Sustained Ventricular Arrhythmias in Patients Receiving Thrombolytic Therapy

To the Editor:

Newby et al¹ report on the outcome of patients with acute myocardial infarction (MI) receiving thrombolytic therapy in the Global Use of Streptokinase Tissue plasminogen activator for Occluded coronary arteries (GUSTO-I) trial whose course was complicated by ventricular arrhythmias. Patients were classified according to arrhythmia type into the following 3 groups: (1) 1439 patients had ventricular tachycardia (VT) only, (2) 1656 patients had ventricular fibrillation (VF) only, and (3) 1085 patients had both VT and VF. These patient groups were further stratified according to the time of occurrence of the arrhythmia, ie, early (<2 days) or late (>2 days).

When comparing the corresponding patient groups, substantial inconsistencies in the number of patients in each group is noted. Thus, the total number of patients in the early (n=354) and late (n=96) VT only groups in Table 4 is only 450 (31%) of the 1439 patients with VT only in Table 2; the number of patients in the early (n=1229) and late (n=209) VF only group in Table 4 is only 1438 (87%) of the 1656 patients with VF only in Table 2; and the total number of patients with early (n=774) and late (n=159) events in the group with both VT and VF in Table 4 is only 933 (86%) of the 1085 with both VF and VT in Table 2. The calculated mortality rates are also inconsistent. How is it possible that in-hospital mortality is higher in patients with early (34.5%) and late (37.5%) VT only in Table 4 than it is in the same group (18.6%) when reported as a whole in Table 2? Similarly, the data on 30-day mortality and 1-year mortality are nearly twice as high in the early and late VT only subgroups (Table 4) when compared with the VT only group as a whole (Table 2).

Because apparently not all patients with sustained ventricular arrhythmias were part of the early versus late subgroup analysis, one must question the conclusion that sustained ventricular arrhythmias, regardless of their timing relative to the acute MI, confer a long-term risk for increased mortality. This is of importance because this finding conflicts with the current viewpoint that ventricular arrhythmias during the acute phase of MI do not adversely affect the long-term prognosis of hospital survivors.²–⁴

The authors speculate that, similar to the findings of Heidbüchel et al,³ the pathogeneses of VT and VF early during acute MI are distinct. This speculation is based on the remarkable contrast in patency rate between patients with VF and sustained VT." Whereas Heidbüchel et al found an occluded infarct-related artery in all VF patients but in only 1 of 9 patients with VT, the current study shows no difference in TIMI flow between patients with VT only and VF only. A difference in TIMI flow was only demonstrated in comparison with patients without ventricular arrhythmias. Therefore, the current study contradicts the findings of Heidbüchel et al if one relates TIMI flow with type of arrhythmia (VT versus VF).

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Response

We appreciate Drs Windecker and Seiler’s insightful comments on our article. They are concerned about some discrepancies in the numbers presented in Tables 2 and 4.¹ We are sure that they realize that the Global Utilization of Streptokinase and Tissue plasminogen activator for Occluded coronary arteries (GUSTO I) study was not designed to assess the relevance of different types of sustained ventricular arrhythmias during the acute and late phases of myocardial infarction. Table 2 includes all patients who had ventricular tachycardia (VT), ventricular fibrillation, or both. In the subanalysis for early versus late occurrence, many of the late cases were lost because information about the late occurrence of VT was not mandatory. Discrimination between early and late onset was also dependent on the voluntary report of the time of onset of the arrhythmia and the need for cardioversion for termination, which had to be included in the patient profile. If one considers that the majority of cases of early VT required electrical cardioversion for termination, it is conceivable that the mortality of such cases is higher than the mortality of cases with a late onset. Indeed, a preliminary report from our group showed a higher mortality in patients with VT who required cardioversion versus those who did not.²

Close attention to the 1-year mortality of 30-day survivors in Tables 2 and 4 demonstrates an absolute similarity in the percentages reported. This should dissipate concerns that we may be looking at selected patients who are not representative of the overall groups with early or late VT.

Drs Windecker and Seidel also think that there is a discrepancy between the study of Heidbüchel et al,³ which showed reperfusion in VT patients, and our study, which showed an overall similar TIMI flow among the ventricular arrhythmia groups. We do not think that the study of Heidbüchel et al is adequately powered to speculate on reperfusion as a discriminating feature between VT and ventricular fibrillation. The number of patients in this study is far too small, and the angiographic data was obtained between 10 and 14 days after thrombolytic therapy. However, Heidbüchel et al considered VT, either early or late, as an expression of a more stable substrate. TIMI flow in the “arrhythmia” groups compared with the “no arrhythmia” patients seems to support this concept because it shows a higher incidence of complete occlusion in the group with arrhythmia, even when the analysis was performed before the occurrence of the arrhythmia. This may not have been absolutely clear in our article.

We hope this reply answers Drs Windecker and Seidel’s concerns about the validity of our preliminary observation, which certainly needs to be confirmed in a prospective study. Again, even if some inevitable selection is present in our population, one cannot deny or ignore that patients with early VT have a 1 year mortality higher than that observed in the group without arrhythmia.

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