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Anthropology and Disasters

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## ANTHROPOLOGY AND DISASTERS

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It is now barely eight months since three research groups specialising in disaster studies amalgamated to form the International Disaster Institute. The I.D.I. exists to improve, through research, the effectiveness of disaster relief and to promote a rational and coherent approach to the problems which face relief and development administrators.

The work of members of the I.D.I. during the last ten years has revolved around the attempt to reduce the tension between the 'academic' critics and the 'doers', and gain positive results for disaster victims. To achieve this, academics

or scientists – call them what you will – have worked directly with relief agencies in what could be described as a form of participant observation.

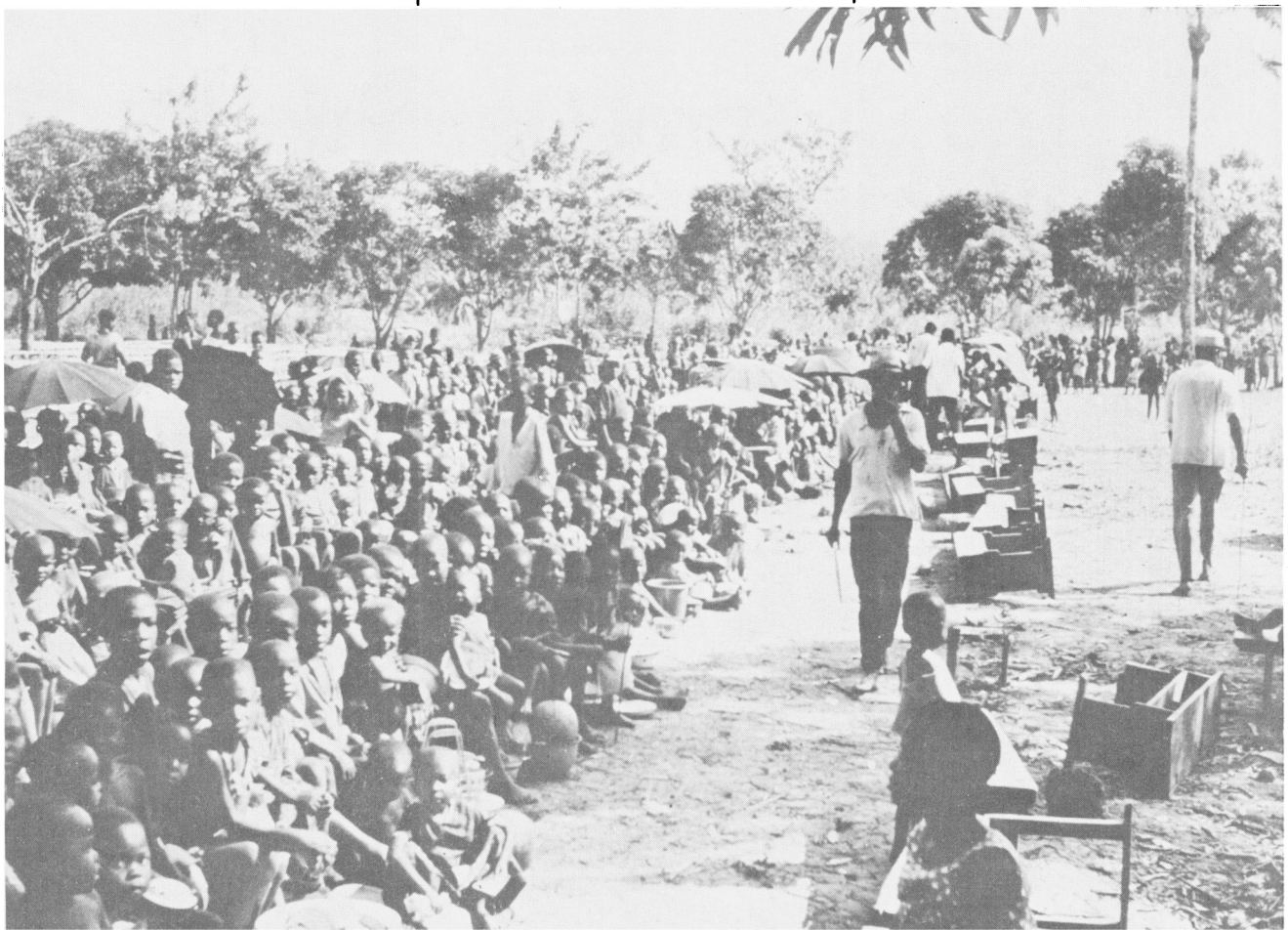
How do anthropologists fit in? There are two answers. Disasters are quite as much special social events as they are special physical events. And it is increasingly in underdeveloped countries that natural catastrophes have disastrous human effects. Special social events on what is often their 'patch' ought to interest anthropologists. Any adequate evaluation of victims' needs and the longer-term effect of aid must include a sensitive appreciation of the social and cultural background to the disaster. This article looks briefly at one kind of disaster, famine, and its social antecedents.

Most societies living in disaster-prone areas have traditional methods for coping with catastrophes such as drought, crop failures, floods and earthquakes. It is only when these coping mechanisms

become so overburdened by the scale of the catastrophe itself, or alternatively, by the sudden and vast influx of foreign aid for which there are no local precedents, that a double crisis occurs. Two questions are then posed. What available methods are there to (i) judge the 'breaking-point' of traditional coping mechanisms, and (ii) assess rapidly what would be effective aid in a given disaster?

There are, of course, no easy answers, and it is incumbent on I.D.I. and organisations like it to be aware of their responsibilities and their limitations. However, a decade of fieldwork has, time and again, emphasised that no amount of sophisticated technology is effective unless it is culturally acceptable and capable of being incorporated into an existing system – a point which has also emerged so clearly in family planning schemes in third world countries.

It would seem that a study such as an epidemiological survey of nutritional status in children, when food shortage



*Can anthropologists help to prevent these scenes? (Ethiopia, Wollo province, 1975)*

becomes acute, is reasonable, feasible and productive. Particular needs in a given section of a population can be identified and treated. A comparative study of disasters, on the other hand, including historical and anthropological data, aims to discover whether generalities or even hypotheses can be advanced. The effects of a given catastrophe are assessed retrospectively; one asks, for example, questions such as what conditions are necessary and sufficient for a famine to occur. This method appears to be based on the most tenuous of intuitive approaches. In fact this latter approach is probably the more fruitful in that it attempts, by treating disasters as social phenomena, to isolate the causes rather than concentrate on the symptoms.

If, for example, one wishes to examine nutritional status of children by whatever criteria are currently acceptable and practical, e.g. weight-for-height, for every clinical case of malnutrition there will be many more for whom weight-for height proves an inadequate technique for measuring sub-clinical cases of malnutrition and consequently identifying children at risk. How can such risk be quantified? If we have area A with 10% of clinical malnutrition and 10% at risk, and area B with 5% of clinical cases but 25% at risk – which should receive the higher priority for food aid?

Furthermore, should we attempt to assess the nutritional status of children at all? We know this age group is at greatest physiological risk from starvation, but is it necessarily at greatest

social risk? The social ethic, as in many societies, may well be to distribute food preferentially to children, therefore placing adults in a more nutritionally precarious condition (this was certainly the case during the Greek famine of 1945-6). Finally we might ask whether in a developing country where malnutrition is endemic, morbidity and mortality are sufficient indices of the severity of a famine, or whether (as some of us have argued) what distinguishes a famine from chronic deprivation – normal in under-development – is not only death, but, equally, social disruption. Both approaches actually beg the question: what are the best indicators of risk and of needs?

A retrospective study of events in Ethiopia between 1972 and 1975 which resulted in a severe famine ('Lessons for Epidemiology from the Ethiopian Famines' by J. P. W. Rivers, J. F. J. Holt, J. A. Seaman and M. R. Bowden in *Ann. Soc. belge. Med. trop.* 1976, 56, 4-5, 345-357) illustrates the point that some of the chief indicators of impending famine are social. The best estimates available show that famine crisis in north-east Ethiopia increased mortality by only 15%. What characterised the famine above all was the concentrated shock and social disruption in a localised area. The camps in which refugees from the countryside crowded, became the focus of belated publicity and relief aid. But owing to hopelessly inadequate provisions of food, shelter, medication and sanitation, it is likely that of some 100,000 deaths which occurred, a large number were

due to the 'disease of chaos'.

The camps were situated at towns on the two main highways of Wollo Province. Roadside towns in Ethiopia generally have established ways of dealing with a constant influx of people from rural areas, and traditionally they are absorbed by becoming servants, labourers or prostitutes. However, the concentration of refugees in 1973 swamped these mechanisms and thus marked the beginning of the real crisis resulting from the drought. What is crucial is the sequence of events which led up to this migration. In 1972, the Ethiopian government possessed (and suppressed) enough information from crop surveys to know that severe food shortages were inevitable in 1973. But even had they seriously wished to act and forestall the tragedy, the essential information – on market prices, panic-selling of land and houses, the break-up of families and villages, through early migration to find harvest work on commercial plantations – only became known retrospectively. Our studies indicate that this breakdown precedes starvation, but we need to know more about the sequence of decisions by farming or pastoral families which defines a social catastrophe after a physical one. In 1974, members of the then London Technical Group undertook a survey in pastoral areas of south-east Ethiopia which had some success in predicting famine by concentrating not solely on nutritional measurements but on unusual pastoral migration, marketing, observed livestock numbers and local opinion.

Language, Thought and Culture: Advances in the Study of Cognition

# The Cultural Definition of Political Response: Lineal Destiny Among the Luo

David Parkin

*School of Oriental and African Studies, University of London, England*

October 1978, xxii + 348 pp., £14.80 0.12.545650.6

Cultural change is not just a straightforward response to external pressures: the culture itself, as a powerful and autonomous system of ideas, plays a large part in shaping people's internal adaptation to new circumstances. Recent far-reaching changes – educational expansion, increasing wage dependency, population growth and urban unemployment – have all had their impact on the populations of Africa, but underlying cultural differences have persisted throughout. It would be misleading, therefore, to think of ethnicity as a mere 'false consciousness', without any bearing on the destiny of a people. The Luo of Kenya, who are the main subject of this investigation into the nature of cultural change and its semantic aspects, are no exception to the rule. In spite of a general shift to wage-earning, they continue to organise many political and domestic activities within a segmentary lineage model adapted to the new way of life and supported by the use of key verbal concepts by those in authority.

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The evidence available would suggest that starvation is a sudden and late event which has antecedents, not impossible to define. If starvation itself continues to be defined in anthropometric rather than social terms, aid will, with rare exceptions, continue to arrive after the crisis has passed. The essential question becomes, how do local food shortage or other crises affect individuals?

Some tentative conclusions may be drawn from the example of the Ethiopian disaster studies:

(1) That famine is not synonymous with death by starvation. It is clear that agencies' inability to accept the distinction between endemic malnutrition and epidemic starvation caused them to respond late and inadequately.

(2) That it was possible, on the basis of what has been called by an anthropologist a 'short and nasty' survey, to predict events by using anthropological as well as epidemiological methods.

(3) That one, possibly, key indicator of famine is large-scale and sudden movement of peoples, especially of pastoralists to urban areas.

(4) That the prevailing system of mutual debt and obligation spiralled viciously down and its collapse was actually enhanced by the influx of aid, in that the logistics of the relief programme itself precluded resumption of pastoralism, in some areas, by concentrating them in camps.

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Billions of dollars, tens of thousands of peoples and a vast technology have, as yet, been unable to prevent, let alone relieve, famines.

It would seem an essential exercise to try to arrive at a more general understanding of the underlying causes — the indicators of risk. From this information eventually one could then advance towards improvements in disaster preparedness.

If we can identify the crucial variables inherent in disasters, then we have the beginning of a genuine science in that predictions can be made. It is important to continue collecting data about communities under sudden stress and the social rules which dictate the distribution of food and other commodities, and the likely changes which may occur under the impact of such crises.

A great deal more needs to be known about the social response, among both victims and donors, to disaster; scientists who may, as yet, have only an impersonal understanding of disasters, do at least begin to know how to ask questions. This has important implications for development policies generally, as well as for disasters.

More specifically, since disasters usually occur in third world countries, it is necessary for that information to be gathered in association with anthropologists. Only when the detailed social context of what actually happens to communities, families and individuals in disasters is known, will one also know how to provide cost-effective relief. Finally a gradual building-up of this kind of detailed information will create a basis on which objective action can be

more nearly correlated with subjective motivation, and scientific ability can match the emotional response.

Much of the work of the I.D.I. concentrates on establishing ways in which such indicative information can be systematically collected in time to

## COMMENT

### UNDERWORLD ANTHROPOLOGY

The last ten or twenty years have seen a reaction against the use of social science to assist those who run established institutions. Researchers have sometimes erred in the opposite direction, identifying with those who are either 'oppressed' by these institutions or who reject them.

The trend is clear, for instance, in the ethnography of prison life. Terence and Pauline Morris's classic study *Pentonville*, published in 1963<sup>1</sup>, is explicitly modelled on the social anthropologist's technique of setting up a 'hut on the village street' (p. 8), and distinguishes the isolation of life in Pentonville from that on a Pacific island. The researchers clearly suffered considerable suspicion from the prison staff — the farewell party which they gave for the staff was sparsely attended — but their book does try to be fair and impartial as between staff and inmates.

Stanley Cohen and Laurie Taylor's *Psychological Survival* of 1972<sup>2</sup>, about the maximum security wing at Durham prison to which they had access as teachers of social science, is almost totally inmate-oriented. The authors explicitly say that for the purposes of their research they took the men's side, and with commendable frankness admit as a limitation of their study that they were unable to do any justice to the prison officers' view as well.

A more recent book still, published in 1974, *Chicano Prisoners: the Key to San Quentin* by R. Theodore Davidson<sup>3</sup>, is a remarkable analysis of the dominance of this well-known North Californian prison by the Mexican mafia. Davidson, working as a graduate student from Berkeley, proceeded almost entirely through sympathetic contact with informants from whom he has extracted detailed information about the leadership structure of this powerful network operating between prisons and between prison and the streets; about their manipulation of the prison economy of drugs, tobacco and cash; about membership initiation, obligations and privileges; about the corruption of guards, and much else. Though much of this is objective fact, he makes little attempt to cross-check his data or supply alternative viewpoints. He clearly admires the Chicanos' characteristic qualities of *machismo* and self-renunciation, but it is hard to believe that he was not from time to time their dupe as well as the carrier of a powerful ethnic myth. The role of heroin in the crimes for which San Quentin inmates are convicted is not emphasised, though I understand from another source that it is associated with about 70% of these crimes. Nor does Davidson make any attempt to

allow agencies to provide aid to identified localities before people become refugees in relief camps.

Frances D'Souza

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understand the psychology of the guards.

These are not new problems of ethnography. The sympathetic study of stigmatised minorities is a humanising enterprise. But it can become as romantic and sentimental as can the anthropology of primitive peoples. *Chicano Prisoners* is a tour de force of prison ethnography; but unlike Cohen and Taylor, Davidson does not seem to be aware how one-sided his account is.

As a curiosity of deviant anthropology, however, it is put in the shade by William Sims Bainbridge's *Satan's Power: a Deviant Psychotherapy Cult*, recently published by the University of California Press (£10.50). The dust-jacket's mixture of a satanic goat with typographic good taste made me assume at first that this was another of those cheerful occult pot-boilers which appear in the lists of such otherwise serious publishers as Routledge & Kegan Paul or Thames & Hudson. But no, it is a strictly analytical ethnography and psychohistory of a hierarchical secret society which started in 1963 in London, from Californian ancestry, and then migrated to Mexico, back to Europe and then to North America where at its peak it numbered some 200 or 250 members of all ranks. It disintegrated in the mid-1970's.

Dr Bainbridge, it must be said, has a good line in dead-pan humour, as when he treats Rosicrucianism and Psychoanalysis as two analogous movements, or compares structural oppositions in the private cosmology of the cult's founder 'Edward de Forest' with those in the theories of Lévi-Strauss (but perhaps he is serious here). Bainbridge contributes some useful insights into how normal middle-class young people get involved with weird sects, by a process he calls 'social implosion' in which internal social ties are strengthened and external ties are weakened; and he stresses how attractive it can be to enrol in an organisation as a therapist rather than as a patient.

But one cannot read *Satan's Power* as pure ethnography. With a show of

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