of this research, the authors' conclusions are totally illogical and invalid.

In this article, the authors conclude that Chronic Fatigue Syndrome (CFS), as defined by the Centers for Disease Control (CDC) Diagnostic Criteria, might be "quite rare" in the general population, as only 1 of 13,538 individuals studied was deemed to have CFS. The official CDC Diagnostic Criteria, however, were not utilized to diagnose cases of CFS. Instead, the researchers reviewed interview questionnaire data collected between 1981 and 1984 for a purpose unrelated to diagnosing CFS. In fact, the CDC Diagnostic Criteria were not formulated and published until 1988.

The data the authors reviewed were collected as part of the Epidemiologic Catchment Area (ECA) Program. The ECA study, however, was implemented for the clinical reappraisal of the Diagnostic Interview Schedule (DIS), a test developed to assess psychiatric morbidity. Another purpose of the ECA study was the estimation of the prevalence of psychiatric disorders.

The diagnosis of Chronic Fatigue Syndrome, according to the CDC Diagnostic Criteria, requires a comprehensive history, physical examination and laboratory workup. Price, et al., relied solely on symptom reports to diagnose CFS and did not conduct any physical examinations or laboratory studies.

Additionally, the questions utilized in the DIS to diagnose CFS only partially resemble some of the symptoms and signs cited in the CDC Diagnostic Criteria. Several important symptoms and signs cited in the CDC Diagnostic Criteria were not even included in the DIS. Utilizing the DIS to estimate the prevalence of Chronic Fatigue Syndrome is as inappropriate as relying solely on symptoms reported during the DIS interview to estimate the prevalence of peptic ulcer or coronary disease, with no physical examination or laboratory assessment.

On August 25, 1992, a letter by Ned Curran, Associate Editor of *Public Health Reports*, was released to the press. This letter announced that Price, et al. found only one case in over 13,000 that "fit the technical description on the syndrome promulgated by the Centers for Disease Control." Since the CDC Diagnostic Criteria were not utilized in the collection of the data, such a statement grossly misrepresents the findings of the study by Price, et al. Additionally, Curran set forth the notion that Chronic Fatigue Syndrome constitutes a "chimeric ailment that hyperkinetic go-getters thought they were heir to" and presented it as though it were part of the research conclusions of Price, et al.

The quality of the research, selection for publication, and manner of notifying the press of the study are far below the standards we would have expected from a journal such as *Public Health Reports*. This research was funded in part by grants from the National Institute of Mental Health and the National Institutes of Health. We would very much hope that in the future our taxpayer dollars will be put to better use.

Ruth Robin, MS, President, David M. Lipkin, MD, Vice President, Gordon W. Hume, MA, Treasurer, Chronic Fatigue Syndrome Society of Illinois, Inc., Chicago.

(Editor's Note: The letter to the press referred to was, in fact, a covering note to members of the media that accompanied copies of the actual article in question. It was designed to pique their interest and draw their attention to the article itself. To achieve that purpose, it was deliberately cast in hyperbole, although it was based on the authors' own synopsis. It was never meant to be a news release as such, standing on its own. The assumption was that news people would read the actual article and make their own interpretations—as they did. To the extent that the note is regarded as insensitive, we apologize. That was not intended.)

Price, et al., Respond

In reply to the letter from Robin, Lipkin, and Hume on "Estimating the Prevalence of Chronic Fatigue Syndrome and Associated Symptoms in the Community," we had addressed in our paper several methodological limitations they correctly identified (*1*). We acknowledged that the criteria of chronic fatigue syndrome (CFS) in our analysis were not identical to the Centers for Disease Control (CDC) criteria because we lacked information on physical and laboratory findings (*1a*); Epidemiologic Catchment Area (ECA) data collection preceded the 1988 CDC criteria (*1b,1c*); and the Diagnostic Interview Schedule (DIS) was not designed to study CFS (*1d*).

We underscored these limitations, and indeed stressed that "the findings of this study need to be verified by future studies using full CDC criteria, including clinical assessment... Such studies...would provide a more precise prevalence estimate of CFS" (*1c*).

Other points raised by Robin et al. need further clarification. The main purpose of the ECA was not "clinical reappraisal of the DIS" (*2*). The DIS questions available in the ECA data do resemble symptom descriptions in the CDC criteria (*1e*), though the battery of these questions was incomplete. Other authors have also successfully used DIS questions to study nonpsychiatric syndromes, including fibromyalgia (*1d*).

Robin et al. stated that we relied solely on patient reporting because of the absence of laboratory work-up. If the ECA study had contained laboratory information, our prevalence estimate of CFS could have been even lower, since potential CFS cases could have been suffering from physical illnesses detectable by laboratory tests. It is also worth pointing out that laboratory evaluation has little utility in the diagnostic process of chronic fatigue syndrome (*3,4*).

The comparison by Robin et al. of CFS to peptic ulcer and coronary artery disease actually speaks to a different point. Peptic ulcer and coronary artery disease can be objectively diagnosed by endoscopic or radiologic proce-

dures. Since there is no diagnostic test for CFS, self-reporting by patients remains an essential component to diagnose CFS (*5*).

In spite of the limitations we and Robin et al. addressed, our study is worthy of the attention of the scientific community, because no data on the prevalence of CFS in the general population in the United States are yet available (*1f*). It is reasonable to start with available data (albeit imperfect) and then move toward a more sophisticated, expensive, and laborious approach to the question of prevalence.

It is important to reiterate another thrust of our paper. We did not show that CFS is exceedingly rare. What we have shown is that the 1988 CDC criteria for CFS would have identified a very small group of patients. Others have found similar problems with the 1988 CDC definition of CFS (*6,7*). A prevalence estimate of a disease can only be as accurate as the diagnostic criteria used to identify patients with the disease. In fact, the case definitions of CFS have just been revised in response to a number of criticisms (*5*). We are anxious to see if our prevalence estimate would increase using the new revised exclusion criteria.

We thank Robin et al. for bringing the matter of the press release to public attention. We had no knowledge of the press release until we were informed by media reporters. This was tactless. The press release inaccurately described our CFS inclusion criteria as if they were exactly identical to the CDC criteria, despite our emphasis that our estimate was based on an approximation of the CDC criteria. The release also lacked sensitivity to individuals suffering from CFS. We certainly did not show that CFS is a "chimeric ailment". Indeed one of us (SW) has forcefully argued exactly the opposite in various previous publications. We recommend that in the future, authors of articles should be given the opportunity to preview the press release content, as is done by many scientific journals. Such a policy could prevent a repeat of these unfortunate circumstances.

Rumi K. Price, PhD, MPE, Research Assistant Professor of Epidemiology in Psychiatry, Department of Psychiatry, Washington University School of Medicine, St. Louis MO; Carol S. North, MD, Assistant Professor, Department of Psychiatry, Washington University School of Medicine; Simon Wessely, MRCP, MRC Psych, Senior Lecturer, Department of Psychological Medicine, King's College Hospital and Institute of Psychiatry, Denmark Hill, London, United Kingdom; Victoria J. Fraser, MD, Assistant Professor, Division of Infectious Diseases, Department of Medicine, Washington University School of Medicine.

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