

DETECTING PSYCHOPATHOLOGICAL REACTIONS TO NATURAL DISASTER: A METHODOLOGICAL NOTE

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Two opposing views in a controversy were examined in this paper. In the psychiatric literature it is argued that natural disasters can cause severe negative psychological reactions in victims. However, in literature generated by sociologists and psychologists, it is purported that natural disasters do not produce adverse reactions. It is argued that the apparent empirical stalemate is probably a function of methodological difficulties, and the contradictory findings are explained on the basis of different measurement strategies and problems of study design.

Keywords: natural disasters, psychopathological predictions, detection.

In recent years there has been considerable debate among social scientists regarding the potential for negative psychological consequences for victims of natural disasters. One position, developed primarily by psychiatrists, argues that catastrophic life events produce adverse psychological reactions which are present both immediately post impact and throughout the long range – persisting possibly for several years or perhaps for the rest of the victim's life (Lifton & Olson, 1976; Moore, 1958; Tichner & Kapp, 1976; Tyhurst, 1957). The competing position holds that, while essentially transient disorders may surface in the short run, longer-run negative reactions are infrequent and probably occur as a function of a variety of "causes", among which disaster impact is only one. The idea that disasters produce few negative psychological consequences, in either the short or the long run, is also supported by empirical data (Fritz, 1961; Quarantelli & Dynes, 1972; Sterling et al., 1977; Taylor, 1977).

It is important to understand that each of the two opposing positions is biased in research; the controversy rests upon the apparently contradictory results of empirical studies. In such cases it is doubtful that further research will contribute to a resolution of the controversy. In the present paper the apparent empirical stalemate is methodologically reviewed and two major pitfalls which begin to account for the "contradictory" research evidence are isolated. The two methodological problems deal with (1) the structure of research designs and (2) measurement of the dependent variable.

In studies of the relationship between psychopathology and disaster, most investigators have sought to determine "whether any cases of mental illness can be found after a major disaster..." (Taylor, 1977, p. 7). Hence, study designs tend to be surveys of a population conducted post impact. The difficulties with this approach are both conceptual and methodological. As stated above, the research task appears to be one of examining individuals post disaster and either finding or not finding mental

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disorder. In fact, the issue of interest should center upon finding disorders which can be linked with disaster impact. Two pairs of concepts from the epidemiology literature are helpful in understanding this problem: prevalence vs. incidence and reactive vs. process mental disorder.

Prevalence refers to the amount of mental disorder (treated and untreated) existing in a specified population over some defined time period – sometimes called period prevalence (Susser, 1968, pp. 205-15). Epidemiological studies have documented prevalence of psychological disorder in communities ranging from low figures of 1.7% in some rural areas (Eaton & Weil, 1955) to 23.4% in the Midtown Manhattan study (Srole, 1962) and more than 50% in the Stirling County study (Leighton, 1963). Incidence describes the number of new cases of mental disorder which arise in a specified population over a specified time period (Dobrenwend & Dohrenwend, 1972, p. 283). Figure 1 shows the relationship of incidence to prevalence; the ordinate represents the level of mental disorder and the abscissa represents time with three points indicated. If disaster impact occurs at T_2 , then it is clear that simply locating cases of mental disorder during the post-impact period does not answer questions about the psychological consequences of disaster. Some cases of disorder would be expected due to prevalence and incidence even in the absence of disaster. Therefore, the appropriate research task is to determine what fraction (if any) of the incidence for period T_2 - T_3 can be traced or directly attributed to the impact of the natural disaster. Accurate assessment of the proportion of incidence attributable to disaster impact is further complicated by the necessity of considering the etiology of psychological disorders.

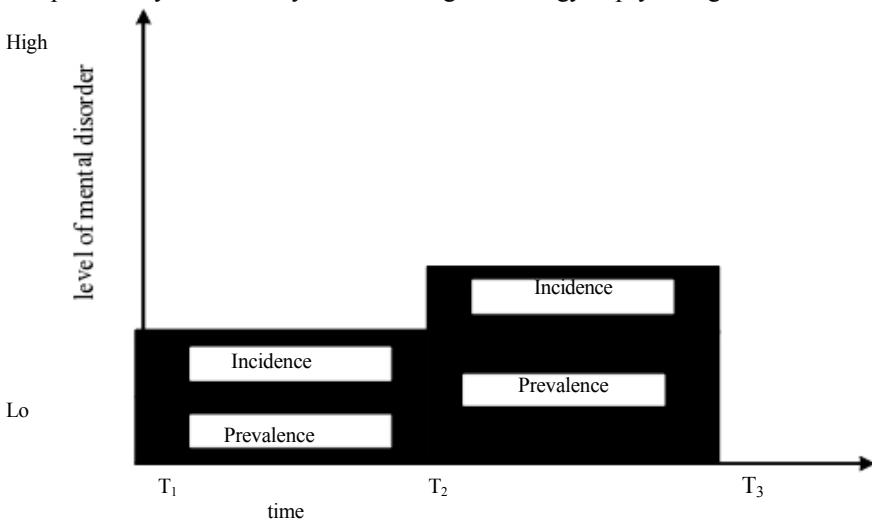


Figure 1. Graph of the relationship between period incidence and period prevalence of mental disorder. (Prevalence for the time period T_2 - T_3 equals prevalence plus incidence for the previous time period, minus remissions.)

When speaking of psychopathology, epidemiologists distinguish between reactive versus process onset. Reactive onset describes the situation where symptoms appear

quickly with little or no warning, while process onset involves a slower process taking months or years over which symptoms gradually increase (Herron, 1962; White, 1964; Zigler & Phillips, 1962). The issue raised by this distinction relates to the nature of the causal relationship between disaster impact and psychological consequences. In the case of reactive onset the question of causality may be seen as reasonably straightforward. It is at least theoretically possible to determine whether or not post-impact psychological symptoms are indeed a function of disaster impact. Hence, methodologically, one is checking to see if disaster is a necessary and/or sufficient cause of psychological disorder (cf. Seltiz et al., 1959, p. 81).

The concept of process onset introduces the possibility of a more complex relationship between disaster and psychological disorder. If onset occurs over time, one must sort out a variety of possible "causes" and determine the relationship of disaster impact to these other factors as well as to any observed psychological consequences. This additional consideration suggests that disaster impact is better thought of as a contributory cause (Seltiz et al., 1959, p. 82) which increases the likelihood of psychological disorder, but does not make it a certainty. Although more demanding in terms of theory and methodologically more complex, the conception of disaster impact as a contributory cause is at least consonant with the findings of studies purportedly supporting both sides of the controversy. Hudgens (1974, p. 120) summarizes this argument succinctly:

"...as I interpret the results of recent studies...investigators have demonstrated a causal connection between stressful life events and subsequent worsening of conditions already under way, between life events and subsequent admission to psychiatric hospitals and clinics and between bereavement and depressions...It does not seem to me that investigators have yet convincingly demonstrated that life stress can cause madness in a person previously of sound mind."

The preceding discussion begins to account for the possibility of contradictory findings in terms of the "after-measure only" type research design which is commonly used in studies of disaster psychopathology. It has been argued that theoretically, at any given time, there exists some level of mental illness in any given population. Thus, an after-only design, which does not account for the base rate of mental illness, is generally inappropriate for answering questions about the relationship between natural disaster and victim psychopathology. Furthermore, it has been argued that this essentially post hoc methodology has contributed to the perpetuation of an oversimplified view of the problem; namely, that disaster is a direct and unconditional cause of mental illness (cf. Lifton & Olson, 1976). Another perspective, which appears to appropriately address the problem, sees disaster as a contributory cause; as one of a number of factors which together determine psychological consequences.

A second methodological difficulty which has contributed to the apparent controversy relates to identifying the phenomena to be studied; namely, determining how "psychological consequences" are operationalized. Most of the studies handle the problem of measuring the dependent variable in one of two ways: either a definition is left implicit and the phrase "mental illness" used to cover the widest range of meanings, or among empirical studies some operational definition of "psychological consequences"

is equated with an observable measure – e.g., victim self-report, interviewer diagnosis, admission to care. This is not to suggest that one should undertake the herculean, and probably impossible, task of “rectifying” numerous, disparate theories of psychopathology and create a single definition of mental illness. It is essential, however, when evaluating studies of the psychological consequences of natural disaster, to carefully attend to specific definitions and operationalizations, as well as the underlying conceptions of psychopathology.

Many conflicting claims regarding the relationship of natural disaster and mental illness may be resolved by acknowledging that researchers have used different conceptions of psychopathology in designing studies and consequently have chosen different measurement strategies which tend to yield incomparable results. Those who have found a correlation between disaster and mental illness have tended to employ a psychodynamic perspective, which directs concern with anxiety states, subjective unhappiness, and other maladjustment evidenced through psychiatric diagnosis (Scott, 1958, p. 29). These studies also depend largely upon victim self-report and clinical interviews as measures of psychological consequences. On the other hand, studies which have not found a strong relationship between disaster and mental illness tend to use a behavioral model of psychopathology sensitive to individuals’ maladaptive behaviors, usually measured by rating scales, observers’ reports or admissions to psychiatric care (Ulman & Krasner, 1970, p. 327).

One should be aware that the abovementioned “measures” are sensitive to different aspects of human behavior and constitute different criteria for establishing the presence of psychological consequences. Thus, it is not appropriate to assume that they are acceptable equivalent indicators of mental disorder. Instead of interpreting the empirical findings as conflicting, then, one should conclude that the different measures employed offer evidence that some types of psychological reaction are common following natural disaster and other types are not. Hence, we have much evidence that people usually answer in the affirmative when consequences are “defined by the victims themselves in response to the question, ‘Have you noticed any emotional stress among family members as a result of the disaster?’” (Moore & Friedsam, 1959, p. 136). When the psychological consequence of interest is admission to psychiatric care, however, it is evident that this reaction is rare and that few victims of natural disaster seek such institutional help (Bennet, 1970, p. 456; Bates et al., 1963). Thus, careful review of the measurement strategies of existing empirical studies directs attention away from the problem of apparently conflicting findings and toward the more productive task of cataloguing the nature and frequency of different types of psychological consequences associated with natural disaster. Finally, by focusing upon the measurement of observable psychological consequences, there is the added advantage of avoiding the conceptual and definitional stalemate which arises when one attempts to derive an acceptable operationalization of “mental illness” (Bandura, 1969, pp. 9-19).

In summary, a careful review of methodological principles provides a reasonable framework for beginning to sort out the apparently contradictory studies of the psychological consequences of natural disasters. In effect, once one accounts for problems of research designs and different operationalizations of “mental illness”,

the conflicting nature of the research findings begins to disappear. Two general conclusions may be drawn regarding the controversy: (1) the frequently used "after-only" survey design reflects inappropriate conception of what the research question should be; and (2) there are probably some short-term psychological consequences of natural disasters, but research designs have been inadequate to effectively determine whether the consequences are wholly positive or negative, or whether they persist into the long run. To correct such difficulties and obtain answers to questions about the nature and persistence of psychological consequences requires the development of study designs which account for base-rates of psychological disorders and theoretical frameworks which provide explicit logics for developing comparable operational definitions.

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