JACC Vol. 27, No. 2
February 1996:514–5

LETTERS TO THE EDITOR

Stress Echocardiography for Diagnosis of Coronary Artery Disease

The report by Dagianti et al. (1) makes the point of comparing exercise echocardiography, dobutamine echocardiography and dipyridamole echocardiography in the diagnosis of coronary artery disease. The topic is interesting, but the report may already appear old at birth. In fact, the field of pharmacologic stress echocardiography is in rapid evolution.

In the continuing quest for ideal diagnostic accuracy, pharmacologic stresses have quickly moved over the years from low dose to high dose regimens and eventually to atropine coadministration (2–4), which optimizes sensitivity. As a consequence, the pharmacologic stress protocols used by the authors can be considered obsolete, although this point was not mentioned as a study limitation. If atropine protocols are used, the sensitivity gap is filled because dobutamine–atropine and dipyridamole–atropine have a similar sensitivity (5).

This sensitivity is particularly important because the issue of sensitivity was the key factor in the conclusion of Dagianti et al. that exercise echocardiography should represent the approach of first choice, dobutamine the second and dipyridamole the third in their diagnostic algorithms. In addition, the authors did not cite the study with the largest patient series (136 patients), in which exercise, high dose dobutamine and high dose dipyridamole were compared by Beleslin et al. (6), who found similar accuracy (82% vs. 77%, respectively) for dobutamine versus dipyridamole. After the publication of the report by Dagianti et al., additional reports appeared that documented the nearly identical accuracy of high dose dipyridamole and high dose dobutamine (7,8).

Finally, the authors did not cite their own previously published data on dipyridamole echocardiography, which reported a striking 92% sensitivity and 100% specificity with transesophageal echocardiography (9). In the present study, the sensitivity falls to 55%, and even 33% in patients with previous myocardial infarction. Rather than pointing out the merits of transesophageal versus transthoracic echocardiography, the present study may be the most obvious demonstration of a statement reported by Picano (4) regarding the two basic laws of published reports on stress echocardiography:

1. No test is so bad that you cannot make it look good.
2. No test is so good that you cannot make it look bad.

Probably before drawing any conclusion on the relative merits of various stress tests, data obtained with state of the art protocols are warranted. The risk may be that useless data may be generated if obsolete protocols are administered (and even proposed), ignoring hard evidence reported by others.

MARCO TORRES, MD, PHD
Department of Medicine
Federal University of Rio Grande do Sul
Department of Cardiology
Hospital das Clínicas de Porto Alegre,
Rua Ramiro Barcelos 2350, Room 2060
Porto Alegre, RS 90035-006, Brazil

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Reply

The vast scientific and clinical potential of stress echocardiography has led to the publication of interesting reports on the diagnostic value of single stress echocardiographic modalities. However, for the practicing cardiologist, the choice of which stress test may be better for his or her patient is the most compelling issue. This issue can be adequately addressed only by studies like our own (1) that directly compare diagnostic efficacy by having the same patient undergo exertion, dipyridamole and dobutamine echocardiography, an approach that was lacking in published reports on stress echocardiography. Unfortunately, such studies are difficult to perform in a large patient series because of ethical and economical reasons. Our results compare well with most previous investigators and with those reported in a recent study by Beleslin et al. (2), which we did not quote because it published after the submission of our study (1). We want to also point out that in that study, recently published in *Circulation*, atropine coadministration, recently proposed to enhance the sensitivity of pharmacologic stress echocardiography, was not included in the dobutamine stress protocol; therefore, according to Torres, this study may also already appear old at birth. However, superiority of dobutamine–atropine echocardiography over exercise echocardiography cannot be inferred for lack of comparative studies. In our study, atropine was not used because we wanted to investigate the effects of dobutamine on hemodynamic variables and on the behavior of left ventricular volumes during the test that would be affected by the cholinergic antagonist. Insofar as concerns the hard evidence on the relative merits of dipyridamole echocardiography by Torres, we point out that in a comparative study by Picano et al. (3), no more recent than ours and quoted in our study, exercise and dipyridamole echocardiography yielded similar diagnostic results, whereas Marangelli et al. (4) recently obtained significantly higher sensitivity values for exercise than for dipyridamole. We would like to mention the most recent report of a higher diagnostic value for exercise echocardiography and dobutamine echocardiography over dipyridamole and even adenosine echocardiography (5). Accordingly, in our report we mentioned our previous experience using the transesophageal approach (6) with the view of underlining the need of improving the sensitivity values of dipyridamole echocardiography. Nevertheless, in that study the sensitivity for one-vessel disease was also low (67%).

0735-1097/96/$15.00
0735-1097(95)00457-2

As concerns the comment about the results of our study (1) in patients with a previous myocardial infarction, we assessed the efficacy of the three tests in detecting the extent of coronary artery disease: The accuracy values, not the sensitivity, in predicting the extent of coronary artery disease were 71% for exercise, 75% for dobutamine and 33% for dipyridamole.

Again, from a clinical perspective and after personal experience spanning nearly two decades, our view is that exercise testing firmly bears comparison with the easier to perform pharmacologic stress. Physical exertion is a better stress than dipyridamole, dobutamine and pacing according to a recent experimental study (7) in that it causes the most severe contractile dysfunction, and in clinical practice it maintains the unique capability of providing physiologic information on the patient's exercise capacity. Pharmacologic stress echocardiography, notably using dobutamine, could thus supplement rather than supplant the more traditional diagnostic role of exercise testing in the evaluation of chest pain. In light of the tangible difference between dipyridamole- and exercise-induced ischemic phenomena, dipyridamole being the coronary vasodilator liable to trigger ischemia in circumstances where no other physiologic activity can elicit the same response, in our opinion it is proper to wonder whether dipyridamole may be put to fruitful clinical use either as a means of diagnosing myocardial ischemia or as a valid tool for prognosis.

ARMANDO DAGIANTI, MD, FESC
Department of Cardiovascular and Respiratory Sciences
"La Sapienza" University
Viale del Policlinico
00161, Rome, Italy

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Antibiotic Prophylaxis Against Infective Endocarditis in Mitral Valve Prolapse

The thought-provoking exchange between Cheng (1) and Stoddard (2) concerning prophylaxis for patients with mitral valve prolapse raised questions that we have all struggled with. Should we recommend antibiotics for all such patients or only for those who demonstrate audible murmurs of mitral regurgitation, as recommended by the American Heart Association guidelines (3)? (What about the many patients who have no click, no audible murmur and no prolapse but have mild mitral regurgitation by Doppler at rest?)

One consideration that Cheng adduced for his argument that all patients with mitral valve prolapse should receive prophylaxis was that "antibiotic prophylaxis against infective endocarditis is highly cost-effective," quoting Gould and Buckingham (4). Analysis of the latter report suggests that this conclusion is far from secure.

First, Gould and Buckingham conceded that "there is no direct proof that antibiotic prophylaxis is efficacious." Lacking that, there is no way to prove their assertion that it is highly cost-effective. Second, they asserted that 15% of cases of infective endocarditis are attributable to dental procedures, citing an article by Bayliss et al. (5). Bayliss et al. reported a figure of 13.7% for cases that occurred as long as 3 months after a dental procedure. Three weeks would have been a more appropriate interval, according to Starkebaum et al. (6), who found that symptoms began within 2 weeks in 84% of the cases that they studied. When Bayliss et al. used an interval of ≤1 month to link a dental procedure with endocarditis, only 3.7% could be attributed to the dental procedure. This is nearly identical to the rate of 3.6% that I found after a literature search that included 1,322 cases (7). Another consideration in calculating the effectiveness of prophylaxis is the number of endocarditis cases with known heart disease before the infection, and that was only 42.5% (i.e., only 1.6% of all cases of endocarditis could have been prevented if prophylaxis had been successful). Considering that a number of cases that have had prophylaxis nevertheless develop endocarditis, even when the offending organism was susceptible to the antibiotic used (8), the percent of cases of endocarditis that could be prevented is surely <1.5%. This small number may explain why the incidence of endocarditis has not changed significantly since the introduction of chemoprophylaxis (5), which caused Bayliss et al. to argue that better dental care and hygiene are much more important than chemoprophylaxis.

Nevertheless, all the studies mentioned here advocate chemoprophylaxis for dental procedures. One good reason is to avoid litigation. At the least, we should make prophylaxis as simple as possible. Fortunately, the American Heart Association in 1991 abandoned their recommendation for intravenous antibiotics for high risk patients, which was never practical for most parts of the country. It is also reasonable to abandon the second dose of amoxicillin, in that the proven duration of bacteremia after extraction is only 15 min (7). The British have used only a single dose for some years (5). But for cost-effectiveness, chemoprophylaxis should receive a lower priority from cardiologists than good dental health.

WARREN GUNTHEROTH, MD, FACC
Department of Pediatrics (Cardiology)
University of Washington School of Medicine
1959 Northeast Pacific St.
Box 356320
Seattle, Washington 98195-6320

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