

## Antibiotics as initial treatment for children with acute otitis media

### Use of odds ratio in calculation of number needed to treat was inappropriate

EDITOR—Christopher Del Mar and colleagues' meta-analysis of antibiotics in acute otitis media in children raises some questions.<sup>1</sup> The authors included only six studies in the analysis, but an earlier meta-analysis by Rosenfeld et al included 33 studies.<sup>2</sup> The authors report that 60% of patients given placebo were pain free within 24 hours but that, two to seven days after presentation, only 14% in the control group still had pain. We would have expected 40% (100% minus 60%) still to have pain.

The number needed to treat, based on pain at two to seven days, was 17 in the meta-analysis. In a Cochrane review of the same trials, however, this number was 12.<sup>3</sup> It seems that the authors used the odds ratio for the calculation, which is inappropriate, since it is calculated as the inverse of the risk difference. Using the risk difference, according to figure 1 with a fixed effects model, as the authors also used, we arrived at 23. Furthermore, the use of the number needed to treat does not eliminate the importance of confidence intervals; the confidence interval we calculated was 15 to 56, which is more meaningful than a point estimate. Finally, the authors mention that the number needed to treat was of the same order as that in the meta-analysis by Rosenfeld et al.<sup>2</sup> This begs the question of why the authors did a new meta-analysis so soon after the first one, especially since the earlier meta-analysis was published in 1994 and Del Mar and colleagues did not search Medline beyond August 1994.

The authors report that there was a trend for antibiotics to confer benefit in terms of deafness at three months. However, there must be an error in figure 1, since the total numbers affected in the two studies shown add up to 38 v 49, whereas the subtotals are 64 v 66.

The authors state in the text and in the abstract that "antibiotics were associated with a near doubling of the risk of vomiting, diarrhoea, or rashes." However, this finding relates to just one study, of the broad spectrum penicillin amoxicillin, which contributes 90% to the weight in their meta-analysis. The statement is therefore irrelevant for small spectrum penicillins, which most clinicians would probably prefer

for treatment of acute otitis media if they choose to treat at all.<sup>3</sup>

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- 1 Del Mar C, Glasziou P, Hayem M. Are antibiotics indicated as initial treatment for children with acute otitis media? A meta-analysis. *BMJ* 1997;314:1526-9. (24 May.)
- 2 Rosenfeld RM, Vertrees JE, Carr J, Cipolle RJ, Uden DL, Giebink G, et al. Clinical efficacy of antimicrobial drugs for acute otitis media: metaanalysis of 5400 children from thirty-three randomised trials. *J Pediatr* 1994;124:355-67.
- 3 Glasziou PP, Hayem M, Del Mar CB. Treatments for acute otitis media in children: antibiotic versus placebo. In: Douglas R, Berman S, Black RE, Bridges-Webb C, Campbell H, Glezen P, et al, eds. *Respiratory infections module of the Cochrane database of systematic reviews* [updated 03 March 1997]. Oxford: Update Software, 1997. [Available in the Cochrane Library (database on disk and CD ROM); updated quarterly.]

### Diagnostic criteria need to be defined

EDITOR—We are concerned that, as a result of Christopher Del Mar and colleagues' statistical meta-analysis of antibiotic treatment of acute otitis media in children, such treatment may be questioned in the future by cost conscious purchasers. There are three reasons for our concerns.

Firstly, the diagnostic criteria quoted in the meta-analysis were varied and not scored as a quality indicator. One paper relied on the appearance of the tympanic membrane alone,<sup>2</sup> and another quoted a diagnostic uncertainty of 58% in children aged under 12 months (the peak age for otitis media).<sup>3</sup> Because an ear drum of a child with secretory otitis media with a mild earache can look exactly the same as that of a child with acute otitis media, we think it important to define the diagnostic criteria—that is, an acutely ill child with a high fever, severe pain, and an abnormal tympanic membrane. In our view it would require a brave doctor to deny antibiotics to such a child.

Secondly, historical data show a decrease in the incidence of mastoiditis since 1946 (which saw the advent of antibiotics) in Europe despite no great change in the nutritional status of the population.

Thirdly, in the developing world mastoiditis is rife (incidence after acute otitis media 18%) and intracranial complications such as subdural or extradural abscess, lateral sinus thrombosis, and labyrinthitis are common.<sup>4</sup> Streptococcal heart disease and glomerulonephritis are also regularly treated. Although it could be argued that

these populations are immunocompromised because of their poor nutritional status, in many parts of the world access to antibiotics is virtually non-existent. Do we really want to start to re-educating the medical students of the Western world in treating these complications?

While we welcome the debate on this subject, we still believe that the case for abandoning antibiotics in a child presenting with genuine severe otitis media is not yet proved.<sup>4 5</sup>

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- 1 Del Mar C, Glasziou P, Hayem M. Are antibiotics indicated as initial treatment for children with acute otitis media? A meta-analysis. *BMJ* 1997;314:1526-9. (24 May.)
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### Advice to authors

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*Letters, whether typed or sent by email, should give each author's current appointment and full address. Letters sent by email should give a telephone and fax number when possible. We encourage you to declare any conflict of interest. Please send a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.*

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### Abandoning treatments that you have used for years is difficult

EDITOR—Why is it so difficult to put research findings into practice, especially when a traditional treatment is shown to be of little value? Christopher Del Mar and colleagues report their meta-analysis of antibiotic treatment for children with acute otitis media.<sup>1</sup> Three days later a general practitioner colleague brought her family to see me in an evening surgery. Her 10 month old daughter had had four infections in total; the first two she had diagnosed as acute otitis media and treated with erythromycin, the third was a minor gastroenteritis, and the last was thought to be another ear infection worthy of antibiotic treatment. By the fourth day of the illness, however, when the child was brought to my surgery, a rash had developed, giving the clue to the viral nature of the infection. The reason for the consultation was that the child's father, having just returned from the United States, where "putting tubes in is routine," was concerned that the child might need grommets to prevent further ear infections and deafness. I pointed out that the evidence for benefit from this operation was weak and heard myself saying to the parents, "I think you're doing the right thing by simply treating each infection as it arises." Then I remembered the paper I had just read, clearly showing that antibiotic treatment conferred no benefit in terms of the risk of further infections or long term deafness; only marginal benefit in terms of the control of symptoms; and a doubling of the chance of vomiting, diarrhoea, or rashes. The mother had also read the paper but, like me, had assumed that we would go on treating her child with antibiotics for any acute ear infection. Why? While I avidly take up new treatments with proved benefits, such as the eradication of *Helicobacter pylori* or anticoagulation in atrial fibrillation, when years of practice are overturned and shown to be of little value it is all but impossible to switch to doing nothing. My perception of what my patients have come to expect must play a major part.

If we could remove the obstruction to implementing research which shows that a treatment can be safely abandoned, then we could reduce unnecessary side effects and consultations and save money for more effective treatments as well. It seems a crucial step. A radical suggestion would be to exclude from the NHS any treatments proved to have no benefit; patients could still have them if they wished, but it would seem reasonable to ask them to foot the bill for expensive placebos.

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1 Del Mar D, Glasziou P, Hayem M. Are antibiotics indicated as initial treatment for children with acute otitis media? A meta-analysis. *BMJ* 1997;314:1526-9. (24 May).

### Authors' reply

EDITOR—As the Cochrane version of our review of antibiotics and otitis media will be continuously improved and updated, we are

pleased to receive comment.<sup>1</sup> In answer to Helle Krogh Johansen and Peter C Gøtzche, we identified eight (not six) trials of antibiotics versus no antibiotics (as shown in our table 1). Only six of these studies reported clinically relevant outcomes. Rosenfeld et al's meta-analysis identified four trials of antibiotics versus no antibiotics and 29 comparing different antibiotics.<sup>2</sup> Because we identified twice as many studies for the principal question and our principal outcomes were patient centred (rather than microbiological), we believed that an update was required. One main outcome is pain, which by 24 hours is present in only 40% of patients. By two to seven days pain is present in only 14% of patients. Because antibiotics afford no benefit at 24 hours, we calculated the number needed to treat for pain at two to seven days. We apologise for the approximation involved in using the odds ratio to estimate the number needed to treat (equals 17). The latest version of the Review Manager software (REVMAN v3) allows two better ways of calculating the number needed to treat.<sup>1</sup> The relative risk of 64% means a 36% less chance of having pain at two to seven days if antibiotics were used initially. Since 14% will still have pain, the number needed to treat is  $1/(0.14 \times 0.36) = 20$ . Alternatively, directly estimating the risk difference in the meta-analysis gives a result of 0.038, with a corresponding number needed to treat of  $1/0.038 = 26$ . The authors correctly point out a transposition error in our figure 1—the subtotal for deafness at three months was carried over from the one month figure and should read 38/182 (treatment) and 49/188 (control); the summary odds ratios indicated in figure 1 are correct. The authors also point out that most of the information about vomiting, diarrhoea, or rashes comes from the study by Burke et al.<sup>3</sup> Unfortunately, only three of the eight trials report important side effects: clinical trials should report adverse as well as beneficial outcomes. Perhaps Jonathan E Osborne and Rhys T Nguyen are right to be concerned about severe infections and the developing world. We could not distinguish different outcomes between severe and milder cases. However, a policy of parsimonious antibiotic prescribing has not led to disaster in the Netherlands.<sup>4</sup> We sympathise with Ian Hill-Smith's dilemma, although we do not suggest that antibiotics are useless; their benefits are just rather modest. Accordingly, their use should be discretionary rather than either prohibited or mandatory.

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1 Glasziou PP, Hayem M, Del Mar CB. Treatments for acute otitis media in children: antibiotic versus placebo. In: Douglas R, Berman S, Black RE, Bridges-Webb C, Campbell H, Glezen P, et al, eds. *Respiratory infections module of the Cochrane database of systematic reviews*. Oxford: Update Software, 1996. [Database on disk and CD ROM.]  
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### Relative risks are inflated in published literature

EDITOR—Relative risks are often reported incorrectly in medical journals. In a paper in the *BMJ*, Jian-Min Yuan and colleagues describe a relative risk of 3.72 for the relation between cancer of the upper aerodigestive tract and heavy drinking as a "3.7-fold increased risk"; this description is incorrect.<sup>1</sup> They also describe a relative risk of 1.30 for total mortality and heavy drinking as a "30% excess risk"; this description is correct. They also write that "heavy drinking was associated with a significant 1.7-fold ... excess in risk of death from stroke"; the excess is 70%.

In an earlier paper by the same authors in *JAMA* there were similar problems.<sup>2</sup> A relative risk of 1.6 was correctly described as a "60% greater risk," while a relative risk of 2.3 was incorrectly described as a "2.3-fold excess risk." The paper also stated that "rates in Shanghai Chinese were 2-fold to 8-fold higher than in Los Angeles whites," but the rates in Shanghai were actually two to eight times those in Los Angeles whites. Such problems were not confined to the interpretation of relative risks. The sentence "In China, the yearly per capita consumption of cigarettes has increased 3-fold between the 1950s and 1987, from about 500 to 1748" is incorrect; the increase is actually twofold or 200%. Peto et al, in an accompanying editorial, stated "that heart attack mortality is five times lower, and that stroke mortality is five times higher."<sup>3</sup> This was a problem because the authors were referring to the ratio of 5.3 (366/69) and 1:4.2 (48/201) respectively.

I have found similar problems in reports by American, British, and Chinese authors (in alphabetical order). This problem is important when relative risks or differences of two measures are described. We have to be cautious about the confusing meaning of the suffix "fold"; n-fold is equal to n times, and is equal to  $n \times 100\%$ . Therefore, a relative risk of 3.5 is 2.5-fold, or 2.5 times, or a 250% increase or excess in risk, not 3.5-fold or a 350% increase or excess. I wonder how long we will continue to see such inadvertent inflation of relative risks in the literature.

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## Study confirms results of systematic review of care in stroke unit

**EDITOR**—The paper from the Stroke Unit Trialists' Collaboration, based on a systematic review of randomised trials that compared organised inpatient stroke care with contemporary conventional care, suggested that management in an integrated stroke unit reduced the frequency of poor outcomes, including death, dependency, and requirement for institutional care.<sup>1</sup> We had the opportunity to gather data on outcome of stroke prospectively, in anticipation of the opening of a dedicated stroke unit in our newly constructed hospital. We have compared treatment in a conventional ward setting from April 1993 to the present with outcomes in the new stroke unit, which has been in operation from mid-1995 (table).

The data show that patients admitted to the stroke unit were more likely to survive, as noted by the analysis by the Stroke Unit Trialists' Collaboration. The reason for this is unclear. Case mix could have been an important factor,<sup>2</sup> but admission to our stroke unit was governed solely by the availability of beds, and analysis of data on length of stay (not shown) suggested that patients with minor strokes (with stays of less than 10 days) were almost twice as likely to be admitted to the general wards. In addition, the outcome for survivors (at least in terms of the probability of them being discharged home) was no different between the groups; there was no indication that the condition of patients admitted to the stroke unit suggested a more favourable prognosis. No patients were given specific treatments for stroke (for example, thrombolysis), although a number were included in the international stroke trial.<sup>3</sup> The only difference in treatment between patients in a ward and patients in the stroke unit was organisational; treatment of patients in the stroke unit was predetermined by an integrated care pathway, providing a seamless, sequential programme of nursing and paramedical input.

Although we found no significant reduction in length of stay, we can confirm that care in a stroke unit in a district general hos-

pital, with the services of a dedicated team with a special interest in the acute management and rehabilitation of patients with stroke, reduces mortality without a significant increase in morbidity or use of resources.

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- 1 Stroke Unit Trialists' Collaboration. Collaborative systematic review of the randomised trials of organised inpatient (stroke unit) care after stroke. *BMJ* 1997;314:1151-9. (19 April.)
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## Modern treatment for internal haemorrhoids

### Day surgery offers permanent cure

**EDITOR**—John L Pfenninger's editorial on modern treatments for haemorrhoids is misleading on many counts.<sup>1</sup> Recurrent symptoms are extremely common after outpatient treatment of haemorrhoids, and the usual outcome of this is multiple visits for persistent symptoms. Most studies of these treatments have had only short term follow up. The few studies with long term follow up have shown that non-surgical treatments provide short term relief of symptoms in only a small proportion of patients while often being unpleasant and poorly received by patients.<sup>2,3</sup> It is clear from published reports that the modern treatments advocated by Pfenninger require multiple sessions to achieve a small, short lived benefit. This in turn exposes the patient to the hazards and complications of these treatments, which are by no means trivial and can on rare occasions be life threatening.

Treatments such as cryotherapy and infrared coagulation are poorly evaluated and have been shown in controlled studies to be inferior to banding or surgery as long

term solutions.<sup>4</sup> They are also more expensive and less readily accepted by most practitioners. On the other hand, recurrence of symptoms is uncommon after an adequately performed haemorrhoidectomy—the most effective permanent cure for symptomatic haemorrhoids. Full three quadrant haemorrhoidectomy can be performed as a day case procedure as it is at our hospital and increasingly also at other units in the United Kingdom. This is a safe, effective, and economical long term treatment that has been prospectively evaluated and is well received by patients. Postoperative pain is easily addressed by a carefully designed perioperative care package, and complications are low when a careful operative technique is used and particularly when anal canal dressings are avoided. The advent of reversible chemical sphincterotomy using 0.2% glyceryl trinitrate cream adds to early comfort.

The development of a cost effective and successful programme of day surgery means that haemorrhoidectomy is now more appropriate than ever in the management of haemorrhoids, in direct contradiction to Pfenninger's editorial; all efforts should be concentrated on developing programmes for day case haemorrhoidectomy.

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## Rubber band ligation is effective and efficient

**EDITOR**—We agree with John L Pfenninger that effective outpatient treatment for internal haemorrhoids is available but we disagree with him about the best method of treatment.<sup>1</sup> Although Pfenninger suggests that infrared coagulation is the superior treatment on the basis of a meta-analysis from 1992,<sup>2</sup> a later meta-analysis concluded that rubber band ligation was better and that fewer treatments were needed than with infrared coagulation.<sup>3</sup> While rubber band ligation was much better than injection sclerotherapy in terms of both response and need for further treatment, infrared coagulation was not shown to have any advantage over injection sclerotherapy.

Our own experience favours the use of rubber band ligation. In a rapid access colorectal clinic, rubber band ligation has allowed one stop management of haemorrhoids on an outpatient basis without the need for multiple follow up appointments and without major complications. With modern suction applicators rubber band ligation can easily be achieved without an

Admission characteristics of and outcomes in patients with stroke admitted to general medical wards (from 1 April 1993 to 31 March 1997) or to stroke unit (from 1 April 1995 to 31 March 1997). Figures are numbers (percentages) of patients unless stated otherwise

	Patients admitted to ward	Patients admitted to stroke unit
<b>Admissions:</b>		
Total admitted	644	129
Men	302 (47)	57 (44)
Women	342 (53)	72 (56)
Mean age (range) (years)	75.4 (29-100)	76 (41-97)
Mean length of stay (days)	23	35
<b>Outcomes:</b>		
Death	277	37
Discharged home	294 (46*; 80†)	72 (56*; 78†)
Discharged to residential or hospital care	73 (11*; 20†)	20 (15*; 22†)

\*Percentage of all patients admitted; †percentage of patients discharged.

assistant to hold the anoscope, contrary to Pfenninger's remarks. We would recommend rubber band ligation as an effective and efficient treatment with advantages for both patients and the providers (and purchasers) of health care.

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- 1 Pfenninger JL. Modern treatments for internal haemorrhoids. *BMJ* 1997;314:1211-2. (26 April.)
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**Author's reply**

EDITOR—No single "best" treatment for all haemorrhoids has been documented. According to the United States Bureau of Health Statistics, the number of surgical haemorrhoidectomies performed in the United States declined from 165 000 in 1982 to 30 000 in 1994. Surgical haemorrhoidectomy is an effective treatment for grade IV internal haemorrhoids, but more conservative or outpatient treatment is generally indicated in lesser degree haemorrhoids, with excellent results.

The standards task force of the American Society of Colon and Rectal Surgeons states that surgery should be reserved for those who "fail more conservative measures" or who have "third and fourth degree haemorrhoids ... with severe symptoms."<sup>1</sup> Emin Carapeti and Robin K S Phillips claim that outpatient haemorrhoidectomy is "safe, effective, and economical ... and is well received by patients." All outpatient modalities discussed in my editorial have fewer complications and are cheaper than surgical excision. No time off from work is required for day case procedures, which must be considered a definite economic advantage. MacRae and McLeod's meta-analysis shows surgical haemorrhoidectomy to be associated with the most pain and cost.<sup>2</sup> Carapeti and Phillips quote studies on sclerotherapy and cryotherapy and state that outpatient modalities are "poorly received" by patients and lack efficacy. That is precisely why my editorial did not advocate them.

Infrared coagulation, bipolar coagulation, and rubber band ligation are the accepted modern techniques. Rubber band ligation is generally associated with more bleeding and pain. It may take fewer treatments and be more efficacious; however, more surgical skill is needed. Sepsis and deaths have also been reported.<sup>3</sup> Internal haemorrhoids near the dentate line may be more difficult to band without causing pain, and flat bleeding haemorrhoids may not provide enough tissue to grasp. No life threatening complications have been reported with infrared coagulation or bipolar coagulation.

Johanson and Rimm's meta-analysis concluded that infrared coagulation is the

optimal non-operative treatment.<sup>4</sup> MacRae and McLeod's meta-analysis concluded that rubber band ligation is the initial treatment of choice for grade I-III internal haemorrhoids, but they also comment on the relatively low numbers of randomised trials evaluating haemorrhoidal treatment.<sup>2</sup>

My conclusions after using all three modalities are that 30-40% of patients who present with haemorrhoids can be treated medically, while 50-60% will require an outpatient procedure. Infrared coagulation and bipolar coagulation are well received by patients who get relief from their symptoms. For the 10% of patients in whom outpatient modalities fail or who present with grade IV haemorrhoids, surgical intervention will be necessary.

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- 1 Standards Task Force of American Society of Colon and Rectal Surgeons. Practice parameters for the treatment of hemorrhoids. *Dis Colon Rectum* 1993;36:1118-20.
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**Author preferred to cite substantive references rather than meeting abstracts**

EDITOR—I have received an unusual number of letters and telephone calls praising the style, content, and message of my article on recent advances in cardiac surgery; the article questioned the wisdom of using the popular media for the immediate publication of surgical advances.<sup>1</sup> While I was hurt by the letter from Gianni Angelini and Peter Wilde commenting on the article, and was concerned by its distortions, I write only to put the record straight on the allegation of failure to quote published work.<sup>2</sup> I referred to an article in the *Lancet* dated 22 March, entitled "Batista procedure proves its value in the USA,"<sup>3</sup> which opens with the data from Cleveland and Japan presented on 17 March this year. I chose to cite it rather than meeting abstracts. My paper was submitted soon afterwards, on 4 April, and was peer reviewed and revised; the corrected proofs were returned to the *BMJ* on 22 June. I had chosen a published article from *Nature Medicine* for general background.<sup>4</sup> The brief article from Bristol published in the *British Journal of Hospital Medicine* appeared in the meantime, after my piece had been submitted, and as it contained no outcome data or new information I preferred to retain the more substantive reference to *Nature Medicine*. The letter to the *Lancet* from Bristol (which Angelini and Wilde themselves cite as "in press") appeared over a month after my article was published, 15 months after their television appearance and newspaper

reports, and was certainly not available to me before my article went to the printers. I hope that the feisty letter from my Bristol colleagues will at least have given readers a second opportunity to find my article. I would be pleased to send out a reprint in response to any request.

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**Effect of deprivation on general practitioners' referral rates**

**Study should have used deprivation index that is independent of age**

EDITOR—There is a fatal flaw in the objective of the study by Julia Hippisley-Cox and colleagues: the Jarman underprivileged area (UPA(8)) score is used as a proxy measure to determine the effect of deprivation on variations in the outpatient referral rates of general practitioners.<sup>1</sup> It is hardly surprising that there is a correlation between the Jarman score and referral rates; the Jarman score was devised specifically to predict workloads in primary care.<sup>2,3</sup>

The Jarman index included as one of its factors the proportion of elderly people living alone, which correlates strongly with the proportion of elderly people in a population. Indeed, the category of elderly people living alone was given the strongest weighting by general practitioners because they knew that the population of elderly people was the major factor which would influence their workload. It is equally well known that age has a strong influence on referral rates. Elderly people are far more likely than young people to be referred to hospital; this is the reason for the age-cost curve in the national resource allocation formula.<sup>4</sup>

The fact that the Jarman index correlates with referrals by general practitioners is a statement of the obvious—it confirms that the index measures what it claims to measure. Practices with high Jarman scores will almost certainly have a high proportion of elderly people and therefore have high rates of referral. To claim that this correlation shows a link with social deprivation is to miss the point. The age structure of a population is not an indicator of deprivation, and many of the most deprived practice populations, such as those found in the inner cities, will have comparatively small proportions of elderly people.

This study fails completely to examine the effect of deprivation on referral rates. Instead it shows that an index designed to predict the workload of general practitioners successfully predicts the referral rates of

general practitioners; this is self evident. To properly investigate the link between social deprivation and referral rates the study should have standardised referral rates for the practice population and then examined the correlation with an index of deprivation that was independent of age.

We hope that the results of this flawed study will not be used as evidence of a link between social deprivation and general practitioners' referral rates.

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1 Hippisley-Cox J, Hardy C, Pringle M, Fielding K, Carlisle R, Chilvers C. The effect of deprivation on variations in general practitioners' referral rates: a cross sectional study of computerised data on new medical and surgical outpatient referrals in Nottinghamshire. *BMJ* 1997;314: 1458-61. (17 May.)

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4 Health Committee. *Second report of session 1995-6. Allocation of resources to health authorities*. London: HMSO, 1996.

### Jarman score measures workload not deprivation

EDITOR—In their article on general practitioners' referral rates<sup>1</sup> Julia Hippisley-Cox and colleagues used one of our papers as a reference, though they do not seem to have read it very carefully.<sup>2</sup> We did not say that data from the census were poor predictors of variations in prescribing. What we said was that the process of assigning data from the census to practice populations does not work well because of the non-systematic way in which patients from a given enumeration district assign themselves to practices. This problem affects all aspects of general practice, not just prescribing.

The other point with which we would take issue is the description of the Jarman underprivileged area (UPA(8)) score as a measure of deprivation. It is and always has been a measure of workload. Given this fact it is hardly surprising that it is correlated with referrals.

**Glen Scrivener** *Data analyst*

**David Lloyd** *Applied statistician*

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1 Hippisley-Cox J, Hardy C, Pringle M, Fielding K, Carlisle R, Chilvers C. The effect of deprivation on variations in general practitioners' referral rates: a cross sectional study of computerised data on new medical and surgical outpatient referrals in Nottinghamshire. *BMJ* 1997;314: 1458-61. (17 May.)

2 Scrivener G, Lloyd DCEF. Allocating census data to general practice populations: implications for study of prescribing variation at practice level. *BMJ* 1995;311:163-5.

### Analyses should take age and sex into account

EDITOR—Julia Hippisley-Cox and colleagues found an association between deprivation and referral rates for patients of all ages in 183 general practices in Nottinghamshire in 1993.<sup>1</sup> The Jarman underprivileged area (UPA(8)) score was their only measure of deprivation. The UPA(8) score is a proxy for

deprivation, but it may also be regarded as a proxy for practice workload.<sup>2</sup>

We used an analogous method to investigate general practitioners' rates of referral for children up to age 16 in east London, including a wider range of practice and population characteristics. We analysed data from the integrated district and regional information system for 164 practices for one year (1995-6), including information on non-attendances and cancellations. The total number of referrals analysed was 23 467. Our findings confirm weak, yet significant, associations between the UPA(8) score, social class, overcrowding, and the overall paediatric referral rate; neither fundholding status nor singlehanded practice was related (table). Practices in better premises (as defined by a 1995 Health Authority premises audit) and those employing a practice manager had significantly higher referral rates; no other practice factors were associated. Our findings are based on exploratory univariate analysis and further multivariate analysis is necessary. The weakness of the associations may reflect the socioeconomic structure of the practice populations in east London, where deprivation is uniformly widespread, making contrasts difficult to capture. Also, the derivation of the sociological variables by proportional allocation of census data at ward level to the postcode distribution of practice lists gives only an approximation of the socioeconomic characteristics of populations.<sup>3 4</sup>

We accept that there is an important relation between deprivation and overall referral rates, and our results also suggest that different age and sex groups may have differing needs and determinants for referral.<sup>5</sup> For example, multiple linear regression

analysis of the same data by age and sex showed that 21% of the variability in the logarithm of referral rates of boys under 1 year old related to a lack of hours for health visiting in practices (accounting for 18% of the variance; constant=5.88; B= -0.006 (SE=0.001); P<0.001) and the proportion of children living in overcrowded circumstances (accounting for 3% of the variance; constant=5.88; B=0.014 (SE=0.007); P=0.035). Broad studies at practice level may mask associations; we strongly advocate more detailed analyses by age and sex that give greater consideration to practice and population factors influencing referrals rates so that areas for needs assessment can be highlighted.

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**Filomena Pereira** *Lecturer*

**Sally Hull** *Senior lecturer*

**Yvonne Carter** *Professor*

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1 Hippisley-Cox J, Hardy C, Pringle M, Fielding K, Carlisle R, Chilvers C. The effect of deprivation on variations in general practitioners' referral rates: a cross sectional study of computerised data on new medical and surgical outpatient referrals in Nottinghamshire. *BMJ* 1997;314: 1458-61. (17 May.)

2 Jarman B. Identification of underprivileged areas. *BMJ* 1983;286:1705-9.

3 Majeed FA, Cook DG, Polonicki J, Griffiths J, Stones C. Sociodemographic variables for general practices: use of census data. *BMJ* 1995;310:1373-4.

4 Carr-Hill RA, Rice N, Roland M. Socioeconomic determinants of rates of consultation in general practice based on fourth national morbidity survey of general practices. *BMJ* 1996;312:1008-13.

5 Armstrong D, Britten N, Grace J. Measuring general practitioner referrals: patient, workload, and list size effects. *J R Coll Gen Pract* 1988;38:494-7.

### Univariate analysis of logarithm of referral rates in paediatric practice for children ages 0 to 16 years and practice factors

Variable (No of practices for which there was information)	Adjusted R <sup>2</sup> (%)	Constant	B coefficient(SE)	P value
UPA(8) score (n=150)	3.7	1.99	0.004 (0.002)	0.01*
Singlehanded GP† (n=157)	-0.5	2.15	0.01 (0.02)	0.6
Fundholder‡ (n=157)	1.76	2.17	-0.04 (0.02)	0.053
List size per principal (n=157)	-0.6	2.15	5.37×10 <sup>-8</sup> (1.05×10 <sup>-5</sup> )	0.99
Employment of practice nurse (n=155)	-0.57	2.15	0.008 (0.023)	0.73
Approval for child health surveillance (n=157)	-0.58	2.16	-0.006 (0.02)	0.77
Health visitor hours per week per 1000 children under 5 years (n=155)	-0.65	2.15	-5.1×10 <sup>-6</sup> (1.3×10 <sup>-4</sup> )	0.97
Employment of practice manager (n=155)	2.68	2.12	0.046 (0.02)	0.02*
Presence of GP trainer (n=157)	1.17	2.15	0.051 (0.03)	0.09
Surgery premise category (n=156)	4.42	2.23	-0.02 (0.007)	0.005*
Asthma prophylaxis/bronchodilator prescribing ratio (n=157)	0.58	2.10	0.114 (0.08)	0.17
% of economically active residents aged 16 and over who are unemployed (n=153)	0.26	2.06	0.005 (0.004)	0.24
% of residents reporting black ethnic group (n=150)	2.76	2.21	-0.004 (0.002)	0.02*
% of residents reporting Asian ethnic group (n=150)	-0.67	2.16	3.6×10 <sup>-5</sup> (7.9×10 <sup>-4</sup> )	0.96
% of residents who are lone parents aged 16 and over who live alone (n=150)	-0.57	2.17	-0.002 (0.005)	0.70
% of residents in households with an economically active head of household in socioeconomic group IV and V (n=150)§	4.38	1.98	0.007 (0.003)	0.006*
% of children living in overcrowded conditions (n=153)	4.8	2.06	0.003 (8.9×10 <sup>-4</sup> )	0.004*

UPA(8)=Jarman underprivileged area score; GP=general practitioner. \*Significant at >5%.

†Relative to baseline of practices with more than one doctor.

‡Relative to baseline of non-fundholding practices.

§These are lower socioeconomic groups.

**Authors' reply**

EDITOR—We are well aware of the origins of the Jarman underprivileged area (UPA(8)) score<sup>1</sup> and the ensuing debate regarding its validity and reliability<sup>2</sup> to which each of the three letters refers. For this reason, we had mentioned in our introduction that the UPA(8) score was used as a proxy measure for deprivation; none the less, the UPA(8) score forms the basis for the current deprivation payment system for general practitioners.

Ted Williams and colleagues suggest we reanalyse our data using an index of deprivation that is independent of age. The table shows the results using the Townsend score.<sup>3</sup> The referral rates have been adjusted for the practices' age-sex structure by including the percentage of men and women aged over 65 years in the regression equation. Using multivariate analysis with the same model as in our paper the Townsend score explained 27% and 30% of the variation in total and medical referral rates; this adds to the evidence that deprivation is related to referral rates.

Contrary to Williams and colleagues' assertion, practices with high UPA(8) scores do not have high proportions of elderly patients and this therefore cannot be the reason for the association between deprivation indices and high referral rates. We found a significant but weak correlation between the practice based UPA(8) score and the percentage of patients aged over 65 years ( $r = -0.16$ ;  $P = 0.04$ ).

Glen Scrivener and David Lloyd seem to have misread our paper in which we commented that "our results are in contrast, however, with other studies ... for example, prescribing [reference number 31 in our paper], number of consultations, and night visits [references 32 and 33]—showing that the UPA(8) score is a relatively poor predictor of variations and thought to be an inappropriate measure for healthcare planning and distribution of resources [references 34 to 43]." The first three references specifically relate to variations in prescribing, consulta-

tions, and night visits. The next nine references (which included the paper by Scrivener and Lloyd<sup>1</sup>) related to articles that had argued against the use of the UPA(8) scores for resource allocation for a variety of reasons.

We look forward to the results of multivariate analysis referred to by Patricia Sturdy and colleagues. They highlight the important issue of using more detailed data to identify the factors that influence referral rates so that areas for needs assessment can be highlighted. If this is followed by targeted intervention then we can start to reduce inequality and improve service provision rather than just describe it.

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**Mike Pringle** Professor of general practice  
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**Katherine Fielding** Lecturer in medical statistics  
**Clair Chilvers** Professor of epidemiology  
 Trent Institute for Health Services Research, Medical School, Queen's Medical Centre

- 1 Jarman B. Identification of underprivileged areas. *BMJ* 1983;286:1705-9.
- 2 Jarman B, Townsend P, Carstairs V. Deprivation indices. *BMJ* 1991;303:523.
- 3 Townsend P, Phillimore P, Beatie A. *Inequalities in health in the northern region*. Newcastle upon Tyne: Northern Regional Health Authority and University of Bristol, 1986.
- 4 Scrivener G, Lloyd DCEF Allocating census data to general practice populations: implications for study of prescribing variation at practice level. *BMJ* 1995;311:163-5.

**Mental health emergencies**

**Details of studies of zuclopenthixol acetate are needed**

EDITOR—The issue of mental health emergencies is rarely addressed. As we are currently working with the Cochrane Schizophrenia Group to produce a systematic review on the use of zuclopenthixol acetate for acutely disturbed people, we were interested to see that Zerrin Atakan and Teifion Davies recommend its use in certain

circumstances.<sup>1</sup> Despite our best efforts at searching electronic databases such as Biological Abstracts, the Cochrane Library, Embase, LILIACS, Medline, PsycLIT, and PSYINDEX and many conference proceedings, and contacting Lundbeck, we have found only three fully published controlled clinical trials from which data can be extracted.<sup>2-4</sup> The paucity of studies may be due to the great difficulty in carrying out trials of emergency treatment in psychiatry, but the fact that all three identified studies give equivocal results suggests that the recommendation of zuclopenthixol acetate in preference to other treatments may be premature. We would welcome any information that the authors or others may have on any controlled clinical trials, published or unpublished, relating to this issue.

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**Anthony David** Professor  
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- 1 Atakan Z, Davies T. ABC of mental health. Mental health emergencies. *BMJ* 1997;314:1740-2. (14 June.)
- 2 Bastrup PC, Alhfors UG, Bjerkenstedt L, Dencker SJ, Fensbo C, Gravem A, et al. A controlled Nordic multicentre study of zuclopenthixol acetate in oil solution, haloperidol and zuclopenthixol in the treatment of acute psychosis. *Acta Psychiatr Scand* 1993;87:48-58.
- 3 Bourdouxhe S, Mirel J, Denys W, Bobon D. L'acetate de zuclopenthixol et l'haloperidol dans la psychose aigue. Resultats du sous-groupe AMDP d'une etude multicentrique belge. [Zuclopenthixol acetate and haloperidol in acute psychoses. Results of the AMDP subgroup in a multicentre Belgian study.] *Acta Psychiatr Belg* 1987;87:236-44.
- 4 Chouinard G, Safadi G, Beauclair L. A double-blind controlled study of intramuscular zuclopenthixol acetate and liquid oral haloperidol in the treatment of schizophrenic patients with acute exacerbation. *J Clin Psychopharmacol* 1994;14:377-84.

**Patients need to be monitored when given rapid tranquillisation**

EDITOR—Zerrin Atakan and Teifion Davies have a box listing precautions with rapid tranquillisation in their article in the ABC of mental health.<sup>1</sup> With the use of the doses suggested, some patients will be nearly anaesthetised, and certainly heavily sedated. There are now stringent guidelines on using intravenous sedation for diagnostic and therapeutic procedures (for example, endoscopy), which include continuous monitoring of oxygen saturation and ideally the presence at all times of a trained anaesthetist. I would suggest that the degree of respiratory depression and obtundation of the airway is greater in some sedated acutely disturbed patients than it is in those having an endoscopy under sedation.

Should we not be applying the same rigid criteria in both instances? Most anaesthetic departments have a good working relationship with their psychiatric colleagues due to the running of lists for electroconvulsive therapy, so should not perhaps the services of the anaesthetists be called on if intense sedation is required? It is not enough merely to state that resuscitation equipment is available if no one knows how to use it properly.

Multivariate associations for total, medical, and surgical referral rates according to characteristics of practice, including Townsend score

Variable	Adjusted B coefficient (95% CI)	P value
<b>Total referral rates*</b>		
Townsend score	9.00 (6.25 to 11.76)	<0.0001
Singlehanded GP†	26.40 (7.20 to 45.60)	0.007
Fundholder‡	-29.62 (-55.97 to -3.26)	0.03
Adjusted R <sup>2</sup> =26.8%; constant=175.49; F=13.46; 2 df; P<0.0001		
<b>Medical referral rates*</b>		
Townsend score	10.26 (7.45 to 13.08)	<0.0001
Singlehanded GP†	21.15 (1.54 to 49.76)	0.03
Fundholder‡	-32.07 (-58.66 to 5.48)	0.02
Adjusted R <sup>2</sup> =29.9%; constant = 99.07; F=15.17 ; 2 df; P<0.0001		
<b>Surgical referral rates*</b>		
Townsend score	-1.02 (-2.53 to 0.49)	0.18
Singlehanded GP†	6.40 (-4.10 to 16.9)	0.23
Fundholder‡	2.20 (-12.05 to 16.44)	0.76
Adjusted R <sup>2</sup> =0.004%; constant=77.98; F=1.13; 2 df; P=0.34		

GP=general practitioner.  
 \*Including percentage of women and men aged 65 years registered with each practice.  
 †Relative to baseline of practices with more than one doctor.  
 ‡Relative to baseline of non-fundholding practices.

Finally, the authors suggest using a butterfly cannula in a large vein to give the sedatives. These cannulas may be easier to insert than flexible Teflon cannulas, but they also have a well known propensity for "cutting out" of the vein at crucial moments—for example, when life saving drugs are being given.

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1 Atakan Z, Davies T. ABC of mental health. Mental health emergencies. *BMJ* 1997;314:1740-2. (14 June.)

### Zuclopenthixol acetate is given by intramuscular injection, not intravenously

EDITOR—The article on mental health emergencies contains a flow chart for rapid tranquillisation of acutely disturbed patients.<sup>1</sup> I was extremely concerned, however, to see the recommendation to give zuclopenthixol acetate 100-150 mg intravenously. This preparation of zuclopenthixol has been designed for, and indeed is only licensed for, deep intramuscular injection.<sup>2</sup> Were it given intravenously at this dose it would be extremely dangerous to the patient, with a high risk of cardiac arrhythmia, profound hypotension, severe dystonic reaction, and many other adverse effects.

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1 Atakan Z, Davies T. ABC of mental health. Mental health emergencies. *BMJ* 1997;314:1740-2. (14 June.)

2 BMA and Royal Pharmaceutical Society of Great Britain. *British national formulary No 33*. London: BMA and RPSGB, 1997:168.

### Authors' reply

EDITOR—Our article on mental health emergencies was necessarily brief, and its advice, particularly on treatment, was simplified for clarity. The correspondents raise several broader issues. We agree with Evandro Coutinho and colleagues that there are few studies of zuclopenthixol acetate in emergency situations and that it may be difficult to pinpoint a rapid tranquillisation regimen that is clearly superior to all others. Thus we restricted our advice to drug regimens that we find effective as first line treatment in emergencies.

Ed Walker points to the potential dangers of rapid tranquillisation. It must be remembered that, as we pointed out, this is an emergency treatment "to control potentially destructive behaviour"; it is to be used only when other treatments are unlikely to be effective and when the anticipated benefits outweigh the expected risks. All psychiatric units should operate guidelines stating in what circumstances rapid tranquillisation may be used and by whom, and the precautions to be taken. The involvement of anaesthetists in these procedures would be welcomed by many psychiatrists but could present huge logistic difficulties since acute psychiatric units are often situated some distance from the nearest general hospital.

We thank Robert Orange-Bromehead for drawing attention to the error introduced into the flow chart on rapid tranquillisation when the artwork was being prepared by the *BMJ*. As Orange-Bromehead notes, zuclopenthixol acetate is intended for deep intramuscular injection, and intravenous administration is potentially dangerous.

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### Caution is needed with rapid tranquillisation protocol

EDITOR—We would add a note of caution to Zerrin Atakan and Teifion Davies's article on mental health emergencies.<sup>1</sup> They discuss the use of pharmacotherapy for rapid tranquillisation for disturbed and violent behaviour and give an algorithm for this, which is a simplified version of one developed in our trust. We have recently reviewed the treatment of acute disturbed and violent behaviour, including its causes, the context in which it occurs, and use of this algorithm.<sup>2</sup> We found that there are few controlled studies of treatment, pharmacological or otherwise, in this situation. We warn that the algorithm represents simply an attempt to synthesise local clinical practice and experience (which is known to be generally idiosyncratic<sup>3</sup>), a review of pharmacological and pharmacokinetic data, and careful consideration of issues relating to patients' safety. Thus its use constitutes an attempt at good clinical practice given limited background information, rather than evidence based practice as such.

In considering contextual issues, we also emphasise that rapid tranquillisation should be used only during an attempt at considered assessment (and treatment if possible) of underlying causes, after a trial (if possible) of non-pharmacological methods and, preferably, with a protocol to avoid inappropriate or counterproductive interventions. Such interventions may also occur when responses are due to pressure on junior staff to act, essentially as a result of "institutional anxiety," which may in some cases be extreme.<sup>4</sup> The importance of a protocol therefore partly lies in relieving stress on junior staff, both medical and nursing, in what is often already a highly stressful situation.

Finally, we note that a further, valid, reason for rapid tranquillisation, in addition to treatment of disturbed or violent behaviour, is relief of distressing symptoms for the patient.

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1 Atakan Z, Davies T. ABC of mental health. Mental health emergencies. *BMJ* 1997;314:1740-2. (14 June.)

2 Kerr IB, Taylor DM. Acute disturbed or violent behaviour: principles of treatment. *J Psychopharmacol* 1997;11:271-7.

3 Pilowsky LS, Ring H, Shine PJ, Battersby M, Lader M. Rapid tranquillisation: a survey of emergency prescribing in a general psychiatric hospital. *Br J Psychiatry* 1992;160:831-5.

4 Lyth Menzies I. *Containing anxiety in institutions*. London: Free Association Books, 1988.

### Data on results of using different antiepileptic drugs do exist

EDITOR—Anthony Marson and colleagues have challenged me to produce "hard empirical evidence" in support of a mechanistic hypothesis in the management of epilepsy.<sup>1</sup> The best data come from a recently presented, as yet unpublished, double blind substitution study.<sup>2</sup> In this, 7% of 215 patients reporting a median of five partial seizures a month when taking monotherapy with carbamazepine (a sodium channel blocker) were seizure free when vigabatrin (a  $\gamma$ -aminobutyric acid (GABA) transaminase inhibitor) or sodium valproate (a sodium channel blocker with GABA-ergic properties) was substituted. A further 14% were seizure free when taking the combination of carbamazepine and vigabatrin or valproate. Less direct but still valid is the observation that combining lamotrigine with carbamazepine and phenytoin (all sodium channel blockers) produced reductions in seizures of 43% and 34% respectively, while the combination of lamotrigine and sodium valproate resulted in 83% fewer seizures.<sup>3</sup> My "star system," which Marson and colleagues referenced, compares the advantages and disadvantages of established and newer antiepileptic drugs by taking into consideration not just recent trial data but also ease of administration, side effect profiles, propensity for interactions, teratogenesis, etc.<sup>4</sup> Until results from large pragmatic randomised studies become available, this approach is likely to prove more useful to prescribers in choosing treatment for individual patients than is a meta-analysis of add-on placebo controlled trials in refractory epilepsy which showed (not surprisingly) no significant difference among the drugs.<sup>5</sup> Whether this is better than nothing is debatable. The evidence base underpinning the management of epilepsy must be clinically relevant as well as scientifically credible. The final product of a mechanistic approach would be welcomed by the many clinicians treating this common condition. Sufficiently robust data are now available for epilepsy specialists to be encouraged down this avenue.

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1 Marson A, Kadir Z, Chadwick D. Large pragmatic randomised studies of new antiepileptic drugs are needed. *BMJ* 1997;314:1764. (14 June.)

2 Brodie MJ, Roy KB. One drug or two? A double-blind comparison of adjuvant vigabatrin and valproate in carbamazepine-resistant epilepsy. *Epilepsia* 1996;37 (suppl 5):170.

3 Brodie MJ, Yuen AWC, and the 105 study group. Lamotrigine substitution study: evidence for synergism with sodium valproate? *Epilepsy Res* 1997;26:423-32.

4 Brodie MJ. Lamotrigine versus other antiepileptic drugs: a star rating system is born. *Epilepsia* 1994;35 (suppl 5):S41-6.

5 Marson AG, Kadir AZ, Chadwick DW. New antiepileptic drugs: a systematic review of their efficacy and tolerability. *BMJ* 1996;313:1169-74.

## Accident and emergency departments should not be considered places of safety

EDITOR—We agree with Amanda Ramirez and Allan House that in accident and emergency departments “the proportion of attendees with psychiatric problems is greatly increased if the accident and emergency department is a ‘place of safety’ to which the police may bring a patient who seems to be suffering from a mental disorder.”<sup>1</sup> However, we would like to highlight one of the recommendations made in a report of the joint working party of the Royal College of Psychiatrists and the British Association for Accident and Emergency Medicine. This stated that accident and emergency departments are not usually appropriately equipped and staffed to supervise patients under section 136 of the Mental Health Act.<sup>2</sup> Accident and emergency departments have open access and are therefore insecure. Staff are frequently occupied by emergencies and cannot provide the level of supervision required for these patients. Accordingly, we believe that it is more appropriate for such patients to be taken to a police station, where a police surgeon can be requested to carry out a mental health assessment, or to an acute psychiatric hospital, where a psychiatrist can carry out an assessment. We would encourage staff of accident and emergency departments to consult with their local police, as we have, to clarify such arrangements.

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1 Ramirez A, House A. Common mental health problems in hospital. *BMJ* 1997;314:1679-81. (7 June.)

2 Royal College of Psychiatrists. *Report of a joint working party of the Royal College of Psychiatrists and the British Association for Accident and Emergency Medicine*. London: RCP, 1996. (Council report CR43.)

## Neonatal mortality of low birthweight babies is increased in mothers with higher education

EDITOR—Rolv Skjærven and colleagues reported an interesting and paradoxical finding: among small babies born to mothers whose own birth weight had been greater than 2000 g, perinatal mortality increased with maternal birth weight.<sup>1</sup> As

the birth weights of a mother and her baby are positively correlated, and birth weight is inversely related to perinatal mortality, this is against one's expectation.

We report a similar paradoxical finding, which may be related to this observation. We have examined the socioeconomic gradient in neonatal mortality in the Czech Republic, using information on all 380 633 singleton live births in 1989-91. Individual data from the national birth register were linked with the national death register for 1989-92 by a unique personal identification number.

Neonatal mortality clearly fell and birth weight clearly increased with maternal education (table). When the data were analysed separately by the birth weight of the babies, however, the inverse educational gradient was present only among babies with birth weight of  $\geq 2500$  g. Among smaller babies the gradient was reversed: small babies of mothers with the lowest education had the lowest risk of neonatal death. A similarly paradoxical finding has been reported in the United States for neonatal mortality in black and white babies.<sup>2</sup> These observations may be analogical to the Norwegian findings and may suggest that low birth weight relative to “predicted” values (by maternal birth weight, education, or race) is associated with an increased risk of death. They also provide some support for Skjærven and colleagues, who suggested that additional criteria of what is “relatively small” at birth may be useful.

We offer another potential explanation, in addition to those listed by Skjærven and colleagues. Among more privileged mothers low birth weight may be more likely to reflect serious intrauterine damage, whereas among less privileged mothers low birth weight may more often include “less serious” growth impairments (resulting, for example, from less severe nutritional deficiencies) that do not lead to death. The biological mechanisms, however, are not clear.

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1 Skjærven R, Wilcox AJ, Øyen N, Magnus P. Mothers' birth weight and survival of their offspring: population based study. *BMJ* 1997;314:1376-80. (10 May.)

2 Buehler JW, Strauss LT, Hogue CJ, Smith JC. Birth weight-specific causes of infant mortality, United States, 1980. *Public Health Rep* 1987;102:162-71.

Neonatal mortality/1000 live births in Czech Republic 1989-91, by maternal education and babies' birth weight (relative risk of death in parentheses)

Mothers' education	Mean birth weight (g)	All children (n=2376)	Neonatal mortality		
			Children with birth weight (g):		
			$\leq 1500$ (n=1110)	1500-2499 (n=606)	$\geq 2500$ (n=660)
Primary	3165	9.0 (1.0)	348 (1.0)	23.8 (1.0)	2.5 (1.0)
Vocational	3308	6.1 (0.67)	402 (1.15)	33.2 (1.39)	1.8 (0.72)
Secondary	3350	5.5 (0.61)	414 (1.19)	39.9 (1.68)	1.7 (0.68)
University	3371	4.8 (0.53)	397 (1.14)	33.3 (1.40)	1.3 (0.52)
Total	3310	6.1	392	32.7	1.8

## Acupuncturists have begun initiative to agree standards to improve safety

EDITOR—The three letters in response to our editorial about acupuncture claimed that safety standards for acupuncture are already in place.<sup>1 2</sup> We recognise the progress that the British Acupuncture Council has made in this, yet this body probably represents only a quarter to a third of the acupuncture profession in Britain. Our aim, and that of the Department of Health, is to encourage all acupuncturists to agree an acceptable standard for the benefit of the public. We are pleased to report that other senior members of the British Acupuncture Council have supported our position, and an initiative is already under way.

John Hicks and colleagues criticise the phrase “overoptimistic prognoses naively based on the theory of Chi.”<sup>1</sup> Perhaps they thought we were condemning the theory of Chi. What we were criticising was people who use traditional Chinese medicine to the exclusion of Western medicine; surely they would agree that treating meningitis by acupuncture<sup>3</sup> in the age of antibiotics is dangerously simplistic.

**E Ernst** *Professor*

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1 Safety in acupuncture [letters]. *BMJ* 1997;315:429-30. (16 August.)

2 Ernst E, White A. Acupuncture: safety first. *BMJ* 1997;314:1362. (10 May.)

3 Maciocia G. *The foundations of Chinese medicine*. Edinburgh: Churchill Livingstone, 1989:221.

## Caller's telephone number can be permanently withheld

EDITOR—I would like to bring a recent experience to the attention of British doctors who might be lagging behind with BT's technology. A patient tried to contact me while I was at home. Naturally, the hospital refused to give my home telephone number but rang me requesting that I phone the patient. I immediately did so, neglecting to prefix his number with 141. Later that day my son was upset to answer the phone to the same patient, who had presumably traced my home number by dialling 1471, which reveals the number of the last caller; had I prefixed the number with 141 my number would have been withheld.

After this, I contacted BT (on 0800 661441) and arranged that my number is permanently blocked unless I specifically unblock it by prefixing a call with 1470. I would recommend this simple procedure to others.

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