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

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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

clinic, and an abnormally placed testis should be detected.¹ As some undescended testes descend spontaneously we suggest that all boys with an undescended testis at 8 months should be reviewed at 1 year and, if the testis has still not descended, referred to a surgeon so that orchidopexy can be arranged during the second year of life.

To reduce the toll of testicular cancer better health education is required as this would also benefit all potential victims, 90% of whom have normally descended testes. Self examination should be taught at an early age and may lead to earlier presentation of testicular tumours.

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- 1 Kaul SA, Roberts DPW. Preschool screening for cryptorchidism. *BMJ* 1992;305:181. (18 July.)
- 2 Hadziselimovic F, Herzog B. Cryptorchidism. *Pediatric Surgery International* 1987;2:132-40.
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Causes of lower gastrointestinal haemorrhage

EDITOR,—In the article on lower gastrointestinal haemorrhage in the ABC of colorectal diseases diverticular disease heads the list of causes of such haemorrhage in elderly people.¹ The author goes on to point out that although diverticular disease was once considered to be the most common cause of major lower gastrointestinal haemorrhage, many cases are now attributed to angiodysplasia.

Though I accept that massive bleeding may rarely occur from a colonic diverticulum, it is, I believe, a common misapprehension that diverticular disease also causes less severe intermittent blood loss from the lower bowel. In over 30 years' experience of general surgery I cannot remember a single case in which some other cause, usually a polyp or carcinoma, was not found for such bleeding. I believe that it is dangerous to attribute bleeding from the lower bowel to diverticular disease and that full investigation, usually including flexible sigmoidoscopy or colonoscopy, is mandatory.

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- 1 Jones DJ. Lower gastrointestinal haemorrhage. *BMJ* 1992;305:107-10. (11 July.)

Pregnancy associated plasma protein A in Down's syndrome

EDITOR,—Among the maternal blood markers of fetal Down's syndrome reported so far three seem to be much more sensitive than the others. These are human chorionic gonadotrophin (both the intact molecule and its free β subunit), neutrophil alkaline phosphatase, and pregnancy associated plasma protein A (PAPP-A). Nicholas Wald and colleagues' report showing PAPP-A concentrations at 9-12 weeks' gestation to be substantially reduced in 19 pregnancies in which the fetus had Down's syndrome serves to confirm PAPP-A as a potentially important marker.¹ In the previous published series of 14 such pregnancies, however, measurements were made at 6-9 weeks' gestation,^{2,3} so that all the published data relate to a period much earlier in pregnancy than when screening for Down's syndrome is currently performed (15-20 weeks).

We therefore examined PAPP-A concentration at the later gestation in Yorkshire region's screening programme for Down's syndrome. In this, maternal blood is collected for measurement of

α fetoprotein, unconjugated oestriol, and human chorionic gonadotrophin concentration, and residual serum samples are stored at -25°C or cooler. The region is served by a single cytogenetics laboratory, whose records were used to identify 18 cases of Down's syndrome in the screened population. For each case five control pregnancies were found, matched for gestational age. All samples were retrieved from storage and assayed for PAPP-A in two analytical batches, without the person performing the assay knowing which samples were from cases. A radioimmunoassay was used at the Royal London Hospital based on a commercial antibody (Dako) and purified PAPP-A from the University of Odense. All results were expressed in multiples of the normal median (MoM) for the appropriate gestation derived from the regression of PAPP-A concentration on gestation in the 90 controls.

The distribution of concentrations in the cases was lower on average than that in the controls but not substantially so, and the difference was not significant. The 25th, 50th, and 75th centiles were 0.54, 0.87, and 1.11 MoM respectively in the cases compared with 0.69, 0.99, and 1.42 MoM respectively in the controls. The 95% confidence interval for the median in the cases (0.64 to 1.20 MoM) did not overlap with that in either the 6-9 week or the 9-12 week series (0.09 to 0.51 and 0.17 to 0.46 MoM respectively). The same reagents were used in all three series, and our results therefore suggest that PAPP-A concentration is a more sensitive marker in early pregnancy.

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- 1 Wald N, Stone R, Cuckle HS, Grudzinskas G, Barkai G, Brambati B, *et al.* First trimester concentrations of pregnancy associated plasma protein A and placental protein 14 in Down's syndrome. *BMJ* 1992;305:28. (4 July.)
- 2 Brambati B, Lanzani A, Tului L. Ultrasound and biochemical assessment of first trimester pregnancy. In: Chapman M, Grudzinskas G, Chard T, eds. *The embryo: normal and abnormal development and growth*. London: Springer-Verlag, 1991:181-94.
- 3 Brambati B, Macintosh MCM, Teisner B, Shrimanker K, Maguinness S, Lanzani A, *et al.* Low maternal serum levels of pregnancy-associated plasma protein-A (PAPP-A) in the first trimester in association with abnormal fetal karyotype. *Br J Obstet Gynaecol* (in press).

EDITOR,—Nicholas Wald and colleagues report low maternal serum concentrations of pregnancy associated plasma protein A (PAPP-A) in the first trimester in pregnancies in which the fetus had Down's syndrome.¹ They found no significant relation between the concentration and gestational age in normal women, presumably because of the limited time window (9-12 weeks), and therefore expressed their data as multiples of the median for the whole control group. Other authors have reported increasing PAPP-A concentrations over the first trimester.^{2,3} In our study (of 444 women with fetuses of normal karyotype; serum samples taken at 6-12 weeks) we used the same radioimmunoassay as that used by Wald and colleagues; we found that the PAPP-A concentration increased strikingly over this time.⁴ The relation between

the median PAPP-A concentration and gestation (expressed as days from the last menstrual period) was:

$$\log_e(\text{PAPP-A}) = 0.512 + 0.091d \quad (R^2 = 92\%, \quad p < 0.0001)$$

Should measurement of PAPP-A concentration emerge as a screening test in the first trimester accurate gestational dating will be needed to interpret the result of the test.

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- 1 Wald N, Stone R, Cuckle HS, Grudzinskas JG, Barkai G, Brambati B, *et al.* First trimester concentrations of pregnancy associated plasma protein A and placental protein 14 in Down's syndrome. *BMJ* 1992;304:28. (4 July.)
- 2 Folkersen J, Grudzinskas JG, Hindersson P, Teisner B, Westgaard JG. Pregnancy-associated plasma protein A: circulating levels during normal pregnancy. *Am J Obstet Gynecol* 1981;139:910-4.
- 3 Bischof P, Duberg S, Herrmann W, Sizonenko PC. Amniotic fluid and plasma concentrations of pregnancy-associated plasma associated protein-A (PAPP-A) throughout pregnancy: comparison with other fetoplacental products. *Br J Obstet Gynaecol* 1982;89:358-63.
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AUTHOR'S REPLY,—It is recognised that pregnancy associated plasma protein A (PAPP-A) concentrations increase with gestational age during pregnancy. As M C M Macintosh and T Chard say, the relevant issue in our study is that, between nine and 12 weeks' gestation, we found no observable association between PAPP-A concentration and gestation so there was no need to generate normal median PAPP-A concentrations specific to gestation. We agree that this should not be taken to mean that gestation could be ignored if measurement of PAPP-A was to be used as a screening test.

More important is to define the gestational period over which measurement of PAPP-A concentration may be a useful screening test. Measurement of PAPP-A concentrations with an enzyme linked immunosorbent assay and using a commercial antibody to PAPP-A (DAKO) at 15-17 weeks of pregnancy in 16 pregnancies in which the fetus had Down's syndrome and 147 in which it did not (controls) indicated that the measurement is not useful at this time; the median value in the pregnancies in which the fetus had Down's syndrome was 1.02 multiples of the median (95% confidence interval 0.81 to 1.28) — that is, virtually the same as that in the control pregnancies of the same gestational age (NW, AV, unpublished data).

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Notifying contacts of people with HIV infection

EDITOR,—R A Keenlyside and colleagues report a useful survey of attitudes to tracing and notifying contacts of people with HIV infection¹; their introduction and discussion, however, show that they are unaware of the General Medical Council's clear guidelines on notification of partners introduced in 1988.²

According to the guidelines, doctors can disclose the HIV status of patients to their sexual partners without the patients' consent. Before doing so,