The cost-effectiveness of dual-chamber pacing

See page 574 for the article to which this Editorial refers

The cost-benefit analysis by Sutton and Bourgeois\[1\] must seem like a breath of fresh air to the many investigators whose work was so meticulously cited. For a long time we have known intuitively that restoring and maintaining atrioventricular synchrony contributes to the quality and duration of life. The authors have provided objective proof. The remarkable consistency of independent reports for disparate centres cited by the authors is strong evidence for the importance of dual-chamber pacing from both the clinical and financial standpoints. My remarks might serve as an extrapolation based on clues present in the text.

It is lamentable that these eminently sensible observations and conclusions are not more widely understood by pacemaker implanters, and that they appear to have had little impact on the payers in health-care systems. For example, although in 1993 we used dual-chamber pacing systems in 83\% of cases at our institution, the corresponding figures for the United States as a whole and for Canada were 45\% and 30\% respectively (\(1^2\) and personal communication). Now that there is more proof that appropriate selection of pacing mode is cost-effective, perhaps the providers and payers will listen.

One factor mentioned only briefly by the authors should be incorporated in the algorithm, namely the difference in complication rates between single and dual-chamber pacing. Lead dislodgement was estimated to be 2\% for single- and 5\% for dual-chamber pacing, and to have only minor impact on costs, but the level of implanters’ expertise was not addressed. Pacemaker-related complications are more common than one might expect, especially in dual-chamber pacing. In 1993, complication rates were 2–3 times as frequent in dual- as in single-chamber pacing\[2\]. In addition to higher electrode-dislodgment rates, there are more device alerts, infections, lead failures, and cardiac perforations in dual-chamber pacing, to name a few specific complications.

Battery capacity and output programming also affect pacing costs. In 1993, for example, pacing-survey respondents in the U.S.A. and Canada failed to programme 30\% and 16\% of pulse generators, respectively, within 3 months of implantation (\(1^2\) and personal communication). Failure to reduce the stimulus voltage and duration to the lowest reasonable settings reduces device longevity considerably and therefore increases costs. Nor could the authors consider the implanters’ technical competence. Inexperienced implanters may have initial complication rates as high as 25\%, and in the U.S.A. the median number of implantations per implanter is less than 12 per year\[21\]. In the U.K., where pacemakers are implanted in fewer centres, this problem of quality may be less prevalent. Implantation-volume and complication-rate data might eventually be added with advantage to the cost-prediction algorithm.

The failure of third-party payers to understand the difference between annualized and amortized costs has always been unfathomable. Even at our centre, administrators fuss constantly over the cost of each device that is implanted, ignoring the long-range savings that might be realized by selecting more appropriate, though initially more expensive, devices. (Long-term savings do not help to balance the current budget). A strange example of amortizing costs was seen in the rapid abandonment of pacemakers with radioisotopic power sources. Even though the pulse generators cost twice as much as the
then standard devices, our studies show that they paid for themselves many times over, with 80% still functioning at 20 years (unpublished data).

It would be immensely interesting to apply the authors' model to a comparison of adaptive-rate single- and dual-chamber pacemakers. One would suspect that a similar difference will be found, because patients typically require augmented pacing rates less than 5% of the time. When adaptive-rate pacing was first introduced, efforts were made to show that adaptive-rate ventricular pacing (VVR) was as effective as atrioventricular synchronous dual-chamber pacing (DDD), even in patients with normal atrial rhythms. Hyperbole has abated somewhat, but not entirely, because it is still far from certain that adaptive-rate pacing modes are required with anything like the frequency with which they are now used.

In this report of a computerized cost-prediction method, it was not appropriate to consider all issues that may affect costs. Consider, for example, the current avalanche of single-pass leads for 'partial' dual-chamber pacing in which the atrium is sensed, but not paced (VDD). One can only assume that the absence of atrial pacing eventually will lead to the deleterious long-term outcomes so thoroughly studied by the authors. Furthermore, one looks forward to the eventual inclusion in the algorithm of additional quality-of-life issues, such as impaired response to physical effort and consequent reduction in economic productivity, and the deleterious effects of episodic retrograde ventriculoatrial conduction.

One wonders, therefore, why dual-chamber pacing is not used more often. In several quadrennial surveys we asked that very question, but survey respondents, though they perform 15 to 20% of all U.S.A. implantations, tend to be the more frequent and dedicated implanters. Explanations varied, but many respondents believed that dual-chamber pacing was not really physiologically necessary, and therefore not worth the trouble to implant. Some conceded that the higher device cost and greater difficulty of implanting two leads were deterrents. How else can one explain the present enthusiasm for VDD pacing with single-pass leads? In the 1993 survey, surgeon respondents implanted significantly fewer dual-chamber pacing devices that did non-surgeons[2]. One can only assume that some physicians were more impressed than others with the physiological advantages of dual-chamber pacing.

A prospective clinical study begs to be done, but will be difficult to accomplish and still take many years to complete, considering all the familiar data-collection problems and the difficulties encountered by previous cost-effectiveness studies in differentiating costs from charges. The author's algorithm is an excellent tool for studying costs and other outcomes such as length of hospital stay and quality of life. It will be fascinating to see new reports after other factors are added to the calculations.

V. PARSONNET
Newark Beth Israel Medical Center,
Newark, New Jersey, U.S.A.

References

European Heart Journal (1996) 17, 496-497

Too old for heart valve replacement?

See page 583 for the article to which this Editorial refers

Daniel Callahan, an American philosopher, has long argued against the expenditure of 'heroic' interventions to save the life of 'too old' patients who have lived 'long enough'[41]. If these interventions are followed by a life of hospitalized misery, so be it. But if an operation, and a 10-day hospital stay, can take an almost bed-ridden near-death octogenarian to a symptom-free life with independence in daily activities, only matters of cost remain. Consider ‘... the budget is always limited, the elderly don’t earn anymore, and the chance should go to a neonate trying to survive in a pediatric intensive care’. But didn’t the elderly contribute for the health care of their pension years?

The debates may appear conjectural today, but the burden of the elderly is growing: in just a few more years a fourth of our population will be elderly and retired. Under maximum focus will be the burgeoning costs of interventions in cardiovascular disease. In this issue, Olsson et al. take up the case of