the article rather than used as an opportunity for arresting but unsubstantiated statements.

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1 Tunstall-Pedoe H, Bailey L, Chamberlain DA, Marsden AK, Ward ME, Zideman DA. Survey of 3765 cardiopulmonary resuscitations in British hospitals (the BRESUS study): methods and overall results. *BMJ* 1992;304:1347-51. (23 May.)

AUTHOR'S REPLY.—The BRESUS group is pleased with the interest created by the first publication from our collaborative survey and regret that we could not publish all the results together. Alistair McIntyre's results agree closely with ours. We agree with him that the initial cardiac rhythm is a major guide to prognosis. Our results await publication but, taking the best subgroup as an example, patients with ventricular fibrillation in the cardiac care unit had an initial survival rate of 77% and, by life table analysis, of 47% at one year. This, however, accounted for only 163, or 4%, of our 3765 arrests and 27% of the arrests in the cardiac care unit.

Restricting resuscitation to these cases—taking Kevin Stewart and Adrian Wagg's argument to the extreme—would have increased our survival rates almost fourfold but would have limited the actual numbers of survivors at one year to one sixth of what was actually achieved. As they state, case selection makes the survival rate a dubious method of assessing performance, and we did not know what case selection was taking place in different hospitals. But, as we showed, numbers of beds and admissions to coronary care units also correlated poorly with numbers of resuscitations. We had thought to relate those resuscitated from apparently dead to the numbers of deaths over the same period in each hospital, but unfortunately this denominator proved to be prohibitively difficult and expensive to determine.

Stewart and Wagg argue that age alone may not matter in prognosis as it is confounded by increasing prevalence of diseases (some of which are listed by Elizabeth Smith). This is difficult to disprove because not all pathological conditions are recognised in elderly people. We have, however, shown a strong gradient with age, consistent with the increasing case fatality with age of almost every medical and surgical condition treated in hospital. The physiological reserve and safety margin of every body system decline with age, but we agree that biological age is more important than calendar age, and we did not suggest that there should be an age cut off for attempting resuscitation. Survival rather than disability was the end point for this paper, but we do have unpublished data on disability, in answer to Smith. The few survivors at one year reported to have severe brain damage in this study were young.

Though agreeing with the importance of case selection, we do not agree with Richard Keatinge's suggested mechanisms by which our results might have been artificially improved. Arrest episodes were identified for the hospital log before the outcome was known; the member of the resuscitation team was responsible for supplying detail but not the original notification. Some hospitals did show a decline in reported numbers of arrests when switchboard staff or the hospital link person were on holiday, but this had no effect on survival rates as fatal and non-fatal events did not seem to be underreported differentially. We do not think that including recurrent arrests after 24 hours would improve survival figures as the survival to discharge of such people must be poor and including each one more than once would lower average survival.

Several correspondents, most notably Peter Sainsbury, want evidence for the statement that "compared with other life saving interventions hospital resuscitation is seen to be cost effective."

This was not a comparative study of costs, nor a formal trial. An unpublished analysis of medical and surgical interventions shows costs per quality adjusted life year ranging from £150 to £19000. We would expect hospital resuscitation to be towards the cheaper end of this range. Improved training and facilities could improve cost effectiveness by improving the preselection of cases, improving survival in the event, or minimising brain damage associated with delayed anoxic resuscitation.^{1,2}

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1 Weaver WD, Copass MK, Buft D, Ray R, Hallstrom AP, Cobb LA. Improved neurological recovery and survival after early defibrillation. *Circulation* 1984;69:943-8.

2 Mullie A, Buylaert W, Michem N, Verbruggen H, Corne L, De-Cock R, et al. Predictive value of Glasgow coma score for awakening after out-of-hospital cardiac arrest. Cerebral study group of the Belgian society for intensive care. *Lancet* 1988;i: 137-40.

Reprocessing data to form QALYs

EDITOR.—The generation of quality adjusted life years (QALYs) from existing reports of quality of life after surgery highlights some of the shortcomings of using published reports to derive locally applicable information.¹ Joanna Coast shows clearly the inadequacy of published studies for her "quick and dirty" method of deriving QALYs; little mention is given, however, to the relevance of the data found to the specifics of local practice in the United Kingdom. Recent reports about the application and modification of decision theory to clinical practice are open to similar criticism as small changes in the applied data can have a considerable impact on the advice that the analysis offers.² Published series, especially if observational, tend to report optimal practice from centres of excellence. In constructing any form of analysis that hinges on a local outcome the use of results from other centres is not wholly appropriate.

We believe that if the kind of technique that Coast describes is to find application then local results, generated from local audit, should be used whenever possible for factors that are determined by local practice, such as operative mortality or quality of life. When patients ask of their doctor, "What are my chances when I place myself in your hands?" they do not want the answer to be based on the results of others working elsewhere. Using local data to derive QALYs or for clinical decision support systems is fundamental to their success.

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1 Coast J. Reprocessing data to form QALYs. *BMJ* 1992;305: 87-90. (11 July.)

2 Thornton JG, Lilford RJ, Johnson N. Decision analysis in medicine. *BMJ* 1992;304:1099-103. (25 April.)

EDITOR.—Joanna Coast's analysis of the problems found in reprocessing data on outcome from published reports into quality adjusted life years (QALYs) is incomplete.¹ She used the Rosser-Kind matrix of quality adjusters for health states defined by two dimensions, disability and distress.² This makes several assumptions which were not made explicit.

The first assumption is that different states of health can be described adequately by using the dimensions of distress and disability. Rosser and Kind's categories of disability are actually closer to "handicap" as defined by the World Health Organisation.³ Their classification is elegant and

has the virtue of simplicity. The descriptions used for each state, however, are extremely brief and cannot really take into account the important effects of resources, relationships, or the physical environment on the impact of disease.

More problematical are assumptions made about the derivation of the scale weights for the different health states described. The original scaling study used descriptions of different degrees of pain to describe levels of distress. The same scale weights are unlikely to apply if symptoms of urinary outflow obstruction are being used to describe distress. The scale weights were derived from 70 subjects, of whom 86% were of professional or managerial status and 66% were under 30 and 90% under 45. In Coast's paper the procedures evaluated were all based on a typical subject aged 65-70, whose background and values may be different. Evidence suggests that the method used by Rosser and Kind to measure scale weights gives different values from those given by other methods.⁴ Rosser and Kind also showed considerable differences between scale weights calculated for different groups of subjects (medical and psychiatric inpatients, nurses, healthy volunteers, and doctors).

Whose values should be given most weight in decisions concerning allocation of resources is arguable. Patients' views are relevant, but in a democratic society should they be represented any more than would be the case if a random sample of the population was selected? Studies of cost effectiveness are crucial if rational and equitable decisions on allocating resources are to be taken. We should be cautious, however, in using imperfect tools for want of anything better—at least, not without making the underlying assumptions clear.

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1 Coast J. Reprocessing data to form QALYs. *BMJ* 1992;305: 87-90. (11 July.)

2 Rosser RM, Kind P. A scale of valuations of states of illness: is there a social consensus? *Int J Epidemiol* 1978;7:347-58.

3 World Health Organisation. *International classification of impairments, disabilities and handicaps*. Geneva: WHO, 1980.

4 Buxton M, Ashby J. The time trade-off approach to health state valuation. In: Teeling Smith G, ed. *Measuring health: a practical approach*. Chichester: Wiley, 1988:69-88.

Preschool screening for cryptorchidism

EDITOR.—Surinder A Kaul and D P W Roberts's call for a screening programme for undescended testes seems to be motivated by a desire to reduce the incidence of testicular cancer.¹ Although a history of unilateral or bilateral undescended testis is an accepted risk factor in patients presenting with testicular tumours, we are unaware of any evidence to suggest that orchidopexy reduces this risk. The advantage gained from orchidopexy is that the testis is placed in a position that permits regular examination and, hopefully, early detection of a tumour. In boys presenting after puberty with unilateral intra-abdominal testis orchidectomy may be more appropriate than orchidopexy because of this risk, especially as spermatogenesis rarely occurs in such testes.

Kaul and Roberts's data on age at orchidopexy give rise to some concern as under 39.1% of boys underwent orchidopexy before the age of 2. Current paediatric surgical opinion advocates orchidopexy before the age of 6, and preferably before the age of 2, to avoid the loss of spermatogonia resulting in infertility.²

In calling for a screening programme Kaul and Roberts fail to acknowledge one that already exists: all boys should be examined at birth, 6 weeks, 8 months, 18 months, and 3 years, either by their family doctor or at a community child health