Primary PTCA for acute myocardial infarction—a logistic comment

The early mortality in the control/thrombolysed patients in the combined analysis of the three immediate PTCA trails referred to above is lower than that reported in any controlled trial of any thrombolytic regimen (5-9%) and the success rate of the initial angioplasty in the intervention groups was very high (≥95%), no doubt testimony to the expertise of those involved. But how likely is it in the foreseeable future that such resources and expertise could be more widely available around the clock to assess and deal with the rising tide of patients admitted with chest pain query acute myocardial infarction? Before grappling with that issue a much larger comparative trial of acute intervention vs the ‘best’ thrombolytic schedule undertaken in many different hospitals with staff of varying expertise is required. Only then shall we get a clearer perspective of the range of risks and benefits from each treatment option.

If such a large trial eventually confirms these preliminary exploits, what of the resource implications of offering such a service on a continual basis countrywide? Using the United Kingdom as an example, there are approximately 300 000 patients with acute myocardial infarctions per year. If we assume for the moment that approximately one third will not reach hospital alive and that roughly one half of those that do will be deemed ineligible for thrombolysis, that still leaves 100 000 patients eligible for primary PTCA instead of thrombolysis. In addition, an unknown proportion of ‘thrombolysis ineligible’ patients may also be suitable for acute intervention. So we could arrive at a yearly demand for intervention of between 100 000–200 000 purely for acute myocardial infarction at a time when the current total PTCA activity is about 12 000 per annum. Even trying to select ‘PTCA preferred patients’, such as those with large anterior infarctions, still makes the logistics of providing such a service awesome.

So could other clinical or financial benefits accrue as a direct result of such a policy, for instance fewer days spent in hospital or convalescence before returning to work, fewer recurrent ischaemic events necessitating readmission and fewer drugs for symptomatic treatment? In this issue, the group from Zwolle in The Netherlands assert a firm yes. They randomized 301 patients to either direct PTCA (152) or streptokinase (149). Over a mean follow-up time of 31 months, there were 12 (8%) deaths in the PTCA group (seven cardiac), compared with 20 (13%) in the...
In addition there were four non-fatal reinfarctions in the PTCA group vs 25 in the streptokinase group, presumably all necessitating readmission to hospital. Patients randomized to PTCA had a higher initial and follow-up left ventricular ejection fraction than those randomized to streptokinase (48 ± 12% vs 43 ± 13%, \( P=0.006 \)). It is asserted that this small superiority of the interventional group over streptokinase is a direct result of early more complete and more sustained reperfusion than those patients randomized to streptokinase. Although we are not given a breakdown of hospital stay, drug treatments, work status, physical activity, or symptom status in this paper, the Zwolle group assert a total saving of Dfl.0-288 per patient, or Dfl.1-793 per survivor, or Dfl.9086 per event-free survivor in those assigned PTCA. Multivessel coronary artery disease and a previous myocardial infarction were associated with increased costs.

Neither in this paper nor in the original reports of the three comparative trials cited above are we told what proportion of all suspected or eventually proven infarctions are represented by those patients randomized to PTCA or thrombolysis. This, I believe, is an important omission when trying to budget for the totality of admitted acute myocardial infarction patients rather than just those suitable for PTCA or thrombolysis.

The modest individual savings reported from Zwolle, when multiplied over a whole country, imply that PTCA might not be so financially daunting as first thought. But the initial outlay in terms of plant and sufficient trained personnel will, in my view, restrict such an enterprise to areas of high enthusiasm in countries either with generous health care budgets or those with an acceptance that other medical activity will inevitably suffer financially.

Finally, in order to compete favourably with thrombolysis, interventional treatment must be close at hand—not restricted to distant tertiary referral units with the inevitable delay in arranging and effecting patient transfer. This would mean that each district hospital would need at least two (perhaps three or four) experienced invasive teams in order to offer a round the clock service. Members of such teams will need to maintain dexterity and competence by engaging also in diagnostic coronary work—and perhaps elective coronary angioplasty. Such extension of activity will enter the debate of surgical cover, not for acute myocardial infarction work necessarily, but for the inevitable creeping development.

Providing an acute PTCA service for patients with AMI, should it prove to be the superior strategy, may thus have logistic consequences way beyond the coronary care unit.

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References


The importance of reducing delay in acute myocardial infarction

See page 429 for the article to which this Editorial refers

The study by Ottesen et al. in this issue\(^1\) highlights one of the most important aspects of early heart attack care. The interval between the onset of an acute myocardial infarction and the initiation of infarct-limiting therapy is important, since the effect of such treatment is inversely related to this interval\(^2\). Despite the fact that this is well-known in the