INTERVIEW WITH ALEC LEIGHTON

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Abstract: Anthropology and psychiatry have long provoked and stimulated one another, thanks to the efforts of individuals working in the overlap between them. Alexander Hamilton Leighton is one such figure, a researcher who has been not so much a bridge between disciplines as a winch, drawing the two fields closer to one another despite resistance.

Leighton is currently Professor of Psychiatry and Professor of Community Health and Epidemiology at Dalhousie University in Halifax, Nova Scotia, Canada. He is also Professor Emeritus of Social Psychiatry at Harvard University's School of Public Health. He is a man in motion, rotating regularly between Halifax, Boston (his wife, Jane Murphy, is Associate Professor of Anthropology at Harvard University), and his home in southwestern Nova Scotia.

Leighton was born on July 17, 1908, in Philadelphia, Pennsylvania. He received his B.A. from Princeton University in 1932, an M.A. from Cambridge in 1934, and his M.D. from Johns Hopkins in 1936. His research and teaching interests have ranged widely (a list of his publications follows this interview), but anthropologists will associate his name with the Stirling County study, a project conducted in Canada's Nova Scotia.

Résumé: Depuis longtemps, grace aux efforts d'individus qui travaillent dans des domaines chevauchées, l'anthropologie et la psychiatrie ont provoqué et stimulé l'un l'autre. Alexander Hamilton Leighton fait partie de ce groupe; il est un chercheur qui, plutôt que représentant un lien entre les disciplines, a habilement su rapprocher les deux domaines en dépit de quelque résistance.

A présent, M. Leighton est professeur de Psychiatrie et professeur de Santé Communautaire et d'Epidemologie à l'université Dalhousie de Halifax, en Nouvelle Ecosse au Canada. En plus, il est professeur honoraire de Psychiatrie Sociale à l'école de la Santé Publique de l'université Harvard. Il est un homme actif voyageant entre Halifax, Boston (sa femme Mme. Jane Murphy, est professeur agrégée d'Anthropologie à l'université Harvard), et sa maison au sud-ouest de la Nouvelle Ecosse.

Leighton est né le 17 juillet 1908, à Philadelphie au Pennsylvanie. Il a reçu son B.A. de l'université Princeton en 1932, un M.A. de Cambridge en 1934, et son M.D. de Johns Hopkins en 1936. Ses intérêts de recherche et d'enseignement sont multiples (une liste de ses publica-

tions est ajoutée à cet entrevue), mais les anthropologistes vont surtout associer son nom avec les études de Stirling County, un projet qui a été mené en Nouvelle Ecosse, au Canada.

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Dr. Barkow: Alec, tell me, how did your special relationship with Canada come about?

Dr. Leighton: Well, it goes back to World War I, when my parents (who lived in Philadelphia, where my father had his business at that time) became frightened by the great polio epidemic in the summer of 1916. They gathered up their children — my sister and myself — and came northeast looking for a place where there was no polio, and that landed them in Nova Scotia. Digby happened to be the first place where they found a house. They were both British in their origins and they were very much attracted to Nova Scotia. They became old-fashioned summer visitors who stayed in the same house all summer long. After the war ended we began alternating, going back and forth across the Atlantic every other year to visit family in Ireland. So it was as a summer visitor that my acquaintance with Nova Scotia began, at the age of 8.

Barkow : Nova Scotia is still getting summer visitors but I am not sure that many of them are contributing as much as you have to the Province. You have long stood for collaboration between anthropology and psychology. Can you tell us about that collaboration during World War II?

Leighton: I was in the Navy then as a psychiatrist, but I had already had some field experience under the tutelage of anthropologists. I had done work with Navajos and Inuit and had become familiar with John Collier and others in the Bureau of Indian Affairs through discussions we had about how to adjust the Indian services to the cultural needs of the people. Due to this previous history, the Department of the Interior got the Navy to lend me to them when Interior took over the administration of a Japanese-American internment camp. They wanted analyses and advice in dealing with the human cultural problems that arose. After a couple of years, I moved into intelligence work under the auspices of the Office of War Information. So I was loaned by the Department of the Interior to the Office of War Information — something like a baseball player being swapped around.

But anyway, I was fortunate in having a very good team of Japanese-Americans as assistants in the Relocation Center. They came with me to Washington to staff our intelligence analysis unit, which was sponsored jointly by the Army, the Navy and the Office of War Information but was administered in the Office of War Information. The job was to supply relevant information on Japanese morale to the people who were conducting psychological warfare and to other people planning strategies relevant to

Japan. So we set up a system and organized an inflow of data both from the Japanese military front and the Japanese home front. From then until the end of the war, we analyzed and reported periodically on what was happening to morale in the Japanese Army and also among civilians in Japan.

Barkow: Who were some of the people in your unit?

Leighton: The basic team were the Japanese Americans. There was Tom Sasaki, who went on to a career in anthropology and was Director of Graduate Studies at the University of Notre Dame when he retired. There was Toshio Yatsushio, who had a career in Southeast Asia largely as an anthropologist for the State Department and is now retired. Then there was Iwao Ishino, who became Professor of Anthropology and spent most of his life at Ohio State University. And there was Scott Matsumoto, who became an epidemiologist and ended his career as Professor of Epidemiology in the University of Hawaii's Department of Public Health. He spent much of his time studying the long-term effects of the bomb at Hiroshima.

They were an unusual group of people and at that time, when they were all in their early twenties, they were extremely alert and effective as data collectors and analysts.

The other people in the team included Clyde Kluckhohn, who was Co-Director of the Unit and there was also Ruth Benedict and Morris Opler and Dorothea Leighton. Those were the main participants.

Barkow: What kind of advice did your unit give the U.S. authorities?

Leighton: I like to think of it as good advice. What we were supposed to do was predict trends in Japanese morale and to identify the factors which seemed to be influencing it upward and downward. Two examples can perhaps illustrate how this operated. The first was the problem of getting Japanese hold-out units to surrender after the war started to go in our favour and we began to take territory. The troops were always confronting these units in Indonesia, Guam and other places. They would hold out to the last man and it was disturbing from a human point of view to have to kill everyone. It was also disturbing militarily and very expensive, because the units were difficult to take and they held up military progress.

So the question was, how do you get the Japanese to surrender? The experts, "the old Japan hands" who had lived years in Japan, and the cultural anthropologists, were inclined to say "You can't. This is the power of Japanese culture, and there is nothing you can do to change it."

The practical military mind, on the other hand, and a good many other people too, tended toward a biological orientation and to say that "the Japanese are just a different kind of animal who is made so he doesn't have fear of death the way we do. There is nothing you can do, you just have to accept it."

The kind of naturalist framework that we had developed was a mixture

of Adolph Meyer's psychobiology and Malinowski's functional anthropology. We postulated a certain range of basic human tendencies with culture affecting the way those tendencies were manifested in actual behaviour, and we doubted very much that any group of people, kamikaze tradition or not, lacked fear of death. To us this did not seem to be in keeping with what we know about evolution and the fundamental truths of animal life, at least among the mammalia anyway. We assumed that fear of death was probably more like a bell-shaped curve, there were people who are very extreme in their ability to resist and in their determination to do so and who believed in the Samurai tradition. There were other people we thought would give up at the drop of a hat if they could, but they were locked into the group of which they were members. Most were between these extremes, we supposed, and could be moved to surrender if we could find something to appeal to them.

Our intelligence data suggested to us that one element in resistance was fear of what would happen to them if they did surrender. They were scared of torture and I may say, unfortunately, not without reason, but this fear was greatly exaggerated of course by their own traditions and by the use of those traditions by their officers and leaders. We had no experimental way of demonstrating that our hypotheses were correct, but fortunately the communists in Northern China, who were of course also fighting the Japanese, had on some occasions managed to take a Japanese prisoner and send him back to talk to those holding out. This seemed to work, so we seized on it as an illustration to bolster our case. In our reports we said that the hold-out situation was being misunderstood, that there was in fact some flexibility among the Japanese and that it would be worth trying to get some Japanese prisoners and send them back to talk to their compatriots through a P.A. system.

At first there was great resistance among the authorities. They were convinced it couldn't be done. Some of the military went and asked the old Japanese hands who spoke the language and were much more impressive than we were. These people were certain it wouldn't work. "You don't understand the Japanese mind," they said. But eventually the military did try it and it worked. They got Japanese prisoners who were GIs like the guys in the fort or cave or whatever, and who talked to them in their language — I mean by that GI to GI, not officer to man or civilian to soldier or other inappropriate crossing of levels. The surrenders began first as a trickle but then became frequent. That is an awfully compacted representation of rather complex propositions and reasoning, but they had practical implications and worked out well.

Barkow: Well that was one piece of advice. What was the other example?

Leighton: Toward the end of the war we became impressed with evidence that Japan wanted out. From about the January or February prior to the

August when they did surrender, the evidence was accumulating. We drew this inference from information we got from inside Japan by way of Switzerland and Sweden. There had been false rumours before this and efforts to fool the West. But from our strategic mapping of all the data we concluded that we were now in touch with the real thing. The political nature of Japan at this time was such that when the Suzuki cabinet was formed it had a mandate to get Japan out of the war with the best bargain possible. That was about May.

Our analysis persuaded us that what was holding up the end of hostilities was the Japanese fear of what the Western powers were going to do to the Emperor. The American feeling was that the Emperor was responsible for the war and its atrocities. He wasn't just a figurehead, it was believed, but was an actual evil force in the East that we had to demolish, just as Hitler and Mussolini had been demolished in Europe. The national policy of targeting Hirohito was very firmly believed in by Roosevelt and other national leaders. It was asserted over and over that we were going to demolish Hirohito, that he would be tried as a war criminal and disposed of accordingly.

It seemed to us that all this was not only an error, but it missed an opportunity. It attributed to Hirohito political powers that in fact he didn't have and grossly misunderstood the symbolic significance that as Emperor he did have and its potential for ending hostilities. The Emperor was one of these symbols that can appeal to all people in the culture, whether you are an old traditionalist or young and not very respectful of old traditions. Here really was a cultural symbol of great power. It was something like élite British feeling about the Crown combined with the much deeper mystical feeling of a Roman Catholic for the Pope. Even that, however, is weak compared to the meaning of the Emperor of Japan. Assaulting him would be humiliation to everybody in Japan.

Of course, at that time we were out to humiliate the Japanese as much as we could, but at the same time we did want the war to end with the least possible loss of life on the part of our own people. Our intelligence analysis unit became convinced that real progress toward ending the war would occur if we dropped our intransigence about the Emperor. We thought the Japanese would agree to everything else because their morale was shot. Support for the Emperor-symbol was about all that was holding it together.

Few would listen. Eventually our boss in OWI, George Taylor, together with the overall Chief, Elmer Davis, went to see Roosevelt about our ideas. They got no place with him, but then after his death they tried again with Harry Truman and the Secretary of War, Henry Lewis Stimson. They got laughed at by both. The Acting Secretary of State, Joseph Grew, who had spent 10 years as Ambassador to Japan, was quite persuaded that Japan wanted to get out of the war and that the attack on the Emperor was a bad

idea. And the Chief of Naval Intelligence, Admiral Ellis Mark Zacharias, who had personal knowledge of Japan, also shared the views that we were advocating. These views were not based on knowledge of living in the country such as these men had. Our views were based on behavioural science concepts applied to data. The more the war went on the more we were persuaded that it was really a serious error in our policy to keep telling the world, and the Japanese in particular, that we were going to hang Hirohito or something of the sort.

Our unit didn't, as it happened, know anything about the atomic bomb that was being prepared. After Hiroshima, the Japanese began to negotiate surrender openly. Our morale analysis thinking was that they would have done it anyway, but the atomic bombs speeded up the process and gave the civilian government of Japan a stronger hand for controlling the militaristic hold-outs.

Nevertheless, when it came down to the final stages of discussing surrender, the dialogue went something like this: The U.S. said, "We won't discuss conditions of surrender — it has to be unconditional." The Japanese asked, "What are you going to do about the Emperor?" Then instead of saying "none of your business," the U.S. Government finally said that we had no intention of harming him, that it was Japan's military and political leaders whom we would hold responsible.

Now whether our team plus Grew plus Zacharias by that time had begun to have some influence, I don't know. But in the end the U.S. did come up with the right answer. Naturally, we felt somewhat vindicated in our conclusions when told that the only question asked by the Japanese Government in the last phase of discussing surrender was about the Emperor.

Barkow: Well, we know that Japan since then has gone from defeat to becoming the world's economic super-power. Was there anything you learned back then that might have permitted you to predict the current Japanese domination of so many industries?

Leighton: I wish I could say we were that foresightful but I don't think it ever entered our thinking.

Barkow: Alec, can you tell us about your own particular role in the unit?

Leighton: The focus was on morale. That involved developing a concept of morale and indicators for telling how the morale was changing up or down. The model for that was developed in the Japanese internment camp study. We were trying to follow weekly changes in morale of a population of about 10 000 people. We established indicators and were able to check out their predictive power with regard to strikes and other disturbances in the camp. When we moved to Washington, we retained the principles but reworked the model so it would be suitable for Japanese military and Japanese home front

morale. The indicators had to be redone to be in keeping with the data we could get hold of. The anthropologists added to the team contributed from their experience in interpreting the meaning of what people did and said in terms of their culture. They operated as generalists concerning how culture worked, rather than as experts on Japanese culture per se. I think this freed them somewhat from conventional biases.

Part of my responsibility — and I wasn't very good at it — was to get the facts and interpretations that we generated to reach as far up in the decision-making process of the government as possible. Our real strength lay with George Taylor, who at that time was Director of the Far Eastern Division of the Office of War Information. He was convinced of the value of our way of working. He helped greatly in getting our messages to people.

Barkow: Let's move on to discuss the study that I believe most anthropologists think of when we hear your name. The "Stirling County Study" was a landmark both for anthropology and for psychiatric epidemiology. Can you tell us how you came to conduct such a project?

Leighton: One never knows what one's basic motivating factors are, I suppose, but it was something like this: As a resident in training in psychiatry, I got impressed by the fact that unlike most other branches of medicine, psychiatry is almost entirely based on pathological phenomena. The focus is, of course, on behaviour, but it only studies pathological behaviour. In the rest of medicine, you have physiology, the study of normal functioning, to set against clinical pathology and post mortems and so on. Psychiatry lacked this comparative base. Its theories of normal functioning were derived from studies of the abnormal.

I am not saying that there were no studies of normal people. Psychology, especially with children, has done work of that kind. But it wasn't in the same framework as clinical psychiatry and there was not much cross-over in thinking. I became bothered by this, and also curious as to what one would learn if one were to make a systematic study of normal people by employing the same kind of techniques that we used to study people who present themselves to psychiatric services.

That was one point. Related to it was the question of how people who are not patients handle those same life situations that give our patients such serious problems. Work with patients suggested that many of the critical events in their lives leading to illness were common human experiences. How do people who aren't patients handle such problems — sexual problems, problems of jealousy, problems of discrepancies between what they would like to be and what they actually are?

When I began to ask questions of "wise persons" as to how to go about research in this area, I was directed to consider anthropology by Adolf

Meyer, the Chief of Psychiatry at Johns Hopkins, and that's really, I suppose, why I took off in the anthropological direction rather than the sociological or the social psychological. There was at that time almost nothing anyhow in sociology or anthropology in Baltimore. Meyer was a member of the Social Science Research Council and knew most of the leading social scientists in the United States. The social sciences had interested him ever since he had been in Kankakee, Illinois and hobnobbed with people like W.I. Thomas. Out of this background he suggested I talk to Malinowski and in fact he brought Malinowski, who was then at Yale, to Baltimore to conduct a seminar. That gave me a couple of days of quite splendid opportunity to talk with Malinowski and to follow it up later with other visits to him. It was a glorious opportunity, for he died within a year or two of that time.

In addition, the psychologist Norman Cameron was taking training in psychiatry at Johns Hopkins about this time, and he put me in touch with Clyde Kluckhohn. They had both come from the University of Wisconsin and had known each other there. About the same time, also, I met Ruth Benedict and Margaret Mead. All this led to my applying to the Social Science Research Council, with Dorothea Leighton, for a year's fellowship to study anthropology. Ralph Linton agreed to be the director of our studies and suggested that after some class room work at Columbia, we go to the field among the Navajos and the Eskimos of St. Lawrence Island.

All this was preparation for studying normal people in our own culture. We were following a dictum given us by Malinowski: "If you want to look at the earth you had better begin by going to the moon and looking back." He also referred to the famous saying — I think it is from Carlyle — "if you want to know about salt water don't ask a cod." He thought we should study another culture or two and from that vantage point look back at our own.

So that led to work among the Navajo and with the Inuit. Then, as I have mentioned, this was followed by work with people of Japanese culture, first in the U.S. and then finally in Japan, for a total period of about four years concentrated on the study of Japanese culture, including study of the language.

When the war was over I considered what to do now, after what had turned out to be quite a bit more of going to moons and looking back at the earth than Malinowski had in mind when he suggested it.

Two thoughts occurred. One was that both cultural anthropology and psychiatry were in a state of confusion due to theory proliferation without sufficient observational data. It seemed almost a form of addiction in people like Geoffrey Gorer, with his theories that all you needed to do to understand Japanese behaviour was to analyze their toilet training! And Weston La Barre — oh, there were many, many people who were highly ingenious at spinning webs of theory, but as a former physiologist and field naturalist, I

couldn't see that that kind of thinking could ever lead to separating highly probable propositions from highly improbable ones. It didn't lead to amassing data, especially quantitative data, that would help you make the kind of distinctions that you had to make if you were going to be serious about being scientific.

Psychiatry was always claiming that it was scientific, and indeed it is in many ways. But the only part of it that had a procedural structure that was scientific was the biological side, which to me was woefully simplistic. It seemed to me that what we needed were picks and shovels and not more theory, and we needed to pull out of theory a few general propositions that could take common sense as a point of departure. In that frame of reference one could plan the collection of quantitative data that would begin to enable us to sort the more likely propositions from the unlikely propositions.

And so I thought about studying a group of people living in their natural settings. I discovered later there was such a thing as population studies. But I was thinking of it more as an ecologist would. More like Fraser Darling's *West Highland Survey*, though I didn't know about it at the time.

My hope was to study the population in its natural setting and then see what kinds of quantitative data you could collect that would be useful in describing what the social patterns and the cultural patterns really were and the variations in them. Getting those things pinned down, what is the difference between what people say and what in fact they do?

On one side this shaped up as community studies. On the other hand it shaped up as studies of individuals along the lines used in studying patients. I took the topical targets of inquiry from the usual mental status and history questions put to every patient seen at the Hopkins Psychiatric Clinic. Doing it this way made it possible to compare patients and non-patients.

I thought qualitative work to be as important as quantitative, so we had observers live in selected communities in order to describe them. We also utilized the questionnaire approach, which had been greatly developed during the war by sociologists and psychologists.

The data collected included age, sex, occupation, income, and biological as well as social data. It wasn't until we were well into it that I began trying to find quantitative methods for gathering and analyzing data and for relating it to qualitative observations. I thought if the qualitative data seem to be telling a story, the story may or may not be true. Therefore, you need samples from which to draw quantitative data to tell you whether the story is true or more-or-less true or way off. That was my notion of interaction between the qualitative and quantitative approaches to the study of communities and individuals.

Thus the Stirling County Study emerged. For a while I thought of doing it in New Mexico, in the Ramah area, south of Gallup where there were pop-

ulations of Navajo, Zunis, Spanish speaking people, and Mormons who had been there since the middle of the 19th century. There were also newcomers from Texas who had moved in during the Dust Bowl Era of the Thirties. They were called "Texicans." This was a wonderful array of cultural contrast for study. But I decided not, partly because it seemed really more than the methods and technical skills that we had available would be able to meet. The degree of cultural diversity was too great. Moreover it was all changing very rapidly. Then there was also the large problem of getting researchers who would be able to speak those different languages with the degree of fluency required by study aims. Stirling County seemed a better starting place for such work. It had just two language groups, French and English, in both of which it would not be too difficult to recruit behavioural science professionals. In addition, Stirling County offered many advantages. It had quite a range of socio-economic conditions within a distance of sixty by thirty miles. It had the two cultural groups just mentioned, each divided into three different styles of life: the fishermen, and the occupations associated with lumbering, and then a small population of farming people. There were also, of course, the entrepreneurial kinds of activities, stores, professional services, etc., that you find clustered in the towns. I was able to spend a year or so going around the county and talking to the people I knew and ascertaining to what extent they thought that this kind of a study would be acceptable and likely to evoke cooperation.

Barkow: So your childhood visits were another advantage. What do you think the long-term influence of the Stirling County Study has been?

Leighton: I haven't a clue. I am always pleased when people tell me they have heard of it or that it had contributed some to their training in either anthropology or psychiatry but I don't really have any idea how much this has actually happened.

Barkow: Alec, before you were interested in psychiatry or in anthropology your first love was biology, wasn't it?

Leighton: Yes, I think that is true. It was a particular part of biology though, it was the behaviour of wildlife. My mother had a tremendous interest in nature, and my father too. He used to read to us on Sundays at dinner, books that he enjoyed. One of these was Gilbert White's *Natural History of Selbourne*. It covers a period of observations from about 1755 to 1780 and is both science and a piece of English literature. It describes wildlife in and about a village in Hampshire. And Izaak Walton was another author my father used to read to us.

That is probably more ancient history than you want to hear, but anyway, from that start I got more and more interested in mammalogy and still am a member of the American Society of Mammalogists. At high school age

I used to go to their meetings and also spent time in the woods. Which got me going into the woods with the Micmac Indians. I wanted their help in seeing moose and beaver and things like that. Other help and encouragement came from hanging about the Academy of Natural Sciences in Philadelphia. Out of this it seemed natural that in college I should major in biology. I thought perhaps I was going to become an ecologist. I spent two summers in the woods as an undergraduate, gathering material for an undergraduate thesis in the Department of Biology at Princeton which ultimately focussed on the behaviour of the beaver. During that time I also got quite interested in neurophysiology as a possible source of information about behaviour. That was part of the reason I went to Cambridge after I graduated from Princeton, in order to work with Edgar Douglas Adrian. He was a leading figure in neurophysiology and Cambridge was a center for that field at that time.

However, another reason for this move was the Great Depression. It seemed to me that becoming a Ph.D. graduate student in biology at that time was not exactly a strong economic position, should my father's business go down the tubes. This seemed a possibility in the early thirties, and I thought I ought to work toward a position in which I could help the family if need be. So I went for an M.D. It seemed that with that degree I could go forward into research or do part-time or full-time practice if that turned out more desirable. In Cambridge I did pre-clinical training in medicine, and then came back to Johns Hopkins and entered clinical medicine. But while in Cambridge I also did the equivalent of a Masters Degree called the Natural Science Tripos, Part II, in Neurophysiology. So I have had both some experience as a field naturalist and a lot of fun in the laboratory investigating the nature of the peripheral nervous system.

Barkow: I think that early background shows in your work because you never forget that human beings, whatever else we are, are animals.

Leighton: So we are.

Barkow: You've striven to make psychiatry a more scientific discipline, one that makes use of epidemiology and of anthropology. Thinking back over the years, where do you see progress and where don't you see it?

Leighton: That is a good question and I don't know that I have thought about it as much as I should have. Certainly not so much as it deserves. I think we are still hung up on theory in psychiatry. I think the same is true in cultural anthropology — more so even than when I came in. The fascination with theory and the unwillingness to do the dogs-body work that you have to do in order to tie things down with quantitative evidence is a handicap to the advance of psychiatry and of anthropology. Both fields are too susceptible to theoretical glitter.

Pharmacology, of course, in a practical way has done great things for

mentally ill people. When I was a resident the history of psychiatry was lit-
tered with the dead careers of people who had tried to get "the answer" to
mental illness by the biological route, and very little progress, aside from
uncovering the etiology of syphilis and pellagra, had been made. This was
one reason why it seemed to me that the time was perhaps ripe for investigat-
ing the social environment. Perhaps the disciplines pertinent to it would have
something to offer.

Biology is, of course, no longer so sterile. I think we have got many
clues that make research on what is now called neurobiology very promising,
and I think that the clinically, socially and culturally oriented people who are
resisting that approach rather than welcoming it are making a mistake. It is a
little disappointing that there hasn't been more interest of a scientific and
investigatory type in human behaviour as such in psychiatry, and you can say
the same of the social sciences. I think sociology has gone backward.

Saying that is of course to blow off an opinion, but perhaps that is what
you are supposed to do in an interview. Right after the war under the leader-
ship of Robert Merton, Paul Lazarsfeld, Edward Suchman and Robin Wil-
liams, there was a strong empirical bent in American sociology. It differed
thus from European sociology, which was mostly political philosophy. It was
trying to build theory cumulatively from factual material and, as far as I can
see, that is not nearly so strong a trend now as it used to be. In anthropology,
culture and personality has gone by the board, which I think is too bad. I
think there were potentialities for methods there that might have gone some-
where. I am afraid that anthropology has become focussed on cultural deter-
minism and cultural relativity to the point of dogma. This is beginning to be
unproductive and counterproductive, because it blocks the way to looking at
other determining and etiological factors which are also in the multifactorial
total. Since the war it seems to me that these disciplines have lost interest in
testing theories and would rather invent them. I think this has happened
because in both psychiatry and in the social sciences there is a very great hos-
tility to biological determinism of any kind. This makes them reluctant to put
things to a test. All this I am saying is a very subjective judgement, but I have
had a lot of students, and I've watched them through the years, and I have
seen them shy off, struggle and kick and fight like a salmon not to be landed
in a field where they will have to put things to a test. They would much rather
spin speculation than gather data that could tell them that this or that idea is
wrong or very unlikely.

Barkow: Once upon a time, missionaries and anthropologists seemed to have
almost a monopoly on cross-cultural experience. Nowadays, university
undergraduates, business people, and volunteers of all kinds find themselves
immersed in other ways of life. I know that back in the 1950s you were
involved in what was probably one of the world's most successful programs

for preparing people for cross-cultural experience. Could you tell us about it?

Leighton: I think you are referring to the Southwestern Seminar that we ran from Cornell, sponsored by the Russell Sage Foundation. That was a lot of fun. It was one of the most interesting pedagogical experiences I have ever had.

On the other hand, I don't think it was all that world-shakingly success-ful, because we were never able to attract very many students, even though we did run it for five years. But it was, as I say, a lot of fun. It was the only time in my life when, over a short period of six weeks, it seemed to me that people really changed their values, their attitudes and their orientations. The students were not behavioural science students but agronomists, engineers, nutritionists, dentists, and doctors, going to work in third world environments and cultures. They were well trained in their particular disciplines. The course was designed for those who had had no cultural anthropological train-ing. The purpose was to give them a real, three dimensional feel for what it is like to be in and of another culture.

The first week we met in such places as the Forestry Camp in the San Francisco Mountains just outside of Flagstaff, Arizona — a lovely spot to reflect and read. We were planning to visit the Papago tribe as the first "for-eign country." The students were given three intense days of studying Papago culture. They read; we had anthropologists, who were Papago experts, come and talk, and then we had the interviewing demonstrations and finally role-playing, in which the students alternated as Papagos and as inter-viewers. That generated a lot of discussion about what was good, what was bad, what was theoretically feasible and so forth. It alerted people and gave some practice. Then we went to the Papago reservation in Southern Arizona, about a day's drive from the Camp. On the way you dropped from summer-time in Maine down to summertime in the Sonoran Desert, where the temper-ature rarely goes below 100° F during the day and where the desert heat makes the cacti shimmer as if they were underwater. This change hits you dramatically when you go down the side of the mesa near Congress Junction. We camped at Sells, which is the headquarters of the Papago Indian Agency. As you know, the Papago reservation is fairly large, about the size of Connec-ticut.

The first day in the Papago country we met with the heads of the ser-vices at Sells. We explained ourselves, and they told us about what the Agency was trying to do. Then they very kindly arranged it for us so that each of the specialties represented by our students could interview his or her counterpart in the agency. Thus, nurses and doctors in our group talked with individual agency doctors and nurses. The agronomists saw the agricultural extension agency workers and so on. The students pumped the agency people about the Indians and about what problems there were in providing services.

And after doing this all day, we had a seminar at night around a camp fire at which each student shared his experience with the others and reviewed the problems the agency was having in his field. The students enjoyed talking to individuals in their own professions. It gave a sense of reality to what they were learning.

The second day they had an opportunity to interview the chairmen of the various Tribal Council committees. This was the first contact with Papagos. Again the students were divided according to their professions and interests and matched with the committee chairmen according to the tasks assigned the committees. In general, these corresponded to the service divisions of the agency. Of course, in this situation, they learned new things, things not heard the day before. Again there was the camp fire seminar in the evening, and this time there was even more excitement because the students felt they were getting a new angle on things and closer to the real truth. The seminars discussed how one might interpret discrepancies.

On day three we had what the students came to call "the parachute jump." This involved starting out at 4:00 a.m. and, after going 20 miles or more out into the desert, stopping at a village to cast out a student. His or her mission was to spend three days there, find somebody who could speak English, first, and then find with the aid of that person someone who would take him or her in for the three nights and provide food. With that settled, they were to begin interviewing village informants on a topic of the student's choosing. Because not many of the Papagos spoke English, part of the challenge was learning how to establish communications, nonetheless. So we went on across the desert, dropping off students until we were rid of them all.

Now, this wasn't completely unplanned and the students knew that. We had permission from the Tribal Council, who had in turn gotten approval from each village headman where we stopped. The villagers knew we were coming and had agreed to it. We did not, however, make any advance arrangements as to who would take in the student. That part was not prearranged. Well, our almost universal experience was that when we came back to pick up the students they were ecstatic, because they had found they could do what had seemed very daunting, at first. They found they could gather information, much of it more detailed and interesting than anything previously learned. They felt it to be a grass roots experience, rewarding and enlightening.

I remember one economist, a student of Kenneth Galbraith, who was very puzzled about the input-output economy of the Papago. He couldn't see how it could possibly operate. He reviewed the books of the Agency and the traders' books, but he couldn't see how they could be making a living. In the village, however, he discovered that many Papagos went to fiestas and to visit relatives in Mexico several times a year. They earned their cash in Arizona

but they did much of their grocery shopping in Mexico, plus a little selling in Arizona of what they bought in Mexico. Prices were much lower in Mexico.

The kind of thing we did with the Papagos was repeated in two other cultural groups. These consisted of Navajos and of the Spanish-speaking villages in the Upper Rio Grande Valley.

At the end, we had a session in which there was a sort of comprehensive exam. In this we asked the students to imagine that each had a close friend who was going off to some country that he had never been to before, a third world country. This friend would be of the same profession as the student. The task was to write the friend a long letter, in which the student would set down for him or her the things he or she should know in order to begin the job. What were the main points about human relations and culture that the friend should know? The students sweated away over that. They enjoyed this writing very much, as it gave them a chance to synthesize their experiences, observations and reading. They seemed to get a lot out of it and I know we the teachers learned tremendously from what they said.

Interestingly enough, some of the students were themselves natives of the countries to which they were going. These were professionals, but upper-class in their countries. They felt they did not know how to work with and teach working class people, even if in their own culture. The very idea that they might have to take up a handful of soil and show it to a peasant and explain about it or demonstrate how to plant — these were things they had no idea about.

Barkow: Why don't we have such programs, today?

Leighton: I don't know, I think there would be a place for them. Perhaps there are some.

Barkow: We have been spending a lot of time talking about what you have done in the past: What are you working on now?

Leighton: Well, in a word, I am working with Jane Murphy Leighton on the analyses of the data that we have been collecting in Stirling County since 1948. That is when the first qualitative field studies were made. Our first systematic survey was in 1952 and we continued to gather data full scale until 1970. Since that date we have mostly been analyzing. We have intermittent time and population samples. As the information becomes longitudinal, it grows tremendously complicated and you move from what epidemiologists call *prevalence* studies, which is the number of cases in a population at a given moment in time, to what they call *incidence* studies, which is the number of new cases appearing over a period of time. With that you enter the intellectual big-time, because that is when you begin to see antecedents and consequents, and where you begin to tie social and cultural processes to mental health consequences and mental illness consequences. We have far more

data than we can probably ever analyze, certainly more than we will ever get completely analyzed. But you do get exciting leads from the material and you do get a wonderful sense of things gradually becoming more and more probable so far as conclusions are concerned, and less and less vague.

It is also fascinating to see how people in different parts of the world, often using very different methods, come up with similar conclusions. Many of our early apprehensions that minor differences in method would make gross differences in findings have so far not proven very true. Life at present is very much like sitting under the Christmas tree all day opening packages.

Jane and her team of epidemiologic and statistical advisers are handling the quantitative analyses and some of the qualitative. My role is to participate in the quantitative analyses, reviewing the nature of the questions we had in mind when the questions were put in the different surveys; and then also the interpretations and the qualitative analysis. I have developed a major interest in the area that you asked about a while ago, that is, the scientific issues about how to study human behaviour and how one can get closure on issues and questions and how to make the work cumulative. And I suppose the way to summarize what I am emphasizing right now is to say that we are interested in the overall synthesis, combining the quantitative and the qualitative approaches so as to make cumulative progress toward more and more refined approximations of the truth. That is what I am putting most of my time into, in coordination with Jane whose analyses are focussed on more specific questions regarding predictors of mental illness and mental health, particularly (and we are just entering into this) regarding what the social and cultural factors involved are. It takes a long time. Christmas has been a long time in coming, but we are getting there.

Barkow : Thank you very much Alec. This has been fun, I hope we can do it again.

Leighton: It has. Thank you very much.

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