We welcome letters to the Editor concerning articles which have recently been published. Such letters will be subject to the usual stages of selection and editing; where appropriate the authors of the original article will be offered the opportunity to reply.

Letters should normally be under 300 words in length, double-spaced throughout, signed by all authors and fully referenced. The edited version will be returned for approval before publication.

Comparison of the Wrightington FC hip with the Charnley low-friction arthroplasty

Sir,

I read with interest the paper in the July 1998 issue by Sochart and Hardinge¹ entitled 'Comparison of the Wrightington FC hip with the Charnley low-friction arthroplasty.'

The paper compares a Wrightington series with a Charnley series and finds the former to be slightly superior. The different geometry between the two stems is then described, and although the words "Wrightington system" and "Charnley system" are used, perhaps to gloss over this slightly, the intended inference is certainly that the differences in the results are due to the different geometry of the stem.

I question this conclusion. Although it has been well documented that the two series are similar and that in both the patients were operated on by very experienced hip surgeons, nevertheless there is one fundamental difference between them. In the Wrightington FC series the medullary canal was always plugged and a cement gun with retrograde filling was used, while in the Charnley series the canal was not plugged, at least not 'routinely', and the canal was filled digitally.

My feeling is that these differences are quite sufficient in themselves to explain the slight variation in the results. Indeed, an alternative conclusion from the paper would be perhaps that, in the absence of secondary effects such as changes in stiffness, the precise geometry of the stem is not important within wide limits.

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1. Sochart DH, Hardinge K. Comparison of the Wrightington FC hip with the Charnley low-friction arthroplasty. *J Bone Joint Surg [Br]* 1998;80-B:577-84.

Author's reply:

Sir,

I thank Mr Northmore-Ball for his interest.

The Charnley low-friction arthroplasty is recognised as the 'gold standard', and seeking to show that my logical evolution of the Charnley rectangular stem was superior was always going to be difficult. This is why I felt that we should wait at least ten years

to compare the results and am gratified that at 15 years there is a survival of 95% for the Wrightington stem compared with 84% for the Charnley.

To say, as does Mr Northmore-Ball, that this is "slightly superior" is tantamount to damning with faint praise.

The cemented total hip arthroplasty is the most successful major elective procedure in surgery. It is vital to preserve the cement mantle.

Mr Northmore-Ball is correct when he states that the canal cement restrictor was only used routinely in the Wrightington series, but I would contend that this is not the major factor.

The 84% survival seen at 15 years is similar to that of other series in which a medullary bone block had been used.¹

Torsional forces in the stem are increasingly recognised as an important cause of loosening.^{2,3} The difference between the two series at 15 years is 95% compared to 84%, but it is likely to be more marked at 20 years.

The cross-sectional shape of the stem is of paramount importance as a component of the stem-cement-bone complex. I disagree with Mr Northmore-Ball on this vital aspect.

The increased resistance to torsion of the Wrightington Frusto-conical stem was shown in 'in-house' tests by Howmedica, in Raheen, Eire. This is a stimulus for us to repeat the test in an academic Bio-Engineering Department.

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Wigan, UK.

1. Wroblewski BM. 15-21 year results of the Charnley low-friction arthroplasty. *Clin Orthop* 1986;211:30-5.
2. Berme N, Paul JP. Load actions transmitted by implants. *J Biomed Eng* 1979;1:268-72.
3. Mjoberg B, Hansson LI, Selvik G. Instability of total hip prosthesis at rotational stress: a roentgen stereophotogrammetric study. *Acta Orthop Scand* 1984;55:504-6.

The control of new prosthetic implants

Sir,

I read with interest the Editorial by Grigoris and Hamblen¹ entitled 'The control of new prosthetic implants' in the November 1998 issue and entirely agree with the conclusions.

The continued discussion of implant failures, in particular the 3M Capital implant, underlines the importance of collecting long-term surveillance data to measure outcomes.

One point seems to be receiving less than full attention. While the prosthesis is of great importance, so also is the interaction between the femoral implant and the bone cement. As the Editorial notes, the Scandinavian registers have confirmed the importance of choosing a suitable bone cement as well as a good femoral implant.^{2,3}

The study by Massoud et al⁴ of the Nottingham experience with the Capital prosthesis showed an unacceptable early failure rate of 26%. The failure here was the Capital implant but an examination of the relationship of the type of cement to loosening of the Capital femoral component bears further investigation. The failure rate of the Capital femoral component when CMW cement was used was 41.7%, while that with Palacos cement was 12.5% ($p < 0.01$).

Although the basic composition of all PMMA bone cements is similar, but not identical, differences have been observed in revision and failure rates.²⁻⁴ This highlights the potential inter-

action between a bone cement and the femoral component in affecting the outcome of joint replacement.

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1. **Grigoris P, Hamblen D.** Editorial. The control of new prosthetic implants. *J Bone Joint Surg [Br]* 1998;80-B:941-3.
2. **Malchau H, Herberts P, Ahnfelt L.** Prognosis of total hip replacement in Sweden: follow-up of 92 675 operations performed 1978 to 1990. *Acta Orthop Scand* 1993;64:497-506.
3. **Havelin LI, Espehaug B, Vollset SE, Engesaeter B, Langeland N.** The Norwegian arthroplasty register: a survey of 17 444 hip replacements 1987 to 1990. *Acta Orthop Scand* 1993;64:245-51.
4. **Massoud SN, Hunter JB, Holdsworth BJ, Wallace WA, Juliusson R.** Early femoral loosening in one design of a cemented hip replacement. *J Bone Joint Surg [Br]* 1997;79-B:603-8.

Authors' reply:

Sir,

We appreciate the points made by Mr Quartey with regard to the importance of polymethylmethacrylate in the success or failure of cemented arthroplasties. Although the exact contributions and relationships are not always apparent, the durability of any prosthetic reconstruction is determined by many factors which include the design of the prosthesis (alloy, shape, surface finish), the fixation, the quality of implantation (surgical skills, instrumentation) and patient-related parameters (age, gender, activity level, primary diagnosis etc). Of these, the quality of the surgical technique and selection of prosthesis are the most important for reducing the risk of revision due to aseptic loosening.

The Swedish Arthroplasty Register showed that there are differences in vivo between cements of standard or high viscosity with the highest risk for revision being seen with Sulfix-6. The best performer was Palacos/gentamicin with a risk ratio (using Sulfix-6 as nominator) of 0.43 (95% CI 0.38 to 0.48). This was followed by Palacos 0.46 (95% CI 0.41 to 0.52), Simplex 0.48 (95% CI 0.42 to 0.54) and CMW 0.54 (95% CI 0.48 to 0.59).

The data certainly support the suggestion that details of the cement as well as the prosthesis used should be included in any Joint Replacement Register.

The paper by Massoud et al² was the first to show early failure of the 3M Capital hip, but we think that their statistical correlations regarding the type of cement should be interpreted with caution. It is possible that other factors including age, surgeon, and type of Capital stem have contributed to the early failure of some reconstructions.

It is to be hoped that the larger ongoing study of 3M Capital hips will provide more information about the mode of failure of this implant.

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1. **Herberts P, Malchau H.** How outcome studies have changed total hip arthroplasty practices in Sweden. *Clin Orthop* 1997;344:44-60.
2. **Massoud SN, Hunter JB, Holdsworth BJ, Wallace WA, Juliusson R.** Early femoral loosening in one design of a cemented hip replacement. *J Bone Joint Surg [Br]* 1997;79-B:603-8.

Sir,

In regard to the Editorial in the November 1998 issue by Grigoris and Hamblen¹ entitled 'The control of new prosthetic implants' and following recent correspondence which showed the need for an implant register,^{2,3} and the possible role of implants cards,^{4,5} we conducted a survey of 16 major implant-producing companies.

Of the 11 which responded, ten expressed a willingness to be part of a co-ordinated central register.

Of the eight companies which responded in detail, most agreed to a standardised implant card, and would co-operate in the research and development of such a card. Only one company had had experience with Smart card technology in this setting, but none felt that Smart cards alone should be relied upon.

Only three companies felt they would be able to assist in financing hospitals with the technology required to process Smart cards.

As to what information should be held on implant cards, there was less agreement beyond the date of the operation in hospital, the grade of surgeon and details of the company. More discussion would be required to ensure that there was a useful input of information relating to the implant itself.

The support of the manufacturers is essential for the success of an implant register for those which reach the stage of clinical use, with or without the use of implant cards.

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1. **Grigoris P, Hamblen D.** Editorial. The control of new prosthetic implants. *J Bone Joint Surg [Br]* 1998;80-B:941-3.
2. **Riordan P, Bickler G, Lyons C.** Lessons of a hip failure: registers of joint replacement operations should be set up. *BMJ* 1998;316:1985.
3. **Harper WM, Abrams K, Elson R.** Lessons of a hip failure: register exists in Trent region. *BMJ* 1998;316:1985-6.
4. **Purkayastha S.** Patient with implants should be given implant cards for reference. *BMJ* 1997;315:1377.
5. **Collier AM.** Patients with implants should be given implant cards: such cards would facilitate recall. *BMJ* 1997;316:1246.

Authors' reply:

Sir,

Mr Collier's letter suggests one method by which the information may be recorded and retained by the patient using Smart-card technology. This would appear to have the support of most of the implant manufacturers which were surveyed. It is disappointing that only three were prepared to provide any finance towards the development. There is a need for the card to have an updating facility to add new information on other joints replaced in patients with, for example, polyarthritis or for those undergoing revision surgery. Given the potential developments in the application of this type of information technology for the storage of medical data, it seems more likely that the details of implants would be included as part of a wider range of personal medical information carried by all patients. The key element in bringing this forward is the early adoption of a unique identifying number for all patients to allow linkage of records but this desirable development still seems some way off.

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Femoral stem fixation

Sir,

I read with interest the Topic for Debate on 'Femoral stem fixation' by Shen¹ published in the September 1998 issue.

The dorsal flange was introduced at Wrightington in 1976 as a developmental step to reduce the likelihood of fatigue stem fracture which afflicted the flatback stem. The effect of the flange on reducing subsidence at the stem-cement interface was considered an

added benefit. At one year, the dorsal flange allowed a mean subsidence of 0.53 mm compared with 1.95 mm in the flatback stem.² Today this finding would surely be interpreted in favour of the dorsal flange, since we now know that increased amounts of stem migration at an early (one to two years) stage is a prognostic indicator for aseptic loosening of the stem.³ The design of the dorsal flange appears to allow for a small amount of downward movement before the flange engages the proximal cement. No attempt is made to bond the cement-stem interface by the smooth satin or 'Vaquasheen' finish which is much smoother than a matt finish and should not be thought of in the same context as a precoat.

The author is correct in stating that the flatback stem was also associated with fracture of the cement at the prosthetic tip at the rate of 26% at one year. This was interpreted by Charnley as meaning that the flatback taper was associated with end-bearing which therefore threatened the integrity of the cement. In equivalent 'first-generation' cementing conditions, the flange eliminated cement fracture in 75 cases at one year and clearly confers a protective benefit to the integrity of the cement mantle. While it is true that stem-cement subsidence is likely to be reduced by modern cementing technology, this is obviously operator-dependent, especially when it is known that 'creep' can only account for 65 µm of subsidence. Further subsidence must occur at the expense of the mantle and thus it is clear that the dorsal flange, by acting as an impediment to harmful amounts of subsidence, is user-friendly to the cement technique.

There are no long-term survivorship data which refer specifically to the flanged stem (as opposed to the Charnley stem), although our experience has been encouraging. We are currently engaged in the production of survivorship data for the flanged prosthesis.

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1. **Shen G.** Topic for Debate. Femoral stem fixation: an engineering interpretation of the long-term outcome of Charnley and Exeter stems. *J Bone Joint Surg [Br]* 1998;80-B:754-6.
2. **Loudon JR, Charnley J.** Subsidence of the femoral prosthesis in relation to the design of the stem. *J Bone Joint Surg [Br]* 1980;62-B:450-3.
3. **Walker PS, Mai SF, Cobb AGG, Bentley S, Hua J.** Prediction of clinical outcome of THR, from migration measurements on standard radiographs. *J Bone Joint Surg [Br]* 1995;77-B:705-14.
4. **Verdonschot N, Huiskes R.** Acrylic cement creeps but does not allow much subsidence of femoral stems. *J Bone Joint Surg [Br]* 1997;79-B:665-9.

Author's reply:

Sir,

I thank Mr Loudon for his comments on stem migration. Neither the flatback collar nor the Cobra flange was able to stop subsidence completely. RSA studies can now determine the location of subsidence.¹ The Loudon-Older criteria of less than 2 mm of subsidence² probably occurs primarily at the stem-cement interface. Subsidence of 4 mm must involve the cement-bone interface, which is undesirable. Subsidence of 5 mm probably involves not only both interfaces but a shear fracture of bulk cement as well.

The Vaquasheen surface is definitely not in the same class as the substrate of the precoat, but compared with the mirror polished surface, it is rough. The asperities of the Vaquasheen surface may be sufficient to create a cement bond through interdigitation. Cementing technology is not a perfectly transferable technique and the use of porous stems in the USA has further decreased the pool of experience. The presence of a flange or collar makes a stem much more user-friendly during insertion.

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1. **Adrian JA, Gill HS, Marks B, Murray DW.** Cement mantle migration after THR: a comparison of Charnley and Exeter prosthesis with RSA. *J Bone Joint Surg [Br]* 1998;Suppl III:80-B:269-70.

2. **Loudon J, Older MW.** Subsidence of the femoral component related to long-term outcome of hip replacement. *J Bone Joint Surg [Br]* 1989;71-B:624-8.

Trauma and orthopaedic surgery on the Internet

Sir,

I read with interest the Current Concept in the January 1999 issue by Oliver¹ entitled 'Trauma and orthopaedic surgery on the Internet'. There is one large orthopaedic database which has not been mentioned which is WorldOrtho (www.worldortho.com). It contains the entire curriculum for orthopaedics for medical students and the essentials for registrars and residents in training. It was developed by Professor R. L. Huckstep and myself.

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1. **Oliver CW.** Current Concept. Trauma and orthopaedic surgery on the Internet. *J Bone Joint Surg [Br]* 1999;81-B:3-5.

Total knee arthroplasty with the PFC system

Sir,

I read with interest the article in the September 1998 issue by Schai, Thornhill and Scott¹ entitled 'Total knee arthroplasty with the PFC system'.

Since we use this system we were pleased with their good results and agree that survivorship is often better expressed in a best-case and worst-case circumstance in relation to follow-up and clinically assessed patients.

We would, however, like to raise a few concerns. They state that none of the 80 knees not available for follow-up had required revision. Since four knees were not traceable, we would like an explanation as to how they know that for certain.

In the results they state that the survivorship at ten years based on the need for reoperation for any reason was 90%. Since there were 24 reoperations, it appears that all 235 knees were taken into the calculation, even if they survived for much less than ten years. We do not agree that their eight knees in patients who died within two years and were probably unrevised, as well as the other 62 knees in patients not alive ten years after knee replacement, can be included in the ten-year successes. They should give the survivorship in their 155 clinically assessed patients. With 24 reoperations, four knees lost to follow-up and 161 knees followed up for more than ten years it appears that a worst-case circumstance could be only 83% survivorship at ten years.

In the discussion they recommend routine patellar resurfacing in rheumatoid arthritis and in most patients with non-inflammatory arthritis. They base this on 22 unrevised all-polyethylene patellae at a mean (they meant 'minimum') follow-up of ten years and the need to resurface one out of 43 previously unresurfaced patellae because of pain (the reader is not told whether this was successful). This statement may be correct but we do not believe that it can be recommended from their data.

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1. **Schai PA, Thornhill TS, Scott RD.** Total knee arthroplasty with the PFC system. *J Bone Joint Surg [Br]* 1998;80-B:850-8.

Authors' reply:

Sir,

We thank Dr Ehrendorfer and Mr Reading for their comments regarding our paper.

As they noted, we could not find and assess clinically four knees in three patients. Our assumption that they did not undergo revision was based on telephone calls to their relatives and primary-care physicians who last had contact with them. We also assume (perhaps incorrectly) that we would have been informed of any revision surgery by the treating physician. That is certainly our policy both out of courtesy and the need to obtain the original operative note for the type of component and size. We regret that this was not clarified.

Regarding our survivorship analysis, we used the accepted methods published by Armitage¹ and Rothman² and Table VIII provided a detailed life-table analysis. Our survivorship curves in Figure 5 included the appropriate confidence limits. Certainly, it is possible to report the results of a procedure by studying only those who have completed a specific follow-up period. The disadvantage of this method is the failure to include those patients who are deceased but still provide useful information as to their function and success until the time that they are withdrawn from the study.

Finally, the authors are correct in pointing out that our recommendation to resurface most osteoarthritic patellae is not statistically proven by our experience with the patients confined to this report. It is based on previously reported data from our institution³⁻⁵ and supported (but not proven) by the lack of failure of cemented all-polyethylene patellae as opposed to the 2% need for secondary resurfacing at ten years in this study.

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1. **Armitage P.** Survival analysis. In: Armitage P, Berry G, eds. *Statistical methods in medical research*. 3rd edition. Oxford: Blackwell Scientific Publishers, 1994:468-97.
2. **Rothman KJ.** Estimation of confidence levels for the cumulative probability of survival in life-table analysis. *J Chronic Dis* 1978;31: 557-60.
3. **Boyd AD Jr, Ewald FC, Thomas WH, Poss R, Sledge CB.** Long term complications after total knee arthroplasty with and without resurfacing of the patella. *J Bone Joint Surg [Am]* 1993;75-A: 674-81.
4. **Brick GW, Scott RD.** The patellofemoral component of total knee arthroplasty. *Clin Orthop* 1988;231:163-78.
5. **Levitsky KA, Harris WJ, Mcmanus J, Scott RD.** Total knee arthroplasty without patella resurfacing: clinical outcomes and long-term follow-up evaluation. *Clin Orthop* 1993;286:116-21.

The protective effect of a cut-resistant glove liner

Sir,

With reference to the article in the May 1998 issue by Sutton, Greene and Howell¹ entitled 'The protective effect of a cut-resistant glove liner', I have found that perforation of a glove is easily detected if a small amount of Betadine solution is poured into the outer glove before double gloving. If the glove is perforated the leak of Betadine is noticed immediately. Not infrequently a new glove has been shown to have a small hole in it.

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1. **Sutton PM, Greene T, Howell FR.** The preoperative effect of a cut-resistance glove liner: a prospective randomised trial. *J Bone Joint Surg [Br]* 1998;80-B:411-3.

Determining the sagittal dimensions of the canal of the cervical spine

Sir,

I read with interest the article by Blackley et al¹ in the January 1999 issue entitled 'Determining the sagittal dimensions of the canal of the cervical spine.'

The authors correctly note that the use of the canal-to-body ratio eliminates error due to radiological magnification, but it also eliminates the much more important variation in absolute measurements which is due to differing subject size. Blackley et al have calculated the correlation coefficient between a ratio and an absolute measurement, namely the canal-to-body ratio, calculated from a radiograph of the lateral cervical spine and the antero-posterior (AP) diameter of the canal measured from an axial CT image, respectively. This is erroneous. As a general rule, small subjects have small vertebral bodies and small vertebral canals. A larger subject may have the same canal-to-body ratio, but a larger actual diameter of the canal. The scatter of observations of cement-to-body ratios v the actual diameter of the canal is therefore dependent on the spectrum of the physical size of the subjects. This is the simplest explanation for the relatively poor correlation shown between the canal-to-body ratio and the diameter of the canal.

It has been suggested that a 'tight' cervical cord could predispose to cord neurapraxia.² An objective measurement of the 'tightness' of the cervical cord could be obtained by calculating the ratio of the cross-sectional area of the cord to the cross-sectional area of the spinal canal. This 'cord-to-canal' ratio can be derived from measurements taken from axial CT or MRI scans. The cord-to-canal ratio is independent of the physical size of the subject in the same way as the canal-to-body ratio.

If there is a strong negative correlation between the canal-to-body ratio and the cord-to-canal ratio, this suggests that the canal-to-body ratio is a useful predictor of stenosis of the cervical spine. It would then be appropriate to investigate whether the canal-to-body ratio predicts the risk of neurapraxia of the cervical cord.

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1. **Blackley HR, Plank LD, Robertson PA.** Determining the sagittal dimensions of the canal of the cervical spine: the reliability of ratios of anatomical measurements. *J Bone Joint Surg [Br]* 1999;81-B:110-2.
2. **Torg J, Pavlov H, Genuario S, et al.** Neurapraxia of the cervical spinal cord with transient quadriplegia. *J Bone Joint Surg [Am]* 1986;68-A:1354-70.

Authors' reply:

Sir,

We thank Mr Milner for his interest in our article. He recognises that the ratio method may eliminate error of magnification and variation in absolute measurements due to differing size and that our paper correlates a coefficient between a ratio and an absolute measurement. Indeed it was the purpose of our study to determine if ratios were reliable for absolute measurement. We agree that a smaller subject may have both a smaller canal and body than a larger subject. Both the smaller and larger subject may have the same ratio yet their canal diameters may be quite different. This possibility led us to investigate the subject further with the methodology which we have outlined. We agree that this is the simplest explanation for the poor correlation which leads to the conclusion that the canal-to-body ratio, or any of the other ratios assessed, is an inaccurate way of obtaining the true diameter of the canal. A narrow diameter is associated with a risk of cervical neurapraxia, injury to the cord when there is bony injury, and myelopathy secondary to spondylosis.

Mr Milner noted that an objective measure of 'tightness' of the cord could perhaps be better obtained by calculating ratios between the cross-section of the canal and the cross-sectional area of the cord. This may be so, but was not the purpose of our study which was to identify if any plain-film measurement or ratio (such as the ratio reported by Torg et al¹) could reliably correlate with absolute measurements as obtained from CT or MR scans. Further investigation of the concept of 'tightness' of the canal using cross-sectional anatomical assessment on CT or MRI was beyond the bounds of our study.

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1. Torg J, Pavlov H, Genuario S, et al. Neurapraxia of the cervical spinal cord with transient quadriplegia. *J Bone Joint Surg [Am]* 1986; 68-A:1354-70.

Neonatal detection of developmental dysplasia of the hip (DDH)

Sir,

I read with interest the Editorial in the November 1998 issue by Jones¹ entitled 'Neonatal detection of developmental dysplasia of the hip (DDH)', having participated in the original examinations of the infants for congenital dysplasia of the hip in the prospective trial with Barlow.²

The Editorial significantly states that "Nevertheless, there is evidence that well-conducted neonatal clinical examination can favourably influence rates of late presentation, especially when performed by expert examiners".

In Barlow's trial all the neonates were examined within a week of birth by a member of the team which consisted of Barlow, myself and one other, in hospital or on a domiciliary visit. Each child was examined by two of the team. Dislocated or dislocatable hips were splinted for eight weeks. Radiographs were taken of doubtful hips. Over 40 000 infants were examined, 10 000 by myself. No child was found to have a missed or recurrent dislocation or dysplasia when re-examined at one year. The learning curve for conducting these examinations is long and assessment by a single person may miss a dislocation. With the current practice of early discharge from hospital it is difficult to arrange proper examination. The greater mobility of the population makes recall at one year difficult for neonatal hip examina-

tions. Nevertheless, it should be possible to arrange a trial between two centres in which in one all neonates are assessed by the Barlow method and in the other its current practice is followed. Surely it should be possible to fund Research Fellows for this purpose.

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1. Jones D. Editorial. Neonatal detection of developmental dysplasia of the hip (DDH). *J Bone Joint Surg [Br]* 1998;80-B:943-5.
2. Barlow TG. Early diagnosis and treatment of congenital dislocation of the hip. *J Bone Joint Surg [Br]* 1962;44-B:292-301.

Author's reply:

Sir,

Your salutary reminder of the meticulous care and expertise which Barlow and his team put into their prospective trial is welcome. It is important to register that clinical screening by dedicated experts can achieve results comparable with those reported from universal sonography.

I share your disappointment that when responsibility for neonatal examination passed to others less expert, 'late diagnosed' cases returned. I also share your regret that it was not possible to undertake the reasonable trial which you suggest in your letter.

I feel that we are both liable to remain unsatisfied. In our present Health Service with its fast-track obstetrics, mobile population, split-site working and dubious accounting systems, there would be huge practical difficulties in reinstating Barlow's methodology and constructing your trial. The same problems face those who favour universal sonography.

Nevertheless, we should be preparing realistic guidelines for hip surveillance to reflect current practice and resources. It may well be shown that the favoured option will be early clinical examination by experts, combined with close vigilance during infancy. This will thus get as close as possible to Barlow's ideal, i.e., that neonatal examination by experts would detect virtually all cases of DDH, leaving the occasional few which would nevertheless be detected at an age when they still had a better chance of a good outcome.

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