AN INCONSTANT PROFESSION:
The Anthropological Life in Interesting Times

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Abstract   I give an overall view of anthropology and of my career within it over
the past fifty years, relating them to changes in the world in general during that time.
All lessons are implicit, all morals unstated, all conclusions undrawn.

INTRODUCTION

I have arrived, it seems, at that point in my life and my career when what people
most want to hear from me is not some new fact or idea, but how I got to this
point in my life and my career. This is a bit discouraging, not just because of
its *memento mori* overtones (when you are seventy-five, everything has *memento
mori* overtones), but because, having spent the whole of my adult life trying to
push things forward in the human sciences, I am now being asked to consider what
that has entailed—why I think my direction can be called forward, and what, if
that direction is to be sustained, the next necessary thing might be. As a result, I
have engaged in the past few years in at least two more or less organized attempts
to describe the general curve of my life as a working anthropologist, and this essay
will be the third, and, I trust, the last. Talking about one’s self and one’s experiences
in a homiletical manner—“go thou and do likewise”—is a bit much the first time
around. Recycled, it loses charm altogether.

The first of these essays in apologetical retrospection, originally given as a
Harvard-Jerusalem lecture in 1990, became the chapter entitled “Disciplines” in
my book *After the Fact* (Geertz 1995a). There I concentrated mostly on matters of
research and scholarship, most especially on my long-term fieldwork in Indonesia
and Morocco—a story of projects leading to outcomes leading to other projects
leading to other outcomes. The second, originally given as an American Council
of Learned Societies “Life of Learning” lecture in 1999, became the first chapter,
entitled “Passage and Accident,” of my most recent book, *Available Light* (Geertz
2000). There I presented a more personal, semi-introspective account of both my
life and my career; a sort of sociointellectual autobiography and self-accounting.
This time—this last time—I want to do something else: namely, to trace the
development of anthropology as a field of study over the more than half-century, 1950–2002, I have been involved in it, and to trace, too, the relationships between that development and the broader movements of contemporary history. Though this also, of necessity, produces something of a “the things I have been through and the things I have done” sort of narrative, I am, for the most part, not concerned with either my work or my persona. I am concerned with what has happened around me, both in the profession in which I have been, however loosely and at times uncomfortably, enclosed, and in what we are pleased to call “the wider world,” in which that profession has been, however marginally and insecurely, enclosed. That world is with us late and soon: There is very little in anthropology that is genuinely autonomous; pretensions to the contrary, however dressed in the borrowed clothes of “science,” are self-serving. We are, like everybody else, creatures of our time, relics of our engagements.

Admittedly, this is a little vast for a short essay, and I am obliged to pass over some very large matters very quickly, ignoring detail and suppressing nuance and qualification. But my intent is not to present a proper history, an inclusive summary, or a systematic analysis. It is, instead,

1) To outline the succession of phases, periods, eras, generations, or whatever, both generally and in anthropology as such, as I have lived through it, and them, in the last half of the last century, and,

2) To trace the interplay between (for the most part, American and European) cultural, political, social, and intellectual life overall and anthropology as a special and specialized profession, a trade, a craft, a métier.

Whether such broad-stroke, impressionistic, the-view-from-here sketching will yield much in the way of insight into how things are, and have been, heading in our field remains to be seen. But, absent a crystal ball, I know of no other way. So far as phases, periods, eras, and the like are concerned, I shall, for my own convenience, mark out four of them. None of them is internally homogeneous, none of them is sharply bounded; but they can serve as useful place-markers in a lurching, tangled, digressive history. The first, roughly between 1946 and 1960—all dates are movable—was a period of after-the-war exuberance, when a wave of optimism, ambition, and a sense of improving purpose swept through the human sciences. The second, about 1960 to about the mid-1970s, was dominated, on the one hand, by the divisions of the universalized cold war, and, on the other, by the romances and disappointments of Third-Worldism. From 1975 or so to, shall we say, in honor of the fall of The Wall, 1989, there was, first, a proliferation of new, or anyway newfangled, approaches to social and cultural analysis, various sorts of theoretical and methodological “turns,” Kehre, tournures d’esprit; and then, on the heels of these, the rise of radically critical and dispersive “post-” movements, brought on by increasing uncertainty, self-doubt, and self-examination, both within anthropology and in Western culture generally. Finally, from the 1990s until now, interest has begun to shift toward ethnic conflict, violence, world-disorder, globalization, transnationalism, human rights, and the like, although where that is going,
especially after September 11, is far from clear. These, again, are not the only cuts that could be made, nor even the best. They are but the reflections, diffuse and refracted, in my own mind of the way of the world and the ways of anthropology within the way of the world.

POSTWAR EXUBERANCE

During the second world war, American anthropologists were, like American sociologists, historians, psychologists, and political scientists, drawn, almost to the man or woman, into government service. After it ended, in what was, in the United States anyway, not that long a time, three or four years, they returned, immediately, again almost to the man or woman, to academia with their conception of themselves and their profession radically altered. What had been an obscure, isolate, even reclusive, lone-wolf sort of discipline, concerned mainly with tribal ethnography, racial and linguistic classification, cultural evolution, and prehistory, changed in the course of a decade into the very model of a modern, policy-conscious, corporate social science. Having experienced working (mostly in connection with propaganda, psychological warfare, or intelligence efforts) in large, intellectually diverse groups, problem-focused collections of thrown-together specialists, most of whom they had previously known little about and had less to do with, anthropologists came back to their universities in a distinctly experimental frame of mind. Multi- (or inter-, or cross-) disciplinary work, team projects, and concern with the immediate problems of the contemporary world were combined with boldness, inventiveness, and a sense, based mainly on the sudden availability of large-scale material support both from the government and from the new mega-foundations, that things were, finally and certainly, on the move. It was a heady time.

I encountered all this at what may have been its point of highest concentration, greatest reach, and wildest confusion: Harvard in the 1950s. An extraordinary collection of persons and personalities had gathered there, and at the nearby Massachusetts Institute of Technology, launching programs in all directions. There was the Department of Social Relations, which—chaired by the systematic sociologist Talcott Parsons, and animated, rather diffusely, by his rather diffuse “General Theory of Social Action”—combined sociology, anthropology, clinical psychology, and social psychology into an at least terminologically integrated whole (Parsons & Shils 1951). There was the Russian Research Center, headed by the cultural anthropologist Clyde Kluckhohn (1951); the Psychological Clinic, headed by the psychoanalyst Henry Murray (1938); the Laboratory of Social Relations, headed by the social statistician Samuel Stouffer (Stouffer 1949). John and Beatrice Whiting, in from Yale, assembled a team and began exploiting the newly created Human Relations Area Files for comparative correlation studies of socialization (BB Whiting & J Whiting 1975). And at MIT, there was the Center for International Studies dedicated to stimulating modernization, democratization, and takeoff in the new states of Asia and Africa and the stranded ones of Eastern Europe and Latin America (Millikan & Blackmer 1961). Just about everything that was in any way in the air
in the social or, as they soon came to be called as the pressures toward unification intensified, the behavioral sciences—from group dynamics (Homans 1950), learning theory (Tolman 1958), and experimental psychology (Bruner & Krech 1950) to structural linguistics (Jakobson 1952), attitude measurement (Allport 1954), content analysis (Inkeles 1950), and cybernetics (Wiener 1962)—was represented by one or another Institute, one or another Center, one or another Project, one or another entrepreneur. Only Marxism was missing, and a number of the students happily provided that (for a general critique from the left of all this, see Diamond 1992).

For me, as a would-be anthropologist—one who had never had an anthropology course and had no particular aim in mind except to render himself somehow employable—the figure I had most to come to terms with in this swarm of talkative authorities was Clyde Kluckhohn. A driven, imperious, rather haunted man, with an enormous range of interests, a continuously restless mind, and an impassioned, somewhat sectarian sense of vocation, he had read Classics at Oxford as a Rhodes Scholar. He had studied the Navajo and other peoples in the American Southwest since having been sent there as a teenager for his health, and he knew his way around the corridors of power, both in Washington (where he had worked as consultant to the Secretary of War and directed morale surveys for the Office of War Information) and, an even greater achievement (considering he had been born obscure in Iowa) at Harvard. The author of what was then the most widely read, and best written, statement of what anthropology was all about, Mirror for Man (1949), a past president of the American Anthropological Association, a fierce controversialist, a player of favorites, and a master money-raiser, Kluckhohn was rather a presence.

Of the various collective enterprises (thinking back, I count at least eight, and there were probably more) that Kluckhohn was at that moment either directing, planning, or otherwise animating, I myself became involved, in turn, in three, which, taken together, not only launched my career but also fixed its direction.

The first, and smallest, was the compendium of definitions of culture Kluckhohn was preparing in collaboration with Alfred Kroeber, then in his late seventies and concluding a sovereign career in detached retirement (Kroeber & Kluckhohn 1952). I was given what, with the aid of other, more senior, graduate students, they had assembled and what they had written in the way of commentary, and I was asked to review it and offer suggestions. I had some suggestions, most of them expository, a few of which were attended to; but the most fateful result of the experience for me was that I was inducted into the thought-ways of the particular form of anthropology then called, rather awkwardly, pattern theory or configurationalism. In this dispensation, stemming from work before and during the war by the comparative linguist Edward Sapir at Yale and the cultural holist Ruth Benedict at Columbia, it was the interrelation of elements, the gestalt they formed, not their particular, atomistic character, as in previous diffusion and culture area studies, that was taken to be the heart of the matter. A phoneme, a practice, a role, an attitude, a habit, a trait, an idea, a custom was, as the slogan had it, “a point
in a pattern”; it was systems we were after, forms, structures, shapes, contexts—the social geometry of sense (Kluckhohn 1962, Sapir 1949, Benedict 1934).

A large number of expressions of this approach to things current in anthropology appeared at that time. Perhaps the most visible and influential, though as it turned out not so long-lived, was the so-called culture and personality movement, in the service of which Kluckhohn, Murray, and a junior member of the Social Relations Department, David Schneider, put together a more or less definitive reader (Kluckhohn et al. 1949). Strongly influenced by psychoanalytical ideas and by projective testing methods, it sought to relate the processes of individual psychological development to the cultural institutions of various societies. Abram Kardiner and Ralph Linton at Columbia, Cora DuBois, first at Berkeley then at Harvard, Erik Erikson, also first at Berkeley and then at Harvard, and Kluckhohn himself in his Navajo work (Kardiner & Linton 1939, Du Bois et al. 1944, Erikson 1950, Leighton & Kluckhohn 1947) were perhaps the most prominent figures in the movement, and Margaret Mead was its battle-fit, out-front tribune; but it was very widespread (Hallowell 1955, Piers & Singer 1953, Wallace 1970). Closely allied to culture and personality there were the so-called national character or culture-at-a-distance studies, such as Benedict’s on Japan, and Mead’s, Rhoda Métraux’s and Geoffrey Gorer’s on Europe and America (Benedict 1949; Mead 1942; Mead & Métraux 1953; Métraux & Mead 1954; Mead & Rickman 1951; Gorer 1948, 1955; Gorer & Rickman 1963), and, of course, those of the Russian Research Center, where sociologists, psychologists, political scientists, and anthropologists attempted to assemble a collective portrait of “the new Soviet man” out of the analysis of communist writings and refugee life-histories (Bauer 1959, Bauer et al. 1956).

My interest in all this was limited by what seemed to me its somewhat mechanical, destiny-in-the-nursery quality and the vastness of its explanatory ambitions. So I drifted instead toward another of Kluckhohn’s large-scale, long-term, multidiscipline, multi-inquirer, systematical enterprises in the interpretation of cultures, the so-called Comparative Study of Values or Ramah (later Rimrock) Project. This project, methodical and well financed, was dedicated to describing the value systems (world-views, mental attitudes, moral styles) of five geographically adjacent but culturally discrete, small communities in northwestern New Mexico—Navajo, Zuni, Spanish American, Mormon, and Anglo (or Texan). Over a period that finally stretched to twenty years or so, dozens of researchers from a wide variety of crossbred specialties—moral philosophers, regional historians, rural sociologists, American Indianists, child psychologists—were dispatched to one or another of these sites to describe one or another aspect of the life being lived there. Their fieldnotes, hundreds upon hundreds of pages of them, were then typed up on cards and filed in the Human Relation Area Files manner at the Peabody Museum of Anthropology, where they could be commonly consulted and a long string of special studies, and finally a collective volume, written (Vogt & Albert 1966, Vogt 1955, Kluckhohn & Strodtbeck 1961, Smith & Roberts 1954, Ladd 1957). As for me, I did not go to the Southwest but worked for some months in the files, then already vast and varied, on a subject set by Kluckhohn—the differential
responses of the five groups to problems set to them all by the common conditions of their existence as small, rural, more or less encapsulated communities: drought, death, and alcohol. Mormon technological rationalism, Zuni rain dancing, Spanish-American dramatic fatalism in the face of drought, Navajo fear of ghosts, Mormon eschatological schemes, Anglo grief-avoidance in the face of death, Zuni sobriety, Mormon puritanism, and Navajo spree drinking in the face of alcohol—all were outlined, rather schematically, and attributed, rather speculatively, to their differing value systems (Geertz, unpublished observations). But whatever the limitations of the report I produced (and it wasn’t all that bad as a first pass at things), the experience turned out to be both a sort of dry-run for the kind of field research—comparative, collaborative, and addressed to questions of meaning and significance—that I would spend the rest of my life pursuing; and a transition to the next phase or period of the immersion of anthropology in the movement of the times: the age of modernization, nation-building, and the all-enveloping Cold War.

MODERNIZATION AND THE COLD WAR

The Center for International Studies at the Massachusetts Institute of Technology, which I mentioned earlier as part of the cluster of social science holding-companies emerging in post-war Cambridge, was set up in 1952 as a combination intelligence gathering and policy planning organization dedicated to providing political and economic advice both to the rapidly expanding U.S. foreign aid program and to those it was ostensibly aiding—the “developing,” “under-developed,” or, for the less sanguine, “backward” countries of Asia, Africa, and Latin America. At first, the Center, something of an anomaly in an engineering school not much given at that time to social studies of any sort, was hardly more than a secretary, a suite of offices, a name, a large amount of money, and a national agenda. In an effort simply to get it up and running, Kluckhohn, who, still moving in mysterious ways, had again been somehow involved in its formation, proposed that a team of doctoral candidates from Harvard social science departments be formed and sent to Indonesia under its auspices to carry out field research in cooperation with students from that country’s new, European-style universities. Five anthropologists, including myself and my then wife, Hildred, also a Social Relations student; a sociologist who was a historian of China; a social psychologist; and a clinical psychologist were given a year of intensive work in the Indonesian language and sent off for two years to the rice fields of eastern Java (not all of them got there, but that’s another story) to carry out, ensemble, parallel, interconnected, and, so it was hoped, cumulative researches: the Ramah Project model updated, concentrated, and projected abroad.

The ups and downs of this enterprise, which itself came to be called “The Modjokuto Project” and the degree to which it achieved the ends proposed to it, have been retailed elsewhere (Geertz 1995a). For the present “March of Time” sort of story, its significance lies in the fact that it was, if not the first, surely one of the earliest of what soon turned into a flood of efforts by anthropologists, or teams of them, to adapt themselves and their tribes-and-islands discipline to
the study of large-scale societies with written histories, established governments, and composite cultures—nations, states, civilizations. (For another early effort in this direction, see Steward et al. 1956.) In the years immediately following, the number of such country-focused projects multiplied (as did, of course, as a result of decolonization, the number of countries), and a sort of super-discipline called area studies, eclectic, synoptical, reformatory, and policy-conscious, came into being to support them (Steward 1950; Singer 1956; Redfield 1953, 1956).

When the Modjokuto team left for Southeast Asia, the Center, as I mentioned, did not yet really exist as a going concern, so its connection with the work we did there—essentially historical and ethnographic, a refitted community study—was nominal at best. By the time we returned to Cambridge, three years further on, however, it had become a large, bureaucratized organization with dozens of specialized researchers, most of them economists, demographers, agronomists, or political scientists, engaged in development planning of one sort or another or serving as in-country policy consultants to particular governments, including that of Indonesia. The work of our team seemed, both to the Center staff and to ourselves, to be rather to the side of the Center’s mission, inconsonant with its “applied” emphasis and too concerned with what the program-minded types took to be parochial matters. We drifted away into writing our separate theses on religion, kinship, village life, market selling, and other irrelevancies, and beginning, finally, our academic careers. I, however, was rather more interested in developmental questions, and in state formation, than my colleagues, and I wished to return as soon as possible to Indonesia to take them up. So, after gaining my doctorate, I rejoined the Center and became more directly involved in its work and with the master idea that governed it: modernization.

This idea, or theory, ubiquitous in Third World studies during the 1960s and early 1970s, and, of course, not all that dead yet, stemmed from a variety of sources. Most particularly, it grew out of the writings of the German sociologist Max Weber and his American followers (of whom, Talcott Parsons was perhaps the most prominent, and certainly the most insistent) on the rise of capitalism in the West (Weber 1950a, b, 1947, 1965; Tawney 1947; Parsons 1937; Bendix 1962; Levy 1960; Eisenstadt 1966; Black 1976). Weber’s conception of the history of the West since the Renaissance and the Reformation was that it consisted of a relentless process of economic, political, and cultural rationalization, the instrumental adjustment of ends and means, and he saw everything from bureaucracy, science, individualism, and double-entry bookkeeping to the industrial organization of labor and the disciplined management of inner life as expressions of such a process. The systematic ordering of the entirety of human existence in rational terms, its imprisonment in an “iron cage” of rule and method, was what, in its essence, modernity was. In particular, his famous, in some quarters infamous, Protestant Ethic thesis—that the harsh, predestinarian beliefs of Calvinism and related inner-worldly ascetic doctrines of the sixteenth and seventeenth centuries provided the moral legitimation and driving force for the tireless pursuit of profit under bourgeois capitalism—spurred a whole host of studies designed to support
and extend it, to find signs and portents of such progress-producing value systems in that most residual of residual categories, the nonmodern, nonrational, noncapitalist non-West (Bellah 1957, 1965; Eisenstadt 1968; Geertz 1956, 1963b).

As for me, my original thesis proposal, put temporarily aside to address myself to describing Javanese religion more generally for the purposes of the common project, was to pursue the possibility that reformist (or modernist) Islam might play a role in Indonesia similar to that which Weber’s Calvinism supposedly played in the West. So, after writing a short book at the Center on the history of Javanese agriculture, which ascribed its failure to rationalize along the capital-intensive, labor-saving lines experienced earlier in the West and, in a somewhat different way, in Japan, to the colonial policies of the Dutch (Geertz 1963a), I headed back to Indonesia hoping to address the Weberian thesis in a more direct and systematic, hypothesis-testing way. I would, I thought, spend four or five months each in a strongly Islamic region in Sumatra, a strongly Calvinist region in Sulawesi, and a Hindu region in Bali and try to ferret out the effects, if any, of different varieties of religious belief on the modernization of economic behavior.

But a funny thing happened on the way to the field. The cold war, previously fought out (the rather special case of Korea perhaps excepted) in the client and satellite states of Europe, shifted its center of gravity to the Third World, and most especially to Southeast Asia. All this—the Malaya emergency, the Vietnam war, the Khmer Rouge, the Huk rebellion, the Indonesian massacres—is much visited, much disputed, history, and I will not rehearse it again here. Suffice it to say this development altered the whole scene of action for those of us trying to carry out field studies in such suddenly world-critical places. The induction of the obsessions and machinations of the East-West confrontation into entrenched, long-standing divisions in religious, ethnic, and cultural life—another, less foreseen, form of modernization—brought local, hand-to-hand politics to a furious boil just about everywhere it occurred, and it occurred just about everywhere.

From the end of the 1950s to the beginning of the 1970s, the charismatic, hero-leaders of the new states—Nehru, Nkrumah, Nasser, Ben Bellah, U Nu, Ayub Khan, Azikwe, Bandanaraiake, Sihanouk, Ho, Magsaysay, Sukarno—bedeviled within and without by these pressures toward ideological polarization, struggled to position their countries in the ever-narrowing, unfilled space between the powers: neutral, nonaligned, newly emerging, “tiers monde.” Indonesia, which soon found itself with both the largest Communist Party outside the Sino-Soviet bloc and an American-trained and -financed army, was in the very forefront of this effort, especially after Sukarno organized the Bandung Conference of 29 Asian and African nations, or would-be nations, in that west Javanese city in 1955 (Kahin 1956, Wright 1995). Nehru, Chou, Nasser, and Sukarno himself all addressed the Conference, which led on to the formal creation of the nonaligned movement. All this, and the general unfolding of things, made of Indonesia perhaps the most critical battleground after Vietnam in the Asian cold war. And in the mid-1960s it collapsed under the weight: failed coup, near civil-war, political breakdown, economic ruin, and mass killings. Sukarno, his regime, and the dreams of Bandung, never more
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than dreams, or self-intoxications, were consumed, and the grimmer, less romantic age of the kleptocrats, Suharto, Marcos, Mobutu, Amin, and Assad emerged. Whatever was happening in the Third World, it did not seem to be the progressive advance of rationality, however defined. Some sort of course correction in our procedures, our assumptions, and our styles of work, in our very conception of what it was we were trying to do, seemed, as they say, indicated.

AN EXPLOSION OF PARADIGMS

By the time I got back to the United States toward the beginning of the 1960s (my neat little three-way project spoiled by the outbreak of anti-Sukarno rebellions in Sumatra and Sulawesi, I had spent most of the year in Bali), the destabilizing effects of the deepening of the great power confrontation in Southeast Asia were beginning to be felt with some force there as well. The profession itself was torn apart by charges and countercharges concerning the activities, or supposed activities, of anthropologists working in Vietnam. There was civil rights and “The Letter from Birmingham Jail,” civil liberties and the Chicago Seven. The universities—Berkeley, Harvard, Columbia, Cornell, Kent State, Chicago—erupted, dividing faculty, inflaming students, and alienating the general public. Academic research on “underdeveloped” countries in general, and on “modernization” in particular, was put under something of a cloud as a species of neoimperialism, when it wasn’t being condemned as liberal do-goodism. Questions multiplied rapidly about anthropology’s colonial past, its orientalist biases, and the very possibility of disinterestedness or objective knowledge in the human sciences, or indeed whether they should be called sciences in the first place. If the discipline was not to retreat into its traditional isolation, detached from the immediacies of contemporary life—and there were those who recommended that, as well as some who wished to turn it into a social movement—new paradigms, to borrow Thomas Kuhn’s famous term, first introduced around this time (Kuhn 1962), were called for. And soon, and in spades, they came.

For the next fifteen years or so, proposals for new directions in anthropological theory and method appeared almost by the month, one more clamorous than the next. Some, like French structuralism, had been around for awhile but took on greater appeal as Claude Lévi-Strauss, its proprietor-founder, moved on from kinship studies to distributional analyses of symbolic forms—myths, rituals, categorical systems—and promised us a general account of the foundations of thought (Lévi-Strauss 1963a,b, 1966, 1964–1967; Boon 1972). Others, like “sociobiology” (Chagnon & Irons 1979), “cognitive anthropology” (Tyler 1969, D’Andrade 1995), “the ethnography of speaking” (Gumperz & Hymes 1964, Tedlock 1983), or “cultural materialism,” (Harris 1979, Rappaport 1968) were stimulated, sometimes overstimulated, by advances in biology, information theory, semiotics, or ecology. There was neo-Marxism (Wolf 1982), neo-evolutionism (Service 1971, Steward 1957), neo-functionalism (Gluckman 1963, Turner 1957), and neo-Durkheimianism (Douglas 1989). Pierre Bourdieu gave us “practice

What was lacking was any means of ordering them within a broadly accepted disciplinary frame or rationale, an encompassing paradigm. The sense that the field was breaking up into smaller and smaller, incommensurable fragments, that a primordial oneness was being lost in a swarm of fads and fashions, grew, producing cries, angry, desperate, or merely puzzled, for some sort of reunification (Lewis 1998). Types or varieties of anthropology, separately conceived and organized, appeared, one on top of the next: medical anthropology, psychological anthropology, feminist anthropology, economic anthropology, symbolic anthropology, visual anthropology; the anthropology of work, of education, of law, of consciousness; ethnohistory, ethnophilosophy, ethnolinguistics, ethnomusicology. What had been, when I stumbled into it in the early 1950s, a group of a few hundred, argumentative but similarly minded ethnologists, as they tended then to call themselves, most of whom knew one another personally, became by the late 1970s a vast crowd of scholars whose sole commonality often seemed to be that they had passed through one or another doctoral program labeled anthropology (there are more than a hundred in the United States alone, and perhaps that many more around the world).

Much of this was expectable and unavoidable, a reflex of the growth of the field and the advance of technical specialization, as well as, once again, the workings of the World Spirit as it made its way toward the conclusion of things. But change nonetheless produced both an intensification of polemical combat and, in some quarters anyway, angst and malaise. Not only did there appear a series of trumped-up “wars” between imaginary combatants over artificial issues (materialists vs.
idealists, universalists vs. relativists, scientists vs. humanists, realists vs. subjectivists), but a generalized and oddly self-lacerating skepticism about the anthropological enterprise as such—about representing The Other or, worse yet, purporting to speak for him—settled in, hardened, and began to spread (Clifford 1988, Fabian 1983).

In time, as the impulses that drove the optimism of the 1950s and the turbulence of the 1960s died away into the routines and immobilities of Reagan’s America, this doubt, disillusion, and autocritique gathered itself together under the broad and indefinite, rather suddenly popular banner of postmodernism (Lyotard 1984, Harvey 1989). Defined against modernism in reproof and repudiation—“goodbye to all that”—postmodernism was, and is, more a mood and an attitude than a connected theory: a rhetorical tag applied to a deepening sense of moral and epistemological crisis, the supposed exhaustion, or, worse, corruption of the received modes of judgment and knowledge. Issues of ethnographic representation, authority, political positioning, and ethical justification all came in for a thorough-going-over; the anthropologist’s very “right to write” got put into question. “Why have ethnographic accounts recently lost so much of their authority?”—the jacket copy of James Clifford’s and George Marcus’ Writing Culture collection (1986), something of a bellwether in all of this, cried:

Why were they ever believable? Who has the right to challenge an ‘objective’ cultural description? . . . Are not all ethnographies rhetorical performances determined by the need to tell an effective story? Can the claims of ideology and desire ever be fully reconciled with the needs of theory and observation?

Most of the work in this manner (not all of it so flat-out or so excited as this, nor so densely populated with rhetorical questions) tended to center around one or the other of two concerns: either the construction of anthropological texts, that is, ethnographical writing, or the moral status of anthropological work, that is, ethnographical practice. The first led off into essentially literary matters: authorship, genre, style, narrative, metaphor, representation, discourse, fiction, figuration, persuasion (Geertz 1988, Boon 1982, Fernandez 1986, Sapir & Crocker 1977, Pratt 1992); the second, into essentially political matters: the social foundations of anthropological authority, the modes of power inscribed in its practices, its ideological assumptions, its complicity with colonialism, racism, exploitation, and exoticism, its dependency on the master narratives of Western self-understanding (Hymes 1972, Asad 1973, Marcus & Fischer 1986, Rosaldo 1989). These interlinked critiques of anthropology, the one inward-looking and brooding, the other outward-looking and recriminatory, may not have produced the “fully dialectical ethnography acting powerfully in the postmodern world system,” to quote that Writing Culture blast again, nor did they exactly go unresisted (Gellner 1992, cf. Geertz 1995b). But they did induce a certain self-awareness, and a certain candor also, into a discipline not without need of them.

However that may be, I spent these years of assertion and denial, promise and counterpromise, first at the University of Chicago, from 1960 to 1970, then at the Institute for Advanced Study in Princeton, from 1970 on, mostly trying to keep
my balance, to remember who I was, and to go on doing whatever it was I had, before everything came loose, set out to do.

At Chicago, I was once again involved in, and this time ultimately as its director, an interdisciplinary program focused on the prospects of the by now quite stalled and shredded—Biafra, Bangladesh, Southern Yemen—third world: the Committee for the Comparative Study of New Nations. This committee, which remained in being for more than a decade, was not concerned as such with policy questions nor with constructing a general theory of development, nor indeed with goal-directed team research of any sort. It consisted of a dozen or so faculty members at the university—sociologists, political scientists, economists, and anthropologists—working on or in one or another of the decolonized new states, plus a half-dozen or so postdoctoral research fellows, mostly from elsewhere, similarly engaged. Its main collective activity was a long weekly seminar at which one of the members led a discussion of his or her work, which in turn formed the basis for a smaller core group of, if not precisely collaborators, for we all worked independently, similarly minded, experienced field workers directed toward a related set of issues in what was then called, rather hopefully, considering the general state of things, nation building (Geertz 1963b). Unable, for the moment, to return to Indonesia, by then fully in the grip of pervasive rage, I organized a team of doctoral students from the anthropology department, of which I was also a member, to study a town comparable in size, complexity, and general representativeness to Modjokuto, but at the far other, Maghrebian, end of the Islamic world: Morocco (Geertz et al. 1979).

The Chicago department of anthropology, presided over at that time by an unusually open and supportive group of elders (Fred Eggan, Sol Tax, Norman MacQuown, and Robert Braidwood; Robert Redfield having only just died), provided an unusually congenial setting for this sort of free-style, thousand-flowers approach to things anthropological. Lloyd Fallers, Victor Turner, David Schneider, McKim Marriott, Robert Adams, Manning Nash, Melford Spiro, Robert LeVine, Nur Yalman, Julian Pitt-Rivers, Paul Friedrich, and Milton Singer were all there crying up, as I was also, one or another line of cultural analysis, and the interaction among us was intense, productive, and surprisingly, given the range of temperaments involved, generally amicable (Stocking n.d.). But when, in the late 1960s, the Director of the Institute for Advanced Study in Princeton, the economist Carl Kaysen, invited me to come there and start up a new school in the Social Sciences to complement the schools in Mathematics, Natural Science, and Historical Studies in existence since Einstein, Weyl, von Neumann, Panofsky, and other worthies had put the place in motion in the late 1930s and early 1940s, I, after a couple of years backing and filling, accepted. However exposed and full of hazard it might be, especially in a time of such division within the academy and the dubiousness of the very idea of “the social sciences” in the eyes of many humanists and “real scientists,” the prospect of being given a blank and unmarked page upon which to write was, for someone by now addicted to good fortune, simply too attractive to resist.
CONCLUSION

It is always very difficult to determine just when it was that “now” began. Virginia Woolf thought it was “on or about December 1, 1910,” for W.H. Auden it was “September 1, 1939,” for many of us who worried our way through the balance of terror, it was 1989 and the Fall of the Wall. And now, having survived all that, there is September 11, 2001.

My years, thirty-one and counting, at the Institute for Advanced Study have proved, after some initial difficulties with the resident mandarins, soon disposed of (the difficulties, not the mandarins), to be an excellent vantage from which to watch the present come into being in the social sciences (Geertz 2001). Setting up a new enterprise in the field from a standing start—the whole field from economics, politics, philosophy, and law, to sociology, psychology, history, and anthropology, with a few scholars from literature, art, and religion thrown in for leavening—demanded much closer attention to what was going on in these areas, not only in the United States but abroad as well. And with more than five hundred scholars from more than thirty countries spending a year as visiting fellows at one time or another (nearly a fifth of them anthropologists of various kinds, origins, ages, and degrees of celebrity), one had the extraordinary experience of seeing “now” arrive, live and in color.

All that is well and good, but as the present immediate is, in the nature of the case, entirely in motion, confused and unsettled, it does not yield so readily to sorting out as does, at least apparently, the perfected, distanced past. It is easier to recognize the new as new than to say exactly what it is that is new about it, and to try to discern which way it is in general moving is but to be reminded again of Hegel’s Dictum: the future can be an object of hope or of anxiety, of expectation or of misgiving, but it cannot be an object of knowledge. I confine myself, then, in finishing up this picaresque tale of questing adventure, to just a few brief and evasive remarks about how things anthropological seem to have been going in the last decade or so.

At the world-history level I have been invoking throughout as active background, the major developments are, of course, the end of the cold war, the dissolution of the bipolar international system, and the emergence of a system, if it can be called a system, which comes more and more each day to look like a strangely paradoxical combination of global interdependence (capital flow, multinationals, trade zones, the Net) and ethnic, religious and other intensely parochial provincialisms (The Balkans, Sri Lanka, Ruanda-Burundi, Chechnya, Northern Ireland, the Basque country). Whether this “Jihad vs. McWorld” (Barber 1995), is genuinely a paradox, or, as I tend to think, a single, deeply interconnected phenomenon, it has clearly begun to affect the anthropological agenda in ways that September 11 can only accelerate.

Studies of ethnic discord (Daniel 1996), of transnational identities (Appadurai 1996), of collective violence (Das 2000), of migration (Foner 2000), refugees (Malkki 1995), and intrusive minorities (Kelly 1991), of nationalism (Gellner 1983), of separatism (Tambiah 1986), of citizenship, civic and cultural (Rosaldo
1997), and of the operation of supra-national quasi-governmental institutions [e.g., the World Bank, the International Monetary Fund, UN bodies, etc. (Klitgaard 1990)]—studies which were not thought to be part of anthropology’s purview even a few short years ago—are now appearing on all sides. There are works, and very good ones, on the advertising business in Sri Lanka (Kemper 2001), on television in India (Rajagopal 2001), on legal conceptions in Islam (Rosen 1989, 2000), on the world trade in sushi (Bestor 2000), on the political implications of witchcraft beliefs in the new South Africa (Ashforth 2000). Insofar as I myself have been directly involved in all this, it has been in connection with the paradox, real or otherwise, of the simultaneous increase in cosmopolitanism and parochialism I just mentioned; with what I called in some lectures I gave in Vienna a few years ago (and hope soon to expand) “The World in Pieces” calling for an anthropological rethinking of our master political conceptions, nation, state, country, society, people (Geertz 2000).

Things are thus not, or at least in my view they are not, coming progressively together as the discipline moves raggedly on. And this, too, reflects the direction, if it can be called a direction, in which the wider world is moving: toward fragmentation, dispersion, pluralism, disassembly, -multi, multi-, multi-. Anthropologists are going to have to work under conditions even less orderly, shapely, and predictable, and even less susceptible of moral and ideological reduction and political quick fixes, than those I have worked under, which I hope I have shown were irregular enough. A born fox (there is a gene for it, along with restlessness, elusiveness, and a passionate dislike of hedgehogs), this seems to me the natural habitat of the cultural . . . social . . . symbolic . . . interpretive anthropologist. Interesting times, an inconstant profession: I envy those about to inherit them.


AN INCONSTANT PROFESSION

Parsons T. 1937. *The Structure of Social Action: a Study in Social Theory with Special Reference to a Group of Recent European Writers*. New York: McGraw-Hill


AN INCONSTANT PROFESSION

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