

Commentary

9/11: Mental Health in the Wake of Terrorist Attacks

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The targets of the 9/11 attacks were chosen for their symbolic importance. Political leaders and experts on terrorism characterized the attacks as acts of psychological warfare. Comparisons were made between 9/11 and the surprise attack by Japan on Pearl Harbor. In one sense, the 9/11 attacks were more insidious and potentially more dangerous than Pearl Harbor. In the aftermath of 9/11, a threat hung "like the cloud of smoke over Ground Zero and parts of Manhattan, [it] remained "in the air," never truly disappearing, never giving a concrete target for protective action. . . . Unlike the bloodiest air raids in war, there was no trusted safety signal . . . and no safe places. . . . [T]he boundaries between direct and indirect exposure were blurred. . . a matter of degree. . ." (Shalev, 2006, pp 607–608).

9/11: Mental Health in the Wake of Terrorist Attacks is a compendium of what happened next: how researchers investigated the psychiatric effects of the attacks and what they believe they have discovered. The 9/11 attacks included the World Trade Center (WTC) towers in New York City, the Pentagon, and an airplane that crashed in Pennsylvania. It was specifically the televised images of the WTC attack that riveted worldwide attention and eroded the boundaries between direct and indirect exposure.

The most severely affected victims were the close relatives and comrades of people killed in the attacks. The Mayor of New York City and his administration offered these people psychiatric care and counseling and took steps to protect them from inquisitive outsiders including trauma researchers. Researchers, unhappy with this decision, took the position that 9/11 was a collective trauma, affecting all Americans, and had created a public health emergency comparable to an epidemic of infectious disease. It would be unfair to deny the WTC victims the op-

portunity to contribute to the collective good.

Four years after the attack, a leading PTSD researcher appealed to Washington to end the "moratorium on research." "The establishment of the Department of Homeland Security implies a preparedness to prevent and respond to terrorism that should be extended to mental health research. . . . [It] is imperative to develop a culture of education in which the academic community can freely communicate what is and is not known, such that survivors of terrorism will understand the value of their participation in research to the generation of useful knowledge" (Yehuda et al., 2005).

There was no moratorium on research. The ban affected mainly researchers accustomed to working with the direct victims of traumatic violence. There were other researchers, many of them relative newcomers to the trauma field, who grasped the novelty of the WTC attack: It was an unprecedented combination of terrorism and television. The target of their research would be the victims of "distant traumatic effects" transmitted by television. There were potentially millions of victims, living in every part of the United States, and there were no obstacles—moral, political, or technological—to interfere with research.

TV images are not included in DSM-IV's list of PTSD traumatic events, and the text does not mention distant traumatic effects. Nevertheless, DSM-IV redefined the stressor criteria in a way that opened a space for distant traumatic effects. The trauma victim is described as someone who experienced, witnessed, or was confronted with an event or events that involved actual or threatened death or serious injury, or a threat to the physical integrity of self or others. Distant traumatic effects represent a distinctive kind of traumatogenic "witnessing."

The term originates in research on children traumatized by TV coverage of the Challenger space shuttle disaster (Terr et al., 1999). A closer analogy with the 9/11 victims is provided in 3 brief accounts published in 1994 in the *British Medical Journal*. The accounts describe what might be called an outbreak

of traumatic symptoms among children who had viewed a television program called "Ghostwatch" (Baille et al., 1994; Forbes and McClure, 1994; Simons and Silvera, 1994). They lived in parts of England and Scotland and were unknown to each other. Some of them had recently experienced a significant life event (parents' divorce); others were described by their parents as characteristically anxious.

The outbreak involved a handful of children who had been taken to their family doctors. If epidemiologists had been motivated to locate a larger sample of *Ghostwatch* viewers, they would doubtless have found many entirely unimpaired children reporting disturbing dreams and avoidance behavior with "Ghostwatch" themes. Could the larger group be described as an "epidemic"? In the editorial coda to *9/11: Mental Health in the Wake of Terrorist Attacks*, Randall Marshall cites the definition of epidemic in the *Oxford English Dictionary*: an event "prevalent among a people or a community at a special time, and produced by some special causes not generally present in the affected locality." If we adopt the *OED* definition, then the "Ghostwatch" scenario can be described as an epidemic. But it would be an odd epidemic, since the trauma symptoms would be generally indistinguishable from normal reactions, the victims would be unimpaired, and the "special causes" would include the researchers who designed the research program.

Post 9/11 trauma research delivers a similar product, a "virtual" epidemic. PTSD is defined by 4 core features: a precipitating event plus symptoms that can be interpreted as re-experiences, avoidance and numbing, and autonomic arousal. Regarded individually, symptoms are nonspecific and easy to confuse with depression and other disorders. Some symptoms, such as "difficulty falling asleep," are clinically unexceptional states. The symptoms become traumatic when it is determined that they are part of this traumatic process: a stressful event (criterion A) creates a traumatic memory (criterion B) that creates and reproduces a pattern of adaptive behavior (criterion C) and physiological re-

sponses (criterion D). All phases must be represented to justify a PTSD diagnosis. Otherwise, individual symptoms remain nonspecific and easily confused with other disorders.

Many of the “trauma” symptoms reported in 9/11 epidemiological research do not meet this standard. Some people reported just 1 or 2 “traumatic” symptoms. Yet these individuals are routinely included in tallies. This practice is also mentioned and criticized in *9/11: Mental Health in the Wake of Terrorist Attacks*, in chapters by Naomi Breslau, Richard McNally, Simon Wessely, and Ariel Shalev. Breslau and McNally believe there was no epidemic, notwithstanding the claim (made by other contributors) that 6 months after 9/11, 7 million Americans living around the country (but excluding New York City) had “probable PTSD” triggered by the attacks. Randall Marshall is provoked: “Where does one begin to respond to Breslau and McNally’s assertion that there ‘was no mental health epidemic after 9/11’?” They are like people who believe that the NASA moonwalk was a hoax, filmed on earth. Their failing is not only intellectual (they dismiss the scientific evidence), it is also moral:

It is unfortunate, but this chapter abandons the basic principle that mental health scientists should concern themselves with recognizing and responding to public health needs. The ethical consequences of minimization or outright denial of human suffering after large-scale traumatic events are profound. [It] was perhaps inevitable that an event with profound political consequences from the start would become politicized (Marshall, 2006, pp 626–627).

Is this true? Most scientists would argue that the most basic principle in assessing scientific research is epistemological. Are “trauma symptoms” and “large-scale traumatic events” somehow special? Are they immune from scrutiny and, in this regard, unlike other psychiatric notions? Marshall is not alone in wanting to emphasize the cultural (moral) dimension of trauma and PTSD. This is a common attitude and is probably shared by many ordinary Americans. And if this is so, then this attitude needs to be considered when researchers interpret responses to questionnaires

about 9/11. Twenty years ago, posttraumatic stress was largely the preserve of psychiatry. Today the language of trauma permeates everyday discourse, television and radio talk, print journalism, popular fiction, etc. The language of posttraumatic stress is the Esperanto of global suffering and the first nondenominational medium through which well-meaning people can and should express compassion and affirm their shared humanity with all classes and cultures.

Post-9/11 researchers asked respondents for many kinds of personal information that might help explain their self-reported symptoms. No one was asked about prior knowledge of PTSD. Would adult respondents with a basic understanding of the PTSD etiology be inclined to situate the interviews within this context—especially when they were asked explicitly whether their self-reported symptoms are related to the WTC attacks (e.g., Galea et al., 2002)? PTSD is not only a psychiatric classification; it is also a cultural template and shapes expectations about how normal people do (should) respond to events like the 9/11 attacks. Did this knowledge affect respondents’ answers and, in this way, contribute to making an association between TV viewing and trauma symptoms? Donald Rumsfeld made a useful distinction between the “things that we don’t know” and the “things that we don’t know that we don’t know.” Apparently cultural and moral meaning of PTSD and trauma is something that the 9/11 researchers don’t know that they don’t know.

Marshall asks, in exasperation, where does one begin when contributors ask questions about the plausibility of 9/11 evidence? He might begin by considering an historical precedent. A million tons of explosive and incendiary bombs dropped on German cities during WWII. Survivors witnessed the gruesome deaths of relatives and neighbors (over 500,000 civilians died). In 1947, Kurt Beringer, a former colleague of Karl Jaspers at the University of Heidelberg, published an account of his experiences treating psychological casualties during and after the war. His report includes information he collected from psychiatrists treating survivors elsewhere in Germany. “Abnormal reac-

tions” were rarely observed, he wrote, and survivors “who had neurotic symptoms were able to . . . carry on with their usual activities” (Beringer, 1947).

Post-9/11 researchers report that repeated exposure to 9/11 images increased the probability of PTSD. Risk tripled in people exposed to 2 early events; 3 events meant a 6-fold increase (Galea et al., 2006, p 33). Most German survivors had been exposed to multiple air raids. Each episode was met the DSM criterion for a “traumatic stressor.” No comparable epidemic was reported in Germany. Why? It seems unlikely that massive numbers of civilian casualties would have slipped by entirely unnoticed. German doctors were familiar with posttraumatic disorders, and many psychiatrists, including Beringer, had treated cases of traumatic neurosis during WWI. There is no convincing evidence that clinical and anecdotal evidence was suppressed after the war. It is true that German psychiatrists were in short supply during this period, and the resources and motives needed for studying reactive disorders in the general population were lacking. But these deficiencies cannot explain this disparity between post-9/11 and post-WWII Germany.

The puzzle is not solved by counting traumatic outcomes and comparing the totals. The problem is conceptual not methodological. The key question is: What counts as an “outcome”? What kind of behavior will count as a “symptom” or a “posttraumatic reaction”? The questions are relevant to both situations: Why were there few posttraumatic cases in Germany despite repeated exposure to terrible events? Why were so many posttraumatic cases recorded in the United States after low-intensity exposure to televised events?

Richard McNally believes that the post-9/11 epidemiologists have contributed to “conceptual bracket creep” (McNally, 2003, p 232). They “medicalize expectable human reactions by failing to discriminate between genuine symptoms of disorder and normal distress reaction.” McNally wants a more rigorous stressor criterion, similar to the original definition in DSM-III and DSM-III-R. It won’t happen. The posttraumatic

syndromes are intrinsically historical phenomena, and they are historical in a way that is different from other psychiatric disorders. The syndromes have been continually redesigned to meet transient social and psychological needs since the late 19th century. The major transformations have come during or following certain times of great historical violence: World War I, the Holocaust, and the Vietnam War. The War on Terror provides the raw material for another chapter in this history. The mass production of PTSD of the virtual kind is something new, but it is not an aberration. It is a metamorphosis.

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