Anthropological Theory in which I coined the phrase "cultural materialism" and presented it as a specifically anthropological alternative to all forms of idealism and to dialectical, Stalinist, and anti-positivist as well as biological reductionist

forms of materialism. I have sought ever since to refine and improve the formulation of cultural materialist principles by testing them against a widening corpus of puzzles ranging from food preferences and avoidances to changes in U. S. family structure, and most recently, to the collapse of Soviet and East European state socialism.

"My plans for the near future include a renewed effort to confront the elitist, obscurantist, and nihilist posturing of post-processual and post-modernist anthropology, not as an end in itself, but as part of an attempt to gain for anthropology a more central intellectual and applied role vis-a-vis the great issues of our times such as the ecological consequences of the spread of consumerism and capitalism; the rise of country-less global corporations; the resurgence of ethnic and racial chauvinism; the deepening poverty of the poor; and the prevention of war."

ROGER KEESING *is professor of anthropology at McGill University in Montreal. From 1974 to 1990 he was professor of anthropology in the Institute of Advanced Studies, The Australian National University. He has studied the Kwaio of Malaita, Solomon Islands, for thirty years, and has also done field research in the northwestern Indian Himalayas. His eleven books - including Kin Groups and Social Structure (1975), Lightning Meets the West Wind: The Malaita Massacre (1980), Cultural Anthropology: A Contemporary Perspective (1981), Kwaio Religion (1982), Kwaio Grammar (1985), Melanesian Pidgin and the Oceanic Substrate (1988), and Custom and Confrontation: The Kwaio Struggle for Cultural Autonomy (1992) - and almost a hundred published papers cover a wide range of topics from cultural theory, social structure, religion, and symbolism, and cognition to colonial history and linguistics.*

"Growing up in an anthropological family, I had childhood experiences of fieldwork; but I discovered anthropology intellectually only as a Stanford undergraduate, after beginning in literature. George Spindler, Bernard Siegel and my father Felix Keesing introduced me to the field; and I studied with Gregory Bateson, who remained a friend and strong intellectual influence in later years.

"I began graduate study in the Harvard Department of Social Relations with Clyde Kluckhohn and Douglas Oliver in 1956-57. When I returned in 1960 after three years as an Air Force officer in Turkey, Kluckhohn had just died, and the new excitement at Harvard centered around British kinship theory in the style of Needham and Leach (introduced by the newly arrived David Maybury-Lewis) and cognitive anthropology ("ethnoscience"), introduced there by Charles Frake and represented in the work of Goodenough and Conklin.

"These two streams of influence channeled my thinking when I set off for Solomon Islands fieldwork in 1962. I had been interested in Central Asia after my experience in Turkey, but political difficulties of the Cold War era made fieldwork there unrealistic. The choice of the Solomons was inspired by Douglas Oliver: "You should find a culturally traditional Melanesian society, the way I did, and do really tough fieldwork while you're still young and resilient physically: you can study some comfortable place when you get older." (I think about that as I scale jungle cliffs and sleep on mud floors back in Kwaio country in my fifties.)

"I intended a synthesis of cognitive and kinship perspectives in studying the cultural code whereby decisions were made regarding marriage, residence and kinship - in a society where "non-unilineal descent groups" (a burning preoccupation of anthropologists in those days) seemed likely to be found. The Kwaio as I encountered them in the 1960s, strikingly

conservative culturally, proved ideal for such study. (Although they were reputed to be wild and dangerous, the Kwaio have been enormously supportive and hospitable.) My PhD dissertation and a number of papers written in the late 1960s and into the early 1970s explored these themes. By this time I was teaching at the new, experimental University of California at Santa Cruz, where undergraduate students I introduced to anthropology included a dozen who went on to get PhD's.

"By early 1970s I had become politicized by my students, and my interests shifted from cognition and social structure to more global and political interests, including a belated self-reeducation in Marxism and social theory.

"My move to a Chair in The Australian National University research institute in 1974 gave me an ideal opportunity to continue Solomons fieldwork (which increasingly went in these new directions) and to make several short fieldwork trips to Churah in the northwestern Indian Himalayas to look at the political economy of peasants and "development."

"In the last twenty years, I have been examining questions I mainly ignored in my early research: class, gender, power, "development" and dependency, colonial discourse, cultural nationalism. I draw theoretical guidance from Marx, and more recently, from Gramsci, Foucault, Bourdieu, Hall, Said, Guha, and a range of feminist theorists (notably Rowbotham, Mitchell, Ehrenreich, English, Irigaray and Wittig). These have been reflected, in writing about the Solomons, in books and articles on struggles against colonial domination, life histories of Kwaio women, the political economy of "development," the politics of "custom," the structures of counter-hegemonic discourse, and predation and violence of urban under-classes.

"My interests in language (sustained in studies of the Kwaio language and the history of

Melanesian Pidgin English) and in the mind have been rekindled by the emergence of cognitive linguistics. The concerns with conventional metaphor, with the iconic, image-based character of language and thought, and an emphasis on embodied experience all point to underlying universals and provide antidotes to anthropology's excessive relativisms and exotica-hunting. These interests have led me back to a renewed engagement with questions of cognition (where the early influence of Bateson on my thinking remains strong) and of cultural theory.

"I have always tried to be open to alternative paradigms, and to be something of a bridge-builder between them. Academics are given to elevating narrow visions and partial truths into total systems and mutually exclusive grand theories. I have been more interested in finding productive ways to fit the partial visions together, with an appreciation for the vast and wondrous complexity that makes humans human."

ADAM KUPER *was born in South Africa and studied social anthropology at the University of the Witwatersrand and at Cambridge University, where he took his doctorate in 1966. He has taught at universities in Uganda, Britain, Sweden, the U.S.A., and the Netherlands and has been a fellow at the Center for Advanced Studies in the Behavioral Sciences at Stanford. His books include ethnographic monographs dealing with villagers in the Kalahari desert and with modern Jamaica, comparative ethnographies of Southern Africa, and historical critiques of social anthropology. His most recent book is* <u>*The Invention of Primitive Society*</u> *(Routledge, 1966). He has edited* <u>*Current Anthropology*</u> *since 1986, and was the first chairman of the European Association of Social Anthropology.*

"Every social anthropologist is trained to work both as an ethnographer and as a comparativist or social theorist. I find that I try to justify myself on both fronts, but through perhaps slightly unconventional projects. My own work has two main foci, one the comparative ethnography of Southern Africa, the other the history and theory of social anthropology. My present research concerns the pre-conquest political systems of Southern Africa. I tend to switch from one set of projects to the other every few years, but this is not a planned rhythm, and I am aware of it only when I look back at what has happened.

"Though my most intensive period of training occurred at Cambridge, under Meyer Fortes, the greatest intellectual influence on my work has been the writings of Claude Lévi-Strauss. Editing Current Anthropology greatly enlarged my intellectual horizons and brought me into contact with anthropologists all over the world. More recently, however, I have been caught up in the revival of European social anthropology."

CLAUDE LÉVI-STRAUSS *is a French "ethnologist . . . famous as an exponent of structuralism . . . After studying both law (licence) and philosophy (licence and agrégation), Lévi-Strauss chose ethnology as his area of specialization. Attracted by the possibility of doing fieldwork, he accepted a position at the University of Sao Paulo in Brazil from 1935 to 1937. During school vacations and again in 1938-39, Lévi-Strauss organized expeditions into the Brazilian interior to study the Bororo, Nambikwara, and other Indian groups. Mobilized during 1939-40, Lévi-Strauss was spirited out of France following the [French] defeat by a Rockerfeller Program to save prominent Jewish intellectuals. In New York from 1941 to 1945, he taught in the New School for Social Research, and on a second stay in 1946-47 served as French cultural*

officer. During the war Lévi-Strauss joined a community in exile in the United States dominated by leading surrealists and became close friends with André Breton and Max Ernest. After holding positions at the National Center for Scientific Research, the Musée de l'Homme and the Ecole Pratique des Hautes Études, Lévi-Strauss was elected to the Collège de France in 1959 where he directed a laboratory of social anthropology until his retirement in 1982. The Académie Française elected him to membership in 1973." (A. Douglas 1992:266) For a list of his numerous awards and honorary degrees see Redfield (1987:455).

His major works include (with English translation titles and dates, French dates are in brackets): La Vie Familiale et Sociale des Indiens Nambikwara [1948], The Elementary Structures of Kinship (1969) [1949], Race et Histoire [1952], Tristes Tropiques (1973) [1955], Structural Anthropology, 2 volumes (1963-76) [1958-73], Totemism (1963) [1962], The Savage Mind (1966) [1962], Introduction to the Science of Mythology: Vol. 1: The Raw and the Cooked, Vol. 2: From Honey to Ashes, Vol. 3: The Origin of Table Manners, Vol 4: The Naked Man (1969-81) [1964-71], The Scope of Anthropology (1968), The Way of the Masks (1982) [1975], Myth and Meaning: Five Talks for Radio (1978), The View from Afar (1985) [1983], The Jealous Potter (1988) [1985], and Histoire de Lynx [1991]. In addition, there exist Conversations with Claude Lévi-Strauss (1969) [1961] edited by Charbonnier and Conversations with Claude Lévi-Strauss (1991) [1988] by Lévi-Strauss and Eribon.

The following are excerpts from a set of discussions between Claude Lévi-Strauss (C.L-S.) and Didier Eribon (D.E.) - printed in Lévi-Strauss and Eribon (1991) - regarding influences shaping Lévi-Strauss's anthropological career:

(D.E.: Why had you decided to become an anthropologist?) C.L.-S.: Let's say that it was a combination of circumstances. Since childhood I had a passion for exotic curios . . . In addition, toward 1930 it began to be known among the young philosophers that a disciple called anthropology existed and that it aspired to obtain official recognition . . . Moreover, I read a couple of works by English and American anthropologists, particularly Robert Lowie's Primitive Society, that won me over because the theoretician and the fieldworker were combined. I was envisaging a way of reconciling my professional education with my taste for adventure . . . Finally, Paul Nizan, whom I had met two or three times at family gatherings . . . told me that he himself had been drawn to anthropology. That encouraged me. (1991:16-17)

(D.E.: Did . . . [Boas's] work mean a great deal to you?) C.L.S.: It was essential . . . Boas was . . . one of the first - somewhere I wrote that it was Saussure, but in fact Saussure never expressed himself on the subject, it flows indirectly from his work - to insist on an essential fact in the human sciences: the laws of language function on an unconscious level, beyond the control of speaking subjects; thus they can be studied as objective phenomena, representative for this reason of other social facts. (1991:38-39)

(D.E.: [Was the] meeting [with the linguist Roman Jakobson] . . . a decisive one for you?) C.L.-S.: It was enormously important. At the time I was a kind of naive structuralist, a structuralist without knowing it. Jakobson revealed to me the existence of a body of doctrine that had already been formed within a discipline, linguistics, with which I was unacquainted. For me it was a revelation. . . . (D.E.: . . . And right away you were able to apply his methods to your work on kinship.) C.L.-S.: Things didn't happen that way. I didn't apply his ideas; I became aware that what he was saying about language corresponded to what I was glimpsing in a

confused way about kinship systems, marriage rules, and more generally, life in society. (1992:41, 99)

C.L.-S.: The nature and importance of my borrowings from linguistics have been misunderstood. Besides being a general inspiration, which, I admit, is enormous, they boil down to the role of unconscious mental activity in the production of logical structures, which was emphasized by Boas, who was an anthropologists as much as a linguist. Second, there is this basic principle that component parts have no intrinsic meaning; it arises from their position. This is true of language, and it is also true for other social facts. I don't believe I have asked anything else from linguistics, and Jakobson, during our conversations, was the first to recognize that I was making an original use of these notions in another area. (1992:112-13)

(D.E.: The idea of transformation has a key place in your analyses . . . Where did you find it, in logic?) C.L.-S.: Neither in logic nor linguistics. I found it in a work that played a decisive role for me and that I read during the war while I was in the United States: On Growth and Form, in two volumes, by D'Arcy Wentworth Thompson, which was first published in 1917. The author, a Scottish naturalist . . . interpreted the visible differences between species or between animal or vegetable organs within the same genera, as transformations. This was an illumination for me, particularly since I was soon to notice that this way of seeing was part of a long tradition: behind Thompson was Goethe's botany, and behind Goethe, Albrecht Dürer and his Treatise on the Proportions of the Human Body. Now the notion of transformation is inherent in structural analysis. I would even say that all the errors, all the abuses committed through the notion of structure are a result of the fact that their authors have not understood that it is impossible to conceive of structure separate from the notion of transformation. Structure is not reducible to

a system: a group composed of elements and the relations that unite them. In order to be able to speak of structure, it is necessary for there to be invariant relationships between elements and relations among several sets, so that one can move from one set to another by means of a transformation. (1992:113)

(D.E.: . . . One of the objections often made against you [is that] you have read a great deal but have done little fieldwork.) C.L.-S.: That was the result of circumstances. If I had gotten a visa for Brazil in 1940, I would have gone back to my initial fieldsites and done more work. If the war hadn't broken out, I would probably have gone on another mission. Fate led me to the United States, where due to a lack of means and the international situation I was not in a position to launch any expeditions but where, on the other hand, I was entirely free to work on theoretical issues . . . I also became aware that in the previous twenty or thirty years a considerable quantity of material had been accumulated; but it was in such disarray that one didn't know where to begin or how to utilize it. It seemed urgent to sort out what this mass of documents had brought us. Finally, why not admit it? I realized early on that I was a library man, not a fieldworker. . . . I did more fieldwork than my critics would admit. In any case, I did enough to learn and to understand what fieldwork is, which is an essential prerequisite for making a sound evaluation and use of the work done by others. (1992:43-45)

(D.E.: . . . What led you to write such a book [as Tristes Tropiques]?) C.L.-S: It all began with a request on the part of Jean Malaurie . . . It had never dawned on me to write about my travels. However, in the period I was going through, I was convinced that I had no future in the university system, so the idea of just writing what came to me was tempting. Also, as time went on, I had gained a certain distance. It was no longer a matter of transcribing a journal of

my expedition. I had to rethink my old adventures, reflect upon them, and draw some kind of conclusions. (1992:58)

(D.E.: . . . You told me the other day that your entire career had been outside the traditional university environment.) C.L.-S.: . . . I had taught in Brazil, in the United States, then in France at the Ecole des Hautes Études. But never in the university. (D.E.: What were the advantages of working outside of the traditional university system?) C.L.-S.: More freedom and, in a sense, tolerance for a less regimented spirit. (1992:75)

C.L.-S.: . . . Freud played a major role in my intellectual development, equal to the role of Marx. He taught me that even phenomena of the most illogical appearance can be subjected to rational analysis. I found Marx's work comparable as it relates to ideologies (which are collective instead of individual phenomena, also essentially irrational): it is possible to reach beyond appearances to find a logically consistent foundation, regardless of the moral judgements one might have with respect to it. (1992:107-08)

C.L.-S.: . . . Marx was the first in the social sciences to use systematically the methodology of models. All of Capital, for example, is a model constructed in the laboratory and set in motion by the author so he could view the results in conjunction with observed events. Also, in Marx I found the fundamental idea that one cannot understand what is going on inside people's heads without connecting it to the conditions of their practical existence, something I have tried to do throughout the Mythology books . . . Only a few lessons from Marx's teaching have stayed with me - above all, that consciousness lies to itself. And then, as I've already said, it is through Marx that I first glimpsed Hegel, and behind him, Kant. You were asking me about the influences on my work: fundamentally, I'm a common-sense Kantian, and at the same time, perhaps, a born

structuralist. . . . Even [in my early childhood] I was looking for invariants! (D.E.: What have you retained from Kant?) C.L.-S.: That the mind has its constraints, which it imposes on an ever-impenetrable reality, and it reaches this reality only through them. (1992:108)

(D.E.: Basically, your researach method in the Mythology series is rather close to that of Dumézil: to define a geographical area and try to find the same mental structures within it. However, there is a fundamental difference; he had an important historical sequence at his disposal, while you, when you analyze the American myths, cannot find their historical depth.) C.L.-S.: I don't need to tell you how much I owe to the work of Dumézil. I learned a great deal and found much encouragement there. But the difference you mentioned is not the only one. Dumézil and I have different goals. He wished to prove that a system of representations, whose presence had been noted in several parts of Asia and Europe, had a common source. For me, on the contrary, the historical and geographical unity was there from the first: America, peopled by successive waves of immigrants who generally all had the same origin and whose entry into the New World took place, according to diffrent authorities, between 70,000 and 5,000 B.P. So I was looking for something else: first of all, to account for the differences between the mythologies whose unity was given by history; and second, starting with an individual case, to understand the mechanisms of mythic thought. (1992:131-32)

C.L.-S: Wagner played a capital role in my intellectual development and in my taste for myths. . . . Not only did Wagner build his operas on myths, but he proposed a way of analyzing them that can be clearly seen in the use of the leitmotiv. The leitmotiv prefigures the mytheme. Moreover, the counterpoint of leitmotivs and poetry achieves a kind of structural analysis, since it works by shifts or displacements to superimpose moments of the plot that

otherwise would follow each other in a linear sequence. Sometimes the leitmotiv, which is musical, coincides with the poem, which is literary; sometimes the leitmotiv recalls an episode that has a structural relationship to the happening at the time, either by analogy or contrast. I only understood that later on, well after I began my analysis of myths, and at a time when I believed myself completely cut off from the spell of Wagnerism. Let's say that I was brooding on Wagner for several decades. (1992:176)

(D.E.: At the end of the Jealous Potter you write that myth is a "magnifying mirror" of the way we habitually think. Is that the issue that guided you through this long series of books?) C.L.-S.: The issue is the same as the one in Elementary Structures of Kinship, except that instead of treating sociological facts it deals with religious facts. But the question doesn't change; in the presence of chaos of social practices or religious representations, will we continue to seek partial explanations, different for each case? Or will we try to discover an underlying order, a deep structure whose effect will permit us to account for this apparent diversity and, in a word, to overcome this incoherence? (1992:141)

ROBERT I. LEVY *is Professor Emeritus of Anthropology at the University of California, San Diego, and Research Professor of Anthropology at the University of North Carolina at Chapel Hill. His first career was in medicine, psychiatry and psychoanalysis. Between 1961 and 1964 he conducted his first anthropological studies in two small Tahitian communities in French Polynesia. From 1964 to 1968 he worked at the University of Hawaii. During this period he helped coordinate studies and programs on comparative psychiatry in East and South East Asia. In 1969 he became one of the founding members of a new Department of Anthropology at the*

University of California, San Diego, organized by Melford Spiro as a center for the study of psychological anthropology. In 1973 Levy began the study of Bhaktapur, a pre-modern, traditional Hindu city in Nepal's Kathmandu Valley. He has published ethnographic and theoretical papers on Tahiti and works on many issues in psychological anthropology, including several influential papers on emotion in anthropological perspective. His book Tahitians: Mind and Experience in the Society Islands (1973) describes the person-centered worlds of understanding and action of some Tahitian villagers. Mesocosm: Hinduism and the Organization of a Traditional Hindu City in Nepal (1990) describes the symbolic and religious organization of Bhaktapur. He is currently working on a volume on the private worlds of some citizens of that city and the interrelations of those private spheres and the larger public sphere of the city.

"My intellectual biography has some of its beginnings in medicine, with its special insights and limiting prejudices, its vision of tightly interrelated systems more or less bounded within human skins, and its emphasis on pathology and breakdown as a privileged way of understanding healthy functioning. Encounters in psychiatric courses with 'psychotic' patients initiated a life long interest in the behaviors and inner worlds of exotic others and their slippery connections to 'our' realities and normality. In mid-century the frontiers of psychiatry seemed to be in psychoanalytic theory, and during the psychiatric residencies which followed medical school the thing to do was to enter psychoanalytic training at one of the psychoanalytic 'Institutes' which were beginning to proliferate in New York and other major American cities. I picked the William Alanson White Institute, a 'neo-Freudian' group (whose members had included the social philosopher Erik Fromm and Harry Stack Sullivan, a then influential theorist

of 'interpersonal' relations.) The faculty of the institute, like other 'neo-Freudian' schools, emphasized what they took to be 'cultural' factors in human development and psychopathology, in the face of what they considered to be an overly 'biological' Freudianism. But, in spite of that 'cultural' emphasis, the Institute kept much of the medical-psychiatric model of some sort of focal pathology centered in the 'patient.' After some years of practice in New York, and - after an informative spell in the army where I saw some radically non-middle class patients and pathogenic situations - in San Francisco, I began to feel progressively dissatisfied with the limitations of both the theory and the practice of psychoanalysis. It seemed to me then that the useful next step would be an attempt to understand the relations of person-centered phenomena to wider socio-cultural processes.

"I was influenced by contemporary research in what was variously called social or transcultural or comparative psychiatry, among which were the important studies of the anthropologist, Alexander Leighton, on the relations of community disorder and personal stress, and by the then flourishing field of 'culture and personality studies,' whose most stimulating writers, for me, included Ernest Beaglehole, Abram Kardiner, Melford Spiro, Margaret Mead, George Devereux, and Geza Roheim. But the most important, transformative, and longest lasting influence on me was Gregory Bateson, whom I first met when he was working near San Francisco on schizophrenia, work which lead to elaborations of his theory of learning (of great anthropological usefulness) and the double bind theory of schizophrenia. Bateson's work, particularly the theoretical papers eventually collected in Steps to an Ecology of Mind, introduced me to the revolutionary shift in models of behavior initiated by cybernetics and communication theory, which allowed behavior/mind/thought, to be understood (in part)

as located and learned in a structured field of dynamic and mutually constructive relations in which individuals were nodes. He provided an entree into the developments of late twentieth century thought (including the French thought of recent decades, which traverses much of the same new ground from a different entrance place) and a partial corrective to the (still flourishing) mechanistic, intra-psychic, and 'culture-personality' models which were residues of nineteenth century ways of understanding. It should be noted that with all the enormous corrective power of these late twentieth century models, they, in their turn, under-specified and undervalued the complex intra-psychic processes and activities of the 'nodes,' - that is, of 'individuals' - and overemphasized cognition or 'mind' as precipitates of dominant cultural schemas at the expense of such determinants of human understanding and action as conscience, emotion, defence mechanisms, intuition and the like - all of which have their own sorts of relations to and resistance to 'culture.'

"By the mid-sixties, to take up the genealogical thread again, I was anxious to leave a problematic psychiatric practice and to try to learn something from new experiences and observations. I was able, through the invitation of the anthropologist and specialist on Oceania, Douglas Oliver, to work in a Tahitian-speaking village and a small urban enclave from 1961-64. In later years I worked in a greatly contrasting community, a traditional Hindu city in Nepal (from 1973-1976). In both places my central interest was in the relation between the structures and forms of community life and the forms of 'mind and experience' of dwellers in different kinds of communities. These relations have continued to preoccupy me."

GEORGE E. MARCUS *is Professor of Anthropology at Rice University where he has taught since 1975. He has been Chair of the department since 1980 and oversaw its reconstitution in line with its present reputation for the critique of anthropology and the encouragement of the contemporary practice of critical ethnographic research. He received a BA at Yale (1968) in politics and economics, studied social anthropology at Cambridge on a Henry Fellowship, and in 1971 entered Harvard's Department of Social Relations (in social anthropology) just as its interdisciplinary graduate program was dissolving. From the early 1970s to the early 1980s, his research was on the Kingdom of Tonga. From the 1980s, he has been concerned with the study of upper-classes and elite institutions in the U.S. and other Western societies. From 1986-1991, he was inaugural editor of the journal Cultural Anthropology. Most recently, he and his colleagues have been especially concerned with developing the relationship between anthropology and the emergent interdisciplinary arena of cultural studies. His major publications include The Nobility and the Chiefly Tradition in the Modern Kingdom of Tonga (1980), Elites: Ethnographic Issues (1983), (with Michael Fischer) Anthropology As Cultural Critique (1986), (with James Clifford) Writing Culture: The Poetics and Politics of Ethnography (1986), and (with Peter Dobkin Hall) Lives in Trust:The Fortunes of Dynastic families in Late Twentieth Century America (1992).*

"I was first drawn to anthropology while still a high school student in the early 1960s through reading letters from the field by a sister and brother-in-law (a Yale graduate student) living among the Semai in (then) Malaya. My subsequent professionalization into anthropology cannot be told in strict genealogical terms of lineage (e.g. "Boas begat X who begat Y, etc. who

begat me"). Rather, I seemed to be repeatedly passing through particular institutions of training that were rather at the intellectual end of particular trends or initiatives, so that while committed to and fascinated by the experiences anthropology offered, I always seemed to have an oblique or critically detached relationship to the specific brands of professional anthropology to which I was being exposed. First, I was at Yale during the mid 1960s after the vitality of the influence of Murdock, on the one hand, and ethnoscience, on the other, had long since peaked. Consequently, while taking anthropology courses, my real sense of excitement came from Paul Mus and Harry Benda's courses on Southeast Asian history (with strong doses of Geertz as Indonesianist), and I wound up majoring in a special program in politics and economics (composed largely of political theorists, development economists, and visiting European scholars).

"Then, I spent three terms at Cambridge during 1968-69, reading social anthropology for the BA Tripos. These were the twilight days of British functionalism, when the triumvirate of Goody, Leach, and Fortes (Tambiah also happened to be on the faculty) stood uncharacteristically together in the face of student radicalism among their own students and several American visitors. I remember clearly a succession of lectures by the "greats" - Evans-Pritchard, Needham, Gluckman, etc. - and I remember how tired they (and the Cambridge group) all seemed compared to the promise of their writings which I was systematically reading that year.

"Next, after a two year stint in the army in South Carolina (when I taught anthropology in the night school of the University of South Carolina, and did some ethnographic puttering among Sea Island Gullah-speakers), I entered Harvard's Department of Social Relations (in social

anthropology). Talcott Parsons and George Homans were still very much presences, but the program had nothing of its past life (for anthropologists, anyhow) when Clyde Kluckhohn led the anthropology section and David Schneider and Clifford Geertz were students. While learning much from Cora DuBois and David Maybury-Lewis, again I cleaved from anthropology and toward courses offered in other fields that seemed to have great (undeveloped) intellectual promise for anthropology -courses offered by Stanley Cavell, Barrington Moore, and Daniel Bell, for example.

"I found my own experience to be similar to that of most of my fellow students in social anthropology at Harvard of the early 1970s. One was free to partake of a vast, rich range of intellectual offerings, but sooner or later, most students affiliated with one or another of the major projects which defined research opportunities in graduate work. I joined the Fiji project of the late Klaus Friedrich Koch (a legal anthropologist and a student of Laura Nader at Berkeley) since I would have the chance to work in the Kingdom of Tonga, a monarchical society that had made interesting adaptations to a long history of colonialism and modernity. My most satisfying and valuable experience in anthropology at Harvard was my participation as a teaching fellow in the social theory course taught by David Maybury-Lewis and Nur Yalman. Lunch-time meetings of the latter and the teaching fellows in this course remain memorable to this day as marking a congenial, stimulating sharing of the old, the new, and the emergent in theoretical trends that still define the horizons of social and cultural anthropology.

"As formal schooling, then, my training in anthropology occurred repeatedly against the backdrop of trends and fashions winding down in institutions that were of societies (U.S., Britain), in retrospect experiencing the incipient dissolution of post-War culture and

arrangements. I suppose I really first learned anthropology in a total, committed way on the job, at Rice, and with increasing creativity after 1980, when the department literally rebuilt itself. During this period, I learned an immense amount about anthropology from my colleagues, Michael Fischer (trained at Chicago and who had been on the faculty at Harvard) and Stephen Tyler (trained at Stanford University and a major spokesperson for and critic of cognitive anthropology). A personal friendship and alliance with Jim Clifford (who had been a graduate student in history while I was at Harvard and who frequently attended department lectures and parties while I was there), beginning with his visit to Rice in 1980, opened for me new and exciting frames in which anthropology as an enterprise could be understood. A year (1982-83) at the Institute for Advanced Study in Princeton, in which both Clifford Geertz and I were in our different ways thinking about anthropology from the angle of writing and representation, was formative for everything that I have done since. The Santa Fe (School of American Research) Seminar in 1984 leading to the publication of Writing Culture crystallized my own thinking since the early 1980s and brought me into long-term association with some of the most intellectually broad anthropologists of my own generation. And since the early 1980s, I have been lucky enough to meet and know a full range of the most committed and creative anthropologists working today. These have been, and continue, to be my most important teachers.

"I have moved from a study of dynastic elites and fortunes to a study of the formation of prominent cultural institutions in the U.S. and other countries. My continuing agenda is to find new ways to describe and write about, from within the ethnographic and cultural anthropological tradition, the massive and minute changes in contemporary societies. I feel

able to do this from within the framework of a rare department that makes intellectual sense to its faculty and graduate students, by constant learning from my network of friendships and associations among an array of remarkably talented anthropologists, and by taking up the intrepid challenge of being open to and engaging with the rich and diverse intellectual terrains that compose contemporary cultural studies, from which much of the intellectual capital and vitality for thinking about contemporary cultural processes - high, low, everyday, global, and local - have so clearly come."

SALLY FALK MOORE *is Professor of Anthropology at Harvard University where she served as Dean Of the Graduate School from 1985-89. She also regularly teaches "Anthropological Approaches to Law" at Harvard Law School. Her books include Power and Property in Inca Peru (1938), Law as Process (1978) and Social Facts and Fabrications: Customary Law on Kilimanjaro 1880-1980 (1986). She is a fellow of the American Academy Of Arts and Sciences and is a past president of the American Ethnological Society and the Society for Political and Legal Anthropology.*

"My professional development had three major phases: an initial period as a lawyer, several subsequent years as a young anthropologist (and young mother) doing library research and publishing, but without professional employment, and the most recent twenty plus years as an Africanist, theorist, and teacher.

"At Columbia Law School (1942-43), I was profoundly influenced by Karl Llewellyn, not so much by his excursion into anthropology, but by his approach to American law. Two of his

ideas were particularly important. One was his skepticism about legal rules and doctrines, his demonstrations that they all came in pairs (or multiples) that were to different effects. It followed that judges decided which rule to "use", that the judge, not the rule, determined the decision. His other pivotal conviction was that the social settings in which contracts were made were a part of their meaning, i.e. that the text alone could not indicate all the implied cultural and social understandings that lay between the contracting parties. Llewellyn's sociological imagination and his irreverence for the then prevalent conception of legal reasoning were enormously liberating perspectives. My year on Wall Street continued my legal and sociological education. I learned much about the way the business world "worked". At the end of the first year I took a leave of absence to serve on the government legal staff at the Nuremberg trials. The experience shocked and fascinated me. Both the nature of the prosecution and the social history of the prosecuted deepened my already passionate interest in the political dimensions of collective social action.

"It was after the Nuremberg experience that I turned to anthropology, originally without any intention of becoming an anthropologist, simply to engage in the comparative study of societies for a year. My goal was to enlarge my understanding of collective causality. At Columbia University I was exposed to the ideas of a distinguished faculty. Though none of my mentors was particularly focussed on the problems that preoccupied me, the faculty was enthusiastically encouraging. I was particularly stimulated by contact with A. L. Kroeber, by the great breadth of his knowledge and his openness, even in old age, to new ideas. I learned a great deal from all of my Columbia teachers and their interests certainly influenced me to write my dissertation on government, law, and property in Inca Peru. However, at the time it was my

early encounter in the library with the writings of Claude Lévi-Strauss that had the most theoretical impact on my thinking. I found his analytic detachment from the world of practical action and from history unacceptable, but the way he analyzed patterns of thought tantalized and invited further exploration in the ideology and rhetoric of politics and law.

"My first child was born in 1952, my second in 1955 and I did not start teaching until the early 1960s, though I continued to write and publish. My husband's academic work took us to England in 1954 and we returned there at intervals over the next decades, ultimately for a total of seven years. As I result, I had a good deal of contact with the world of British social anthropology. These connections were strongly reinforced from the moment when, in the early 1960s, the family moved to Los Angeles. There my husband joined the history department at U.C.L.A. And there I met Hilda Kuper, a South African (who had studied with Malinowski) and M. G. Smith (a Jamaican, whose degree came from University College, London), and the three of us became fast friends. The African Studies Center at U.C.L.A. was at that time a place of considerable intellectual excitement and ferment. The end of colonial rule had led to a radical reassessment of anthropological theory and practice in Africa. Though I taught at U.S.C., I regularly attended the seminars run by the U.C.L.A. Center and deepened my ties with the many British anthropologists who came to participate in the Center's activities, several of whom I had met before. Hilda Kuper and Mike Smith and Max Gluckman, a frequent visitor to U.C.L.A., together encouraged me to embark on fieldwork in Africa. I started in 1968 on Kilimanjaro, and have returned intermittently ever since. The interest of those three - Kuper, Smith and Gluckman - in politics, law and theory, and, for that matter, in my own law-grounded, historically informed, non-structural functional perspectives on these subjects led to

many long hours of productive argument and discussion. For me, from the British and Los Angeles phases on, anthropology ceased to be a solitary enterprise pursued in the library, and my professional life enlarged its scope, and its pleasures. As is obvious, my preoccupation with 'the anthropology of the present' is not a new interest."

ROBERT F. MURPHY *(1924-1990) was Professor of Anthropology at Columbia University, where he taught from 1963 until his retirement in 1990. A cultural anthropologist, he did field work among the Mundurucu Indians (Brazil), the Shoshone-Bannock Indians (North America), the Tuareg (Africa), and most recently, in the field of the anthropology of disability. Murphy received the Mark Van Doren Award for distinguished teaching in 1977, and, in 1988, the Lionel Trilling Award for his book* <u>*The Body Silent*</u> *(1987). Among his numerous publications, perhaps the best known are* <u>*The Dialectics of Social Life*</u> *(1971),* <u>*Cultural and Social Anthropology*</u> *(3rd edition, 1989), and* <u>*Women of the Forest*</u>*, co-authored with his wife Yolanda (2nd edition, 1985).*

Prepared by Yolanda Murphy and Dr. Barbara Price:

"Murphy began his career as a student of macrostructures and multilinear evolution; became in mid-life a student of symbolically-mediated social interaction focused on what happened between individuals in groups; and finally, his increasingly overriding concern was the nature of self, the nature of the human condition into which, in all societies, we all are born. His intellectual evolution represents far less the adoption and subsequent abandonment of one "school" or another, than it does the addition and integration, throughout his life, of new bodies of knowledge and theory. Indeed, much of his career was spent rejecting the

constraints and confrontations of "schools" altogether. Rather, what perpetually fascinated him were the anomalies, the "exceptions" to conventional wisdoms. The investigation of these exceptions was designed not only to delineate and explain these individual cases, but to refine and reshape the original generalizations themselves.

“Murphy's early life formed his character, his values, and the ironic wit with which he approached his later life and work. A child of the Depression who grew up in Rockaway Beach, New York, in a recently impoverished middle-class Irish Catholic family, he attended Far Rockaway High School along with other highly motivated and highly verbal students, most of whom had planned to attend college. Financial pressures, however, compelled Murphy to work after graduation. In 1943, he joined the U.S. Navy, saw action in the Pacific, and was discharged in 1946 with the rank of technician first class. His experiences of both the Church and the Navy left him with a life-long skepticism of authority, whether ecclesiastical, political, or intellectual. Under provisions of the G.I. Bill, he applied to and was accepted at Columbia University shortly after his demobilization.

“Accordingly, Murphy became a part of a unique upheaval in the American class system. The veterans who entered Columbia (and the other major U.S. universities) were a new generation, often brash and intellectually curious, formed in the crucible of the Second World War. Although they came from all social classes, many of them were working class in origin, who, in the days before the War, would have been discouraged from attending college. Many were the first in their families to do so, seeing in higher education a way out of poverty, out of dull, repetitive jobs. As Murphy phrased it, Columbia "formed me, brought out my potential, and encouraged me to become whatever I wanted to be, to do whatever my abilities permitted.

It opened up a universe for me that was rich beyond anything I had ever imagined" (The Body Silent 1987:168).

"It was as an undergraduate at Columbia that Murphy discovered both anthropology and the writings of Sigmund Freud. Although he had left the Church at the age of 16, its doctrines of guilt and atonement, its symbolism, and its emphasis on sexual repression resonated in his strong response to Freud, in whom he saw the same themes, and whose work he would later draw into his investigations of the self. Still as an undergraduate, Murphy stumbled into an anthropology course given by Charles Wagley. It opened his eyes to a whole new world, gave him a new perspective on his own war experiences, and set his feet on the path of his career. Entering Columbia as a graduate student in 1949, he received his Ph.D. in 1954. Among the graduate students at Columbia during the late 1940's and early 1950's were Eric Wolf, Morton Fried, Robert Manners, Elliott Skinner, Stanley Diamond, Rene Millon, and Marvin Harris.

"While a graduate student, Murphy continued to study with Wagley, who stimulated his interest in Lowland South American Indians and prepared him for his doctoral field research among the Mundurucu of Brazil in 1952-53. The other major intellectual figure at Columbia at the time was Julian Steward, whose work in the conjoined theoretical fields of multilinear evolution and cultural ecology became a lifelong influence on Murphy, particularly so in his interests in kinship and the organization of labor. Upon his return from the field he accepted a position with Steward as Research Associate at the University of Illinois along with Eric Wolf, whom he considered a powerful intellect and whose ideas he respected. The association with Steward is especially apparent in Murphy's earlier works (Headhunter's Heritage 1960, "Tappers and Trappers" 1956, and Shoshone-Bannock Subsistence and Society [co-authored

with his wife Yolanda] 1960).

"In 1955, Murphy joined the University of California, Berkeley as Assistant Professor of Anthropology. At this time there was great rapport between the anthropologists and the sociologists, including an intense joining seminar which involved such participants as David Schneider (whom he considered his post-graduate mentor), Rene Millon, Lloyd Fallers, Reinhard Bendix, Philip Selznick, Erving Goffman, and Talcott Parsons. The interchange of ideas in this forum led Murphy to explore the works of Simmel, Durkheim, and Levi-Strauss, whose influences are perhaps most evident in his subsequent Tuareg research (1959-1960). Although his initial theoretical plan was to have integrated Steward's cultural ecology with a structural-functional model, he turned instead to Simmel and Levi-Strauss for an analysis of the data ("Social Distance and the Veil" 1964). With this new synthesis also, he returned to his Mundurucu data and applied Simmel's work on conflict to an analysis of Mundurucu warfare ("Intergroup Hostility and Social Cohesion" 1957). In his later years at Berkeley, Murphy became increasingly interested in structuralism and gave what may have been the first seminar on this topic there in 1961 or 1962. His acceptance of this position, however, was always less than complete. Considering it devoid of all systems of action and of human sentiment, he later modified and transformed a number of its elements in The Dialectics of Social Life.

"With great gladness, Murphy returned to Columbia University as Professor of Anthropology, joining colleagues such as Wagley, Morton Fried, Conrad Arensberg, Alexander Alland, Elliott Skinner, and Marvin Harris. He felt that the diversity, not only of faculty but of students, reflected a rich and influential tradition that had generated a department committed to research and theory. Dialectics was written there, partly in the context of the anti-war

protest movements at both Berkeley and Columbia. A formulation of dialectics of interaction, it was also a critique of anthropological positivism and perhaps one of the first attempts to bring phenomenology into anthropology. He also wrote Cultural and Social Anthropology and, with his wife Yolanda, Women of the Forest.

"By the middle 1970's, Murphy was becoming increasingly ill, but continued to teach and publish. His office became a salon in which students and faculty could meet, argue, and exchange ideas. His own illness challenged him to look at paralysis as an intellectual problem, a metaphor of something deep in the human condition. Accordingly, he began a new research project to study the lives of the disabled. In The Body Silent, his last book, which is simultaneously autobiographical and generalizing, he attempted to integrate and synthesize all of his previous thinking, most notably to develop from Dialectics what he considered had been left unfinished or unaddressed in the earlier work. Quoting him (The Body Silent 1987:221-222):

> In my hospital reveries on illness and decline, I had a haunting sense of having rehearsed for the present in all my past year, of reliving my history in hyperbole, of undergoing a savage parody of life itself. I was caught in a process from which there was not escape, one that was so inevitable that I could not resist it, only watch spellbound. In a perverse way, the progress of my physical degeneration seemed meet and proper, for in each moment of my existence were all my yesterdays and all of my tomorrows. And my recapitulation of the past- and future- was not idiosyncratic, my own private nightmare. Rather it has been in some ways an enactment in exaggerated form of the course of all of social life.

LAURA NADER *is Professor of Anthropology at the University of California, Berkeley where she has been a member of the Anthropology Department since 1960. She worked among the Zapotec and Trique of Mexico, the Shia Moslems of south Lebanon, urbanites in Morocco, and among people of the United States. Her most intensive fieldwork is among the Zapotec of Oaxaca, Mexico (1957-1968), and in the United States (1970 -). Her research resulted in numerous articles. Her most outstanding books are Talea and Juquila - A Comparison in Zapotec Social Organization (1964), The Ethnography of Law (1965), Law in Culture and Society (1969), The Disputing Process (1978), No Access to Law (1980), and Harmony Ideology - Justice and Control in a Zapotec Mountain Village (1990). With colleagues she published on Energy Choices in a Democratic Society (1980) and on the future of American children (All Our Children 1980). She is a member of the American Academy of Arts and Sciences.*

"I began my studies in the Department of Anthropology at Harvard University under Clyde Kluckhohn as a consequence of reading his popular book Mirror for Man at a critical intellectual juncture. Although Kluckhohn had studied at Oxford under Robert Marett and worked in Vienna with Father Schmidt and others of the Kulturkreise School, when I knew him he was most influenced by his linguistic study with Edward Sapir and was involved in work with A. L. Kroeber on the concept of culture. It was because of Kluckhohn that I attended the Summer Institute of Linguistics at the University of Michigan, Ann Arbor (summer 1956). Later when I expressed interest in spending a year of study in England due to my admiration for the work of E. R. Leach and Raymond Firth, Kluckhohn said I could read what the British had to say whereas

the imaginative thinking was in France with people like Levi-Strauss. I stayed at Harvard. For Kluckhohn an education in anthropology was a breadth not a specialist education. He encouraged students to carefully read Kroeber's 1948 Anthropology, to visit the experimental psychologists in Mem Hall, to be critical in our readings of the sociologists, not to forget the importance of reading novels (which was his way of teaching about American society), to recognize the importance of evolutionary biology and the importance of time. Anthropology was about all of us, everywhere.

"Douglas Oliver taught the Harvard first year course; he also studied in Vienna. He introduced students to anthropology through a year long course in which we read classic monographs. It was in the reading that I encountered Gregory Bateson's Naven. Bateson was self-conscious about the construction of ethnography when hardly anybody else seemed to be, and along with Kluckhohn raised the difference between the scientific and artistic modes of presenting a culture. At the same time, Oliver lectured the class on his work in the Pacific discussing methodological innovations applied from Elliot Chapple's work. Fine grained fieldwork was the standard for Oliver, but it was contact with Beatrice and John Whiting of the School of Education's Laboratory of Human Development where I learned about comparison. The why of it was to understand human universals and variations. The Whitings had completed their degree work at Yale and the influence of Sapir and Malinowski, who was visiting there, was a strong part of what they imparted to me. The influences at Harvard were of a heterogeneous nature.

"As a student among the Mountain Zapotec I found myself struggling to record a fine grained ethnography while feeling that I was missing what came later through film and cultural

analysis. I compared two very similar villages, the comparison was on a continuum rather than posed as variables present or absent. The Mexican anthropologists who informed me were Julio de la Fuente and Roberto Weitlaner. De la Fuente's monograph on the Yalalag Zapotec and his work with Malinowski on indigenous market systems were important markers for Oaxacan ethnography. The model from the market research illustrated a network of relationships between seemingly autonomous villages. De la Fuente warned me of E. C. Parsons' failure to document variation in her Mitla work because of too great a dependence on a single informant. Roberto Weitlaner took me into Otomi villages where he taught me how to take a genealogy, how to ask questions that made sense to the Otomi. He was a model of a rapport builder. Weitlaner was trained as an engineer, then educated in American linguistics, and the many hours over coffee in Sanborns were the best field seminars anyone could have had in historical reconstruction.

"In 1960 I joined the Department of Anthropology at U.C. Berkeley. Clyde Kluckhohn and A. L. Kroeber both died that year, and Geertz, Fallers, and Schneider had departed for Chicago leaving an age break between younger and older Berkeley anthropologists. I was to teach about the anthropology of law. Law was not a subject taught to me at Harvard, but I was thoroughly familiar with the literature. Malinowski's work on law appealed to me because his vision was wide-angled, and although I admired Gluckman's work, I found his focus on courts too lawyerly. When I initiated the Berkeley Village Law Project I decided to continue to center the work on the dispute case (as Gluckman did) because I thought of it as a minimal unit comparable to units such as phonemes and morphemes that were part of the discourse in linguistics. Disputing is a universal phenomena in human culture, but not to be confused with

the dispute resolution paradigm, the use of which is sometimes been found to be insular. As a unit of action the dispute case may be manipulated by the parties or by power structures. It was Eric Wolf's work that helped me understand such manipulations as powerful mechanisms of global colonization and pacification.

"Berkeley in the 1960's had its intellectual impact. Sherwood Washburn was writing about race as a social and cultural concept. Elizabeth Colson was writing about tradition and change; George Foster was studying how people behaved under conditions of scarcity; and Robert Heizer was studying long continuities in Nevada cave sites in the context of a nuclear age. Dell Hymes encouraged many of us to write a critical anthropology. Reinventing Anthropology (1969) included my piece on studying up an effort to broaden the ethnographic domain. The notion of public interest ethnography was pioneered (Spradley 1970), but the movement was overwhelmed by trends that had little tolerance for anthropological precursors or even an anthropology that could use both scientific and artistic methods of understanding. Yet for me the heterodoxy of the earlier period persisted and new worlds opened.

"My teaching and research reflect these heterogeneous influences. I teach the Introduction to Sociocultural Anthropology, the Anthropology of Law, a course called Comparative Society, and maintain area interests in Middle America and the Middle East. Most recently I teach an undergraduate course on Controlling Processes - the dynamic components of power. My related seminar "Orientalism, Occidentalism and Control", I write about in this volume as a new venture in historical ethnography. The inspiration for this current work stems from the discovery that concurrent use of comparative methodologies yields new understandings more profound than the use of single approaches. I am currently writing a series of essays on

contemporary practices which use the concepts of social and cultural control together with the now common idea that ethnographers and their cultures are an instrument of writing ethnography and, therefore, part of the analysis of hegemonic forms of culture."

NAOMI QUINN *is Associate Professor in the Department of Cultural Anthropology at Duke University and currently chairs that department. She is President-elect of the Society for Psychological Anthropology. Her most recent and most sustained research - focussing on U.S. Americans' cultural model of marriage and crystallizing her views on the nature of culture - is the subject of a book under preparation and of a series of articles including "The Cultural Basis of Metaphor" (1991) and "Convergent Evidence for a Cultural Model of American Marriage" (1987). The latter appeared in a volume she co-edited with Dorothy Holland, Cultural Models in Language and Thought. She has, as well, an ongoing interest in anthropological research on gender, reflected in her early review article, "Anthropological Studies of Women's Status" (1977). Most recently, she and Claudia Strauss have begun research on culture acquisition, initial results of which appear in "Preliminaries to a Theory of Culture Acquisition" (1992).*

"Cultural anthropology, once I discovered it in college, was my calling. John Whiting, guest lecturing in my introductory anthropology course at Harvard, opened the possibility that cross-cultural research could be explanatory and not just descriptive. My undergraduate thesis advisor was Bea Whiting, who taught me to be both a close observer and a comparative thinker. Bea and Johnny and the interdisciplinary group of graduate students who worked with them at the research laboratory in Palfrey House gave me a taste of what it was like to work in

a research environment; a commitment to evidence and to anthropology as science; and an abiding interest in the effects of childhood experience. After college, recovering from hepatitis, I worked at Palfrey House as a coder on the Six Cultures project.

"By the time I completed graduate coursework and went to Ghana to do my dissertation field research, I had already accumulated quite a bit of fieldwork experience. While still an undergraduate I had participated in a Harvard-Cornell-Columbia field school in highland Ecuador, where I later returned under a Fulbright Scholarship (and contracted the aforementioned hepatitis). A graduate student at Stanford, I attended another summer field school in Oaxaca, Mexico. While these experiences were more formative than full-fledged research, they confirmed my affinity for field research and made a thoroughgoing naturalist out of me.

"In graduate school at Stanford, Roy D'Andrade (one of the then graduate students I had met at Palfrey House) became my advisor and my life-long mentor. A fine teacher, he gave me a sure sense of research problem, an inventive, pragmatic approach to research design and methods, and a fascination with the unfolding history of our discipline. At Stanford my interest in the psychological was expanded to include cognition. I was engaged in the emerging ethnoscience tradition with its mentalist theory of culture, its effort to reconstruct people's cultural assumptions from careful analysis of what they said, and its respect for formal method. The first two of these themes shaped my anthropology directly; the third I reacted against. As did other students, I developed a critique of ethnoscience's over-commitment to formal methods and the limitations of the semantic theory underlying these formalisms. This paired theoretical and methodological dissatisfaction fueled our urgent search for something better in

the way of cognitive theory, and, eventually also, our invention of new methods of discourse analysis more appropriate to our revised theoretical assumptions.

"Intellectual genealogies focussed on the individuals who influenced one may neglect the more diffuse but sometimes even more profound influence of broad intellectual movements. On me, as on other cognitive anthropologists of my generation, the most important of these intellectual influences, schema theory (fundamental to the paper in this volume) came from the new interdisciplinary field of cognitive science. A major setting for my education in cognitive science was the Social Science Research Council Committee on Cognitive Research, which I joined when Roy D'Andrade recommended me as his replacement, and on which I served in the seventies and eighties with psychologists like Eleanor Rosch and Amos Tversky and the linguist Charles Fillmore. During one of the conferences organized under the auspices of that committee, I got reacquainted with another linguist who happens also to be a distant cousin of mine, George Lakoff, and I became acquainted with George's work on metaphor. This work influenced me enormously, once again as much in the task of formulating a critique of it as in its direct influence on the way I thought about cultural understandings and their relation to discourse.

"Genealogy as a metaphor could bias one to think only backwards intergenerationally. But one of those most influential on my thinking has been my younger co-author in this volume, Claudia Strauss. Collaborating with Claudia has pressed me to be more expansive in my theoretical ambitions and more logical in my argumentation. We laugh about my tendency to stress what we call in our paper "centripetal" properties of culture and overlook "centrifugal" ones, and her tendency to do just the reverse - as much as anything a reflection of our

respective generations.

"In my story not only intellectual continuity but intellectual fadism has had a place. I was hardly out of graduate school when the tradition in which I worked came under an attack motivated less by sound intellectual objections than by the latest struggle for disciplinary hegemony. As we describe in our paper, cognitive anthropology was erased, or nearly so. In my professional life I have tried to foster appreciation for the place of cognition in culture theory and for a more open, integrative theoretical stance than is typically tolerated in our fad and faction-ridden discipline. Claudia's and my paper in this volume is offered as a concrete demonstration of the value of these principles."

ROY A. RAPPAPORT *is the Walgreen Professor for the Study of Human Understanding at the University of Michigan where he has been a member of the Anthropology Department since 1965. A past president of the American Anthropological Association (1987-1989), he has done ethnographic fieldwork in Papua New Guinea (1962-64, 1981-82) after brief archaeological field work in the Society Islands (1960). More recently, he has worked on environmental issues in the United States, most particularly on nuclear waste disposal and oil drilling on the Outer Continental Shelf. His best known publications are* Pigs for the Ancestors *(2nd enlarged edition, 1984) and* Ecology, Meaning, and Religion *(1979).*

"I came to anthropology from an earlier career in inn-keeping because I felt decreasingly comfortable in the world of the late nineteen fifties and wanted to come to a deeper and more rigorous understanding of my own growing sense of alienation. I first thought of sociology, but

one of the other contributors to this volume, Robert Levy (my first matrilateral parallel cousin), counseled me toward anthropology. A friend, Kai Erikson, and his father, Erik, also urged me to enter anthropology, the latter setting up an appointment for me with Clyde Kluckhohn at Harvard. In deference to Erik Erikson, Kluckhohn saw me, taking the occasion to deliver a monologue on why Harvard would not admit a thirty-three year old with a ten year old bachelor's degree in Hotel Administration (G.P.A. B-). I can't say I blame him, but at any rate I went to Columbia, an institution which, through its School of General Studies, had a mechanism for provisionally accepting anyone who walked in off the street.

"The dominant paradigm at Columbia in those days was White's general evolution, which was most articulately represented by Morton Fried, an important early influence on me. Although I quickly rejected the Whitian perspective, the vision of a lawful and unified order underlying the multiplicity of structures and events more apparently constituting the human world was, and remains, exciting to me. Harold Conklin had not yet departed for Yale, and I found his classes in both ethnoscience and ecology very useful. In retrospect, Conrad Arensburg's courses in political anthropology, which focused upon the formal characteristics of hierarchies and their operation, had an abiding effect upon my thinking about the structure of adaptive systems. Two fieldwork seminars with Margaret Mead were invaluable, but, as an adjunct professor, she was seldom present and had, unfortunately, little influence upon most of the students. I took only one course with Marvin Harris, but the materialism he was developing at the time was something that we all needed to contend with.

"Four months of archaeological fieldwork in the Society Islands in 1960 with Kenneth Emory and then with Roger Green were also important, for firsthand knowledge of Polynesian land

and seascapes suggested to me the explanatory potential of general ecology. Upon returning to the United States I encountered Marshall Sahlins' recently published Social Stratification in Polynesia and also read widely in biological ecology which, at the time, was more or less committed to ecosystemic approaches. Pete Vayda, also centrally interested in ecology, joining the Columbia faculty at the time, and I began to work with him. He eventually became my dissertation chairman. Fred Barth visited from Norway that year and offered a course in ecological anthropology. By the end of the Winter Semester I was pretty much committed to ecological studies. Vayda and I co-authored two papers for the Pacific Science Congress in Hawaii in 1961 and then wrote a grant proposal for ecological work in Papua New Guinea which he, Ann Rappaport, Cherry Lowman-Vayda, and I commenced in 1962.

"In reaction against the special form of ecology that Julian Steward thought necessary to accommodate the concept of culture, I had intended to study a local group of tribal horticulturalists in the same terms that animal ecologists study populations in ecosystems, and made observations and measurements to that end (e.g. areas under cultivation, yield per unit area, per capita intake, energy input per unit area). I was therefore surprised, to say the least, to discover that environmental relations among the people studied seemed to be regulated by a protracted ritual cycle. After completing Pigs for the Ancestors, I realized that I could provide an account of the place of ritual in a particular ecological system, but did not know why those functions were vested in ritual, nor anything about ritual itself. I subsequently became as interested in ritual and related matters (e.g. the concept of the sacred and religion in general) as in ecology and have remained so ever since.

"I joined the Michigan department in 1965. My senior colleagues included Leslie White,

with whom I shared an office one year, Elman Service, Eric Wolf, Marshall Sahlins, and Mervyn Meggitt. Rob Burling, Aram Yengoyan, and Norma Diamond were a bit in advance of me; Conrad Kottak, Kent Flannery, and Henry Wright came a year or so later. All influenced me to some degree. Perhaps most important initially were Sahlins, Wolf and Meggitt: Wolf for the capacity of his mind, Meggitt for his rigor and erudition, Sahlins, himself in transformation from evolutionism and ecology to structuralism, for simply challenging the materialist truisms of ecology and evolutionary anthropology.

"I met Gregory Bateson in Hawaii in 1968 and he immediately became - and had remained - the most profound of influences upon me. Formally a student of Alfred Haddon, but, more deeply of his own father, the biologist William Bateson, his view of evolution and adaptation as informational processes, particularly as developed in a number of essays in Steps to an Ecology of Mind, seem to me to be synthetically promising. As Stephen Toulmin has observed, Bateson points not only to directions in which anthropology should move but that science in general should move.

"My interests in ritual in particular and religion in general owe something to both Durkheim and Weber but are more obviously and directly indebted to other figures, both ancient and modern, outside as well as inside anthropology. They include importantly: Heraclitus of Ephesus, St. Augustine, Giambattista Vico, Charles Sanders Peirce, Gershom Sholem, J. L. Austin, Herbert Simon, Claude Shannon, Arnold van Gennep, Victor Turner, and Anthony F. C. Wallace. At present I am trying to find the time to complete the final draft of a large work on ritual that has occupied me intermittently for many years.

"I have, in recent years, become increasingly interested in the ecological, social, and

political problems troubling the late twentieth century. I have been serving on a very active National Academy; of Science panel on the social and economic impacts of outer continental shelf oil drilling and a State of Nevada panel on the effects of locating the national high-level nuclear waste repository at Yucca Mountain. While President of the American Anthropological Association, I established a number of task forces and panels on contemporary problems. I plan to devote myself further to such matters in the future.

MARSHALL SAHLINS *is Charles F. Grey Distinguished Service Professor of Anthropology at the University of Chicago. He is a member of the American Academy of Arts and Sciences and the National Academy of Science. His best known publications include: Social Stratification in Polynesia (1958), Evolution and Culture (co-edited with Elman Service, 1960), Moala: Culture and Nature on a Fijian Island (1962), Tribesmen (1968), Stone Age Economics (1972), The Uses and Abuses of Biology (1976), Culture and Practical Reason (1977), Historical Metaphors and Mythical Realities (1981), Islands of History (1985), and Anahulu: The Anthropology of History in the Kingdom of Hawaii, Volume One: Historical Ethnography (1992).*

The following is abridged from an article by Jocelyn Linnekin on Marshall Sahlins in Thinkers of the Twentieth Century (edited by Roland Turner, 1987:668-670):

"The hallmark of . . . [Sahlins'] work is that he suggests creative and original solutions to difficult theoretical problems. A reviewer called him 'one of the finest synthesizing minds in anthropology.' Sahlins's analytic procedure is akin to rationalism rather than the 'on-the-ground' empiricism that many anthropologists hold sacred. In a prose that is elegant and

literary, if occasionally obscure, he freely discusses areas where anthropology and philosophy intersect, underscoring his points with citations from such diverse sources as Kant and Joseph Heller, Hobbes and Gilbert and Sullivan.

"At the most general level, Sahlins's career has been dedicated to investigating the relationship between nature and culture, and specifically their order of determinacy: is culture constituted out of practical action, or is it arbitrary (in the linguistic sense) and logically prior to nature? In his early work, Sahlins presumed that nature came first in the equation, reflecting the tutelage of his mentor Leslie White. His dissertation investigated a Whitean hypothesis relating social stratification in Polynesian societies to differential energy-capture: 'the degree of stratification varies directly with productivity.' In this work, Sahlins adopted White's 'layer-cake' model of culture, with the 'technoenvironmental base' as prior and determinant, and social stratification relegated to the dependent 'superstratum.' An impressive piece of library research, Social Stratification in Polynesia is still an indispensable reference work for students of Polynesia, whether or not one agrees with its theoretical conclusions (which Sahlins himself has disavowed). Sahlins has since pointed out that stratification, far from being a dependent variable of energy-capture, is itself a spur to production, forcing the population to produce more than is needed to support the domestic group: 'the political life is a stimulus to production.'

"After Moala, Sahlins broke decisively with the materialist paradigm, and has become one of its most articulate critics. The essays in Stone Age Economics stress the fundamental differences between modern and primitive societies, and warn against applying concepts such as scarcity, supply-and-demand, and maximization to non-Western economies: 'Economic Man

is a bourgeois construction.' Influenced by the work of Karl Polanyi, Sahlins takes the 'substantivist' position in opposition to economic 'formalism,' which holds that Western economic concepts are appropriate for the study of primitive societies. In an oft-quoted essay, Sahlins refutes anthropology's conventional understanding of hunters and gatherers as preoccupied with the food quest and living on the edge of starvation. Field studies have revealed that hunters and gatherers do have 'leisure' as well as an adequate diet. Sahlins draws on this material to assert that hunters and gatherers are 'the original affluent society.'

"Sahlins's current theoretical position has more in common with French structuralism and 'semiotics,' the theory of signs derived from the work of Ferdinand de Saussure, than with American symbolic anthropology. His association with Claude Lévi-Strauss is well-known. Sahlins is one of the few American anthropologists to work through the implications of Lévi-Strauss's concept of structure. Culture and Practical Reason, a brilliant and sweeping essay in intellectual history, documents Sahlins's passage from materialism to idealism [or his view of how anthropology really supersedes this opposition]. Here he examines the work of major social theorists in light of two paradigms, the 'cultural' and the 'practical.' . . . He asserts that the 'practical' construction of culture reflects the ideology of Western society. As an alternative to a view of culture as Western Society, Sahlins offers 'some semiotic dimensions of our economy' to illustrate 'Western Society as Culture.' In this discussion he skillfully deciphers some of the 'cultural codes' that order seemingly 'practical' behavior in our own society.

"Sahlins is at his best when exploring apparent dichotomies and paradoxes. In Historical Metaphors and Mythical Realities he attempts to resolve the 'radical opposition' between structural anthropology and history by showing how history is ordered by prior categories and

cultural precedents: 'all structural transformation involves structural reproduction, if not the other way around.' Historical Metaphors is a first installment in Sahlins's analysis of the encounter between Hawaiians and Europeans in the early contact period. Displaying an unusual enthusiasm for foreign goods and customs, the Hawaiian chiefs accelerated the destruction of their own culture. Sahlins explains that their behavior followed from certain well-established cultural precedents: 'This apparently headlong rush to their own culture doom on the chiefs' part, this kind of 'acculturation,' can be shown to reflect basic Hawaiian principles, and, by virtue of these principles, to be selective rather than indiscriminate. For in realizing themselves as European chiefs, the Hawaiian nobility reproduced a customary distinction between themselves and the underlying population.'

Against those who would subordinate culture to biological or material determinants, Sahlins asserts the uniqueness of humankind and the priority of the symbolic faculty. . . . In spite of his stature within the discipline, Sahlins eschews disciples, and has no interest in heading a 'theoretical school.' He is that rare scholar who is capable of challenging his own preconceptions. For this reason, one can learn from him, but not follow him. For this reason also, anthropologists eagerly await publication of his latest theoretical forays."

NANCY SCHEPER-HUGHES *is professor of anthropology, director of the graduate program in medical anthropology, and 1992-1993 director of the Center for Latin American Studies at the University of California, Berkeley. Beginning in July 1993, she will hold the Chair in Social Anthropology at the University of Cape Town in South Africa. Previously, she taught at Southern Methodist University in Dallas, Texas and at the University of North Carolina, Chapel Hill.*

Extensive fieldwork has taken her to a mountain village in western Ireland (1974-1975), to psychiatric institutions in Boston, Massachusetts (1979-1980), to Spanish-American villages and the Taos and Picaris Pueblos of Northern New Mexico (1979, 1985, 1986), to a twenty-five year involvement with the people of an impoverished shantytown in Northeast Brazil (1964-1992). Most recently, she has been studying AIDS and public policy in Brazil and in Cuba. Her best known publications include Saints, Scholars and Schizophrenics: Mental Illness in Rural Ireland (1979, 1982), Child Survival: Anthropological Approaches to the Treatment and Maltreatment of Children (1987), and Death Without Weeping: The Violence of Everyday Life in Brazil (1992). She was the recipient of the Margaret Mead Award (1981), the Stirling Award (1985), a John Simon Guggenheim Fellowship (1987), and the Eileen Basker Memorial Prize in Medical Anthropology (1992).

"I was born in 1944 in the Eastern European Catholic and Hasidic immigrant neighborhood of Williamsburgh Brooklyn. It was at a time when recent refugees from war torn Europe were about to confront a new wave of impoverished and dislocated immigrants from rural Puerto Rico. It was within that clash of cultures, missed interpretations, and racial/religions misgivings, phobias, and hatreds that my anthropological educations began, quite unbeknownst to myself.

"At Queens College in New York City, I began my studies in English literature and philosophy and took my first course in anthropology - "Peoples and Cultures of Africa" taught by Hortense Powdermaker - as an elective and a bit of a lark. The assigned readings for that course, which included the classic writings of A. R. Radcliffe-Brown, Monica Wilson, Ira Schapera, E. E. Evans-Pritchard and others, captured my imagination even more than the seminars in existential

literature and philosophy. By the end of the semester (spring 1964), I decided to leave college to join the still nascent organization of the U.S. Peace Corps (see Scheper-Hughes 1993).

"Assigned to Northeast Brazil (1964-1966), I lived and worked as a paramedic and community organizer in a hillside shantytown or recently expulsed sugarcane cutters who had come to reside on the margins of a sugar plantation market town in the state of Pernambuco. It was there in the Alto do Cruzeiro that some initial traumas, doubts, and questions regarding the effects of scarcity, hunger, and infant death on what is commonly called 'mother love' first arose. I treated the subject in a series of short stories when I returned to the United States and re-enrolled in Queens College, this time as a student of creative writing. Once again, however, I was drawn to anthropology and to Hortense Powdermaker and I took her class on "culture and personality" which introduced me to the neo-Freudians in addition to the writings of Margaret Mead, Edward Sapir, Cora DuBois, George Spindler, Yehudi Cohen, and Melford Spiro, among others. But it was Powdermaker's own writings on the cultural psychology of race relations in the deep South (see Powdermaker 1943) that most excited me, and at the end of <u>that</u> semester (spring 1967), I again left school to work with SNCC (the Student Non-Violent Coordinating Committee) and other civil rights groups in Selma, Alabama. I was put in charge of a field project that explored hunger and malnutrition among more than 500 black farm families living in the blackbelt counties of Southwest Alabama (Scheper-Hughes 1968a, 1968b). The field reports were used in a civil rights class action suit, "Peoples vs. the Department of Agriculture" (Civil Action No. 544-68, U. S. District Court, Washington, D.C., April 1968) that helped bring the food stamp program into counties where the local white officials had initially tried to block it.

"As a graduate student in anthropology at U.C. Berkeley, I worked as a research assistant for

Hortense Powdermaker who had retired to California (see Scheper-Hughes 1991) while studying under the direction of Mary Diaz, Gerald Berreman, George Foster, George DeVos, and Margaret Clark, though I was also influenced by Elizabeth Colson and Eugene Hammel through their seminars on theory and method respectively. There was no single, dominant paradigm at Berkeley during these years when the campus politics of Vietnam, Cambodia, and the Third World Strike often eclipsed organized seminars. Meanwhile Dell Hymes (1969) radical challenge to conventional anthropology, Reinventing Anthropology, convinced my cohort of graduate students that if anthropology were to survive at all it must take a new and critical form. Some of my classmates chose the way of marxist and neo-marxist anthropology while others chose the way of inventing a feminist anthropology.

"I participated in both critical movements at Berkeley though much to the perplexity of some of my peers, I chose to study schizophrenia among bachelor farmers in rural Ireland as a projection of cultural themes for my dissertation research. Working in the early 1970s with the first translations of the writings of Michel Foucault, I was taken by his perception that madness was a cultural construction with a specific history that needed to be explored in a variety of social contexts.

"My first book, Saints, Scholars and Schizophrenics, was a blend of old and new approaches (of socialization and adult personality, TAT tests, and of reflexive/interpretive and critical anthropology). It was theoretically eclectic, combing and applying the insights of Erik Erikson, Gregory Bateson, R. D. Laing, and Michel Foucault to a tiny population of Irish speaking shepherds and fishermen. Soon after receiving the Margaret Mead Award, however, my Irish book became embroiled in a large and distressing controversy. The approach I was developing -

a kind of existential cultural critique - was viewed as "ethnocentric" in that it deviated from the implicit anthropological premise to write only about what is "good" and "right" about a given society and culture, especially (as in the case of western Ireland) a post-colonial society. One was not to use anthropology, as I had, in order to diagnose the ailing parts of the social body gone awry and I was labeled a dangerously "skillful pathologist of the human condition" by one Irish critic.

"My Irish book departed radically from Conrad Arensberg's (1937) description of Irish country life in The Irish Countryman because it was a child-centered ethnography told, not through the perspective of the old men seated comfortably at the pub and at the hub of rural life, but from the perspective of their sons, the young lads and boy-o's who would have to wait until their 40s and 50s to come into their own, and from the perspective of the young Irish village girls who could not wait to escape village life and its constraints. Saints, Scholars and Schizophrenics offered a counter-hegemonic view of Irish country life, but one that seemed to some sensibilities "anti-Irish", "anti-Catholic", or "anti-clerical". The fact that I had written only about "public secrets", those that everyone in the village knows - such as the high incidence of alcoholism, depression, and madness, sexual alienation and frustration - but that no one wants to talk about, was not consolation to Irish villagers who preferred to be "left alone" with their culturally derived defenses.

"As a post-doctoral fellow at Harvard University, I studied the problematic return of large numbers of hospitalized mental patients to the tough, working class neighborhood of South Boston. A series of papers (1981, 1983, 1987) diagnosing the "dilemmas of de-institutionalization" in the United States was followed by the publication of Psychiatry Inside

Out (1987), a co-edited translation of the writings of Franco Basaglia, a radical Italian marxist-phenomenologist, whose Democratic Psychiatry Movement contained the elements I saw as necessary for the kind of cultural revolution needed to return severe mental patients to a real place in society. Because of its uncompromisingly anti-institutional analysis, Psychiatry Inside Out was met with considerable hostility from the American psychiatric profession.

"Since 1982 my field research in Brazil has concentrated on the topics of mother love and child death, the medicalization of hunger, illness as protest, and on the ontological insecurity of the body for marginalized rural workers who are prey to the violence of hunger, medical maltreatment, and death squad torture and executions on a daily basis. A preliminary article, "Culture, Scarcity and Maternal Thinking" (1985), generated a heated controversy about the interpretation of emotions. The publication of Death Without Weeping: The Violence of Everyday Life in Brazil (1992) concludes a decade of anthropological research in the shantytowns of Northeast Brazil in the form of an experimental, "womanly-hearted" ethnography that weaves elements of narrative, plot, suspense, irony and other literary forms into a broad thesis on the effects of chronic scarcity, sickness, and death on the human spirit and on maternal thinking and practice in particular.

"At present, I am writing a book, The Rebel Body, that brings together a series of reflexive essays on the body as a primary sight of resistance and defiance in the post-modern world. "Embodied Knowledge" (the above chapter) is drawn from that work in progress.

"Many of the topics I have written about - stigma, scapegoating, and distributive injustice within families; the everyday violence of medicine and psychiatry when its practiced in bad faith; the madness of hunger and destructive hunger of motherhood in the shantytowns of

Brazil - are concerned with the need to ground anthropology in ethics. If anthropologists cannot begin to think critically about social institutions in moral or ethical terms, our discipline strikes me as quite weak and useless. But the problem of how to articulate a standard (or divergent standards) for moral reflections that does not privilege our own "Western" cultural assumptions and presuppositions remains open and unresolved.

MARILYN STRATHERN *is Professor of Social Anthropology at Manchester University and William Wyse Professor Elect at Cambridge. Initial fieldwork in the Papua New Guinea Highlands led to Women in Between (1972), the co- authored Self-Decoration in Mt. Hagen (1971) and then to two New Guinea Research Bulletins on dispute settlement and Hagen migrants. A critical assessment of that earlier work was among the reasons for writing The Gender of the Gift (1988), while Partial Connections (1991) addressed comparison after postmodernism. Kinship at the Core (1981) was a study of an Essex village, whose Gender of the Gift is so to speak After Nature (1992), a critique of the relationship between anthropological theorizing about individual and society and indigenous ('English') kinship. Most recent works are the collected essays, Reproducing the Future (1992), and the co-authored Technologies of Procreation (1993). She is a Fellow of the British Academy.*

"Going to Cambridge in 1960 cured me once and for all of the illusion that the universe might have a center; my own students have since demonstrated that there is nothing lineal about the transmission of ideas from teacher to pupil; and when they were little my children taught me that time comes in, also rather small, lumps. I have little faith in genealogies then;

rather, I imagine my work as contextualized and re-contextualized by others.

"In making the selective nature of this self-account apparent, I restrict myself to female figures. This is not to say that I have not been influenced by men. On the contrary, as a student I was deeply affected both by Jack Goody's sociological precision and by Edmund Leach's cultural transgressions, a duplex to some extent repeated in the difference between Andrew Strathern's heady pragmatism (I owe much of my fieldwork style to him) and Roy Wagner's relentless powers of recursion. If there is a descent line between A. Strathern and J. Goody, one of their inspirations was also initially mine: Radcliffe-Brown's Structure and Function, picked up in Foyle's, a once-famous second-hand bookshop, excited me before I even got to Cambridge, and the thesis (1968) that became Women in Between was gripped in the problematics of social order. In an odd interlude in Port Moresby, I read among other things Ann Oakley's newly-published Sex, Gender and Society (and in 1973 wrote a book on gender that was never published). But in the summer of 1978 I was struck sideways by Wagner's The Invention of Culture. The book derived much of its power from the antecedent it unwrote for me (Structure and Function); reading Michelle Stanworth's Reproductive Technologies ten years later in turn rewrote for me what a feminist agenda in the 199Os might look like.

"My mother, Joyce Evans, had led me to take for granted the rightness of focusing on women's affairs, a confidence broken by Annette Weiner's early criticism. Another duplex: between the complacency induced by a known audience and the shift that a critical one brings. Always polemical myself - divided between those for whom I write and those against whom I write - I realize I deserve what I get. And, when it matters, as in this case, eventually find gratitude.

"The first audience for my anthropological efforts was Doris Wheatley (Director of Studies at Girton, Cambridge), while Esther Goody and Audrey Richards marked - with a care I still recall - my second and third year essays. E. Goody became my Ph.D supervisor (in the same graduate cohort as Maurice Bloch and Adam Kuper), Paula Brown happily being appointed to oversee the fieldwork part in Papua New Guinea, and instilled in me a respect for social detail. Much later, Richards generously made available the materials she had accumulated on Elmdon, Essex, and on which Marianne Leach had subsequently worked, though I think she (i.e. Richards) never really liked the book I wrote from them. A steadying ethnographic influence in Port Moresby (1972-6) was Ann Chowning, whose staggering personal knowledge of the country defied most generalizations.

"It was coming away from the 1977 ASA conference organized by Jean La Fontaine that Carol McCormack and I found we both had problems with the nature-culture dichotomy. The collection of essays that followed (Nature, Culture, and Gender, 1980), further stimulated by a year at Canberra with the Gender Relations study group and a brief visit to Berkeley (where Elizabeth Colson was in her last year), threw me into The Gender of the Gift. Its moments of clarity owe much to the insights of more recent fieldworkers in Melanesia, including Debbora Battaglia, Aletta Biersack, Gillian Gillison and Margaret Jolly.

"At this point, as with my present colleagues at Manchester, history can only find expression as a current sense of debt. Indeed, if history requires a distance on moments thus made previous, then perhaps it is the uninvited guest who is always most influential: oneself. While I might otherwise have continued fieldwork in Papua New Guinea, I am grateful for the circumstances that have instead forced me into un-writing and re-writing my own work. Kinship

has recently resurfaced among my concerns, and in new form - recontextualized by the biological and cultural possibilities joined in the technologies of procreation. Brought up to think that anthropology's contribution always lay rather uniquely in this domain, I find the once luxury of re-thinking has in fact become an urgent and thoroughly practical necessity."

STANLEY J. TAMBIAH *is Professor of Anthropology at Harvard University, where he has taught since 1976. He began his field work in Sri Lanka (1956-59), the island of his birth, and then since 1960 has concentrated on Thailand, about which country he has written three monographs. Recently, since 1983, he has revived his interest in Sri Lanka, whose disastrous ethnic conflict has engaged him. He is the author of the following books:* Buddhism and the Spirit Cults in Northeast Thailand *(1970),* World Conqueror and World Renouncer: A Study of Religion and Polity in Thailand against a Historical Background *(1976),* The Buddhist Saints of the Forest and the Cults of Amulets: A Study of Charisma, Hagiography, Sectarianism and Millennial Buddhism *(1984),* Culture, Thought and Social Action *(1985),* Sri Lanka: Ethnic Fratricide and the Dismantling of Democracy *(1986), and* Magic, Science, Religion and the Scope of Rationality *(1990). A book entitled* Buddhism Betrayed? Religion, Politics and Violence in Sri Lanka *appeared in the spring of 1992. He served as the president of the Association for Asian Studies (1989-90), and is a fellow of the American Academy of Arts and Sciences.*

"My research and writing has in areal terms primarily related to South and Southeast Asia, and, in substantive and theoretical terms, touched on: (1) kinship and marriage transactions, (2) schemes of classification and their social uses, (3) communicative and performative features of

ritual, and (4) the interrelations among religion, politics and society both historically and in recent times. My current work on ethnic identity, ethnic conflict and collective violence, though primarily focused on South Asia, necessarily takes into account the momentous developments taking place in Eastern Europe and the (former) U.S.S.R."

"My graduate studies were conducted at Cornell University (1952-54) in what was then a joint department of sociology and anthropology. Especially under the tutelage of Robin Williams, Jr., who was himself a student of Talcott Parsons at Harvard, I was introduced to the writings of major social theorists, especially Max Weber, whose comparativist studies and theorizing on the major "world religions," systems of authority including charisma and the routinization of charisma, forms of rationality, and processes of historical change have had a major and enduring bearing on my own formulations. A close study of Durkheim's major writings was also an absorbing pursuit. On the anthropological side, aside from reading extensively ethnographic texts on South and Southeast Asia, I became familiar with the work of Robert Redfield, whose discussions of peasant societies, the folk-urban continuum, and primary and secondary civilizations were relevant to my own concern with processes of change in a variety of Sri Lankan rural communities. Morris Opler, Bryce Ryan, Lauriston Sharp and Peter Blau were some of my other instructors at Cornell.

"After completing my graduate studies I returned to Sri Lanka to teach for a few years, and then in 1960 I went to Thailand to engage in teaching and research under the auspices of UNESCO. In 1963, I went to Cambridge University in England, and became closely associated with Meyer Fortes, Edmund Leach, and Jack Goody. At Cambridge I acquired a detailed knowledge of their theoretical cum ethnographic contributions to the study of kinship, politics

and social organizations (and became familiar with a range of studies subsumed under the gross label "British structural-functionalism, for example, the work of Malinowski, Radcliffe-Brown, Evans-Pritchard, Audrey Richards, Max Gluckman, and Victor Turner. But my most important colleague, friend and "mentor" at Cambridge was Edmund Leach, whose developing interests in "structuralism," structural linguistics, semiotics and classification, and whose adaptation of the contributions of Roman Jakobson and Claude Lévi-Strauss positively stimulated me. My first monograph on Thailand and many of my essays in Culture, Thought and Social Action bear witness to Leach's influence, though at the same time I was discovering on my own the possibilities of Austinian linguistic philosophy for a performative theory of ritual.

"While at Cambridge I had also begun a new phase of field work in Thailand, complemented by library study, on the relation between Theravada Buddhism, kingship and polity - both historically and in contemporary times. I left England in 1973 for the United States to join the Department of Anthropology at the University of Chicago. There my many-stranded interests in comparative and historical studies, in the dialectical relation between religions, politics and society, and in the branches of semiotics (including the theories of Charles Peirce) were further clarified and extended by association with Marshall Sahlins, Michael Silverstein, Frank Reynolds (of the Divinity School) and several other colleagues.

"In 1976 I moved to Harvard, and have continued to pursue and enlarge these interests in both ethnographic texts and theoretical writings. But a new concern gripped me when the ethnic conflict in Sri Lanka between the Sinhalese and Tamils reached a crisis stage in 1983. I felt a strong personal need and an intellectual urge to try and grasp that conflict in the full knowledge that I was both a Sri Lankan, by birth a member of the Tamil minority, and an

anthropologist. A sustained interest in the ongoing Sri Lankan conflict and the knowledge that similar conflicts were raging in the neighboring countries of India, Pakistan, Bangladesh (and many other parts of the world, including most recently in Eastern Europe) have prompted me to undertake a comparative study of collective violence and civilian riots in South Asia. While all my previous theoretical and substantive interests and knowledge are relevant to the study of ethnic conflict and collective violence, I have the nervous as well as expectant sense that I am entering a domain which is difficult to map and demands a stretching of capacities to conceptualize and interpret."

ANDREW P. VAYDA *is a professor of anthropology and ecology at Rutgers University. Formerly a professor at Columbia University, he has taught also at the University of Indonesia (most recently in 1990) and other Indonesian universities and has directed research projects in Indonesia and Papua New Guinea. The journal, Human Ecology, was founded by him, and he was its editor for five years. He has published several books and more than ninety articles. His current interests lie in the areas of concepts, methods, and explanations in anthropology and human ecology; tropical forest adaptations and land-use changes; and interrelations of cognitive and techno-environmental change. Some of his articles reflecting these interests are cited in the bibliography of his article in this volume.*

"When I was a pre-law undergraduate student at Columbia University in the 1950's, what first attracted me to anthropology as a career was Elman Service's teaching, which presented a world view integrating Julian Steward's ecological orientation with Leslie White's evolutionism

and culturology. Service left Columbia just before I became a graduate student there, and so Morton Fried, who shared many of Service's ideas and interests, became my main teacher and advisor. Although I abandoned evolutionism and culturology many years ago, it is probably to Service and Fried that I owe my abiding interest in relations between people and their environments. My specific research, concerned mainly with the South Pacific and with maritime southeast Asia, has included both extensive work with documentary materials and several periods of anthropological and ecological field work.

"The book, Maori Warfare and the 1956 Ph.D. dissertation from which it derived, were based on study in the libraries and archives of New Zealand during 1954-55, and my interest in war in relation to environmental and demographic phenomena (the subject of my 1976 book, War in Ecological Perspective) dates from this period. In 1956-57, I did research on cultural change on three coral atolls of the Northern Cook Islands. From 1958 to 1960, I taught at the University of British Columbia and expanded my historical research to encompass warfare in Borneo as well as in New Zealand; I also began research with Wayne Suttles on the relation between fluctuations in food resources and the occurrence of ceremonial distributions of goods in Northwest Coast Indian and Melanesian societies. All of this research continued after I joined the faculty of Columbia University in 1960. Fifteen months during 1962-63 and four months in 1966 were spent in New Guinea on a project which allowed me and my associates in a multi-disciplinary team to test in the field some of the propositions that had emerged from the library research on war, economics, and ecology.

"Collaborations with Anthony Leeds, Roy Rappaport, and the philosopher, Paul Collins, and much of my own work during the 1960's were concerned with cultural mechanisms regarded as

contributing to balances between human populations and their resources. Then, in the 1970's, I became increasingly concerned with 'unbalanced' relations between people and their environments. The move in 1972 from Columbia University, where I was then a full professor, to Cook College at Rutgers University was motivated partly by the desire to do research on such relations in contemporary settings and by the expectation that the college's interdisciplinary ecological programs would afford better opportunities for such research than I had had at Columbia. Founding and editing the interdisciplinary journal, Human Ecology, also contributed to changing the direction of my work and interests because of the increased contact with scholars in many disciplines and with a wide range of work on people-environment relationships in large modern societies as well as in the small, relatively isolated ones such as those I had previously studied. In accord with these reorientations, in 1974 I became associated with UNESCO's Man and Biosphere (MAB) Program, an international research and training program for developing an integrated social and ecological approach to problems and for providing information and methods for improved environmental policy-making and management. In connection with this program, I participated in numerous international conferences and in feasibility studies concerned with forest conversion and conservation in Indonesia and Malaysia. Also, from 1979 to 1984, I directed two U.S.-Indonesian interdisciplinary research projects on interrelations of human actions and biotic change in the forests of the Indonesian province of East Kalimantan. The rapid changes occurring there, involving diverse movements of people, resources, and ideas across social, geographical, and ecosystemic boundaries, made me see more clearly the limitations of equilibrium approaches in ecological anthropology or human ecology and made me question more strongly the

assumptions whereby such predefined wholes as cultures, societies, communities, and ecosystems are made the units of analysis in social and ecological science. Since completion of the East Kalimantan investigations, I have been much concerned with these and related methodological issues (including issues that philosophers and others have raised concerning contingency, human action, agency, and intentionality) and have been devoting much of my teaching and writing to them.

"In thinking about such matters, I have received stimulation and insight from discussions with such anthropological colleagues as Bonnie McCay, George Bond, George Morren, Susan Lees, Fredrik Barth, Iwan Tjitradjaja, Myron Cohen, and James Anderson, and I have benefited from reading widely outside of anthropology. Among the authors to whom I feel especially indebted are the following: Karl Popper, for ideas about situational analysis and unintended consequences; Isaiah Berlin, for liberation from theoretical and methodological monism; philosophers like Donald Davidson and John Searle, for clarification of the critical role of intentionality in explaining human actions; Alan Garfinkel, for the concept of explanatory relativity; Jon Elster and Harold Kincaid, for bolstering my impatience with theories and clarifying alternatives to them in social science explanations; and Stephen Jay Gould, for ideas about the.importance of chance and contingency in historical change and evolution.

"My interests in both cognitive and ecological anthropology were brought together in my most recent Indonesian field experiences, which occurred in Java in 1990 and in East Kalimantan in 1992 and were focused on variation and change in the agro-ecological knowledge and practices of rice-farmers. With my long-time ecological collaborators, Timothy Jessup and Kuswata Kartawinata, I am now making plans for new field work in East Kalimantan on

interrelations of cognitive, behavioral, and environmental phenomena."

SYLVIA YANAGISAKO *is a professor of anthropology at Stanford University where she has been teaching since 1975. She has conducted research among American Indians and Japanese Americans in the state of Washington and among Italian capitalist families in Como, Italy. Her book Transforming the Past: Kinship and Tradition among Japanese Americans offers a theoretical framework for understanding the historically-specific processes through which people interpret and transform their kinship relationships. She co-edited the collection Gender and Kinship: Essays Toward a Unified Analysis with her colleague Jane Collier, with whom she co-authored the theoretical overview appearing in this volume.*

"The initial intellectual influences on me can be traced to the pre-World War II plantation political-economy of Hawaii and its post-World War II transformation. I grew up in Hawaii at a time when a plantation society dominated by a white, landholding oligarchy was being rapidly transplanted by a more diversified military-tourist economy controlled by a more ethnically-diverse coalition of interests. My family's history of plantation labor and petty entrepreneurship endowed me with a vigilant suspicion towards all myths of a unified Social Good. At the same time, the ethnically heterogeneous and racially-mixed population of students who were my classmates in Honolulu taught me the fluidity and negotiated character of ethnic identity and cultural practices, albeit within a structure of political-economic inequality.

"The first teacher to have a major intellectual influence on me was Ms. Setsu Okubo, my

teacher at Roosevelt High School in Honolulu, who turned a required civics course for seniors into a compelling critique of North American military-industrial interests. In the context of the issues facing the United States in the early sixties - including the Cold War and the Cuban missile crisis, the Black civil rights movement, and the war on poverty - I came to question the ideology of those who claimed they governed in the interests of all people.

"During my undergraduate and early graduate career at the University of Washington in the Sixties, my intellectual perspective was shaped less by my professors than by the national and international dialogue generated by the anti-Vietnam war movement, the youth cultural revolt, and the Black civil rights and Black power movements from which the former two drew a good deal of their inspiration, political analysis, and tactics. The explosion of underground publications and other fora for political and cultural critique provided me with alternative theories and concepts to the structural-functionalist, Durkheimian models of society then dominant in American anthropology.

"After completing an M.A. thesis on the educational problems faced by Indian children in white-controlled public school systems which left me unsatisfied with anthropology, I dropped out of graduate school with friends to organize a commune in Hawaii. This experiment in communal living taught me a great deal about the pervasiveness of gender inequality, including my own internalization of ideologies of gender and power. Although I was somewhat isolated from the developments unfolding outside my community, I soon realized that my personal experience paralleled the emergence of the second wave of feminism in the rest of the country.

"When I returned to the University of Washington after a three-year break in my graduate studies, I found much had changed. My plan to continue research among American Indians was

precluded by the growing Native American movement and its critique of the role of anthropologists in their cultural domination. My advisor Laura Newell, a physical anthropologist interested in population issues among Japanese Americans, suggested I do a kinship study of them, because she wanted to understand the cultural forces shaping population processes. At the same time, another faculty advisor, Michael Lieber, introduced me to David Schneider's book American Kinship: A Cultural Account. This book, along with Schneider's articles, challenged the course kinship studies and anthropological theory had taken for the past century. Although Schneider drew upon a tradition of cultural theory that can be traced through Max Weber, Franz Boas, Ruth Benedict, and Talcott Parsons, his treatment of American folk and anthropological ideas about biological kinship as a symbolic system denaturalized kinship in a way that had never been done before, opening up new lines of analysis which are still being explored today.

"My first years as a faculty member in the Stanford anthropology department (1975-1980) introduced me to a broad range of critical cultural theory which had been lacking in my graduate career. I learned much from Jane Collier, Bridget O'Laughlin, Michelle Rosaldo, Renato Rosaldo, and later Donald Donham about how cultural analysis could be made a more powerful tool when it attended to issues of power and inequality. In the second half of the 1980s, the interdisciplinary faculty seminar on Cultural Studies at Stanford provided an intellectual home for me and for my exploration of cultural theory in other disciplines.

In the Eighties, my perspective was also strongly influenced by my graduate students. Lisa Rofel, Anna Tsing and Kath Weston taught me, among other things, to question the heterosexist assumptions of kinship studies and cultural theory, including much of feminist

theory. Roger Rouse taught me to locate anthropological theories within their own national and transnational material histories of political crises and cultural movements. This lesson is quite obviously reflected in the way I have constructed this narrative of my intellectual development.

"I am currently writing a monograph on the transformation of gender, family, and industry in capitalist family firms in the silk industry of northern Italy. In focusing on the ways in which changing ideas about gender have shaped the transformation of industrial firms in an advanced industrial society, my study extends feminist analysis beyond its conventional domain of the study of women and institutions that have been construed as "female" to the heart of the presumably "male" sphere of industrial structure and the economy."

ERIC R. WOLF *is Distinguished Professor of Anthropology at Herbert Lehman College and the Graduate Center of the City University of New York. He has done field work in Puerto Rico, Mexico, and the Italian Alps. He has contributed to the comparative study of peasantries and to work on the articulation of complex societies. His best known books are Sons of the Shaking Earth (1959), Peasants (1964), and Europe and The People Without History (1982). He is a member of the American Academy of Arts and Sciences.*

"I suspect that my intellectual drive springs from the predicaments of growing up in a thoroughly assimilated Jewish family in multi-ethnic, but ever more nationalist and anti-Jewish Central Europe. As a child I was fascinated by the life-ways of animals. In my early 'teens I discovered mountaineering and Germanic folklore. Displaced to England in my late 'teens, I

encountered natural science, and read J. B. S. Haldane. I began college in New York (1940) with the idea of studying biochemistry, but then vagabonded through the social sciences, eventually coming to rest in Hortense Powdermaker's course on culture and personality and Joseph Bram's lectures on the anthropology of Asia. Powdermaker had studied with Malinowski at L.S.E.; Bram had been a student of Boas.

"Three years in the U.S. mountain troops then provided the funds, on the G.I. Bill, to finish college and pursue graduate anthropology at Columbia University. In the war years I also expanded my understanding of socialist perspectives, especially through reading the economist Paul Sweezy, the Caribbean polymath C.L.R. James, and - in the summer preceding graduate school - Karl Wittfogel's ecological/political-economic study of China (1931). At Columbia I first took course with Ruth Benedict, and later with Julian Steward, who invited me to join his field work team in studying Puerto Rico (1948-49). This work, in turn, led to further immersion in the problems of Latin America. Studies at Columbia also put me in contact with an unusually able group of fellow-students - Stanley Diamond, Morton Fried, Robert Manners, Daniel McCall, Sidney Mintz, and Elman Service. We formed a study group and learned much from one another. John Murra sometimes joined the group, when in New York on visits from Chicago.

"Benedict and Steward, each in their own way, had intensified my own interest in how sub-groups and regions came to be welded into over-arching nations, and I pursued this interest further in library and field research in Mexico (1951-52). There I encountered two gifted Spanish refugees - Pedro Armillas, who introduced me to a new kind of archaeology, and Angel Palerm, with whom I came to share convergent intellectual and political concerns. These contacts and engagements then made the fifties a period of productive inquiry into the

interplay of groups and institutions during the course of Mexican history.

"After teaching at a variety of institutions, I returned to field work on ecology and nationhood in two peasant communities located on either side of a language frontier in the Italian Alps (1960-61). I was then fortunate to join a group of colleagues at the University of Michigan who were working on a synthesis of ecological and evolutionary perspectives, a group that then included Marshall Sahlins, Elman Service, and Roy Rappaport. Where I had focused on Mexico in the 'fifties, the decade of the 'sixties at Michigan allowed me to pursue further the comparative study of peasantry. I was also able to take part, with William Schorger, in sending students into the field to both the European and the African shores of the Mediterranean; and John W. Cole, then a graduate student, and I carried forward our field study or the communities in the Alps that would eventually result in a book, The Hidden Frontier (1972). In 1971 I moved to the City University of New York to teach undergraduates at Lehman College in the Bronx, and graduate students at the Graduate Center expanded the scope of my inquiries to deal with the inclusion and participation of widely diverse societies and cultures in global systems of interaction. This work led to the writing of Europe and The People Without History (1982). I remain both fascinated and puzzled by the way in which the social and cultural entities studied by anthropologists are at once interlinked with one another, and yet repeatedly insistent on their separateness and distinctive character."