Young Adults Surviving Myocardial Infarction

To the Editor:

The recent report by Warren et al (Chest 75:667-670, 1979) merits comment. They found, in a group of 68 patients under the age of 36 who had suffered a myocardial infarction, nine (13 percent) had normal coronary arteries on angiogram.

There is no doubt that we are seeing younger patients in our coronary care units with their first myocardial infarction. Most cardiologists are subjecting these patients to coronary arteriography as part of their post discharge evaluation for the reasons cited by the authors. Their incidence of arteriographically normal coronary arteries seems quite high, but the series comprises a select group of patients. They all survived their infarctions and some were referred from other hospitals. It must be realized that this was not a consecutive series of all young myocardial infarction patients seen in the coronary care unit and then subjected to angiographic evaluation.

The implications of the study by Warren and colleagues are significant, especially in light of recent knowledge concerning the pathophysiology of ischemic heart disease. Coronary vasospasm must be considered in all patients with angina and/or myocardial infarction, especially in the absence of significant atherosclerosis. Dual channel ambulatory monitoring (even in the absence of chest pain) may aid in its detection. In a recent study in three patients with variant angina, 1045 spontaneous episodes of ST segment elevation were observed, 89 percent of which were asymptomatic.1 Ergonovine testing during diagnostic coronary angiography should also be considered in such patients. Even though the sensitivity and specificity of ergonovine testing for the detection of coronary vasospasm is not defined, the test appears to be highly specific in patients with typical Prinzmetal's angina.3 It has recently been reemphasized that patients with coronary vasospasm may have one (or more) myocardial infarctions even in the absence of atherosclerosis.3

Two clinical conditions which may mimic ischemic heart disease and/or be associated with myocardial infarction in the absence of atherosclerosis, must also be considered. The first is the mitral valve prolapse syndrome. These patients often have electrocardiographic changes suggestive of ischemic heart disease; furthermore, they can present with acute myocardial infarction and angiographically normal coronary arteries.4 Coronary spasm has been suggested as the underlying mechanism and receives further support by a recent study. Buda et al5 retrospectively found ten patients with angiographic spasm (eight were at the catheter tip), and nine had evidence of mitral prolapse. We have recently encountered four patients with the prolapse-click syndrome and evidence of coronary spasm (Mautner RK, Phillips JH; unpublished observations). Hypertrophic obstructive cardiomyopathy (HOCM) is a second condition in which patients may present with angina and electrocardiographic evidence of infarction. Furthermore, Maron et al6 recently reported seven patients with HOCM who, at necropsy, had evidence of transmural myocardial infarction with no significant atherosclerosis of the extramural coronary arteries. This study and our recent report of three patients with HOCM and associated coronary vasospasm7 suggests a causal relationship.

In conclusion, coronary artery spasm should be considered and its documentation sought in patients with ischemic syndromes and no demonstrable atherosclerosis. The total contribution of spasm in the clinical area has not yet fully unfolded; however, there is no doubt that its impact is significant.

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Effort Independence and Forced Expiratory Flow

To the Editor:

The May, 1978 issue of Chest contained an editorial by Dr. Sobol entitled, "Effort Independence and Forced Expiratory Flow—End of an Era?" I have not seen any reaction to this editorial since that time, and therefore, I am writing this letter.

Dr. Sobol suggested that the term "effort independent," as applied to the flow-volume curve, be abolished. I would like to take exception to this suggestion and to the implications of such a recommendation.

As I see it, Dr. Sobol's thesis is that since measurements from the flow-volume curve, such as maximal flow at a given percentage of the vital capacity, are not totally reproducible, it therefore follows that the entire concept of relative effort independence of maximal expiratory flow is no longer a useful or valid concept. The term effort independence was coined to describe the phenomenon of expiratory flow limitation, namely: there is a limit to maximal expiratory flow at a given lung volume such that pressures in excess of the lowest pressure necessary to produce maximal flow do not increase the flow. In the early description of this phenomenon, it was emphasized that there was a reproducible precise limit to
expiratory flow in a given subject only if the mechanical properties of the lung remained constant, and only if flow was measured at the same lung volume. There are many articles in the literature attesting to the fact that there is indeed a flow-limiting mechanism during forced expiration.

The problem in terms of reproducibility often arises from the fact that most measures are made at a given percentage of the vital capacity and measurements of maximal expiratory flow are very sensitive to changes in the vital capacity volume. For example, if the flow-volume curve has a slope of 2.4 L/sec per liter expired volume, then a reduction of VC of 0.5 L can either lead to a "false" increase or decrease of maximal flow of approximately 0.6 L/sec. This does not mean that the flow-limiting mechanism does not still operate. It means that one has not been able to accurately match lung volumes on repeated efforts. One can frequently obtain several flow-volume curves that give varying measures of \( V_{EM} \), for example, but by aligning the various curves, it is clear that over most of the flow-volume range the curves are identical.

Another factor that can contribute to variability of repeated efforts is thoracic gas compression, and this is illustrated in the Figure. Panel A shows two consecutive efforts by the same subject with flow and volume measured at the mouth—the conventional way of obtaining these curves. There is clearly a difference between the curves with the flow at 50 percent VC (2L) from the solid curve being 5.9 L/sec and that from the dashed curve 4.4 L/sec. Yet when volume was measured from a body plethysmograph (panel B), it can be seen that the two efforts were identical. The difference seen in A stemmed from the fact that during the dashed curve intrathoracic pressure was much higher, causing more gas compression, than during the solid curve. This gas compression "artifact" is well-recognized and was clearly described by Ingram and Schilder (J Appl Physiol 21:1821, 1966). The message is that, in truth, the two efforts were identical and the expiratory flow-limiting mechanism was operative. The apparent "variability" seen in A resulted from the manner in which the curves were recorded and does not invalidate the concept of flow-limitation and effort independence as originally described.

What is needed to improve the reproducibility of measures of maximal expiratory flow are improved techniques for handling the flow-volume data to minimize errors that are introduced, for example, by slight changes in volume and variation in subject effort. Similarly, efforts are needed to maximize the information that is contained in the forced vital capacity maneuver, particularly in the flow-volume presentation. Thus, studies such as those recently reported by Pezlin and associates (Am Rev Respir Dis 119:271, 1979) are clearly more productive than studies emphasizing the fact that some measurement from the curve is rather variable.

The emphasis should be placed on minimizing the noise and maximizing the physiologic data that can be obtained. Along this line, Tien et al (J Appl Physiol 46:565, 1979) have been examining computer-based techniques to establish the "true curve" for a given individual. With proper data processing, they present evidence indicating that a subject's flow-volume curve at a given point in time is extremely reproducible and carries surprising amounts of information in terms of contour. These are the types of approaches that will make the analysis of the forced expiratory maneuver even more valuable than it is currently.

In my view, Dr. Sobol confuses the issue by placing so much emphasis on the lack of reproducibility of maximal flow that by inference one could conclude that the concept of expiratory flow limitation is invalid. Although he may not mean to imply this, it is the message that many readers will take home from his editorial. Clearly, the mechanism of expiratory flow limitation should not be equated with the usual measures of reproducibility. Flow limitation is an established fact. The challenge is to improve our techniques of handling flow-volume curves. I see little merit or logic in suggesting that since we still have not accomplished this the whole concept is suspect. Indeed, I suggest that rather than having reached the "end of an era," we are just embarking on an era of greater understanding of the flow-limiting mechanism and the forced vital capacity maneuver.

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To the Editor:

Since language is the fundamental form of human communication, it should, particularly scientific language, be as precise and descriptive as it is in our power to make it. The term "effort independent" is as far from this requirement as one could imagine. It implies "regardless of effort." I doubt whether anyone would contend that air will issue from the lung between functional residual capacity and residual volume without the intervention of effort, yet this portion of the expiratory volume occupies that presumably unique segment characterized as "effort independent." In point of fact, the entire "effort independent" segment is critically dependent upon effort until maximum flow is achieved. Beyond this point, flow is presumed not to increase despite increasing effort (although even this concept is not universally accepted). Since effort is required to reach optimum flow, "effort independent" is a misnomer and misleading.

I do not consider the issue a mere matter of semantics, since it is evident that the phrase has been taken quite literally by many. However, the thrust of my article was that "effort independence" has become synonymous with "reproducibility," and this in turn has been applied to a portion of the spirogram. Dr. Hyatt's letter adds another dimension to the "confusion." Flow limitation is equated with "effort independence," which in turn is equated with reproducibility. There was nothing in my editorial which was critical of the concept of flow limitation, nor was any such "inference" intended. Despite this, Dr. Hyatt's letter is almost exclusively a defense of the validity of the concept of flow limitation, a concept which I have no reason to question. Although I do not question it, I do vehemently deny that it can be equated with "effort independence" and in turn with reproducibility. My car will only go about 20 miles per hour up the hill that