



Cora Dubois

SOME ANTHROPOLOGICAL HINDSIGHTS¹

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Cora Du Bois

Zemurray Professor (emerita), Peabody Museum, Harvard University,
Cambridge, Massachusetts 02138

FOREWORD

I have lived through most of the twentieth century. No one would dispute that it has been one of profound and rapid changes that were often harrowing. In these two respects it is by no means unique. To have lived so long has been a valuable experience. To have had 10 years of retirement has been an unforeseen boon. It has provided freedom to take stock of personal experiences and to appraise anthropology and history, both of which have afforded informative perspectives on the human condition.

Change, anthropology, and history have brought me to the conclusion that anthropology is not a social or behavioral science but a humanistic philosophy. With this conclusion goes a warning by Bertrand Russell: "In philosophy the first

To provide the reader with some guidelines to these final appraisals and conclusions there follows a brief summary of my experiences.

JOURNEY IN AND OUT OF ANTHROPOLOGY

My first exposure to anthropology was an introductory course at Barnard (1927) given by Boas and Benedict. I was then in my last year as a history major; the following year I wrote an MA thesis on the changes between Hellenic and Hellenistic Greece. Seven months later I decided that an-

¹A scholarly paper would require references to the many authors who have led me to the opinions expressed here, but my eyesight no longer permits such an undertaking. Also, I hope no reader will be offended by use of the male gender throughout. It is used in the generic rather than the sexual connotation.

thropology was to be my PhD. I had at that time no doubts that I wanted to be an “intellectual” and that Academe was my Grove. On Benedict’s advice I registered in 1929 for graduate training at Berkeley where Kroeber and Lowie were the dominant influences.

For 13 years (1929–42) I ran the usual course of an academic anthropologist in those days: teaching sections in anthropology at Berkeley, a PhD in 1932, a switch in interest to what was then called Personality and Culture. This was followed by a year’s fellowship from the National Academy of Sciences to explore the uses of psychiatry in anthropology. The first half of that year was spent at Harvard in Harry Murray’s psychological clinic, as an observer in the wards of the then Boston Psychiatric Hospital, and in a seminar at the Boston Psychoanalytic Society. The second half of the year was spent in New York where I began my affiliation with Kardiner as collaborator in a training seminar for psychoanalysts and psychiatric social workers. That seminar was continued for a second year while I taught at Hunter College. In 1937 I left for 2 years of fieldwork peoples relatively untouched by the Dutch colonial administration of the Netherlands East Indies. From 1939 to 1942 I taught at Sarah Lawrence College and resumed my contacts with psychiatrists and with anthropologists at both Columbia and Yale. This completed the first stage of identification with academic anthropology as it was then. I wish to underline that I was singularly fortunate in having escaped the long and discouraging stultification of the depression of the 1930s. At that time job openings were far more restricted than they are at present.

For the next 12 years I was exposed to what I have come to call “the real world” of political power, of status-ridden bureaucracies and narrow self-interests. Specifically, the Office of Strategic Services in 1942 for 3 years, the latter half of which were spent in Ceylon. With the end of World War II, I was transferred from OSS to the Department of State as head of the Southeast Asia unit in the newly established Research and Intelligence Division. The experience was instructive if not gratifying. We were unwelcome in the State Department and many of us were suspect during the anti-Communist witch-hunt of the McCarthy era.

In 1949 I took a leave of absence from the State Department with no intention of returning. For a year and a half I served as a consulting anthropologist with the World Health Organization, partly in Geneva and partly in India and Southeast Asia. There I met the same power and status hierarchies that, in other contexts, I had met in the U.S. government. Meanwhile I had been offered a tenured professorship in the Department of Anthropology at Berkeley, which I agreed to accept in the fall of 1951. On my return to the U.S. I found the academic community in Berkeley

undergoing the same witch-hunt under the local Tenney committee that Washington was experiencing under McCarthy. Quixotically, I publicly refused to accept the appointment, disregarding Kroeber's advice that such flurries come and go. After all, he had studied fashions in clothing.

The Institute of International Education asked me to establish a small research unit in the expectation of a generous grant from the Ford Foundation and in the hope of refining the programs and placement of foreign students in America as well as those of American students overseas. The anticipated support from the Ford Foundation never materialized. In that small institution the hierarchic organization was not beset, so far as I could see, by cutthroat arrivism. I did encounter, however, what a second or third level organization undergoes when it is dependent on outside grants. To put it as mildly as possible, such institutions are forced to adapt themselves to the demands of donors rather than cleaving to their own convictions or experience. The donors from government agencies and wealthy foundations were often staffed by ill-informed persons and guided by political considerations. In sum, it was my first exposure to the "managerial revolution" that now flourishes in governmental regulatory agencies and in formerly independent universities and scholarly associations.

During those 12 years I felt trapped in what some called applied anthropology and others later called "relevance." I had by then learned that I had little aptitude for, or sympathy with, power, managerial administration, or so-called applications. I put myself back in the academic marketplace. In those days (1949–1954) one still did not have to waste energy in compiling applications and in concocting often intellectually sterile "publish or perish" articles. Appointments were fostered by well-wishers through word of mouth and invitation.

After several not very attractive academic offers had been made to me, quite unexpectedly and without equivocation I was offered the Radcliffe Zemurray Professorship, a tenured post for a woman scholar to be affiliated with relevant departments—in my case the Harvard Department of Anthropology and the then-still-vigorous Department of Social Relations.

For the first time, and for 15 years (1954–69), I was exposed to a distinguished university and faculty with both undergraduate and graduate students. To speak frankly, I was unprepared for the changes that had taken place in anthropology during my 12 years of absence. During those years I had made some effort to remain abreast of what was happening in the discipline by spare-time reading, incidental lectures, and brief summer-school teaching. I had no real grasp of the contributions to knowledge in the discipline, the specialized fragmentation that had occurred, and the marked increase in the number of graduate students. Nor had I any perception of the changes that had occurred in terms of colleague relationships

since my first 15 years in anthropology. In those years we all knew each other. Although we had our theoretical differences and even personal hostilities, we stood solidly in mutual support against outside attack. In the years at Harvard I found my immediate colleagues always courteous and supportive, but at best we were an aggregate of isolates, at worst of self-seeking careerists.

I was also quite unprepared for the work load hidden under the formal assignment of teaching one undergraduate course three times a week and a graduate seminar once a week. I discovered that some colleagues devoted their talents to admirable and popular undergraduate courses assisted by a series of teaching fellows, rather than to personal relationship with promising students. At the other extreme were colleagues so absorbed in their own specialized research that they carried only a few promising students as eventual successors to their preoccupations. I found myself caught between these extremes. As the only tenured woman in the two departments, as a generalist, as a noncaptivating lecturer, I fumbled for the first years.

Teaching, however, seemed to me an important and engrossing task. As I got some solid ground under my feet, I recognized that my colleagues were able to provide graduate students with research opportunities for their PhDs by procuring outside funds for more or less ambitious projects. I selected that course. In 1960 I received a modest 2-year National Science Foundation grant for an interdisciplinary project in one of India's "double towns." In this case it was a new state capital of administration and an old conservative temple center. This was my first exposure to what applications for grants entailed in time and managerial costs, both in the U.S. and in India. The second and more interesting aspect of the enterprise was an attempt to tackle, as an ethnographer, even a single community in a culture far more sophisticated than mine.

The result was that I was forced to question the validity of the ethnographic approach to the analysis of complex cultures. This was the last gradient in my successive exposures from salvage ethnography in California, to problem-oriented research in a truly "primitive" group in Alor, to ethnography in India. It was a discovery that should have been obvious much earlier. It revealed clearly in practice what theory had already suggested: that holism was an unrealizable fantasy; that we did not understand even our own culture in its entirety. It reinforced my conviction during the 12 years in "real life" that the area studies which burgeoned after the intimate contact with alien cultures in World War II were important.

The final stage of my search for an understanding of the human condition has taken place since my retirement (1969–1979). Freed from the institutional pressures to teach and to cope with the intricacies of bureaucratic demands on government sponsored research projects, I have indulged my

own curiosities. I no longer feel obliged to keep up with the often trivial or repetitive plethora of “publish or perish” literature. Those who have been students are more or less under full career stress; they are colleagues and friends as well as sources of stimulation.

Out of this personal journey in and out of anthropology has come the following series of opinions, observations, and queries.

ANTHROPOLOGY IN CONTEXT: THEN AND NOW

From 1920 to 1970, anthropology, both institutionally and intellectually, has experienced far-reaching changes. It has been marked institutionally by proliferation and intellectually by specializations that range from secondary school education to the PhD level. It is anyone’s guess how many of these changes are to be attributed to population growth in the United States; to the “democratization” of education; to assertive ethnicity in contrast to earlier assimilation; and/or to the change from isolationism to the nation’s present role as a world power based on wealth, technology, and consumerism. One might also include the drawing power for the young and their teachers to the “romance” of American anthropology, which seemed to promise an understanding of the growing variations and uncertainties of the modernizing world. Fieldwork alone provided many rationalizations: the opportunity for travel and adventure, the demand for adult autonomy, a path to professional achievement. In a more cynical vein, such advantages were handsomely subsidized.

In sum, anthropology had great drawing power. The psychological, social, and symbolic nexus of this “syndrome” deserves analysis by a sensitive and wide-ranging historian. What follows in these notes represents no more than a brief resumé of one person’s view of changes in the discipline. I am convinced, in particular, that there is a point in quantitative increases at which qualitative shifts set in.

In the 1920s and early 1930s there were only five or six academic departments, among which Columbia, Chicago, and Berkeley were salient. Museums played a greater professional role than they do today. The American Museum of Natural History in New York, the Bureau of American Ethnology and the National Museum in Washington, and the Field Museum in Chicago, among others, offered research employment to distinguished scholars of the day. Publication was in large part financed institutionally in monographic series; few journals existed, among which the *American Anthropologist* was prominent. Quick publication, the “publish or perish” pressures of the contemporary scene, were not yet in force. Nor was the exploitation by publishers of paperbacks yet in vogue, geared to their present market of libraries, high schools, and introductory courses at the

undergraduate level. When I first labeled myself an anthropologist to lay people who did not look blank and change the subject, the responses were frequently, "Oh, you measure heads!" or "I'd like to show you my arrow-head collection!" or "What do you think about the lost continents of Atlantis and Mu?"

As I indicated earlier, the "cohort" of anthropologists was small. I have no figures at hand, but even as early PhD candidates we all knew the names, something of the personalities, and often the individuals themselves. Juniors usually were cordially received by their elders. The mood was fraternal, which is not to say always amicable. The annual meetings of the American Anthropological Association were small (often not more than 300). Annual meeting places were selected on the basis of three varied criteria: a secretary willing to undertake the practical aspects; a small college or university where a nascent department needed "moral support," and/or the availability of inexpensive lodging. Subsidies for attendance and "placement" had not yet occurred to anyone. Quantity had not yet altered quality. The "managerial revolution" was not yet necessary. No one envisioned a set rotation of convention cities and their hotels with reservations years in advance.

Although anthropology considered itself a unified discipline, certain specializations were already apparent. These were physical anthropology, archaeology, and linguistics. Cultural anthropology was pretty much a catch-all for all that was left over. In Berkeley, at least, graduate students taking their five-day written qualifying exams were expected to have at least a bowing acquaintance with the salient findings in each field, which they were expected to acquire more or less on their own.

By the late 1930s, however, and particularly after World War II, specialization became all too apparent. Goals shifted, and as they did terminology became more obscure and pretentious; technologies, often borrowed from other disciplines, were more engrossing; specialized associations and their publications, as well as invitational conferences supported by outside grants, could meet the growing demands of both new knowledge and the increased numbers of academic specializations; turning out a spate of PhD's could meet the needs of expansion. If anthropology was to retain any resemblance to a unified discipline, the skills of managerial specialists rather than of trained anthropologists seemed, some 10 or 12 years ago, an inevitable solution. The result has been a miniature version of the American Academy for the Advancement of Science, with Washington headquarters where ties with government policies and subsidies are effectively advanced. For better or worse, the Association has now become, as a unified discipline, one of those third-gradient associations that are based more on expedencies

than on a community of scholars. The choice now lies in whether as individuals we prefer the popular clichés “Bigger is Better” or “Small is Beautiful.”

This superficial appraisal of anthropology neglects, however, very real contributions it has made and continues to make substantively to knowledge, to hard sciences, and to humanistic philosophy. The contributions may seem tangential to “harder” sciences and professions, but in my view they are by no means inconsequential. For example, and most cursorily stated:

Physical anthropology, originally concerned with racial categorizations, is now allied to very significant advances in human biology. It emphasizes the errors or the overgeneralizations of biological evolution by insisting on fine distinctions between *Homo sapiens* and his congeners. This is not a new idea. A basic proposition of “the survival of the fittest” was recognized critically more than half a century ago when some scholars of biology insisted on “species specific” distinctions in contrast to the overarching theory of biological evolution with *Homo sapiens* as its culmination. Put simplistically, if “survival of the fittest” is at issue, then coral reefs have greater survival potential than reptiles, gregarious mammals, and possibly *Homo*. It is adaptation rather than generalized biological evolution that needs poised consideration.

In the past half century anthropological archaeology has evinced two major expansions of subject matter. The most striking to this observer has been in the variety of technologies for dating. Outside of archaeological findings per se, they range from chemically based technologies to ecological research. However, these techniques require of archaeologists new competences in their use—competences the general anthropologist usually does not command.

Furthermore, anthropological archaeologists have increasingly extended their interests in fields of high cultures which formerly were the provinces of scholars with area-specific specializations including decipherment of scripts and language specializations. I have the impression, however, that the major emphasis in American archaeology still lies in the Western hemisphere. It still ranges from salvage archaeology to the refinements required to untangle the history of the high cultures of what was once called the New World.

The ties between ethnography and archaeology remain relevant and important. This conjunction gave rise in the 1930s to another specialization called ethnohistory, in which stress was placed on reading changes from the present backward in time rather than forward—a counterbalance to the traditional evolutionary habit of reading from the past forward. In addition, as in other branches of anthropology, there has been a steady flow of self-

appraisal variously, and often somewhat pretentiously, labeled theory, methodology, or techniques.

Possibly the greatest divergence from the early work in anthropology is in linguistics. In this field my knowledge is even more limited than it is in biological anthropology and archaeology. Nevertheless it is clear even to one as ill-informed as I am that linguistic anthropology has established ties with philosophy, symbolism, human biology, and psychological learning theory. I have been persuaded that it is one of the most important and best-developed leads into an overall understanding of the human condition.

The cultural anthropology of my youth was a residual category. The word "culture" was used so loosely then, as it still frequently is, that it became meaningless. Meanwhile, the growth of specialized disciplines and theories in adjacent disciplines such as economics, political science, and psychology increasingly affected cultural anthropology, which became in time a collection of specialized topics whose only claim to the designation "anthropology" lay in its introduction of cross-cultural comparisons. These comparisons have two major virtues: first, they added to our more detailed knowledge of other population aggregates; second, they served to caution against overgeneralization of our culture-bound world view.

The latter cautionary role, unfortunately, had very little impact among social scientists. This was all too evident after World War II when the U.S. became a world power. Western nation states, whether capitalistic or socialistic, launched on a period best characterized as neocolonial. Under the influence of the central government's policy makers, the growing accessibility of international travel, and government subsidies, scholars in our sister disciplines were encouraged to embark on comparative studies. These were too often based on culture-bound theories, concepts, and techniques irrelevant to the areas in which they operated. Particularly misleading was the transfer of questionnaire and computer techniques. Unfortunately, many anthropologists who were captivated by the growth and successes of our Western ideologies were led into comparable culture-bound errors. For anthropology this frequently diverted attention from the question of what is pan-human and what are culture-specific generalities.

Parenthetically, it seems to me that neocolonial programs, whether in their imposition of military, economic, or altruistic goals, too often have proved ineffectual. Yet we persevere; we learn too little too late. I am far from convinced that words like "progress," "development," "evolution," etc, with their more or less hidden implications, offer reliable, culture-free appraisals of the human condition. We have projected our own experience with rapid change, forgetting that man is perseverative as well as flexible.

A PRELIMINARY DEVICE

At this point I should state certain assumptions concerning the human condition and the goals of anthropology as I conceive them. I feel sure that many of my colleagues will not agree with some of them.

First, anthropology is a philosophical humanism; it is not a pure or social science as the word "science" is now used. It is rather a science in the earlier sense of the word, as it was used in the past century: an attempt to understand.

Second, the goals of our discipline are to advance an understanding of the panhuman condition and its specific variations.

Third, this attempt to bring man's constants and variations into conjunction should rest as far as possible on empirical data.

Fourth, empiricism implies that the anthropologist, as observer, be as culture-free as possible.

Fifth, it implies also that the anthropologist, as a philosophical humanist, recognizes explicitly the muddled implications of the word "culture." For the sake of simplicity let us dismiss its implication of fashionable elitism. The more important task is to distinguish between what is distinctive of a population aggregate empirically observed or historically recorded, and what we mean by "panhuman." No one revises language; one can, however, become aware of levels of generalization. Hereafter I shall try to be consistent in using Culture (a capital C) to designate the panhuman level of generalization, and culture (with a lower-case c) to designate the configurations of any definable population aggregate.

To make these goals somewhat more specific, I shall move on to the five dimensions which, in my opinion, encompass the human condition. The somatic and technoecological dimensions provide outer limits of human potentialities. Within these two lie three other dimensions that can be facetiously dubbed the "Bermuda Triangle." They are the psychological, the social, and the symbolic. I have the impression that these three dimensions yield more easily to myth and to facile, often culture-bound, theorizing than to culture-free observation and analysis.

What follows is a brief and unresolved series of statements on some of the five dimensions proposed.

The somatic dimension refers to the biological capacities and limits of *Homo sapiens*, from his genetic limits to his bifurcate brain and ordering mind. It does not cast aside the biological or archaeological evidence of comparative biologists or the early "prehistorians." It simply cautions against overhasty comparisons either from or to adjacent hominids or even gregarious mammals. In the process we tend to find limits, too, in evolutionary theory.

Laying aside self-glorification, “the survival of the fittest” has for *Homo* two pitfalls. One is time. *Homo sapiens* is sufficiently recent in geological terms to make it difficult for us to judge his capacity to survive in comparison to other life forms from coral reefs to insects, reptiles, birds, or even mammals. Another consideration is a processual one. Where genetic capacities are dominant, extinction operates. Where flexibility is dominant, survival through adaptation to ecological changes or to evasion of predators seems to be at issue.

In sum, where *Homo sapiens* stands in the nice balance of nature remains an unsolved question. As one concerned with the human condition, I see the question of the panhuman condition unresolved for the present from the standpoint of the somatic dimension.

In the technoeological dimension, as in the somatic, anthropologists are brought into a dependent relationship with other disciplines and forces such as geology, climatology, botany, zoology, historic and descriptive geography, soil analysis, etc. The anthropologist must consider the findings of these disciplines in order to understand man’s various technological efficacies and limitations to adaptation. The denuding of forest lands under the pressure of agriculture is a well-known case in point. Overgrazing of grasslands is another. Today the exploitation of energy and mineral resources under the pressure of Western technological development has become all too obvious.

The knowledge that has accumulated in these two bordering dimensions impresses me as far more solid and testable than that of the three central human dimensions of what I have labeled the Bermuda Triangle—the psychological, the social, and the symbolic.

This sketch of the five dimensions needs at least brief elaboration. I assume that each dimension is itself composed of a series of systems. Unfortunately, there is as yet no common and indisputable agreement on what these systems are—particularly in the Triangle. I have only a few firm convictions on this score. The first is that they are not co-terminous with academic disciplines. The second is that there are intricate networks between systems within a dimension, as well as important networks of interaction across dimensional boundaries. This assumption of networks has the virtue of introducing process into categorization. It is my conviction that sound, empirical categorization is basic to sound processual formulations.

QUESTIONS AND STRATEGIES

If anthropology in the future is to arrive at a philosophical humanism, it will have to address itself to the task of querying some of the methods and techniques of present-day “social scientism.” This will mean some hard

thinking about comparisons, the use of questionnaires, statistical techniques, genuine and spurious generalizations. Above all it will raise the question of what is the nature of explanation.

Is reductionism, which has been so revealing in molecular biology and genetics, appropriate to philosophical humanism? How far should we expend some of our best talents in studies to which our training and goals are only peripheral? Might it not be wise to concentrate our efforts on *Homo sapiens*, leaving the boundary dimensions of the somatic and technoeological at least in part to specialists who are engaged in those fields, and reserving for ourselves only the findings that are specifically relevant to *Homo sapiens*, and, of course, also reserving the privilege of criticizing those specialists when they enthusiastically overextend their speculations into what is species specific to *Homo*.

This does not imply that we should ignore seminal studies in other fields, but rather that we should select what is relevant to our purposes without becoming embroiled in their problems and procedures. When we turn to the findings of our neighboring disciplines it should always be with a keen sense of "goodness of fit." To do this astutely we should first have an organized view of our own task. This I do not perceive to be the case after a century of diligent effort and a considerable archive of cross-cultural reporting. Even our considerable knowledge in what I have facetiously called the Bermuda Triangle (psychological, social, and symbolic dimensions) is in considerable disarray, if understanding what is uniquely specific to *Homo sapiens* is at issue.

This situation has turned me late in life to a consideration of the symbolic dimension and particularly to language.

Less as a confession than as a cautionary guideline, may I inject a brief statement of the sources of this blind spot? First, I have no aptitude for languages. Second, Berkeley in my day stressed that the major role of ethnography was translation. Third, we were taught only two elementary aspects of linguistics: the distribution of linguistic stocks as they were then perceived, and a rudimentary phonetic transcript for the recording of texts.

A first reconsideration of this position came late and slowly. On separate occasions two respected philosophers implied or stated flatly to me that translation from one language to another was, if not impossible, at least grossly misleading. Further doubts were raised as I read more attentively the publications of fellow academicians. Too often these scholars used neologisms not only for insights new to them but for well-worn ideas perfectly comprehensible in plain English. The proliferation of acronyms puts to shame the ethnographer's use of native terms. To all this have been added the rapid changes in slang on the one hand and, on the other, the decline in language teaching in the schools. Clearly English, at least, is

changing as fast as the culture to which it is indigenous. If this be so, it may be legitimate to assume that the same flexibility may be found in other contemporary cultures undergoing rapid changes—with attendant difficulties for non-culture-bound categorization and translation.

All this has not led me to abandon the role of the anthropologist as translator, but it has emphasized the complexity of that role. It has drawn my attention to the seminal importance of man's unique capacity to symbolize, and to the importance of language as a primary and early imprinter of the young in culture-specific variations as well as in species-specific adaptations.

We are faced with the paradox of man's culture-specific imprinting which tells him what is "real," "plain common sense," and/or "perfectly natural" on the one hand, and man's species-specific variations on the other. Why are some individuals as well as social groups savage in one respect and, simultaneously, altruistic in others? Here language and linguistics have much to contribute to a philosophical humanism.

Our immediate sister disciplines (psychological sociology, political science, and economics) have contributed useful knowledge of our own culture, but because of their culture-bound "scientism" they evince little aptitude for seeing the human condition from a wider and comparative viewpoint. It is almost as though we were reading their work as we might read that of an unusually prolific and well-informed ethnographer portraying the life-style of the Bango-Bango.

This has led to serious consequences in "the real world." I have often been dismayed by American foreign policies and the support received by them from social disciplines. The expenditures and the failures have been, to say the least, noteworthy. Even more discouraging has been the incapacity to learn from those failures.

In part, however, this may be laid at the door of anthropology. It tends to be seen either as esoterica or as instructive teaching of tolerance for young learners. We have yet to persuade most decision makers of the importance of cross-cultural understanding and of the panhuman as well as culture-specific importance of population aggregates. For example, some American educators have felt hurt and resentful when foreign students have said, "Americans are friendly but they are not friends." Or a businessman returning from Taiwan says, "They are just like us"—his exposure being limited to an area of common interest. A student returning from a year's study in India, on the other hand, will say, "The Indians are really mysterious. I will never understand them."

The resolution of such misunderstandings lies in a philosophical humanism that embraces both the panhuman aspects and the cultural imprinting of *Homo sapiens*.

ENVOI

What has so far been suggested leaves untouched many topics that were listed in my original outline and in all too many drafts. Space has required both omissions and condensation. The intent is to question rather than to be dogmatic. Put once again and crudely: my experience both in and out of anthropology, crystallized during a decade of retirement, has persuaded me that when research into the human condition deals with data based on our own culture, it may inform and persuade us—but it may also encourage cultural involution. Further, I have been persuaded that when we apply concepts and methodology cross-culturally we more often than not risk gross errors of interpretation that skew our understanding of the human condition.

It is also my opinion that the task of anthropologists is to view the human condition whole as well as to appraise culture-specific configurations in terms of panhuman liabilities and potentialities. This double task has not been realized, and may never be, solely in terms of Western “pure” science and social scientism. Accretion of knowledge, by the same token, does not constitute understanding. This is why I would prefer to see anthropology become what I may ineptly have labeled philosophical humanism.

If as a discipline we insist on taking ethnography and the history of even our own cultural configurations as science, we run the considerable risk of becoming at best a cautionary rather than an overarching discipline which was our original if somewhat naive goal. There have been and still are a few elders who have such a view of anthropology. I have met a few well-educated young anthropologists who feel that anthropology has given them a unique sense of a “world view.”

All of which reminds me of a remark by Gregory Bateson when we were colleagues in the Office of Strategic Services in Ceylon during World War II. A new staff member, a psychiatrist, had arrived one evening and was assigned to share Gregory’s quarters. When some of us eagerly queried Gregory the next morning, he said of the new colleague: “He’s wonderful! He has both ears to the ground and his feet in the air.”

I feel myself identifying with both gentlemen.