Letters to the Editor

Sir, Fabian et al. [1] have made an excellent and much-needed contribution to the methodology of systolic time intervals, a technique which is becoming more important in noninvasive evaluation, particularly where precision is most essential, e.g. cardiac clinical pharmacology. I should like to make a few comments that perhaps the distinguished authors would wish to comment on.

In company with very many people their historical introduction omits mention of Blumberger, the original pioneer who, by the 1940s, had covered nearly all the material that many of us have used since then in one or another variation. Indeed, Blumberger introduced the index LVET/PEP, which yields a whole number instead of a decimal, and I have the impression that subsequent workers (who virtually never cite Blumberger’s work) inverted it mainly to be different.

My more important comment concerns heart rate correction for pre-ejection period (PEP). The authors, as many others have done, have found an apparent heart rate-PEP relation, though at a low correlation coefficient (r). They did not, however, state the significance of the correlation (p) which in most cases has been extremely low or, as in our work, absent. Indeed, we reported on a mixed group of patients and normals who show no significant correlation either for the whole group or for those with individual diagnoses [2]. Since atrial pacing does not change the PEP within reasonable heart rates, the conclusion can be that one or more other factors affect PEP and heart rate simultaneously and therefore, from time to time, particularly in normal subjects, a weak correlation at borderline or nonsignificant p levels may be found. Indeed, in the same issue of Cardiology, Puviani et al. [4] do not correct PEP for heart rate, presumably for this reason. Indeed, Fabian et al. [1] have cited the pacing studies of Cokkinos, one of the many authors who have observed no PEP change with pacing. Moreover, it should not be surprising that PEP/LVET in the work of Cokkinos (and others) was heart rate-related. If only one of the terms of a ratio, in this case LVET, is rate-related, then the whole ratio must be rate-related.

I was delighted to see that in their group of normal men, Fabian et al. [1] obtained the same regression equation for LVET against heart rate as we obtained in normal men [3].

These remarks are meant more to stimulate the discussion rather than in direct criticism of good work which was a necessary contribution to systolic time intervals. I would strongly encourage Fabian and colleagues to attend the next meeting of the ISM – International Society of Mechanocardiography – (Budapest 1985) where these important considerations could be discussed on the floor and personally.

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Reply

Sir,

We should like to thank Professor Spodick for the review of our paper and his most valuable suggestions. It is very gratifying to know that the paper has stimulated this discussion. The introductory part of our paper [1] was not intended as a historical overview. We are familiar with the studies of Blumberger [2] and cited them repeatedly in our papers published in Czechoslovakia. In agreement with Professor Spodick we think that they represent pioneer work in the field of systolic time intervals. It was not the aim of our study to assess PEP relation to heart rate. Our paper reports on our own findings only. In the initial manuscript we discussed the PEP/heart rate relation which the reviewers justly qualified as superfluous and not in conformity with the purpose of the paper. Therefore we deleted this passage in the ultimate version of our manuscript. Obviously, the PEP value is influenced by many factors and its relation to heart rate may be secondary. Yet it seems also rational to assume that studies covering a wide range of heart rate may reveal a more pronounced relation between the two values given a low slope of the straight line expressing the relationship. In our view, this problem is very controversial and requires further study.