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LECTURE

Deconstructing anthropology

First Annual Stephen F. Gudeman Lecture

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This lecture makes a start at deconstructing some of anthropology's most venerable avatars. Classical theories invoked a certain kind of person as the subject of anthropology. He was the savage, the tribal, the indigenous. More recently he became simply The Other. Always, he was our mirror opposite, ourselves turned upside down in a fairground mirror. And the theories that tried to explain this imaginary actor recycled a recurrent set of ideas and arguments about nature and culture, and savagery and civilization.

If we are to return to the real world we must free our thinking of these imaginary dichotomies, and set aside the repetitive cycle of mythical transformations that they support. Begin with the recognition that we are very like the people we study. Then construct a cosmopolitan anthropology that will confront current theories, models and methods with the experience and the understanding of the people we live with as ethnographers.

Keywords: History and theory, culture, social evolution, cosmopolitan anthropology

I

Steve and I met in October, 1962, at King's College, Cambridge. We were both studying anthropology, we were much the same age, in our early twenties, and we were both foreigners in what was then a very English university town. Meyer Fortes and Edmund Leach were fellows of King's College, characterized by Leach as "a bastion of British upper-class values of the most archaic kind."[1]

One drizzly autumn evening, Steve and I went for a beer at The Eagle. There is now a blue plaque on the wall of the pub: "It was here on February 28th 1953 that Francis Crick and James Watson first announced their discovery of how DNA carried genetic information." Watson actually said that they had discovered "the secret of life." Whatever. Crick and Watson were regulars at The Eagle. They might well have been there that very evening. But we were busy. We had begun to talk about anthropology. And anthropologists. We have kept talking ever since. I see this evening's talk as a continuation of our conversations, but with the great advantage that Steve will have to sit quietly and listen.

Back then, the small world of Cambridge social anthropology was divided between two feuding parties. One was led by Fortes, the other by Leach. Steve had weekly supervisions with Leach, and was one of his favorite students. As a fledgling Africanist, I had been recruited by Fortes.

Our two leaders were having terrific public rows. The issue was nothing less than human nature. Fortes believed that people—or at any rate, the sort of people anthropologists were supposed to study, "tribal" folk—are brainwashed by rituals and kept in line by paternal authority, backed up by the ancestors. Their social structures are perpetual motion equilibrium machines. Groupthink is compulsory.[2]

Leach had rubbished all that in a scattershot manifesto, *Rethinking anthropology*, and a combative monograph, *Pul Eliya*. Both books appeared in 1961, the year before Steve and I started talking in The Eagle. The key premise of Leach's polemics was that everybody, every-

1. Leach 1984: 11.

2. Kuper 2016.

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where, is out for Number One. Competition is endemic, authority contested, social arrangements racked by internal contradictions. Rules are ambiguous and up for negotiation. Rituals are "play-acting and pretense."[3]

The Fortes/Leach stand-off was in part a clash of personalities. Differences in social background were not irrelevant. Nevertheless, the arguments between the two men also had a lot to do with the great divide in British social anthropology, between the party of Radcliffe-Brown and the party of Malinowski. Back in the 1920s, our founding fathers had agreed that the old arguments between evolutionists and diffusionists were beside the point. Anthropologists should abandon historical reconstructions and study how societies worked. It was soon apparent, however, that Radcliffe-Brown and Malinowski had very different ideas about what they called primitive societies, and, especially, about how they worked.

Radcliffe-Brown was a disciple of Émile Durkheim. He believed that the orderly functioning of primitive societies was sustained by ritual performances of solidarity. Authority was sacred. Dissent was not only treason, it was blasphemy. According to Malinowski, however, the people of the Trobriand Islands interpreted myths and rituals to suit themselves, stretched the rules, gamed the system. "Whenever the native can evade his obligations without the loss of prestige, or without the prospective loss of gain, he does so, exactly as a civilised businessman would do."[4]

The two men also had different ideas about science. Radcliffe-Brown was a positivist. Malinowski was a neopositivist, with sophisticated concerns about the role of the observer. Radcliffe-Brown thought that scientific research should proceed from observation to comparison and then finally to generalization. Malinowski liked to generalize from the Trobriand Islanders to "savages" everywhere, and indeed to people anywhere.

These two parties had confronted one another for a generation, but by the time that Steve and I became regulars at The Eagle, they were both falling apart. Edward Evans-Pritchard, once Radcliffe-Brown's trusted lieutenant, had converted to Catholicism and given up on social science. In fact, he turned a complete somersault. He had been a positivist, like Radcliffe-Brown, but in a public lecture at Oxford in 1950 he declared "that social anthropology is a kind of historiography, and therefore ultimately of philosophy or art, . . . that it studies societies as moral systems and not as natural systems, that it is interested in design rather than in process, and that it therefore seeks patterns and not scientific laws, and interprets rather than explains."[5]

I read Evans-Pritchard's lecture and tentatively suggested to Fortes that I might want to introduce some history into my ethnography. He told me that I had to choose. I could be either an anthropologist or a historian. I should have known better. Fortes remained a Radcliffe-Brown loyalist. When, a decade later, a critical article I wrote on Radcliffe-Brown was accepted for publication by *Man*, Fortes asked me to withdraw it.

But if the party of Radcliffe-Brown was in crisis, all was far from well in the Malinowski camp. Leach, the most brilliant of the Malinowskians, was even at war with himself. In the 1950s he became possessed by the ideas of Claude Lévi-Strauss. However, Malinowski and Lévi-Strauss had very different ideas about human nature. Lévi-Strauss was a neo-Kantian philosopher. He represented the Amazonian Indians as idealist philosophers who lived out their lives in strict accordance with their beliefs. A shaman might start off a phony, but he would come to believe his own shtick.[6] Malinowski saw the Pacific Islanders as realists, cynics and schemers. When they preached, they were trying to pull the wool over your eyes. The anthropologist had to ignore the spin and work out what the natives were really up to.

Leach's masterpiece, *Political systems of Highland Burma*, was, he wrote, "organised as a kind of dialogue between the empiricism of Malinowski and the rationalism of Lévi-Strauss."[7] He gave Malinowski the best lines, but, he confessed, "I feel that sometimes I am on both sides of the fence."[8] He would say that he was a functionalist on weekdays, and a structuralist on Sundays.

We research students enjoyed these polemics. As a sort of salute to those early days, I thought that I might have a go this evening at deconstructing some of anthropology's most venerable avatars.

My argument is that most anthropological controversies belong in a museum of antique ideas. To put it another way, the grand theories have a lot in common with myths. The same ideas, the same arguments, turn up again and again, but each time in a new fancy dress.

3. Leach 1961: 298.

4. Malinowski 1926: 30.

5. Evans-Pritchard [1950] 1962: 26.

6. Lévi-Strauss 1963: 167–85.

7. Leach 1982: 44.

8. Tambiah 2002: 40.

Lévi-Strauss argued that a myth is best understood as a transformation of other myths. Each myth stands another myth on its head, inverts story lines, draws alternative morals.[9] In much the same way, anthropology's theories and paradigms confront each other in a hall of mirrors.

Fortes once told me that Leach had the public schoolboy idea that just by turning a proposition upside down he was being original. When I interviewed Leach for *Current Anthropology*, towards the end of his life, he put it this way:

> the sequence is always dialectical. There was . . . 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 humanities 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.[10]

It is not only anthropologists who hark back obsessively to old mentors and dead theorists. John Maynard Keynes famously remarked that "Practical men who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back."[11] In his *Conversations in Colombia*, Steve reported that Panamanian peasant farmers were in thrall to the economic theory of the eighteenth-century French Physiocrats. Very like those Colombian peasants, anthropologists in the twenty-first century struggle to break free from intellectual paradigms that date back two hundred years. So I will make a historical argument. This is all the more appropriate since, by my reckoning, Steve and I have practiced anthropology for one quarter of the history of our discipline. And as William Faulkner wrote, "The past is never dead. It's not even past."[12]

9. Descola 2016.

10. Kuper 1986: 380.

11. Keynes 1936: 383–84.

12. Faulkner 1951: 73 (Act 1, Scene 3).

II

Steve worked his way through the central preoccupations of Cambridge anthropology, which had to do with kinship and the family. He wrote a structural analysis of *compadrazgo*, in the style of Leach and Lévi-Strauss, which won him the Curle Essay Prize of the Royal Anthropological Institute.[13] His Cambridge doctoral dissertation analyzed the family and household in rural Panama.[14] Here, it seems to me, the influence of Meyer Fortes is apparent. Fortes argued that our primal emotions are forged and our ethics learned in what he termed "the domestic domain." He contrasted this domestic domain with the "politico-jural" domain, the realm of outside agencies that impose often uncomfortable controls and obligations on families and households. These two domains were never perfectly aligned, but together they ordered life choices.[15] This model would feed into Steve's conception of what he called the "house economy," or "the base," which operates everywhere in tension with the economy of the market.

We took the kinship stuff seriously, but even rookies like ourselves could see that those Cambridge debates were narrow and parochial. Nobody was interested in the history of the discipline. (After all, those old ideas had been exploded. Surely?) We were supposed to focus on the work of a few, select, mainly British anthropologists, plus Durkheim, Mauss, and perhaps Lévi-Strauss. We were not encouraged to read any American anthropologists later than Lewis Henry Morgan, who had died in 1881. (And Morgan himself was of interest only because he was credited with the invention of kinship theory. Nobody bothered to tell us that his version of social evolutionism had been endorsed by Marx and Engels, and became the orthodoxy of Soviet and Chinese anthropology.[16])

Steve had broader horizons. As an undergraduate at Harvard, he had studied under Evan Vogt and Clyde Kluckhohn. In a recent interview, he confessed that throughout his career he has been torn between European social anthropology and American cultural anthropology.[17]

13. Gudeman 1971.

14. Gudeman 1976.

15. Kuper 2016.

16. Kan and Arzyutov 2016.

17. Interviewed by Alan MacFarlane in 2010. http://www.alanmacfarlane.com/ancestors/gudeman.htm

But our teachers at Cambridge knew little about American anthropology, and they cared less. They seldom travelled to AAA meetings. The reason was, apparently, quite simple. British and American anthropologists were talking about different things. American anthropology was about "culture." The British treated culture as an epiphenomenon. Social relations were what really mattered. We were reminded that in Europe the widow wears black, the bride wears white. In China, traditionally, white was for funerals, red was for weddings. But whatever color their dresses might be, brides were brides, and widows were widows. As Leach put it: "Culture provides the form, the 'dress' of the social situation . . . The same kind of structural relationship may exist in many different cultures and be symbolised in correspondingly different ways."[18]

Raymond Firth, the dean of the small community of British social anthropologists, pointed out that there were nevertheless some proper social anthropologists in the USA. He suggested that Fred Eggan—who had, after all, been Radcliffe-Brown's student at Chicago—should be invited to bring over a few of their up-and-coming young men for an Anglo-American summit. This came to pass at Jesus College, Cambridge, in June 1963.[19] Steve and I were dispatched to the Cambridge railway station to pick up two of the young American visitors, Marshall Sahlins and Eric Wolf. (They cracked up when we told the cabby to take us to Jesus.) The American delegation—or, as Eric Wolf remarked, the delegation from the University of Chicago—also included Clifford Geertz and David Schneider. On the whole, everyone was being diplomatic, but Schneider caused a fuss by dismissing the whole field of kinship studies, which was the ark of the covenant of British social anthropology. (Schneider recalled, "it was a good chance for me to essentially say, 'Fuck off! I've had it with that stuff.' And that was good."[20])

III

So what was happening in American cultural anthropology? This had become a large and complex enterprise after World War II, certainly as compared to British or French social anthropology. It was also more diverse intellectually, and it cast its nets more widely. Nevertheless, throughout the twentieth century the field was riven by a feud between two parties, the social evolutionists and the cultural relativists.

In the late nineteenth century, the key institutions of American anthropology were to be found at the Smithsonian Institution in Washington, DC. These were the Bureau of American Ethnology and the Ethnology Department of the Museum of Natural History. Both were directed by social evolutionists, influenced by the theories of Lewis Henry Morgan. University departments of anthropology came later. The most important was established by a German immigrant, Franz Boas, who founded a graduate school of anthropology at Columbia University in 1899. Boas had been trained in the Berlin school of ethnology, under Adolf Bastian. The Berlin school had no time for the social evolutionism that was favored by the Smithsonian people. Their focus was on regional cultural histories, migrations, and the diffusion of ideas and techniques.

Boas's department at Columbia University was virtually a branch of the Berlin school. In due course, his protégés took the Berlin doctrines to new anthropology departments in Chicago, Philadelphia and Berkeley. One key idea was that race, language, and culture vary independently. Another was that cultures, or civilizations, are loosely organized composites, not integrated wholes, and that they are open to the world rather than closed in on themselves. Boas's faithful interpreter, Robert Lowie, summed up this doctrine in two slogans. Cultures "develop mainly through the borrowings due to chance contact." Consequently, a civilization is a "planless hodgepodge . . . a thing of shreds and patches."[21] It would obviously be absurd to suppose that all those "planless hodgepodges" follow the same historical trajectory, or that such "a thing of shreds and patches" forms an organic unity, as romantic nationalists liked to suppose.

Boas was also a relativist. In an early confrontation with the Smithsonian people, he insisted that "civilization is not something absolute, but . . . is relative, and . . .

18. Leach 1954: 16–17.

19. This was in fact the annual meeting of the Association of Social Anthropologists of Britain and the Commonwealth, usually a small event at that time, with fewer than a hundred participants. The joint meeting led to the publication of the first four ASA Monographs, which published most of the papers given at the meeting.

20. Schneider 1995: chapter 7.

21. Lowie 1920: 440–41.

our ideas and conceptions are true only so far as our civilization goes."[22] Perhaps above all, however, he was an empiricist. He once told a graduate student that "there are two kinds of people: those who have to have general conceptions into which to fit the facts; those who find the facts sufficient. I belong to the latter category."[23]

In practice, Boas was most at ease when wielding facts to demolish theories. Big ideas about race and culture were demonstrably false, or, at best, premature. The Boasians particularly enjoyed picking to pieces the grand narratives of the social evolutionists. However, all that nit-picking could be dispiriting. Roman Jakobson suggested that if Boas had been charged to tell the world about the epoch-making voyage of Christopher Columbus, he would have said: the hypothesis that there is a shorter sea-route to India has been disproved.[24] Alfred Kroeber, an old-school Boasian, put his finger on the problem: "As long as we continue offering the world only reconstructions of specific detail, and consistently show a negativistic attitude towards broader conclusions, the world will find very little of profit in ethnology. People do want to know why."[25]

Kroeber himself came up with a theory of cultural patterns, and what he called configurations of culture growth.[26] These ideas did not catch on. Edward Sapir, Ruth Benedict and Margaret Mead, second-generation Boasians, felt that it was time to change course. Like the functionalists in Britain, they were not very interested in history, but, instead of Durkheimian sociology, they went in for psychoanalytic ideas. And they embraced the romantic, organic view of culture, celebrated by Sapir in his essay, "Culture, genuine and spurious,"[27] and by Ruth Benedict in her *Patterns of culture*, which drew on Nietzsche and Spengler.[28] This encouraged excursions to the wilder shores of relativism. Sapir put it best: "No two languages are ever sufficiently similar to be considered as representing the same social reality. The worlds in which different societies live are distinct worlds, not the same world with different labels attached."[29]

Margaret Mead suggested that Sapir and Benedict were just bringing Boas up to date,[30] but Lowie, an old-school Boasian, would have none of that. He dismissed Sapir's ideas about culture as "beyond the sphere of science altogether."[31] Yet whatever the old guard thought about it, the second generation of Boasians embraced a full-blown cultural holism. And, inevitably, this provoked a reaction. And, predictably, the challenge to the culturalists came from the social evolutionist camp.

In fact, two neo-evolutionist schools of thought emerged in the 1950s. One, led by Leslie White, was inspired by Karl Marx. Societies progressed through stages: hunter-gatherers with their patrilineal bands; tribesmen with their clans and lineages; chiefdoms, with their hierarchies; and then, finally, states. The mode of production explained everything. There was also an ecological strain, that explained apparently bizarre customs, rituals, or taboos as unconscious but wonderfully effective ways of adapting to the environment.[32]

The other neo-evolutionist school followed Herbert Spencer rather than Marx. Its sympathies were with capitalism rather than communism, and with imperialism rather than the new Third World utopianism. Before World War II, American anthropology had been a small, insecure discipline, concerned almost exclusively with the Native American population. But in 1945 the United States emerged as the leading global power. The European and Japanese empires collapsed. Competing with the Soviet Union and Communist China to capture hearts and minds, the United States was now drawn, willy-nilly, into nation building.

Providentially, a theory of development was to hand. It was widely assumed that all the former colonies, now new states, were very similar to one another, and that they shared a common destiny. They probably would, certainly they should, repeat the evolution of the United States itself. They had already advanced from colony to

22. Boas 1887.
23. Cited by Kluckhohn and Prufer 1959: 22.
24. Jacobson 1944: 194.
25. Kroeber 1920: 380.
26. Kroeber 1944.
27. Sapir 1924.
28. Benedict 1934.
29. Sapir 1929 (1949): 162.
30. Mead 1972: 126.
31. Lowie 1965.
32. If anyone wants to recover this way of thinking, I recommend a reading of Marvin Harris's *Rise of anthropological theory: A history of theories of culture*, published in that revolutionary year, 1968.

republic. Now that they were free, they must become capitalists.

An economist, Walt Whitman Rostow, plotted the path to be followed in a book, published in 1960, entitled *The stages of economic growth. A non-Communist manifesto*. These stages of growth were oddly, even eerily, reminiscent of those described in the *Communist manifesto* itself. The first stage was "traditional society," the second "transitional society." Then came "take-off." (Rocket ship metaphors were much in vogue at the time. Sputnik and all that.) After a "mature stage" came the "age of high mass consumption." If the new states accepted Rostow's guidance, and put themselves on the side of history, they would become rich, and consume masses of stuff. Not only that, they would progress, develop, become civilized, or, in the new idiom, *modernize*.

And surely the time was ripe. In 1961, in a memo to President Kennedy, Rostow wrote that "barring a catastrophe, it is likely that a good many of the countries in the underdeveloped world will during the 1960s, either complete the take-off process or be very far advanced in it."[33] Kennedy was impressed. He brought Rostow into his administration and launched the Alliance for Progress to modernize Latin America.

Unfortunately, progress towards a civilized condition, or rather modernization, turned out to be unexpectedly difficult. There were obstructions in the path of history. The problem was soon identified. It was, of course, all down to culture. The new states were hobbled by ancient enmities, foolish traditions, and dysfunctional customs. The anthropologists, the culture mavens, were called in to account for the dead weight of tribalism, superstition, and conservatism. American anthropology went global, and the field began a wonderful phase of growth, even if the former colonies did not themselves enjoy the same good fortune.

But while there might be road-blocks, even detours on the way, the modernization narrative envisaged a happy end-point, even the end of history. All nations were bound to become liberal democracies. All economies would be capitalist powerhouses. By the early 1960s, however, the Vietnam War was casting a terrible shadow on those optimistic scenarios. In 1966, Rostow became the national security advisor to Lyndon Johnson. He doubled down on the promises of his "*non-Communist manifesto*." And he was a hawk on Vietnam. But opposition to the war grew. American campuses were in turmoil. Anthropologists felt that they were on the front line. One response was to embrace a Marxist version of evolutionism and look forward to the end of capitalism and imperialism. An alternative reaction—equally venerable, perhaps less likely to damage career prospects—was to reject the narrative of modernization. There was a revival of cultural romanticism. Formerly left-wing intellectuals now talked up the importance of identity and difference. By the 1970s, it was Herder and Nietzsche and Spengler all over again.

Consider the intellectual trajectories of Clifford Geertz and Marshall Sahlins, two of the most influential American anthropologists of the post-World War II generation. In 1952, Geertz and his wife, graduate students in anthropology at Harvard, were recruited to join an interdisciplinary team that was being assembled by a new Center for International Studies at MIT. The Center was directed by the CIA's former Director of Economic Research, Max Milliken. Walt Rostow was a key member. Clifford and Hildred Geertz and their colleagues were dispatched to Java. Their mission was to identify the conditions for "take-off" in Indonesia.

Geertz's initial assessment was optimistic. "Indonesia is now, by all the signs and portents, in the midst of such a pre-take-off period," he wrote, and he claimed to see "the beginnings of a fundamental transformation in social values and institutions toward patterns we generally associate with a developed economy."[34] His first publications looked forward to the triumph of a nationalist ideology that would transcend religious divisions, and to the mobilization of indigenous elites who would manage the imminent economic "take-off." But things did not go to plan in Indonesia. By the time Geertz came to that Anglo-American conference in Cambridge in 1963, he was changing course, giving up on modernization and becoming a culturalist. His Cambridge presentation, later a canonical text of the new movement, was "Religion as a cultural system." From this point on he would insist that anthropology should be not a social science but rather a kind of hermeneutics, its sole agenda "the interpretation of cultures." And culture was now redefined, or, as Geertz put it, cut down to size.[35] A text, or perhaps a discourse, culture represented "an ordered system of meanings and symbols."[36]

33. Latham 1998: 199.

34. Geertz 1963: 1.

35. Geertz 1973: 4.

36. Geertz 1973: 245.

The young Marshall Sahlins was the coming man in the neo-evolutionist school.[37] His paper at that 1963 Cambridge conference, "On the sociology of primitive exchange," became a classic of social evolutionism. But then, in 1968, he spent a sabbatical year in Paris. This was a year of student uproar, a time of surrealist slogans and passionate, unruly teach-ins. Many young people were converting to Marxism. Sahlins himself had been a Marxist for years. Now he turned a somersault, abandoned dialectical materialism, and embraced Lévi-Strauss's structuralism. He produced structuralist interpretations of Hawaiian myths. More recently, he has become a cultural determinist and a convert to David Schneider's ideas about kinship.[38]

In 1969, a newly minted Cambridge PhD, Steve moved back to the US, to the University of Minnesota. He was a good decade younger than Geertz and Sahlins, but he was undergoing a similar intellectual evolution. Like Geertz, he had started out with a modernization project. His first field studies in rural Panama, sponsored by the Harvard Business School, were funded by the Agency for International Development. This agency had been set up in the State Department by President Kennedy, advised by Rostow. Its mission was to promote modernization. Rostow personally "first cleared the project" that sent Steve to Panama.[39]

The government of Panama had set the country on a forced march to economic modernization. It promoted peasant cultivation of sugar cane for the export market. Later the authorities took over blocks of land and hired local farmers as laborers. And then, influenced by a Harvard Business School report that Steve helped to produce, the dictator of Panama, General Torrijos, built sugar mills. However, there were hidden costs. Sugar cane displaced food crops. Subsistence farmers had to use their meager cash earnings to buy household essentials. Sugar cultivation impoverished the soil.

Steve found there was no way that an ethnographer could make the planners in Panama City realize what was happening on the ground. They refused even to visit the villages. "They were working at the market end," Steve observes. "No language or concepts connected us."[40] For the planners, it was an article of faith that the customs and institutions of the old economy constituted a barrier to modernization. Steve recalls that "modernization theories . . . offered a solution to the awkward presence of house economies. They are traditional systems. With capital investment, technical education . . . , the proper incentives, and the improvement of infrastructure . . . . House economies will disappear. Rural inhabitants will join market life."[41]

But then, as the contradictions in the modernization policy became apparent, the sugar price crashed. The mills closed. In his monograph, *The demise of a rural economy*, Steve described the sad aftermath: "[Torrijos'] modernist project led to the end of the house economy in the village where I had lived."[42]

IV

Disillusioned now with modernization theory, Steve took a culturalist turn, like Geertz and Sahlins. His *Economics as culture*, subtitled *Models and metaphors of livelihood*, presented a range of vernacular conceptions of the economy.[43] I think, though, that this exercise left him dissatisfied. He now moved on to a more complex synthesis, drawing on the central debate in American economic anthropology. Here, too, a bitter feud was raging, between the Formalists and the Substantivists.[44] This divide went back to Karl Polanyi's legendary seminars at Columbia University in the 1940s.

An original thinker in the Marxist tradition, Polanyi contrasted two stages of economic organization.[45] Premodern economies produce goods in family units. Between equals, goods and services were exchanged as gifts. Chiefs demanded tribute payments and they corralled their followers to do public works. Then, following what Polanyi called "the great transformation," modern market economies emerged. Marshall Sahlins attended Polanyi's seminars, and he developed case studies of economies without markets, which were collected in his *Stone age economics* (1972).

"Substantivists" like Sahlins thought that to understand a "stone age economy" it was necessary to decode

37. Sahlins 1960.

38. Kuper 2018.

39. Gudeman 2016: 168.

40. Gudeman 2016: 170.

41. Gudeman 2016: 124.

42. Gudeman 1978.

43. Gudeman 1986.

44. To recapture the arguments of the day, see LeClair and Schneider 1968.

45. Polanyi 1944.

exotic cosmologies, and to unravel complicated systems of kinship and marriage. "Formalists" countered that rational choice and the constraints of supply and demand must operate even in non-market economies. Polanyi himself later came to the view that the differences between market and non-market systems were not absolute.[46] After all, Marcel Mauss had remarked that the economy of mutuality endures, even flourishes, in capitalist societies.[47]

Steve developed a more radical position. And this is where his conception of what he called the "house economy" comes in. Everywhere, the house economy is associated with the ideal of family solidarity. It "aims for sufficiency and nurtures social relationships," Steve writes. In contrast, markets "are made up of separate actors focused on gain."[48] Nevertheless, these two very different economies normally have to operate side by side. "One is the high-relationship economy that is rooted in the house . . . Neglected by economic theory, it is prominent in small-scale economies, and hidden and mystified yet salient in capitalism. The other side consists of competitive trading. Anthropologists know one side of economy and economists know the other, but the two are intertwined."[49]

The crucial point is that the house economy is not a relic of a pre-market age. It can and does operate alongside the market. That was the case in the Trobriand Islands a century ago. That is true in Main Street, USA, to this day. The Trobrianders had both the *kula* gift exchange and the *gimwali*, which was a tough-minded system of bartering. Europe and the USA have the Christmas *kula*, the wedding *potlatch*, and that peculiar hybrid, the family business. An economics that deals only with the market will leave out a crucial dimension of the economic experience of most people, most of the time, wherever they may be.

Yet although the market and house economies cohabit, they are not necessarily, or even usually, happily married. More typically, they are locked in an uneasy partnership. Obliged to live together, they do their best to keep out of each other's way. Nobody should confuse being a wheeler-dealer in the market with taking part in a gift exchange. In the Trobriand Islands, it is an insult to say that a man deals with a *kula* exchange as though it was trade, *gimwali*. Everywhere it is illegitimate to try to profit at the expense of relatives, friends or guests. On the other hand, a gift given at the wrong time, or in the wrong context, may be denounced as a bribe. And when people shop for Christmas gifts, they worry about the "commercialization" of Christmas, which should be spent in the bosom of the family, on a holiday from the market economy.

Yet, however strictly the boundaries between the two economies are policed, and despite the chronic tensions between their values and strategies, the house and the market must somehow work together. The economy of the house provides necessary back-up for the market economy. Conversely, Steve points out, in "the competitive search for profit, market economies can undermine themselves by destroying their base of purchasing power in the house."[50] Steve concludes that the rebalancing of market and house economies is a constant, existential issue. The fateful error of the planners in Panama was to assume that the house economy represented the past, the market economy the future.

V

Looking back at American anthropology as it had been in the sixties, Sherry Ortner recalled that there was, in the end, a stand-off between the two mainstream parties, the cultural relativists and the social evolutionists, branded, back then, as "symbolic anthropology" and "cultural ecology." Ortner found that each was "unable to handle what the other side did (the symbolic anthropologists in renouncing all claims to 'explanation,' the cultural ecologists in losing sight of the frames of meaning within which human action takes place)." Moreover, "both were also weak in what neither of them did, which was much of any systematic sociology."[51]

Ortner hoped that there would be a turn to sociology, specifically to Pierre Bourdieu's sociology of practice. In the event, however, both the evolutionists and the culturalists doubled down. What followed can perhaps only be explained by an appeal to Gregory Bateson's conception of schismogenesis (Bateson 1935, 1936), a process by which confrontations drive the protagonists to adopt more and more extreme positions.

46. Polanyi 1957: 256.

47. Mauss [1925] 1990: 98.

48. Gudeman 2016: 2–3. Cf. Gudeman 2008.

49. Gudeman 2016: 2.

50. Gudeman 2016: 167.

51. Ortner 1984: 134.

The social evolutionists took up sociobiology. Their inspiration was the discovery of the double helix structure of DNA by Crick and Watson. The human genome was being mapped. Medicine would be revolutionized. Social science could become truly scientific at last.

Ironically enough, James Watson himself had a low opinion of his Harvard colleague, E. O. Wilson, the leading light of the new movement. Nor did he share Wilson's faith in genetic determinism. Watson told an interviewer that he and his wife used to debate the cause of their own son's mental illness. "She said it was heredity; I said it was the environment . . . I don't really know now."[52] But Wilson had no doubt that practically everything we do is determined, unconsciously, by genetic programs. Honed by eons of evolution, our instincts, habits, and customs are geared to survival and reproduction. We are all still essentially hunter-gatherers, if not animals, or even birds and bees. Wilson was actually an entomologist himself, but he came up with a theory of the human condition. "The real problem of humanity is the following," he pronounced, "we have paleolithic emotions; medieval institutions; and god-like technology. And it is terrifically dangerous, and it is now approaching a point of crisis overall."[53]

From the culturalist camp, Sahlins delivered an abrasive critique of sociobiology.[54] For their part, Geertz and Schneider turned their backs on social science and on biology. They were now philosophical idealists, concerned only with the interpretation of symbolic discourses. So there we were, divided, once again, between two views of human nature. The old mind/body dichotomy had come back to haunt the anthropologists. The sociobiologists saw us as animals. To the culturalists, we were spiritual beings, living in a world of our own imagining.

And now the process of schismogenesis went into overdrive. Soon even Geertz, even Sahlins, were left behind. Geertz's program was too tame for the post-Vietnam War generation. There was a whiff of Western arrogance about it. How could Geertz pretend to read the minds of Javanese? And which Javanese, precisely? And in any case, meaning was not that easy to pin down. The young guns had read Derrida. Interpretation was out, deconstruction was in. In 1986 James Clifford and George Marcus edited a collective manifesto, *Writing culture: The poetics and politics of ethnography*.[55] Branded, somewhat misleadingly, "post-modernism," their ultra-relativism became the next big idea in American cultural anthropology.

I thought that the "postmodernists" were as wrong-headed, in their own way, as the sociobiologists. A year or so after *Writing culture* appeared, I visited the ethnology section of the Academy of Sciences in Moscow. The director invited me to talk about current trends in Western anthropology. I launched into a critique of post-modernism and was just getting into my stride when I realized that nobody had any idea what I was talking about. So I backtracked, summarized the thesis of *Writing culture*, and then denounced it. In the discussion that followed, it became clear that I had converted the audience to postmodernism. A few months later I was visiting the ethnology department at one of the most right-wing Afrikaans-language universities in South Africa. They asked me to talk about new developments in theory. I duly made my case against postmodernism. They didn't know what I was going on about. I backtracked, summarized the theory, and then demolished it. And I converted the whole audience to postmodernism.

On reflection, this was not altogether surprising. In both Moscow and Bloemfontein, I had landed among colleagues who were experiencing wrenching ideological challenges. In Moscow they had all been social evolutionists. In Bloemfontein they had all been cultural determinists. Now the Russian anthropologists were living through the death throes of the Soviet system and the implosion of the Marxist theory of history. The South Africans were witnessing the last rites of Apartheid, and the end of state-fostered cultural determinism. The Russian and Afrikaner anthropologists were therefore delighted to discover that all theory is nothing but ideology. It wasn't just them. *Everyone* had been deluded.

Why, then, did the postmodernist message appeal to a new generation of American anthropologists? I speak tentatively here, as an ethnographer must when talking to the natives, but my sense is that an extreme cultural relativism tied in with broader ideological tendencies, and in particular, with identity politics and a pervasive suspicion of science. Any talk about truth was taken as a sign of naiveté. Presented with a vexing objection to one's views, the key question to ask was not, is that true, but rather: *Where is he coming from?*

52. Thompson 1989.

53. "An intellectual entente," *Harvard Magazine*, October 9, 2009.

54. Sahlins 1976.

55. Clifford and Marcus 1986.

VI

Bateson warned that a process of schismogenesis is liable to end in a crash. There certainly were a lot of crashes, and some American anthropology departments were broken into pieces. But, of course, most anthropologists in the USA and Europe were skeptical of these intellectual cargo cults. They thought it was possible, and worthwhile, to try to understand, at first hand, how other people were getting on with their lives.

Steve rejected the postmodernist conceit that ethnographies are fabrications, to be read only in order to uncover their dishonest rhetorical trickery. He knew very well that ethnographers, like immigrants, can find out how things work in another society. But he recognized that ethnographies could do with more openness and reflexivity. In 1984, he began a new field study, in Colombia, together with Alberto Rivera, a Colombian who had studied with him at the University of Minnesota. They travelled about the Andean countryside, interviewing peasant farmers, and, as they drove from one village to another, they discussed what they were learning from their interviews. Gradually they drew their informants into these conversations. At some stage they began to recognize parallels between the largely implicit economic assumptions of the *campesinos* and the theories of the French Physiocrats, pioneer economists of the 1760s and 1770s. The Physiocrats themselves had drawn on contemporary European folk ideas about economics, ideas that Spanish peasant immigrants brought to Colombia. And so the scope of the conversations broadened once more, to encompass echoes of earlier conversations, in other places. The end product was the monograph *Conversations in Colombia*, which appeared in 1990.[56]

Steve and Alberto Rivera were doing their best to understand the ways in which the Andean *campesinos* made sense of things in their own terms. However, Steve was no longer content with the idealist perspective that he had adopted in his *Economics as culture*. Folk models help people to think about the world, and they may sometimes guide action, but they do not by themselves account for the ways in which families choose to earn and save and spend. There is analytical, even theoretical, work to be done in order to explain those real-world choices.

The model of fieldwork as a conversation is a potent counter to the postmodernist fantasy of what the ethnographer does. It is relevant, also, for theoretical work. My deepest concern about the state of anthropology is that there are too few urgent conversations about ideas. But luckily for me, Steve has been coming to Europe for longish stays, mostly at the Max Planck Institute for Social Anthropology in Halle, where he and Chris Hann, another ex-Cambridge anthropologist, launched a project on economy and society. Steve and I set up workshops at meetings of the European Association of Social Anthropologists, drawing in young colleagues. And we picked up on our own long-running conversation.

VII

So where do we stand now? Steve may insist that I speak only for myself. Well then, speaking for myself, my hope is for an anthropology that is realist, cosmopolitan, and inter-disciplinary. To get there, we must be clear about what—and who—we are studying, and why, and, of course, how.

Anthropology started out as the science of the savage, or the primitive. This mythical creature was a shape shifter. (Almost always, however, he was a man.) For Rousseau—and for his disciple, Lévi-Strauss—he was the last free man, at one with nature and yet wonderfully attuned to the spirit world. For Lévy-Bruhl, he was prelogical. In Freudian fantasy, he was polymorphically promiscuous. Malinowski and Mauss and Sahlins—and, sometimes, Gudeman—represented him as the polar opposite of Economic Man.

Anthropologists would now be embarrassed to talk about primitive peoples, or stone age societies (though the new perspectivists share Lévi-Strauss's romantic ideas about the Neolithic). A generation ago, with decolonization and "modernization," there was a move to rebrand anthropology as the science of the Other. As it turned out, however, this Other was still our opposite number, our alter-ego, our own image turned upside down in a fairground mirror.

A more realistic starting point would be the recognition that we are all the same kind of person, though differently situated. According to Bruno Latour, *We have never been modern*.[57] I am not so sure about that. At least since Columbus, perhaps even since Marco Polo, *everyone* is modern. It is also true that most of us are also

56. Gudeman with Rivera 1990.

57. Latour 1991. For an excellent discussion of Latour's ideas, see a special number of the journal *Social Anthropology* (Legrain and van de Port 2013).

rather traditional. Rational enough, at least much of the time, yet susceptible to mystification. Increasingly interconnected, and yet at once local and global, homebodies and traders, dreamers and schemers, agents and patients. The fact is that we ourselves are very like the people we study, although we may operate with different tools, and in other circumstances.

This is not a new idea. On the eve of World War II, toward the end of his short life, Edward Sapir distanced himself from his early relativism. Discussing Ruth Benedict's *Patterns of culture*, he told his class at Yale: "I suspect that individual Dobu and Kwakiutl are very like ourselves; they just are manipulating a different set of patterns . . . You have to know the individual before you know what the baggage of his culture means to him."[58] At virtually the same time, Malinowski remarked that when he started out as an anthropologist, in the early twentieth century, the emphasis had been on the differences between peoples. "I recognised their study as important," he wrote, in a scribbled draft for a never-to-be written textbook on anthropology, "but underlying sameness I thought of greater importance & rather neglected."[59]

Our informants may tell tall stories about animals with human characters, spirits with human passions, virgin births, magic rings, angels and demons; and yet they will behave most of the time very much as you or I would behave, if we were dealt the same hands, and confronted with the same options. So if we want to understand those realistic, pragmatic and cosmopolitan people, our contemporaries, we need a realistic, pragmatic, cosmopolitan anthropology.

And a cosmopolitan and realistic anthropology is needed out there in the world. Very nearly all social science research funding goes to the study of the inhabitants of North America and the European Union. Ninety-six per cent of the subjects of studies reported in the leading American psychology journals are drawn from Western industrial societies.[60] Mainstream economics journals publish more papers dealing with the United States than with Europe, Asia, Latin America, the Middle East and Africa combined, according to a report in the *Economist*. And, the report noted, economics is very largely a science of the rich: "The world's poorest countries are effectively ignored by the profession."[61] A cosmopolitan anthropology will test established theories, models, and methods in different conditions, and it will confront these models and methods with the experience and the understanding of the people we live with as ethnographers.

What, then, about our theories? For two centuries, cultural anthropologists were either social evolutionists or cultural determinists. The evolutionists tried to arrange all societies into a series from primitive to civilized. The culturalists imagined a world made up of unique, local forms of life. Elsewhere, in their own echo chamber, the social anthropologists took their ideas from the social sciences, though all too often from yesterday's theorists. And then, on the other side of a more and more impenetrable boundary wall, physical anthropologists huddled. They pushed biological explanations, but their paradigms changed every decade. First, everything was determined by race; then by cranial capacity; then by animal instinct; then by kin selection; then by genes for this and that. Perhaps we are all the unreconstructed descendants of hunter-gatherers. Recently we were told that it all comes down to synapses in the brain. Only one part of the doctrine has remained constant: the claim that biology trumps culture.

But it may not be necessary to start from a fully-fledged theoretical position. It is sometimes a good idea to begin with a question rather than an answer: a question of fact, or a puzzle to be solved, or a problem to be sorted out. "What, in heaven's name, are we trying to find out?"[62] as Edmund Leach demanded back in 1962, at the very moment that Steve and I were beginning to talk about anthropology.

Once a question is posed and a tentative answer put forward, a conversation should follow: a frank, egalitarian and open-ended conversation, a sort of ideal seminar. Critics, outsiders—even other kinds of anthropologists—should be invited to join in.[63] It will quickly become apparent what sort of evidence may be relevant here, and what kinds of arguments are on offer. With luck, robust and testable hypotheses will be hammered out. Perhaps the questions will be recast. In any case, the conversation moves on.

58. Irvine 1994: 183.

59. Cited by Young 2004: 76.

60. Arnett 2008.

61. "The useful science?," *Economist*, January 4, 2014.

62. Leach 1962: 131.

63. See Kuper and Marks 2011.

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