In some of the examples reported by Chesler, clear cut break in the echo trace between posterior aortic wall and mitral valve was not always evident. However, in the five examples of double outlet right ventricle, posterior displacement of the mitral valve in relationship to the posterior aortic wall was present in each case. According to Solinger et al., echocographic mitral-semilunar valve discontinuity implies a consistent break in the echo trace, or echoes must be recorded from an intervening structure separating the anterior mitral valve leaflet from the echoes of the posterior margin of the semilunar valve. Rapid angulation of transducer beam or downward motion of the transducer may produce an apparent or artificial break in the echo trace. Muscle tissue which may be present between the aorta and mitral valve in double outlet right ventricle may produce echoes and suggest continuity between the two structures although the mitral valve will be recorded posterior to the posterior aortic wall.

Mitral-semilunar valve discontinuity has been regarded as a specific angioGraphic sign of double outlet right ventricle. We agree with Dr. Gutgesell that discontinuity when applied to echocardiography is confusing.

The displacement of the mitral valve in relationship to the posterior aortic wall is what echocardiography defines. Our report emphasizes this echocardiographic relationship and describes the numerous disorders that affect it.

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References

On Pacing
To the Editor:

The Letter to the Editor, “On Prophylactic Pacing” by Drs. Lister and Gosselin (Circulation 51: 1170, 1975) requires further comment in our opinion. There are several points that should be made:

1. Our group is surprised at the “eggs and apples comparison” in the Letter by Drs. Lister and Gosselin. Two papers are referred to in the Letter on Prophylactic Pacing. The first is one by Lie et al. and the second is an abstract by Narula et al. The paper by Lie et al. refers to patients with acute myocardial infarction and discusses the indications for use of prophylactic temporary pacemakering in this group of patients. The abstract by Narula et al. refers not to patients with acute myocardial infarction, but to symptomatic patients with bilateral bundle branch block who present with vertigo, dizziness and/or syncope, and who were referred for permanent pacemaker implantation by other physicians. We can see little in common in these two groups of patients other than the fact that they both represent patients and that pacing catheters were considered in both. This type of confusion of different groups of patients is indeed unfortunate, and is one of the factors that sometimes makes it quite difficult to evaluate data and opinions in the literature.

2. We too share concern, as do all responsible physicians, with the increasing cost of medical care in the United States. However, our study was designed to help decrease such costs by preventing unnecessary pacemaker implantation in certain types of patients. In a period of three years, we have seen 12 patients in whom unnecessary permanent pacemaker implantation has been avoided. These patients have now been followed for three years and subsequent pacemaker implantation after the study reported in the abstract has not been necessary. If we assume that the average patient survives for any interval between one to three power pack changes after the initial pacemaker implantation, one can approximately calculate that a total of 35-40 initial and reimplant pacer implantations were avoided in this group of patients. The cost of the initial pacers, the reimplant pacers as well as the cost of rehospitalization and the cost of repeated office visits for followup evaluation of pacemaker function was thus avoided in this group of patients. With this in mind, we are puzzled by the confusion evident in the remarks of Drs. Lister and Gosselin. If Drs. Lister and Gosselin have any data suggesting that conduction studies are not of help in preventing unnecessary pacemaker implantation in patients with bilateral bundle branch block and symptoms of syncope and/or dizziness, we suggest that these data should see the light of day via the usual routes, i.e., in a medical journal, in a paper which would be subject to the usual scientific reviews. We believe that an expression of opinion without supporting data will not alter the diagnostic or therapeutic approaches of most physicians.

3. There is a heavy concentration of patients with coronary artery disease in the Miami area. We would like to suggest that a more fertile and effective area for concern with medical economics would be in consideration of stricter indications for coronary artery surgery in general and “prophylactic” coronary artery surgery in particular.

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References

Pacing the WPW Patient
To the Editor:

In 1968, we reported the first use of a permanent demand pacemaker in the treatment of supraventricular tachycardia due to Wolf-Parkinson-White syndrome. This patient is living and well now almost 8 years since implantation and has been successfully able to revert her recurrent tachycardia on many occasions. We have an additional experience in a second patient with Type A WPW using a coronary sinus pacemaker and I am aware of an additional patient with WPW syndrome with paroxysmal atrial fibrillation and junctional tachycardia treated in a similar manner (R. Langendorf, unpublished observation). There have been a number of other case reports in the literature using this technique in
Letter: Pacing the WPW patient.
S Goldstein

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