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reports showing changes in PEP and HR the directional results for PEPI are generally the same as for PEP itself, probably owing to disproportionate effects of the intervention on PEP and HR.

Despite these criticisms, which I hope Dr. Lewis and associates will address in detail, this is an excellent paper that should be welcomed by everyone interested in systolic time intervals. I have already made it required reading among our noninvasive staff (as are most of Dr. Lewis's previous contributions).

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References


The author replies:
To the Editor:

We wish to thank Dr. Spodick for his comments on our manuscript. Apparently he is in agreement with our results which indicate 100 mm/sec is the optimum paper speed. He raised the question of observer bias. We indeed conducted our study as he suggested ... "completely coded and randomized to reduce observer bias so that observers could never read data from the same subject in sequence except by chance." All observers received individual copies of each tape-recorded pulse arbitrarily coded and in random order according to patients and paper recording speeds.

Apparently in Dr. Spodick's as yet unpublished study, he found no statistically significant difference for the LVET while we noted such a difference. It should be noted that we used a computer analysis to achieve a standard against which to judge the LVET measurements at each paper speed. This may account for our different results. However, even if the mean values were not statistically different, the greater observer variability at slower speeds dictates that 100 mm/sec is the optimal recording speed. I feel that this debate over paper speed has become somewhat academic since modern technology has resulted in a number of different recording devices with 100 mm/sec capability.

The argument over whether to correct the PEP for heart rate is essentially an "empirical vs functional" argument. Until it can be proved that PEP is functionally independent of heart rate, we must rely on carefully collected empirical data. From such studies it is clear that a significant, albeit weak, correlation exists. Naturally, only studies with large numbers of subjects will reveal this relationship. Atrial pacing data cannot be applied to this situation, as this is an unnatural mode of increasing the heart rate (i.e., no increase in adrenergic tone).

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Small vs Large Surface Area Electrodes

To the Editor:

We were interested in the article by Hughes et al. (Circulation 54: 128, 1976) because the promise of lower long-term thresholds with small-area electrodes has brought them into widespread use. If a hazard exists in their failure to sense, it would seriously jeopardize their use, as competitive pacing is still a highly unattractive and potentially harmful long-term complication.

We would agree with the observations of the authors that the combination of a small-area electrode and a low input impedance of the detection device may lead to failure of sensing. However, we would like to assert that small-area electrodes coupled with high impedance detection circuits such as are used in most modern pacemakers do not constitute such a hazard. In other words, the signal provided by a small-area electrode is determined by the combination of the interface impedance of the electrode and the input impedance of the detection device, or pacemaker. Also, a small-area electrode should produce a larger and sharper inherent signal than a very large area (within limits) since the large area will average the signal more than a smaller one.

The failure to clearly point out the unique and almost artificial circumstances of their experiments is illustrated in their table 1. The results demonstrate how the 1 KΩ load, in combination with the electrode impedance, attenuates the signal strength. As pointed out by the authors, the 1 KΩ load is much lower than commercially available pulse generators. The effects with actual pacemakers having impedances of 20 KΩ will be about twenty times less severe. The true "sensed" electrogram would be represented by an unloaded measurement, but these values are unfortunately not provided.

Some other points are:

1) bipolar electrodes do not inherently sense smaller signals than unipolar electrodes.
2) Fibrous tissue does not greatly affect the electrode-tissue impedance — the impedance is mainly at the metal-electrolyte interface. The fibrous tissue does, however, separate the electrode from viable tissue and so reduces the sensed signal.
3) In figure 5, the electrode tip design for optimal sensing is surely offered as an example of principle rather than as an actual electrode. The geometry of the large band sensing electrode is such that it would be very difficult to have it in intimate contact with excitable tissue if used as an endocardial electrode.

In summary, small-area electrodes as they are currently used do not constitute a hazard for sensing when the higher interface impedance associated with these electrodes is appropriately used with a higher input impedance of the detecting pacemaker.

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The authors reply:

To the Editor:

We agree completely with Dr. Cuddy that bipolar electrodes are not inherently inferior to unipolar electrodes. The ability of any type electrode to sense R-wave potentials is based primarily on its surface area, not the mode in which sensing is done. I hope we did not leave that impression with anyone in our paper.

Secondly, we have only limited information of the chronically implanted electrodes' ability to sense. We would, however, expect the sensing ability to be reduced by fibrous tissue ingrowth.
Small vs large surface area electrodes.
T E Cuddy and M B Raber

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