activity performance, showing that antianginal efficacy can be assessed reliably without necessarily using attack frequency as a variable.

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References

The authors reply:
To the Editor:
It is unfortunate that Dr. Nyberg has chosen to extract specific statements from the general context of our article “Considerations in Evaluating New Antianginal Drugs.” In so doing, he has removed them from the overall thrust of our article in order to emphasize some points of special concern to him. Thus, the statement “without a demonstrated decrease in anginal frequency results of other studies are academic” was made after a preceding sentence that indicated that this was requisite preliminary information. We are obviously well aware of the relationship of many attacks of angina pectoris to activity; furthermore, we indicate in the next sentence that confirmation of an agent’s effectiveness is accomplished through testing procedures, which are specified as activity-related procedures in the body of our article. Thus, we have no disagreement with Dr. Nyberg regarding evaluation of angina and relationship to activity.

We do take strong exception to his suggested approach to compare nonresponders and responders as a valid scientific procedure. While the general experience might be that 40% of patients will be placebo responders, in a specific trial, particularly one involving small numbers of patients, one cannot assume that this or any other figure will obtain. Thus, the highest level of confidence is reached only when one does a placebo-controlled, randomized, prospective trial in which the effects of an indistinguishable inert agent can be compared with a proposed new medication.

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Hospital Care for Acute Myocardial Infarction
To the Editor:
Despite the importance of the research questions posed by Dr. Goldman and co-workers in their recent article1 and the need for such data in order to explain recent encouraging declines in the mortality rates of coronary heart disease,2-3 a methodologic problem exists in the interpretation of their findings. In conducting any epidemiologic study, and certainly in one of the magnitude of the study reported, one must have clear and concise preestablished diagnostic criteria in order to accept or reject patients into the study population. This tenet is particularly important when one is using as the primary source of data hospital medical records, given their limitations as a research tool as well as the attendant problems in the recording, coding and transferring of collected information to a computerized diagnostic format. Failure to adhere to these principles places the results of such a study in jeopardy and makes them difficult to interpret, given the considerable latitude for introduction of various biases.4 For example, in a recently completed nonconcurrent prospective epidemiologic study,5 of 2444 medical records reviewed with a diagnosis of acute myocardial infarction from 20 metropolitan Baltimore hospitals, only 1401 (57.4%) met the study criteria for acute myocardial infarction and formed the study population. Given these caveats and despite the immensity of the task, until the medical records of patients included in the study of Goldman et al. are reviewed in a systematic manner, the interesting findings of these authors must be viewed with reservation.

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References

The authors reply:
To the Editor:
Our principal goal was to determine the extent to which the decline in national ischemic heart disease mortality between 1973 and 1978 could be ascribed to changes in the in-hospital mortality of patients with acute myocardial infarctions. The former statistics are based on death certificates rather than on standardized diagnostic criteria, and we used analogous methods by relying on hospital discharge diagnoses.

Just as Gore and Goldberg are correct in their assertion that patients with the primary discharge diagnosis of acute myocardial infarction might not have infarcts by standardized predetermined criteria, many patients have inaccurate death certificate diagnoses.1-3 Thus, the epidemiologic data regarding declining national ischemic heart disease mortality share the same uncertainties as our report. The solution to both uncertainties is the same: a laborious clinicopathologic review of many thousand hospital discharge diagnoses and death certificates in an attempt to verify the more readily available data. Although national statistics and our report represent the best data available, our article’s title indicates that we agree with Gore and Goldberg’s assertion that the findings reported from both data sets must be regarded as evidence and not as proof.

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