1 *The idea of calamity in a technocratic age*

KENNETH HEWITT

K. Hewitt, ed., Interpretations of Calamity: From the Viewpoint of Human Ecology

> I am compelled to fear that science will be used to promote the power of dominant groups rather than to make men happy. Bertrand Russell (1925)

Human communities may have always suffered losses from flood, drought or storm. The argument I wish to develop here, however, is that the prevailing scientific view of these problems is a quite recent *invention*. And by that I mean a partial and reconstructed view, carefully detached from almost all previous ideas of calamity, and reflecting the singular social context of its origins. That may not seem unusual. There is a widespread feeling that any topic or view not actively developed in the most recent studies and literature must be outmoded if not actually worthless. Equally, there is an assumption that this development depends upon the initiatives of established researchers and major institutions in, especially, the wealthiest industrial nations. Yet, for the scientific community a consistent forward movement is supposed to rest upon serious logical considerations rather than mere fashion. It presumes more comprehensive and precise empirical bases, in turn co-ordinated with increasingly powerful general concepts.

Contemporary natural disasters research is certainly rich in the results of scientific enquiries, whether in geophysics or the psychology of stress. The applications of scientific research are not, however, its definitive feature. It may have internal coherence or at least conviction. That does not alter my sense that it capitalises rather arbitrarily upon scientific discovery. Indeed it accords with 'the facts' only insofar as they can be made to fit the assumptions, development and social predicaments of dominant institutions and research that has grown up serving them. Moreover my assessment of it leads me to believe that such developments have become the single greatest impediment to improvement in both the understanding of natural calamities and the strategies to alleviate them. That is why I have felt justified in devoting most of this chapter to a critique of the underpinnings of the prevailing views in hazards research. As such, I may seem to stray far from the immediate interests of those concerned with flooding or earthquake disaster or weather-damage to crops. However, the worth and message of subsequent chapters that do focus on those phenomena depends not only upon their individual merit but on what they imply collectively about the state of hazards research. And essentially they go against the prevailing thrust of that research.

Looking across the range of studies and actions relating to natural hazards,

I am suggesting that one can recognise a convergence of opinion or approaches; a sufficient consensus to speak of a 'dominant view'. *Dominance* is evident in the resources allocated; in the numbers of highly trained personnel involved and the volume of their published works; in the public visibility and acceptance of these works; and perhaps most of all in the attachment of this view to the more powerful institutions of modern states. In the work of any subfield or study, the dominant view might be revealed by the literature quoted and emulated. It may appear in the terminology used or the audience anticipated. For example, the more visible work of geography or sociology seems to me largely to express such dependency even when making useful innovations.

What will be described as the dominant consensus has certainly not gone uncriticised, both within and from outside the various fields relating to natural hazards. Nor does it go unchanged. However, it seems that this consensus has gone forward resisting any fundamental criticism. Its changes have been chameleon-like exercises in superficial novelty – absorbing, co-opting or ignoring dissent at will. Of course, one must beware of giving it the attributes of a thing or actor. Rather, we have the convergence of a wide range of thinking upon a unified perspective that constitutes what Thomas Kuhn has called a 'paradigm' (Kuhn 1962), or more specifically what Edward Said calls 'an academic-research consensus' (1978, p. 275). For the dominant view of hazards is not merely enshrined in rarified language and technical apparatus, it is fully symptomatic of the social contexts in which it has arisen and that still form its main points of reference. Its strength depends less upon its logic and internal sophistications than on its being a convenient productive 'world view' for certain dominant institutions and academic spokesmen. In other words it is, above all, a construct reflecting the shaping hand of a contemporary social order. From a sociocultural perspective, it is itself a phenomenon requiring investigation as part of the so-called 'social construction of knowledge' (Mannheim 1952, Berger & Luckmann 1967).

The unease or outright criticism of accepted hazards interpretations in the chapters that follow seems to me to stem essentially from the struggle to articulate and to pursue intellectual and societal perspectives that the dominant view has served to stifle. Hence, behind the empirical and methodological detail looms the larger – and for social science the most fundamental of all research questions – self-conscious examination of the psychosocial underpinnings of thought, assumption and practice.

I shall not attempt here to review hazards research in a detailed fashion. In any case, there is no lack of reviews or lengthy studies of the various geophysical conditions involved. My purpose is rather to examine the styles of argument, the uses of information and managerial assumptions that *divide off* the dominant consensus not merely from other research, but from the variety of possible viewpoints and concerns of hazards research. I shall try to show the common ground that channels and reflects basic motivations and social contexts.

The dominant view in outline

The superficial features of the dominant view are not hard to discern, though it requires specialised, lengthy training to contribute to them. There is generally a straightforward acceptance of natural disaster as a result of 'extremes' in geophysical processes. The occurrence and essential features of calamity are seen to depend primarily upon the nature of storms, earthquakes, flood, drought. It may be accepted that 'hazard', strictly speaking, refers to the *potential* for damage that exists only in the presence of a *vulnerable* human community. Actual usage almost invariably refers to an objective geophysical process, such as a hurricane or frost, as 'the hazard'. In turn, damage and human actions are defined by, or as responses to, the type, magnitude, frequency, and other dimensions of these processes (e.g. Smith 1957, Part I; Hewitt & Burton 1971, White 1974, Unesco 1980, Burton *et al.* 1978, Ch. 2; Grayson & Sheets 1979).

Conceptual preambles and the development of a refined language of 'risk assessment' appear to have swept away the old unpalatable causality of environmental determinism seen in, say, Huntington's work on storms (1945, Ch. 21). The *sense* of causality or the direction of explanation still runs from the physical environment to its social impacts. The most expensive actions and the more formidable scientific literature recommending action are concerned mainly with geophysical monitoring, forecasting and direct engineering or land-use planning in relation to natural agents (NAS 1980a & b, Soloviev 1978, Ang 1978, Yoshino 1971).

Few researchers would deny that social and economic factors or habitat conditions other than geophysical extremes affect risk. The direction of argument in the dominant view relegates them to a dependent position. The initiative in calamity is seen to be with nature, which decides where and what social conditions or responses will become significant. Here is the geophysicist Bolt (1978, p. 156) discussing an especially 'bad year' for earthquake disasters: 'Paradoxically, despite these grim statistics, 1976 had slightly less than the average number of large earthquakes . . . [the] figures demonstrate that the misfortune of 1976 was not that more large earthquakes than normal occurred, but rather that more than usual occurred *by chance* in susceptible highly populated regions' [my italic]. The implication always seems to be that disaster occurs because of the chance recurrences of natural extremes, modified in detail but fortuitously by human circumstances.

Likewise, the geography of risk is usually treated as synonymous with the distribution of natural extremes such as large earthquakes, and with the natural features directly associated with them such as faults, flood plains, drought 'polygons' and avalanche tracks. To be sure, reference is made to past major disasters in assessing risk (Swiss Re 1978). Some account may be taken of population density, or national economic 'levels', themselves measured and treated so as to seem commensurate with geophysical parameters (see Kantarovitch *et al.* 1970, Burton *et al.* 1979). In such terms, the Soviet

geographers Gerasimov and Zvonova, for example, do speak of the difference between 'the intensity of a disaster' and the 'potential danger' which, they say, 'remains the same' (White 1974, p. 243). However, even geographers have for the most part been content to treat the geography of hazards as synonymous with the spatial distribution and frequencies of geophysical extremes. The maps in Sections IV and V of White (1974) are examples. (See also Strahler & Strahler 1973, pp. 218–25; Ayre 1975, Berlin 1980, vol. I.)

In the dominant view, then, disaster itself is attributed to nature. There is, however, an equally strong conviction that something can be done about disaster by society. But that something is viewed as strictly a matter of public policy backed up by the most advanced geophysical, geotechnical and managerial capability. There is a strong sense, even among social scientists for whom it is a major interest, that everyday or 'ordinary' human activity can do little except make the problem worse by default. In other words, the structure of the problem is seen to depend upon the ratios between given forces of nature and 'advanced' institutional and technical counterforce.

One can summarise the bulk of the work and expenditures within the dominant view as falling into three main areas:

(a) An unprecedented commitment to the monitoring and scientific understanding of geophysical processes – geologic, hydrologic, atmospheric – as the foundation for dealing with their human significance and impacts. Here the most immediate goal in relation to hazards is that of prediction.

(b) Planning and managerial activities to contain those processes where possible, through flood control works, cloud seeding, or avalanche defences, and where it is not possible, physically to rearrange human activities in accordance with the objective geophysical patterns and probabilities. That involves zoning, building codes and 'fail-safe' artifacts. A remarkable unity of language has emerged here to discuss geophysical processes, physical planning and the assessment of risks.

(c) Emergency measures, involving disaster plans and the establishment of organisations for relief and rehabilitation. The ability to put in place the insights developed by geophysical research and planning is important here. Study however is necessarily subordinate to action. Action is most commonly and directly put in the hands of military, or quasi-military organisations. (Since most of the world's people and land areas have little access normally to the products of modern geophysical science and management technology, it is through emergencies that they become involved in the perspectives of the dominant view.)

In terms of research, the main areas of expertise are those of the physical sciences and engineering. However, social sciences play a substantial role, notably in studying 'crisis behaviour' and emergency measures; or in focusing upon places and groups singled out by the experience, expectation or existence of major natural disaster in their area (Disaster Research Group 1961, Baker & Chapman 1962, Grosser *et al.* 1964, Brictson 1966, Dynes & Quarantelli 1968, NAS 1976, Hewitt & Burton 1971, White 1974, Haas *et al.* 1977).

There have been important social analyses that disagree more or less profoundly with the dominant view (e.g. Walford 1878–9, Part II; Sorokin 1942, Kendrick 1956, Lifton 1970, Copans 1975, McNeill 1976, O'Keefe *et al.* 1976, Torry 1978). However, in recent decades, social scientists have tended to concentrate increasingly upon direct socioeconomic and behavioural relations of the three areas of the dominant view noted above. They ask how individuals or groups appraise the risks of occupying areas classified as typhoon coasts or flood plains. The results tend to be compared with geophysical knowledge of typhoons and floods. They ask how people respond to forecasts, requests to conserve water and hazard-zoning legislation. They examine how people and institutions 'cope' when the volcano erupts or a crop is destroyed.

These interests seem entirely reasonable in themselves. They become less so as they are tributary to supposedly more sophisticated geophysical and engineering knowledge. Moreover, by this narrow focus upon information that centres the problem upon natural extremes and damaging events, they easily miss the main sources of social influence over hazards. But the bulk of hazards work by social scientists is focused in that way and serves to reinforce the 'geophysicalist' and technological reductionism of the dominant view.

It is not unimportant how completely the funding and mobilisation of research depends upon the agencies of national governments or international organisations. A singular feature is the near universal adoption by them of the same concerns and supporting ideas. Some would say that the dominant view reflects particular ideological and group commitments. Yet the main concerns expressed in it are being pursued or sponsored by more or less identically named institutions in most states, universities and city administrations. The view appears at its sturdiest and is most forcefully sponsored in multinational co-operation and under the auspices of international agencies (cf. Eijkmann 1911, Armytage 1965, Ch. 21).

Burton *et al.* (1978, pp. 212 *et seq.*) have criticised the 'natural science' and technological preoccupations of relevant United Nations' studies. Nearly all the materials from Unesco's 'Disasters Division' illustrate precisely the concerns and research categories of the dominant view. The reader might consult a recent publication on earthquake risk as one of the more thorough, competent and convincing examples (Unesco 1980). Moreover, that book reveals a unity of language and mission in the work of participants from Turkey, Switzerland and Japan, no less than from the United States, Soviet Union and New Zealand. This might seem to be a tribute to the unity of science. In my opinion that is a secondary matter.

Rather, what these materials show is, first, that the dominant view constitutes a technocratic approach. That means, the work subordinates other

modes and bases of understanding or action to those using technical procedures. More precisely, technocracy gives precedence in support and prestige to bureaucratically organised institutions, centrally controlled and staffed by or allocating funds to specialised professionals. For social scientists it is important to recognise that technocracy is not only or necessarily an obsession with technology in the narrow sense of engineering structures and machines (cf. Mumford 1967, 1970; Armytage 1965). Forthright critiques of the 'technological fix' approach can fall short of recognising the full scope of this phenomenon. The 'social, economic and political "people" factors involved in hazards reduction', that White and Haas (1975) emphasise, also can be and usually *are* approached technocratically. In fact, my own argument amounts to saying that the 'natural science–technological fix' approach to hazards is itself, essentially, a *sociocultural construct* reflecting a distinct, institution-centred and ethnocentric view of man and nature.

My sense of these developments as a socially constructed knowledge, is not, however, that commonly expressed in 'perception' studies of hazards and environment. I am not concerned to turn the tables on the experts and show that they are, in their way, as subjectively, attitudinally limited as the laymen in flood plains or people who buy homes astride an active fault. They surely are, but I do not find it useful to reduce the problem to the way individual perceptions are shaped at the interface of sense experience and personality. We are concerned with the way 'thought follows reality'. But the 'realities' here are not assumed universals of the empiricist's sense data and their psychological assimilation in acts of human perception and cognition. Rather we are looking at conditions that shape these pliable processes; the conditions that influence what facts we are likely to recognise and deem important; the acquired, accepted ways of interpreting them. These are matters of the social order. They relate to the societal means that shape learning and formal enquiry, that validate and communicate scientific discoveries, and especially that govern their implementation. As such, they are integral to Marxian notions of economic control and ideology. And we owe some of the most penetrating modern expressions of how 'thought follows reality' in this sense to Marx and Feuerbach (Marx 1964). Some of the more incisive criticism of hazards work in the dominant mode has also been Marxist (see Chs 5, 13 & 14). It seems to me, however, that like so much else in modern states, the dominant view of hazards differs little across the broadest spectrum of political affiliations. My own view of its social construction and implications is Weberian rather than Marxist. That is to say, I see here an expression of the way institutions – especially centralised, official and bureaucratically organised ones – route their human and material resources into particular styles of work and practice (see Weber 1947, Albrow 1970). We have to consider how far the dominant view in hazards research represents a continuing and deepening example of what Mills (1959) called 'the bureaucratic ethos'. That involves the channelling of scientists into a distinctive approach to 'facts', and a distinctive view of the nature of theory and practicality. Even in their most

felicitous forms these tend to reflect mainly the organisation sponsoring the research or the researcher's image of what that organisation can and should do. Moreover, science and research themselves, however practical in orientation, tend to be determining factors only at certain levels in these institutions – rarely the highest – and only in the outlook of a certain class of persons within them – rarely those with the main powers of decision.

The dominant view suits very well institutions that carry out technical studies, develop technical plans, and train technical managers, or favour research oriented to such projects. Moreover, 'technical' invariably means wedding science to technology, preferably in what is considered the most 'advanced' form. Therefore it is a creature of the most powerful, wealthy and centralised institutions. Such, of course, is the style adopted most widely by dominant organisations of government, business and culture today (Habermas 1973, Ch. 7). There is little hope of doing much about that.

However, in drawing attention to this truism, I suggest first that natural calamity is one of the special and especially intractable problems for a technocracy. Secondly, many of those involved appear unaware that they are doing anything but pursuing an 'objective', scientific, and even a necessary, research strategy. Thirdly, we are in many ways stuck with a form of technocratic strategy that is peculiarly archaic and inflexible, at least from a sociocultural and geographical perspective. Fourthly, I suggest that the international system where the strategy is operating, and the social and intellectual debates raging in the most powerful states, themselves challenge the effectiveness as well as the truth of the dominant view. The chapters that follow reveal its many failings on the ground. Hence, it becomes a major issue to discover and describe the basis of the view's robustness within the technical context, as well as in relation to other, quite different approaches to natural calamity.

An 'enclosure system'

There is a close analogy between the dominant view of hazards and Michel Foucault's description of how 'madness' came to be treated, indeed invented, by the 'Age of Reason' (Foucault 1965). Natural calamity in a technocratic society is much the same sort of pivotal dilemma as insanity for the champions of reason. Disaster in the 20th-century international system involves comparable pressures upon dominant institutions and knowledge, as did the 'crazed poor' in the social and economic crises that formed the underbelly of the Enlightenment. Madness and calamity are very disturbing. They directly challenge our notions of order. Both threaten to be interpreted as 'a punishment for a disorderly and useless science' (ibid. p. 32). They can be seen as clear limits to knowledge and power, because they are initiated in a way that seems uncontrollable by society. However, there is also a long history of belief that both are 'judgements' upon human activity, a sentiment echoed in attitudes to environmental problems in the ecology movement today, for example.

In both cases, however, there has arisen a dominant view that counteracts these difficulties with a positive creed, an assertion of potency where the grounds for conviction seem the least. It is exactly here that we can see the benefits to a technocratic approach of dividing off hazards. It is very convenient to treat calamity as a special problem for advanced research in the areas noted above, and this outweighs other considerations. The problem is made manageable by an extreme narrowing of the range of interpretation and acceptable evidence. The resulting partial view has been achieved much as the 'great confinement' of the poverty-stricken and 'crazy' in the 18th century was to form the foundation for dominant perspectives on madness, crime and punishment – and the most severe criticism of them in our century (Rothman 1971, Cooper 1978). We are not only dealing with the substance of such questions, or with a particular philosophy and set of practical procedures. We are also dealing with a careful, pragmatic and disarming *placement* of the problem.

The language of discourse is often a good indicator of basic assumptions. In hazards work one can see how language is used to maintain a sense of *discontinuity* or *otherness*, which severs these problems from the rest of man–environment relations and social life. That is most obvious in the recurrent use of words stressing the 'un'-ness of the problem. Disasters are *un*managed phenomena. They are the *un*expected, the *un*precedented. They derive from natural processes or events that are highly *un*certain. *Un*awareness and *un*readiness are said to typify the condition of their human victims. Even the common use of the word [disaster] 'event' can reinforce the idea of a discrete unit in time and space. In the official-sounding euphemism for disasters in North America, they are '*un*scheduled events'.

What emerges is that 'hazards' are not viewed as integral parts of the spectrum of man–environment relations or as directly dependent upon those. We have abandoned that aspect of the earlier environmentalists from Strabo to Huntington. One does find philosophical introductions and conclusions locating 'hazards' within a panorama of human ecology and its diverse collections of adaptive problems. But they are described and dealt with as a separate problem.

We have noted the precedence given to specialised geophysical research and the geophysical processes its subfields concentrate upon. However, common features, found across the various specialisations, serve to reinforce the sense of separateness of disaster and its causes. In nearly all cases attention is directed towards the occurrence or likelihood of discrete, sharp bouts of damage. Natural hazards may be acknowledged to include a continuum of damages from ordinary wear and tear by sun or wind to major catastrophes. Human societies may be seen to exist in a continuous process of interaction with the habitat, and under conditions that ultimately link us to the entire system of relations in the biosphere. Yet, it is the temporally and spatially limited 'event' that is the specialty in hazards work – a Sahel drought, Hurricane Betsy, avalanches.

This geophysical approach is supported by a well entrenched logic. It identifies hazardous events in strong, step-like changes of geophysical measures that accompany disastrous damages. The changes may involve river levels, seismic shocks, wind gusts and so on. The picture of a pointer-reading swinging off the scale is one of science's contributions to the common stock of disaster imagery. Human vulnerability is, in turn, tied to these extreme 'pulses' in nature through such notions as 'damage thresholds'. My diagrams in Hewitt and Burton (1971, Figs 1 & 21) expressed this with forthrightness! (see also Haas *et al.* 1977, Ch. 1).

What we also find ourselves dealing with here are notions of stability and instability. In Scheidegger's words: 'evidently, if a prevailing status quo is preserved no catastrophe occurs. Therefore a catastrophe generally entails the *termination* of a stable state' [my italic] (1975, p. 2). He also calls 'stable', the 'normal' state. Though he expressly refers to geophysical stability, the sense of a breakdown in a stable set of relations to the habitat is found equally in the social interpretations of these risks. Hazards are taken as natural events that destabilise or *violate* ordinary life and relations to the habitat. Research commonly takes the idea of failure in social systems as a fairly exact analogue of that in mechanical systems – the result of an (exceptional) force that exceeds the strength of the material or structure. Haas and Ayre (1969, p. 7) have expressed this more conscientiously than most:

> 'It does not seem unduly farfetched to think of a human community, in its response to dynamic input, as a second-order system described by the system of coupled nonlinear equations,
>
> $[M]\{\ddot{Y}\} + [D]\{Y\} + [R]\{Y\} = \{F\}$ where $\{Y\}$ is the response vector; $[M]$, $[D]$ and $[R]$ are matrices of "mass", "energy dissipation" and "restoration" . . .'

Given that the kind of mathematical model described here is virtually impossible to apply in practice, this statement expresses a metaphysical belief, not unlike the 'mumbo-jumbo' of numerologists. However, what is important is the further implication of a threshold, below which stable, 'normal', orderly activities and human competence apply; above it, disorder, the unexpected, the unplanned. This makes plain what is merely assumed in much of disaster research. J. W. Powell summed up the position by calling disaster 'the impinging upon a *structured* community of an *external* force on a scale wide enough to excite public alarm, and to disrupt *normal* patterns of behaviour' [my italic] (quoted Lemons 1957, p. 2).

The concern with rare, 'chance' events directs our attention in other ways. The more severe occurrences, the 'worst-case scenarios', tend to become definitive or at least symbolic of the whole problem (Barton 1969, Ch. 1; Bolt 1978, Ch. 2; Burton *et al.* 1978, Ch. 1). Certain kinds of disaster, especially those in major urban centres, or the very largest examples (i.e. those that are

least frequent and most extraordinary of all disasters), come to represent the whole problem by a sort of historical–geographical compression. One sees this happening in the way the plight of San Francisco, the 1906 earthquake disaster, its anticipated recurrence and the San Andreas Fault, come to be touchstones of seismic risk problems, even as many other disasters occur and are forgotten. Furthermore, destruction, impairment, and what must be restored by *outside assistance* tend to form the entire concern in individual disasters. And these descriptions commonly deal only with the most severely damaging aspects. Patterns of survival and evidence of 'stability' in structures – which in most disaster zones could be seen to involve by far the larger fraction of people and property affected – are of little concern.

The disaster archipelago

In practice, then, natural hazards have been carefully roped off from the rest of man–environment relations. The enclosure where they reside is variously labelled 'unscheduled events', 'emergency social situations', 'negative resources' or simply 'hazards'. Not only do such acts of definition isolate them. Not only do such problem-formulations sever their explanation from supposedly 'normal' 'predictable' events. Major allocations of resources to manage 'risk' have involved, out of all possible 'alternative adjustments', attempts physically to wall in the places and occasions of disaster. Flood control work exemplifies this attempted enclosure system.* Meanwhile the dominant response to actual disaster is to invest and seal off the damage zone with military personnel and a network of officials, clearly demarcating it as a zone of exclusion from ongoing (peaceful?) life. In many examples we find the authorities making huge efforts to keep the people *in* the 'disaster zone' from moving out (see Waddell, Ch. 2), and others from moving in. Elsewhere, as in the 1980 Italian earthquake, we have a tremendous struggle to relocate people against their will (cf. Oliver-Smith 1974, Torry 1978). When there is a forecast, we see in industrial states the growing civil defence strategy of massive total evacuation that leaves 'the hazard' in a no-man's land. These may well be fair and desirable responses in some cases and within the possible options of given communities. They nevertheless serve to reinforce the 'otherness' of the source of disaster, and also to emphasise the blanket classifying of entire areas as disaster zones.

By these and other means, natural disaster is quarantined in thought as well as practice. The geography of disaster is an *archipelago* of isolated misfortunes. Each is seen as a localised *dis*organisation of space, projected upon the

* Though the language is different, I do not see this as unlike Gilbert White's much earlier formulation of the problem in flood responses, as being to counter narrow, confining emphases upon 'technological' solutions with a sense of a 'range of possible adjustments' (White 1961). However, two decades or more later, the problem looms as less one of 'choice of use', and more one of intellectual and institutional frameworks that circumscribe (technical) choice in general.

extensive map of human geography in a more or less random way due to independent events in the geophysical realms of atmosphere, hydrosphere and lithosphere. More specifically, each disaster is an unplanned hole or rupture in the fabric of *productive* and orderly human relations with the habitat or 'natural resources'.

The importance of isolating the problem in this way may not seem as necessary as in disease control, in the asylums or lazar houses Foucault tells us they replaced in the 18th century. There may not seem a logic to it like the 'Gulag Archipelago' of Soviet labour camps, and similar mechanisms devised to remove 'undesirable' elements from intercourse with the rest of society. But the analogy and convenience of this isolation is there, deep in the structure of technocratic thought and its precursors.

In each field that has entered into hazards research it seems that, sooner or later, great excitement develops about how to classify and partition off the issues involved in the subject. However, this is rarely done in an open-ended, philosophical and curious way. Rather, it becomes a 'territoriality' question, a zoning regulation for these sorts of studies. Nowadays, to judge from our efforts in geography, it most often takes the form of plausibly locating the hazards 'box' within a model or diagram of the components and lines of interaction in man–environment studies (Kates 1971, Burton & Hewitt 1974, Fig. 12.2; Ayre 1975, p. 12). A moment's reflection usually shows that these are not 'models' of reality at all, but *managerial devices* to show the relations among the study areas or dimensions currently fashionable in the discipline and related fields.

The diagrams themselves tend to resemble nothing so much as an 'organisation chart' of a large bureaucracy. They often bear little or no relation to actual places or conditions, the material interactions or human experience involved. They are prescriptions for showing where academic and managerial categories fit together. However, it is not merely that reputations are made by this sort of activity. The placement of the problem is a necessary founding act whereby each specialty field demonstrates its coherence with the whole dominant view. It is an act that will be paid homage to as an intellectual demonstration at the start of each new study. This is important because hazards studies have rarely been research in the full sense. Most are seen in terms of 'applications', commonly on behalf of or as an assessment of public policy and the performance of particular agencies. The 'expertise', therefore, is invariably that of a discipline such as seismology or cognitive psychology, or a technique in, say, statistics or remote sensing. The 'research' is essentially an empirical study of questions specified by an agency's responsibility or a critique intended to contribute to management within such institutional frameworks. These considerations encourage the encapsulating of the problem in an official-looking diagram or 'model' of man–nature relationships.

Once we have located the hazards 'box', however impulsively, we can then concentrate upon it. We are at liberty to define parameters, revalidate properties, gather expensive data and seek cures or restraining devices, all within a

technical *monologue*. Thus encapsulated, the problem appears neutralised, objectified. Past and present notions that seem technically obscure or unpalatable are defused. Most comforting of all, the forces involved, so charged with drama, emotion and blame in the everyday world, become subordinate to objective dimensions and impersonal dynamics. We and the agency are simply doing a job. The work can then develop as a *well crafted* monologue. That is not only because the language is so specialised. We are now free to speak of hazards *alone*, as if all these 'events' and all that happens within each one belong not only to a separate domain, but to a single, albeit 'multivariate' reality.

'Monologue' is Foucault's word to express how society and its instrument psychiatry closed themselves off from a *dialogue* with madness and its sources in the everyday world. The psychiatrist was to be reason's priest, and not obliged to contemplate his own madness, nor his part in the social conditions that might drive others over those bounds where abnormality or passion were to be confined. So it is with hazards research in its main stream. It has invented its problem field to suit its convenience. It does not reflect upon the extent to which the institutions it serves – the societies that have made such technocratic authority possible – could be part of the problem. It does not reflect upon the flaws in itself, except in relation to what is deemed sophisticated in the current fashions of the scientific community. It gathers data about people at risk, but may not engage in dialogue with them. Most disaster reports in the so-called Third World are by persons who cannot speak the language of the area affected, or have no background in its sociocultural composition (see Chs 2, 5, 13).

However, none of this could alone sustain the dominant consensus. It could not leave so many feeling satisfied to treat hazards as a separate set of problems if there were not a deeper level of scientific, or supposedly scientific, understanding implicit in it. That underpinning of the justification for keeping hazards separate, of paying lip service only to their place in other man–environment relations, brings us back to the sense in which disaster is a specially intractable problem for scientific rationalism and technocracy. We cannot, in fact, divorce the development of the dominant view from the entire perspective of materialism and the social pressures upon materialist and secular institutions required to interpret and deal with unpalatable or apparently unmanageable material events. The formulations of the dominant view must be seen against the background of what can serve as acceptable explanations of natural disaster in a technocratic framework; as a strategy that allows us to 'save the facts' and ourselves!

The convenience of accidents

Conceptually and analytically, what the dominant view (the perspective that all the related hazards work has converged upon) does is to define the distinctive features of the problem through the language and apparatus of 'the accident'. Natural 'calamities' are, before anything else, severed from the rest of material life by being what the *Oxford English Dictionary* calls 'unforeseen contingencies'.

Toblin (1977) states that '[disaster] implies the occurrence of an *unusual* event which was *not* adquately *predicted* in time or place to allow measures to be taken for the protection of the threatened people and property' [my italic] (p. 13). The Soviet geographers Gerasimov and Zvonkova (1974) note that 'Natural hazards derive from highly dynamic processes whose elemental essence consists in their *indefinite* and *equivocal* manifestations' [my italic] (p. 243). We will not enquire here why Lenin, and from a quite different ideological perspective Sorokin, emphasised other socioeconomic or sociocultural interpretations, for this is the language of detente! White (1974) is quite firm about it and also states clearly its other implication that 'were there perfectly accurate predictions of what would occur and when it would occur in the intricate web of atmospheric, hydrologic and biological systems, there would be no hazard' (p. 3).

In such a view, the human ingredients are necessarily of a dependent or tangential nature, being responses and contingencies that stem from *un*anticipated damage by nature. To the extent that there are human conditions specifically affecting hazard, they are found to lie in the category so commonly invoked in accidents – 'human error'. The literature is full of reactions or conditions described as *un*informed, *un*sound, *un*planned, or impaired, irrational or arational.

Again, natural disasters are 'accidents' only in the sociocultural implications of the dominant view, not for its geophysical focus or the technologies it would bring to bear on the problem. There is an important contrast brought out between these 'accidents' and the scientific possibilities for handling them. Once research is under way, then indeed the 'controls', mechanisms and models take over the discussion. For it is refined statistical and geophysical techniques that alone are thought able to reduce these rare, high-magnitude, uncertain events to some sort of rational description. The common run of humanity is considered to have poor or 'subjective' impressions of 'the hazard' and its occurrence. Likewise, their ability to survive it at all seems puzzling. As one recent official document put it, 'Although human beings, unlike other animals, can project themselves into the future and make plans to deal with future *uncertainties*, they are more apt to be oriented to the "here and now". Present satisfactions, pleasures and rewards far outweigh their future counterparts and particularly their *indefinite* or *uncertain* counterparts' [my italic] (Working Group of Earthquake Hazards Reduction 1980).

Never mind the Saruman-like tone, the unsupported (unsupportable?) assumptions and an omniscience that would attribute such banalities to 'human beings', this is a genuine, concerned (but technocratic) expression of what hazards are about and how people fit into them. The role of research is also placed carefully. We are apparently in the presence of a new kind of

'white-man's burden'! There is little evidence that it is taken to apply only to North American or urban societies (cf. Waddell below).

For a variety of reasons, the research literature strives to dissociate itself from the full implications of these emphases and some will no doubt feel I am developing an unfair caricature of it. 'Accident research' has been at pains to rid itself of the view of accidents as 'accidental' (Haddon *et al.* 1964). The sociologists Quarantelli and Dynes (1972) have voiced the need for a 'principle of continuity' built into disaster research, to avoid the sense of the bizarre, or the impression that crisis behaviour bears little relation to pre-existing conditions and human roles. North American hazards work has continued to shift its language towards more neutral terms, preferring to speak now of 'risk assessment' or 'probabilistic consequences' (Whyte & Burton 1980), and to set research in ever more refined statistical and global 'systems' models.

Such developments reflect genuine unease. I suggest, however, that the sense of, indeed, the fundamental metaphysic of accident is so entrenched in the dominant view that these efforts are largely window dressing, dealing with issues that do not penetrate beyond conceptual preambles. In the technocratic style of work there is a structure of assumptions, and a use of science and management that always situates natural calamity beyond an assumed order of definite knowledge, and of reasonable expectation. More importantly it places disaster outside the realm of everyday responsibility both of society and individual. More important still, it makes assumptions about everyday life – about its being 'normal', 'stable', 'predictable' – that are in turn debatable. We shall return to that.

What we must now ask is how this accidentalism operates as a solution to the problem of explanation in a technocratic context. How have we, as scientists, managed to inform it with the appearance of logical necessity and technical sophistication?

Problems of explanation

The break with the past Viewed historically, and perhaps in relation to the sense of disaster among the vast majority of people who are today excluded from its monologues, the dominant view of hazards is decidedly odd. But the source of its oddness lies deeper than the expediency of a technocratic 'package'. Unlike virtually all past views of calamity, materialism, especially in its technocratic form, cannot readily attribute disaster to 'acts of God' *or* to 'Acts of Man'!

The former is inadmissable in scientific, utilitarian thought. However, it is the question of 'Acts of Man' that is more problematic. Obviously humans are involved. It is their vulnerability and misfortune that are the central concern. But the question of human *action* is the difficulty here. Action is not mere process. Properly defined, it refers to organised, deliberate attempts to bring about change or maintain a certain status (cf. Arendt 1958). It is not a matter

of spontaneous, impersonal, unmanaged change such as physical science sees in the workings of the atmosphere or lithosphere. Action constitutes the very domain of management and planning. Thus, action is comfortably spoken of in the exploitation of natural resources or government response to an actual disaster. But the utilitarian assumptions of the dominant view cannot contemplate human *action* as leading to destruction, to the collapse of institutions or disorganisation of the space economy. Materialism assumes that human activity derives from 'self-interest', whose first rule is 'survival', or at least belongs to an underlying principle of adaptation. An activity that directly invites catastrophe would not be wilfully put in place, *except* 'by accident'. To orchestrate devastation in a rational, materialist world is to be criminal or mad. War for some people is an exception, the domain of human action *par excellence*. Beyond that however, to argue that government, business, science or other institutions create disaster has been in a sense outlawed from rational discourse. It is portrayed as possible only by invoking 'conspiracy theories', and conspiracies again are supposed to be practised by criminals and imagined by the paranoid. I would just note here that the chapters that follow seem to me to give the lie to the idea that 'Acts of Man' do not bear a large responsibility for recent disasters, although few of the authors have found it necessary to talk of conspiracies.

It is also worth pausing to note how great is the assurance of the dominant view's proponents not merely of its superiority but that it has totally discredited views that prevailed in the past. The latter are generally labelled as religious, cosmological, magical or fatalistic. They are views that do not find the source or explanation of calamity to lie in an inherent potentiality of brute matter or its physical processes. They generally appear to us to depend upon non-material, magical and ethical imagery or direct human agency to interpret disaster (see Eliade 1954, Ch. 3; Blum & Blum 1963, Tuan 1980). 'God' is invoked, but rarely as an arbitrary actor with respect to man. Even when speaking of divine wrath as the immediate source of destruction, past societies invariably attribute this in turn to human responsibility. This may be expressed as competition between religio-magical communities and the strength of their divine sponsors or their faith. More often, calamities are ascribed to the workings of a moral *calculus* in human affairs. The ethical and societal aspects of human behaviour are central. In the West and Islamic lands until recently, the dominant view had been essentially that represented by the Old Testament prophets. For them, the message behind calamity or its threat is a people become immoral, idolatrous or vain (Heschel 1962, Ch. 9; Scott 1968, Ch. 6).

I need hardly review the sorts of developments that lead us to neglect and demean this enormous part of the human experience. For even sympathetic studies tend to present it as, at best, archaic. This entire cultural past is laid aside as a tissue of what John Brill calls 'original, childlike and primitive concepts' (1956, p. 6). And the further removed historically or geographically is a society from urban–industrialism, the surer studies of disaster are to find

its people to be 'fatalistic', 'subjective', and in the thrall of 'mystical', 'arational' or at least 'pre-scientific' notions (e.g. Kates 1971, cf. Torry 1978). But it is also an article of faith in the dominant view that by a sort of reverse logic the further removed people are from urban–industrialism and its technocratic forms, the more completely they are at the mercy of an elementary biophysical struggle with the habitat. Part of the justification for the technocratic monologue is, of course, that the vast majority of these societies seem not to have the faintest idea of their thoroughly Malthusian condition and the natural selection that arbitrates it! Necessarily therefore, the technocrat may presume to speak *for* these people, but can find little value in dialogue *with* them or learning from them.

The dominant view then, belongs to an historically special culture that seeks to interpret the world through its underpinnings in material phenomena and mechanism. Yet, disaster remains a difficult, perhaps a decisive, test of such explanations. It is a delicate problem for the prevailing interpretations of nature and human development and the way they are appropriated by technocratic institutions. These interpretations tend to be uniformitarian, evolutionary and normative. They articulate with a view of human life as essentially progressive. Disaster, taken literally, however, suggests revolutionary change. Flood, famine, pestilence suggest retrogression rather than progress. Severe degradation of the habitat suggests devolution, even the threat of extinction.

These would not be such problematic questions, but for the recent history of the sciences that has propelled them from the dispassionate realm of natural philosophy into the main street and market places of modern states. The sciences are identified with, and most of their more visible public figures *want* to be identified with, predicting, controlling and reproducing natural forces at will, rather than merely understanding them. In the process the sciences have become integral to the power and authority of leading institutions.

Disaster interpretations in the past were set within landscapes ascribed to the works of the Almighty, the labour of men's bodies and work of their hands. Today's landscapes show the most striking changes through technocratic initiative. In the minds of those who promote and resist such changes, applied science is made firmly responsible for them. If blame in damaging events (the still widespread world of moral rather than material calculation) were to be ascribed to a particular source, then it could very well be turned upon the technocratic ethos. Moreover when knowledge is mobilised for purposes of control, its failures and limits, especially if they result in human misery and disarray, become a threat not merely to its credibility as knowledge, but also as power.

Surely these are major considerations in the thrust to make natural extremes the cause, and accidents the human framework of, disaster? The 'space of the accident' emerges as an expedient if not a 'face-saving formula' (cf. Hawkins 1964).

On remaining calm Appeal to natural processes as cause is, however, a convenient scientific rationalisation too. The processes involved can readily be set within scales of space and time that dwarf them into normalcy. Globally, or over short spans of 'geological time', the conditions in natural disasters come to appear commonplace. Catastrophist notions – the idea that rare, extremely violent or unique events have shaped past and future – have met with solid and apparently secure resistance in the geophysical and biological sciences. Neocatastrophism, except in events as remote as the 'Big Bang', meets with even less favour than Cuvier's ideas. Though their expertise may be *applied* to sets of extreme events, geophysicists can feel that the extremes are part of measurement continua, and that such events as earthquake or volcanic eruption are, in the end, part of the inexorable development of the solid Earth. The power of scientific thought has derived especially from discovering scales and perspectives where phenomena seem to fall into elegant and parsimonious forms, regardless of how far these scales and perspectives diverge from the compass of everyday human experience.

The pressures to concentrate upon the geophysical conditions in disaster is great when we consider how much more difficult such detachment is from their sociocultural significance. When our concern is with people, the sense of disorder, of 'worlds' threatened if not destroyed, of meaningless and arbitrary death, is less easily dispelled from disaster. It challenges the belief in coherent development through conservative and lawful processes. Albeit emotional or subjective, disaster leaves its victims feeling their world will never be the same again; that an unprecedented revolutionary change took place – or ought to have taken place! Catastrophism has a strong psychosocial appeal as explanation for the powerless and victims of great distress. The dominant views of disaster in the past dealt with all of this by firmly locating *blame*. Calamity was not divided off in its *meaning* from the rest of life. The destruction was not therefore meaningless or *absurd* in Camus' sense, as it tends to appear in the materialist universe of the dominant view. It would be quite naïve to imagine that the legacy of such ways of thinking does not still exert an enormous pressure upon our dominant institutions and the scientific ethos.

While I cannot say this 'space of the accident' is the only one where technocratic thought could place natural calamities, and while it has not been the only scientific view, it is a persuasive one for science and technology. It makes of disasters not a 'judgement' but an 'unplanned side-effect'. They become not a limit but merely a 'frontier' of knowledge between the tamed and the wild, the controlled and the as-yet uncontrolled. Natural hazards, like disease, poverty, even death, become simply the unfinished business of our endeavours. We can then focus daunting technical equipment and expertise upon tasks technocracy understands: forecasting physical conditions; ever more complete containment of natural processes; educating government and the public; devising general, centrally controlled systems to protect those at risk; to zone 'high hazard' areas; redesigning installations; and if all else fails, organising relief on a grand scale. The hierarchy of expertise is thus

preserved. The wealthiest, best-equipped institutions can help and lead the less sophisticated or fortunate. Of course, whatever else may be done, there is no place for any sort of 'grass roots' input; no way for any but the 'experts' to break into the technical monologue. A 'citadel of expertise' has indeed been created here (cf. Roszak 1973).

Uncertainty and prediction

We have been discussing this 'citadel', as it were, from the outside looking in. We have asked about the interface between society and science, as it influences the kinds of questions we are obliged to consider and the sorts of answers that will satisfy us. But these are considerations often far removed from the everyday preoccupations of science and technology. Few scientists would feel commitment to the dominant view were it not also informed with logical and technical features of some sophistication and intrinsic interest. This, from the inside looking out, comes first. That it touches base in plausible ways with human need, managerial concerns and the politics of support for scientific activity is, for most of us, someone else's work.

The scientific ingredient that most helps to maintain the dominant view is the thoroughly respectable notion of *uncertainty* and related ideas. Uncertainty is the umbilical cord that grounds the otherwise gratuitous notion of the accident, the separate assessment of 'extremes', in a challenging and refined language. Elsewhere this ground involves the very frontiers of physical science. And it is a form of reasoning that has done more than anything else to transform social science, environmental science and most applications of scientific work in recent decades; namely through the use of statistical techniques and inference. Universally the hazards literature states that the fundamental problem with hazards, the ultimate reason why disasters occur, is that people have little or no way of telling when, where or to whom they may happen. The quotation from White (p. 44) represents this in its purest form. The uncertainty thus identified is not just intrinsically important. It is used to specify what is and is not likely to improve our grasp of the problem. And it provides scientific credibility for the treatment of natural disasters as 'accidents'.

Uncertainty then, in the form of probabilistic reasoning about threatening natural processes and the occasions of major damages, defines the technical logic and challenge of the dominant view. Herein lies its concern not merely with natural extremes, stochastic forecasting models and actuarial types of risk assessment. We are also led to flirt with the fashionable spin-off of the enormous statistics and probability industry. Examples exploited by geographers include aspects of the theory of games, Bayesian decision making, and various cognitive models designed to deal with uncertainty or imprecision in the 'perception of hazard'. The idea of 'bounded rationality' is an example

currently in vogue (Gould 1963, Gibson 1976, Kates 1978, Burton *et al.* 1978, Whyte & Burton 1980).

Statisticians, whose job it is to recognise the basic nature of the data with which a field works, have been serving all areas of hazards research through similar styles of analysis and probability models. The widespread application of 'extreme value statistics' is a case in point (Gumbel 1958). A statistician has described what is at issue in much hazards work with respect to seismic risk (Vere-Jones 1973). He recognises three categories of risk: 'geophysical risk', meaning the probability of an earthquake of a given magnitude in a given location; 'engineering risk', the probability that a given structure will fail; and, thirdly, 'insurance risk', meaning the probability that clients will make claims against a given policy. This nicely catches the flavour of hazards research as a concern with rare stochastically governed events. This also defines how they are 'accidents' in a seemingly rational sense. The realm of accidents has long been a specialty of statisticians (Maguire *et al.* 1952, Haddon *et al.* 1964, Gibson 1976). There is a substantial body of work, often not with applications in mind, that examines large-magnitude natural events as separate phenomena, using the same or similar probability models as in accident research (Hewitt 1970, Scheidegger 1975). It shows them often to appear as random or nearly random points in time and space, or to be separated by 'recurrence intervals' of nearly random length. Evidently, if it is these events that are 'the hazard', a probabilistic definition of the problem seems unavoidable. If human societies are unprepared or ill prepared for these events mainly because of their rare and uncertain occurrence, then predictability has to be the essence of the problem of management. In the end, only improved knowledge of when natural extremes occur, a chipping away at the degrees of uncertainty, are offered as a rational solution. Hence, the colossal commitment to means of improved forecasting.

We need to pursue the scientific and technical basis of this reasoning no further here. The logic of using statistical models to describe larger-magnitude geophysical events or sets of disasters is not in question. It is the transference of that logic, and the reduction of the interpretation of hazards largely to it, that I would call a face-saving and misleading formula. Impressive as the techniques may be in their home disciplines of statistical hydrology or seismology or actuarial science, they serve to misrepresent the sources and significances of natural disaster except in very narrow technical contexts. My argument is not that uncertainty, in a general sense, has no meaning here. It can reasonably be seen as a major ingredient of these as of most human affairs today. Nor am I suggesting there is no value in striving to foresee future developments and risks. The problem is with the way the *source* of the uncertainties involved is described; with that, the *kind* of prediction championed, and hence the severance of the interpretation of disaster from the rest of material life through these devices.

The myth of 'ordinary life'

Ultimately, the inadequacies of the dominant view arise less from what it says about disaster, than what it chooses to infer about the rest of human activity and its environmental relations. It is here that the whole fabric of the view and its foundation in the ideas of geophysical uncertainty and social accident take on the character of *myth*. As we have seen, its essential interpretive structure involves treating everyday life and disaster as opposites. The ongoing conditions that provide the setting for disaster are inferred to be 'stable', 'orderly' and 'predictable', or at least sufficiently so to be called 'managed' and even 'planned'. In the language of Burton *et al*. (1978) this is the 'human-use-system', typified by patterns of settlement and activity permitting effective and controlled use of natural resources. 'Hazard' arises from the intrusion into this activity of unforeseen, essentially independent natural processes of extreme and rare occurrence. The only ongoing manifestation of hazard is incautious settlement on natural features or in zones where those extreme events recur. Thus is the meaning of everyday life severed from that of disaster. Man's relations to nature are given two modes – one normal, secure, *productive* and the other abnormal, insecure and the occasion of losses. The dominant view pursues its analyses as though the continuities and discontinuities, the sources of stability and instability in human affairs, are uniquely defined at the times and for the places where damaging extremes terminate ordinary life.

Few things have done more to furbish the imagery of everyday life with the epithets 'normal', 'ordinary', 'scheduled', than the statistical treatment of social and natural conditions. Increasingly, social and environmental scientists work with data of the kinds defined, standardised and gathered by government and other centralised institutions. Statisticians know well that 'normal' social conditions are as much a fiction as the 'average man'. That does not prevent such constructs becoming the cornerstones of technocratic ideas of 'reality'.

Together, these notions of 'normal life' on the one hand and statistical uncertainty of natural extremes on the other form the rationalising of the 'accidentalism' of the dominant view. Everyday life appears therefore to affect disaster only fortuitously or by default.

The type of prediction being discussed in the dominant view is in turn of an official, technological and centralised sort. Even when asking about frost in New Guinea, drought in the Sahel, or earthquakes in remote Himalayan valleys, 'prediction' for the dominant view means that sort of forecasting served by the monitoring, data processing and mathematical expression that technocratic agencies provide. Such prediction is, in its turn, modelled from the sort of forecasting required in the day-to-day, clock-time regulation of industrial economies, machine technology and mass institutions. It is associated with those forms of social control peculiar to the productive and institutional forms of urban–industrialism. What appears as uncertainty in *that* context is taken to hold sway everywhere. Yet, with respect to rare, extreme fluctuations on nature, this sort of prediction encounters the same difficulties and failures as in its forecasting of the extreme swings and dislocations of economic and political kinds.

One might begin by pointing to the well known difficulties of preparing, communicating and getting appropriate responses to technocratic forecasting. Far more critical, however, is what is implied by making technical prediction the essence of the problem. What does it imply about 'ordinary life' and the means to encompass and defuse hazard within it?

First we can agree with Burton *et al*. (1978, p. 19) that hazard arises from the interaction of 'natural and social systems'. And suppose for the moment we agree that there would be no hazard if geophysical events were wholly predictable. That must surely mean, first, that all aspects of the natural environment are predictable or benign *except* the 'extremes'; but, secondly, that *all other aspects of human life are themselves either predictable or irrelevant*. It also implies that, if prediction of nature improves, society automatically follows with adjustments that remove risk to that level. Everyday life thus comes to appear as a 'frictionless' process, readily poured into whatever mould technology and the certainties of nature require. It is not difficult to see why some accuse this approach of a covert environmental determinism (Waddell 1977).

This approach also infers that persons and institutions are uniformly and unambiguously committed to removing known, manageable risks from everyone's life, failing to do so only where the risk is highly uncertain. There is a lot of evidence that human groups and institutions are rather less fervent about equity and social justice than that. In any case, it is an odd view, given the evidence that improved prediction and control of natural processes can have the effect of making people more careless, acting on a false sense that 'risks from nature' have been dealt with.

Even were human affairs in no way responsible for disaster or its forms, except by default, these assumptions about predictability would be debatable. Moreover, they lead us swiftly from the complex realm of human ecology to the inner sanctum of that 'new, illiberal practicality' that Mills (1959) described as characteristic of this sort of technocratic applied science. Specifically it leads to that dangerous transfer of the notion of 'predictability' as a feature of scientific method, to the partial and scientistic notion of *predicting society*, which is tantamount to controlling it. This is not the stuff of scientific enquiry but technocratic wishful thinking, a rhetoric that sees society as running smoothly only when fully predicted and managed by its 'experts'.

Moreover, what of that other, even larger body of thought and literature, much of it also technocratic, that describes *everyday life* in the 20th century as something *extra*ordinary? Technological innovation and risks, 'future shock' and unprecedented human powers are some of the main stresses said to be involved. From that view, it is natural disasters that are in some ways 'ordinary', the age-old scourges of civil societies signifying a kind of continuity in

human experience and predicaments. Here one might explore Robert W. Kates' suggestion that, after all, perhaps risk or hazard is conserved, merely being shifted around by socioeconomic change.

In such terms, the prevailing interpretation of disasters described above takes on the quality of myth. Of course, in a sociocultural framework that is not the condemnation it may appear. Careful examination of the mythologies of the past shows them to express and have been grounded in definite psychosocial contexts and predicaments. They are important evidence of the way knowledge is a social construct, although it is true we usually call an example 'myth' when its relevance has gone or it appears anachronistic in the context being discussed. I am calling the dominant view of hazards a myth in that sense too.

In the past, most myths actually reflected the views and problems of particular classes or activities; of princes or priests, farming or childbirth. I have described the dominant view of hazards in such a limited perspective of technocracy. Nevertheless, mythologies are generally grounded in a cosmic or genesis-type of myth that supposedly gives the others overall coherence. The dominant view of hazards masquerades as though it were the equivalent of a cosmic myth, subsuming all other approaches in an objective and fundamental ground. I find that unacceptable at least for social science, even if one believes in scientific materialism and promotes the technocratic organisation of human affairs.

For all that, one does not readily or lightly abandon a dominant view, whether myth, theory, paradigm or 'academic-research consensus'. This is not accomplished on mere grounds of logic or demonstration (cf. Kuhn 1962, Polanyi 1958, Ch. 1). Such procedures may help the scholar to modify a view or convince those involved to modify it. Geographers and other social scientists have been trying to do that in the hazards field for several decades. Indeed, I think the dominant view of hazards has arisen out of sufficiently rich founding statements by scientists to embrace all but the most radical proposals. But, as Whitehead said of 'the philosophy of organism', these rich grounds are lost in aspects that 'subsequent systematizations have put aside' (1929, p. v). For instance, as the hazards work of geographers has been gradually adsorbed into the dominant view, the rich possibilities deriving from the ideas of human ecology and geographic diversity have also been lost.

Alternative viewpoints

In contrast to the three features of the dominant view singled out earlier, it seems to me the chapters that follow tend to demonstrate:

(a) The important degree to which natural hazard is *not* explained by, nor uniquely dependent upon the geophysical processes that may initiate damage.

(b) The important degree to which human awareness of and responses to natural hazards are *not* dependent upon the geophysical conditions, whether their mechanisms, frequency or past experience of them. Rather hazard is seen to depend more upon concerns, pressures, goals, risks and, above all, orchestrated social changes that are tangential to, if not wholly indifferent to the particular society–environment relations where disaster has occurred. Perhaps more crucial still, effective or ineffective means to avoid or reduce risk are found to depend upon the ongoing organisation and values of society and its institutions.

(c) The important extent to which natural disaster, its causes, internal features and consequences are *not* explained by conditions or behaviour peculiar to calamitous events. Rather they are seen to depend upon the ongoing social order, its everyday relations to the habitat and the larger historical circumstances that shape or frustrate these matters.

These emphases do not arise from trivial or minor aspects of hazards and their human contexts. If they have validity, it has a profound bearing not just on the kind of social and environmental understanding geographers or anthropologists might contribute, but also on the general significance to be placed upon these problems.

In isolation, of course, in the absence of the dominant view described, our emphases would also add up to an unbalanced view. It would be wrong to suggest that events associated with flood or earthquake in no way reflect the nature of these geophysical processes. It would be indefensible to argue that the disruptions occasioned by disaster produce no distinctive, even unique, crisis phenomena. There are particular aspects of hazard that can be helped by improved geophysical forecasting. Nor are any foreseeable human actions going to remove the need to bring emergency assistance to ill equipped victims of natural calamities.

However, the burden of what follows requires the social scientist to consider seriously whether the dominant view has not got the whole problem of disaster back-to-front. My own view, and the one I see supported by what follows, is that:

(a) Most natural disasters, or most damages in them, are *characteristic* rather than accidental features of the places and societies where they occur.

(b) The risks, pressures, uncertainties that bear upon awareness of and preparedness for natural fluctuations flow mainly from what is called 'ordinary life', rather than from the rareness and scale of those fluctuations (see Ch. 14).

(c) The natural extremes involved are, in a human ecological sense, more expected and knowable than many of the contemporary social developments that pervade everyday life.

There is a good deal of evidence that the settings, where recent disasters have occurred, are suffering extraordinary sociocultural change and environmental impacts in an ongoing way (e.g. Hewitt 1970, 1976, 1980). Are these transformations, in and due to social circumstances, more manageable, expected, or certain for the victims of disaster than natural extremes? What is more characteristic of the Sahel, and to be expected by its long-time inhabitants: recurrent droughts or the recent history of political, economic and social change? What are more certain along the San Andreas Fault: occasional large earthquakes, or the sprawling developments of its so-called, 'post-industrial' society?

A careful look at a century or two of history in the 'hazard-prone' regions of today generally shows the sorts of geophysical processes associated with disaster to be entirely likely, even inevitable. In any group of inhabitants there are those who know the processes have occurred and can occur again. However, I am using certainty and uncertainty here in a broad biosocial sense, in terms of cultural reproduction, rather than the technological prediction discussed above. In most places and segments of society where calamities are occurring, the natural events are about as certain as anything within a person's lifetime, or at least that of himself, his children and grandchildren. One of the few real *advantages* we have with these risks is that the large task of being ready for them can be accomplished incrementally, *because* they are relatively rare events! Or should a sane social order disregard the likelihood of massive destruction, simply because it is not quite sure on which day of which year in the next decade or two it will occur? Is it, as Brecht's philosopher suggests, 'because people know so little about themselves that their knowledge of nature is so little use to them. [They] can cope with earthquakes, but not with their fellows' (Brecht 1965, p. 31). In hazards research, at least by social scientists, even for earthquakes it is our 'fellows' with whom we are required to cope first, and earthquake processes second. Here again one must resist a technocratic fiction.

Are people unaware and poorly prepared because natural extremes are rare and unpredictable? Are they indifferent to the possibility of flood or earthquake because preoccupied with 'present gratifications'? Or is it because the everyday conditions of work, life support, social and mental security or the artificial environment require *all* of their risk-avoiding and risk-taking energies? Do 'laymen' appear 'poorly adapted' to us because the socially narrowed world of technocratic or academic specialists leave us incapable of recognising the realities with which other persons and groups must deal?

Surely, in the urban–industrial, commercial societies for which the dominant view is tailored, most people simply have not the time or means to prepare for and recover from natural disaster. It has become as difficult for individuals and families to set aside time, resources and worry to guard against these things as to care for their aged parents, the chronically sick, the handicapped, mentally deranged, and all the other 'abnormal' and *especially* 'unproductive' elements of the human condition. Moreover, one of the characteristic impacts of modernisation is to weaken and eventually destroy the traditional arrangements whereby extended family, village, 'tribe', reciprocal duties of lord and people, absorbed and dealt with such problems. This is surely a major aspect of the process that puts the poverty-stricken, the beggars, orphans, amputees, victims of famine and flood on the streets of cities in 'developing countries', as it did in Europe and North America until 'institutions' were created to hide them in. Such social developments, flowing from that most fundamental of all geographical and human ecological processes of modernisation – *alienation from the land** – are integral to the unavoidable vulnerability of 'ordinary' folk to natural calamity; to the futility of their developing a sophisticated knowledge of the risks even if they had the leisure for it; and, in the end, of the responsibility that indeed rests firmly upon centralised, technocratic institutions and hazards research.

There are natural forces and some damages in most disasters that lie beyond all reasonable measures any society could make to avoid them. What I believe to be definitive of the disasters I have examined is, however, that most of them would not be disasters, and many of the damages would not (indeed *do not*) occur except as a direct result of characteristic and vulnerable human developments (e.g. Hewitt 1976). These developments record mainly the mismatch between the requirements of sensitive, secure environmental relations at the local or regional levels – more exactly in certain segments of society and activity at these levels – and the demands of those extensive geographies of power and economy with which technocratic strategies have grown up, and mainly serve.

What is being explored in the chapters below is firstly a revised vision of how and why disaster occurs, giving full credit to the ongoing societal and man–environment relations that prefigure it. This immediately makes the range of phenomena that form the main stream of the social sciences of direct interest. It means that the common concerns and competence of human geography, human ecology and anthropology are of intrinsic interest to the understanding of hazard, rather than fortuitous matters arising only, and in special ways, when there is the impact of natural extremes or their threat. If society in its everyday development is integral to 'risks from nature', then questions of social order become central matters of research and discussion. That includes the exercise of political and economic power, as integral to vulnerability and management and the redistribution of risk by institutional means. If and where social scientists prefer to treat these matters in terms of 'impersonal, objective' forces, of natural extremes and crisis responses, then their social science itself is, as Copans points out below (Ch. 5), a major part of the problem.

There are also serious implications for the evaluation of crisis management too. The dominant view serves to justify the channelling of a disproportionate share of resources and expertise into projects that are only indirectly or not at

* And therefore from nature and the 'man–environment relations' that must develop to deal with natural extremes.

all concerned with the human misfortunes involved. Whatever its intrinsic interest, the enormous commitment to geophysical monitoring and prediction deals with a peripheral rather than a central ingredient of disaster. The evidence assembled below and elsewhere serves to suggest that the dominant view supports forms of official response to disaster that are almost guaranteed to see that those whose need is greatest will be heard, understood and helped the least. Relief and reconstruction are shown to be often disproportionately focused upon restoring, and *more* than restoring, the infrastructural arrangements of the more powerful institutions of the economy, the state and international system, rather than direct responses to the needs of victims.

Nevertheless, the chapters in this book do not suggest radical abandonments of the technocratic approach. Like most contemporary scholarship they are more or less powerfully influenced by technocratic ways of working. They might even be subsumed under a technocratic framework – *if* it were not of the sort described above. Most of us utilise the same sorts of data and methods found in the dominant view. However, the perspectives brought to bear and the evidence that influences us most do not square with the dominant view. We need a new consensus. That consensus is unlikely to do much about the grosser misconceptions of the dominant view or to be intellectually honest and scientific unless enquiry is much more independent of the pressures and interests of technocratic institutions. It is also unlikely to improve matters unless, in due course, it can influence these institutions at least to adopt frames of reference that are more aware of their own serious limitations in face of these problems and of the predicaments of those who most often suffer disaster and are least equipped to deal with it.

Of singular importance for geographers and anthropologists is the sense in which the dominant view is unashamedly indifferent to history and to human and environmental diversity; the way it becomes more abstracted and irrelevant to human predicaments the farther removed they are from urban–industrial centres and processes. When looking at hazards in a cross-cultural context, and disasters in non-Western, non-industrial contexts, one begins to have the suspicion that the authority of the dominant view derives from much the same source as Said (1978) sees in the European view of 'The Orient'. It involves an invented geographical vision that is powerful 'a) because a white [*sic*] specialist with highly refined scientific techniques could do the sifting and restructuring, and b) because a vocabulary of sweeping generalities . . . referred not to a set of fictions but rather . . . [in classical empiricist terms] . . . to a whole array of seemingly objective and agreed upon distinctions' (p. 233).

Concluding remarks

To summarise, I find much that is fascinating and useful in work that falls within the dominant view. In criticising it, I have criticised most of my own

past work which largely pursued the dominant perspective. Yet, I believe that this perspective, which pervades natural disasters research, is the single greatest impediment to improvement in its quality and effectiveness. The perspective functions as though 'objective', 'general' and rigorous, but its rigour and generality are achieved through an extreme, opportunistic narrowing of interpretation and empirical interest. This involves a covert environmental determinism and the language of the accident. Yet it serves to conceal both a particular *metaphysic of enquiry* and *politics of management*. The former involves the face-saving formula of a 'natural sciences' style of analysis. The latter relies on a sort of *habeas corpus*, whereby disaster is appropriated and severed from its roots in the rest of material life. Behind that is a view of management whose obsession is with 'normality' in the productive functions of society. Moreover, technocratic thought never for a moment pauses to question how those functions require and depend upon a centrally planned socioeconomic order or whether that is always or necessarily more reliable and sophisticated. Hence, to that order goes precedence in the treatment of disaster. The point is most obvious in so-called Third World, 'peripheral' areas, as can be seen in the descriptions of Waddell, Morren, Copans, Hall, and Watts below. Regan's discussion of the Irish 'potato famine' reminds one that there is a certain structural recurrence in these relationships of development, dependency and centralised power. But the obtuseness of the dominant view is no less evident in the socioeconomic ramifications of natural hazards in the so-called First World, 'affluent', or central states (see Chs 3, 4, 11 & 12).

In sum, the geophysicalism of the dominant view hides within the assumptions that natural calamity is essentially the breakdown of the productive functions of society and, as crisis, is essentially an infringement upon the centralised ordering of space – or in remoter areas, an indicator of what happens when you lack the benefits of this order. The restorations of productivity and reimposing of 'normal' relations become the main prescriptions of crisis management, relief and reconstruction. The ability to predict or contain natural processes in a technocratic framework becomes the main goal for disaster prevention. Now, I question whether this recognises some major, indeed *the* major, ingredients of disaster. Because it fails to do so I think it fails to effectively deal with hazards problems. I think, in particular that it fails to recognise how the roots and occurrence of contemporary disasters depend upon the way 'normal everyday life turns out to have become abnormal, in a way that affects us all' (Brecht 1965). The chapters below show the assumption that what is best in dominant view may not be best for the victims of disaster. Meanwhile the continuing burden and changing forms of damage from natural processes create a growing sense that the management strategies supported by the dominant view become more and more like King Canute commanding the waves.

References

Albrow, M. 1970. *Bureaucracy*. New York: Macmillan.

Ang, A. H-S. 1978. *US–Southeast Asia Symposium on Engineering for Natural Hazards Protection*. Rep. Nat. Sci. Foundation Univ. Illinois, Urbana.

Arendt, H. 1958. *The human condition*. Chicago: University of Chicago Press.

Armytage, W. H. G. 1965. *The rise of the technocrats: a social history*. London: Routledge & Kegan Paul.

Ayre, R. S. 1975. *Earthquake and tsunami hazards in the United States: a research assessment*. Inst. Behavioural Science, Univ. Boulder, Colorado.

Baker, G. W. and D. W. Chapman (eds) 1962. *Man and society in disaster*. New York: Basic Books.

Barton, A. H. 1969. *Communities in disaster: a sociological analysis of collective stress situations*. New York: Doubleday.

Berger, P. and T. Luckmann 1967. *The social construction of reality*. Garden City: Doubleday.

Berlin, G. L. 1980. *Earthquakes and the urban environment*. Boca Raton, Florida: CRC Press.

Blum, R. and E. Blum 1963. *The dangerous hour: the lore and culture of crisis and mystery in rural Greece*. London: Chatto & Windus.

Bolt, B. A. 1978. *Earthquakes: a primer*. San Francisco: W. H. Freeman.

Brecht, B. 1965. *The Messingkauf dialogues*, (transl. J. Willet). London: Methuen.

Brictson, R. C. (ed.) 1966. *Symposium on emergency operations*. System Dev. Corp. Santa Monica, California.

Brill, J. 1956. *The chance character of human existence*. New York: Philosophical Library.

Burton, I. and K. Hewitt 1974. Ecological dimensions of environmental hazards. In *Human ecology*, F. Sargeant (ed.), II, 253–84. Amsterdam: North-Holland.

Burton, I., R. W. Kates and G. F. White 1978. *The environment as hazard*. New York: Oxford University Press.

Cooper, D. 1978. *The language of madness*. Harmondsworth: Penguin.

Copans, J. (ed.) 1975. *Sécheresses et famines du Sahel*. Paris: Maspero.

Curry, L. 1964. The random spatial economy: an exploration in settlement theory. *Ann. Assoc. Am. Geogs* 54, 138–46.

Disaster Research Group 1961. *Field studies of disaster behaviour*. National Academy of Sciences–National Research Council, Washington, DC.

Dynes, R. R. and E. L. Quarantelli 1968. Group behaviour under stress. *Society and Social Research* 52, 416–29.

Eijkman, P. H. 1911. *L'internationalisme scientifique*. The Hague: W. P. van Stockum.

Eliade, M. 1954. *Cosmos and history: the myth of the eternal return*, (transl. W. R. Trask). New York: Harper.

Foucault, M. 1965. *Madness and civilisation*, (transl. R. Howard). New York: Mentor Books.

Gerasimov, I. P. and T. V. Zvonkova 1974. Natural hazards in the territory of the USSR: study, control and warning. In White (1974, pp. 243–53).

Gibson, S. B. 1976. The use of quantitative risk criteria in hazard analysis. *J. Occup. Accidents* 1, 85–94.

Gould, P. R. 1963. Man against his environment: a game theoretic framework. *Ann. Assoc. Am. Geogs* 53, 290–7.

Grayson, P. K. and P. Sheets (eds) 1979. *Volcanic activity and human ecology*. New York: Academic Press.

Grosser, G. H., H. Wechsler and M. Greenblatt (eds) 1964. *The threat of impending disaster: contributions to the psychology of stress*. Cambridge, Mass.: MIT Press.

Gumbel, E. J. 1958. *Statistics of extremes*. New York: Columbia University Press.

113

Habermas, J. 1973. *Theory and practice*, (transl. J. Viertel). *New York: Beacon Press*.

Haas, J. E. and R. S. Ayre 1969. *The western Sicily earthquake of 1968*. Washington, DC: National Academy of Sciences.

Haas, J. E., R. W. Kates and M. J. Bowden (eds) 1977. *Reconstruction following disaster*. Cambridge, Mass.: MIT Press.

Haddon, W., E. A. Suchman and D. Klein 1964. *Accident research: methods and approaches*. New York: Harper & Row.

Hawkins, D. 1964. *The language of nature: an essay in philosophy of science*. San Francisco: W. H. Freeman.

Heschel, A. J. 1962. *The prophets: an introduction*. New York: Harper & Row.

Hewitt, K. 1970. Probabilistic approaches to discrete natural events: review and theory. *Econ. Geog.* 46 (2), 332–49.

Hewitt, K. 1976. Earthquake hazards in the mountains. *Natural History* LXXXV, 30–7.

Hewitt, K. 1980. Review: 'the environment as hazard'. *Ann. Assoc. Am. Geogs* 70 (2), 306–311.

Hewitt, K. and I. Burton 1971. *The hazardousness of a place: a regional ecology of damaging events*. Univ. Toronto, Dept of Geog. Res., Publn no. 6.

Huntington, E. 1945. *Mainsprings of civilisation*. New York: Wiley.

Kantarovich, L. V., G. M. Molchan, V. I. Keilis-Borok and E. V. Vilkovich 1970. *A statistical model of seismicity and an estimate of the basic seismic effects*, (transl. J. Findlay). Izvestia Earth Physics, no. 5, 85–102.

Kates, R. W. 1971. Natural hazard in human ecological perspective: hypotheses and models. *Econ. Geog.* 47, 438–51.

Kates, R. W. 1978. *Risk assessment and environment hazard*. Scientific Committee on Problems of the Environment (SCOPE), Rep. no. 8. Chichester: Wiley.

Kendrick, T. D. 1956. *The Lisbon earthquake*. London: Methuen.

Kuhn, T. 1962. *The structure of scientific revolutions*. Chicago: University of Chicago Press.

Lemons, H. 1957. Physical characteristics of disasters: historical and statistical review. *Ann. Am. Acad. Pol. and Soc. Sci.* 309, January, 1–14.

Lifton, R. J. 1970. *History and human survival*. New York: Random House.

Maguire, B. A., E. S. Pearson and A. H. A. Wynn 1952. The time intervals between industrial accidents. *Biometrika* 39, 168–80.

Mannheim, K. 1952. *Essays on the sociology of knowledge*, P. Keskemeti (ed.). London: Routledge & Kegan Paul.

Marx, K. 1964. *The economic and philosophical manuscripts of 1844*, transl. M. Milligan, D. J. Struik (ed). New York: International Publishers.

McNeill, W. H. 1976. *Plagues and peoples*. Garden City, New York: Doubleday.

Mills, C. W. 1959. *The sociological imagination*. Oxford: Oxford University Press.

Mitchell, K. 1974. Natural hazards research. In *Perspectives on environment*, I. Mannors and M. Mikesell (eds). Commission on College Geography, Assoc. Am. Geogs Publn 13.

Mumford, L. 1967 and 1970. *The myth of the machine*. New York: Harcourt, Brace & World.

NAS (National Academy of Sciences, US) 1976. *Earthquake prediction and mitigation options for USGS and NSF programs* (Newmark report). Washington, DC.

NAS 1980a. *The atmospheric sciences: national objectives for the 1980s*. Steering Committee for the Atmos. Res. Rev., Comm. Atmos. Sci.; Ass. Math. Phys. Sci., Nat. Res. Council, Washington, DC.

NAS 1980b. *US Earthquake observations: recommendations for a new national network*. Comm. Seismol. Nat. Res. Council, Washington, DC.

O'Keefe, P., K. Westgate and B. Wisner 1976. Taking the naturalness out of natural disaster. *Nature* 260, 15 April.

Oliver-Smith, A. 1977. Disaster rehabilitation and social change in Yungay, Peru. *Human Organization* 36, 5–13.
Polanyi, M. 1958. *Personal knowledge: towards a post-critical philosophy*. New York: Harper & Row.
Quarantelli, E. L. (ed.) 1978. *Disasters: theory and research*. London: Sage.
Quarantelli, E. L. and R. R. Dynes 1972. When disaster strikes. *Psychology Today* 5, 66–70.
Roszak, T. 1973. *Where the wasteland ends*. Garden City, New York: Doubleday.
Rothman, D. J. 1971. *The discovery of the asylum: social order and disorder in the New Republic*. Boston: Little, Brown.
Said, E. 1978. *Orientalism*. New York: Random House.
Scheidegger, E. 1975. *Physical aspects of natural catastrophes*. New York: Elsevier.
Scott, R. B. Y. 1968. *The relevance of the prophets*, (revised edn). New York: Macmillan.
Smith, de W. (ed.) 1957. Disasters and disaster relief. *Ann. Am. Acad. Pol Soc. Sci.* 309.
Soloviev, S. L. 1978. Tsunamis. In Unesco (1980, pp. 118–39).
Sorokin, P. A. 1942. *Man and society in calamity*. New York: Dutton.
Strahler, A. N. and A. H. Strahler 1973. *Environmental geoscience: the interaction of natural systems and man*. Santa Barbara, California: Hamilton.
Swiss, Re. 1978. *Atlas of seismicity and volcanism*. Kummerly-Frey, Switzerland: Swiss Reinsurance Company.
Toblin, J. 1977. Disaster prevention and control in the earth sciences. *Impact of Science on Society* 27, 131–9.
Torry, W. I. 1978. Natural disasters, social structure and change in traditional societies. *J. Asian Afr. Studs* 13, 167–83.
Tuan, Yi-Fu 1980. *Landscapes of fear*. New York: Pantheon Books.
Unesco 1980. *The assessment and mitigation of earthquake risk*. Paris: Unesco.
Vere-Jones, D. 1973. The statistical estimation of earthquake risk. *Bull. NZ Soc. Earthquake Engng*, September, 122–7.
Waddell, E. 1977. The hazards of scientism: a review article. *Human Ecology* 5(1), 69–76.
Walford, C. 1878–9. *Famines of the world, past and present*. New York: Burt Franklin.
Weber, M. 1947. *The theory of social and economic organizations*. Oxford: Oxford University Press.
White, G. F. 1961. The choice of use in resource management. *Natural Resources J.*, 23–40.
White, G. F. (ed.) 1974. *Natural hazards: local, national, global*. Oxford: Oxford University Press.
White, G. F. and J. E. Haas 1975. *Assessment of research on natural hazards*. Cambridge, Mass.: MIT Press.
Whitehead, A. N. 1929. *Process and reality: an essay in cosmology*. New York: Free Press.
Whyte, A. V. and I. Burton (eds) 1980. *Environmental risk assessment*. SCOPE, Report no. 15. Chichester: Wiley.
Working Group on Earthquake Hazards Reduction 1980. *Earthquake hazards reduction: issues for an implementation plan*. Office of Sci. and Technol. Policy, Executive Office of the President, Washington, DC.
Yoshino, M. M. (ed.) 1971. *The water balance of monsoon Asia*. Honolulu: University of Hawaii Press.

2 *Coping with frosts, governments and disaster experts: some reflections based on a New Guinea experience and a perusal of the relevant literature*

ERIC WADDELL

'Disaster' in the New Guinea Highlands

In 1972 there was a prolonged drought in much of New Guinea.* Above 2300 m it gave rise to some 30 nights of ground frost over a four-month period. The frosts did considerable damage to the natural vegetation and to the food gardens of the subsistence agriculturalists living there. This damage is to be explained by the fact that they have a largely tropical lowland (and therefore non-frost resistant) domesticated food complex. The immediate reaction of local expatriate observers was to interpret this 'extreme geophysical event' as being of 'disaster' proportions. Following representations to the central government a massive famine relief programme was mounted.

The programme had two stated objectives: first, to maintain the existing nutritional status of the population, and, secondly, to ensure a rapid return to normalcy. It lasted 8 months, involved feeding up to 150 000 people using Australian rice and Japanese tinned fish that had probably been caught off the coast of New Guinea. It dictated the co-operation of the Royal Australian Air Force, plus the co-opting of Papua–New Guinea Administration and local missionary personnel. In direct costs, $(Aust.) 3.0–4.5 million were spent on the exercise. As a relief operation it was a *total* success. No one died, the nutritional status of the population actually increased (mortality rates dropping to two-thirds the normal figure), and the return to 'normalcy' occurred much faster than expected. It became 'a famine that never was'.

Why was the programme mounted in the first place among a population where the level of 'dependency' was extremely low? Through most of the area 'contact' had been established by the colonial government only in the 1950s. The production of commercial crops (pyrethrum and temperate vegetables) had only commenced a few years before, with total annual sales not above $(Aust.) 250 000.

* This summary is based on Waddell (1975).