Controller Studies and Medical Controversies

To the Editor:

The editorials in the June 1978 issue of Chest again raised a basic conceptual problem, i.e., controversies in medicine which may be unnecessary. Shubrooks\textsuperscript{a} description of the study by Johnson and colleagues\textsuperscript{b} includes the very significant sentence, “Although no claims of controlled studies can be made, it can at least be said that the majority of these patients can do well after surgery for aortocoronary bypass;\textsuperscript{1}\textsuperscript{p781} and, later, “Experience with surgery in such patients is certainly too limited at present to warrant any definite conclusions. . . ” Shubrooks thus touches on “controlled studies” and conclusiveness as an editorialist. I should think that, in that role at least, he should have insisted on controlled studies. Moreover, the question to the editorial board and reviewers is: Why publish any uncontrolled study, and particularly one void of “any definite conclusions”? This could be mitigated by clearly labelling such studies as technical reports; i.e., they may demonstrate that most patients can survive surgery and document symptomatic relief but, being uncontrolled, continue to generate controversy—which generates editorials.

The editorial by Noehren and Klauber\textsuperscript{e} reviewing intermittent positive-pressure breathing (IPPB) is a good summary of the “state of the art.” It is it such a common sight in large hospitals to see therapy with IPPB being administered and hear the pat-a-cake of respiratory therapists’ hands on rib cages that one would have thought that these were well-established and accepted methods; yet IPPB is another editorial-generating subject of controversy. Unfortunately, these editorialists omitted proposing controlled trials of therapy with IPPB. They did mention “objective tests” and “statistics.” “Objective” tests (presumably, tests yielding pictures, graphs, or numbers) provide a statistical base with a certain value in suggesting effects of treatment; however, how you arrive at your statistics depends on the design of study. One of the beauties of the controlled clinical trial with random assignment of qualifying patients is that it does not ask the mechanism of an effect of treatment, but it rather asks: What actually happens to the patients? How does treatment affect morality, morbidity, and the quality of life?

These comments are not meant to directly criticize thoughtful editorialists. The point is that where controversies persist among acknowledged experts, it is likely that no one has done a critical test of what they are disputing. For many (but perhaps not all) circumstances, an appropriately designed controlled clinical trial with random assignment of patients to alternate treatments or no treatment is most likely to reduce controversies.

David H. Spodick, M.D., D.Sc., F.C.C.P.
Professor of Medicine
University of Massachusetts Medical School
and Director, Division of Cardiology
St. Vincent Hospital, Worcester, Mass

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To the Editor:

One could hardly disagree with the need for well-designed, randomized controlled studies as the best means to eliminate controversies in medicine. It is unfortunate that many clinical situations do not lend themselves to this type of study. The article by Johnson et al in Chest does not claim to be such a controlled study, but I believe that it is worthwhile because it reports the findings in a large series of patients with a relatively uncommon entity. It is unlikely that within a reasonable period of time, any one institution, or even a group of institutions, would accumulate a sufficient number of patients with variant angina suitable for and agreeable to randomization between medical and surgical therapy for a definitive controlled study to be performed. Even in the far more common problem of coronary arterial disease with typical angina pectoris, we await such studies, which hopefully will be forthcoming; however, one can learn much from clinical studies based on large numbers of cases, whether or not they are termed “technical reports.” It is very helpful to know that a majority of patients with variant angina and obstructive coronary arterial disease will do well following bypass surgery if this treatment is chosen for them and that possibly a rare patient may benefit from surgery in the situation of variant angina with near normal coronary arteries. This is especially true, since for many of these patients, surgery is considered after failure of medical therapy.

Also, unfortunately, the controversies considered in my
editorial2 are not limited to medical vs surgical therapy but include the entire question of the anatomic substrate and physiology involved in “variant angina.”

Samuel J. Shubrooks, Jr., M.D.
Cardiology Unit, New England Deaconess Hospital
and Assistant Professor of Medicine
Harvard Medical School, Boston

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Lung Biopsy in Sarcoidosis

To the Editor:

In the editorial entitled “Lung Biopsy in Sarcoidosis” (Chest 74:120-121, 1978), Clyde H. Koontz, M.D., has written very clearly on the indications for lung biopsy in sarcoidosis. I think that his editorial comments are very pertinent, but I disagree with him in one point. He states that there are some asymptomatic patients with normal findings on physical examination, a negative tuberculin skin test, and bilateral hilar adenopathy in whom a histologic diagnosis is not required.

It is my personal viewpoint that a histologic diagnosis is required in all cases of sarcoidosis. I think that the diagnosis should be established histologically by mediastinoscopic examination in such a case. I think that the patient who does not have a confirmed diagnosis of sarcoidosis might be lost to follow-up and might then show up in three to five years with irreversible stage-3 sarcoidosis.

I do not believe that we can exclude lymphoma absolutely in the young person with stage-1 sarcoidosis who still may be asymptomatic. I don’t believe that we can exclude the diagnosis of mediastinal granuloma, although in my experience, usually these granulomas are unilateral on the right side in the chest roentgenogram.

I would like to see all patients with suspected sarcoidosis having histologic proof so that they then can be reexamined carefully every four months with tests of pulmonary function, including the carbon monoxide diffusion capacity and a chest x-ray film, so that we don’t allow a patient to progress into irreversible stage-3 disease when we have good treatment at hand. If the Kveim test ever becomes routinely available, then this would be a reasonable alternative way to go.

David E. Dines, M.D.
Department of Thoracic Disease
Mayo Clinic, Rochester, Minn

To the Editor:

Dines raises some important questions; however, my experience in medicine has taught me the pitfalls of saying “always” or “never.” Many experienced pulmonologists have maintained that there are some asymptomatic patients with stage-1 sarcoidosis whose symptoms are so classic that histologic diagnosis is not required.1,2 This approach does place a special burden on the physician to follow the patient’s course prospectively. Certainly, I would like to have a histologic diagnosis in each case, but sometimes one must ask at what price (both in terms of the possible morbidity from the procedure and its cost).

Mediastinoscopic examination may be the procedure of choice for histologic diagnosis for many cases of stage-1 disease; however, my personal preference remains transbronchial lung biopsy during an initial fiberoptic bronchoscopic procedure, unless there are mitigating circumstances. In both our series3,4 and that reported by Winterbauer et al,5 there were no patients with bilateral hilar adenopathy alone who were truly asymptomatic with normal findings on physical examination who had lymphoma; however, during the same period, we encountered four additional patients with bilateral hilar adenopathy who had clinical features atypical for sarcoidosis and very worrisome for malignant neoplasm (eg, firm, hard supraclavicular lymph nodes, superior vena cava syndrome). Transbronchial lung biopsy was not done in these cases because it was believed that lymph node tissue would be necessary to exclude malignant neoplasm. Two of these four patients had sarcoidosis at mediastinoscopic examination, and two patients had lymphoma.

I certainly agree with an approach of close regular follow-up of patients with sarcoidosis. The four-month interval which Dines suggests seems most reasonable, at least during the first several years. The extent of testing of pulmonary function on such routine visits is another issue. I believe that spirometric testing is certainly the most reliable, reproducible, and clinically useful study. I would like to do routine determinations of the carbon monoxide diffusing capacity (or, preferably, exercise studies with measurement of expired gases) but cannot justify the cost of such studies on a routine basis, since they usually are not essential in defining therapy.

Clyde H. Koontz, M.D.
Tacoma, Wash

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External Pulse Tracings in Midventricular Obstructive Cardiomyopathy

To the Editor:

Tye et al1 reported striking abnormalities in the external pulse tracings (carotid; apical) in a patient with the diagnosis of midventricular obstructive cardiomyopathy.2,3 The ejection time was markedly prolonged, and there was a midystolic notch in both carotid and apical tracings that was coincident with the termination of the ejection murmur. The systolic ventriculographic image that was illustrated showed

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