The otherwise excellent review of epidemic hysteria by Boss (1), which was published in Epidemiologic Reviews, is flawed by the author’s decision to organize his data by using Francois Sirois’ influential paradigm for diagnosing epidemic hysteria (2). It is also misleading because it omits recent criticisms of this schema that fails to distinguish between epidemic hysteria and collective delusions—a limitation that Sirois himself acknowledges (3). These are two separate problems; the former represents the pathologic spread of conversion symptoms, and the latter is typified as the nonpathologic manifestation of conformity dynamics and reality testing exacerbated by human perceptual fallibility. For clarity, it should be emphasized that psychiatrists and medical practitioners typically use the word “delusion” to describe a persistent pathologic belief associated with serious mental disturbance, usually psychosis. I use the term “collective” or “mass delusion” as typified within the standard sociologic and social psychological nomenclature to describe the spontaneous, temporary spread of false beliefs in a given population (4).

Sirois (2, 5) has developed a five-part classification taxonomy of epidemic hysteria ranging from the “explosive type,” almost exclusively confined to small, cohesive, institutionalized social networks, to “diffuse outbreaks” in community or regional settings, whereby a particular cohort is overwhelmed by the rapid spread of false rumors and beliefs. However, one category in his schema, diffuse outbreaks, has been challenged (6–8). In a dialogue I had with the author (9), Sirois acknowledged the limitations of this category, conceding that “I agree here with Bartholomew that these community outbreaks are best studied by sociology and bear only a loose linkage with epidemic hysteria, as they are more often examples of group anxiety” (3, p. 47).

In following the original Sirois paradigm (2), Boss (1) erroneously includes under the epidemic hysteria rubric two diffuse outbreaks affecting the population at large: the Martian panic (10) and the Seattle, Washington, windshield pitting epidemic (11). While these incidents were characterized by widespread anxiety and are often described as “mass hysteria,” no one reported illness symptoms or conversion reactions.

On the evening of October 30, 1938, many people in the United States became anxious or panicked after listening to a realistic radio broadcast depicting a fictitious Martian landing in New Jersey (10). While there was an absence of illness symptoms, the reactions by some residents were widely attributed at the time to hysteria-induced irrationality or “mass hallucination.” This assessment was further reinforced by reports that in addition to panic, during the broadcast some New Jersey residents reported to police that they observed “Martians on their giant machines poised on the Jersey Palisades” (12, p. 379), while others claimed to have heard the machine guns or fires raging in the distance (13) or to feel the heat rays as described on the radio (14). But such behaviors are explainable without recourse to hysteria, irrationality, or pathology. Human perception is unreliable (15–18), and an effect can be exacerbated by stress, ambiguity, and uncertainty (19–22). A person’s frame of reference also has a strong influence on how external stimuli are interpreted and internalized (23).

Certainly the Martian panic (10) and the Seattle pitting episode (11) were uncommon events triggered by anxiety fostered by adherence to a false belief and exacerbated by the mass media. However, while mass anxiety is a prerequisite for epidemic hysteria, group anxiety, in conjunction with the rapid spread of a seemingly irrational belief, should not be the basis for diagnosing epidemic hysteria, since no illness symptoms were reported. Rationality is determined by social and cultural circumstances and is not a constant from which unproblematic assessments can be made of a particular behavior from outside of any given period, culture, subculture, or social group.

Professional medical ideology is typified as an unproblematic value-neutral enterprise that cuts across social and cultural strata in its dispassionate application of universal scientific principles to explain, diagnose, and treat disease and disorder. However, researchers must be cautious in their construction of classificatory schemes, as taxonomies do not exist as objective elements in nature awaiting description. The fundamental impediment to better understanding epidemic hysteria is the inability to formulate a unitary interdisciplinary definition. For instance, some investigators exclude from their definition the rapid spread of benign, transient illness signs and symptoms that are precipitated within an institutionalized, organized, or ritualized religious setting, since they were not spontaneous but induced by artificial means (24). Distinguishing between epidemic hysteria and collective delusion is a beginning. The present lack of agreement is a challenge to the medical and psychiatric communities.

REFERENCES

5. Sirois F. Perspectives on epidemic hysteria. In: Colligan M,

206
After close review, I must respond to the interchange regarding the article by Hagan et al. (1) on needle exchange and hepatitis in Seattle, Washington. They found significant increases in the risk of both hepatitis B and hepatitis C in the participants in the Seattle needle exchange. In their invited commentary, Moss and Hahn (2) went through great rigors to rationalize the findings of Hagan et al. and then attacked the Director of National Drug Control Policy, General Barry McCaffrey, for what they characterized as "language reminiscent of the McCarthy era." Is it not significant that Bruneau et al. (3) and Straathede et al. (4) found failure of needle exchange programs and increases in human immunodeficiency virus conversion despite the needle exchanges in Montreal and Vancouver, Canada?

Perhaps the answer really does lie in the unthinkable possibility that needle exchanges just do not work! Why does the public health community seem to have such a difficult time rejecting needle handouts while continuing to claim that they should be the "cornerstone" of public health programming? After all, needle exchange programs do nothing to change the underlying destructive activity of intravenous drug use. No exchanges have demonstrated clear advantages over aggressive outreach and treatment. Would we adhere to programming that increases the incidence of tuberculosis or venereal disease?

The needle exchange community should take heed from Hagan et al.'s closing thoughts: "Drug treatment programs that lead to cessation or reduction in drug injection may lower the risk of both [hepatitis C virus] and [hepatitis B virus] in current drug injectors" (1, p. 217).

REFERENCES


Robert E. Bartholomew
Department of Psychology and Sociology
James Cook University of North Queensland
Townsville 4811
Queensland, Australia

FIVE OF THE AUTHORS REPLY

In Dr. Voth’s letter (1) responding to our article (2), he seems to have missed the point of our remarks regarding drug treatment as a hepatitis C virus (HCV) and hepatitis B virus (HBV) prevention strategy. Drug treatment for addictions remains an extremely limited resource in this country, with demand for treatment by drug users consistently exceeding supply (3). Furthermore, drug treatment tends to attract primarily older injection drug users, most of whom have already been infected with HBV and HCV by the time they enter a treatment program. In our study, which focused on those who continued to inject during the period when they could have acquired infection, there was no effect of drug treatment on HBV or HCV incidence. This is consistent with another study that could not find a protective effect of methadone treatment against HCV infection (4).


Eric A. Voth
The International Drug Strategy Institute
Topeka, KS 66606

Letters to the Editor 207

RE: “SYRINGE EXCHANGE AND RISK OF INFECTION WITH HEPATITIS B AND C VIRUSES”