Whose Knowledge?*

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Komu patří vědění?

Abstract: Behaviour of an individual is seen as the result of a series of decisions taken on the basis of his taken-for-granted knowledge about the universe – that knowledge is shared by specific others. That is the social reality we are trying to explain. The individual is able to account for his behaviour and state of his knowledge in contingent, episodic and anecdotal ways because of its "taken-for-grantedness". However, a detailed study permits us to present both his actions and his knowledge in a systematic way, together with the principles by which he organizes them. The fact of action being taken as result of a series of decisions means that the individual is not just a "norm-fulfilling unit", he is, within limits given by his knowledge, manipulating his social world.

Keywords: anthropologist's observation and interpretation of social reality, people's notions of functioning of their society

I.

There is a story about two psychoanalysts who had their offices on the same street. Every morning they passed each other without greeting. One morning, however, one of them said "good morning". The other did not answer, went on, and after a few steps turned around. "Now", he mused, looking after the first, "I wonder what he really meant by that?".

The point of this story could almost be taken as a parole de guerre of social anthropology. In a sense, the real meaning of what man, people, societies say, do, or otherwise express, is what most anthropology is about. The same is valid for all other social sciences too, but since this is basically a discussion among anthropologists, I prefer to speak only about this subject. To put it in other words, anthropology tries to find a real, i.e. true (not only satisfactory) explanations of why people do the things they do and say the things they say. Put simply like that, it seems a pretty straightforward, if broad and formidable, task. We can say naively – well, why not ask them? Naively, because it is precisely at this point that the difference between "real" and satisfactory understanding, or explanation, of human behaviour enters, and starts to play havoc with that task. The main problems which are involved here derive from two sets of assumptions on which the anthropological theory of explanation is based and which are taken, by and large, as axioms. The first set refers to the societies or people studied by anthropologists:

1. The people observed may have satisfactory (to them) explanations, but these are rarely, if ever, true explanations, since the people have no adequate knowledge of the causes and consequences of their behaviour.

2. The explanations the people have are, in fact, devices "used to summon behaviour as much as to explain it" [*Wilson 1970: xi*]. That is, their explanations are, in fact,

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legitimizations, rationalizations or justifications of the phenomena they purport to explain. Therefore, the people have no means of assessing the truth of an explanation, other than the observable or believed-in effectiveness of a given behaviour.

3. The explanations the people have are particularistic and contingent, not generalizing, therefore, they cannot have any standards of critical discussion and refutation by contradictory evidence. In fact, they cannot even have the concept of contradictory evidence.

The second set refers to the anthropologists and can be formulated as an almost word for word reversal of the first set:

1. The anthropologists' knowledge is adequate, or can be made adequate, for true explanations, since it discerns through observation and induction, causes and consequences of particular events.

2. Their explanations are intended to account for phenomena – not to rationalize or justify their occurrence, therefore, they can be assessed as true or false on the basis of the comprehensibility of that account, regardless of what the people observed take as right or wrong.

3. Their explanations are, or can be, generalized and independent of the phenomena explained, therefore, they are able to discover contradictory evidence, assess its importance and either refute the explanation or reidentify the phenomena previously taken as contradictory evidence. As Jarvie deftly puts it: "(…) our standards of critical discussion are better than no standards of critical discussion, and the latter is the situation of the savage" [*Jarvie 1970: 61*].

In anthropological writings, these twin sets of assumptions, or any part of them, are seldom made explicit and even more seldom, if ever, taken as a subject for discussion. They are just tacitly taken for granted (the exceptions being, to a certain degree, cognitive anthropology and discussions about emic- and etic-oriented studies in America), and discussion of them is defined as philosophical and therefore not pertaining to the range of things anthropologists should be discussing. As a matter of fact, it is in the philosophy of science that we can find, especially in recent years, anthropological data and arguments used in discussions of the standards of rationality, of the assessment of the adequacy of knowledge, of satisfactory, true and false explanations, etc. I am entering this particular field with some hesitation, and only after claiming the right to be naive as a practitioner of one discipline when treating subjects generally recognized as belonging to another [*cf. Gluckman 1964: 16 ff.*]. However, since we are more or less uncritically working with assumptions that are freely discussed in philosophical writings, and with our data too, I believe we should have our say about them as well.

The very nature of the assumptions formulated above, and the fact that they form a taken-for-granted background of our normal work, make it difficult, if not impossible, to discuss them only in the rather abbreviated form in which they are expressed here. To appreciate their importance and their consequences for anthropological interpretation, a somewhat broader background discussion is needed. Basically, these assumptions derive from what is taken to be the existential status of social reality (i.e. from the ontology of social sciences) and from what is taken to be its epistemological status, or, more specifically, what are postulated as legitimate processes of making meaningful generalized statements about it. My position is that we are making an unwarranted division of the universe under consideration into two separate spheres: social reality itself, and procedures for

making generalized statements about it, as if they were two different universes. In other words, we are ascribing differential status to the observed people's activities, beliefs and knowledge, and to the anthropologists' activities, beliefs and knowledge. The first, which is to be observed and explained, is conceived of as existing somewhere outside of the anthropologists' sphere, as having an independent existence; simply as being "social reality". (This concept, though not commonly used, has appeared often enough in anthropological writings to justify its use here.) The latter, which is taken to be different, must then be either non-social reality, or social non-reality, or perhaps non-social non-reality. This may sound like a rather bad pun, but intend to argue that anthropologists tacitly assume the position that it is, indeed, non-social reality.

As a case in point, let us consider briefly the concept of culture in its classical Tylorian sense, which includes all things social. Most anthropologists would agree that it is man-made, that culture is the product of man's activities. If such is the case, then clearly people's accounts of how it came into being, or how it goes on, are real accounts, accounts of what "really happened or happens". People's explanations are basically statements about the relationship between their knowledge and their activity, statements about the actual emergence of cultural phenomena or events. Yet, these accounts or explanations are seen by the anthropologists as being „satisfactory for them, but "false for us". Like the psychoanalysts mentioned at the beginning of this paper, the anthropologists are looking for the real explanation, for the real meaning of observed or inferred phenomena. Thus, the agreement about culture being man made becomes an empty concession, since the real meaning, or true contents, and in the last instance the very existence of the culture, is conceived of as being independent of those who are supposed to have made it. In common practice, this real explanation is assumed to have been achieved when a given phenomenon or event is somehow related to some larger plan or charter. Typical examples of such plans or charters are structure, evolutionary stage, or ecosystem; the choice between these and other similar charters depends on what particular set of assumptions about the nature of social reality the anthropologist holds to be true. The general laws determining the ordered arrangement of generalized units of such a charter, or the general principles which cause them to be so arranged, do not apply only to social phenomena, but to the universe at large, or at least form a specific subset of such principles. For instance, organic, mechanical, and similar analogies are often used in anthropological explanations, purportedly to illustrate some point, but in fact to strengthen it. Thus, social reality is seen as explained, or even as meaningfully existing, owing to non-social or not-only-social causes. It is not, in any significant sense, "made" by the people, in spite of the generally accepted declaration to the contrary. People act only as agents of non·social, or not-only-social forces. (They can be made aware of them, but this does not make them more social.) The existential status of social reality is assumed to be that of the "real thing" existing "out there" [*cf. Filmer et al., 1972: 18 tf*].

The individual's activities, which have meaning or purpose for him and derive from his knowledge, are not really those which the anthropologist relates to his plan or charter. These activities, or other events, are seen by the anthropologist as derived from the causal principles of the charter which naturally changes their meaning and puts them on a different level. Nothing recognizable to an individual or dependent on him, is left, save possibly a series of acts seen as expenditures of energy, which is not what we study. For all practical purposes, culture becomes simply an attribute of a class of phenomena called

human groups, more or less in the same way as dimensions are attributes of a class called physical objects. Thus, the definition of the universe studied by anthropologists as social reality is not only an empty term, but actually a misleading one, since it names boundaries, or, more exactly, a property which the same anthropologists are implicitly denying by their activities.

This, however, is only the first half of the argument: I have tried to show that anthropologists study culture, or social reality, as a consequence of non-social, or not-only-social forces or organizing principles, thus ascribing to it the existential status of non-social reality. The second half of the argument involves the nature of those legitimate processes of making meaningful, generalized statement about social reality which I mentioned earlier, i.e. its epistemological status. I would like to return here to the assumption that popular explanations are false or inadequate, because they are summonses for behaviour as much as, or even more than, they are explanations for it. For the anthropologist, they are neither nearer to, not further from, true explanations. They are contingent, subject to manipulation for reasons of effectiveness, and so on. In short, they are subject to the demands of the social world and they are legitimate or true if they are accepted by it. The individual is a creative agent in this context: he formulates the explanations according to his own knowledge and as a function of his and others' behaviour. Therefore, his explanations belong, at best, to the social reality itself and their formulation is a social action.

Anthropologists' explanations, by contrast, do not summon any behaviour, except possibly approval, elaboration or refutation by other anthropologists. They are presented as final explanations, and their relative distance from truth is the only criterion of their validity. They are subject to the demands of eliciting meaningful accounts of the social world, though, as we have seen, in terms of non social or not-only-social principles.

The anthropologist appears as acogniting and eliciting agent. He formulates his explanations as a result of the formalized processing of data, and both the processing of the data and the resulting explanations are seen as independent of the data processed, that is, of the social world. No legitimate contingent relations to what is going on around the anthropologist are recognized: the fact that the anthropologist belongs to the same category as those who inhabit the social world is supposed to be irrelevant to his activities *qua* anthropologist.[1] This definition of what anthropologists do, or are supposed to do, would probably be accepted by most of them. It can be best illustrated with Harris's definition of etic statements:

> Etic statements depend upon phenomenal distinctions judged appropriate by the community of scientific observers. Etic statements cannot be falsified if they do not conform to the actor's notion of what is significant, real, meaningful or appropriate. Etic statements are verified when independent observers using similar operations agree that a given event has occurred. An ethnography carried out according to etic principles is thus a corpus of predictions about the behaviour of classes of people. Predictive failures in that corpus require the reformulation of probabilities or the description as a whole. [*Jarvie 1970: 61*]

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[1] I am referring here to the anthropologist as an agent eliciting explanations and laws, not to his activities, for example, a radical in racial disputes, or an advocate for a particular group of people in general. These activities. though performed frequently by him, relate to his main activities at best marginally, and at worst not at all.

Though Harris's definition is slightly couched in terms that sound contingent or optional, such as "phenomenal distinctions judged appropriate by the community of scientific observers", they are not meant to be so. Traced back far enough, the criteria of appropriateness will always be truth criteria derived from the anthropologist (or a community of scientific observers) assuming as true a set of non-social or not-only-social organizing principles of the universe at large.

Thus, people's explanations or meaningful statements about phenomena are seen by anthropologists as a part of social reality, as social activities. On the other hand, anthropologists consider their own explanations and accounts as statements about social reality from the outside, as activities not determined by pressures and rules existing in the social world and, therefore, in the last instance, as non-social activities. They are externally social in the sense that they are carried out by human beings (anthropologists), but they are supposedly determined and organized by principles independent of any social reality.

I would argue that this is a part of anthropological and, in general, scientific mythology and that the distinction between people's explanatory and accounting activities and anthropologist's explanatory and accounting activities as they stand, is unwarranted and illegitimate. This can be demonstrated, I believe, by showing that anthropologist's activities and explanations exhibit, upon closer examination the same characteristics that are considered by them as fallacious in people's explanations. This will be simile to, though not identical with, the point Kuhn is making about the development of science:

> (…) these same historians confront growing difficulties in distinguishing the "scientific" component of past observation and belief from what their predecessors had readily labelled 'error' and "superstition". The more carefully they study, say Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. [*Kuhn 1971: 2*]

What Kuhn proposes is, basically, that "scientific by their standards" and "scientific by our standards" belong to the same sphere. The "scientificity" of statements or validity of laws is measured according to the distance from truth, but the truth criteria themselves are contingent and are agreed upon by people who inhabit the social world. If accepted, this proposition would make it rather difficult to accuse people's explanations of falsity and contingency and to see anthropologists' explanations as having a differential dimension of truth and having, ideally, no contingent relations with the contemporary social world. There is, in fact, ample evidence that anthropologists are aware that their theories, or to be more exact, other anthropologists' theories, can be considered as resulting from the demands of the social world they inhabit, and not from the demands of simply eliciting the most meaningful accounts of the reality. When the evolutionists, who at the time represented the most diffused and most consistent theoretical school, were being criticized, the critique was based on two main points. The first was that the data, often collected by evolutionists themselves, did not in fact necessarily prove the existence of an ongoing evolution of human societies nor the existence of immutable laws of cultural evolution. The

second was that evolutionism itself was not formulated as a result of the quest for truth, but as legitimization or justification for the superordinate position of a handful of developed societies. In other words, the principles on whose basis evolutionary explanations were formulated, were considered by later critics as having been adopted by the evolutionists because of the particular organization of their social world, and not because of the socially independent quest for truth.

A similar argument applies also to structuralism or functional structuralism which replaced evolutionism as a leading theoretical school. Structuralism also claimed that its basic assumptions about the nature of social reality, and the basic principles on which it bases its explanations (structural and/or functional interdependence of phenomena, equilibrium, homeostasis) are socially independent and ultimately lead to the accounts of a social universe which is not "meaningful to us", but true. However, there have appeared, over the last few decades, a host of new theoretical approaches that criticize the assumptions and principles of structuralism as not being derived from the objective demands to find true explanations, but, again, as being contingent to a particular organization of the social world. Most of the structuralists, who belonged to a large colonial empire, took this empire as a natural and necessary formation and therefore dedicated their attention to the forces which hold it, or any large social group, together. And in their turn, the representatives of some more radical theoretical approaches have been criticized for holding their methodological principles because of their commitment to a specific project of the future, that is, again because of the conditions of their own social world.

I am not interested here in the merits of these critiques, nor in the defence of one theory against another. I am using this very brief and simplified argument only to show that anthropologists themselves often accuse each other, not of being less true, but of holding theories because of the demands of social reality, that is, contingent ones. Let us consider evolutionism, structuralism, and a host of new approaches, as paradigms in the sense of Kuhn's definition, i.e. as "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" [*Kuhn 1971: viii*]. The usual idea was that one paradigm can be invalidated by a cumulative process of scientific achievement. Kuhn calls this "the concept of development-by-accumulation" [*Ibid.: 2*] and criticizes it on the grounds that a simple accumulation of data is not enough to change a paradigma. This requires profound changes in how the world is seen and what scientific work in such a world is conceived to be [*Ibid.: 4 and passim*]. In the example I have mentioned above, the practitioners of each paradigm were seeing themselves as pursuing the true knowledge and explaining social reality in the only possible meaningful way. In all cases, they were accused by their opponents not only of not producing satisfactory evidence, which could be ascribed to insufficient techniques, imprecise interpretations, and so on, but of formulating the whole paradigm because it was in agreement with how social reality was organized in their time, and not because of its real explanatory value. That is, their paradigm was not really derived from the demands of true explanations and true accounting, but from the demands of their own social world, which means that it is therefore a satisfactory and contingent paradigm for them, but not true for us, regardless of whether or not they are aware of it. Harris's definition of emic statements is, I believe, appropriate here:

> Emic statements refer to logico-empirical systems whose phenomenal distinctions or "things" are built up out of contrasts and discriminations significant, meaningful, real, accurate, or in some other fashion regarded as appropriate by the actors themselves. [*Harris 1968: 571*]

Of course, the term "emic" is used to describe people's cognitive systems and explanations. However, we have seen that the cognitive systems within which anthropologists work, and therefore the explanations of social reality they offer, must be taken, on anthropologists' own evidence, as being no less contingent, no less subject to the demands of social reality, and no less emic than people's accounts of social reality. Therefore, they cannot be measured as being nearer to, or further from, the truth than people's explanations, but as being more or less satisfactory in offering knowledge "meaningful to us". What anthropologists do, is no less a part of the social world, and no less a social activity, than what people do.

My opening argument can be summed up briefly in the following way: anthropologists hold, more or less explicitly, a set of ontological assumptions about the nature of the universe they study. It is conceived of as a social reality, produced and maintained by the people they study. Anything people do or say, including their explanations and accounts of the social world, is a part of this social reality. At the same time, anthropologists hold a set of epistemological assumptions about the nature and legitimacy of their own activities and explanations, making thus a qualitative difference between these and social reality. Their activities are defined as the search for true explanations of social reality. They see their activities as the eliciting of causal principles or laws which order the universe; these principles are therefore subject not to the effectiveness of the social world, but to the criteria and demands of this eliciting. To put it crudely, anthropologists see themselves not as members of the social world but as agents of truth.

I am trying to show that both these sets of assumptions are being held illegitimately. This can be demonstrated by the actual practice of anthropologists. As concerns the first set of assumptions, the universe of study is called social reality, but the phenomena and events composing it are treated as having the same characteristics and as subject to the same organizing principles as the universe at large. Therefore, the specification of the object of study as social reality is redundant and, in fact, misleading or, as I have said above, illegitimate. As concerns the second set of assumptions, anthropologists claim non-social status for their activities and explanations, separating them from the universe of study. However, they mutually refute these claims by showing that other anthropologists' explanations are not less true, but more socially determined, for all practical purposes in the same way as people's explanations are socially determined. Since there is no main anthropological theory which was not so criticized, it is impossible to uphold the qualitative difference between people's knowledge and explanations and anthropologists' knowledge and explanations, and the qualitative difference claimed by anthropologists again becomes redundant and therefore illegitimate.

II.

In the second section of this paper, I would like to discuss some methodological or, more exactly, interpretative consequences which necessarily follow when the above-mentioned sets of assumptions are put into operation. In our work, we are basically concerned with giving meaningful accounts of the things people do and say. Mostly, anthropologists

have very clear ideas about how to do it and what constitutes meaningful accounts. Let us take as an example the position formulated succinctly by Harris when discussing the emic-etic approaches. He uses, as an illustrative example, the lineage fission among the Bathonga of Mozambique:

> Now it is a regular etic feature of Bathonga life that the local lineage fissions when population exceeds 100 or 200 people, that the break involves the establishment of new households with a junior son and his mother at the core, and that the break is accompanied by all sorts of hostile expressions, including witchcraft accusations. To regard the fission event as a result of the inter-section of all of the codes that might conceivably have influenced the behaviour of the agnates (…) is a hopeless task. The fission of a Bathonga homestead is a cultural event and is not conceivable in any operational sense as a manifestation of a code. On the contrary, it is simply and clearly and operationally conceivable as an etic phenomenon in which the rate of fission expresses (…) the density and spacing of the animal and human population (…) [*Harris 1968: 610–612*]

Leaving aside ancillary problems, such as whether there can be etic phenomena or only etic or other statements about phenomena, or why a cultural event is necessarily not a manifestation of a code, I see two main questions arising from this quotation. The first is, why is Bathonga lineage fission an etic event? And the second, in what sense is the event that Harris studies in fact Bathonga lineage fission?

With regard to the first question, what happens is that a junior son and his followers leave the local lineage and establish themselves elsewhere. The son has his reasons for doing so. One of these can be that he feels in danger of being hurt or killed by witchcraft; another is that he expects to gain success and prestige, access to which is far more difficult for him in his original lineage; the third is, possibly, that he feels obliged to follow the rule, the fourth, for all I know, could be that it seems to him there are too many people around. It would be facetious to take Harris at face value and suppose that he has chosen the etic approach only because there are so many factors involved that it would be a "hopeless task" to try to sort them out. On the other hand, I cannot see why the fission of a lineage cannot be conceivable in any operational sense as a result of a series of decisions; after all, every Bathonga junior son conceives it thus and lives in a world where everybody else does. Thus, it would be more correct to suppose that Harris takes Bathonga lineage fission as an etic event because he believes that in this way he can say something more important about it – at least a thousand times more important, I should add, judging by his critique of Frake's attempt to derive the Subanum settlement pattern from emic rules: "Frake does not describe the actual pattern of dispersal of household sites. One description of such a pattern showing long-term stability or change in relationship to population size and production factors would be worth a thousand emic rules" [*Harris 1968: 603*].

Now, the lineage fission is presented to us not as a simple statistical concurrence of the type: "Whenever a Bathonga lineage reaches the size of 100–200 members, it splits up." If that were the case, we would be faced with a statement of the type: "Wherever the mailboxes are blue, the post functions more efficiently than wherever they are red." I am sure a number of appropriate cases could be found. For Harris, there is a causal relation involved: lineage fission is caused by the "density and spacing of the animal and human population under the techno-environmental conditions of Southern Mozambique". Differing from Harris, I believe that, unless we want to hold the position that identical environments produce by themselves identical technologies (which would amount to a rather

crude ecological determinism), given the environmental conditions, it is basically technology that determines the critical point of permissible density and spacing of the population. Technology, built up of knowledge, skills, methods, recipes, tools, equipment, etc., is an emic system par excellence, because it certainly consists of phenomena which are "significant, meaningful, real, accurate, or in some other fashion regarded as appropriate by the actors themselves" [*Harris 1968: 571*]; otherwise they would not have them and use them. Given this technology, when a group of Bathonga grows to certain proportions (according to Harris, 100–200 members) certain things start to occur which did not occur before: a drop in consumption, a feeling of the younger son of being barred from success, witchcraft accusations, etc. These problems, or more exactly the situation resulting from them, need to be solved. From our viewpoint, there are several alternative ways of solving them, such as a change in the technology or reorganization of the group. However, these alternatives do not exist in the Bathonga world: the only option they have is to split the group and thus get rid of the problematic situation. The move of one part of the original group to a new homestead is in this sense a fully emic event. Its only claim to eticity would then possibly hinge on the question of whether or not the members of the group are themselves aware that their growing numbers have something to do with it. That I do not know, but I would strongly suggest that in the absence of positive proofs that they are not aware, it could be very misleading just to assume it. If they are aware of this, then there is nothing at all about the lineage fission which is mysterious, meaningless or unaccounted for by the Bathonga themselves. And in such a case, the term "etic cultural event" does not amount to more than a shorthand term for a complex of emic cultural events, to be used only because to disentangle that complex would be a "hopeless task". Therefore, one significant dimension is chosen; this is ascribed etic status and used as explanans. Harris presumably considers the quantified basis of his conclusion and its predictive value as a proof that his choice of significance is correct. As to quantification, why could it not be said that the lineage splits whenever witchcraft accusations start to exhibit a determined frequency or intensity, or, for that matter, when the damage caused by witches amounts to so much? Should this statement be considered etic, too? As to the matter of predictive value, does Harris really mean that there could not be a lineage which splits with 99 members or stays united with 201 members? Almost certainly not. I do not doubt that he would have a valid explanation for any off limit case, but the point is that such an explanation would certainly be a contingent one. So, the prediction should not be read as: "given the techno-environmental conditions, it is impossible for a Bathonga lineage to split with 99 members, and to stay with 201 members." It should read as: "there are no known cases of Bathonga lineages splitting with less than 100 members and not splitting with more than 200 members." Again, the question arises, what makes it different from the prediction that the lineage will split with such-and-such a frequency of witchcraft accusations or such-and-such a frequency and intensity of witchcraft activities? It seems to me that the difference between both predictions, and therefore the eticity of Harris's statement, depends simply on his belief that population density is the "real" reason, while witchcraft is not. Why? Because population density makes sense to him, while witchcraft does not. That would make it Harris's emic statement, and not an etic one. Perhaps he calls it etic because population density makes sense to some other anthropologists. But, then, witchcraft makes sense to all Bathonga.

Now as to the second question, namely, in what way is the event that Harris studies Bathonga lineage fission? The fission is described as a necessary result of the growth of a lineage size to 100–200 members, under given techno-environmental conditions. When that occurs, a certain number of members move out and settle elsewhere. Even supposing that this is an etic event, what is cultural about it? When described like that, it belongs to the same category of events as the fact that when a herd of wildebeests consumes all the grass on one pasture, it moves to another one. The only difference, and therefore the only reason for calling it a cultural event, is that Bathonga fission occurs in a human group. Harris conceives of culture exactly in the sense I mentioned earlier: simply as an attribute of a class of phenomena called human groups. My point is that Bathonga lineage fission is not, and therefore cannot meaningfully be accounted for as, a simple transference of a certain number of persons from one place to another. If this were so, and if it were caused by pressure or population density only, it would not matter who actually left. Provided a certain number of people moved, the situation would be solved. However, Bathonga lineage fission is a structured event; it is the departure of a junior son with his mother and followers, every one of them being recruited according to certain emic principles. All of them depart because of their specific emic reasons. The departure of any other part of the group would not be a lineage fission. It is, therefore, a social event (or, to maintain the terminology, a cultural event) which cannot meaningfully be described in etic terms. We can do it, as Harris does, but only at the cost of depriving it of any social or cultural content and defining it as a "natural" event, that is, changing it into something that has no existential status whatsoever in the social world we are studying. The reasons Bathonga have for what they are doing are an indivisible part of the reality we are studying. To replace them with the reasons we take to be true means to deny the existence of a part of that reality. Since that reality is indivisible, it means, in practical terms, to deny its whole existence and to replace it with a reality we have shaped according to our reasons and purposes. It can be done (we should know – it was, and is, done often enough), but it should not be presented as the study of the natives' reality.

Another example will, I hope, make still clearer what I have in mind. The Mapuche in Southern Chile have female shamans, called machi, who officiate in cases of sickness. A machi performs for the sick person a healing ceremony which is more or less standard in the sense of having been described many times for many tribes. This consists of invoking her familiar spirits and with their help chasing away the spirit of sickness, by means of ringing magic bells, shooting rifles around the outside of the house, smoking over the patient, etc. Finally, she gives the sick person herbs for making tea. It is a system which practically offers itself for study as two separate systems of activities: one pertaining to the sphere of "ritual", which is derived from beliefs about the causes of sickness; and the other, pertaining to the sphere of "real curing", which derives from the machi's knowledge of the actual healing effects of the herbs. It was, is, and probably will be, studied many times over in exactly these terms. What I am suggesting is that Mapuche know it as a single and indivisible system of activities and that it has no other source, or expression of existence, than in the knowledge and actions of the Mapuche. When the observer applies his own knowledge and divides it in two, he is creating a thing which did not exist, and does not exist, in the reality he pretends to study. Once again, in practical terms it means that the object of his study has no empirical referent; it does not exist.

Holy's paper about Cewa sorcery is dealing with a very similar problem. One of the sociological ways of interpreting sorcery is to treat it as a sort of safety valve, or "strain-gauge", for social tensions. However, since we know there is no such thing as sorcery, we cannot really take into account the relations between a sorcerer and a supposed victim, because these are strictly imaginary: the tensions which are being vented, or resolved, must be between "real" persons who, in this case, can only be accusers and accused. Unfortunately, such an approach leads in the last instance, as Holy shows in his analysis, to a virtual denial of existence of an important part of the social reality we are supposed to be studying. We are using our knowledge and our methodology not to account for the existing phenomena, but to decide which phenomena exist. By doing this, we are making an unwarranted and unjustified comment on reality.

I would like to return once more to what I said at the beginning of this section, namely that we are basically concerned with giving meaningful accounts of the things people do and say – in other words, with explaining their social reality. The reasons why they do and say these things, that is, the knowledge from which their sayings and doings derive, are as much a part of that reality as anything else. As I have argued above, we cannot simply supply their doings and sayings with reasons of our own: we have to take them as an indivisible part of our object of study. We begin our study by observing activities, but to make only descriptive statements about activities is considered "ethnography" in a pejorative sense: only when we start looking for reasons for activity does the account or explanation truly begin. Looking for reasons necessarily involves assumptions about knowledge: to have a reason means to know that the activity will somehow work towards cancelling this reason. Consider, on the one hand, the statements: "I have a reason for wanting a metal axe: it is more effective than a stone one", or: "I have a reason for moving out of my lineage: my relatives try to bewitch me"; and on the other hand the statements: "There is a reason why South Sea peoples did not develop metallurgy: there are no metal ores there", or: "There is a reason for lineage fission: it maintains the optimum population density." The difference between the first and the second pair of statements is clearly seen: the first involves the knowledge of the actor and derives from it. The second does not involve the knowledge of the actor, but supposedly some impersonal (suprasocial) knowledge, in actual fact the knowledge of the observer. The activities of the actor are derived from reasons which do not exist for him. It would almost amount to a truism to say that, in most anthropological work, the first type of statement is considered a very low level explanation, if at all, and only the second type of statement is considered to be really significant. (Exceptions are found, especially in the fields of cognitive anthropology and ethnoscience.)

I have argued that, in considering only the second type of statement as really explanatory, we are denying the people's reasons the existential status we ascribe to their activities: the activities are "real", but the reasons are "only for them" or imaginary, and so their facticity is denied. In doing this, we are committing an ontological sin. At the same time, in trying to explain activities by reasons which did not enter into their shaping (and it would be difficult to deny that activities are consciously derived from reasons), we are perpetrating an epistemological absurdity.

One of the main grounds anthropologists have for trying to formulate the second type of reason is the assumption of our "rational" conception of reason and the natives'

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"irrational" one. This, of course, leads to the question of what is rational and what are the criteria of rationality, a question which is being widely discussed, mostly, if not exclusively, by philosophers. The problem is rather succinctly stated by Lukes:

> In what follows I shall discuss a philosophical problem arising out of the practice of anthropologists and sociologists which may be stated (…) as follows: when I come across a set of beliefs which appear prima facie irrational, what should be my attitude towards them? Should I adopt a critical attitude, taking it as a fact about the beliefs that they are irrational, and seek to explain how they came to be held, how they manage to survive unprofaned by rational criticism, what their consequences are, etc.? Or should I treat such beliefs charitably: should I begin from the assumption that what appears to me to be irrational may be interpreted as rational when fully understood in its context? More briefly, the problem comes down to whether or not there are alternative standards of racionality. [*Lukes 1974: 194*]

I can see in this quotation two serious problems which bear on my argument in this section. The first is a methodological one: how can I possibly "seek to explain" an irrational belief as irrational? The only possibility would be to say that people holding it are intrinsically irrational, for example, savages, pagans, or members of some other immutable category. This, however, would be just about everything I could say about it, and them, by way of explanation. Moreover, it would immediately be invalidated by showing that these same people also have rational beliefs or activities. To call a belief irrational means to close the door to any possible explanation. It can be understood and explained only by showing how it can rationally be held, that is, why and how it is rational.

The second problem, which seems to me to be even more important because it is possibly here that anthropology and philosophy part company, is: why am I so automatically expected to adopt an attitude, critical or charitable, toward the belief in question, or indeed toward any phenomenon contained in the social reality? I can legitimately inquire into the existence or non-existence of the belief, by asking if verbal assertions or ensuing behaviour show that people hold this particular belief. I can also inquire into the meaning and significance of their holding it; into the ways they manipulate it or are manipulated by it – or more exactly, are manipulated by their, and others', holding it. There are other fields of inquiry as well, which I will try to generalize later on. There is, however, nothing legitimate in my inquiry into the rational or irrational character of the things I am studying: firstly, because nothing useful can be gained "explanation-wise" by this distinction, and secondly, because by doing so I am making a gratuitous comment on the nature and, in the last instance, existence of the very things I am studying, thus adding to the reality something which did not exist before; that is, I am changing it.

I am afraid that the methodological implications I have been discussing in this section are so far only negative. They derive from specific, stated or unstated assumptions about the nature of the reality we are studying, and equally specific assumptions about what are "real" explanations of it, as distinct from satisfactory contingent explanations. My argument is practically identical with Welsh's:

> Positive sociology's attempt to use the natural science paradigm necessarily involves, then, assuming that social phenomena possess the same characteristics as natural phenomena. It is incumbent, therefore, on positivistic sociology to demonstrate this similarity. What sociological phenomenology, on the other hand, argues is that positivistic sociology seriously mistakes the

characteristics of the social world in assuming their comparability to those of the natural world and hence that positivistic sociology must necessarily constitute a mistaken enterprise. [*Welsh 1972: 16–17*]

Welsh then goes on defining the main differentiae of the natural and social world: basically, the natural world possesses no intrinsic meaning, while the social world is a world constituted by meaning [*Welsh 1972: 17*]. From this, it follows that: "(…) the social world is a subject, not an object world. It does not constitute a reality sui generis divorced from the human beings who constitute its membership. Rather, the social world is the existential product of human activity and is sustained and changed by such aktivity" [*Welsh 1972: 18*].

This means that we have to take the declaration that culture, or structure, or society, or whatever, is manmade, which so far has existed as a more or less empty credo, as an operational methodological assumption on which all our subsequent endeavours should really be based. Social reality is a process, continually created, maintained and changed by meaningful activities of men. It is not a world composed of facts external to men and, to any extent, independent of them. This is its main characteristic, its main ontological feature, and as such should determine all inquiries and studies. "Social order is the emergent product of human activity and the manner of its emergence, therefore, must become the central concern of sociological investigation" [*Welsh 1972: 20*].

Thus, our main concern is not with what institutions exist; what is their specific history, independent of how they are historicized by the natives; what are their specific functions, in reference to the structuring principles of the natural world; or why and how they exist independently of what people take them to be and mean. Our concern is with the fact that people live in a known, agreed upon, social world, whatever the actual mechanics of that agreement might be, which they continually create, recreate and change by their activities. Their socially acquired knowledge makes it possible to manipulate the phenomena of this world and at the same time puts certain constraints and limitations on them, which are of course basically selfimposed. This double process of manipulating social phenomena and being manipulated by one's knowledge of them shows properties that make it possible for us to formulate meaningful generalizable explanations. This I take to be the main task of social anthropology.

III.

In the third and concluding section of this paper, I shall on the one hand give some attention to various concepts that I have so far taken more or less for granted, and, on the other hand, consider some implications of the methodological principles outlined in the second section. The title of this volume contains an implicit assumption of the necessary relationship between knowledge and behaviour. It is assumed that behaviour derives from the actor's knowledge (see Jarvie's argument against the thesis that "belief does not explain action"; [*Jarvie 1964: 149 ff.*]). This is based on the axiomatic conception of human behaviour as purposive, as goal oriented. I call it axiomatic, because it cannot be proved or refuted without incurring tautologies. However, without accepting it, we could not do any systematic work in the social sciences, unless we accept the natural science paradigm. This is possible only at the cost of denying the social universe that which makes it a specific universe of study, that is, its social character. Activity can be conceived of as purposive or

goal orientated in a meaningful way, only if it is seen as a result of reasoning out that it has something to do with the attainment of goals. Such reasoning out is possible only if we ascribe to the actor necessary knowledge, not only of the goal and of the effectiveness of the action, but in general of phenomena and their relationships in the relevant section of the social world.[2] I accept here Berger and Luckmann's definition, in their own terms a very simplistic one, of knowledge as "certainty that phenomena are real and that they possess specific characteristics" [*Berger – Luckmann 1966: 13*]. It is, of course, irrelevant what form or kind of reality the phenomena have or have been ascribed. Given this definition, the difference between knowledge, belief, superstition, etc., becomes meaningless, since all of these terms mean taking certain phenomena as existing, in whatever way, and as having specific attributes and mutual relationships. Therefore, my use of the term knowledge should be understood in this sense. If belief implies anything other than knowledge, then Azande do not believe they have witches: they know it in the same sense Christians do not believe in the existence of God: they know He exists. The only possible difference I can think of between knowledge and other types of certainty about phenomena would be the existence or otherwise of tangible proofs, but this is a rather tricky point and, moreover, for our purposes, an irrelevant one. If somebody acts on the basis of his certainty that such-and-such exists and has such-and-such attributes, it does not greatly matter what is the basis of his certainty.

As I have mentioned already, many actions (this term includes non-verbal actions, as well as verbal ones, such as assertions and propositions) that we can observe are not, in actual fact results of long reasoning processes indulged in by the actors at every single opportunity. The actor just knows beforehand, automatically, or because he was "taught" so, that such-and-such an action leads to such-andsuch a goal, or that a certain proposition truly describes certain phenomena and their attributes. When a Bathonga junior son goes away to save himself from witchcraft threats, he does not inquire whether or not previous cases of lineage splitting led to the cancellation of such threats, or why it is that witches can function only within the group and not outside of it. He just knows that the safest way to deal with witches is to move away. Similarly, we know how to deal with the lack of cigarettes without needing to go into deep meditation or problem-solving mental process about it. We just go to the nearest shop, hand over the appropriate sum of money, and get them. All this knowledge we acquire during our social life. Since these and most other human activities are social activities, in the sense that they consist of behaviour towards or with regard to other people, they belong to the intersubjective world of shared meaning [*cf. Phillipson 1972: 125*].[3] Social acts are purposive and goal-oriented not only for the actor, but also for specific others. Therefore they are normative, taking as a necessary minimum of normativity the comprehensibility of the act to others, at some level of social grouping. And this is also that level of social grouping which shares with the actor the knowledge of a given section of the social world.

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[2] This cannot be understood literally: I do not mean that any single activity is always preceded by a long reasoning process: for many activities we are simply "taught" that they are effective. others have become quite automatic. However, the connection between activity and the attainment of the goal can always be made explicit.

[3] I do not intend this as an opening for scholastic discussion of what is and what is not a social act. I do not doubt that there are parts of human behaviour which cannot be defined as social acts, these however should interest us about as much as data about bees and butterfties.

The behaviour then can be explained by the knowledge from which it derives, or, more exactly, behaviour can be accounted for as rational, purposive or goal-oriented, only in the context of the world known by the actor. This proposition confronts us with a peculiar problem that is absent in the natural science paradigm. If what we are explaining is the action, and what we are explaining by it is the knowledge of the actors, we should have independent means of establishing the existence of that knowledge. If, on the other hand, what we are explaining is knowledge, and what we are inferring it from are the activities, we should have independent means of establishing the rationality or goal-orientation of the activity. Moreover, we can never establish the existence of knowledge or the rationality of the action directly, since they both exist "within the people's heads". We are constantly presented with a "black-box" problem, for which the only solution is to infer the existence of the contents from outside indications. Practically, the only evidence we can have that an individual has certain knowledge or belief is his verbal assertion to the effect, and/or the fact that he behaves accordingly. This seems to be relatively straightforward and unproblematic if a particular piece of knowledge can be seen as accounting directly for a non verbal action.

If we see a Bathonga junior son moving out of his lineage, giving as the reason his fear of witchcraft, and if we are able to obtain from him an assertion that there are witches and that they are dangerous or possibly even that the only safe defence against witches is distance, we can consider this particular case closed. By the same token, if we see a Chilean paying to a Mapuche Indian a very low price for his sheep, and if we can obtain from him the information that Mapuche are lazy drunkards and would spend the money on booze anyway, we have again ascertained the Chilean's reason for action and the knowledge on which it is based. In both cases, we account for a particular action by demonstrating that there exists not only the knowledge that this action will bring about the desired goal, but also the knowledge that in the relevant section of the universe such-and-such phenomena exist and have such-and-such relationships. However, we are still moving in a relatively closed action-knowledge dyad. The verbal statements in both cases can be reformulated as action dispositional propositions of the type: – if there is no stopping of witchcraft activities, I will move away; or: – if I buy a sheep from a Mapuche Indian, I will get it cheaper than from a Chilean. At best, we are able to show that the relevant knowledge is held by the actors.

However, let us consider statements which are, or can be taken as, assertions of knowledge, such as a Bathonga stating that there are witches, or a Chilean stating that Mapuche Indians are lazy drunkards. To account for such statements by showing the existence of relevant knowledge would be pointless, since that is exactly their purpose. Clearly, the problem here is not to demonstrate that the knowledge exists, but why it exists. This can be done by showing its relation to and congruence with other instances of knowledge in the relevant section of the social world, by demonstrating that there are social conditions which account sufficiently for it being held, and by relating it to a broader context of past and present actions. This is the basic procedure which makes it possible for us to move from the knowledge-action dyad to an ongoing process of generating activities on the basis of existing knowledge, of adapting knowledge to existing circumstances, and of manipulating circumstances by goal-oriented activities. I have tried to show some interpretative implications of this procedure in the paper on contemporary Mapuche land tenure later in this volume.

However, the fact that the knowledge exists "within the people's heads" leaves us still with one problem, usually formulated as: "How do I know he believes or knows what

he says he believes or knows?", or, to fall back on Berger and Luckmann's definition that I mentioned earlier: "How do I know he holds to be true, or existing, what he says he holds to be true, or existing?" When a man says, for example, that he believes in God, and fulfils all the usual obligations of a true believer, how can I say whether he really believes? In the same sense, we can also ask if Bathonga really believe in witches, or if Chileans really hold true that Mapuche Indians drink excessively. It is conceivable that a politician can pretend he believes in God just to obtain votes, that a Bathonga junior son pretends he believes in witches just to have a reason to move out of his lineage, and a Chilean pretends to know that Mapuche Indians drink excessively just to legitimize his swindling them. There are, I believe, two possible answers to this problem. The first is, that the problem does not, in any important sense, arise. The explanation of subsequent actions by relevant knowledge is not invalidated by the fact that a member of the society under observation is able to simulate the reasoning. In fact, even such a man has to operate on the basis that his behaviour will be accounted for by others as following from the intersubjectively shared knowledge, that is, he has to behave so that his activities can be seen by others as derived from that knowledge. The second answer is that in important cases there usually are ways of recognizing that a man operates on simulated knowledge, for example, by studying his behaviour in incompatible situations.

If, for instance, the above-mentioned politician is only pretending to be Christian to obtain votes, then presumably he will behave differently when he tries to obtain atheists' votes. If not, then the difference between pretending to believe and believing has no relevance for activities and becomes an academic one. David Riches discusses a somewhat similar point in his paper on alcohol abuse in a modern Eskimo settlement in Northern Canada in this volume (published previously in Belfast), though not as the central problem of the paper. Canadian officials who are going to work in an Eskimo settlement are informed beforehand that there is a flourishing cooperative functioning there, set up by their predecessors. After their arrival they find that this is, in fact, a sort of "report-reality" and that the cooperative is functioning, in actual fact, rather badly. Since the existence of a flourishing cooperative is, nevertheless, firmly established in all previous reports, they are afraid that a realistic description would bring the blame on them, and they therefore prefer to maintain the fiction: in other words, they pretend the existence of a certain set of phenomena. Their behaviour is thus based, in one important sphere, on a makebelieve reality.

Despite the use of these and other methodological procedures, the study of people's knowledge, reasons, etc., necessarily means that a lot of the anthropologist's knowledge or, more exactly, conjectures are always being brought into it. This is usually considered as having been invalidated by Radcliffe-Brown's famous argument against "if-I-were-a-horse" reasoning. Since this polemic classification is often considered as refutation without any further proof, I believe it could be useful to look into its real merits. To begin with, let us have the whole story. Radcliffe-Brown alludes here to Mark Twain's story about a boy who, seeing that his horse was not in the enclosed pasture, went and found him. When asked how, he answered: "Well, I went to the pasture, went down on my knees, ate some grass and asked myself: now, if I were a horse, where would I go?" As a joke, it never fails – but as an argument? Let us consider an alternative story: a farmer was looking for his lost horse around the house; a friend asked him if he had any reason to think the horse might be there – the farmer answered: "Well, I really don't know, but it is quicker

and more comfortable to look for him here." The moral is that you have to look for your horse where he possibly might be, and he will be in a place where it is comfortable for him to be, and not necessarily comfortable for you to seek. So, if you do not get on your knees and eat some grass, you might never find him. Actually, there are other reasons to make more feasible my position that the "if-I-were-a-horse" way of acquiring explanations, is in actual fact, the only way to get them, and, at the same time, the way which anthropologists, despite their approval of the funny argument, really use. If you ask anybody what is the most distinctive feature of the anthropologist's work, you will most probably be told that it is long term fieldwork. Now, most of the anthropologists who went through fieldwork take particular pride in how well they got on with the natives, how perfect a "rapport" they had. This was possible only because they were able to behave, maybe not as natives, but at least in a way comprehensible to them, in a way that fitted into some part of the natives' knowledge. To be able to do that, the anthropologist has to make a series of guesses and conjectures about natives' knowledge and appropriate behaviour: these guesses, or more exactly his behaviour resulting from them, are subjected to the same sanctioning processes as the behaviour of any member of the group he is studying, though possibly in a different way. In other words, if his conjecture is wrong, he gets his fingers rapped. After some time he obtains a reasonable working knowledge how to move around, which also means a reasonable knowledge of knowledge and reasons behind actions. Ethnoscientists consider that an ideal ethnographic description should be the sum total of what a man must know to be able to behave in a culturally appropriate manner [*cf. Howard 1963: 409*]. After all, I have said already that the social world is a world of intersubjectively shared meanings. If they are shared, they can be taught and learnt. And the anthropologist is in a better position to learn them than anybody else, save the natives. Incidentally, this also demonstrates that fieldwork is not only a necessary initiation rite for young anthropologists, as some would have it, or a deep spiritual experience during which a layman dies and an anthropologist is born. It is the most effective and, I suspect, the only possible way to obtain the descriptions and accounts we need for our work.

Now, what I have said so far could be misunderstood as equating an anthropologist's work with the work of a well informed reporter, with the difference that people's reasons and knowledge are brought in more systematically and consistently. That is definitely not my position: I am not trying to say that we should do what Lewis, for example, did in his Latin American studies [*e.g. 1959, 1966*], that is, give straightforward accounts of what people do and why they say they do it. Such accounts are, of course, necessary starting points, since they are the reality we are studying, with the purpose of giving it more "meaning" than that which is directly apparent on the surface. I have argued that what is broadly called "positivistic social science" simply substituted another meaning. Phenomenological sociology, which declares itself to be opposing it on practically all points, usually presents Garfinkel's ethnomethodology as its practical accomplishment. Now, Garfinkel's position, simplified almost beyond recognition here, I am afraid, is that people behave in a meaningful way because they provide, as they go, meaningful accounts of what they are doing: meaningful in the context of largely taken-for-granted, shared knowledge. What he proposes as the main task of sociological undertaking, in fact as the only legitimate task, is to make explicit this taken-for-granted knowledge and the methods people use to make their accounts, and therefore their actions, comprehensible and meaningful:

> Their (i.e. ethnomethodological) study is directed to the tasks of learning how members'
> actual, ordinary activities consist of methods to make practical actions, practical circumstanc-
> es, commonsense knowledge of social structure and practical sociological reasoning analysable;
> and of discovering the formal properties of commonplace, practical commonsense actions, 'from
> within' actual settings, as ongoing accomplishments of those settings. The formal properties obtain
> their guarantees from no other source and in no other way. [*Garfinkel 1967: vii–viii*]

However, what has been done by way of research (or, more exactly, experimentation) in ethnomethodology so far, tends simply to show the existence of this taken-for-granted knowledge, mostly by the expedient of forcing people into situations where it does not apply and where they are necessarily confused. Firstly, I do not believe that this makes the taken-for-granted world more than commonsensically apparent; after all, many writers use the device of letting their actors behave at odds with situational circumstances. Since no accounting for the taken-for-granted knowledge is being done or even sought, the ethno-methodologists tend to move in closed "knowledge-action" dyads. That does not appear to me as the only possible or even the most useful method of handling the problem. Secondly, it seems that the mutual legitimization of actions and accounts is taken as the only purpose of social behaviour, goals are, I suppose, seen as private or individual. Only the ways of reaching them are to be systematized and legitimized. I would argue that actions are made meaningful or rational when they can be considered, by specific others, as accomplishing, on the basis of shared knowledge, commonly approved goals. Often a commonsense clas-sification of behaviour as irrational does, in actual fact, mean not that the action as such is irrational, but that the goal to be reached by it is so.

Instead of a final summit up of the arguments presented in this paper, I would like to point out a few ways in which not only activities and "action-dispositional" knowledge, but also taken-forgranted knowledge can be explained or accounted for in a more mean-ingful manner and the principles of its organization exhibited. When anthropologists are accounting for actions by norms, they are making the system of actions and system of norms practically coextensive (Barth, in a different context, says: "(…) one form, set of regular patterns of behaviours, is translated into another, virtually congruent from, made up of moral injunctions (…)"; [*Barth 1966: 2*]). The proportion of one norm to one activ-ity or one set of activities is sought, I would argue, that this is impossible. We can take the example of postmarital residence of the patrilocal Berti society in Western Sudan, where, regardless of the norm, 22.9% of marriages are matrilocal and neolocal [*Holy 1974: 112, 115*]. Anthropologists traditionally assume that, in such cases patrilocal residence is explained by the norm and what is problematic are only the non-patrilocal marriages, which are usually explained by introducing contingent factors. This is clearly erroneous. If the norm of patrilocality can explain or account for actual marriages, then all marriages should be patrilocal. If, on the other hand, there exist contingent factors, these should be taken as explanatory tools not only for nonconforming marriages but also for conforming ones. In other words, all residence should be accounted for by the same principles: their relative strength or weakness, or presence or absence, must be taken into account. Any action, even the norm conforming one, would thus appear as a result of the conjunction of many and various factors, norms, etc., in short, as a result of the decision of the actor. This decision is taken on the basis of his (and others) taken-forgranted, shared knowledge. However, neither the action itself nor its account by the actor or others can bring this

knowledge, or even the situationally relevant part of it, systematically into evidence. The very "taken-for-grantedness" makes it impossible for them. They are usually able to state an abstract model of the action ("this is how we do this"), and an account of an actual case based on contingent principles, but not the relation between the two. A Mapuche Indian, when he speaks about crop-sharing, defines it as a strictly economic agreement where both partners are chosen on economic grounds only, one has land, the other seeds, and so on. However, when accounting for a particular pair of partners, he will state, as the reason for their working together, that they are first cousins. Similarly, Cyrenaica Bedouin state that in blood-feud they kill any member of the tertiary section of the guilty man. If they had to explain why they did not kill this particular member, they would say "(…) he is my brother-in-law" [*cf. Peters 1967*]. In such cases, the contingency of the second explanation is not seen as contradicting the model, because they do not belong to the same "norm-game". The anthropologist can, by combining these isolated, schematic and broken accounts and actions, make explicit and systematic the whole or a significant part of that taken-for-granted world, and thus show how what is showing only partly and incidentally, works. On the basis of this, he can further show what are the general organizing principles of the people's knowledge and how they make possible the application of knowledge to behaviour. However, he can only show them; he cannot, legitimately, supply organizing principles: his task is, at best, to make them coherent and comprehensible.

There is another dimension of the taken-for-granted knowledge which makes it possible for the anthropologist to bring out its systematic qualities. This is its unequal distribution within the society. It was, and to a large extent still is, assumed that the "knowledge of a society" is formed by the sum total of the knowledge of its members, and the norms are formulated on the basis of this total knowledge. This is what makes it possible to speak about nonconforming, or even deviating, behaviour as an objectively valid category. However, it would seem obvious that there can be no adding up of knowledge. I can live in a society which sent a manned rocket to the moon, but the relevant knowledge involved in this, or any significant part of it, does not form a part of my known world any more than of the known world of a member of any society that did not do it. The society of which I am a member may have a long and varied history, but only a very small portion of that history, a few selected episodes, belongs to my meaningful world. By the same token, there is no reason to suppose that complicated and abstract genealogical models belong to the common knowledge of all members of a given tribe. In other words, every member of a society has his field of knowledge, his taken-for-granted reality, which overlaps with those of other members, some of them almost completely, some of them to a variable extent, and some of them minimally. A member of the society can, at best, only envisage the existence of this differentiation. The anthropologist should be able, by continuous systematization of his observations, inferences, accounts, etc., to explain their existence and mutual relations, more so since he is not necessarily bound to any one of these fields.

The methodological position I have been trying to outline in this paper can be summed up very briefly as follows: an individual's behaviour is seen as the result of a series of decisions taken on the basis of his taken-for-granted knowledge about the universe; the knowledge is shared by specific others. That is the social reality we are trying to explain. He is able to account for his behaviour and state his knowledge in contingent, episodic and anecdotal ways, because of its "taken-for-grantedness". However, a detailed study permits

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us to present both his actions and his knowledge in a systematic way, together with the principles by which he organizes them. The fact of action being taken as result of a series of decisions means that the individual is not just a "norm-fulfilling unit"; he is, within limits given by his knowledge, manipulating his social world. Again, a detailed study should make explicit the mechanics of this manipulation. And finally, his knowledge is not something which is given or intrinsic to him: the anthropologist has also a legitimate problem in explaining why this particular social reality came into being, in terms of the broader social conditions to which it is related.

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