Possessed by Possession: Or How One Thing Led to Another
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In January of 1993, on the occasion of his 80th birthday, Richard Nixon explained his longevity as due to the fact that he never looked back. Since I do intend to look back a bit in this talk, it seems then that I may be risking my longevity. I was going to say I might be taking my life into my hands. But then, I suppose, that may be true in more senses than one!

I have long been interested in possession, as the title indicates. That topic will indeed provide much of the substance of what I have to say. But to start off, I want to go back to an earlier time, before I knew about possession or had much formal interest in religion, to a time when I was struck by the force of cultural differences, which, in turn, may well have tempted me into anthropology.

I came to New York as a teen-age refugee from Nazi Germany -- I say Germany because that is what Austria was then. I went back to Vienna last year -- for the first time in 54 years. One of the first things I saw was a placard announcing: "Der Besuch der Alten Dame," (The Visit of the Old Lady). I half wondered whether that was meant for me. But it turned out to be the announcement at the Burghtheater -- the city's major classical theater -- of Dürrmatt's play The Visit. (You may remember the film version with Ingrid Bergmann and Anthony Quinn.)

But back to my story. In New York, I went to high school. As it turned out, it was the only public girls' high school in the city.1 All the others were "co-educational," in the language of the period. There were 8,000 girls. Classes were 50 minutes long, and at class change times you could be trampled to death if you took the up stairway to go downstairs or vice versa. My Viennese Gymnasium, by contrast, was co-ed -- the only one in the city -- and had about 500 students in all, spread over eight levels. More striking perhaps than the size and organization of the school was the fact that at first, and for a while, all the girls looked alike to me. They had wavy hair, lots of make-up, painted finger nails; they wore plaid skirts, and sweaters, and pearls, or dickey's, bobby socks and saddle shoes. And they all had the same handwriting, too. I should add that I don't recall any non-whites, or what would nowadays be called "minorities." In fact, many, perhaps even most, came from a background not unlike my own -- from

more or less the same part of the world, except that their families had arrived in this country a generation or two earlier. Clearly, my inability to tell them apart, their sameness and strangeness, could not be accounted for in "racial" terms, again, in the language of the day. And it wasn't only the clothes and the make-up: it was body language, facial expressions, manners of moving, behaving, speaking and so forth. In short, they were products of a different, unfamiliar, cultural world.

The school tried to "assimilate" those of us who were recent arrivals, and there was a fair number in the fall of 1939. I particularly remember a course on manners, though I don't recall the title. We were taught how to set a table, how to introduce people, and, especially, how to have something called "poise." The key element here was not to speak too loudly in public places and, above all, not to gesture, what was called to "talk with your hands." That, it seemed, marked you immediately as an "immigrant," a negative category. I thought back to this class later, when I encountered David Efron's (1941) study of gestures among Jewish and Italian immigrants and their American-born children. And also, when I went to college and discovered, in speech classes, that it was necessary to punctuate one's presentations with appropriate gestures, when many of the presenters -- presumably products of high school classes in poise -- were standing ramrod stiff and straight.

In the Fall of 1941 I went to college and met Boas before I met anthropology. "Met" is too strong a word ... I heard him speak, at a German Social Democrat meeting, about the war. He spoke in German, as did all the others, except the representative of the American Civil Liberties Union, who was, I believe, a famous WASP. Boas was old and frail, and he died not long after this event. But I don't remember what he said.

I did meet anthropology shortly thereafter, in the persons of Hortense Powdermaker and Joseph Bram. I was attending Queens College, the youngest of the colleges of the City of New York. Powdermaker had been a student of Malinowski's and later, as a post-doctoral researcher at Yale, a protégé of Sapir's. After her first fieldwork in Melanesia, she had gone to do research among Blacks in the South -- in Indiana, Mississippi, to be specific. She was one of the very first American anthropologists to study American culture.2 She is best known among today's students, I suppose, for her professional autobiography, Stranger and Friend (1966). Bram had been a student of Redfield's -- I recall he introduced us to Redfield's (1941) Folkcuture of Yucatan in his American Indian course. His own fieldwork among Puerto Ricans in New York and on the island came later.

1 B. Kaplan tells me there was another one.

2 See Chernoff, ed. (1991) for memories of Powdermaker by colleagues and students, as well as reflections on her research.
Powdermaker taught courses in, among other things, "race relations" and "culture and personality," and sent us off to listen to talks by people such as C. Lévi-Strauss, E. Fromm and Karen Horney, all of whom were working in New York in those years.

A number of things stand out in my mind as I recall this undergraduate experience of the war years, from the perspective of one who has taught anthropology for so many years. It was a combined department of sociology and anthropology in a small liberal arts college. There were about 2,000 students. Today Queens is part of the City University of New York and has some 35,000 students. Access to the college was by residency in New York City and was based on grades in high school and on the State Regents' examinations. Tuition was free. Instruction was limited to social and cultural anthropology. "Race" and "language" appeared in the introductory course, but there was nothing on human paleontology or prehistory. The other striking feature is that there were virtually no textbooks for any of the courses. Bram had us read parts of Boas' (1938) General Anthropology and Powdermaker, as I recall, parts of Linton's (1936) Study of Man. Readings were generally on a high level, compared to the simplified language and spoon-feeding of so many of today's undergraduate textbooks. I remember being assigned C.G. Seligmann's (1910) The Southern Massim for an introductory course report. A very dry and dull book, it seemed to me then. But I must confess, I never went back to check that impression.

What I did take from all this was a sense of anthropology as a tool for coming to grips with cultural differences and of cultural criticism. It added a very special dimension to my learning about American culture. By the time I was a junior, I was hooked, and pretty sure I wanted to do graduate work in Anthropology. To graduate early, I took some credits at the New School for Social Research: a course entitled "Moot Problems in Anthropology," taught by C. Lévi-Strauss. As we now know, he was then working on his massive kinship book. He was excited by his discovery of American anthropology, in particular the riches presented by the vast documentation of American Indian cultures to be found in the Bulletins of the Bureau of American Ethnology — and the fact that he could pick up some of these at secondhand book stores on Fourth Avenue. And he was stimulated by then current developments, such as the work on Culture and Personality, particularly the recent publications by A. Kardiner, C. DuBois and others in that movement. Much of his later thinking was influenced by his contacts at the time with his friend Roman Jakobson and also with the communications theorist, Claude Shannon, his neighbor. How he saw New York in those years is reflected in some of the reminiscences he has published (Lévi-Strauss 1983).

By the Fall of 1945 the War was over and I was beginning graduate work in anthropology at Northwestern. The department was very small, but it had two major figures: M.J. Herskovits, its founder and chair, and A.I. Hallowell, who had come from the University of Pennsylvania. From Hallowell, I learned about psychological anthropology and about American Indians; from Herskovits, about Afro-America and Africa. There was only a handful of graduate students, but it was a very international group. Classes were small and instruction was intensive. In the summer of 1946, Professor and Mrs. Hallowell took a group of us to the Lac de Flamanbeau Chippewa Reservation. The group included Melford Spiro, and, from the University of Chicago, Bill Caudill. We had been trained in the use of the Rorschach and were collecting a sample of records for comparison with those Hallowell, who had pioneered the tests' cross-cultural use, had collected among Canadian Ojibwa. He later published his analysis of the data (Hallowell 1955). The records themselves, including a life history I collected, were later published on microfiche in a collection of Primary Records in Culture and Personality (Kaplan, ed. 1956). In the meantime, Professor Herskovits had made plans for some of us to do research in Afro-American communities. So the following year Ruy Coelho, a Brazilian, went to study the Black Caribs of Honduras, for a comparison to Taylor's (1951) work in Belize (then British Honduras) and I went to Haiti. Mel Spio went to Ifaluk as part of a program of Micronesian anthropology coordinated by G.P. Murdock. Micronesia has just recently been taken over by the Navy. My research in Haiti was to continue, in some sense, the work Professor and Mrs. Herskovits had conducted there in 1934 (Herskovits 1937). I wanted to use my culture and personality training as well as Herskovits' approach to Afro-American studies. And so I discovered spirit possession. Specifically, I thought I wanted to utilize the Rorschach test, in conjunction with other types of data, to approach the basic question: what makes it possible for people to engage in that type of behavior?

Two things should be noted here: First, Herskovits was then vigorously opposed to problem-oriented, hypothesis-focused research. Indeed, he heavily criticized Robert Redfield and his colleagues at the University of Chicago for doing just that. He argued that fieldwork had to be holistic, all-encompassing. Focusing on narrow problems and the testing of hypotheses would risk blinding the fieldworker to significant materials. Secondly, in the framework of cultural relativism, he argued strenuously that behavior that was socially valued and ritually sanctioned, such as possession trance, was, by definition, not pathological. This went against the position of the published work of a Haitian physician (Dorsainvil 1931) and that of
several Brazilian psychiatrists who had taken an interest in Afro-Brazilian rituals. Yet he also noted that little was known about the dynamics of possession trance.

My fieldwork in Haiti led to my enduring interest in possession, in psychological anthropology, and in the Caribbean, and I shall have more to say about that. In Haiti, I also met my husband, who had come there from Europe. His later work as an artist was significantly affected by his experience of Haiti.

Much has been said about the personal equation in fieldwork, a very popular subject these days. It was, however, a subject I was not really prepared for. Given the fact that my adviser and his wife had worked in Haiti, a comparison seems appropriate. He was an American university professor, a high status position. They were there as a couple. They had been to Africa and could speak with personal authority on the subject. They were experienced fieldworkers. I was a young woman, single, a student, and, in practical terms, relatively inexperienced. As far as the literate portion of the society was concerned -- those that I met -- I was a European. I got to hear a good deal of criticism of America and Americans, and I was expected not to be as much of a racist as Americans were thought to be. Given all this, together with the 13 year interval between our studies and the differences in field sites, my work could in no way be seen as a restudy of theirs.

Haiti impressed us by its dreadful poverty. There were virtually no negotiable roads outside the immediate area of the capital. Mail service was poor, even in the city; international telephone contacts were shut off at night. There was no electricity in the slums -- that is, most of the city, or in the rural areas. Diseases were rampant: they included tuberculosis, syphilis, yaws, leprosy, malaria. Infant mortality was high. There was a single psychiatrist for the entire country. There were few outsiders: the small number of war-time refugees had, for the most part, left, and the tourists had not yet arrived -- except for a few special cases, Truman Capote among them. To a significant degree, the country was isolated. When I came back to the U.S., people thought I had been in Tahiti. And yet, compared to the present conditions of that poor country, however great the misery of those years, as we look back, we see those times as a relatively benign period. Today, of course, Haiti is much less remote: there are Haitians in American cities, and Haiti is in the news. Vodoun, like Cuban santería, is practiced in many places here.

When I returned to Northwestern in the Fall of 1948, Hallowell had gone back to Philadelphia, and my Rorschach materials were no longer viable as the core data of my dissertation. The universities were changing rapidly under the impact of the GI Bill of Rights. Herskovits recommended me for a temporary position at Ohio State, and by January 1 I was in Columbus, for a six month stay -- to replace John Bennett (Beloit, 1937) who was going to Japan. I am still in Columbus, retired in 1990, after more than 41 years. The moral? Beware of temporary positions!

I then only knew two things about Ohio State: at the 1948 Homecoming Game in Evanston, Northwestern had beaten Ohio State and had gone on to the Rose Bowl. Classes had been canceled in celebration. The other was that Richard Morgan, the archaeologist at the Ohio State Archaeological and Historical Society, had recently been fired. The reason: his wife was accused of being a "fellow traveler" -- in the language of the times, pro-Communist. Though Morgan tried to rally support from anthropologists, he never worked as an archaeologist again, and Ohio archaeology was the poorer for it, and for a long time. That, however, is not the whole story. The Morgan case was not an isolated event and must be seen as indicative of the state of academic freedom at that time, and for a long period thereafter. I was reminded of this recently when I read in a paper by the Cultural Studies Group at the University of Chicago (1992) that American faculties were protected by academic freedom.³ Here is another illustration of my point: In doing a bit of clearing out of my files recently, I ran across a letter, from the chairman of the department of Sociology and Anthropology, to the President of the University, forwarding a petition from the faculty, expressing deep concern over two communications being widely circulated, one by a faculty member, the other anonymously. The latter which styled itself a "Report on Subversion at the Ohio State University" characterized 600 faculty members -- about a third of the faculty -- as "agitators parading as teachers" and as "disloyal to the American spirit and example of freedom." The date of this letter? You may well ask: the year was 1965! Long after the demise of the House Un-American Activities Committee, and the end of Senator Joseph McCarthy. And before the intense conflict over the Vietnam War erupted. The author of this document had encouraged students to sit in classes to spy on faculty members and report back to him.

But back to 1949. Ohio State was then bursting at the seams, trying to cope with the massive influx of new students. The University had an enrollment of 22,000 -- something never heard of before. (It now has 61,000 students ...). Classes were held in temporary structures, Quonset

³ They write that in the West there is "the presupposition of a liberal-public discursive space ..." (p. 534).
huts with potbellied stoves, and others. Old buildings were being enlarged. A large number of the students were ex-GIs, undergraduates and graduates, recently returned from the wars. We know, retrospectively, that as a result of the GI Bill, 7.8 million veterans eventually went to school. In 1949-50 there were 853,000 -- a third of the total U.S. university enrollment (Lora 1982). A majority of the students were males. For a new teacher, I found a heavy teaching load of courses I had not taught before, with many of the students in my classes older, often much older, than myself. It was an experience I had occasion to recall later, in the 1970s, when I was chair of my department. By then so-called non-traditional students, mostly women, had begun to appear among our graduate students. A young assistant professor complained to me that he could not deal with the two GTAs assigned to him -- as women older than he, they intimidated him. It was hard to be sympathetic.

In the Spring of that year I attended the meetings of CSAS in Bloomington and read a paper. When Barry Isaacs later wrote his history of CSAS it turned out I was one of only three women to read papers on that occasion. I hadn't noticed that. Retrospectively, this scarcity of women on the program seems the more surprising, since there were senior women on our faculties. It was only when they began to retire about a decade later that a shortage of women scholars was to be seen.

Among the materials I began to teach was a new course in the curriculum, culture and personality. I taught it, with continuous modifications, every year until 1992. My text book Psychological Anthropology (1979) grew out of that experience, and reflects the course as it evolved over the years. I recall also an early seminar on that topic, in which I assigned Bettelheim's famous paper "Individual and Mass Behavior in Extreme Situations" (1943), based on his own Concentration Camp experiences. I argued that extreme situations tested the limits of human adaptability. Last fall, a student in one of my classes wrote a paper on that topic, using in addition to Bettelheim's work, Turnbull's book on the Ik (1972) and Nancy Schepers-Hughes' study of Brazilian mothers (1992). However much we may have learned, some of the problems remain constant.

In those days I taught every course in the book: Religion, Social Structure, North American Indians, North American Archaeology, Physical Anthropology, etc., and some sociology as well. And of course the introductory course which, in those days, was a broad general survey, including physical anthropology, prehistory, language and culture. Over the years I added a good many courses to our offerings. In teaching it seemed to me important, from the very beginning, to provide students with the critical tools for understanding the world in which they lived. Reflecting, as I was, on a series of personal experiences of cultural differences and culture shocks -- New York, Haiti, Columbus -- each requiring substantial reworking of my own understandings of the world, I had found my anthropological training and anthropologically informed thinking helpful. In teaching, then, the key was the concept of culture, together with a substantial exposure to ethnographic data -- the reality and significance of cultural diversity, or, as we were more likely to say in those days, cultural relativity. The American, largely Midwestern, small town way the students knew was only one of many. Coming from small towns, for the most part, to a campus of more than 20, then 30,000 students and more, that is, to a campus population larger than many of their home towns, was for many of them, a shocking and transforming experience. The contexts in which they lived, they needed to know, was, in Hallowell's terms, a specific culturally constituted behavioral environment. Other such environments existed. Anthropology, I thought, could help us stand, even if only ever so slightly, outside our own culture and to take a critical perspective on it. Critical anthropology is, after all, not an invention new to the 1980s and '90s. It surely goes back to Boas, to Sapir's "Culture, Genuine and Spurious" (which just then had been reprinted in his collected writings [Mandelbaum, ed. 1949]), to Benedict, Mead, to Herskovits and his vigorous writings on cultural relativism. Much of what is new is the current language, but not all of the now fashionable ideas.

Psychological anthropology, it seemed to me then and does now, represents a key approach to understanding culture as lived and experienced by the individual. Culture is not inborn, nor is it learned like a textbook, or to be put on or taken off like a suit of clothes. It is built into the individual from the very beginning -- inseparable from personal identity, from dimensions of self and personality. In the 1950s there was still a substantial interest in anthropology in neighboring disciplines, particularly among psychologists.

There appears to have been a general decline of interest in anthropology by mainstream psychologists from a high point in the '40s to mid-50s. This has, in part, paralleled the decline of personality and culture within anthropology. Thus, the high water mark for inter-disciplinary cooperation between psychology and anthropology centered about World War II. The discovery of psychological perspectives on other cultures and that of culture as a factor in the behavior of people represented matters of interest. Psychological warfare and the need to understand enemies and allies, as well as to mobilize the homefront appear relevant to an assessment of the period.
America's turning in on itself in the '50s, the retreat from cultural relativism -- however defined -- and claims of "methodological rigor" as defense mechanisms against the perception of undesirable knowledge came to the fore throughout the social sciences. Paradoxically, the current new relativism of the interpretive orientation in anthropology, as Spiro (1986) has pointed out, is reducing the perception of the relevance of anthropology, rather than enhancing it as relativism once did. Since the mid '50s a cross-cultural psychology has developed. However, this is not mainstream psychology and has not been integrated into psychology curricula. Some of its roots are to be found in Third Word industrial and child psychology. Moreover, testing of hypotheses, Piagetian or other, in non-Western societies, by itself and in isolation, hardly constitutes an anthropologically informed approach. As Schwartz (1992) points out, too often there is "cross-cultural" without "cultural." The present gap between the disciplines exists in teaching and research, in sense of problem and in methodology. Seeking universal laws of animal behavior, choosing experimental species -- from pigeons to monkeys to freshmen -- for research convenience, allows little room for interest in cultural diversity. The very fact that our psychological colleagues speak of "human subjects" and "running Ss," and that much of what anthropologists are interested in in the behavior of their informants is extraneous to the type of controlled study they conduct, speaks to the point.

During the Kennedy administration, the NIMH was seeking to expand social science research in mental health related subjects, and also to encourage training in this area. Taking advantage of this opportunity, Louanna Pettay, a physical anthropologist, and I applied for a grant for a cross-cultural study of dissociational states. This research was prompted on the one hand by questions arising from my work on possession in Haiti and on the other, the new and growing work in holocultural studies.

Possession in Haiti led to some simple key questions: How widespread is this? Is it just Haiti? Obviously, no -- there were other Afro-American cultures where possession trance religions flourished: Cuba, Trinidad, Brazil. And there were the West African societies, to which many of the beliefs and practices could be traced. But it wasn't just Africa and Afro-America. There was also Bali, and Southeast Asia. How different were European beliefs in spirit possession? What about American Indians? What about shamanism? And so on. Clearly, some ordering of worldwide ethnographic data was in order. As we read, it was clear that where explanations were attempted, historic, functional, psychopathological or other, they were generally ad hoc explanations, tailored to the specific case. Was it possible to arrive at a broader picture? Was there a biological dimension that could be investigated? In other words -- what was happening in the body?

We got the grant and eventually the study ran for five years. A group of students worked with us, reviewing and coding data from the literature, and, in the later phases, some conducted targeted fieldwork. I recently had occasion to review the technical and theoretical context with which we started, and here it is only necessary to say that we came in at the very ground floor of holocultural research (Bourguignon 1993). Computer technology (hardware and software) was primitive by today's standards and difficult to access. Even duplicating was limited and expensive. There was no standard Ethnographic Sample -- and indeed there was debate about the issue of sampling. HRAF did not code for the data we were interested in and so we went to the original ethnographic literature, in several languages. Eventually, we reviewed more than twenty-two hundred sources on a grand total (a very grand total) of 1283 societies. Our final statistical studies, however, were limited to a sample of 488 societies for which data on a variety of societal variables were available in Murdock's Ethnographic Atlas (1967) and which avoided the major pitfalls of sampling. In the intervening years the ethnographic literature dealing with possession beliefs, trance and related phenomena has vastly expanded, as has, it appears, the actual distribution of possession and trance religions across the world. The lay public in this country and elsewhere, has increasingly become interested in these and related matters. A sizable number of publications have resulted from our work as have a number of dissertations, during the period of the study and later (Bourguignon, ed. 1973, Bourguignon and Greenbaum 1973, Bourguignon and Evasco 1977, Bourguignon 1978, Bourguignon 1991 [1976], Goodman 1972, Goodman, Henney and Pressel 1974, Lerch 1980).

When we first approached these materials, we found some confusion in the literature. The terms "trance" and "possession" were often used interchangeably, and which one was preferred depended somewhat on the cultures discussed. "Possession" was used both for a state, or behavior, and also for certain types of beliefs. The condition itself was often spoken of as hysteria, hypnosis, epilepsy, seizures, etc., all of this in quite an ad hoc manner, without supporting psychiatric or neurological study, or expertise. To clear the ground, we proposed to limit the term "possession" to a belief, a way of interpreting various phenomena, including alterations of behavior. The behavior itself we termed "trance," specifying that technically it was, for the most part, best described as dissociation, a neutral term referring to a mechanism that might, or might not, be pathological. (In the course of the period of our research, the term "Altered States of Consciousness" first came into use, and eventually we employed it as an umbrella term). Trance
and Possession, we found quickly, did not necessarily go together, although they often did. Thus, when trance, or if you prefer, ASC, was interpreted as possession and was informed by a possession belief, we spoke of Possession Trance (PT). When it occurred in the absence of such a belief, as for example, in the Plains Indian Vision Quest, we spoke simply of Trance (T). We found, too, that possession belief (P) could be used to interpret phenomena other than trance, such as certain illnesses or the acquisition of powers to heal or to harm, ascribed to the presence of an indwelling power or spirit. Various African witchcraft beliefs represent a case in point. We then discovered in coding the data from the ethnographic literature that, while a few societies had none of these features — or none were reported — which is not quite the same thing — many had more than one. Thus, a society might have both Trance (T) and Possession Trance (PT). For example, the Azande, as reported by Evans Pritchard (1936, 1962) had Trance among male diviners and Possession Trance among female spirit mediums. No separate belief in Possession, however, was reported.

On the island of St. Vincent, in the Caribbean, Jeannette Henney (1973), a member of our research team, found among a group called Shakers, Trance (T) in the form of a visionary state during a private initiation ritual termed "Mournin" and Possession Trance (PT) in public rituals of worship. Given the possible combinations and permutations, we ended up with eight types of societies.

How widespread, according to our data, were institutionalized dissociational states? How widespread were forms of possession belief? The answer was, and remains, impressive: 90% of our sample societies were reported to have some form of institutionalized dissociational states, or, if you prefer, ASC. 74% had some type of possession belief. How could one account for such high frequencies, as well as the difference between them? It seemed reasonable to suggest that trancing is a universal human capacity, which is utilized differentially in human societies. It is shaped and evaluated in different ways, meeting different societal needs, corresponding to different systems of perception, to different culturally constituted behavioral environments. A frequent, but by no means the only way, to structure trance is to link it to beliefs in possession by spirits or other entities. Possession belief, although also very widespread, is significantly less so than trancing. Such beliefs clearly are a matter of interpreting experience; they are cultural inventions, inventions that may have taken place many times, related to other widespread ideas, such as the existence of one or more souls, separable from the body. And, more broadly, to a belief in spirit beings. Putting things into starker terms, one might say that the human capacity for trance constitutes a raw material utilized for social and personal ends and manipulated by cultural means and themes. Possession is such a major theme; spirit traveling or soul absence, is another. In, contemporary terminology, as in the work of Thomas Csordas (1990), beliefs, cultural themes, are seen as "embodied" in Trance and Possession Trance behaviors. In the light of these findings, the "special case" of Haiti hardly seems very special at all. True, we need to understand Haitian history and world view, social structure and a good deal more for an intensive case study. But Haitian vodouists are not unique — their beliefs and behaviors must be understood, above and beyond their specific context, as fitting into a much broader picture.

Regional distributions of trance types vary, and this suggests the influence of diffusion. Possession Trance is associated, at a statistically significant level, with sub-Saharan Africa, and Visionary Trance with North America (Bourguignon and Evascu 1977). Certain beliefs and ritual behaviors appear to be more "congenial" with, or "suited" to, some types of societies rather than others. This is where our correlational studies, using data from the Ethnographic Atlas (Murdock 1967), shed some light on the matter. Possession Trance societies were found to be consistently more complex than Trance societies, while those having both types generally fell in between. The significant variables include subsistence economy (agriculture versus hunting and gathering), population size, size of local group, stratification, slavery, settlement patterns, jurisdictional hierarchies, mode of marriage, family form, polygynous marriages, and segregation of adolescent boys. These findings are consistent with regional differences: Most obviously, sub-Saharan Africa is highly dependent on agriculture, Native North America on hunting and gathering. We end up with what might be called regional profiles of the major world areas (Bourguignon and Greenbaum 1973). The relationship between Trance Types and societal complexity have been confirmed in more recent studies by Winkelman (1986) and Shaarah and Strathern (1992). The coded data on our larger sample of 1283 societies are now to be published on disc by the journal World Cultures (Bourguignon and Ucko, n.d.).

There has been a great expansion of research into these topics and much new data are available. It also seems clear, as we saw in a session at last year's CSAS meetings in Cleveland organized by Sue Kenyon, that there is a great flowering of possession trance religions in many parts of the world. This strengthens a suggestion I have made elsewhere: Religious uses of altered states of consciousness reflect stress points in the societies in which they occur. Among the agricultural peoples of Africa, much of that stress occurs in the lives of women. Among hunter gatherers, as in North America, much of the stress occurs in young men, at a time when they must
face their adult tasks as hunters. In the current world situation, stresses are associated with the transformations or destructions of old ways of life; there then exists a need for coping strategies, for manipulating the universe in direct and personal terms. The New Age Religions in this country reveal this, too. I do not wish to be simplistic. I recognize the complex interactions here between needs and preparation for experience on the one hand and the presence of those ready to draw on a given cultural heritage, or more than one, to invent and discover new ways of shaping it. Power and politics, technology and money, all play their part. This is not the place to pursue these complex issues that have been treated by many before me. However, what may have appeared to some — and I was told this repeatedly — to be a recondite and even antiquarian pursuit of an esoteric subject in the early '60s, has turned out to be of considerable relevance to issues of contemporary importance. Our findings provide special insight into these phenomena. I see this clearly as I look back on my original work in Haiti, the concerns I had early on and how my own approach has changed, reflecting, I believe, changes in anthropology over that period.

As I noted elsewhere (Bourguignon 1978:487): "We no longer ask: What is Haitian modal personality like and how does it develop so that we may account for the phenomenon of ritual possession? Instead, the question is: what is the Haitian behavioral environment like so that ritual possession makes sense? How is that behavior learned and rewarded, and moreover, of what do the rewards consist?" When we look at our statistical findings, we can then apply these newer questions to the large group of societies under study. What do Possession Trance societies have in common? and: How are they different from those that lack this complex of features? It is furthermore clear that intensive local ethnography cannot be the final step in research directed toward larger theory building. Without denying the significant degree to which cultures and societies are unique, and that in any given case the interaction between researcher and researched is special, and merits intensive reflective analysis, such study is limited in what it can accomplish. We must be careful not to end up in some solipsistic self contemplation. Turning anthropology into literature may be fun, but it involves, I think, an admission of defeat in the face of the challenges offered by the task of furthering knowledge about our own species.

But to return to the task at hand. One of the things we found, and that is consistent with the observations of others, is that possession trance behavior is frequently the domain of women. On the other hand, visionary (or shamanistic) trance is more likely to be the domain of men, and is also more likely to involve techniques of trance inductions that, particularly in the Americas, include hallucinogenic substances. It may be worth noting here that in the literature that came out of the "psychedelic revolution" of the 60s, much attention was given to Native American uses of such substances and the associated trance behavior. Much attention was also given to trance achieved through meditation practices. The possession trance religions received much less attention.

In our own work, the observation of gender-linked preferences of trance types came at a time when interest in the study of women's worlds was expanding. In the early 70s I began to teach a course on the anthropology of women — one of the earliest courses focusing on women on our campus. When students returned from the field, I invited them to speak to my class about women's lives and work at their field sites. And pretty soon we were planning a book. It was eventually published as *A World of Women* (Bourguignon ed. 1980).

In 1980, a group of senior women faculty members began, on a voluntary basis, a Council on Academic Excellence for Women. We sought ways of supporting women students through special programs, workshops, mentoring, newsletters, and so forth. Soon there were calls for help from junior faculty women, and staff members. I chaired the Council for four years and it gave me a good deal of insight into the structure of our very large university. The Council is still in existence and several other organizations, such as the Women's Grass Roots Network are now active. Using an internal Affirmative Action grant we conducted a study of junior faculty life at Ohio State, which focused on women and minorities (Bourguignon et al. 1987). Some of our recommendations have been adopted, but, as elsewhere, reports pile on reports, good will is expressed, the next group is appointed, and so forth. I suppose I may call this a foray into applied anthropology, and, like others before me, I have found it frustrating and disappointing. The Council enhanced contacts among women faculty members, and that may have been one of its most important functions. It led to my co-teaching, with Francille Firebaugh, then Dean of the College of Home Economics and later Vice Provost for International Affairs and now Dean of the College of Human Ecology at Cornell University, of an interdisciplinary Seminar on Women in Development. We were able to repeat that several times, bringing together both faculty and graduate students from a broad range of disciplines. There were, however, only a handful of men who participated. This seminar led, in turn, to the formation of a Women in Development group on campus.

Another outgrowth of the work of the Council, for me, was an interest in women in science, or, more precisely, the relative lack of women in science in this country. Ann Bellisari, now of Wright State, and Semra Somersan, of Istanbul, worked with me on reviewing the literature and
forming some initial hypotheses on the subject (Bourguignon, et al., n.d.). Together with Lisa Chiteji, who has most recently worked in Belize, we proceeded to an intensive interview study of women graduate students, both in science and non-science fields, both Americans and international students, looking for characteristic themes. We have drawn some preliminary findings from these materials (Bellisari 1991) and are still optimistic about bringing the analysis of these interesting interviews to completion.

I have, of course, not given up the subjects of trance and possession, and have used materials from that research in exploring a variety of problems. Writing in honor of Professor Hallowell recently (Bourguignon 1990) I have explored the evolutionary dimension of the human psychocultural use of trance. I have also considered the relationship of possession trance and women's fantasy (Bourguignon 1991), and elsewhere the implications of a concept of possession and the experience of possession trance for theories of the self (Bourguignon, n.d.). It is interesting to look back at this work, and see how its context has changed. With regard to Haiti, the missionaries saw vodoun and possession trance as evidence of the presence of the Devil. In the work of Haitian intellectuals of the 1920's and '30s (Price-Mars 1928), it came to be seen as part of a heroic Haitian identity and evidence of an African heritage. And that is how Herskovits saw it -- an African heritage that survived, in large part, European influence, producing on the one hand a syncretic new whole, and yet also an attitude of ambivalence, a socialized ambivalence. Where, for the pioneering Haitian folklorists, ethnography had been a tool for cultural and political self assertion in the face of the American Occupation, in the hands of Duvalier it became a tool for political oppression. At roughly the same time, in the 1960s, psychiatrists discovered vodoun -- and other folk religions around the world -- and saw in it, and them, systems of psychotherapy. Jean Rouch, a little earlier, in his study of migrant workers in Ghana (then the Gold Coast) found in the possession trance religion of the Hauka a system not only of therapy but also prophylaxis (see Rouch's film Les Maîtres Fous). Tourist entrepreneurs have seen Haitian vodoun, like other folk religions, as folklore and theater. More recently, Lambeck (1989) speaks of a possession trance religion, this time in the Comorros Islands, as a text to be approached with the tools of discourse analysis. And again and again, we hear of such religions as a means of rebellion or resistance. And so it goes. The phenomena we study have a life and a development of their own. The way we study them and seek to understand them corresponds to changes in our own culture and society, as reflected and expressed in changes in the language, the categories, the theories of anthropology. When we study a society and its culture in isolation as a unique phenomenon, we cast it into one frame and can hope to answer one set and only one set of questions. When we see it as belonging to a class of cultures and societies, we ask and perhaps answer others. When we see possession trance in Haitian vodoun as a phenomenon unique to that society, we may achieve a certain depth of understanding but will be mislead by ignoring the larger field. When we see that this tradition and the behavior and experiences proper to it, represent a variant on a widespread pattern, our understanding is enlarged. The Haitians are no longer strange, bizarre, neurotic, odd, unusual, or whatever, but fit right into the center of things. 90% of human societies have institutionalized some form of Altered States of Consciousness. The phenomenon is "normal," and consequently not "deviant."

When we read earlier writers -- if we do -- we are tempted to criticize them for not speaking our language, for not casting their questions and observations in the idiom of our day. Yet our current frames are fleeting, as a view of anthropology over the last 40 some years clearly shows. And in spite of changes in language and our claims to advanced sophistication, at least some of these changes are not true paradigm shifts, but rather reflect cyclical returns. After all, Sapir told us to take note of individual diversity in the societies we study. That was in 1938, in a paper he entitled: "Why cultural anthropology needs the psychiatrist." And the returns reflect interchanges between anthropology and neighboring -- or not so neighboring -- disciplines. We have borrowed text and discourse from French literary criticism, which has borrowed from Lacan and Foucault who read Kardiner and explored the Rorschach. In Communications, Business and Education people speak of culture -- the culture of the classroom, of the firm, of doing ethnography, and so forth. We flow with intellectual trends: structuralism, post-structuralism, post modernism, feminism, post feminism and what is next?

And yet, much is the same. There is an argument afoot over the four fields, so the discipline. Forty years ago it was about centripetal and centrifugal forces in anthropology, and over inter-disciplinary research.

For me, one thing has led to another. But so it has for the community of anthropologists and for the discipline. When we are seen from the vantage point of literature, we are borrowers; from that of communications or education, or cultural studies, we are being robbed.

In a larger perspective, that is surely interesting feature of the history and sociology of knowledge and scholarship. In the present context of the American university, it is the stuff of turf battles. In ten or twenty years, all of this will look quite different to you. Scholarship goes on -- retrospectively the crises of the moment become readable in a larger context,
but what is read will depend on the moment of reading and the position of the reader, to a significant extent. And those readings may come as some surprise to participants in today’s arguments. Now we can see only vaguely what might seem significant in the context of a later period. The only thing that seems clear is that the history of anthropology is part of the larger history of intellectual trends, and these, in turn, must be seen in the context of world history: colonialism, decolonization, mass migrations, wars -- hot and cold -- and so forth. But that observation is hardly original!

Perhaps of more immediate concern to me, as I leave that field of activity, is the role of anthropologists as teachers, both of undergraduates who may take only a course or two, and of those who will be the next generation of anthropologists, people whom we seek to transform from students into colleagues. As I said earlier, I started out by thinking, and acting on the belief, that the study of culture allows us to look at ourselves as well as others. But perhaps something more than the old idea of anthropology as "mirror for Man," in Kluckhohn’s (1949) words. Rather, to gain perspective, to be critical of the social forces that impinge on us and manipulate us. To demythologize, if you will. That is, of course, of importance within the academy as well as outside it. It has been surprising to me that in their home societies anthropologists are pretty much like everybody else, under the spell of their own cultures. It is easy to speak about and to denounce economic and cultural imperialism abroad. What do we see at home? And how will you help your students to see it? That is the question for you. For the immediate future. I wish you well in this great task.

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