

Thick Description: Toward an Interpretive Theory of Culture

Clifford Geertz

I

In her book, *Philosophy in a New Key*, Susanne Langer remarks that certain ideas burst upon the intellectual landscape with a tremendous force. They resolve so many fundamental problems at once that they seem also to promise that they will resolve all fundamental problems, clarify all obscure issues. Everyone snaps them up as the open sesame of some new positive science, the conceptual center-point around which a comprehensive system of analysis can be built. The sudden vogue of such a *grande idée*, crowding out almost everything else for a while, is due, she says, “to the fact that all sensitive and active minds turn at once to exploiting it. We try it in every connection, for every purpose, experiment with possible stretches of its strict meaning, with generalizations and derivatives.”

After we have become familiar with the new idea, however, after it has become part of our general stock of theoretical concepts, our expectations are brought more into balance with its actual uses, and its excessive popularity is ended. A few zealots persist in the old key-to-the-universe view of it; but less driven thinkers settle down after a while to the problems the idea has really

Reprinted from Clifford Geertz, *The Interpretation of Cultures: Selected Essays* (New York: Basic, 1973), 3–30.

generated. They try to apply it and extend it where it applies and where it is capable of extension; and they desist truth, a seminal idea in the first place, a permanent and enduring part of our intellectual armory. But it no longer has the grandiose, all-promising scope, the infinite versatility of apparent application, it once had. The second law of thermodynamics, or the principle of natural selection, or the notion of unconscious motivation, or the organization of the means of production does not explain everything, not even everything human, but it still explains something; and our attention shifts to isolating just what that something is, to disentangling ourselves from a lot of pseudo-science to which, in the first flush of its celebrity, it has also given rise.

Whether or not this is, in fact, the way all centrally important scientific concepts develop, I don't know. But certainly this pattern fits the concept of culture, around which the whole discipline of anthropology arose, and whose domination that discipline has been increasingly concerned to limit, specify, focus, and contain. It is to this cutting of the culture concept down to size, therefore actually insuring its continued importance rather than undermining it, that the essays below are all, in their several ways and from their several directions, dedicated. They all argue, sometimes explicitly, more often merely through the particular analysis they develop, for a narrowed, specialized, and so I imagine, theoretically more powerful concept of culture to replace E. B. Tylor's famous "most complex whole," which, its originative power not denied, seems to me to have reached the point where it obscures a good deal more than it reveals.

The conceptual morass into which the Tylolean kind of *pot-au-feu* theorizing about culture can lead, is evident in what is still one of the better general introductions to anthropology, Clyde Kluckhohn's *Mirror for Man*. In some twenty-seven pages of his chapter on the concept, Kluckhohn managed to define culture in turn as: (1) "the total way of life of a people"; (2) "the social legacy the individual acquires from his group"; (3) "a way of thinking, feeling, and believing"; (4) "an abstraction from behavior"; (5) a theory on the part of the anthropologist about the way in which a group of people in fact behave; (6) a "store-house of pooled learning"; (7) "a set of standardized orientations to recurrent problems"; (8) "learned behavior"; (9) a mechanism for the normative regulation of behavior; (10) "a set of techniques for adjusting both to the external environment and to other men"; (11) "a precipitate of history"; and turning, perhaps in desperation, to similes, as a map, as a sieve, and

as a matrix. In the face of this sort of theoretical diffusion, even a somewhat constricted and not entirely standard concept of culture, which is at least internally coherent and, more important, which has a definable argument to make is (as, to be fair, Kluckhohn himself keenly realized) an improvement. Eclecticism is self-defeating not because there is only one direction in which it is useful to move, but because there are so many: it is necessary to choose.

The concept of culture I espouse, and whose utility the essays below attempt to demonstrate, is essentially a semiotic one. Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning. It is explication I am after, construing social expressions on their surface enigmatical. But this pronouncement, a doctrine in a clause, demands itself some explication.

II

Operationalism as a methodological dogma never made much sense so far as the social sciences are concerned, and except for a few rather too well-swept corners—Skinnerian behaviorism, intelligence testing, and so on—it is largely dead now. But it had, for all that, an important point to make, which, however we may feel about trying to define charisma or alienation in terms of operations, retains a certain force: if you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do.

In anthropology, or anyway social anthropology, what the practitioners do is ethnography. And it is in understanding what ethnography is, or more exactly *what doing ethnography is*, that a start can be made toward grasping what anthropological analysis amounts to as a form of knowledge. This, it must immediately be said, is not a matter of methods. From one point of view, that of the textbook, doing ethnography is establishing rapport, selecting informants, transcribing texts, taking genealogies, mapping fields, keeping a diary, and so on. But it is not these things, techniques and received procedures, that define the enterprise. What defines it is the kind of intellectual effort it is: an elaborate venture in, to borrow a notion from Gilbert Ryle, "thick description."

Ryle's discussion of "thick description" appears in two recent essays of his (now reprinted in the second volume of his *Collected Papers*) addressed to the

general question of what, as he puts it, "*Le Penseur*" is doing: "Thinking and Reflecting" and "The Thinking of Thoughts." Consider, he says, two boys rapidly contracting the eyelids of their right eyes. In one, this is an involuntary twitch; in the other, a conspiratorial signal to a friend. The two movements are, as movements, identical; from an I-am-a-camera, "phenomenalistic" observation of them alone, one could not tell which was twitch and which was wink, or indeed whether both or either was twitch or wink. Yet the difference, however unphotographable, between a twitch and a wink is vast; as anyone unfortunate enough to have had the first taken for the second knows. The winker is communicating, and indeed communicating in a quite precise and special way: (1) deliberately, (2) to someone in particular, (3) to impart a particular message, (4) according to a socially established code, and (5) without cognizance of the rest of the company. As Ryle points out, the winker has done two things, contracted his eyelids and winked, while the twitcher has done only one, contracted his eyelids. Contracting your eyelids on purpose when there exists a public code in which so doing counts as a conspiratorial signal is winking. That's all there is to it: a speck of behavior, a fleck of culture, and—*violà!*—a gesture.

That, however, is just the beginning. Suppose, he continues, there is a third boy, who, "to give malicious amusement to his cronies," parodies the first boy's wink, as amateurish, clumsy, obvious, and so on. He, of course, does this in the same way the second boy winked and the first twitched: by contracting his right eyelids. Only this boy is neither winking nor twitching, his is parodying someone else's, as he takes it, laughable, attempt at winking. Here, too, a socially established code exists (he will "wink" laboriously, over obviously, perhaps adding a grimace—the usual artifices of the clown); and so also does a message. Only now it is not conspiracy but ridicule that is in the air. If the others think he is actually winking, his whole project misfires as completely, though with somewhat different results, as if they think he is twitching. One can go further: uncertain of his mimicking abilities, the would-be satirist may practice at home before the mirror, in which case he is not twitching, winking, or parodying, but rehearsing; though so far as what a camera, a radical behaviorist, or a believer in protocol sentences would record he is just rapidly contracting his right eyelids like all the others. Complexities are possible, if not practically without end, at least logically so. The original winker might, for example, actually have been fake-winking, say, to mislead outsiders into imag-

ining there was a conspiracy afoot when there in fact was not, in which case our descriptions of what the parodist is parodying and the rehearser rehearsing of course shift accordingly. But the point is that between what Ryle calls the "thin description" of what the rehearser (parodist, winker, twitcher . . .) is doing ("rapidly contracting his right eyelids") and the "thick description" of what he is doing ("practicing a burlesque of a friend taking a wink to deceive an innocent into thinking a conspiracy is in motion") lies the object of ethnography: a stratified hierarchy of meaningful structures in terms of which twitches, winks, fake-winks, parodies, rehearsals of parodies are produced, perceived, and interpreted, and without which they would not (not even the zero-form twitches, which, as a *cultural category*, are as much nonwinks as winks are nontwatches) in fact exist, no matter what anyone did or didn't do with his eyelids.

Like so many of the little stories Oxford philosophers like to make up for themselves, all this winking, fake-winking, burlesque-fake-winking, rehearsed-burlesque-fake-winking, may seem a bit artificial. In way of adding a more empirical note, let me give, deliberately unprecedented by any prior explanatory comment at all, a not untypical excerpt from my own field journal to demonstrate that, however evened off for didactic purposes, Ryle's example presents an image only to exact of the sort of piled-up structures of inference and implication through which an ethnographer is continually trying to pick his way:

The French [the informant said] had only just arrived. They set up twenty or so small forts between here, the town, and the Marmusha area up in the middle of the mountains, placing them on promontories so they could survey the countryside. But for all this they couldn't guarantee safety, especially at night, so although the *mezrag*, trade-pact, system was supposed to be legally abolished it in fact continued as before.

One night, when Cohen (who speaks fluent Berber), was up there, at Marmusha, two other Jews who were traders to a neighboring tribe came by to purchase some goods from him. Some Berbers, from yet another neighboring tribe, tried to break into Cohen's place, but he fired his rifle in the air. (Traditionally, Jews were not allowed to carry weapons; but at this period things were so unsettled many did so anyway.) This attracted the attention of the French and the marauders fled.

The next night, however, they came back, one of them disguised as a woman who knocked on the door with some sort of a story. Cohen was suspicious and

didn't want to let "her" in, but the other Jews said, "oh, it's all right, it's only a woman." So they opened the door and the whole lot came pouring in. They killed the two visiting Jews, but Cohen managed to barricade himself in an adjoining room. He heard the robbers planning to burn him alive in the shop after they removed his goods, and so he opened the door and, laying about him wildly with a club, managed to escape through a window.

He went up to the fort, then, to have his wounds dressed, and complained to the local commandant, one Captain Dumari, saying he wanted his *'ar*—i.e., four or five times the value of the merchandise stolen from him. The robbers were from a tribe which had not yet submitted to French authority and were in open rebellion against it, and he wanted authorization to go with his *mezrag*-holder, the Marmusha tribal *sheikh*, to collect the indemnity that, under traditional rules, he had coming to him. Captain Dumari couldn't officially give him permission to do this, because of the French prohibition of the *mezrag* relationship, but he gave him verbal authorization, saying, "If you get killed, it's your problem."

So the *sheikh*, the Jew, and a small company of armed Marmushans went off ten or fifteen kilometers up into the rebellious area, where there were of course no French, and, sneaking up, captured the thief-tribe's shepherd and stole its herds. The other tribe soon came riding out on horses after them, armed with rifles and ready to attack. But when they saw who the "sheep thieves" were, they thought better of it and said, "all right, we'll talk." They couldn't really deny what had happened—that some of their men had robbed Cohen and killed the two visitors—and they weren't prepared to start the serious feud with the Marmusha a scuffle with the invading party would bring on. So the two groups talked, and talked, and talked, there on the plain amid the thousands of sheep, and decided finally on five-hundred-sheep damages. The two armed Berber groups then lined up on their horses at opposite ends of the plain, with the sheep herded between them, and Cohen, in his black gown, pillbox hat, and flapping slippers, went out alone among the sheep, picking out, one by one and at his own good speed, the best ones for his payment.

So Cohen got his sheep and drove them back to Marmusha. The French, up in their fort, heard them coming from some distance ("Ba, ba, ba" said Cohen, happily, recalling the image) and said, "What the hell is that?" And Cohen said, "That is my *'ar*." The French couldn't believe he had actually done what he said he had done, and accused him of being a spy for the rebellious Berbers, put him in prison, and took his sheep. In the town, his family, not having heard from him in so long a time, thought he was dead. But after a while the French released him

and he came back home, but without his sheep. He then went to the Colonel in the town, the Frenchman in charge of the whole region, to complain. But the Colonel said, "I can't do anything about the matter. It's not my problem."

Quoted raw, a note in a bottle, this passage conveys, as any similar one similarly presented would do, a fair sense of how much goes into ethnographic description of even the most elemental sort—how extraordinarily "thick" it is. In finished anthropological writings, including those collected here, this fact—that what we call our data are really our own constructions of other people's constructions of what they and their compatriots are up to—is obscured because most of what we need to comprehend a particular event, ritual, custom, idea, or whatever is insinuated as background information before the thing itself is directly examined. (Even to reveal that this little drama took place in the highlands of central Morocco in 1912—and was recounted therein 1968—is to determine much of our understanding of it.) There is nothing particularly wrong with this, and it is in any case inevitable. But it does lead to a view of anthropological research as rather more of an observational and rather less of an interpretive activity than it really is. Right down at the factual base, the hard rock, insofar as there is any, of the whole enterprise, we are already explicating; and worse, explicating explications. Winks upon winks upon winks.

Analysis, then, is sorting out the structures of signification—what Ryle called established codes, a somewhat misleading expression, for it makes the enterprise sound too much like that of the cipher clerk when it is much more like that of the literary critic—and determining their social ground and import. Here, in our text, such sorting would begin with distinguishing the three unlike frames of interpretation ingredient in the situation, Jewish, Berber, and French, and would then move on to show how (and why) at that time, in that place, their copresence produced a situation in which systematic misunderstanding reduced traditional form to social farce. What tripped Cohen up, and with him the whole, ancient pattern of social and economic relationships within which he functioned, was a confusion of tongues.

I shall come back to this too-compacted aphorism later, as well as to the details of the text itself. The point for now is only that ethnography is thick description. What the ethnographer is in fact faced with—except when (as, of course, he must do) he is pursuing the more automatized routines of

data collection—is a multiplicity of complex conceptual structures, many of them superimposed upon or knotted into one another, which are at once strange, irregular, and inexplicit, and which he must contrive somehow first to grasp and then to render. And this is true at the most down-to-earth, jungle field work levels of this activity: interviewing informants, observing rituals, eliciting kin terms, tracing property lines, censusing households . . . writing his journal. Doing ethnography is like trying to read (in the sense of “construct a reading of”) a manuscript—foreign, faded, full of ellipses, incoherencies, suspicious emendations, and tendentious commentaries, but written not in conventionalized graphs of sound but in transient examples of shaped behavior.

III

Culture, this acted document, thus is public, like a burlesqued wink or a mock sheep raid. Though ideational, it does not exist in someone’s head; though unphysical, it is not an occult entity. The interminable, because untermineable, debate within anthropology as to whether culture is “subjective” or “objective,” together with the mutual exchange of intellectual insults (“idealist!”—“materialist!”; “mentalist!”—“behaviorist!”; “impressionist!”—“positivist!”) which accompanies it, is wholly misconceived. Once human behavior is seen as (most of the time; there are true twitches) symbolic action—action which, like phonation in speech, pigment in painting, line in writing, or sonance in music, signifies—the question as to whether culture is patterned conduct or a frame of mind, or even the two somehow mixed together, loses sense. The thing to ask about a burlesqued wink or a mock sheep raid is not what their ontological status is. It is the same as that of rocks on the one hand and dreams on the other—they are things of this world. The thing to ask is what their import is: what it is, ridicule or challenge, irony or anger, snobbery or pride, that, in their occurrence and through their agency, is getting said.

This may seem like an obvious truth, but there are a number of ways to obscure it. One is to imagine that culture is a self-contained “super-organic” reality with forces and purposes of its own; that is, to reify it. Another is to claim that it consists in the brute pattern of behavioral events we observe in fact to occur in some identifiable community or other; that is, to reduce it. But though both these confusions still exist, and doubtless will be always with us, the main source of theoretical muddlement in contemporary anthropology is a view which developed in reaction to them and is right now very widely

held—namely, that, to quote Ward Goodenough, perhaps its leading proponent, “culture [is located] in the minds and hearts of men.”

Variouly called ethnosience, componential analysis, or cognitive anthropology (a terminological wavering which reflects a deeper uncertainty), this school of thought holds that culture is composed of psychological structures by means of which individuals or groups of individuals guide their behavior. “A society’s culture,” to quote Goodenough again, this time in a passage which has become the *locus classicus* of the whole movement, “consists of whatever it is one has to know or believe in order to operate in a manner acceptable to its members.” And from this view of what culture is follows a view, equally assured, of what describing it is—the writing out of systematic rules, an ethnographic algorithm, which, if followed, would make it possible so to operate, to pass (physical appearance aside) for a native. In such a way, extreme subjectivism is married to extreme formalism, with the expected result: an explosion of debate as to whether particular analyses (which come in the form of taxonomies, paradigms, tables, trees, and other ingenuities) reflect what the natives “really” think or are merely clever simulations, logically equivalent but substantively different, of what they think.

As, on first glance, this approach may look close enough to the one being developed here to be mistaken for it, it is useful to be explicit as to what divides them. If, leaving our winks and sheep behind for the moment, we take, say, a Beethoven quartet as an admittedly rather special but, for these purposes, nicely illustrative, sample of culture, no one would, I think, identify it with its score, with the skills and knowledge need to play it, with the understanding of it possessed by its performers or auditors, nor, to take care, en passant, of the reductionists and reifiers, with a particular performance of it or with some mysterious entity transcending material existence. The “no one” is perhaps too strong here, for there are always incorrigibles. But that a Beethoven quartet is a temporally developed tonal structure, a coherent sequence of modeled sound—in a word, music—and not anybody’s knowledge of or belief about anything, including how to play it, is a proposition to which most people are, upon reflection, likely to assent.

To play the violin it is necessary to possess certain habits, skills, knowledge, and talents, to be in the mood to play, and (as the old joke goes) to have a violin. But violin playing is neither the habits, skills, knowledge, and so on, nor the mood, nor (the notion believers in “material culture” apparently embrace)

the violin. To make a trade pact in Morocco, you have to do certain things in certain ways (among others, cut, while chanting Quranic Arabic, the throat of a lamb before the assembled, undeformed, adult male members of your tribe) and to be possessed of certain psychological characteristics (among others, a desire for distant things). But a trade pact is neither the throat cutting nor the desire, though it is real enough, as seven kinsmen of our Marmusha sheikh discovered when, on an earlier occasion, they were executed by him following the theft of one mangy, essentially valueless sheepskin from Cohen.

Culture is public because meaning is. You can't wink (or burlesque one) without knowing what counts as winking or how, physically, to contract your eyelids, and you can't conduct a sheep raid (or mimic one) without knowing what it is to steal a sheep and how practically to go about it. But to draw from such truths the conclusion that knowing how to wink is winking and knowing how to steal a sheep is sheep raiding is to betray as deep a confusion as, taking thin descriptions for thick, to identify winking with eyelid contractions or sheep raiding with chasing woolly animals out of pastures. The cognitivist fallacy—that culture consists (to quote another spokesman for the movement, Stephen Tyler) of “mental phenomena which can [he means “should”] be analyzed by formal methods similar to those of mathematics and logic”—is as destructive of an effective use of the concept as are the behaviorist and idealist fallacies to which it is a misdrawn correction. Perhaps, as its errors are more sophisticated and its distortions subtler, it is even more so.

The generalized attack on privacy theories of meaning is, since early Husserl and late Wittgenstein, so much a part of modern thought that it need not be developed once more here. What is necessary is to see to it that the news of it reaches anthropology; and in particular that it is made clear that to say that culture consists of socially established structures of meaning in terms of which people do such things as signal conspiracies and join them or perceive insults and answer them, is no more to say that it is a psychological phenomenon, a characteristic of someone's mind, personality, cognitive structure, or whatever, than to say one's mind, personality, cognitive structure, or whatever, than to say that Tantrism, genetics, the progressive form of the verb, the classification of wines, the Common law, or the notion of “a conditional curse” (as Westermarck defined the concept of ‘ar in terms of which Cohen pressed his claim to damages) is. What, in a place like Morocco, most prevents those of us who grew up winking other winks or attending other

sheep from grasping what people are up to is not ignorance as to how cognition works (though, especially as, one assumes, it works the same among them as it does among us, it would greatly help to have less of that too) as a lack of familiarity with the imaginative universe within which their acts are signs. As Wittgenstein has been invoked, he may as well be quoted:

We . . . say of some people that they are transparent to us. It is, however, important as regards this observation that one human being can be a complete enigma to another. We learn this when we come into a strange country with entirely strange traditions; and, what is more, even given a mastery of the country's language, we do not *understand* the people. (And not because of not knowing what they are saying to themselves.) We cannot find our feet with them.

IV

Finding our feet, an unnerving business which never more than distantly succeeds, is what ethnographic research consists of as a personal experience; trying to formulate the basis on which one imagines, always excessively, one has found them is what anthropological writing consists of as a scientific endeavor. We are not, or at least I am not, seeking either to become natives (a compromised word in any case) or to mimic them. Only romantics or spies would seem to find point in that. We are seeking, in the widened sense of the term in which it encompasses very much more than talk, to converse with them, a matter a great deal more difficult, and not only with strangers, than is commonly recognized. “If speaking for someone else seems to be a mysterious process,” Stanley Cavell has remarked, “that may be because speaking to someone does not seem mysterious enough.”

Looked at in this way, the aim of anthropology is the enlargement of the universe of human discourse. That is not, of course, its only aim—instruction, amusement, practical counsel, moral advance, and the discovery of natural order in human behavior are others; nor is anthropology the only discipline which pursues it. But it is an aim to which a semiotic concept of culture is peculiarly well adapted. As interworked systems of construable signs (what, ignoring provincial usages, I would call symbols), culture is not a power, something to which social events, behaviors, institutions, or processes can be causally attributed; it is a context, something within which they can be intelligibly—that is, thickly—described.

The famous anthropological absorption with the (to us) exotic—Berber horsemen, Jewish peddlers, French Legionnaires—is, thus, essentially a device for displacing the dulling sense of familiarity with which the mysteriousness of our own ability to relate perceptively to one another is concealed from us. Looking at the ordinary in places where it takes unaccustomed forms brings out not, as has so often been claimed, the arbitrariness of human behavior (there is nothing especially arbitrary about taking sheep theft for insolence in Morocco), but the degree to which its meaning varies according to the pattern of life by which it is informed. Understanding a people's cultures exposes their normalness without reducing their particularity. (The more I manage to follow what the Moroccans are up to, the more logical, and the more singular, they seem.) It renders them accessible: setting them in the frame of their own banalities, it dissolves their opacity.

It is this maneuver, usually too casually referred to as "seeing things from the actor's point of view," too bookishly as "the *verstehen* approach," or too technically as "emic analysis," that so often leads to the notion that anthropology is a variety of either long-distance mind reading or cannibal-isle fantasizing, and which, for someone anxious to navigate past the wrecks of a dozen sunken philosophies, must therefore be executed with a great deal of care. Nothing is more necessary to comprehending what anthropological interpretation is, and the degree to which it is interpretation, than an exact understanding of what it means—and what it does not mean—to say that our formulations of other peoples' symbol systems must be actor-oriented.¹

What it means is that descriptions of Berber, Jewish, or French culture must be cast in terms of the constructions we imagine Berbers, Jews, or Frenchmen to place upon what they live through, the formulae they use to define what happens to them. What it does not mean is that such descriptions are themselves Berber, Jewish, or French—that is, part of the reality they are ostensibly describing; they are anthropological—that is, part of a developing system of scientific analysis. They must be cast in terms of the interpretations to which persons of a particular denomination subject their experience, because that is what they profess to be descriptions of; they are anthropological because it is, in fact, anthropologists who profess them. Normally, it is not necessary to point out quite so laboriously that the object of study is one thing and the study of it another. It is clear enough that the physical world is not physics and *A Skeleton Key to Finnegans Wake* not *Finnegans Wake*. But, as, in

the study of culture, analysis penetrates into the very body of the object—that is, *we begin with our own interpretations of what our informants are up to, or think they are up to, and then systematize those*—the line between (Moroccan) culture as a natural fact and (Moroccan) culture as a theoretical entity tends to get blurred. All the more so, as the latter is presented in the form of an actor's-eye description of (Moroccan) conceptions of everything from violence, honor, divinity, and justice, to tribe, property, patronage, and chiefship.

In short, anthropological writings are themselves interpretations, and second and third order ones to boot. (By definition, only a "native" makes first order ones: it's *his* culture.)² They are, thus, fictions: fictions, in the sense that they are "something made," "something fashioned"—the original meaning of *fictio*—not that they are false, unfactual, or merely "as if" thought experiments. To construct actor-oriented descriptions of the involvements of a Berber chieftain, a Jewish merchant, and a French soldier with one another in 1912 Morocco is clearly an imaginative act, not all that different from constructing similar descriptions of, say, the involvements with one another of a provincial French doctor, his silly, adulterous wife, and her feckless lover in nineteenth-century France. In the latter case, the actors are represented as not having existed and the events was not having happened, while in the former they are represented as actual, or as having been so. This is a difference of no mean importance; indeed, precisely the one Madame Bovary had difficulty grasping. But the importance does not lie in the fact that their story was created while Cohen's was only noted. The conditions of their creation, and the point of it (to say nothing of the manner and the quality) differ. But the one is as much a *fictio*—"a making"—as the other.

Anthropologists have not always been as aware as they might be of this fact: that although culture exists in the trading post, the hill fort, or the sheep run, anthropology exists in the book, the article, the lecture, the museum display, or, sometimes nowadays, the film. To become aware of it is to realize that the line between mode of representation and substantive content is as undrawable in cultural analysis as it is in painting; and that fact in turn seems to threaten the objective status of anthropological knowledge by suggesting that its source is not social reality but scholarly artifice.

It does threaten it, but the threat is hollow. The claim to attention of an ethnographic account does not rest on its author's ability to capture primitive facts in faraway places and carry them home like a mask or a carving, but on

the degree to which he is able to clarify what goes on in such places, to reduce the puzzlement—what manner of men are these?—to which unfamiliar acts emerging out of unknown backgrounds naturally give rise. This raises some serious problems of verification, all right—or, if “verification” is too strong a word for so soft a science (I, myself, would prefer “appraisal”), of how you can tell a better account from a worse one. But that is precisely the virtue of it. If ethnography is thick description and ethnographers those who are doing the describing, then the determining question for any given example of it, whether a field journal squib or a Malinowski-sized monograph, is whether it sorts winks from twitches and real winks from mimicked ones. It is not against a body of uninterpreted data, radically thinned descriptions, that we must measure the cogency of our explications, but against the power of the scientific imagination to bring us into touch with the lives of strangers. It is not worth it, as Thoreau said, to go round the world to count the cats in Zanzibar.

V

Now, this proposition, that it is not in our interest to bleach behavior of the very properties that interest us before we begin to examine it, has sometimes been escalated into a larger claim: namely, that as it is only those properties that interest us, we need not attend, save cursorily, to behavior at all. Culture is most effectively treated, the argument goes, purely as a symbolic system (the catch phrase is, “in its own terms”), by isolating its elements, specifying the internal relationships among those elements, and then characterizing the whole system in among those elements, and then characterizing the whole system in some general way—according to the core symbols around which it is organized, the underlying structures of which it is a surface expression, or the ideological principles upon which it is based. Though a distinct improvement over “learned behavior” and “mental phenomena” notions of what culture is, and the source of some of the most powerful theoretical ideas in contemporary anthropology, this hermetical approach to things seems to me to run the danger (and increasingly to have been overtaken by it) of locking cultural analysis away from its proper object, the informal logic of actual life. There is little profit in extricating a concept from the defects of psychologism only to plunge it immediately into those of schematicism.

Behavior must be attended to, and with some exactness, because it is through the flow of behavior—or, more precisely, social action—that cultural

forms find articulation. They find it as well, of course, in various sorts of artifacts, and various states of consciousness; but these draw their meaning from the role they play (Wittgenstein would say their “use”) in an ongoing pattern of life, not from any intrinsic relationships they bear to one another. It is what Cohen, the sheikh, and “Captain Dumari” were doing when they tripped over one another’s purposes—pursuing trade, defending honor, establishing dominance—that created our pastoral drama, and that is what the drama is, therefore, “about.” Whatever, or wherever, symbol systems “in their own terms” may be, we gain empirical access to them by inspecting events, not by arranging abstracted entities into unified patterns.

A further implication of this is that coherence cannot be the major test of validity for a cultural description. Cultural systems must have a minimal degree of coherence, else we would not call them systems; and, by observation, they normally have a great deal more. But there is nothing so coherent as a paranoid’s delusion or a swindler’s story. The force of our interpretations cannot rest, as they are now so often made to do, on the tightness with which they hold together, or the assurance with which they are argued. Nothing has done more, I think, to discredit cultural analysis than the construction of impeccable depictions of formal order in whose actual existence nobody can quite believe.

If anthropological interpretation is constructing a reading of what happens, then to divorce it from what happens—from what, in this time or that place, specific people say, what they do, what is done to them, from the whole vast business of the world—is to divorce it from its applications and render it vacant. A good interpretation of anything—a poem, a person, a history, a ritual, an institution, a society—takes us into the heart of that of which it is the interpretation. When it does not do that, but leads us instead somewhere else—into an admiration of its own elegance, of its author’s cleverness, or of the beauties of Euclidean order—it may have its intrinsic charms; but it is something else than what the task at hand—figuring out what all that rigamarole with the sheep is about—calls for.

The rigamarole with the sheep—the sham theft of them, the reparative transfer of them, the political confiscation of them—is (or was) essentially a social discourse, even if, as I suggested earlier, one conducted in multiple tongues and as much in action as in words.

Claiming his ‘ar, Cohen invoked the trade pact; recognizing the claim, the sheikh challenged the offenders’ tribe; accepting responsibility, the offenders’

tribe paid the indemnity; anxious to make clear to sheikhs and peddlers alike who was now in charge here, the French showed the imperial hand. As in any discourse, code does not determine conduct, and what was actually said need not have been. Cohen might not have, given its illegitimacy in Protectorate eyes, chosen to press his claim. The sheikh might, for similar reasons, have rejected it. The offenders' tribe, still resisting French authority, might have decided to regard the raid as "real" and fight rather than negotiate. The French, were they more *habile* and less *dur* (as, under Mareschal Lyautey's seigniorial tutelage, they later in fact became) might have permitted Cohen to keep his sheep, winking—as we say—at the continuance of the trade pattern and its limitation to their authority. And there are other possibilities: the Marmushans might have regarded the French action as too great an insult to bear and gone into dissidence themselves; the French might have attempted not just to clamp down on Cohen but to bring the sheikh himself more closely to heel; and Cohen might have concluded that between renegade Berbers and Beau Geste soldiers, driving trade in the Atlas highlands was no longer worth the candle and retired to the better-governed confines of the town. This, indeed, is more or less what happened, somewhat further along, as the Protectorate moved toward genuine sovereignty. But the point here is not to describe what did or did not take place in Morocco. (From this simple incident one can widen out into enormous complexities of social experience.) It is to demonstrate what a piece of anthropological interpretation consists in: tracing the curve of a social discourse; fixing it into an inspectable form.

The ethnographer "inscribes" social discourse; *he writes it down*. In so doing, he turns it from a passing event, which exists only in its own moment of occurrence, into an account, which exists in its inscriptions and can be reconsulted. The sheikh is long dead, killed in the process of being, as the French called it, "pacified"; "Captain Dumari," his pacifier, lives, retired to his souvenirs, in the south of France; and Cohen went last year, part refugee, part pilgrim, part dying patriarch, "home" to Israel. But what they, in my extended sense, "said" to one another on an Atlas plateau sixty years ago is—very far from perfectly—preserved for study. "What," Paul Ricoeur, from whom this whole idea of the inscription of action is borrowed and somewhat twisted, asks, "what does writing fix?"

Not the event of speaking, but the "said" of speaking, where we understand by the "said" of speaking that intentional exteriorization constitutive of the aim of

discourse thanks to which the *sagen*—the saying—wants to become *Aus-sage*—the enunciation, the enunciated. In short, what we write is the *noema* ["thought," "content," "gist"] of the speaking. It is the meaning of the speech event, not the event as event.

This is not itself so very "said"—if Oxford philosophers run to little stories, phenomenological ones run to large sentences; but it brings us anyway to a more precise answer to our generative question, "What does the ethnographer do?"—he writes.³ This, too, may seem a less than startling discovery, and to someone familiar with the current "literature," an implausible one. But as the standard answer to our question has been, "He observes, he records, he analyzes"—a kind of *veni, vidi, vici* conception of the matter—it may have more deep-going consequences than are at first apparent, not the least of which is that distinguishing these three phases of knowledge-seeking may not, as a matter of fact, normally be possible; and, indeed, as autonomous "operations" they may not in fact exist.

The situation is even more delicate, because, as already noted, what we inscribe (or try to) is not raw social discourse, to which, because, save very marginally or very specially, we are not actors, we do not have direct access, but only that small part of it which our informants can lead us into understanding.⁴ This is not as fatal as it sounds, for, in fact, not all Cretans are liars, and it is not necessary to know everything in order to understand something. But it does make the view of anthropological analysis as the conceptual manipulation of discovered facts, a logical reconstruction of a mere reality, seem rather lame. To set forth symmetrical crystals of significance, purified of the material complexity in which they were located, and then attribute their existence to autogenous principles of order, universal properties of the human mind, or vast, a priori *weltanschauungen*, is to pretend a science that does not exist and imagine a reality that cannot be found. Cultural analysis is (or should be) guessing at meanings, assessing the guesses, and drawing explanatory conclusions from the better guesses, not discovering the Continent of Meaning and mapping out its bodiless landscape.

VI

So, there are three characteristics of ethnographic description: it is interpretive; what it is interpretive of is the flow of social discourse; and the interpreting

involved consists in trying to rescue the “said” of such discourse from its perishing occasions and fix it in perusable terms. The *kula* is gone or altered; but, for better or worse, *The Argonauts of the Western Pacific* remains. But there is, in addition, a fourth characteristic of such description, at least as I practice it: it is microscopic.

This is not to say that there are no large-scale anthropological interpretations of whole societies, civilizations, world events, and so on. Indeed, it is such extension of our analyses to wider contexts that, along with their theoretical implications, recommends them to general attention and justifies our constructing them. No one really cares anymore, not even Cohen (well . . . maybe, Cohen), about those sheep as such. History may have its unobtrusive turning points, “great noises in a little room”; but his little go-round was surely not one of them.

It is merely to say that the anthropologist characteristically approaches such broader interpretations and more abstract analyses from the direction of exceedingly extended acquaintances with extremely small matters. He confronts the same grand realities that others—historians, economists, political scientists, sociologists—confront in more fateful settings: Power, Change, Faith, Oppression, Work, Passion, Authority, Beauty, Violence, Love, Prestige; but he confronts them in contexts obscure enough—places like Marmusha and lives like Cohen’s—to take the capital letters off them. These all-too-human constancies, “those big words that make us all afraid,” take a homely form in such homely contexts. But that is exactly the advantage. There are enough profundities in the world already.

Yet, the problem of how to get from a collection of ethnographic miniatures on the order of our sheep story—an assortment of remarks and anecdotes—to wall-sized culturescapes of the nation, the epoch, the continent, or the civilization is not so easily passed over with vague allusions to the virtues of concreteness and the down-to-earth mind. For a science born in Indian tribes, Pacific islands, and African lineages and subsequently seized with grander ambitions, this has come to be a major methodological problem, and for the most part a badly handled one. The models that anthropologists have themselves worked out to justify their moving from local truths to general visions have been, in fact, as responsible for undermining the effort as anything their critics—sociologists obsessed with sample sizes, psychologists with measures, or economists with aggregates—have been able to devise against them.

Of these, the two main ones have been: the Jonesville-is-the-USA “microcosmic” model; and the Easter-Island-is-a-testing-case “natural experiment” model. Either heaven in a grain of sand, or the farther shores of possibility.

The Jonesville-is-America writ small (or America-is-Jonesville writ large) fallacy is so obviously one that the only thing that needs explanation is how people have managed to believe it and expected others to believe it. The notion that one can find the essence of national societies, civilizations, great religions, or whatever summed up and simplified in so-called “typical” small towns and villages is palpable nonsense. What one finds in small towns and villages is (alas) small-town or village life. If localized, microscopic studies were really dependent for their greater relevance upon such a premise—that they captured the great world in the little—they wouldn’t have any relevance.

But, of course, they are not. The locus of study is not the object of study. Anthropologists don’t study villages (tribes, towns, neighborhoods . . .); they study in villages. You can study different things in different places, and some things—for example, what colonial domination does to established frames of moral expectation—you can best study in confined localities. But that doesn’t make the place what it is you are studying. In the remoter provinces of Morocco and Indonesia I have wrestled with the same questions other social scientists have wrestled with in more central locations—for example, how comes it that men’s most importunate claims to humanity are cast in the accents of group pride?—and with about the same conclusiveness. One can add a dimension—one much needed in the present climate of size-up-and-solve social science; but that is all. There is a certain value, if you are going to run on about the exploitation of the masses in having seen a Javanese sharecropper turning earth in a tropical downpour or a Moroccan tailor embroidering kaf-tans by the light of a twenty-watt bulb. But the notion that this gives you the thing entire (and elevates you to some moral vantage ground from which you can look down upon the ethically less privileged) is an idea which only someone too long in the bush could possibly entertain.

The “natural laboratory” notion has been equally pernicious, not only because the analogy is false—what kind of a laboratory is it where *none* of the parameters are manipulable?—but because it leads to a notion that the data derived from ethnographic studies are purer, or more fundamental, or more solid, or less conditioned (the most favored word is “elementary”) than those derived from other sorts of social inquiry. The great natural variation of cultural

forms is, of course, not only anthropology's great (and wasting) resource, but the ground of its deepest theoretical dilemma: how is such variation to be squared with the biological unity of the human species? But it is not, even metaphorically, experimental variation, because the context in which it occurs varies along with it, and it is not possible (though there are those who try) to isolate the y's from x's to write a proper function.

The famous studies purporting to show that the Oedipus complex was backwards in the Trobriands, sex roles were upside down in Tchambuli, and the Pueblo Indians lacked aggression (it is characteristic that they were all negative—"but not in the South"), are, whatever their empirical validity may or may not be, not "scientifically tested and approved" hypotheses. They are interpretations, or misinterpretations, like any others, arrived at in the same way as any others, and as inherently inconclusive as any others, and the attempt to invest them with the authority of physical experimentation is but methodological sleight of hand. Ethnographic findings are not privileged, just particular: another country heard from. To regard them as anything more (*or anything less*) than that distorts both them and their implications, which are far profounder than mere primitivity, for social theory.

Another country heard from: the reason that protracted descriptions of distant sheep raids (and a really good ethnographer would have gone into what kind of sheep they were) have general relevance is that they present the sociological mind with bodied stuff on which to feed. The important thing about the anthropologist's findings is their complex specificness, their circumstantiality. It is with the kind of material produced by long-term, mainly (though not exclusively) qualitative, highly participative, and almost obsessively fine-comb field study in confined contexts that the mega-concepts with which contemporary social science is afflicted—legitimacy, modernization, integration, conflict, charisma, structure . . . meaning—can be given the sort of sensible actuality that makes it possible to think not only realistically and concretely *about* them, but, what is more important, creatively and imaginatively *with* them.

The methodological problem which the microscopic nature of ethnography presents is both real and critical. But it is not to be resolved by regarding a remote locality as the world in a teacup or as the sociological equivalent of a cloud chamber. It is to be resolved—or, anyway, decently kept at bay—by realizing that social actions are comments on more than themselves; that where

an interpretation comes from does not determine where it can be impelled to go. Small facts speak to large issues, winks to epistemology, or sheep raids to revolution, because they are made to.

VII

Which brings us, finally, to theory. The besetting sin of interpretive approaches to anything—literature, dreams, symptoms, culture—is that they tend to resist, or to be permitted to resist, conceptual articulation and thus to escape systematic modes of assessment. You either grasp an interpretation or you do not, see the point of it or you do not, accept it or you do not. Imprisoned in the immediacy of its own detail, it is presented as self-validating, or, worse, as validated by the supposedly developed sensitivities of the person who presents it; any attempt to cast what it says in terms other than its own is regarded as a travesty—as, the anthropologist's severest term of moral abuse, ethnocentric.

For a field of study which, however, timidly (though I, myself, am not timid about the matter at all), asserts itself to be a science, this just will not do. There is no reason why the conceptual structure of a cultural interpretation should be any less formulable, and thus less susceptible to explicit canons of appraisal, than that of, say, a biological observation or a physical experiment—no reason except that the terms in which such formulations can be cast are, if not wholly nonexistent, very nearly so. We are reduced to insinuating theories because we lack the power to state them.

At the same time, it must be admitted that there are a number of characteristics of cultural interpretation which make the theoretical development of it more than usually difficult. The first is the need for theory to stay rather closer to the ground than tends to be the case in sciences more able to give themselves over to imaginative abstraction. Only short flights of ratiocination tend to be effective in anthropology; longer ones tend to drift off into logical dreams, academic bemusements with formal symmetry. The whole point of a semiotic approach to culture is, as I have said, to aid us in gaining access to the conceptual world in which our subjects live so that we can, in some extended sense of the term, converse with them. The tension between the pull of this need to penetrate an unfamiliar universe of symbolic action and the requirements of technical advance in the theory of culture between the need to grasp and the need to analyze, is, as a result, both necessarily great and essentially irremovable. Indeed, the further

theoretical development goes, the deeper the tension gets. This is the first condition for cultural theory: it is not its own master. As it is unseverable from the immediacies thick description presents, its freedom to shape itself in terms of its internal logic is rather limited. What generality it contrives to achieve grows out of the delicacy of its distinctions, not the sweep of its abstractions.

And from this follows a peculiarity in the way, as a simple matter of empirical fact, our knowledge of culture . . . cultures . . . a culture . . . grows: in spurts. Rather than following a rising curve of cumulative findings, cultural analysis breaks up into a disconnected yet coherent sequence of bolder and bolder sorties. Studies do build on other studies, not in the sense that they take up where the others leave off, but in the sense that, better informed and better conceptualized, they plunge more deeply into the same things. Every serious cultural analysis starts from a sheer beginning and ends where it manages to get before exhausting its intellectual impulse. Previously discovered facts are mobilized, previously developed concepts used, previously formulated hypotheses tried out; but the movement is not from already proven theorems to newly proven ones, it is from an awkward fumbling for the most elementary understanding to a supported claim that one has achieved that and surpassed it. A study is an advance if it is more incisive—whatever that may mean—than those that preceded it; but it less stands on their shoulders than, challenged and challenging, runs by their side.

It is for this reason, among others, that the essay, whether of thirty pages or three hundred, has seemed the natural genre in which to present cultural interpretations and the theories sustaining them, and why, if one looks for systematic treatises in the field, one is so soon disappointed, the more so if one finds any. Even inventory articles are rare here, and anyway of hardly more than bibliographical interest. The major theoretical contributions not only lie in specific studies—that is true in almost any field—but they are very difficult to abstract from such studies and integrate into anything one might call “culture theory” as such. Theoretical formulations hover so low over the interpretations they govern that they don’t make much sense or hold much interest apart from them. This is so, not because they are not general (if they are not general, they are not theoretical), but because, stated independently of their applications, they seem either commonplace or vacant. One can, and this in fact is how the field progresses conceptually, take a line of theoretical attack developed in connection with one exercise in ethnographic interpretation and

employ it in another, pushing it forward to greater precision and broader relevance; but one cannot write a “general Theory of Cultural Interpretation.” Or, rather, one can, but there appears to be little profit in it, because the essential task of theory building here is not to codify abstract regularities but to make thick description possible, not to generalize across cases but to generalize within them.

To generalize within cases is usually called, at least in medicine and depth psychology, clinical inference. Rather than beginning with a set of observations and attempting to subsume them under a governing law, such inference begins with a set of (presumptive) signifiers and attempts to lace them within an intelligible frame. Measures are matched to the theoretical predictions, but symptoms (even when they are measured) are scanned for theoretical peculiarities—that is, they are diagnosed. In the study of culture the signifiers are not symptoms or clusters of symptoms, but symbolic acts of clusters of symbolic acts, and the aim is not therapy but the analysis of social discourse. But the way in which theory is used—to ferret out the unapparent import of things—is the same.

Thus we are led to the second condition of cultural theory: it is not, at least in the strict meaning of the term, predictive. The diagnostician doesn’t predict measles; he decides that someone has them, or at the very most anticipates that someone is rather likely shortly to get them. But this limitation, which is real enough, has commonly been both misunderstood and exaggerated, because it has been taken to mean that cultural interpretation is merely post facto: that, like the peasant in the old story, we first shoot the holes in the fence and then paint the bull’s-eyes around them. It is hardly to be denied that there is a good deal of that sort of thing around, some of it in prominent places. It is to be denied, however, that it is the inevitable outcome of a clinical approach to the use of theory.

It is true that in the clinical style of theoretical formulation, conceptualization is directed toward the task of generating interpretations of matters already in hand, not toward projecting outcomes of experimental manipulations or deducing future states of a determined system. But that does not mean that theory has only to fit (or, more carefully, to generate cogent interpretations of) realities past; it has also to survive—intellectually survive—realities to come. Although we formulate our interpretations of an outburst of winking or an instance of sheep-raiding after its occurrence, sometimes long after, the

theoretical framework in terms of which such an interpretation is made must be capable of continuing to yield defensible interpretations as new social phenomena swim into view. Although one starts any effort at thick description, beyond the obvious and superficial, from a state of general bewilderment as to what the devil is going on—trying to find one's feet—one does not start (or ought not) intellectually empty-handed. Theoretical ideas are not created wholly anew in each study; as I have said, they are adopted from other, related studies, and, refined in the process, applied to new interpretive problems. If they cease being useful with respect to such problems, they tend to stop being used and are more or less abandoned. If they continue being useful, throwing up new understandings, they are further elaborated and go on being used.⁵

Such a view of how theory functions in an interpretive science suggests that the distinction, relative in any case, that appears in the experimental or observational sciences between "description" and "explanation" appears here as one, even more relative, between "inscription" ("thick description") and "specification" ("diagnosis")—between setting down the meaning particular social actions have for the actors whose actions they are, and stating, as explicitly as we can manage, what the knowledge thus attained demonstrates about the society in which it is found and, beyond that, about social life as such. Our double task is to uncover the conceptual structures that inform our subjects' acts, the "said" of social discourse, and to construct a system of analysis in whose terms what is generic to those structures, what belongs to them because they are what they are, will stand out against the other determinants of human behavior. In ethnography, the office of theory is to provide a vocabulary in which what symbolic action has to say about itself—that is, about the role of culture in human life—can be expressed.

Aside from a couple of orienting pieces concerned with more foundational matters, it is in such a manner that theory operates in the essays collected here. A repertoire of very general, made-in-the-academy concepts and systems of concepts—"integration," "rationalization," "symbol," "ideology," "ethos," "revolution," "identity," "metaphor," "structure," "ritual," "world view," "actor," "function," "sacred," and, of course, "culture" itself—is woven into the body of thick-description ethnography in the hope of rendering mere occurrences scientifically eloquent.⁶ The aim is to draw large conclusions from small, but very densely textured facts; to support broad assertions about the role of cul-

ture in the construction of collective life by engaging them exactly with complex specifics.

Thus it is not only interpretation that goes all the way down to the most immediate observational level: the theory upon which such interpretation conceptually depends does so also. My interest in Cohen's story, like Ryle's in winks, grew out of some very general notions indeed. The "confusion of tongues" model—the view that social conflict is not something that happens when, out of weakness, indefiniteness, obsolescence, or neglect, cultural forms cease to operate, but rather something which happens when, like burlesqued winks, such forms are pressed by unusual situations or unusual intentions to operate in unusual ways—is not an idea I got from Cohen's story. It is one, instructed by colleagues, students, and predecessors, I brought to it.

Our innocent-looking "note in a bottle" is more than a portrayal of the frames of meaning of Jewish peddlers, Berber warriors, and French proconsuls, or even of their mutual interference. It is an argument that to rework the pattern of social relationships is to rearrange the coordinates of the experienced world. Society's forms are culture's substance.

[. . .]

NOTES

1. Not only other peoples': anthropology *can* be trained on the culture of which it is itself a part, and it increasingly is; a fact of profound importance, but which, as it raises a few tricky and rather special second order problems, I shall put to the side for the moment.
2. The order problem is, again, complex. Anthropological works based on other anthropological works (Lévi-Strauss, for example) may, of course, be fourth order or higher, and informants frequently, even habitually, make second order interpretations—what have come to be known as "native models." In literate cultures, where "native" interpretation can proceed to higher levels—in connection with the Maghreb, one has only to think of Ibn Khaldun; with the United States, Margaret Mead—these matters become intricate indeed.
3. Or, again, more exactly, "inscribes." Most ethnography is in fact to be found in books and articles, rather than in films, records, museum displays, or whatever; but even in them there are, of course, photographs, drawings, diagrams, tables, and so on. Self-consciousness about modes of representation (not to speak of experiments with them) has been very lacking in anthropology.

4. So far as it has reinforced the anthropologist's impulse to engage himself with his informants as persons rather than as objects, the notion of "participant observation" has been a valuable one. But, to the degree it has led the anthropologist to block from his view the very special, culturally bracketed nature of his own role and to imagine himself something more than an interested (in both senses of that word) sojourner, it has been our most powerful source of bad faith.

5. Admittedly, this is something of an idealization. Because theories are seldom if ever decisively disproved in clinical use but merely grow increasingly awkward, unproductive, strained, or vacuous, they often persist long after all but a handful of people (though they are often most passionate) have lost much interest in them. Indeed, so far as anthropology is concerned, it is almost more of a problem to get exhausted ideas out of the literature than it is to get productive ones in, and so a great deal more of theoretical discussion than one would prefer is critical rather than constructive, and whole careers have been devoted to hastening the demise of moribund notions. As the field advances one would hope that this sort of intellectual weed control would become a less prominent part of our activities. But, for the moment, it remains true that old theories tend less to die than to go into second editions.

6. The overwhelming bulk of the following chapters concern Indonesia rather than Morocco, for I have just begun to face up to the demands of my North African material, which, for the most part, was gathered more recently. Field work in Indonesia was carried out in 1952–1954, 1957–1958, and 1971; in Morocco in 1964, 1965–1966, 1968–1969, and 1972.