Letters

Outcome of pregnancy in women with insulin dependent diabetes

Centralisation of care leads to better outcome

EDITOR—The finding of a persistently poor outcome of pregnancy in women with insulin dependent diabetes in two (northern) English regions is an important statement of the problem.[1 2] Both studies provide figures and show outcomes that are no different from those widely reported in the past. Unfortunately, neither give evidence of any degree of centralisation of obstetric or diabetic care, with on-site neonatal intensive care, although this is a proved means of improving the outcome of pregnancy for diabetic mothers.[3] The St Vincent declaration guidelines on the outcome of pregnancy, referred to in the accompanying editorial (p 263), are based on the Scandinavian reports held up as examples of good practice and state that "an interdisciplinary team should provide centralized diabetic pregnancy care in a hospital treating at least 20-30 cases a year. Pregnant diabetic patients should regularly visit the centre, before, during, and after the pregnancy."

The combined diabetes pregnancy clinic at the Royal Maternity Hospital in Belfast has existed for over 40 years, and outcome audit has shown the value of this approach.[4] In an audit of over 800 pregnancies in diabetic mothers identified in Northern Ireland over the past 10 years the perinatal mortality in mothers cared for throughout at this centralised clinic during 1985-95 was 27/1000, compared with 70/1000 for patients referred later in pregnancy and 33/1000 for those mothers cared for in other maternity hospitals in Northern Ireland.

Overall perinatal mortality for the whole population of Northern Ireland (1.5 million) during this decade was 9.3/1000 total births, so that even at a centralised clinic there is still an increased risk in diabetic pregnancy. Centralisation of care and improved co-operation among the obstetricians and diabetes physicians within a health region will lead to a better outcome.[5]

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1 Casson IF, Clarke CA, Howard CV, McKendrick O, Pennycook S, Pharoah POD, et al. Outcomes of pregnancy in insulin dependent diabetic women: results of a five year population cohort study. *BMJ* 1997;315:275-8. (2 August.)
2 Hawthorne G, Robson S, Ryall EA, Sen D, Roberts SH, Ward Platt MP. Prospective population based survey of outcome of pregnancy in diabetic women: results of the Northern Diabetic Pregnancy Audit, 1994. *BMJ* 1997;315:279-81. (2 August.)
3 Harley JMG, Montgomery DAD. Management of pregnancy complicated by diabetes. *BMJ* 1965;i:14-6.
4 Traub AI, Harley JMG, Cooper TK, Maguiness S, Hadden DR. Is centralized hospital care necessary for all insulin-dependent pregnant diabetics? *Br Obstet Gynaecol* 1987;94:957-62.
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Rate of congenital malformations is almost certainly gross underestimate

EDITOR—Casson et al make several misrepresentations in their paper on the outcomes of pregnancy in women with insulin dependent diabetes.[1] In the United Kingdom the confidential inquiry into the outcome in babies of diabetic mothers was conducted over the period 1979-80 and included Scotland and Northern Ireland. Thus the perinatal mortality was for the whole of the United Kingdom. The Office for National Statistics (formerly the Office of Population Censuses and Surveys) provides data for only England and Wales. When data for Scotland and Northern Ireland were added the population perinatal mortality was 14.9/1000 for 1979-80.

The rate of congenital malformations available from the Office for National Statistics is almost certainly a gross underestimate, since reporting is voluntary. During 1960-77 the Liverpool and Bootle Congenital Abnormalities Registry (Eurocat) surveyed for congenital malformations and detected 7580 malformed babies (3.2%) out of a population of 236 443; for 1981 the Office of Population Censuses and Surveys gave a figure of 2.1%. The congenital malformation rate for the current study was 9.7%, a figure not very different from that found in the Rigshospitalet in Copenhagen up to 1978 (7.6%)[2] or the United Kingdom study for 1979-80 (7.1%).

The definition of a malformation is difficult, especially when it is minor and causes minimal interference with the individual's life. When malformations that alter quality of life, require corrective surgery, or cause death are considered, comparisons between different series show that the malformation rate has not changed either in the United Kingdom or in other European countries.[3–5]

While the malformation rate among babies of mothers who had been diagnosed as diabetic before becoming pregnant remains high, it probably is not 10 times higher than that among babies in our background population. The United Kingdom, unlike the Scandinavian countries, has no obligatory reporting system for babies of diabetic mothers, and this makes analysis and examination of trends extremely difficult. Thus in the United Kingdom it is difficult to assess whether the targets set out in the St Vincent declaration are being approached, let alone met.

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1 Casson IF, Clarke CA, Howard CV, McKendrick O, Pennycook S, Pharoah POD, et al. Outcomes of pregnancy in insulin dependent diabetic women: results of a five year population cohort study. *BMJ* 1997;315:275-8. (2 August.)
2 Molsted-Pedersen L. Pregnancy and diabetes, a survey. *Acta Endocrinol* 1980;94(suppl 238):13-9.
3 Hanson U, Persson B. Outcome of pregnancies complicated by type 1 insulin-dependent diabetes in Sweden. *Am J Perinatol* 1993;10:330-5.
4 Nielsen GL, Nielsen PH. Outcome of 328 pregnancies in 205 women with insulin-dependent diabetes mellitus in the county of Northern Jutland from 1976-90. *Eur J Obstet Gynecol Reprod Biol* 1993;50:33-8.
5 Cnattingius S, Berne C, Nordstrom ML. Pregnancy outcome and infant mortality in diabetic patients in Sweden. *Diabetic Med* 1994;11:696-700.

Advice to authors

We receive more letters than we can publish: we can currently accept only about one third. We prefer short letters that relate to articles published within the past four weeks. We also publish some "out of the blue" letters, which usually relate to matters of public policy.

When deciding which letters to publish we favour originality, assertions supported by data or by citation, and a clear prose style. Letters should have fewer than 400 words (please give a word count) and no more than five references (including one to the BMJ article to which they relate); references should be in the Vancouver style. We welcome pictures.

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.

We may post some letters submitted to us on the world wide web before we decide on publication in the paper version. We will assume that correspondents consent to this unless they specifically say no.

Letters will be edited and may be shortened.

Any improvement in metabolic control during pregnancy reduces risk of adverse fetal outcome

EDITOR—Unexpectedly high perinatal mortality and morbidity have been reported recently in studies of pregnant women with insulin dependent diabetes.[1 2] Metabolic control as measured by haemoglobin A_{1c} concentration was suboptimal, and better periconceptional regulation might have improved outcome. Neither of the studies, however, specifically addressed the predictive value of various levels of haemoglobin A_{1c} concentration on the outcome of pregnancy.

We have reported a similar outcome in pregnancies of women with insulin dependent diabetes,[3] but our data included haemoglobin A_{1c} concentrations before conception and during the first trimester for 60 and 171 pregnancies respectively. We performed logistic regression analyses, including haemoglobin A_{1c} values as predictor variables for adverse outcome (spontaneous abortion and lethal or severe malformations).[4] We observed a consistent increasing risk of adverse outcome with increasing haemoglobin A_{1c} concentration. The associations were most pronounced in the high range, but there was no indication of a threshold below which metabolic control seemed to be of minor importance.

Comparison of outcome in pregnancies in which metabolic control remained unchanged or deteriorated with that in pregnancies in which metabolic control improved yielded an odds ratio of an adverse outcome in the former group of 3.1 (95% confidence interval 0.99 to 9.6). When the outcome in pregnancies in which haemoglobin A_{1c} values remained above 7.9% was compared with that in pregnancies in which the values either changed from above to below 7.9% or remained under 7.9% the odds ratio of an adverse outcome increased to 3.8 (1.2 to 12).

Our data strongly indicate a clinically significant and consistent relation between haemoglobin A_{1c} concentration and adverse outcome in pregnancies of women with insulin dependent diabetes, without any indication of a cut off value below which further improvement in the haemoglobin A_{1c} concentration is of minor importance. Although our study may have been subject to confounding by indication, the message to clinicians and pregnant diabetic women seems clear: any improvement in metabolic control at any time during pregnancy seems to reduce the risk of adverse fetal outcome.

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1 Casson IF, Clarke CA, Howard CV, McKendrick O, Pennycook S, Pharoah POD, et al. Outcomes of pregnancy in insulin dependent diabetic women: results of a five year population cohort study. *BMJ* 1997;315:275-8. (2 August.)
2 Hawthorne G, Robson S, Ryall EA, Sen D, Roberts SH, Ward Platt MP. Prospective population based survey of outcome of pregnancy in diabetic women: results of the Northern Diabetic Pregnancy Audit, 1994. *BMJ* 1997;315:279-81. (2 August.)
3 Nielsen GL, Nielsen PH. Results of 312 pregnancies among white class B-F mothers in northern Jutland from 1976-1992. *Dan Med Bull* 1994;41:115-8.
4 Nielsen GL, Sørensen HT, Nielsen PH, Sabroe S, Olsen J. HbA1c in IDDM pregnancies as predictor of adverse fetal outcome. *Acta Diabetol* 1997;34:217-22.

Management of impaired glucose tolerance in pregnancy needs study

EDITOR—Casson et al report the rates of pregnancy loss and congenital malformation and measures of fetal growth in a population of insulin dependent diabetic women.[1] Hawthorne et al conclude that diabetic pregnancy remains a high risk state, with perinatal mortality and fetal malformation rates much higher than those in the background population despite intensive management of diabetes.[2]

In diabetic antenatal clinics most of the women seen are those with impaired glucose tolerance in pregnancy (gestational impaired glucose tolerance) rather than those with insulin dependent diabetes. This is an important group of women, whose management should also be evaluated. Abnormal glucose tolerance in pregnancy is widely reported to be associated with increased perinatal morbidity and major congenital abnormality.[3] Various management regimens have been advocated for women with impaired glucose tolerance which aim to return glucose control to normal and thereby improve perinatal outcome.

A randomised controlled study of the management of women with impaired glucose tolerance compared standard antenatal care with intensified care including dietary advice, capillary glucose monitoring, and serial ultrasonography.[4] Although the babies in the group receiving standard care had a significantly higher birth weight, this was offset by delivery occurring one week later than in the intensified care group. Thus this study, rather than supporting intensified antenatal care, did quite the opposite. It therefore seems that intensive management regimens for impaired glucose tolerance have been introduced prematurely.

A prospective randomised study has now been initiated at Bradford Royal Infirmary and the united Leeds teaching hospitals to test the hypothesis that managing women with impaired glucose tolerance without monitoring their glucose concentrations will not lead to a deterioration in perinatal outcome. This regimen is being compared with one in which impaired glucose tolerance is monitored and treated, with the aim of achieving normal plasma glucose concentrations. Fetal outcome in this study will be assessed by factors including length of stay on the special care baby unit, premature delivery, birth trauma, and number of capillary samples obtained. Measurements of maternal outcome include the incidence of caesarean section and induction of labour, number of antenatal visits, number of capillary samples obtained, and requirements for introduction of insulin.

We hope that this study will go some way towards answering the question of whether impaired glucose tolerance in pregnancy needs to be managed as aggressively as it tends to be and whether the St Vincent declaration applies to this group of patients as well as to those with insulin dependent diabetes mellitus.

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1 Casson IF, Clarke CA, Howard CV, McKendrick O, Pennycook S, Pharoah POD, et al. Outcomes of pregnancy in insulin dependent diabetic women: results of a five year population cohort study. *BMJ* 1997;315:275-8. (2 August.)
2 Hawthorne G, Robson S, Ryall EA, Sen D, Roberts SH, Ward Platt MP. Prospective population based survey of outcome of pregnancy in diabetic women: results of the Northern Diabetic Pregnancy Audit, 1994. *BMJ* 1997;315:279-81. (2 August.)
3 Hod M, Merlob P, Friedman S, Schoenfield A, Ovadia J. Gestational diabetes mellitus. A survey of perinatal complications in the 1980s. *Diabetes* 1991;40:74-8.
4 Li DF, Wong VCW, O'Hoy KMKY, Yeung CY, Ma HK. Is treatment needed for mild impairment of glucose tolerance in pregnancy? A randomised controlled trial. *Br J Obstet Gynaecol* 1987;94:851-4.

Authors' reply

EDITOR—Although the perinatal mortalities that Hadden and Traub cite show the benefit of centralised care for pregnant women with diabetes, the figure from the combined diabetes pregnancy clinic at the Royal Maternity Hospital in Belfast (2.7%) and that for other maternity hospitals in Northern Ireland (3.3%) both fall within the 95% confidence interval for our study (1.7% to 5.5%). Our study included some hospitals that ran combined clinics and others that did not. The geographical distribution of these hospitals is such that it would be logistically difficult for all women in the area to be cared for at a combined clinic with the criteria for experience quoted by Hadden and Traub.

We fully acknowledge Lowy's point, that data on congenital malformations from the Office for National Statistics are underestimates, but, as discussed in our paper, these are the only contemporary figures available for comparison. For our study we used the criteria given by the Office for National Statistics to exclude minor malformations from the analysis.

Nielsen et al draw attention to the possibility of using haemoglobin A_{1c} concentrations as predictors of the outcome of pregnancy in diabetic women. Information on diabetic control, treatment regimens, and complications of diabetes was collected as part of our study, and these data will form the basis of further analyses.

Rogerson and Bancroft make the point that infants of women with impaired glucose tolerance during pregnancy may also be at increased risk of poor outcome. We look forward to hearing the outcome of the

study they describe and the contribution it will make to evidence based practice in this area.

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Measures are needed to allow elderly inpatients to vote in general elections

EDITOR—Smith and Humphreys expressed concern over the eligibility of patients detained under the Mental Health Act to vote.[1] We have similar worries about elderly patients losing their opportunity to vote.

We interviewed 248 patients (87% of the total geriatric inpatient population) on 10 geriatric wards in two Leeds hospitals within 14 days of last year's general election. A medical assessment was made of the patient's fitness to vote on the day. Patients were not interviewed if they were too unwell or were away from the ward. Altogether 190 were in hospital on the day of the election, of whom 115 were judged medically fit to vote. Only 19 voted, all by prearranged postal vote. Sixty six of the remainder stated that they would have voted had they been at home. Fifty eight patients were home at the time of the election. Of these, 46 were medically fit to vote and 28 did so, 15 by post and 13 at a polling station.

Postal votes for the election had to be registered by 14 April 1997; late applications could be made by 23 April if accompanied by a medical letter. There was no mechanism to vote after that date other than in person. Political parties used to visit long stay institutions to organise postal voting for their constituents, but this is no longer the practice. No provision was made locally to transport inpatients to polling stations. As a result we found that a much smaller proportion of inpatients voted (17%) than of patients who were at home at the time of the election (61%).

Better provision should be made for older patients to vote. Staff, managers, and the public should be aware of the system for registering postal votes, and better transport arrangements should be available. The mechanism for casting distant votes should be reassessed, and consideration should be given to extending the deadline for casting a postal vote, particularly for people over retirement age.

Elderly people remain politically active[2] and, with demographic change, will form an increasing proportion of the electorate in the future. If the current situation does not change then many elderly people will be denied a fundamental democratic right.

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1 Smith H, Humphreys M. Changes in laws are necessary to allow patients detained under Mental Health Act to vote. *BMJ* 1997;315:431. (16 August.)
2 Parry G, Moyser G, Day N. *Political participation and democracy in Britain*. Cambridge: Cambridge University Press, 1992:168-71.

Integration of hepatitis B vaccination into national immunisation programmes

Authors should have taken open minded view of all relevant evidence

EDITOR—There is currently considerable interest in strategies of vaccination against hepatitis B in areas of low endemicity, such as the United Kingdom. Since there is little argument over whether this vaccination is safe, effective, and desirable given sufficient resources, the debate has largely focused on the relative cost effectiveness of selective versus mass immunisation options.

In their article Van Damme et al used economic (and other) arguments to advocate mass immunisation against hepatitis B.[1] Unfortunately, their section on economic evaluation is misleading. Specifically, on page 1035 they state that "cost effectiveness studies performed in countries with low endemicity (Belgium, Canada, United Kingdom, United States) consistently find that universal vaccination is economically attractive." They support this statement by referencing four studies. They do not, however, reference those studies that do not agree with this statement (such as that by Williams et al on the cost effectiveness of vaccination against hepatitis B in the United Kingdom[2]). The evidence is therefore not consistent.

More serious than this apparent error of omission is that the authors proceed to reinforce their argument by stating that "Cost effectiveness ratios varied from $1000 to $20 000 (£625-12 500) per discounted life year gained depending on the country's epidemiological and organisational characteristics," which, they argue, compares favourably with such ratios in other, existing, prevention programmes. The clear implication of this statement is that the United Kingdom study that they quote (by Mangtani et al[3]) found that the cost per discounted life year gained for universal immunisation lies in this range. But in fact Mangtani et al estimated the cost per discounted life year gained to be £51 817 and £94 821 for mass immunisation of adolescents and of infants respectively—roughly four to eight times higher (four to eight times less cost effective) than the upper limit that Van Damme et al quote. Ironically, in their discussion of the economic evidence Van Damme et al state that "Health policy makers should look carefully at these studies." I agree wholeheartedly, provided that policy makers and those who advise them take an open minded view of all the relevant evidence.

The case for mass vaccination in the United Kingdom remains to be made convincingly. In the end, the best argument for mass vaccination against hepatitis B may be that global eradication of the virus is unlikely to be achieved without it.

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1 Van Damme P, Kane M, Meheus A on behalf of the Viral Hepatitis Prevention Board. Integration of hepatitis B vaccination into national immunisation programmes. *BMJ* 1997;314:1033-6. (5 April.)
2 Williams JR, Nokes DJ, Anderson RM. Targeted hepatitis B vaccination—a cost effective immunisation strategy for the UK? *J Epidemiol Community Health* 1996;50:667-73.
3 Mangtani P, Hall AJ, Normand CEM. Hepatitis B vaccination: the cost effectiveness of alternative strategies in England and Wales. *J Epidemiol Community Health* 1995;49:238-44.

Authors' reply

EDITOR—In our article we used different arguments, including economic ones, to advocate routine immunisation against hepatitis B. Edmunds focuses on the economic arguments, and his letter is based on two issues: that "the debate has largely focused on the relative cost effectiveness of selective versus mass immunisation options" and that we may have misled readers by quoting only studies that supported our position.

With respect to the first issue, although Edmunds and some other workers in Britain and Scandinavia believe that selective immunisation (for high risk groups) is a viable strategy for controlling hepatitis B, we do not agree. No country has succeeded in vaccinating a significant proportion of the high risk groups. The failure of selective immunisation against hepatitis B is due not only to the potentially correctable fact that few resources have been devoted to the problem but, more importantly, to the fact that these high risk groups are not effective targets for immunisation programmes. If selective immunisation is not an effective public health strategy for controlling hepatitis B on a community basis, cost effectiveness analyses that recommend it as the primary option are not realistic and give health officials and politicians who are reluctant to devote resources to control of hepatitis B an excuse not to do so.

With respect to the second issue, we chose to cite four studies which we thought were reasonably well done. The United Kingdom study by Williams et al was published in December 1996, after we sent our article to the *BMJ* (date of acceptance 10 September 1996).[1] Our article did not focus on economic evaluations or on the situation in the United Kingdom specifically. We agree that the range of all published economic evaluations in countries with low endemicity

is wider than that mentioned in our article.[2] Our article is an excerpt from a chapter published elsewhere[3] and refers to the majority of studies performed in regions of low endemicity. Edmunds is correct in noting that the results of Mangtani et al's study[4] were not in the range mentioned in our article, but we did not intend to deceive readers. Because of different methodologies and different assumptions on possible alternatives and input data, direct comparison of different studies is impossible and wide ranges of cost effectiveness outcomes are reported. However, all the cited studies reach a similar conclusion: that universal hepatitis B immunisation programmes showed cost effectiveness ratios that are comparable to those of other well accepted public health programmes that have already been implemented.

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1 Williams JR, Nokes DJ, Anderson RM. Targeted hepatitis B vaccination—a cost effective immunisation strategy for the UK? *J Epidemiol Community Health* 1996;50:667-73.
2 Jefferson T, Demicheli V. Is vaccination against hepatitis B efficient? A review of world literature. *Health Economics* 1994;3:25-37.
3 Van Damme P, Beutels P, Tormans G, Van Doorslaer E, Meheus A. Cost-effectiveness of hepatitis B vaccination in low-endemic countries. In: Rizetto M, Purcell R, Gerin J, Verme G, eds. *Viral hepatitis and liver disease. Proceedings of IX triennial international symposium on viral hepatitis and liver disease.* Turin: Edzioni Minerva Medica, 1997:710-3.
4 Mangtani P, Hall AJ, Normand CEM. Hepatitis B vaccination: the cost effectiveness of alternative strategies in England and Wales. *J Epidemiol Community Health* 1995;49:238-44.

Covers of *BMJ* need to be strengthened

Editor—Leading medical journals such as the *New England Journal of Medicine*, *Lancet*, and *BMJ* have their table of contents conveniently and conspicuously exposed on the cover. In none of these journals is the table of contents duplicated within the journal. Thus, if the cover is lost or damaged there is no way to get information on the contents other than to leaf through the whole issue, unless electronic databases are consulted. Because of the delay in entering reference data in databases and the delay in updating local databases, contents information on several of the most recent issues of journals is not retrievable. The most obvious risk factors for loss of or damage to the cover are the quality of the cover and binding and the number of readers per journal. Library copies of journals that are read frequently are thus particularly at risk of loss of or damage to the cover.

Issues of *New England Journal of Medicine*, *BMJ*, and *Lancet* in library 2

I was persistently irritated by the fact that a large proportion of covers of issues of the *BMJ* in the Francis Countway Library of Harvard University, Boston, and in the Medical Library of the National Hospital, Oslo, were missing or placed on wrong issues of the journal. I therefore undertook a controlled study to evaluate the loss of contents information due to damage to the cover of the most recent issues (first half year of 1997) of the *BMJ*, *Lancet*, and the *New England Journal of Medicine* in these libraries.

Library copies of the *BMJ* were more likely to lose their table of contents through loss of or damage to the cover than were copies of the other medical journals: 40 of 52 copies (77%) of the *BMJ* in the two libraries combined had minor or major damage to the cover, as against 0 of 51 copies of the *Lancet* and 0 of 52 copies of the *New England Journal of Medicine* (table). Loss of contents information may lead to serious difficulty in retrieving recent articles in the *BMJ*. Recent articles are frequently cited, and journal impact factors are based on counts of the citations of articles published during the preceding two years.[1] Difficulties in the retrieval of information on recent articles in a journal might thus lead to inappropriate non-citation and consequently to a diminished impact of the journal in question.

Owing to its popularity and the relatively poor quality of its cover, the *BMJ* should seriously consider strengthening its cover, in particular for library copies.

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1 Garfield E. How can impact factors be improved? *BMJ* 1996;313:413-5.

Overview of minor and major damage to covers of *BMJ*, *Lancet*, and *New England Journal of Medicine* (*NEJM*), for issues for first half year of 1997 in libraries 1 and 2

	BMJ		*Lancet*		*NEJM*	
	Library 1	Library 2	Library 1	Library 2	Library 1	Library 2
Intact covers	7*	5	26†	25	26	26
Loose covers	13	10	0	0	0	0
Lost or damaged covers	6	11	0	0	0	0

Number of issues of each journal in library 1 = 26; library 2 had 26 issues of *BMJ* and *NEJM* but only 25 copies of *Lancet*.
*Three covers repaired with tape.
†Three covers very loose.

❄❄We wish that we could convince ourselves that covers come off the *BMJ* more often than off the *Lancet* or the *New England Journal of Medicine* because the *BMJ* is removed from the shelves and read more often than the other journals. Unfortunately, we cannot. We have therefore taken steps to put right the problem that Dr Norstein has identified.

We have already started to use heavier and stronger paper for the cover of our international edition, and this month we will make several changes to the printing process. Together these changes should ensure that covers do not come off the *BMJ*, and perhaps Dr Norstein will repeat his study. We too will assess whether the changes solve the problem. If they do not then we will consider changing the way we bind the journal.—Editor

Polymerase chain reaction as marker of infectivity in people with hepatitis C

Summary vertical transmission rates may be misleading

Editor—We recently completed a systematic review of worldwide published and unpublished data on vertical transmission of hepatitis C virus.[1] We agree with Dore et al that the probability of transmission of the virus from a mother with antibody to hepatitis C virus but who is negative for the virus by polymerase chain reaction is very low,[2] although such transmissions have been documented.[1] We are concerned, however, about their calculation of summary vertical transmission rates (their table 1).

Firstly, they have not included all the published studies that are eligible by their stated criteria (references available on request), including one that reported 100% transmission from mothers positive by polymerase chain reaction.[3]

Secondly, they have included two partially duplicated studies: most of the infants reported on by Paccagnini et al were included in the collaborative study by Zanetti et al (A Zanetti, unpublished communication).

Thirdly, we think it inappropriate to include in these calculations the two large retrospective studies of mothers infected from contaminated anti-D immunoglobulin.

Fourthly, we found in our review that use of reported transmission rates was problematic, as studies varied considerably in the diagnostic criteria used for infection and non-infection with hepatitis C virus and in the duration of follow up of the infants.

Finally, we found considerable heterogeneity in transmission rates, even after applying standardised diagnostic criteria and stratifying by maternal hepatitis C viraemia status and HIV serostatus. This may have been due to varying distributions of other risk factors for transmission and also to differences in polymerase chain reaction methodologies. Dore et al suggest that developments in polymerase chain reaction technology have overcome the initial unreliability of the technique. Many of the studies included in the authors' table 1, however,

were carried out before the second EUROHEP hepatitis C virus study, which showed continuing problems with both false positive and false negative results.[4]

It is important that all relevant data are included in systematic reviews and that the results of separate studies are not combined uncritically.[5] In our opinion the summary transmission estimates reported by Dore et al are misleading. Variations among studies could be lessened by the development of standardised diagnostic criteria for vertical transmission of hepatitis C virus and by improvements in the reliability of polymerase chain reaction for hepatitis C virus RNA. Summary transmission risks could then be estimated and women of childbearing age who have hepatitis C could be more effectively counselled.

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1 Thomas SL, Newell ML, Peckham CS, Ades AK, Hall AJ. A review of hepatitis C virus vertical transmission: risks of transmission to infants born to mothers with and without HCV viraemia or HIV infection. *Int J Epidemiol* (in press).
2 Dore GJ, Kaldor JM, McCaughan W. Systematic review of role of polymerase chain reaction in defining infectiousness among people infected with hepatitis C virus. *BMJ* 1997;315:333-7. (9 August.)
3 Thaler MM, Park CK, Landers DV, Wara DW, Houghton M, Veereman-Wauters G, et al. Vertical transmission of hepatitis C virus. *Lancet* 1991;338:17-8.
4 Damen M, Cuypers HT, Zaaijer HL, Reesink HW, Schaasberg WP, Gerlich WH, et al. International collaborative study on the second EUROHEP HCV-RNA reference panel. *J Virol Methods* 1996;58:175-85.
5 Chalmers I, Altman DG, eds. *Systematic reviews.* London: BMJ Publishing Group, 1995.

Authors' reply

EDITOR—The main objective of our review was to assess whether hepatitis C virus RNA as detected by polymerase chain reaction could be used as a marker of infectiousness in a person with antibodies to the virus. The secondary objective was to estimate the risk of transmission of the virus through various modes.

Thomas et al report that cases of transmission of the virus have been documented from people with undetectable hepatitis C virus RNA.[1] We did not claim that such cases could not occur. An analogy can be drawn with transmission of hepatitis B virus, which has been documented from people with no evidence of hepatitis B surface antigen or hepatitis B e antigen.[2 3] Nevertheless, absence of these markers is considered to indicate that a person is unlikely to be infectious. We concluded that hepatitis C virus RNA is a strong marker of infectiousness, and our conclusion that "negative results by polymerase chain reaction indicate an extremely low probability of transmission of hepatitis C" remains unchanged. We also raised the possibility of false positive results of polymerase chain reaction and recommended that a person with hepatitis C should be counselled on the basis of a persistently positive or negative polymerase chain reaction rather than a single assessment.

Thomas et al are concerned that we did not include all published studies in our review, and they cite in evidence an early study of vertical transmission in 10 mother-infant pairs.[4] The 100% (8/8) transmission rate from mothers positive for hepatitis C on polymerase chain reaction in this study may have been due to a lack of specificity, as seven of the eight infants who were positive on polymerase chain reaction were reported to have lost their hepatitis C antibodies by 12 months. Inclusion of this study would not have altered the pooled vertical transmission rate.

Exclusion of Paccagnini et al's study (of 37 mother-infant pairs)[5] would likewise have had little effect on our estimated rate of vertical transmission. We expressed concern in our discussion about including the two large retrospective studies of mothers infected by contaminated anti-D immunoglobulin and for this reason calculated vertical transmission rates with and without these studies.

We see no reason to attribute the heterogeneity in hepatitis C transmission rates to variation in diagnostic criteria. Differences in the populations and sample sizes in the studies are a much more plausible explanation. In either case, estimation of pooled hepatitis C virus transmission rates, despite substantial heterogeneity, remains valid. Thus we strongly reject Thomas et al's claim that our estimates are misleading.

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1 Thomas SL, Newell ML, Peckham CS, Ades AK, Hall AJ. A review of hepatitis C virus vertical transmission: risks of transmission to infants born to mothers with and without HCV viraemia or HIV infection. *Int J Epidemiol* (in press).
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5 Paccagnini S, Principi N, Massironi E, Tanzi E, Romano L, Muggiasci ML, et al. Perinatal transmission and manifestation of hepatitis C virus infection in a high risk population. *Pediatr Infect Dis J* 1995;14:195-9.

Committee said that midwives and GPs should work in partnership

EDITOR—I was perturbed to find a serious error in Craft's article on women's health in the childbearing years and after.[1] She states that the "Cumberledge (sic) Committee recommended that all the care for pregnant women should be provided by midwives."

As I was the general practitioner on the Cumberlege committee, which produced the report *Changing Childbirth*,[2] I can assure Craft that the expert maternity group recommended nothing of the kind. Indeed, this is almost the reverse of what it said in that it wished to focus obstetricians on the 15% or so of women who develop complications and need skilled treatment, as Craft herself says at the beginning of her article. One of the other objectives listed in *Changing Childbirth* was that "midwives and GPs should work in partnership in the best interests of the woman," and it also said that "GPs should continue to play a valuable role in providing continuity of carers for women and their families at this very important time in their lives."

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1 Craft N. Women's health: the childbearing years and after. *BMJ* 1997;315:1301-4. (15 November.)
2 Department of Health. *Changing childbirth. Part 1: report of the expert maternity group.* London: HMSO, 1993.

Inhaled corticosteroids in wheezing associated with viral infection in schoolchildren

Meaning of "wheezing associated with viral infection" needs to be defined

EDITOR—After reading Doull et al's paper about the effect of inhaled corticosteroids on episodes of wheezing associated with viral infection in school age children I was left with several questions.

Firstly, why did the authors use beclomethasone, when Allen and Hanburys is at pains to let paediatricians know that fluticasone is more effective?

Secondly, why did the authors use the Diskhaler, when Allen and Hanburys is at pains to let paediatricians know that the Accuhaler is more effective?

Thirdly, was the inhaler technique checked in any of the children? If so, when? And what criteria were used?

Fourthly, what precisely do the authors mean by "wheezing associated with viral infection"? The paper mentions upper respiratory tract infections. I presume that this phrase often meant a runny nose, and it may be relevant that some children with a runny nose have an allergic rhinitis associated with an exacerbation of allergic asthma.

It would be unfortunate if this paper were to discourage doctors from prescribing inhaled steroids for wheezy children of school age.

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1 Doull IJM, Lampe FC, Smith S, Schreiber J, Freezer JJ, Holgate ST. Effect of inhaled corticosteroids on episodes of wheezing associated with viral infection in school age children: randomised double blind placebo controlled trial. *BMJ* 1997;315:858-62. (4 October.)

Author's reply

EDITOR—Ehrhardt expresses concern over the type and delivery system of inhaled corticosteroid used in our study and over the meaning of "wheezing associated with viral infection." The study was conceived and

designed in the university medicine department at Southampton General Hospital. At that time neither fluticasone nor Accuhaler was available in Britain. Assistance was sought from Allen and Hanburys for minimal financial support and for the supply of blinded placebo and beclomethasone dipropionate.

As the paper makes clear, there is strong evidence that recurrent wheezing associated with viral infection in children is a separate entity from atopic asthma.[1] The pattern of such wheezing is of discrete episodes of viral infection of the upper respiratory tract leading to lower respiratory tract symptoms and decreased lung function. We showed in this group of children that although lung function and bronchial hyperresponsiveness improved, regular beclomethasone had no effect on the discrete respiratory episodes. Our findings are in keeping with studies in younger children,[2] and regular inhaled corticosteroids seem unlikely to be of benefit in wheezing associated with viral infection. We and others have shown significantly decreased growth in children receiving beclomethasone dipropionate at a dose of 400 μg/day.[3–5] Thus the cost:benefit ratio is against the use of regular beclomethasone dipropionate in wheezing associated with viral infection.

We believe that Ehrhardt is misguided in his implication that any wheezy child of school age should receive inhaled corticosteroids. The use of these drugs in childhood should be reserved for disease processes in which they have been shown to be effective, and formulations and delivery systems that minimise systemic side effects should be considered.

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1 Silverman M. Out of the mouths of babes and sucklings: lessons from early childhood asthma. *Thorax* 1993;48:1200-4.
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Physiotherapy for soft tissue shoulder disorders

Conclusion that therapeutic ultrasound is ineffective was based on weak evidence

EDITOR—Van der Heijden et al claim therapeutic ultrasound to be ineffective in treating patients with soft tissue shoulder disorders and recommend that treatment guidelines should be updated or reimbursements for the use of therapeutic ultrasound in soft tissue shoulder disorders should be withheld.[1] Their conclusion, however, seems to be hasty: lack of evidence of effect is not evidence of lack of effect.

Their conclusion is based on a total of three sham controlled trials with adequate internal validity, which failed to show superiority of therapeutic ultrasound over sham treatment.[2–4] In the other three trials of therapeutic ultrasound included in the analysis therapeutic ultrasound was not shown to be superior to reference therapies (active treatment).

Because of small group sizes the β error of the reviewed studies clearly exceeds 20%. Downing and Weinstein themselves estimated the power of their study to be 0.3.[2] The reliability of the outcome measures used (pain, mobility, activities of daily living) is not perfect. This also reduces study power and thus the validity of the evidence whether or not therapeutic ultrasound is effective.

Homogeneity of those three studies with respect to diagnosis and administration of the treatment was not assured. Besides diagnosis and stage of disease, dose variables (power density, sonation time per treatment, number of treatments, etc), mode of delivery (continuous or pulsed), and frequency used (1 or 3 MHz) are potential determinants of effects of therapeutic ultrasound, and still to be investigated in clinical studies. In a published meta-analysis of ultrasound therapy in musculoskeletal disorders a dose-response relation could not be calculated because of incomplete information on the treatment variables.[5]

Internally valid sham controlled, double blind studies with adequate group sizes are a precondition for firm conclusions about the use of therapeutic ultrasound in soft tissue shoulder disorders.[2] Dose aspects should be included. In contrast to many other physiotherapeutic remedies, ultrasound has an advantage in that blinding of patients can be managed by heating the transmission gel or cooling the transducer head. On the basis of the evidence presented we would suggest that no firm conclusion is possible at present. That is certainly not a satisfactory conclusion, but it is what the authors (correctly) conclude about other forms of physiotherapy and what can be concluded at present.

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1 Van der Heijden GJMG, van der Windt AWM, de Winter AF. Physiotherapy for patients with soft tissue shoulder disorders: a systematic review of randomised clinical trials. *BMJ* 1997;315:25-30. (5 July.)
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Authors of systematic review misreported one trial that did give significant results

EDITOR—I carried out one of the clinical trials reviewed in the paper by van der Heijden et al, of low level laser therapy versus dummy laser.[1 2] I note from table 2 of their paper that the results of my study were analysed as having found "no significant difference." My study found that there was a significant difference between the two groups (table 1).

When the laser group was tested for changes in muscle force and tenderness within the group, there was a significant difference in muscle force ($P<0.01$) but not in tenderness $P=0.06$ (table 2). In view of the closeness of this second P value to 0.05 and the significance of all the other results, there was a strong possibility of a type 2 error in this one result.

Although I did not calculate 95% confidence intervals, I did determine the standard error of the mean, which gave a good indication of central tendencies. With the results of my study indicating that laser therapy may well be an effective treatment for shoulder tendinitis when advice to reduce intrinsic overload of the tendon is also given, I believe that the reference to my work in table 2 of the review is misleading.

Van der Heijden et al suggest that physiotherapy modalities should be compared with steroid injections. A rapid reduction in inflammation resulting from an injection will probably give good results in the short term. However, will the tendon be stronger and any more able to cope with cumulative stress through overuse? My study found that rest from the offending activity coupled with laser therapy was an effective means of treating shoulder tendinitis. Advice alone with dummy laser failed to reduce symptoms. It is important to prepare patients to help themselves when treating soft tissue conditions associated with overuse and degeneration, rather than to go for the quick fix.

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Table 1 Mean measurements in actual and dummy laser groups

Measurement	Actual laser group	Dummy laser group	P value
Pain analogue scale (mm)	23	–2	<0.01
Muscle force (N)	53	–10	<0.001
Tenderness (N)	14	–2	<0.05

Table 2 Mean measurements before and after treatment in actual laser group

Measurement	Before treatment	After treatment	P value
Muscle force (N)	133	175	<0.01
Tenderness (N)	42	49	0.06

1 Van der Heijden GJMG, van der Windt AWM, de Winter AF. Physiotherapy for patients with soft tissue shoulder disorders: a systematic review of randomised clinical trials. *BMJ* 1997;315:25-30. (5 July.)
2 Saunders L. The efficacy of low-level laser therapy in supraspinatus tendinitis. *Clin Rehab* 1995;9:126-34.

Authors' reply

EDITOR—Many trials of physiotherapy for shoulder enthesopathies have been published, most with inferior validity and small sample sizes. As our systematic review found, surprisingly few have reported positive results. Physical modalities, such as ultrasound and laser therapy, are used as adjuvants to exercise therapy in the treatment of enthesopathies. The trials in our review selected common populations of patients in which to evaluate the contribution of (mainly) physical modalities to recovery.

We agree with Brockow et al that reporting of "diagnosis" and "administration" needs more attention. For many trials in our review crucial information about eligibility criteria and delivery of treatment is lacking. This, however, does not automatically indicate inappropriate delivery of treatment. The three trials cited by Brockow et al provided enough information about eligibility criteria and delivery of treatment for us to conclude that they were dissimilar. In our opinion, these dissimilarities correspond with practice patterns where no consensus exists among experts about optimal delivery of ultrasound for musculoskeletal disorders and large variation can be observed between therapists.

Brockow et al consider (post hoc) low power to be due to poor reliability of outcome measures and small trial sizes. We drew conclusions on outcome measures that are well accepted in physiotherapy practice—that is, global improvement and pain severity, which were most often rated on ordinal or visual analogue scales. These scales have been shown to have sufficient reliability for such outcome assessments,[1 2] and most valid trials used standardised procedures to optimise reliability of outcome assessments. Brockow et al suggest, furthermore, that low statistical power may have reduced the validity of the trials. Though related, internal validity and statistical power of trials are separate design issues. Statistical power—that is, trial efficiency—is not a prerequisite for internally valid trials, nor can it compensate for biases and confounding. The methods assessment in our review clearly shows that trials with small sample sizes can be internally valid. In our opinion, none of the internally valid trials yielded clinically relevant effect estimates. We consider the low power to be due to the relatively large improvement in the control groups. Consequently, trial efficiency is reduced and unrealistic trial sizes are needed to show significance of observed effects.[3] In our opinion, there is still no valid evidence favouring the effectiveness of ultrasound.

Finally, Saunders rightly points out an error in our systematic review; in her paper she indeed stated that the observed effects were significant. Unfortunately, however, neither her paper nor her letter provides sufficient information to enable calculation of confidence intervals of observed effects.

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1 Carlsson AM. Assessment of chronic pain. I. Aspects of reliability and validity of the visual analogue scale. *Pain* 1983;16:87-101.
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Survival is better indicator than mortality in geographic comparisons of health

EDITOR—I have thought for some time that the way in which mortality and the risks of death are reported can give a misleading impression. Wise's news item about a report on premature deaths in Britain confirms my worries.[1 2]

In any comparison of mortality (or any other statistics), if the denominator is a small number and getting smaller over time then, even if the numerator is unchanged, the effects over time can suggest dramatic change. For example, suppose infant mortality is 30 per 1000 in a northern city and 10 per 1000 in a rural county. It is correct to say that mortality is three times higher in the urban area. If things improve so that, after 10 years, mortality is 25 per 1000 and 5 per 1000 respectively, things have got relatively worse as mortality is now five times higher in the urban north. This can be taken to extremes: if there were no deaths in the rural county and one death per 1000 births in the urban north we would be able to say that things were infinitely worse up north—but this would hardly do justice to the facts.

These calculations give a misleading impression because they use the small number of deaths as a basis for comparisons. Consider what happens if we concentrate on survival, using the earlier example. At the start, 970 children survive per 1000 in the urban north and 990 per 1000 in the rural county, an absolute difference of 20 per 1000 (or 2 percentage points) and a relative difference of 2.06% (990/970 = 1.0206). This is potentially a small difference given the relative affluence of each area. As things change, to 975 survivors in the urban area and 995 in the rural area per 1000 births, the absolute difference remains 20 survivors but the relative advantage of the affluent area is now only 2.05% (995/975 = 1.0205)—that is, the relative advantage of the risk has narrowed but only slightly.

At a time when statisticians have shown the importance of separating absolute and relative risks, it is disappointing to find so many references to mortality concentrating on potentially misleading indicators of the risk of death. We should be focusing on survival in geographic comparisons of health, since it is survival that matters and survival that gives a less sensational but accurate picture of the health of different regions.

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1 Wise J. Britain has become less equal in death. *BMJ* 1997;315:384. (16 August.)
2 Joseph Rowntree Foundation. *Death in Britain: how local mortality rates have changed: 1950s to 1990s.* York: York Publishing, 1997.

Sexually transmitted infections in women who have sex with women

Surveillance data should include this category of women

EDITOR—De Cock and Low call for the inclusion of further data, particularly on ethnicity, in the surveillance of sexually transmitted infections.[1] We believe that more information relating to women's sexual and risk behaviour and acquisition of sexually transmitted infections should be included.

We were amazed by the inadequacy of the data used in a recent presentation by a member of staff from the Public Health Laboratory Service Communicable Disease Surveillance Centre. When the rates of infection with all the common sexually transmitted infections were reported the data were broken down into three categories: men, homosexual men, and women. The reason why the inclusion of ethnic data is necessary has been highlighted,[2] but no mention has been made of the omission of data relating to sexually transmitted infections that have been acquired by women who have sex with women.

Monitoring forms routinely used by all genitourinary medicine clinics, which are analysed centrally, do not have a column for recording sexually transmitted infections acquired through female homosexual contact. There is an assumption that lesbians and women who have sex with women are at low risk of acquiring sexually transmitted infections. As data are not routinely gathered to prove or disprove this hypothesis, however, it is difficult to assess. There is certainly some evidence to show that lesbians have a relatively high prevalence of viral sexually transmitted infections such as infections with herpes simplex virus and human papillomavirus. In addition, they may not attend for regular cervical cytology and could be at risk of developing cervical cancer.[3]

Research under way in New York suggests that, among women who inject drugs, those who have sex with other women may have up to a 40% increased risk of contracting HIV infection (S Friedmann, National Development and Research Institutes, NY, personal communication, 1997).

Could there be a similar tendency for certain women to be at greater risk in Britain?

We believe that this inequality in recording, monitoring, and analysing national data on female homosexual acquisition of sexually transmitted infections should be addressed urgently. The data collected should be used to improve the health of lesbians; meanwhile, everyone should remember the potential for misuse of these data and for stigmatisation of women—much in the same way that data on ethnicity can be abused.[4]

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1 De Cock KM, Low N. HIV and AIDS, other sexually transmitted diseases, and tuberculosis in ethnic minorities in United Kingdom: is surveillance serving its purpose? *BMJ* 1997;314:1747-51. (14 June.)
2 Lacey CJN, Merrick DW, Bensley DC, Fairley I. Analysis of sociodemography of gonorrhoea in Leeds, 1989-93. *BMJ* 1997;314:1715-8. (14 June.)
3 Edwards A. Sexually transmitted diseases in lesbians. *Int J STD AIDS* 1990;1:178-81.
4 Fitzgerald M. Gonorrhoea and ethnicity. *BMJ* 1997;315:1160. (1 November.)

Reply from Public Health Laboratory Service

EDITOR—Ford and Clarke express concern over the lack of surveillance data on sexually transmitted infections acquired through sex between women. Their suggestion that lesbians may be at high risk of viral sexually transmitted infections and HIV infection is not supported by data from Britain. Unpublished data from a sentinel study of three genitourinary medicine clinics in England found the prevalence of genital herpes and warts to be lower in the 635 homosexual and bisexual female attenders than in the 46 484 heterosexual female attenders (6% *v* 9%, respectively); and of the 14 397 adult cases of AIDS reported in Britain by the end of September 1997, only three were in homosexual or bisexual women, all of whom had a history of injecting drug use (Communicable Disease Surveillance Centre (CDSC), unpublished data). In the Public Health Laboratory Service's survey of injecting drug users, four of the 270 women who reported previous homosexual contact were infected with HIV compared with 44 of the 4326 who did not (CDSC, unpublished data).

The observation that lesbian injecting drug users in New York have a 40% higher risk of contracting HIV infection than other female injecting drug users should be interpreted with caution. In a related study it has been pointed out that "it is quite likely that . . . women who reported having sex with women became infected as a result of engaging in high-risk injection (or perhaps sexual) behaviors with male injectors who had sex with men."[1]

The most important source of surveillance data on sexually transmitted infections in England and Wales is statistical returns from genitourinary medicine clinics. These are subject to periodic review by a multidisciplinary group, including genitourinary medicine clinicians. The current structure, which stratifies selected conditions in men according to whether they were homosexually acquired, was adopted in 1988 because homosexual men were believed to be at particularly high risk of acquiring sexually transmitted infections and because sex between men was and is the major route of HIV infection in Britain.

Clearly, women who have sex with women are at some risk of acquiring sexually transmitted infections, and we agree that more data on their sexual health are required. We believe that this would best be achieved through the routine collection of disaggregated data, including demographic data and information on risk behaviours, from genitourinary medicine clinics. Such a major development across Britain would require considerable negotiation with the parties concerned. It would, though, enable groups at increased risk of acquiring sexually transmitted infections to be more clearly defined and might help elucidate those risk behaviours that make them vulnerable to infection.

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1 Friedman SR, Jose B, Deren S, Des Jarlais DC, Neaigus A. Risk factors for human immunodeficiency virus seroconversion among out-of-treatment drug injectors in high and low seroprevalence cities. *Am J Epidemiol* 1995;142:864-74.

Southern Africa is good place to research role of fetal malnutrition in chronic diseases

EDITOR—We are interested in Scrimshaw's allusion to populations in Latin America in his editorial on the relation between fetal malnutrition and chronic disease in later life.[1] There, in the 1960s, despite a high prevalence of low birth weight, the prevalence of atherosclerosis and of myocardial infarction was low. Also in the 1960s, in villages in Africa, there was a high prevalence of low birth weight but little rise in weight and blood pressure with age.[2] Diabetes was near absent and coronary heart disease totally so. Cancers related to diet, especially in rural areas, were uncommon. Old Africans died mainly of infections.[3] In this context, the Barker hypothesis would seem to operate minimally.

Nowadays, little more than a generation later, in South Africa the prevalence of low birth weight in African babies is still high, at about 15%. In the cities, obesity in women, hypertension, and diabetes have higher prevalences in Africans than in the white population.[2] Why is the prevalence of obesity in African women five times higher than that in men? Why does coronary heart disease remain uncommon despite the relatively high presence of risk factors? Why is colon cancer absent in rural dwellers and uncommon in urban dwellers?[2] During this period health has improved: in urban areas 95-97% of infants survive, and life expectancy is about 60 years.

As to the future, in the continent of Africa the huge majority of people will remain impoverished for decades to come. Even should prosperity supervene, the percentage of babies with low birth weight is unlikely to be reduced: it is still 13% in African Americans.[4]

Certainly, the acquisition of "womb to tomb" data on health and ill health must be intensified. Our precise knowledge of causes of degenerative diseases is still so limited. Thus for coronary heart disease, the most researched of diseases, known risk factors explain only half of the variance in its occurrence.[5] There can hardly be a more favourable venue for research into the subject at issue than southern Africa, with its wide range of communities of different ethnic origin, circumstance, and urbanisation.

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1 Scrimshaw NS. The relation between fetal malnutrition and chronic disease in later life. *BMJ* 1997;315:825-6. (4 October.)
2 Walker ARP. The nutritional challenges in the new South Africa. *Nutr Res Rev* 1996;9:33-65.
3 The aged ailing African [editorial]. *Lancet* 1973;ii:1472.
4 Current trends: infant mortality—United States, 1992. *MMWR* 1994;43:905-9.
5 Leeder S, Gliksman M. Prospects for preventing heart disease. *BMJ* 1990;301:1004-5.

Occurrence of orf in humans was reported by Grant Peterkin in 1937

EDITOR—The obituary of George Marner Lloyd mentions that "he identified the link between humans and animals in orf (published in the *Lancet* in 1951)."[1] My late father, Dr G A Grant Peterkin, who was a consultant dermatologist in Edinburgh, published a paper on the occurrence in humans of contagious pustular dermatitis of sheep (orf) in November 1937.[2]

As a rural general practitioner and therefore familiar with orf—and occasionally known by my colleagues as Son of Orf—I think that the record should be kept straight.

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1 George Marner Lloyd [obituary]. *BMJ* 1997;315:433. (16 August.)
2 Grant Peterkin GA. The occurrence in humans of contagious pustular dermatitis of sheep ("orf"). *Br J Dermatol Syphilis* 1937;49:492-7.