THE FUTURE OF HAZARD RESEARCH: A REPLY TO WILLIAM I. TORRY

William I. Torry's (WIT's) lengthy critique of our book *The Environment as Hazard* (BKw), and his associated 'general reflections on disaster research,' deserve a constructive reply despite their acerbic tone, gratuitous assertions, and special concern for neglected disciplines. To answer all the points of criticism would be a tedious rehearsal of citations and evidence from the geographical literature of which WIT seems unaware, but which is well known to practitioners in the field. Instead, we respond in three ways. First we show that the critique is based upon some fundamental misconceptions of the nature of hazard research and the purpose of *The Environment as Hazard*. Second, we respond in detail on the matter of definitions. Our rejection of WIT's criticisms here most clearly illustrates the difference in our approach. Third, we welcome the positive suggestions that are found in WIT's invective and suggest the need to act on them.

In the opening paragraphs of his essay WIT states that he 'looked forward to this volume ... long before it appeared in print,' and he thinks that 'when men of such stature [sic] collaborate to distill the findings of so many years of research, one is entitled to expect a very special product indeed.' Thus does WIT set out to review a book according to his own preconceived expectations. 'Perhaps,' writes WIT 'those who expect a lot invite disappointment.'

These preconceptions inform much of the rest of the review. WIT (p. 369) writes that the book 'represents a remarkable vision that carries with it some formidable methodological prerequisites. First, a comprehensive survey of the literature is in order ... The preliminary product should be a classificatory schema.' And so on. The prerequisites, the literature survey, the classificatory schema, seem to be WIT's canons of how to write the definitive work on disasters.

*The Environment as Hazard* is not such a book, and it was never intended to be. WIT has been misled by his own false expectations. We do not dispute the need for a full-blown

*Canadian Geographer*, xxv, 3, 1981
critique of hazard studies’ and we would be delighted if WIT or others would undertake such a task. Our effort was and is much more modest. We aim to summarize the results of the collaborative research carried out chiefly in the period 1968–76 under the auspices of the International Union’s Commission on Man and the Environment, and supported by the US National Science Foundation. The preface makes this perfectly clear. Yet WIT twice quotes out of context the first sentence of the preface which states: ‘In one sense this volume is a summation of what we think has been learned as to how individuals and social groups respond to extreme events in nature.’ and uses this to quip (p. 369) that BKW ‘have failed to learn a great deal and their ignorance is totally unjustified.’ In fact, the preface continues: ‘In another sense it [the volume] is a point of departure, as it contains tentative conclusions that many of our colleagues believe should be reexamined, and uses methods that cry out for refinement. We are sensitive to these deficiencies, but have concluded that a rough synthesis at this time will foster rather than hinder more searching examination of the basic questions.’ To this claim, as WIT does, that ‘their statement bears an authoritative tone, capable of conveying to those unfamiliar with the relevant literature a misleading impression that The Environment as Hazard is definitive and probing,’ is intentionally to misinform the reader, presumably to provide a rationale for the obloquy which follows.

Probing? We certainly hope so. Definitive? Not at all. Indeed as many of our collaborators can attest we quite consciously rejected any attempt at such an ambitious task and continually referred to the manuscript in preparation as ‘the slim volume.’ Is it nevertheless true that we ‘failed to learn a great deal’ and that our ignorance is ‘totally unjustified’? Clearly, from the context, what WIT means here is that we failed to pay attention to the work of non-geographers, especially anthropologists and sociologists. As far as our own research and that of our collaborators is concerned, he accuses us of making statements which ‘exceed the limits of existing evidence.’ Our failures evidently include lack of citation to francophone publications on the Sahel; failure to cite ‘geography’s most important community-level disaster relief study – the most incisive culture-specific account of frost adjustment modes in print’; virtually ignoring ‘the vast anthropological hazard/disaster literature’; ‘inexplicable disregard of relevant ethnographies’; allowing ‘historical case studies [to escape notice]’; bypassing ‘historical and epidemiological studies altogether’; and so on (WIT, p. 369).

Quite apart from his misinterpretation of our purpose, we are not convinced that what WIT wants would greatly affect our use of the comparative geographical studies in advancing our applied and practical objectives. Much of the literature we did consult is too overloaded with ethnographic detail and too case-specific to permit useful generalizations to be drawn which would help those at risk from natural hazards today.

WIT wants us, as well as others who are writing in the field, to spend more time getting all our thinking straight before we seek to apply any of it. He admonishes us to study more. Since we lack understanding at the local level, we must defer action until such work has been carried out and gathered into a new model linking ‘game theory, central place theory, general systems theory, psychoanalytic theory and role theory to collective behaviour in hazard/disaster settings’ (WIT, p. 374). A commendable task! From different premises some Marxist critics say, in effect, that since the international economic and social order is responsible for exposing people to hazards in the first place, or for exacerbating hazard effects, or for undermining traditional coping mechanisms, the first priority for action is to overturn that order. We do not consider it necessary, however, to spend two years in the village gaining the local understanding we lack before seeking to use knowledge already gained to remedy serious deficiencies in hazard management at national and international levels. Nor do we think it impossible to mitigate hazards, even in a world marked by substantial social and economic inequality, and even without the help of the new model WIT would like us to develop. Surveying some examples of the wide variety of response to hazard, and some of the examples of the massive disasters that can result, we are convinced that steps can be taken now to alleviate the problem and to reduce its growth in the future.
understandings that are available now and that, if adopted, can lead in the medium term to reduced vulnerability to hazard. We find the conclusions wrt draws in his own critique of the field coincide remarkably closely with this view.

We are grateful for wrt’s instruction on the further work that needs to be done, as well as to the Marxist critique for emphasizing the role of broad socio-economic structures in creating hazard. But arguing for overall social revolution or more detailed study as necessary steps to reducing the toll of global hazard is the counsel of despair. The alleged deficiencies in our work derive from our pragmatic philosophy in which we maintain that what has been learned already is immediately useful to decision-makers and those who are at risk from natural hazards.

wrt also chides us for terminological confusion over disasters, hazards, and extreme events and has seemingly not made much effort to understand our use of terms. He cites our definitions in part and then purports to be confused by them. It is patently clear that we draw a careful distinction between the extreme events in nature themselves, and the hazards that are created by the interaction of human use with natural events (pp. 19-22). It is perhaps unfortunate that in the English language words such as flood and drought are commonly used in both ways: i.e., a flood is a period of high flow on a hydrograph to which a variety of hydrological definitions can be given, and it can also be a hazard involving water and property located in the flood plain. It would, of course, be quite possible whenever the word flood (or drought, etc.) is used, to indicate by some social-science jargon or symbolic terminology in what sense it is being used. One might say ‘flood event’ or ‘flood hazard’ for example. But in our experience the creation of compound nouns rarely does much to aid comprehension in English.

The English language creates similar difficulties in distinguishing between hazard and disaster. Like the average person we use the two words interchangeably except that by disaster we mean a more damaging occurrence, in terms of deaths, injuries, monetary losses, disruption, or psychic disturbance. This usage is quite apparent. Not all hazards are disasters. The overwhelming proportion of losses from extreme events in nature probably occur on a small scale. Small flood losses, accidental drownings of one or two people, modest declines in nutritional intake due to drought, and the disruption of public and private urban facilities due to heavy snowfall are not disasters, even though they are associated with hazards. The distinction between hazard and disaster is a common-sense qualitative and quantitative distinction that any layperson not befuddled by the need for pseudo-scientific exactitude can readily comprehend.

We do, in part of our analysis, adopt a definition of disaster that was first used by Sheehan and Hewitt: at least 1 million dollars in damages or 100 deaths or injuries. The reason for the adoption of this clear-cut, easy to apply, and necessarily arbitrary definition is simply that only in this way can roughly consistent data about the number, distribution, and trends of such events be produced. To call the method ‘conceptually empty’ and ‘largely irrelevant’ is unhelpful. Clearly people measure the magnitude of hazards by the number of deaths and injuries and economic loss. In much of the world the losses in goods and services need to be, and are, estimated in monetary terms as a matter of public policy and for comparative requirements. No doubt wrt’s ‘small atoll community, relying largely on subsistence cultivation and fishing could take several months to recover without outside aid, after the onslaught of a powerful hurricane,’ even without losses of 1 million dollars or as many as 100 deaths or injuries. That does not mean that we fail to note the gravity of such a misfortune, even though it would not appear in Sheehan and Hewitt’s catalogue. An important thrust of geographical research into natural hazards has been the consideration of the long-term adjustment process of society to environment and not simply the most evident disastrous events. We use the Sheehan and Hewitt approach because it provides an approximate indication. We do not pretend that it defines reality but only that it is a conceptually relevant way of handling data that otherwise remain intractable.

As elsewhere in his review, wrt has taken us to task for what we have done but for failing to conform to his own methods and meanings. One reason for his displeasure with our discussion of disasters appears to be that it does not conform to his own definition. ‘As I have
indicated elsewhere,' he writes (p. 370), 'a disaster involves damage to a community or communities so severe that most or all major public and private facilities no longer provide essential social and economic services without extensive replacement or repair.' 'This definition,' he tells us, 'applies whether or not people are killed.' We are sure it does, and we may find it helpful. We do not. It is conceptually less complete, relying only on damage or disruption to 'services.' It is not less arbitrary than the Sheehan and Hewitt definition, and it could not be applied consistently to any set of data that we know of or can imagine being assembled on a global scale. When applied to any specific case, the definitions of 'severe damage,' 'major facilities,' and 'extensive replacement' are inoperable. The matter of definitions is admittedly difficult and we lay no special claim to having arrived at an ideal solution — simply one that for some practical purposes is useful and yields new insights.

Finally, although we are unhappy with our concentration on geographic studies and their conduct, he does not take major exception to our findings as to what lessons are worth sharing on the national and international levels. It is these suggestions as to public policy which have received much attention and to which we invite further critical review. In a number of high-income countries, as well as in many developing countries, there is a growing concern over the current trends as we have identified them.

We note that Witt's own appraisal of deficiencies in public action with regard to natural hazards does not differ greatly from our own. We agree that 'hundreds of millions of the developing world's urban and rural poor co-exist in an increasingly precarious relationship with their natural surroundings' (p. 376). Increased vulnerability to extreme events is also to be observed in developed countries. A great deal more research will be required to specify all of the relationships involved and thereby to illuminate the choices that may be made by individuals and public agencies in further response to extreme events. In time this no doubt will be aided by one or more comprehensive monographs of the type which Witt advocates and which we did not seek to produce. A broad array of questions need to be pursued. At the same time, the lessons that can be drawn from the studies to date need to be applied if enlarged dislocation and distress are to be avoided. We hope that our exploratory effort will help in both directions.

NOTES AND REFERENCES


2 Witt is referring to E. Waddell, 'How the Enga cope with frost: response to climatic perturbations in the Central Highlands of New Guinea,' Human Ecology, 3 (1975).

3 As put forward in numerous private discussions but nowhere fully stated in print.


IAN BURTON,
University of Toronto
ROBERT W. KATES,
Clark University
GILBERT F. WHITE,
University of Colorado