SOME TRENDS IN THE ANTHROPOLOGY OF ACTION.

Rik Pinxten

Abstract

I focus on some (not all) approaches to action in cultural anthropology. The main distinctions run parallel with those in other social sciences: behavioristic, cognitive and "praxeological" perspectives.

In most of the paper I concentrate on the methodological and philosophical implications of the more recent trends in action research within anthropology. In particular I draw attention to the "hermeneutic" or "interpretive anthropology" that is gaining impetus in the present decade. The paper presents a selective overview with some insights into the presuppositions of anthropological terms and concepts.

1. *A short history*

The meaning of the term "action" has undergone a series of changes in the course of anthropological research in the present century. I will restrict myself to the postwar era.

a. In the 50s and 60s an influential behavioristic proposal was worked out by Marvin Harris (1964). He offers what could be labeled as a "positivistic" analysis of cultural actions. That is to say, he explicitly draws on methodological criteria from logical positivism to delineate the range of study of the concept of action: that is to say, only those aspects which are externally observable and can be measured and/or structurally described can be subject to scientific action research. Anything we designate with terms like "goal directedness", "purpose", "meaning", "intention" and the like, falls outside of the scope of scientific research on action. Harris develops a genuine syntax of actions: at the ground level he distinguishes actones; these are considered to be "the lowest-level operationally meaningful universal common denominators of human culture". (1964, 51) Actones are

linked by means of structural relations to form more complex units of action. Apparently, no efficient and meaningful analysis of action proved to be feasible with this approach, and it was altered into a broader (but still rather rigid) but methodologically poorly developed "cultural materialism" later on (Harris, 1982). A forerunner of the latter may be seen in the "ecological" focus of Barker & Wright (see my 1976 for further details).

An important feature of this sort of approach is that which I call the "fixed" or "passive agent". Indeed, the approach of Harris (and of Barker & Wright) presupposes that - for the purpose and the scope of analysis - the agent is a black box which only has a certain amount of observable output. From an epistemological point of view, only the actions (or output) of the agent can be observed scientifically by a trained observer. The very criteria of scientificity dictate that the agent's action are maximally objectivized (or de-subjectivized), such that the actions can identified, structured and divided in constituent units by the observer in terms of his analytic device. The final justification of this device and its units lies in its proclaimed universality. For the purpose of this paper it is important to note that the total process of knowledge building about the agent's action is the privilege of the observer: from the perspective of research the agent is passive, i.e. he only participates in the process as an object of study.

b. Kenneth Pike, the American linguist, has a Bloomfieldian background. Beyond Bloomfield (and in line with Sapir) he aims at a unifying theory of action (1970), spanning the total range from syntax, over semantics and pragmatics, to the study of goal-directed action. Typical for Pike's program is that he builds his theory on what he considers to be safe foundations: linguistics (and primarily phonology). He "blows up" the linguistic model and proposes as correlates to linguistic units (such as phonemes and morphemes), behavioral units called "tagmemes". It is clear that Pike triggered off a great amount of semantic studies in anthropology, namely indirectly in the subdiscipline that was labeled ethnoscience (see Pinxten, 1976). Today one speaks about "cognitive anthropology" when referring to this type of studies. Contrary to Pike, however, the cognitivists did not pay a great amount of attention to the study of action. Their focus was and is primarily on processes and knowledge-gathering and representation. An occasional study deals with verbalized action plans (see Werner & Schoepfle, 1987 for an overview). Their epistemological and methodological views differ from those of the behaviorists on crucial points: the agent/speaker and his output (actions and products) can be known by observation

combined with sophisticated forms of communication. That is to say, the researcher can elicit information from the agent/speaker by means of interviewing. The responses of the agent/speaker are taken by the researcher to yield dependable information on the cognitive setup, the heuristic and inductive procedures, the inferences of the thinker, and so on. In other words, the cognitivists claims to be able to gain dependable information about the processes "inside" the agent (and intrinsic to his culture), provided the researcher uses the appropriate procedures of information gathering and controls his data adequately. On the methodological questions in this tradition of research a vast literature is available now (Naroll & Cohen, 1973; Werner & Schoepfle, 1987).

c. In the 70s a group of anthropologists came to the fore under the label "symbolic anthropology". In the 80s this label is dropped for the epistemologically tinted one of "interpretive anthropology" (Geertz, 1983). The pioneers in this subdiscipline must be situated at the University of Chicago: especially Victor Turner's study of ritual (1969) was influential for this group. Turner tries to develop an analysis of ritual action which would combine a structural and process view. He defines agents, spatiotemporal contexts of action, action units and meanings or goals of action. The pair of paradigms he uses is composed of the concept of structure from Lévi-Strauss (giving a static backbone to ritual, that reminds us of the syntactic structures in language) and the anti-structural aspect of "communitas", which Turner draws from the history of religions (Martin Buber's "Essential We"). In my opinion, the general intuition which is still mostly implicit in Turner's work, is worked out in considerable detail in the epistemological and methodological analyses of the interpretive anthropologists of today (see Geertz, o.c.). I will elaborate on these below (sub.4).

d. Finally, the praxeological approach to sociocultural phenomena should be mentioned. In the 70s some French social scientists (especially Pierre Bourdieu and Marc Augé) started to develop a praxeological perspective on sociocultural phenomena. Their first aim was to criticize the aprioristic, static and highly theoretical status of structuralist studies. Where structuralists described phenomena in terms of supposedly "ahistorical" subconscious and deep-structural features of a culture, the praxeologists emphasize that cultural phenomena exist first and foremost as practices and products of action. They claim that structuralist and behaviorist approaches alike did not take the nature of action, interaction and communication seriously, and then go on to develop the necessary conceptual tools that will serve the

social scientist in his work. For example, Bourdieu (1980) develops an elaborate presentation of the following four action-concepts:

- *field*: each action is performed by an agent in a particular field of social relations, amounting to a particular context of power. The meaning, possible impact, adequacy and so on of an action depends to a great extent on the field it is performed in.
- *practical beliefs*: every agent incorporates a certain amount of attitudes, postures, habits, behavioral rules, etc. through education. Those features are captured by the concept of practical beliefs. E.g. the practice of kneeling in front of a holy shrine in the catholic tradition is literally incorporated by the believer: he will kneel automatically, without thinking, when the occasion presents itself.
- *habitus*: the habitus is the collective code for actions which agents share. It can be seen as the set of action plans and action potentialities (i.e. the set of action procedures one can set to work) an agent has at his disposal. The concrete action is the product of the dialectics between a habitus and a field.
- *symbolic* capital: the symbolic capital of an agent is the set of social and cultural relationships, prestige and status and of all other forms of "credit" the agent is granted in the belief system of a community. The community bestows a certain amount of symbolic capital on an individual corresponding to the material and symbolic guarantee that individual represents for the group. Credit can be accumulated (or gained) and withdrawn (or lost) depending on the estimation of the agent's value. E.g. initiation rites may enhance one's credit; treason will damage it.

Praxeology aims at a non-reductionist analysis of complex sociocultural action-in-context. In comparison with the foregoing approaches the following features appear:

- the units of action are not simply externally observable (cf. behaviorism),but the semantic-cognitive-symbolic dimension (cf. cognitivists) is an integral part of an action.
- the "object" of study is constructed in the processes of interaction and communication between the researcher and the subject of research (individual, group, community). Each takes an active part in this process of knowledge gathering: the researcher observes, the subject directs attention, asks questions, and so on, and the subject observes the researcher, responds or not, redirects the observer's attention, allows the researcher to come to know certain things or rather lies to him or does not admit him at certain levels of the cultural knowledge system. Elsewhere, I called this characteristic of the process of knowledge gathering in the social sciences the principle of

intrinsic "double bias" (Pinxten, 1981). With the appearance of praxeological research in anthropology I see a shift from an emphasis on techniques for unilateral observation (e.g. between the two wars) towards an active research on qualifications for communication and interaction in the social sciences (especially Bourdieu, 1981 and Werner & Schoepfle, 1987).

2. *Anthropology and the natural sciences*

The behaviorist believed that the natural sciences could serve as an example for any scientific endeavor on whatever object one prefers to study (e.g. Harris, 1964, explicitly draws on operationalism, in the sense of Bridgman). In practice, this led to a drastic reduction of the subject matter of anthropology. Harris (ibidem) declared that anything that could not be studied within the limits of the behaviorist methodology was to be dropped as an object of research. In fact, he claims that sense, meaning, purpose, intention, symbolic aspects, and so on are not fit for genuine scientific study. It is clarifying to illustrate the consequences of such a point of view by means of a concrete case. Suppose we would like to study the phenomenon of "consecration" in the catholic Holy Mass. According to behaviorist methodology we can describe the attire of the main agent (i.c. the priest) and of the audience (i.c. the believers): the color, the form of the pieces of cloth, etc. We can also describe in great detail the actions the priest (and the audience) perform: at the moment we are focusing on, the priest lifts a roundish piece of bread and (later) a chalice filled with wine with both hands and looks up at them for a while; a bell rings and a moment of silence is observed; then the paraphernalia are put down again. We can split these sequences of action into smaller units describing the movements of the arms and of the head of the priest, and detailing the concurrent changes in posture of the believers (lowering their heads at some point, remaining immobile at a certain moment and so on). But,however detailed we describe the actions we can externally observe, we shall not reach the symbolic meaning which lies in and is expressed through these actions in the eyes of believers and priest: the actions guide and in a certain sense induce the transubstantiation of matter (bread and wine) in the body and the blood of a God (Christ, Son of God the Father in Christian belief). The paradoxical aspect of behaviorism is that indeed we know and came to learn about the religious meaning (and even the high significance) of the acts during the consecration sequence, and every

catholic behaviorist will hold the belief(s) implied, but according to behaviorism it is impossible to really know them. I think there is a category mistake here: it should be granted that we cannot study the mysterious, ambiguous but clearly culturally important and powerful belief system spoken of with the methodological principles of the behaviorist. But the mere fact that we can know these beliefs and that they have been transferred from one generation to the other, and from one place to the next, shows that these cultural meanings are manipulable and learnable. Should they still be banned from scientific study? I disagree on two basic points with the behaviorist:

(a) the object construction: the behaviorist sacrifies content for presumed clarity. The situation of (early) physics is often used as an example here. The reasoning goes as follows: the (early) physicist defined his object of study by focusing on those aspects that were liable to be studied with the high standards of a scientific, controllable, repeatable empirical approach. That this implied the sometimes unwarranted reduction of physical reality (e.g. by leaving out irreversible time as a parameter, Prigogine & Stengers, 1984) has long been "forgiven" to physicists, because of the success or efficiency of their scientific approach. The behaviorists were inspired by this success and proposed a more or less similar line of research for sociocultural phenomena. Adequacy in description or success in prediction did not ensue,and the majority of scholars turned away from behaviorism. However, I think it is important for any further orientation of the social sciences to understand why success did not come. In my opinion the object construction of behaviorism is almost pathetically narrow (a situation that is induced by an ill conceived reverence for physics): sociocultural phenomena are of a higher order of complexity than physical phenomena, and thus the models and methods which are adequate for the latter are inadequate for the former (or, to put it more academically, the magnitude of the order of parameters is incomparably higher in the case of sociocultural phenomena). In the second place, it is my conviction that sociocultural phenomena are intrinsically temporal, and that the reduction we have known in physics (deleting "time") simply truncates the subject matter in anthropology (Pinxten et al.,1988)

(b) in the second place, I refuse to follow the behaviorist in his reductionist move. I think that the fact that cultural meaning proves to be transferable or learnable should lead to a reinterpretation of the status of scientific reséarch. I subscribe to Campbell's (1974) view in this respect: science is a particular type of selector (or, more appropriately, selective retention

process) and thus should be characterized as a specific type of evolutionarily sensible, cultural adaptation system. It only gradually differs from language, explorative learning or other sociocultural selectors. Campbell's proposal of the distinction between science and other selectors was at that time: "... that the selective system which weeds out among the variety of conjectures involves deliberate contact with the environment through experiment and quantified prediction, designed so that outcomes quite independent of the preferences of the investigator are possible" (1974: 434). However general this criterion is, it allows us to distinguish scientific praxis from religious or even from common sense. The generality of the characteristic allows for an open and critical view on a multitude of methodologies, which is not surprisingly coming from a profound methodological renovator such as Campbell. I introduce Campbell's view to point at a view on science which seems to be markedly different from the behaviorist one: I see a broad panorama of scientific endeavors which "fit" in this open outlook. Moreover, their adequacy is to be established *after the fact* (except for the broad criterion that is spelled out in the citation) rather than *a priori* (like in the behaviorist outlook).

F. Boas (himself a former physicist) encouraged a nonreductionist approach, emphasizing field work as a necessary empirical strategy for anthropological knowledge building and promoting the method of "participant observation" as a valid research device. (B. Malinowski is often indicated as the initiator of systematic field work methodology, but Boas certainly had the greatest impact in promoting it: M. Mead, R. Benedict, R. Redfield and so on and so forth were his pupils). Especially for nonverbal data the method of "participant observation" seemed promising at first. Anthropologists went to distant places and lived there with the local people and (to a great extent) like the local people. They got adopted, underwent initiation ceremonies, learned the language, planted and fed themselves like the natives. The presumption of this line of approach was (and is) that the researcher will be in the position to observe the culture he or she visits by participating in it. Thus, a "view from inside" would be possible. The methodological emphasis is an "einfühlen", while the epistemological presupposition is that the subject matter to be studied (the Other, or Culture) is fundamentally alike or akin to the researcher: they are human beings as we are, they think, act, have needs, etc. just like we do. Therefore, we can know them by somehow "becoming like them" that is by becoming a participant in their cultural system. This reasoning proved to be epistemologically naive. Indeed, no re-

searcher starts out to a distant field as a *tabula rasa*. Instead, he or she is a highly educated Westerner to begin with. Consequently, however cautious one goes about collecting and interpreting data, one will by necessity filter these through the deeply rooted and"incorporated" lenses one gained through one's education. We cannot *undo* our background, so we should not react as if we do in our methodology. Furthermore, the Other accepts the researcher *as a foreigner*, even if the latter may turn out to be dependable or beneficial or sympathetic. Thus, participation in another culture is doubly restricted: by the researcher's cultural filter and by the boundaries and possibilities dictated by the Other (whether or not the researcher is aware of them).

In retrospect, then, both the reduction of social sciences to "science proper" (i.e. physics) of the behaviorist, and the phenomenological alternative of the participationists are flawed in their attempt to present an encompassing scientific perspective for anthropology.

The alternative I have been hinting at, and which is detailed already to a great extent in praxeological approaches, overcomes or bypasses the criticisms voiced so far. In the critical, reflexive and fundamentally process-centered anthropology I discern in (the narrower studies of) praxeology today, I point to the following characteristics:

- the object of study is action, agents, products of action, and so on. In this view sociocultural phenomena are fundamentally *temporal*. Secondly they are culture specific in a deep sense: any particular phenomenon, any action, belief or product has a sense or a meaning *within the native perspective* of the agent. In the research procedures knowledge is more adequate the more it approaches the native perspective on the phenomenon. (cf. Fabian, 1983)

- the epistemology of this alternative approach looks upon the knowledge system and upon knowledge building as particular types of action. The knowledge system is not so much a "cognitive map" representing an outside reality to some extent, but rather a (permanently provisional) product of the intense interaction between researcher and subject of research. Knowledge building is the complex, temporal and (multi)culturally biased process of these interaction processes. This view does not necessarily imply an epistemological relativism. However, it does invite the scientist and the philosopher of science to reconsider the criteria and prerequisites for adequate scientific work.

The paragraphs above are sketchy, but it is beyond the scope of this paper to go into further detail on these matters (howe-

ver, see Pinxten et al. 1988). The question of adequacy will hold our attention in the following section.

3. *Optimization of action*

Cognitive and praxeological approaches can be (and actually are) used to improve or optimize the action and action complexes studied. In anthropology optimization necessarily implies a moral or political choice. In practice one can distinguish between three alternatives:

(a) *enculturation*: optimization of action is understood in terms of adaptation of actions (technology, working behavior, etc.) to the dominant western pattern. This often implied a mending and sometimes an annihilation of the native cultural habits and practices which are esteemed to be a mere hindrance for genuine (i.e. western) development. Ever since the independence of the former colonies we witness a more or less thorough critique by independence movements on enculturation programs.

(b) *native revitalization*: this amounts to the recognition and revitalization of the old or traditional culture in order to contrast it with the "foreigner" and former colonial oppressor. This ideological choice lies at the basis of many forms of "africanité" as they were promoted by some missionaries (e.g. P. Tempels) and some political leaders (e.g. L. Senghor, J. Nyerere). A countermovement developed against revitalization trends, stating that to return to the traditional culture in fact means to go back to the situation where oppression by a foreign power was possible and started. Thus revitalization is identified with submission and enslavement.

(c) *sophistication of native culture*: this third option (of which I am an advocate: Pinxten et al. 1983) recognizes the actual economical, political and military dominance of the western industrial culture, but chooses at the same time for plurality of knowledge and action traditions. In a nutshell I claim that the thousands of cultures we know about have survived with their own history and their own subsistence system for many centuries. They have adapted themselves continuously to changes in the environment and alterations through culture contact. The mere fact of their history demonstrates the survival value of the action strategies and knowledge perspectives these cultures incorporate. If one takes this point of view, any attempt to "optimize", "develop" or achieve "progress" vis-à-vis these cultures will (also) have to respect their otherness. Especially the approaches of the cognitivist and the praxeologist are suited

for the study of these cultures with due respect for them, since these perspectives allow for more and deeper understanding of the "native point of view" (Geertz,1983) than the others.

The elaboration of plans for optimization of action is a concrete matter. I can only mention in passing two examples to illustrate my point (plenty references can be found in the literature cited for these approaches). One example concerns the desertification in South America through plantation policies. The original inhabitants of the Amazonian forest in North Brazil practice a slash and burn agricultural system, resulting in small parcels of agricultural land between the big trees (which were not cut). This allowed for small crops for many centuries. Around 1900 a great number of these peoples were driven off their land, the big trees were cut and huge plantations were installed. However, within a decade, desertification set in. With the growth of modern ecological theory it seems that the "wisdom" of the native system gets recognition. Optimization in this case would amount to the introduction of some small scale technological tools (steel plows, for example) and techniques within the limits of the native system, I presume, rather than the annihilation of it. A very different example tells of the use of healing rituals for mental diseases in the traditional Navajo culture. Whereas Navajo medicinemen grant that western medicine is fast and dependable for some physical injuries, they claim that the ceremonial treatment they practice on mental patients ("restoring" them by means of a complex ritual sociodrama) is more efficient than the therapies of western psychiatrists. Optimization and enhancement of efficiency should then be sought within the tradition of ritual action, and is illustrated in the recent history of Navajo culture: e.g. some technical devices can be integrated (knives and grindtools) but the core of the ceremonial is safeguarded. In a similar way our group in Gent has been working on the optimization of formal thinking (geometry and mathematics) by making use of the preschool knowledge of Navajo children (e.g. Pinxten, 1987) and Turkish immigrants (Soberon & Snoeck, 1987).

4. *Anthropological research and interpretation of action*

It is clear from the foregoing sections that anthropology has come to look upon the study of culture in general, and of cultural action in particular in a varied and non-naive way. Over the years a large literature emerged dealing with the problem of interpretation of verbal and nonverbal data in anthropology.

Some of this literature grew in the context of interdisciplinary research: e.g. several sociologists and anthropologists have used ramifications and adaptations of linguistic methods and models to study everyday actions (e.g. E. Goffman) or medical actions (e.g. A. Cicourel) or religious actions (e.g. D. Hymes). The sophistication of action research by complementary analysis of verbal behavior is another line of approach (e.g. the works by V. Turner and O. Werner cited above).

A recent subdiscipline in anthropology seems - at first sight - to go one step further; some authors have claimed that interpretation is in fact the total programme of anthropological research. They gave the thrust for a branch called "interpretive anthropology" (Geertz, 1973 and 1983). They do not turn to modern linguistics, but rather develop the old hermeneutic tradition of text interpretation. I will make clear what this type of research amounts to by means of a concrete study of Geertz: "The Balinese Cockfight" (1973).

- Geertz starts his research in the classical ethnographic way: he first tries to get a global picture of the context, the agents and the actions of the cockfight by means of observation on the spot. He describes the spatiotemporal aspects of the game, the betting system, and so on. When the local authorities happen upon the scene (cockfights are legally forbidden) he hides at somebody's home together with some of the bystanders at the cockfight. Geertz immediately takes advantage of this complicity situation and starts asking questions about the event and about the role and status of all participants.

- through questioning and discussion Geertz thus checks his primary observations and complements them by means of verbally transmitted information. This occasions a first shift in interpretation from the level of the observer's categories to that of the native perspective. He adopts the latter wherever possible.

- in a third move Geertz then supplements his research with observations and interviews about the broader social network (kinship ties, status, etc.). This yields information about the code system that is implicit in the betting behavior that Geertz observed: it proves to be the case that only some senior persons can initiate the betting and that their kin are obliged to join in the betting in line with the senior kinship member. Honor and dishonor for the kin group are at stake and - after a few more rounds in information gathering - Geertz is able to demonstrate that the cockfight is a highly integrated cluster of symbolic moves which amounts to a metaphoric struggle: the kin group wins or loses honor and respectability in the face of all others

via the performance of the cock.

Interpretation of action in Geertz's anthropology thus is worked out by means of a series of subsequent analyses: the sense or "meaning" of a particular action is only fully determined provided one gets a clear view on the progressively more general and encompassing actions and action contexts. In a way, any particular action is embedded in larger units and the interpretive anthropologist tries to make clear how the sense of the particular action is codetermined by that of higher order actions.

Geertz's ethnographic work has a great appeal for a number of reasons. However, his resolute choice for a hermeneutic or interpretive approach (with more or less poetic labels such as "thick description" and "local knowledge") resulted in a loss of methodological rigor. In a sense one can say that the interpretive trend drives the researchers more and more away from even the minimal scientific criteria (cf. Campbell above) and into highly idiosyncratic analyses. Critiques by both philosophers of science and fellow anthropologists point at this danger (e.g. Wolf, 1987).

5. *Concluding remark*

The actual variety of outlooks in present-day anthropology forbids any definite and general conclusions. This abundance of color is a bonus, I think, and we will have to strive for a substantial epistemological scrutiny of anthropological theory and its methods before we can hope to reach any decisive statements. But then, human culture is by definition one of the most complex phenomena we ever tried to study in a scientific way; it is then not surprising that the discipline will take a great amount of time to reach maturity.

Anthropology, R.U.Gent

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