remains to be seen. In any case, as a precedent for important confirmatory studies in the future, I believe that 19 patients is just too small a sample size to be recommended. One could ask the question: what therapy would I choose if I had a child suffering form persistent pulmonary hypertension? Well, I would certainly choose ECMO based on the available evidence. However, I would also choose ECMO even if the data were only 7/9 versus 6/10 in its favor. In other words, when your own neck is on the line you always want to choose the treatment that appears to be best. Unfortunately, if everyone is permitted to do this the resulting anarchy would totally undermine the scientific rationale on which the best modern medical research is based. For this reason, emotive questions like the preceding one tend to cloud our reasoning when we debate the merits of randomization.

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Comment

Peter Armitage and D. Stephen Coad

Dr. Ware has performed a valuable service in two particular respects. He has given us a carefully documented case study, tracing the role of the statistician from the interpretation of past data, to the planning of a new investigation and the analysis and presentation of its results. Editors of statistical journals frequently bemoan the paucity of case studies amongst the papers submitted to them. Here is an excellent example of such a study.

More specifically, Dr. Ware has described one of the very few clinical trials using any form of outcome-dependent allocation. Armitage (1985) has drawn attention to the need for more interchange of ideas and experience between theorists and practitioners concerned with this aspect of clinical trial methodology. Dr. Ware's paper is a welcome contribution to the literature, from both a practical and a theoretical viewpoint.

There are several examples in therapeutic medicine of unresolved questions, for which the evidence relies almost entirely on nonrandomized comparisons, but where investigators have, for ethical reasons, been reluctant to initiate randomized trials. It is hard to resist the view, expressed, for instance, by Chalmers, Block and Lee (1972), that randomized studies ought to be initiated at a very early stage of the introduction of new methods (they would say for the first patient). In the wake of the earlier inconclusive trial of ECMO, and the controversy to which it gave rise, the present investigators naturally had to tread cautiously, and their wish to restrict the use of CMT as far as possible is understandable. In a rather similar, and equally controversial, situation recently, the (British) Medical Research Council gave firm backing to an extensive trial of vitamin supplementation for women becoming pregnant after an earlier pregnancy resulting in a neural tube defect, to see whether supplementation reduces the risk of a further affected infant. Some had argued that evidence from nonrandomized studies was sufficient to justify routine use of supplementation. The MRC took the view that firm and reliable evidence was needed, and that a randomized trial, carefully monitored, was justified (Wald and Polani, 1984). Its results are awaited eagerly.

The evidence for the superiority of ECMO over CMT, patchy as it is, seems to us fairly convincing. Our view, though, is heavily affected by the fact that patients in phase 2, all of whom received ECMO, were apparently at higher risk than those in phase 1. The eligibility criterion was tightened to exclude some less severely affected patients, and a higher proportion than in phase 1 were outborn, a characteristic apparently conferring higher risk. Had this feature not been present we should have been only moderately impressed, on the grounds that the comparability of phases 1 and 2 was in doubt and that the evidence from phase 1 was weak.

As regards phase 1, we are skeptical of any analysis that suggests a difference much more significant than is given by the Fisher exact test. The Bayesian probabilities for $p_1 > p_2$ are small, partly because an arbitrary amount of prior (and therefore posterior)

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