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An Interview with Edmund Leach

by Adam Kuper

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ERL: An autobiographical interview must begin with family mythology rather than history.

My background is 19th-century Lancashire. All four of my great-grandfathers were Rochdale mill-owners living within four miles of one another. By mid-century the families were already very closely intermarried. The numbers were huge. My mother’s mother’s mother was one of 17 children; my childhood I thought of the world as consisting exclusively of kinsmen and family domestics, a good start for an anthropologist.

My mother was already related to my father before they were married, but she was 22 years his junior and of quite different upbringing.

By that time the Leaches had got out of textiles, and the family fortunes were linked with a sugar plantation in northern Argentina—a country that I have never visited, though my brother and sister and many of my cousins were born and bred there.

I saw very little of my father (who died in 1932), and both my grandfathers died before I was born, but I had a splendid deputy “grandfather figure” in the person of my mother’s uncle, Sir Henry Hoyle Howorth, author of a celebrated five-volume History of the Mongols (1876–1927). HHH, who died in our house in 1922, was a thoroughly unorthodox member of the intellectual Establishment: Member of Parliament, Fellow of the Royal Society, Fellow of the Society of Antiquaries, President of the Royal Archaeological Institute, Member of the Council of the Anthropological Institute and of the Zoological Society, Trustee of the British Museum, notable art collector (a number of his pictures are now in the National Gallery, London), author of works with titles such as The Mammoth and the Flood (1887) and The Golden Days of the Early English Church (1917). I had no sympathy for the cricket-playing, pheasant-shooting enthusiasms of my cousins. Uncle Henry provided me with an alternative goal which I have consistently pursued with some success.

AK: Is that why you made your escape by going to China?

ERL: Well, who knows?

AK: But first you were sent to a public school?

ERL: Oh, yes, I had to follow family tradition. I went to Marlborough. My father and all his brothers and many of my cousins had been there beforehand. I was the twenty-first Leach. Until I arrived, all Leaches had automatically become members of the cricket eleven. When it was discovered that I could hardly see the ball, my housemaster lost all interest. Much later, when I had made my way into the Upper Sixth, I was ruthlessly coached so that I could bring honour to the school by winning a mathematics scholarship to Clare College, Cambridge. Without question, my years at Marlborough were the unhappiest days of my life, worse even than the six years of army service 1939–45.

I went up to Cambridge in 1929. I was not a real mathematician. After a year I switched to engineering and graduated with a First in 1932. I was still very immature, but Cambridge was a glorious experience.

On graduation I first looked for employment in England, but the best thing on offer was a job in a gas-works, and this time the family tradition—“go abroad, young man, go abroad!” (my father had gone to New Zealand in a windjammer)—served me well. I signed up with the Far Eastern trading firm of John Swire and Sons (Butterfield and Swire), whose present interests include ownership of Cathay Pacific Airways. After a year in the London office I went out to China and had spells of duty in Hong Kong, Shanghai, Chungking, Tsingtao, ending up in Peking. It was very much an “old school tie” sort of firm. What mattered was that you had an Oxbridge degree; what sort of degree was irrelevant.

On holiday I travelled widely. I recall that I climbed four of the five sacred mountains of China. Chungking itself was still a mediaeval city, all steps and sedan chairs. No roads or motor vehicles except the odd half-disintegrated bus. I did not speak good Chinese but enough to get around. I was fascinated by the whole cultural system, which was so very different from anything that I had encountered in Europe, but I had no plans as yet to become an anthropologist.

At the end of my contract, in the fall of 1936, I was offered a renewal, but I had had enough of commerce and sacked myself. The visit to Peking was to plan a voyage home by the Trans-Siberian Railway with a friend in the British Embassy. The theory was that we would get as far as Moscow by rail and
then float down the Volga on a barge. Anything seemed plausible in those days! At that point Stalin started bumping off his generals. The great Stalinist purges had begun. Our contact in the Russian Embassy said that all visas were cancelled but that perhaps if we hung around for three months things would clarify.

Peking in those days was a dream city. The Chinese centre of government and the "real" foreign embassies were located at Nanking on the Yangtse, but the foreign embassies retained their language schools in Peking, and there was a large polyglot community of Western artists of diverse kinds who had fished up there more or less by mistake and found Peking the most beautiful (and also the cheapest) city in the world. I gather that very little of the Peking that I knew has survived.

The Westerners indulged themselves in drink and debauchery at the embassies' expense. At a party in the British Embassy a very drunk American named Kilton Stewart, a former Mormon missionary, announced that he was on the way to "Bottle the Bugger." "Would anyone like to come to 'Bottle the Bugger'" I said, "Well, that sounds an interesting place; where is it?" It transpired that it was a small island with the map name of Botel Tobago, south-east from Formosa (Taiwan). I was one of a party of four (including Stewart) who visited Botel Tobago in the period December–March 1936–37. We were, I think, the first non-Asiatics to make such a visit in this century. The inhabitants, the Yami, were "real primitives," in the classical ethnographic sense, the sort of people whom "real" anthropologists are expected to study! We were not real anthropologists, but we did our best.

I myself only remained on the island for about eight weeks, and I had no idea what I was up to. I made ethnographic notes as well as I could and drew accurate scale drawings of the boats and houses. When I got home to England (by sea, not by rail), I had my notes and drawings and photographs. I knew that Rosemary Upcott, a childhood friend, had married an anthropologist, Raymond Firth. I asked Rosemary to introduce me to her husband, and that was the beginning of a lifelong discipleship. Characteristically, Raymond said, "Well, this looks very interesting, though of course you have asked all the wrong questions!" Even so, he introduced me to Malinowski.

This was the spring of 1937. Bronio Malinowski in those days was a law unto himself. If he liked you, he liked you, and you could do no wrong. Fortunately, he did like me. For the next 18 months (in retrospect it seems much more) I was in Malinowski's pocket. He was intensely charismatic. I did not know him at all well personally, but I attended his seminars. He was supposed to give a weekly lecture to undergraduates, but this he disliked doing. He was deeply offensive to his audience, especially to the women. When they walked out in protest, the lecture session was transformed into a graduate seminar. The seminar had an official allocated time from five to seven on the same day, but it usually started at two and went on indefinitely. By that time it was notorious, a sort of circus attended by all kinds of celebrities who were not necessarily connected with anthropology at all. But somehow we learned a lot. My own contemporaries included William Stanner and Phyllis Kaberry. Ian Hogbin was a visitor. Radcliffe-Brown had only very recently arrived from Chicago to take up his Oxford chair, and the rivalry between Oxford and the London School of Economics was not yet apparent. British social anthropology meant LSE anthropology; it was as simple as that.

In the summer of 1938, inspired by a love affair with an archaeologist, I visited Iraq, planning to write a thesis on the Rowanduz Kurds. It came to nothing. After the Munich crisis and Chamberlain's gesticulations about "peace in our time" I was back in London with an aborted project. I spent the next academic year, 1938–39, working as Raymond Firth's research assistant, an extremely valuable experience from my point of view. Malinowski was on sabbatical leave at Yale (he never returned), but Meyer Fortes came back from West Africa at that time and taught me during the spring and summer semesters.

Then, in the summer of 1939, the Firths left for Malaysia to conduct the research which produced, among other things, Raymond Firth's Malay Fishermen (1946), while I left for north-east Burma to undertake field research among the Kachin. The monograph that I had planned to write would not have had the quantitative detail which characterises Raymond Firth's book, but it was to be a socio-economic study of the same general kind. I hoped to display the organisation of the local community in terms of domestic production and the network of trading. Segmentary lineages and cross-cousin marriage didn't come into the story at all. I think that if it had not...
been for the disasters of war we really would have produced volumes which were built around a common body of theoretical interests. But who can say? It didn’t work out like that.

Hitler’s war started almost at the same time as I arrived in Burma. There was no going back to England this time round. I signed up for the Burma army and went off into the field. Later I was called up for officer training. I had planned to get married when I returned home in the summer of 1940, but in fact Celia joined me in February and we were married in Rangoon. We went off into the hills together. I was not called back for “active service” until the autumn of 1940.

By any reckoning I had an extraordinary series of war experiences. As far as the Burma army was concerned, I was odd man out, but I was potentially useful because I spoke the Kachin language and the Kachins were, in effect, the Gurkhas of the Burma army. At first the army used me as a recruiting officer, which was weird as my political sympathies were not in that direction at all, but I was under orders.

When the Japanese eventually arrived at the end of 1941, I got shunted into a crazy cloak-and-dagger outfit run by H. N. C. Stevenson, a Frontier Service officer who had had some training in anthropology under Malinowski. He persuaded the governor of Burma that if the British were driven out of Burma then we ought to leave behind a network of intelligence officers with local linguistic competence. I got sucked into this madhouse.

Celia and infant daughter were flown to India in March 1942, and I didn’t see them again for three and a half years. I was supposed to hang around Hpilang (the base of my earlier fieldwork) with a radio set. My assistant lost his stores and the radio, and we had to head for home. It is a long story. I got back to England in the summer of 1945, supposedly on short leave, but after Hiroshima all return trips were cancelled. I was demobilised in January 1946. After eight years I was still registered for an LSE Ph.D. Raymond Firth was now professor and head of department. I was not at all sure that I wanted to have anything more to do with anthropology, but between us we agreed that I should reread all the literature of the Kachin (and of other Burma frontier “tribes”), going back to the beginning of the 19th century, and reassess it in the light of my “on the ground” experiences. I completed the thesis in the spring of 1947. Radcliffe-Brown was my external examiner. It is a messy affair. Bits of it reappear in Political Systems

Taking pigs to market, near Hpilang, Kachin Hills (1934 photo by the late J. H. Green).
of Highland Burma (1954), but that is a very different kind of book.

I went off to Borneo almost immediately afterwards on a scheme which had been cooked up by Raymond. He had sold the Colonial Office on the idea that they needed a lot more information about the inhabitants of their newly acquired Crown Colony of Sarawak. I was to make recommendations. The outcome was pleasing because nearly everything that I recommended was eventually carried out: Derek Freeman worked with the Iban, Bill Geddes with the Land Dayak, Stephen Morris with the Melanau, Ju-kang T’ien with the Kuching Chinese, Tom Harrisson with the Coastal Malays.

When I returned, I joined the LSE staff.

AK: How was the thesis transformed into Political Systems of Highland Burma?

ERL: The thesis and the book are clearly related, but they are very different. The book is about the Kachin (and their Shan neighbours). The thesis is about a much vaguer category, “the hill tribes of Burma and Assam.” It is much more oriented towards Raymond’s interest in socio-economic interactions. It contains a lot of mathematical formulae, all rather bogus. The book is another matter; it was not published until 1954. It entailed an enormous amount of reading in obscure sources. I actually resigned from the teaching staff of the LSE and worked full-time on the book for over a year. It needed to be my own kind of thing. When I rejoined the LSE in the status of “Reader,” it was a new appointment.

AK: But intellectually it is such an original book. It’s difficult to see the links between that book and Malinowskian anthropology or anything else that was going on at the time.

ERL: I needed a new terminology. Everyone kept talking about “tribes” as if they were closed systems. As you moved across the road you moved from Tribal Area “A” to Tribal Area “B.” This was plain nonsense as far South-East Asia is concerned, and I suspected that it was nonsense too in many parts of Africa where “tribes” were credited with hard-edged boundaries. I needed some way of talking about social systems which were not bounded systems. Network theory—Barnes, Mitchell, etc.—was tackling the same sort of problem but in a different way. I particularly liked Barnes’s image of a fishing net which goes on and on, folded over, you never come to an end of it.

AK: But your book is also rather historical . . .

ERL: Well, yes. I wanted to understand how a synchronic “functionalist”:“transformational” model (the idealist structure at the back of my gumsaigumiao schema) could be fitted to history as it had actually happened. At that time I was very guarded. But this is why, at the present time, I find the Sahlins (1985)/Valeri (1985) stuff about Hawaii so exciting. It seems to me that almost for the first time something is developing in the forms of anthropological presentation where we can reread historical ethnography in such a way that we begin to see the material from the point of view of the observed rather than of the observer. I have come to realise much more sharply than before that in no circumstances has the “European” observer—anthropologist, missionary, ship’s captain, traveller—ever been able to give an objective account of the “manners and customs of a primitive society.” The very fact that the observer was there at all completely altered the nature of what was observed. The “Death of Cook” business is an extreme example, but the case is general.

When I was working on Political Systems of Highland Burma and reading the 19th- and 20th-century reports, I kept asking myself: What did this observer really observe? How did it look from the other side? I knew the terrain that was being talked about; why was it seen in this way? I could give a more sophisticated set of answers now than I gave then, but that was part of what I was after.

AK: So Political Systems was untribal and unsynchronic and unmaterialistic and uneconomic and completely different from all the monographs that were coming out at the time. How was it received?

ERL: By the time it was published I had moved from the LSE to Cambridge to work under Meyer Fortes. Fortes’s initial comment was scathing. Like Raymond in his foreword, he took the line that this is not what genuine social anthropolo-
gists should be doing—an interesting special case; perhaps, but not a style with any general applicability. Both my mentors viewed my reductionist models with grave distaste. But in the end the book made its way and has run through many reprints. It became something of a standard text on both sides of the Atlantic and has been translated into French.

AK: Then Lévi-Strauss started writing about the Kachin . . .

ERL: You must keep an eye on the chronology here. The first Kachin paper by myself was “Jinghpaw Kinship Terminology,” completed in Calcutta in 1943. It was published in the 1945 volume of the Journal of the Royal Anthropological Institute, but this did not actually appear in print until 1948. My thesis, which was completed in 1947, makes few references to the Kachin marriage system or to the relationship between that system and the local political structure. Where these topics are mentioned (for example, page 173), the details are not discussed. The reader is simply referred to “Leach (1945)—publication delayed.” In 1952 Lévi-Strauss (1953) drew atten-
tion to Leach (1945) as an example of structuralist analysis (in Lévi-Strauss's sense of "structuralist"). But at that date few British anthropologists can have heard of Lévi-Strauss. His book *Les structures élémentaires de la parenté* was published in 1949. My Curr Prize essay "The Structural Implications of Matrilateral Cross-Cousin Marriage" (1951) is a direct commentary on Lévi-Strauss (1949). Audrey Richards belatedly reviewed Lévi-Strauss (1949) in *Man* (1952) but does not seem to have appreciated its significance. Even in 1954, when my *Political Systems* appeared, few British anthropologists other than Rodney Needham and myself had read Lévi-Strauss (1949) with any care. But I was in a different position. The Kachin provide the central hinge around which much of Lévi-Strauss's argument turns.

Lévi-Strauss relied, of course, on pre-1940 ethnography, but he garbled it much more than he need have done by mixing up two entirely different bodies of data: material relating to the "Kachins" and material relating to the "Haka Chins" living some 500 miles to the west. But what I found very strange was that his structural intuitions, applied to inaccurate ethnography, were able to perceive features of the political implications of the Kachin marriage system which had nowhere been made explicit in the published ethnographic data, not even in my own.

As is well known, Lévi-Strauss's structuralist thinking was greatly influenced by his contacts with Roman Jakobson in New York during the war. My own interest in linguistics also stemmed from Jakobson but much later. I spent a year at the Center for Advanced Study in the Behavioral Sciences at Palo Alto in 1960–61, when Jakobson and Halle were both there. Jakobson at that time was finishing off his work on distinctive-feature theory in phonetics.

AK: There seems to be a great jump from your Kachin study to your work in Sri Lanka, where you did a study which in many ways resembles much more the geographical and materialist determinism of American anthropology of that period.

ERL: That was partly a response to my contemporaries. You asked me earlier about responses to *Political Systems*. They were mostly variations on an empirical theme: "This kind of fancy theory is all very well, but if a man can't do ethnography..." And I thought, "To hell with it, I'll do some ethnography." Very definitely I wanted to show my immediate colleagues that I was not just gassing off. In the Kachin case, because of the war and not being allowed to keep diaries when "behind the Japanese lines," I had brought back very little data. In the Sri Lanka case, although I was not in Pul Eliya for very long, I was able to collect and present a great deal of data.

Also, the dialectical opposition between myself and Meyer Fortes was building up. For Meyer "kinship" was a thing in itself, whereas I felt that, while kinship is something that we can get hold of as anthropologists, it is really just a way of talking about property and class relations. This feeling came from my childhood experiences. I still felt it in the Kachin Hills. I still feel it today. In the Sri Lanka case, the point of my book is that talk about kinship, which went on all the time, was really talk about claims to irrigation water. In the Kachin Hills talk about kinship was talk about the networks of political power. But kinship is not a "thing in itself"; this is where I differ from Radcliffe-Brown, from Fortes, from Lévi-Strauss.

AK: Were you also reacting against Evans-Pritchard?

ERL: E-P? No, I don't think so. I never knew E-P at all well, and I admired his later work after he had shed his links with Radcliffe-Brown. I don't think I was ever happy with the formalism of *The Nuer* and *African Political Systems*. But that takes us right back to 1940. The still earlier *Witchcraft among the Azande* (1937), though intended to show E-P's independence of Malinowski, seems to me to have merit precisely because of its Malinowskian qualities.

I did not know Radcliffe-Brown at all until late in his life, when he had retired from Oxford and was living a largely existence in a London hotel. I used to take tea with him from time to time. He seemed a pathetic figure. Darryl Forde was the mainstay of his existence at that time. I found R-B very pompous. I could not stand the way he would continually teach other people their business. The "society as an organism" model, which he seems to have taken with complete seriousness during his Chicago period, struck me as absolute rubbish.

AK: You weren't influenced by the Americans, then? It was a period of American materialist anthropology and ecological determinism and so on.

ERL: No, Marvin Harris and I did not share the same campsite. I was at one time rather close to David Schneider, who was on the staff of the LSE at the same time as I was in the early '50s. And I shared interests with Harold Conklin of Yale because of his expertise in Ifugao irrigation systems. And latterly many of the most interesting "American" anthropologists were trained in England, for example, Maybury-Lewis, Tambiah, and Yalman at Harvard. Former colleagues and pupils of mine are to be found all over the United States and Canada. We have influenced each other. Let me recap what I said about Sri Lanka. When Nur Yalman was doing the fieldwork which led to *Under the Bo Tree* (1967), I was, for part of the time, also in Sri Lanka. I was his official supervisor. I visited him in the field at least twice, and he visited me.

AK: I knew that Yalman worked under your supervision, but I had never put together your two studies in time and space in quite that way. They make a very odd contrast. He is writing in terms of alliance theory, "kinship is a thing in itself," and there are you, his supervisor, 40 miles away down the road, planning a book which says, "of course kinship is not a thing in itself."

ERL: That's how interactions work.

AK: Well, that's how your interactions seem to work, because your stories are all about how your books developed as dialectical argument with various focal figures.

ERL: Yet, the sequence is always dialectical. There was, as I said earlier, a point in my anthropological development when Malinowski could do no wrong. In the next phase Malinowski could do no right. But with maturity I came to see that there was merit on both sides. I see this as a Hegelian process, a very fundamental element in the way that thinking in the human sciences develops over time. But when this sequence leads you round in a circle, you are not just back where you started. You have moved on a bit, or you have moved somewhere else. But always the process involves the initial rejection of your immediate ancestors, the teachers to whom you are most directly indebted.

AK: Did you ever become a convert to structuralism?

ERL: What a question! If by structuralism you mean Lévi-Strauss's brand of structuralism, that is hard to answer. So far as I know, Lévi-Strauss has never expressed approval for anything I have written with the exception of the "Jingpaw Kinship Terminology" (1945) item mentioned already. In writing
about his work I have always been critical, but it is criticism shot through and through with admiration. In turn, my own work, especially such successful items as *Culture and Communication* (1976), has become heavily impregnated with "structuralist" ideas. They are not "structuralist" in a sense that Lévi-Strauss or his close disciples would accept, but that is not quite the issue. If Marshall Sahlins can now be rated a "structuralist," as I think he should be, then I am in the same camp or at any rate only a little way down river.

It has been pointed out by others, including David Schneider, that there is a chronological break in Lévi-Strauss's interests. There was the phase when he was primarily interested in the structural permutations of systems of kinship terminology. At that point his structuralism was markedly mathematical, but when he got on to Mythologies and started to talk about "the human mind" we were in a different ball game. Some of the earlier myth analyses, especially "The Story of Asdwal," are more or less acceptable because they are concerned with a limited terrain. The variant stories fit together as a functionalist whole! But in the later volumes of Mythologies the manner has become Frazerian. Any evidence will do; it can be drawn from any part of the map. The "structure" is imposed on the evidence; it does not emerge out of the evidence. But I remain fascinated by the great guru's ingenuity!

AK: You have applied your own structuralism mainly to the Bible. It has always struck me that part of your inspiration here comes from a fundamental dislike of organised religion.

ERL: I don't think I have ever made any bones about that, yet a number of very distinguished Church of England clerics are among my closest friends. Many people—even anthropologists!—seem to find it difficult to understand how one might be interested in "religion" as a phenomenon while feeling a marked personal aversion to "organised religion," in the sense of some particular institutionalised church, Christian, Jewish, or whatever.

But the reason that I write about the Bible is that here is a shared body of mythology which everyone can consult. Lévi-Strauss cheats by using translated précis of sources which the rest of us cannot consult, except with extreme difficulty. But biblical texts, in many different versions, are available for everyone. By using biblical sources you can (potentially) have a genuine argument. Maybe it is fun to flutter the Christian dovecotes by throwing in a text from the Talmud, but if the reader takes the trouble he/she can check up.

AK: So you want to persuade your readers about the method by using an accessible body of data. In that way you avoid pulling rank as a specialist ethnographer. Is that it?

ERL: There is a strong element of that, but I am also interested in having a discussion about anthropological issues with people who are not professional anthropologists.

Let me give you an example. There has always been on sale in King's College Chapel a single-sheet pamphlet guide to the windows. It probably hasn't been changed for 50 years. But a reprint will shortly be needed, and the Dean of Chapel recently asked me to help him with a revision which would pay more attention to the "structuralist" transformations by which the "type" stories of the Old Testament are matched by the "antitype" stories in the New Testament. I think we have done rather a good job. But the analysis serves to bring out a key point of another kind, one which contemporary Protestant Christians would rather not know about. This is the fact that when the windows were being designed at the beginning of the 16th century, the image of the Virgin explicitly stood for "The Church." This has the result that the Virgin intrudes into the pictures in all sorts of places where she should not appear at all according to the biblical texts. Clearly, one does not get very far with this sort of thing on a single sheet of paper, but it opens up possibilities. But perhaps only in King's would the Dean of Chapel cooperate with an atheist ex-Provost to produce such an anti-Establishment result!

AK: Were you brought up a Christian?

ERL: Oh, very much so. I am formally a member of the Church of England.

AK: There seems to have been rather an emotional reaction . . .

ERL: I don't really think so. I suspect that my mother would have liked me to become a missionary! She was a very serious church-goer. What she really believed in I have no idea. And of course at Marlborough, a school founded for the benefit of the sons of impoverished clergymen, we went to chapel at least twice a day. I was bound to react against such a regime. I think that the last time I attended Holy Communion must have been about half-way through my time as an undergraduate, around 1930.

AK: You react very strongly. You write books in reaction to a point of view, or you become involved in a secular critique of the Bible in reaction to organised religion. I have always seen you as a kind of opposition figure. I think that is partly because when I was here as a research student in 1962 you were part of the opposition, and not only within the Faculty of Archaeology and Anthropology. You were part of the opposition in King's to the way that the college was then run. You were also a leader of the opposition to the way that British anthropology was then run and what it stood for; and so on. It was therefore strange for me to see you become, later, in some ways a successful Establishment figure.

ERL: Well, it seems strange to me, too. But having won most of my battles I could hardly turn round and become the leader of the Counter-Reformation.

AK: And yet that is how you are perceived by a younger generation. As Provost of King's, a knight, President of the Royal Anthropological Institute, Chairman of the Association of Social Anthropologists, and so on and so forth, you are an archetypal Establishment figure. How have you been able to do this?

ERL: When I am annually asked for amendments to my entry in *Who's Who*, I am always provoked to laughter. Who is this comic clown? There am I, aged 75, with all this long list of "honours." The whole hierarchy of the Establishment—the good and the great—is a joke. But I use it. And why not? I still have (academic) "political" objectives. If I can use my "influence" to achieve my ends, I shall. The Young Turks who think of me as a conservative old fogey would still approve of my objectives if they knew what they were!

AK: What would your Uncle Henry have said?

ERL: Oh, he would have approved strongly. He was always in opposition. He refused to believe in ice ages long after everyone took them for granted. In politics, when an M.P., he formed a splinter party on the Irish issue. Which side he was on I have no idea. But I am told that his New Party could at most marshal two votes and that he was thrown out at the next election.
AK: Still, I am astonished to find that in some circles you have come to be seen as a reactionary.

ERL: I don't find it so strange. there can be guile in these matters. Here in King's I am certainly seen as very conservative. It has already been forgotten that women became members of the College for the first time (after 525 years) while I was Provost. It is an interesting story.

To make the proposal plausible, we had to have allies. There was much backstage negotiation with our neighbours, Clare and Churchill. Both these colleges eventually first admitted women at the same time as King's. But they had problems. In Clare the Master was enthusiastic; at Churchill the Master was opposed. The Clare story was that the Old Guard dug up a retired fellow who had not been seen for 20 years. He turned up in a Bath chair and cast his vote against the admission of women. So they were back at square one.

We took things slowly and talked it out for a couple of years or perhaps more. The opposition were clearly in a minority, so he asked them to change sides and let this historic decision go through on a unanimous vote. One splendid old boy refused to take that one and said he must abstain. So King's voted for the admission of women nem. con.!

That could only have happened in King's. But there was some anthropology in it, too. If I had not already begun to acquire the reputation of being much more conservative than my eratic predecessor, I doubt if we would have made it at all. I am no fisherman, but it was all rather like landing a salmon.

The Construction of Primary Data in Cultural Anthropology

by H. Russell Bernard, Pertti J. Pelto, Oswald Werner, James Boster, A. Kimball Romney, Allen Johnson, Carol R. Ember, and Alice Kasakoff

In April 1985, a conference was held at the National Science Foundation in Washington, D.C., on the construction of primary data in cultural anthropology. We were concerned with data gathering rather than with data analysis. We felt that, while much attention had been focused on data analysis, particularly on complex statistical analysis, insufficient attention had been paid to issues of validity, reliability, and accuracy of our basic raw materials. Recognizing that one bias has entered into data, statistical analysis will not get it out, we were concerned with improving the construction of primary data in cultural anthropology. To address this issue we felt it useful to focus discussion on the four main types of data construction in our discipline: (1) relatively open-ended, unstructured interviews with key informants, (2) structured interviews of respondents who, in the case of surveys, may number in the hundreds or thousands, (3) direct observation of behavior and environmental features, and (4) extraction of information from existing records such as native texts, court proceedings, marriage records, and so on. In the case of the Human Relations Area Files, this last method involves the coding of the ethnographic work of colleagues.

This concern with the quality of primary data is certainly not new in anthropology. Malinowski addressed it forcefully in his research program. The ethnoscience perspective of the '60s was based largely on a concern for validity (what do the natives really think?) and for reliability (do question frames produce the same results over a series of informants or from a single informant interviewed by a series of anthropologists?). More recently, anthropologists have become concerned also with accuracy and precision of data. For example, a question such as "How often does it rain around here?" may tap an informant's cognition about the periodicity of rainfall, and if that is what the researcher intends to get at, then it is a valid

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