CORRESPONDENCE

- *All letters must be typed with double spacing and signed by all authors.*
- *No letter should be more than 400 words.*
- *For letters on scientific subjects we normally reserve our correspondence columns for those relating to issues discussed recently (within six weeks) in the BMJ.*
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Searching published reports

SIR,—All learned journals require that their authors have satisfactorily reviewed published work. Over the past decade there have been several critical reviews of authors' use of statistics[1] and recommendations to try to tighten up the use and presentation of statistics.[2] Is it now time to do the same for the review of published reports? It sometimes seems as if authors have not read, or choose to ignore, previous reports.

Short reports in the *BMJ*, by the rules of submission, must have no more than five references. In their short report on patients' access to their own psychiatric records Morris Bernadt and colleagues make use of only three,[3] one of which is to the Access to Health Records Act. They begin their discussion with the words, "The few reports that there are about the consequences of patients having access to their medical records. . . ." This assertion is not supported by examination of published work: two recent reviews cited a total of 31 papers which appeared to be reports of original research and included many with results of the consequences of patients having access to their medical records.[4,5] For example, there are reports on the effects on patients' anxiety,[6] on the number of records lost,[7] and on corrections to the record.[6]

Later in their discussion Bernadt and colleagues claim, "No study has systematically examined the influence of demographic data and diagnosis." Yet, for example, there are reports looking at the influence of age, sex, and diagnoses on the censoring of patient held records.[8] The authors also state, "We know of only one study which examined patients who requested to see their records as opposed to research recruits. . . ." In several of the studies reported previously—for example, that of Jones *et al*[8]—records have routinely been given to all patients in a given category, and so it is inappropriate to call these patients research recruits.

In discussing the arguments for and against patients' access to records, Kirby opens with "Clinicians' reactions to the idea of greater patient access to case notes vary from lukewarm support to downright opposition. . . ."[9] Kirby does not back this claim with evidence. In fact, several papers and letters from clinicians offer their enthusiastic support.[10] Later Kirby argues, "There is no systematic evidence" (on the contrasting degrees of enthusiasm between hospital doctors and general practitioners), ignoring several British hospital studies of women receiving antenatal care holding their own records,[7] the issue of a computer produced summary in a hospital diabetic clinic,[3] and the copying of letters from hospital consultants to general practitioners to patients.[10]

Of the 31 papers listed in the two reviews,[4,5] 25 were listed on MEDLINE and most of these would have been found by using the MESH headings (*a*) "medical records" and "truth disclosure"; (*b*) "medical records" and "patient education"; (*c*) "medical records" and "attitude to health"; (*d*) "medical records" and "patient participation"; or (*e*) "medical records" and "health education." (A list of 101 references on the subject of patients' access to records, of which 53 are to original research, is available.)

If scientific knowledge of medicine and health services is to progress we need to make full use of the work of others. In the United States MEDLINE is now being used effectively in routine clinical practice.[11] Jennett *et al* argue that the use of a bibliographic searching tool such as MEDLINE should be an element in the medical undergraduate curriculum.[12] Perhaps also research journals should require their authors to include details of their method of searching published work.

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Hazards of pharmacological tests of growth hormone secretion

SIR,—A Shah and colleagues show courage and candour in reporting two deaths and serious neurological sequelae after tests of growth hormone secretion in children.[1] Although there is a system for reporting adverse medical events after the administration of drugs, there is no equivalent for documenting problems after investigations and alerting the medical community of associated risks, and thus their report is valuable. It is well known among growth specialists, however, that insulin induced hypoglycaemia is potentially dangerous, and at least one child was previously known to have died after an insulin tolerance test.

Shah and colleagues highlight the danger of reversing the hypoglycaemia with excessive quantities of hyperosmolar glucose solutions, which probably caused or exacerbated cerebral oedema. But I challenge the necessity of using a test that carries such a risk, whatever the mechanism of cerebral oedema and death, to investigate short stature when other, safer tests are available.

The insulin tolerance test is still probably the most commonly used provocation test for growth hormone secretion and is regarded as the gold standard. When the Health Services Human Growth Hormone Committee investigated the relative value of several tests the insulin tolerance test performed as poorly as others, the rate of false positive results being 25%.[2] The technical value of the test is that it assesses hypothalamo-pituitary-adrenal status as well, but this information, adequate for clinical purposes, can be inferred from other tests. (At Manchester growth clinic we have diagnosed and categorised growth disorders and hypopituitarism in children without using the insulin tolerance test for the past 10 years.)

Diagnosing and defining isolated growth hormone deficiency and more generalised hypopituitarism in children will continue to be important so that they are managed with appropriate treatment regimens. Safe pharmacological and physiological stimuli of growth hormone secretion are available, and non-invasive investigations such as urinary excretion of growth hormone are under assessment.[3] With the publication of Shah and colleagues' paper it seems unlikely that parents will give their informed consent readily, and the insulin tolerance test will find its rightful place in medical history.

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1 Shah A, Stanhope R, Matthew D. Hazards of pharmacological tests of growth hormone secretion in childhood. *BMJ* 1992;**304**:173-4. (18 January.)
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SIR—A Shah and colleagues' Lesson of the Week is a sharp and regrettable reminder to all clinicians, including gynaecologists, of the hazards of pituitary challenge tests.[1]

Our experience with these investigations has been largely restricted to adolescents with suspected hypogonadotrophic hypogonadism, often in combination with growth hormone deficiency. Although hepatic, renal, and adrenocorticotrophic reserves are expectedly higher in this age group, we still observe appreciable adverse manifestations, including nausea, sweating, and tachycardia, with the standard doses of thyrotrophin releasing hormone, luteinising hormone releasing hormone, and soluble insulin that we use in our unit. When concomitant deficiency of gonadotrophin and growth hormone is suspected interpreting the total pituitary function profile becomes far more complicated and frustrating. Often results are unsatisfactory with minimal clinical value in the overall management of the patient.

Inconclusive results may erroneously be attributed to an "inadequately" stressed pituitary gland, which may result in an overwhelming desire to repeat the investigation with higher doses of pituitary stimulants. Our view is that such temptations must be firmly overruled. Even for this age group the hazards associated with such higher doses outweigh any potential clinical benefits.

In such circumstances obtaining the growth hormone profile after exercise may be beneficial. Above all, clinical sense and competence must prevail above potentially dangerous laboratory investigation. In a considerable proportion of our patients with confirmed growth hormone deficiency fusion of epiphysial cartilage would have been completed. Growth hormone replacement therapy seems immaterial in such patients. Gonadotrophin deficiency is treated with the combined contraceptive pill, pulsatile ovarian stimulation with luteinising hormone releasing hormone, or nothing.

The safety guidelines recommended by the authors are valuable. But the fact that two of their cases occurred in dedicated endocrine units and one presumably in a district general hospital hardly justifies their conclusion that the risks must be much higher in non-specialist units. The unpredictable nature of complications of pituitary stress tests is well recognised.

In our district general hospital all procedures are performed in a day central treatment unit, with flexibility for extended admission on demand, that is located close to the intensive therapy unit. The nursing staff have considerable experience; we believe that both their early recognition of potential complications and their intervention are paramount with regard to optimal outcome. In addition, we strictly observe our unit's policy of giving the intensive therapy unit early warning that the test is imminent. We consider that these factors deserve far more emphasis irrespective of the location of the clinical setting, and they certainly seem possible in every district general hospital providing laboratory services of this nature.

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Failed hip replacements

SIR,—We endorse several of the sentiments expressed by R N Villar in the editorial on failed hip replacements.[1] The apparent success of total hip replacement has extended its indications, and as a result we can expect ever increasing numbers of revisions.

At our centre the probability of survival of a Charnley hip replacement after 10, 15, and 20 years is 91%, 87%, and 82% respectively.[2] These results are comparable with those in other published series. The consequence of such figures, taking into account patients' survival, is at least five revision procedures per 100 primary arthroplasties. In 1991 we performed 56 revision hip arthroplasties at an estimated mean unit cost three times that of a primary arthroplasty. Extrapolation of these figures suggests that every 100 primary arthroplasties will result in a minimum potential extra expenditure equivalent to 15 primary arthroplasties. The Swedish experience of two less successful prostheses gave probabilities of survival of 63% and 28% at 10 years.[3] The burden of revision accruing from such poor results has worrying financial implications, especially under market constraint.

The wide range of currently available prostheses tempts surgeons with new design features, but follow up data sufficient for analysis of survival are available for only a few prosthetic designs. Undoubtedly, technical skill and choice of prosthesis at primary arthroplasty determine outcome. These factors are even more relevant at revision.

While revisions increase in numbers, complexity, and expense we believe that some financial provision should be made at primary arthroplasty for the likelihood of revision surgery, which may be performed at a specialist centre. This figure can be calculated only for prostheses with a documented probability of survival and surgery of certain quality. We conservatively estimate this to be an addition of about 15% to the cost of a primary total hip arthroplasty.

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SIR,—In an editorial R N Villar drew attention to the increasing number of revision hip arthroplasties being performed in the United Kingdom.[1]

The need for a considerable proportion of these revision procedures could be delayed or avoided. Any measure or technique proved clinically to reduce the rate of complications in hip arthroplasty should be used at the primary intervention. Uncemented components have not been shown to improve the long term results of hip arthroplasty, and many designs have caused considerable early morbidity. With cemented total hip replacement improved techniques are frequently not used by the operating surgeon.[2] Prophylactic measures taken against early complications such as deep infection or venous thrombosis are often not optimal. Likewise, although it is known that the surgical technique used at operation will affect the longevity of the prosthesis,[3] improved methods are often not practised.[2]

Fractures of acrylic cement and loss of bone stock are mentioned in the editorial as causes of loosening[1] whereas they are often a consequence of the failure of mechanical fixation of the implant. Refinements in surgical technique such as cleaning the bone, intramedullary plugging, and applying pressure to the cement have been shown clinically to improve mechanical interlock and longevity of the prosthesis.[3] They should be used routinely.

At 10 year follow up there is a 20-fold difference in the incidence of loosening of the femoral component after cemented hip arthroplasty in the absence of sepsis.[4,5] Reports from a surgeon's personal series of operations[6] are generally better than those reflecting collective experience at institutions.[7,8] Results from centres with a specialised interest in hip arthroplasty are comparable with those from a single surgeon.[9,10]

Villar should be supported in the call for revision hip surgery to be carried out by specialists. The need for revision surgery would be diminished if a greater proportion of the more complex primary hip arthroplasties and operations on patients under 60 were also performed in specialist centres or by surgeons with a specific interest in the technique.

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Voice after laryngectomy

SIR,—In his editorial on voice after laryngectomy Michael Gleeson refers to the inferiority of radiotherapy compared with primary surgery in locally advanced (T_3,N_0,M_0) disease.[1] We disagree for several reasons.

T_3,N_0,M_0 laryngeal cancer is a heterogeneous entity including disease ranging from small glottic lesions fixing the vocal cords to large transglottic tumours fixing the entire hemilarynx. Therefore no single treatment is necessarily appropriate for all cases. In this and many other British centres the primary management of almost all laryngeal cancer including T_3,N_0,M_0 disease is with radical radiotherapy. The patient is then followed up closely at a combined otolaryngology and oncology clinic, and salvage laryngectomy is performed if indicated. Gleeson supports his preference for primary surgery by referring to a retrospective study by Kaplan *et al* of just 10 cases of T_3,N_0,M_0 laryngeal cancer treated with primary radical radiotherapy.[2] It is clearly preferable to base policy decisions on larger studies. In a recent large unselected series of 376 cases from this centre the five year cause specific survival for all stages was 75·5% (J M Heywood *et al*, unpublished observations). Of the 99 patients with T_3 tumours (83 were T_3,N_0), 23 subsequently required salvage laryngectomy, with a five year cause specific survival of 66% (R P Crellin *et al*, unpublished observations).

Harwood *et al*, in a review of 144 selected cases of T_3,N_0,M_0 glottic cancer, showed that local control and cause specific survival after primary radiotherapy with surgical salvage did not differ significantly from those after primary surgery.[3] This policy was particularly effective for female patients, with a local control rate of 92%.

It is misleading to state that only up to one in five patients choose radiotherapy in preference to surgery, particularly as in McNeil *et al*'s study the subjects were not patients but healthy people.[4] Although all patients are theoretically able to make an informed choice of alternatives, most will be guided by their specialist, whose personal preferences or prejudices will inevitably influence