by psychology. Yet, as in the recent article by Claman and Robertson,[1] studies on this subject often seem to proceed on the assumption that women with and without breast implants are otherwise equivalent except for this issue. The authors compared the occurrence of antinuclear antibodies in women with and without breast implants. No attempt seems to have been made to determine if these subgroups were the same before the surgical procedure.

Workers throughout the emerging field of psychoneuroimmunology argue that behavioral state may influence immune function. And articles regarding depression and immune dysregulation point out the relevance of the behavioral state to standard clinical disease.[2] Might this be important in the immune studies done on women with breast implants? Are we to assume that women who see themselves as needing physical modification are of the same psychological makeup as women who do not?

Genetics also has long been accepted as a fundamental issue in autoimmune disease. Yet, if we ignore this in the breast implant arena, we are left with the assumption that women who choose breast implants bear the same genetic substrate as women who do not.

In recent decades young women have recurrently predominated in furors over several poorly defined, chronic syndromes, including those of chronic fatigue (alternatively, Epstein-Barr virus), fibromyalgia, chronic candida, chronic whiplash, thoracic outlet, and others. These syndromes share the recurrent demographics of young women with long-term subjective symptoms bearing marginal correlation to laboratory factors. Of these syndromes, the first three bear witness to the recurrent question regarding some underlying immune dysfunction. The Epstein-Barr virus syndrome is especially pertinent for its historical and evolutionary perspectives. How many more incarnations of this issue must we have before we start recognizing the similarities of this recurrent story?

It remains to be determined whether the breast implant furor will turn out to reveal a biologic issue or another incarnation of a sociologic issue. Or, as in the case of some of the syndromes mentioned, we might discover a mixture: a molehill of pathology flourishing in a mountain of symptoms under the influence of social expediency. If we are to accurately answer the immunologic question, we must understand both the silicone and its recipient.

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TO THE EDITOR: The article "Antinuclear Antibodies and Breast Implants" by Claman and Robertson is very provocative, suggesting the possibility of an autoimmune problem related to breast implants.

To better assess the situation, however, one needs to know the incidence of antinuclear antibodies on Hep-II cells in a group of women who had "various symptoms" (group II) who had *no* implants. The same is true for women who have an autoimmune disease but do *not* have implants.

The question we are really asking is whether or not there is an increase in antinuclear antibodies in patients because of an implant. Because other diseases may cause antinuclear antibodies to be present, the denominator needs to be known.

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Dr Claman Responds

TO THE EDITOR: Perhaps the most interesting result in our study is that women with breast implants who feel well have a higher than expected prevalence of positive antinuclear antibody (ANA) tests. Dr Barbuto points out that women who seek breast implants (most often for augmentation) differ psychologically from those who do not. This is true and has been documented. He also points out that there is literature indicating that the behavioral state may influence immune function. To our knowledge, however, there are no data to indicate that behavioral states by themselves influence ANA positivity. It would be nice to have psychological profiles of our patients and controls before surgery, as suggested by Dr Barbuto. This is not possible with our patients, however, as the implants were put in as long as 25 years ago. For a new prospective study, it would be useful to have both screening psychological profiles and baseline ANA tests, although it is easy to see both practical and theoretical problems in trying to get these studies done. With regard to genetic influences, we agree that they are important. We are HLA-typing the next cohort of patients and controls.

Dr Koons makes a good point: we did not include ANA data from control women comparable to our group II—women with fatigue, arthralgias, and myalgias but no implants. We omitted these women, but not because we did not try to find them. We have had promises from private rheumatology clinics and a large general practice clinic—promises but virtually no referrals, particularly of women with those symptoms without a previous ANA test. We are now trying the newspapers.

Because we lacked this control group, we did not emphasize the prevalence of positive ANA tests in the women in group II. We cited an article that found a prevalence of 9% positive ANA tests in patients with fibromyalgia. We also have additional data of our own (run in a different ANA laboratory) showing a prevalence of 12% positive ANA tests in women with diagnosed fibromyalgia. What is far more interesting is the 18% prevalence of ANAs in group I—asymptomatic but with implants—compared with their appropriate asymptomatic

control group 0 without implants, which had 0% positive ANA tests.

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Previous Donner Party Research

TO THE EDITOR: The conclusions concerning Donner Party mortality presented by S. A. McCurdy in this journal[1] merely restate those that I presented in an article published in a major anthropological journal four years ago.[2] My article went uncited by McCurdy even though the results of my work have been discussed by such authors as Shipman[3] and Diamond[4] in widely distributed sources and were restated in my recent book on the history of the Great Basin.[5]

Even a passing effort by McCurdy to discover previous work in this area would have revealed my paper as well as those by Shipman and Diamond. For instance, it took me less than a minute to find the Grayson and Diamond references by searching under "Donner Party" in the Online Computer Library Center (OCLC) catalogue. It is unfortunate that McCurdy made no effort to discover whether he was merely repeating the work of others and that he gave no credit to those who came before him. As a result, he gave the impression of producing novel results when, in fact, he had not. It is equally unfortunate that THE WESTERN JOURNAL OF MEDICINE did not choose reviewers who might be expected to be familiar with research in this area.

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Dr McCurdy Responds

TO THE EDITOR: I appreciate the opportunity to acknowledge Dr Grayson's previous work[1] regarding the Donner Party, and I regret not having learned of it in time to cite it in my recently published article.[2] In preparing the manuscript, I read historical texts on the episode and conducted a MEDLINE search of the biomedical database back to 1966. Unfortunately, the anthropological research journal in which Grayson published was not indexed in this database. Nor were the secondary sources he mentions. In the year preceding its publication, I shared drafts of the manuscript and discussed the work with colleagues and interested persons, presented it at the national American Public Health Association meeting, and finally submitted it for formal peer review. None of this brought his article to my attention. When I learned of his work shortly before release of my article, I called out of courtesy to let him know that it would be forthcoming and that I was sorry not to have known of his work in time to cite him.

Grayson and I both examined demographic characteristics of the Donner Party and their influence on mortality, but salient differences in approach, methods, and findings exist between our studies. He showed that persons dying were more likely than survivors to be old or very young, male, and to have smaller kin groups. This descriptive approach did not allow a quantitative comparison of these factors for their relative importance or effect on mortality.

My work is new in bringing an analytic and epidemiologic focus on quantitative assessment of risk factors. Multivariate regression techniques yielded relative risks for mortality that were adjusted for known cofactors, allowing comparison with respect to the strength of their independent effects. For example, the analysis shows in quantitative terms the preponderant effect of age (relative risk of 6.6 for those younger than 6 years and a relative risk of 8.4 for those 35 years of age or older). The comparatively weaker effects of male sex and traveling without other family members (each with a relative risk of 2.0) are also shown. Our findings also differ with respect to the effect of family size on survival. No independent effect of family size per se is evident after restricting analysis to persons traveling with at least one other family member. In keeping with the epidemiologic focus, I also noted possible implications for medical responses to starvation disasters. Age and sex identify high-risk groups and are immutable factors. In contrast, family networks may be affected by response efforts, which could influence survival rates.

I hope readers of the journal will find this epidemiologic analysis useful and interesting despite the unintentional omission in not crediting Grayson's previous work.

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The Editors are pleased to receive letters commenting on articles published in the journal in the past six months, as well as information or short case reports of interest to our readers. ALL MATERIAL SUBMITTED FOR CONSIDERATION MUST BE DOUBLE-SPACED. Letters NO LONGER THAN 500 WORDS are preferred. An original typescript and one copy should be submitted. All letters are published at the discretion of the Editors and subject to appropriate editing. Those of a scientific nature will be peer reviewed. Authors should include information regarding conflict of interest, when appropriate ("I warrant that I have no financial interest in the drugs, devices, or procedures described in this letter"). Most letters regarding a previously published article will be sent to the authors of the article for comment. Authors of accepted letters will have an opportunity to review the edited version before publication.