

## THE CRITIC'S CORNER

### Response to George Rogers' Review of *The Real Disaster is Above Ground*

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We are profoundly troubled by Professor Rogers' review of our book. While it is gratifying for us as authors to receive positive reviews and disappointing to receive negative ones, it is maddening to receive one that is basically inaccurate. This review falls into that category. Frankly, since the editors of IJMED were alerted to the factual errors and misrepresentations found in the review, we are very surprised that the review found its way to print.

The review opens: "In lieu of a central thesis, ...." Yet in paragraph 2, Rogers states, "...the central thesis of the book is that CTDs...give rise to social organizational decay...." First, we are criticized for not having a central thesis, and then for the one we have.

We are criticized in the first paragraph for promising a comparative case study; we never did. We tried to put the Centralia case into a broader perspective in the Preface, but never stated we were doing a comparative study. The Introduction makes clear that we are telling "the Centralia story," and that "the social reality we are setting out to explain is the destructive community conflict that characterized Centralia's response to a twenty-five-year-old mine fire." (p. 6) In fact, with this criticism, Rogers blatantly misrepresents the book, even to the point of changing the title to fit his misinterpretation: "But *Real Disasters* is about Centralia alone; the authors make no attempt to observe or analyze any other case." The book's title is *The Real Disaster* (singular), not *Disasters* (plural).

In paragraph 3, Rogers asserts that many basic questions about the Centralia case are not answered, when in fact they are. "The reader is left asking how the fire started? [this is answered on page 30 of the book] Was it really technological? [also see page 30, as well as the detailed discussion throughout Chapters 1 and 3] When did the fire start? [on page 3 as well as 30] How long did it burn before the community began to recognize the danger?" [page 3 and elsewhere throughout; see e.g., pages 32-33]

The inaccuracies continue in paragraph 4, where Rogers claims that we "assert a variety of interesting ideas in the final chapter; ideas that could

probably be supported by the data they collected, but are devoid of empirical backing as they are presented.... They could have shown how the frequency of conflict rose during the warning period [we did; that's what Chapter 3 is all about: "The shifting, elusive nature of the fire as a physical phenomenon bred misunderstanding among village residents," p. 42], or how initial conflict started over the threat cues...." [we did this too, defining threat belief systems on pages 59-60, and spending the rest of Chapter 4 documenting the rising conflict resulting from them.]

The remainder of the paragraph asserts that we present no evidence that the conflict "places the basic consensus of the society/community at risk" [the entire book documents this] and then asserts that not all conflict is destructive to communal associations [of course we agree, stating so explicitly on pages 169-170, where we analyze some differences between conflict in technological disasters compared with natural disasters: "It has been pointed out that conflict can facilitate the stable growth of a society.... In a chronic technological disaster, however, conflict typically emerges when competing interpretations of warning and threat cues have become institutionalized in concrete groups; this type of conflict places the basic consensus of a society in question...."]

The review's final paragraph returns to the errors of the first paragraph by criticizing the book for not doing what was never intended, but this time, it is for not explicating and analyzing the disaster typology that we merely suggest as informative in our Introduction.

We cannot understand why Professor Rogers would write such a review. A person reading it will have no idea of what the book is really about. There is no description of the town, the mine fire, the emergent groups, the conflict, etc. Rather, he has pieced together a series of incorrect and misleading statements, some of which even appear mean-spirited; to wit: "Most researchers cannot afford to spend (parts of) two years...collecting 34 one-to-three-hour interviews; this amounts to one interview every three weeks; and conduct a stress study on 35 residents...and then not analyze the data in the book about the research." Thorough fieldwork, of course, requires time. And, the data re analyzed, inductively.

Finally, we must admit to being upset with the editorial process at IJMED. We received a pre-publication copy of Rogers' review. We alerted the Book Review Editor to the factual errors in this review (including misrepresenting the very title of the book) and requested that it not be published. It was published anyway, and to whose benefit? Certainly not the authors of the book, the readers of the review, or the reviewer himself.

### Revisiting *The Real Disaster is Above Ground*

George O. Rogers

After re-reading *The Real Disaster is Above Ground* and the author's letter to the editor, nothing is presented to alter my initial review of the book. While the author's concerns are appreciated, the book remains interesting, enjoyable, and entertaining, but it does not contribute to scientific knowledge in the area of human response to acute natural or chronic technological disaster(s).

A fundamental disagreement involves the implications of the central thesis, that CTDs give rise to social organizational decay. The authors apparently are satisfied with focusing only on one cell of the four-fold typology of chronic and acute vs. technological and natural disasters. Only occasional, vague references are made to disasters presumably in the acute-natural disaster category. If they intend to make generalizations about the typology, the reviewer is not satisfied with focusing entirely on one cell. Focusing on CTDs without comparison to other cells in the typology can be only descriptive. The authors "...tried to put the book into a broader perspective in the preface, but never stated we were doing a comparative study." Yet comparisons are made throughout the text using phrases like "lasts long," "harder to detect," and there are specific references to natural disasters. In the conclusion, the authors (p. 163) continue to describe CTDs as differing considerably from acute natural disasters. If description is the authors' sole intent, they have succeeded, but the comparative phrases used throughout the book suggests a deeper motive. They are caught in a classic "case-study dilemma"; they want to make conclusions about the nature of the typology, but they only have data about one case in one cell. The often vague comparisons leave the reader uncertain as to whether the differences eluded to are the result of being chronic, or the result of being technological. Or must a CTD be both technological and chronic to elicit the described patterns of behavior?

*The International Journal of Mass Emergencies and Disasters* is a scientific journal, hence any book reviewed must be evaluated accordingly. To contribute scientifically *The Real Disaster is Above Ground* not only would describe the events in Centralia, any journalist can do that, but it must describe accurately and with enough precision to be replicated. Moreover, the study would contribute scientifically if it explained why these events

took place. This explanation should also be in terms that can be replicated by other researchers. Moreover, replication involves testing or observing the processes in similar and dissimilar situations so that an understanding is developed about when, where, and under what circumstances similar outcomes can be expected. It is in this sense that *The Real Disaster is Above Ground* fails to provide the necessary detail for scientific replication.

While CTDs are described throughout, no definition is offered to allow researchers to replicate the study. For example, one characteristic of a CTD is that it "...lasts longer and is considerably harder to detect" (p. 9). Presumably this is making a comparison to natural disasters, but it could be a comparison to acute disasters; the reader cannot be sure since no comparative clause is offered, only the words "longer" and "harder." But no indication is given of how much longer it lasts, or how much easier to detect it must be to no longer be a CTD. As pointed out in the review: how long must a disaster last to become chronic? Presumably it becomes chronic before it is 20 years old, but is it chronic if it lasts 12 months? In order for other scientists to evaluate the contribution, via replication, the reader must know when results can be expected to apply. The authors' descriptions do not meet this criterion. Because no conceptual or operational definitions are offered for the key elements of a CTD, the reader is left guessing about important details of the typology.

As to my "factual inaccuracies." The authors say that they answer the question how the fire started (para 4, sentence 2). But what they say is:

When refuse was discovered burning in an illegal garbage dump southwest of the borough limits, near the Odd Fellows Cemetery, in May 1962, borough workmen flooded the fire with water and installed a clay seal to contain it. (p. 30)

The reader is still left with the question of *how* the fire started. For example, was it lightning, arson, or spontaneous combustion? How is the initiating event related to the manifestation of the social behavior exhibited? If the initiating event remains unknown, did that uncertainty play a role in the observations made by the authors?

*The Real Disaster is Above Ground* mentions coal in the next sentence. "...by July the fire had spread to a nearby outcrop of coal in the Buck Mountain vein" (p. 30), but still does not mention a coal mine fire. Is not an "outcrop of coal" a naturally occurring geologic phenomena? How is a garbage fire, that happens to reach a naturally occurring outcrop of coal, technological? While it is clear that the authors believe the mine fire in Centralia was technological, and in fact the reviewer cannot disagree, it is less clear *what makes* the mine fire in Centralia technological. This is a

classic dilemma of making the distinction between technological and natural hazards. If one considers the mine fire technological simply because it occurred in a mine, which is by nature technological, then nearly every disaster by virtue of its impact on human technologies is a technological disaster.

While it is clear that *The Real Disaster is Above Ground* suggests that the mine fire started in 1962, what remains unclear is when the community began to recognize the danger. Can the reader consider the first efforts by borough workmen in May 1962 as recognition of danger, or did it happen some time later, when it became clear that it was not controlled or controllable? How did the recognition of danger spread? How is the spread of the awareness of the dangers related to the social conditions described? These are fundamental questions that remain at least partially unanswered in the book.

As to my "mean-spiritedness," the authors devote considerable effort to describing the various methodological approaches used (e.g., Chapter 1). This leads the reader to expect a certain amount of integration of the various approaches. But the reader is disappointed. The only data discussed in substantive terms are qualitative. Even though data from a survey of more than half the population of Centralia were collected (p. 7-8), they are not analyzed in the book. This is not only disappointing because it fails to integrate qualitative and quantitative approaches, it also could be misleading. The quantitative data are the only data that represent all residents. The qualitative data are more focused on those residents who were not only directly impacted, but may also have been pro-active in the social processes. Why would the authors choose to omit discussion of data so clearly relevant? The reader can only speculate.

It is clear that the authors and the reviewer share a fundamental disagreement about the contribution of *The Real Disaster is Above Ground*. That this disagreement is brought to light and discussed in *IJMED* is fitting, proper, and good science. But to charge that the editorial process at *IJMED* has failed is simply not the case. If anything, the editors of *IJMED* have demonstrated the highest standards in professional integrity, and for this they deserve our highest praise. Lest there be any confusion, the reviewer sent a pre-publication copy of the review to the authors at their request. The editors responded to the author's concerns by allowing either the review to be withdrawn by the reviewer, or the opportunity for this exchange and clarification. Had the editors taken any other course of action they would have sent the subtle but effective message to future reviewers that only positive reviews are acceptable, and conversely to authors that objections

to reviews can keep them out of print. In essence, they would have circumvented the scientific process of review, challenge, clarification, and response.