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Ladislav Holy and Milan Stuchlik

Excerpt

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Introduction

It has become almost a feature of current theoretical discussion in social science to stress that there is a basic division of approaches; more exactly, that there has emerged, and become established, an alternative to what might be called traditional or normal social science. The terminology applied to this division varies: some authors talk of positivistic and non-positivistic social science, others of explanatory and interpretative approaches, or of collectivistic and individualistic studies. Whatever the variation in terms, the division is always roughly the same and has ultimately to do with assumptions about the status of man in the world. For our purposes, and especially in anthropology, the same division can be referred to in somewhat simpler, or more descriptive, terms: according to the basic problems which orient the research, we can speak about approaches that study how societies, social systems, or structures function, and approaches that study why people do the things they do.

Every self-respecting anthropologist would immediately object that good anthropology does both at the same time; and, since such an objection is, in some sense, justified, some clarification is necessary. Anthropology, probably more than any other social science, has always been rather strict about long-term fieldwork, participant observation, and other research techniques developed specifically for obtaining information about what people actually do, down to the most minute details. At the same time, the analysis and explanation of obtained data has usually been carried out in terms of society, social system or social structure: possibly this has been done more consistently in anthropology than in any other social science, because anthropologists have been dealing predominantly with small-scale societies with comparatively simple organization, where the system, or the structure, or generally the total social whole is more immediately conceptually available. Thus, it could be said that, in a way, anthropological research has been oriented towards both problems at the same time. The divisive point, however, lies in how these problems have been handled.

The interest in totality, in social system and its functioning, has been the central interest at least of British anthropology for the greater part of this

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century. Though few anthropologists would want today to agree with Radcliffe-Brown's bold assertion that systems of society are as real as biological systems, the Durkheimian notion of society as objective reality is implicitly upheld in most actual research. The fickle and idiosyncratic activities of concrete individuals have to be studied in as much detail as possible, but what is important about them is not their individuality but how they are patterned, how their existence can be explained in terms of society and its component groups and institutions, and how they, at the same time, manifest the existence of society and its component groups and institutions. Though 'what people actually do' might enter into such procedures at a descriptive level, the question 'Why do they do it?' is never properly asked; more exactly, it is considered to be synonymous with the question 'Why are such things done?', and the explanation is given in terms of the functioning of the society or a relevant part of it.

Still in the minority, but already established, there have emerged approaches which deny the legitimacy of the question of 'how the social system functions' unless it is answered in terms of the reasons, intentions and purposes of the people who somehow form or create the social system. For these approaches, concrete activities enter into research procedures not only at the descriptive level but at the analytical and explanatory level as well. The social system is not something behind the recurrent pattern of activities, but emerges from them, is created and changed by them. Therefore, it can hardly be used to explain them.

Thus, the division is not simply about whether or not research takes into account society on the one hand and people's activities on the other. It is basically about the autonomy of agency: if society, or structure, is an objective reality to whose demands people respond in specific ways, then it is an autonomous agency and individual people are its agents, and the only acceptable explanation is in terms of the functioning of the system. If, on the other hand, society or structure emerges from, and is maintained or changed only by what people do, then individuals are autonomous agents and systems are consequences of their actions and, in the last instance, explicable by them.

There is another aspect of this division which needs to be mentioned: the nature of things social. The 'structure' or 'society' school of thought conceives, with differing degree of explicitness, of 'social' as meaning 'independent of individuals'. Social reality is external to individuals, beyond and above them. Even if it will be admitted by the adherents of this approach that in the last instance things like collective representations exist, in the vulgar sense of the term, in people's heads, they were put there from the outside: therefore they must exist outside as well. Things pertaining to individuals are things psychological, not social. Objectivity, in the sense of independence of vagaries of individuals, is the essential property of social phenomena. The 'action' school of thought conceives of 'social' as that property of phenomena which

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makes them known, understandable and meaningful to those directly involved with them. This difference is, obviously, directly related to the autonomy of agency. For the first position, 'social' is that property of phenomena which puts them beyond the possibility of manipulation by individuals. For the second position, 'social' means created and manipulated by individuals, in ways understandable to other individuals.

This essay, taken as a whole, is an argument for the second position, i.e. for the approach oriented by the problem 'why people do things they do'. We intend to formulate a set of working assumptions about the nature of social reality and its availability to the observer, and point out the consequences of these assumptions for research procedures. However, to present such an argument has some problems of its own. In theoretical discussions within their own discipline, anthropologists pay considerable attention to the techniques of obtaining data and to the problems of their analysis and explanation. Inquiries into basic assumptions, the epistemological and ontological problems, have been left, by a sort of tacit agreement, almost exclusively to philosophers. In other words, it has usually been accepted that theoretical problems *in* anthropology should and will be discussed within the discipline, but theoretical problems *of* anthropology should be discussed from the outside. Unfortunately, as Gellner noted (limiting himself to linguistic philosophy, but it seems to us that his comment can safely be extended to philosophy in general), philosophers do not have any 'accurate, close or up-to-date acquaintance with the actual working of social studies' (1959:230). Therefore, their discussions have tended to be rather remote from the actual practice of anthropology, and a whole range of important problems have remained, so to say, in suspension. Or at least such was the state of affairs till a comparatively short time ago. Over the last few years, however, the situation has changed rather drastically, though this change has been more notable in other social sciences, especially in sociology. The interest in philosophical discussion, and the need for philosophical backing, has grown to such an extent that some critics now comment that theoretical sociology is rapidly becoming second-rate philosophy, and is getting further and further away from the actual research carried out in it.

This is not a trend we particularly wish to follow. Though the importance of a clear philosophical basis or backing for any approach in social science cannot, in our view, be stressed enough, we feel that there is some justification for the second part of the criticism. The more philosophical a theoretical discussion in anthropology becomes, regardless of whether it is good or bad, first-rate or second-rate philosophy, the more difficult it usually is to relate it to practicalities of research; hence the main problem of our argument: even if we felt competent to discuss from first principles, which we do not, it might be counterproductive to our purpose, which is, as we mentioned above, to relate a set of basic assumptions about social reality, boundaries of social science, and the status of man as an autonomous agent to the procedures of actual research.

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Or, to put it another way, though this essay is about anthropology and not in anthropology, we still wish to keep the discussion within the discipline.

For these reasons, we have chosen the method of using extensively what might be called ethnographic examples, except that our ethnography is quite often constituted by the practice of anthropology. In other words, we are applying the question of ‘Why do people do the things they do?’ to some particular instances of anthropological research, asking ‘What assumptions make this research possible?’ and ‘Why are these conclusions reached?’ Admittedly, this may make some passages sound rather polemical; however, this was not our intention. We are not primarily concerned with criticizing particular instances of research or particular approaches. Our primary concern is simply to use the examples for the clarification of some of the points we are trying to make, both in positive and negative ways. Straightforward ethnographic examples, often quite lengthy ones, are used for the same purpose. A number of these are based on our own research which has, to paraphrase Geertz, whatever its other faults, at least the virtue of being our own – a distinct advantage in a discussion of this sort.

This method makes it possible, we hope, to show the importance of any specific methodological stance for, and its close connection with, concrete research and its procedures. Undoubtedly, some of the terminology we are using is rather simplistic, but it has the advantage of not being confusing. Whenever more precise definitions or circumscriptions are needed, we try to supply them. At any rate, simplicity of terminology, if it does not detract from the clarity of the contents and the consistency of the argument, can hardly be considered a fault.

Most of the specific problems we are discussing in this essay have been present, in some form or other, in the majority of theoretical discussions in social science during the last decade. We cannot claim to be saying much that is startlingly new. However, we are trying to combine several points or questions which have often been raised separately and present them in a systematic fashion. We are also trying to expose some of the more obvious misunderstandings adhering to the interpretative school of social research and some of the conceptual fallacies or confusions of the explanatory school. To be able to do this, we have to go over ground that is fairly well covered in current methodological discussion and repeat what might be considered obvious things. Unfortunately, a certain degree of obviousness and triviality cannot be avoided: we hope that it may at least make clearer the consequences of the points which will be raised, both for the definitions of problems and for the types of research procedures which will be outlined later in this essay.

1

Anthropological data and social reality

‘Participant observation’ is without doubt one of the most important stock-in-trade terms of social anthropology. It conveys the image of research carried out directly among the people one is studying, usually for a considerable length of time, carefully observing and documenting minutiae of their day-to-day life. ‘Having been there’ and ‘having seen this and that done’ is the ultimate guarantee of the veracity and accuracy of any information divulged about those people. If it were customary to publish, or otherwise make available, not only the papers and monographs resulting from research but also the actual fieldnotes and the recordings of data, it would soon emerge that ‘participant observation’ is a blanket term for a broad range of ways in which the information comes to the observer. When we read in a monograph that, for example, tribe X subsists on shifting cultivation, we may assume that this information has not been obtained by the researcher participating in any shift of the fields: fieldwork is usually not long enough for that. What it presumably boils down to is that the anthropologist noted the distribution of currently cultivated fields, observed traces of more or less recent cultivation on other patches of land, was told by the villagers that they had cultivated these patches, and possibly that after some time they would clear and cultivate some hitherto unused land. When an anthropologist describes the family in society Y as having an authoritarian structure with the father at the head, he will again have derived this from a number of more specific data in his fieldnotes: observed instances of fathers behaving in what may be called an authoritarian way, descriptions of what fathers may do or order to be done, and possibly opinions of people on how the family should be or is organized. Some of these data were obtained, undoubtedly, through observations; some of them, equally undoubtedly, came to the anthropologist in the form of expressed opinions, value judgements, etc. Unless we understand ‘participant observation’ in a rather simplistic sense of ‘being on the spot’, these latter forms can hardly be the result of it.

To say that tribe X has shifting cultivation, or that tribe Y has an authoritarian family structure is conveying information, but strictly speaking it is not

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giving data, unless we can specify to what such information refers. In the first case this can be done: we can say that 'shifting cultivation' refers to a pattern, or sequence, of actual observable processes. In the second case the task is more difficult: does authoritarian structure refer to the pattern of observed authoritarian acts of fathers, to the set of orders any father can give, or to a number of expressed opinions? We suggest that in normal anthropological procedure it often refers indiscriminately to all three; it includes all these and other kinds of data, and the differences between them are annulled by putting them all under the same heading, as results of participant observation.

In this chapter, we intend to discuss the differences between kinds of data and the question of their reference. More specifically, we wish to argue two main theses. The first is that the differences in data are not merely the consequence of differing data-gathering techniques or ways in which the information comes to the knowledge of the observer, but the consequence of their referring to different levels or domains of social reality. In other words, differences in data often connote the existential difference between levels or domains of reality. Therefore, social reality cannot be conceived of as a unitary system. The second is that the fact that social reality has often been conceived of in this way has led to its misrepresentation and to incorrect formulation of problems and problem-solving procedures. Seemingly simple and non-problematic techniques like participant observation, or even field-work, have played quite an important role in this.

The last major discussion of the nature of data and their proper use in social anthropology took place in the 1960s, with the development of situational analysis or the extended-case method (cf. Gluckman 1961, and Van Velsen 1967, for review). The earlier, and at that time still predominant, position can best be characterized by a well-known quote from Radcliffe-Brown, describing the procedure of science as not being

concerned with the particular, the unique, but only with the general, with kinds, with events which recur. The actual relations of Tom, Dick and Harry or the behaviour of Jack and Jill may go down in our field note-books and may provide illustrations for a general description. But what we need for scientific purposes is an account of the form of the structure (Radcliffe-Brown 1952:192).

In his view, particular data, of whatever kind, are to be used as bases for generalized description, this description rendered as a structure and eventually illustrated by aptly chosen particular data. On a very small scale, this is exactly the process whereby the family in the above-mentioned society Y comes to be presented as having an authoritarian structure. The practitioners of the extended-case method criticized particularly this way of handling concrete data. They pointed out that in actual fact the data were not used for analysis at all: they were merely illustrations for the structural schemata devised by the anthropologists.

For situational analysts, particular data, actual interactions, the observed

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cases were something which had to be the subject of analysis. Their regularity or patterning, the structure, should only be elicited directly from these cases and also demonstrated on them (Gluckman 1961:10–11). Thus, the style of analysis this approach proposed was considerably different from that upheld by the preceding approach. However, the criticism, fully justified as it was, did not go far enough and the extended-case style of analysis still did not solve the main problem. The step from structure to actual cases had been an important one, but let us consider what it meant in more practical terms. A particular case analysed by an anthropologist often extends over a considerable period of time, starting long before he comes to the field (cf. Mitchell 1956:95 ff, 116 ff) and evolving during his stay there. His data about it are thus formed by a rather varied collection of information, consisting of informants' reports of past events, their justifications and explanations of past events, his own observations of present events, informants' reports and justifications of present events, etc. Yet all these data inform him, more or less in the same way, about the case viewed as a pattern of events, and in the last instance about the form of life or social structure. The differences in data are still treated as incidental; in other words, 'data' is still a unitary concept, a sum total of information obtained about a particular case and through it about social structure conceived of in an equally unitary way.

Of course, the whole discussion was again about the problem of reference. Addressing anthropological analyses in general, rather than situational analyses in particular, Leach succinctly posed the question of 'how far the anthropologist's concept of social structure refers to a set of ideas or to a set of empirical facts' (Leach 1961a:5). Let us examine this question with the help of a concrete example: segmentary lineage structure. The amount of fieldwork which the concept of segmentary lineage structure has stimulated and the prominence it has attained in theoretical and methodological writings clearly indicate that its formulation has been considered one of the most important achievements of social anthropology. It seems justified, therefore, to use it as a 'case study'.

So, when an anthropologist concludes that such-and-such a society has a segmentary lineage structure, what does he mean by it? There seems to be a considerable degree of consensus that he is referring to a set of notions held by the members of that society, or, in Leach's terms, to a set of ideas. Evans-Pritchard asserts this when he mentions that the principle of segmentation and opposition between segments 'can be stated in hypothetical terms by the Nuer themselves' (Evans-Pritchard 1940:143). Fortes expresses the same view when he states that the paradigm of the lineage system of the Tallensi 'is in the mind's eye of every well-informed native when he discusses the structure of his society and takes part in the public affairs' (Fortes 1945:30). Similarly, Southall sees the concept of segmentary lineage structure as a part of the natives' 'projective system' (Southall 1952:32). Talking about the Nuer, the

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Tiv and the Bedouin, Lewis points out that the ‘political-jural ideology is uncompromisingly one of descent’ (Lewis 1965:97). He clarifies what this ideology is meant to be by a quotation from Middleton and Tait: ‘co-ordinate segments which have come into existence as a result of segmentation are regarded as complementary and as formally equal’ (Middleton and Tait 1958:7). The fact that the descent or lineage principle is referred to as ideology indicates that it is taken to be a notion held by the actors; it is a part of their conceptual universe. This view is expressed by Middleton and Tait themselves (*Ibid.*:76). Peters (1967) emphatically presents the concept of the segmentary lineage structure as the Bedouin folk model and Salzman (1978a) and Seddon (1979) talk about it as the actors’ ideology.

So far, there is no problem. The segmentary lineage structure is a set of notions which members of some societies hold about the proper organization of their social relations. They have them in their ‘mind’s eye’ and are able to tell the anthropologist that they hold them, even to discuss them in hypothetical terms. However, a considerable number of anthropologists present the segmentary lineage structure, implicitly or explicitly, also as a representation of ongoing social processes, or, in Leach’s terms, as referring to a set of empirical facts. The actors not only hold and discuss these notions, but also make them manifest in their behaviour, organize their actual social relations and activities in terms of a segmentary social structure. This is tacitly implied in all the classifications which distinguish it as a specific political system (Fortes and Evans-Pritchard 1940; Middleton and Tait 1958). The view that the concept of the segmentary lineage structure refers to actual social processes has been subscribed to, with qualifications, by Gellner (1969:62–3), and recently by Salzman (1978a) who argues that even in societies where the dictates of segmentary lineage structure are not always followed in behaviour, the lineage ideology can nevertheless be seen as having some constraining effect on behaviour. Some anthropologists, notably Fortes, have gone even further and postulated a direct relationship between the segmentary lineage structure and observable social processes. In reference to the Tallensi, he says that the ‘Tale society is built up round the lineage system . . . It is the skeleton of their social structure, the bony framework which shapes their body politic; it guides their economic life and moulds their ritual ideas and values’ (Fortes 1945:30). The Nuer, the Tallensi and the Tiv ‘. . . may be said to think agnatically about social relations like the Romans and Chinese . . . The paradigm of patrilineal descent is not just a means of picturing their social structure; it is their fundamental guide to conduct and belief in all areas of their social life’ (Fortes 1969:290–1).

Sahlins’ well-known argument that the segmentary lineage system is an organization of predatory expansion and a social means of intrusion and competition in an already occupied ecological niche would not even make sense if it were not derived directly from the assumption that the segmentary

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lineage structure embodies the ongoing social processes. Sahlins explicitly maintains that segmentary sociability is a salient mechanism of the political process in segmentary lineage systems, 'operating automatically to determine the level of collective political action' (Sahlins 1961:332).

Thus, we can distinguish two positions as far as the reference of the segmentary lineage structure is concerned. For some of the anthropologists mentioned above, it refers exclusively to a set of ideas, ideology, myth, or simply a set of notions. For others, it refers simultaneously to a set of notions and the pattern of social processes, the way in which the members of a society organize their activities. The first position faces a rather interesting problem: there is no denying that the set of notions called segmentary lineage structure exists; since it exists for the members of a society, it is their social reality. On the other hand, this set of notions is not manifested in social processes, in the organization of their activities. Since social processes, or the activities of the members of a society, exist equally undeniably and are also social reality, it is necessary to distinguish two different kinds of social reality which do not have to be directly related. Unless the observer is prepared to ascribe to both the same degree of facticity or reality, he has to ascribe to one of them ontological priority. This is a problem we will be discussing at considerable length later on; in this context, it would be only a digression. However, it is important to note that the postulation of two levels or domains of social reality is directly necessitated by defining segmentary lineage structure as a set of ideas, ideology or myth.

This problem does not exist for the second position. The segmentary lineage structure is simply a form of society manifested both in actors' notions and in social processes. However, this position runs into considerable problems of its own. On closer inspection, it appears that the view that the segmentary lineage structure is a representation of empirically observable social processes is not all that well founded. For instance, most of the available case histories of hostilities between Nuer tribal sections and their political alliances (Evans-Pritchard 1940:144–5, 229–30; Howell 1954:19–20) indicate that the opposition between tribal sections is not as balanced as Evans-Pritchard's paradigmatic presentation would suggest (cf. Holy 1979b for a more detailed discussion). This has been recognized, to a certain extent, by Evans-Pritchard himself in his admission that the hostilities and alliances between tribal sections were not always as regular and simple as they were explained to him and as he stated them to be (Evans-Pritchard 1940:144); he also admitted that 'political actualities are confused and conflicting. They are confused because they are not always, even in a political context, in accord with political values, though they tend to conform to them, and because social ties of a different kind operate in the same field, sometimes strengthening them and sometimes running counter to them' (Evans-Pritchard 1940:138).

There are numerous other cases where a careful reading of the description of

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actual situations discloses departures from the ideologically asserted balance and opposition of segments, but the Nuer evidence should be sufficient to establish the problem.

It is in this context that Leach's original question acquires its critical strength. However, Leach's criticism is not the only one. Several anthropologists have levelled similar critical comments at the concept of segmentary lineage structure, and at the notion of structure as represented by it in general. The main deficiency is seen specifically in the fact that the distinction between a 'set of ideas' and a 'set of empirical facts' is not clearly and consistently made. As a consequence of it, the relation between structural forms and actual behaviour is postulated as nonproblematic and thus effectively removed from the analysis. Smith argues that the 'weakness of lineage theory and study . . . has been to mistake the ideology for actuality, and not to look behind it for more general and abstract categories of action, in terms of which it is to be explained and its constitution determined' (Smith 1956:65). Schneider criticized the structural model for its 'failure clearly to distinguish the segment as a conceptual entity from its concrete counterpart as a group' (Schneider 1965:75). Buchler and Selby pointed out that the model eliminates 'the possibility of establishing correspondence rules that link theoretic constructs with behavioral systems . . . This is due to an exclusive concern with the system of jural constraints which structure the basic actual framework' (Buchler and Selby 1968:102). Barth criticized the model for 'seeking explanation too exclusively on the conceptual side of the dichotomy' between the segment as a conceptual entity and its counterpart as a group (Barth 1966:6). Holy has come to the conclusion that the use of the segmentary lineage model was 'ultimately responsible for neglecting important areas of research into both the actual political processes and notions about them, existing in societies which have been classified as having segmentary lineage systems' (Holy 1979a:19).

Though somewhat different in content, all these and similar criticisms make the same point: the view under criticism is making a very serious error in considering the segmentary lineage structure as referring indiscriminately to the actors' set of notions and their actual activities. The question arises of how it is possible that even the recognition of the above-mentioned discrepancies, i.e. of the fact that activities on the ground are not always structured in terms of the segmentary model, has not detracted from the idea that structure expresses or embodies behaviour. In our view, this is because the ultimate goal of such research has been to formulate a holistic structure of society as its final description and as the explanation of whatever is going on in it. The analysis has not been oriented towards what the members of the society do and why they do it, but towards how the society is structured. The structure has to be, of course, elicited from the data, but, by the same token, the data are seen as indiscriminately referring to the structure. Even a cursory look at any monograph will show us that the data comprise actually observed behaviour,