protocol with detailed estimates of staff and costs take time. By the time the protocol is considered and accepted by a grant giving body and the research staff are appointed it is quite possible that the situation will have changed. The opportunity to obtain an important baseline may have passed, or staff willing to cooperate may have moved. As long as money for health services research is dealt with in the same way as that for other research this problem will continue. We need more suitable ways of funding this research and a career structure for those involved in it.

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SIR,—Professor G A J Ayliffe (27 February, p 691) mentions that most health districts already have an effective district control of infection system.

The five Birmingham district health authorities, coterminous in boundary with Birmingham City Council and the family practitioner committee, have adopted a corporate approach to control of infection in the city. The computerised systems approach allows the most appropriate people automatically to recognise and carry out their individual functions within a managerial, legal, and professional framework which is the responsibility of the city's only medical officer for environmental health. The framework does not include executive responsibilities.

I suspect that problems have arisen not because of lack of skill or resources but because of a narrow definition of responsibility for control of infection, which the Acheson report accentuates and makes even narrower; it proposes to impose an executive authority, though it is difficult to see how such authority will be exercised—for example, over nurses, consultants, general practitioners, environmental health officers, and administrative and clerical staff.

The components of a control of infection system are becoming increasingly complex. General practitioners, with the help of community paediatricians, are increasingly taking over responsibility for immunisation and child health; environmental health officers have developed skills in various aspects of public health. Diversification is a virtue of necessity. If a metropolitan district like Birmingham finds a single medical officer for environmental health sufficient to draw together the various strands of control of infection into an effective response mechanism the need to employ officers with executive authority in each district becomes highly questionable. For Birmingham this would mean setting up five different offices, which would then have to find the resources to replace local authority activities in this area. The cost would be prohibitive.

Since the government is not afraid of diversity in achieving objectives in the public sector it should insist that individual districts should review their control of infection systems and adopt the best practices available within their existing resources but not embark on a universal adoption of the recommendations made at a time when most district health authorities have already begun to evolve effective systems in the light of the Stanley Royd and Stafford outbreaks.

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Parathyroid hormone and renal transplants

SIR,—Dr Z Varghese and others (6 February, p 393) showed that the pretransplant concentration of C terminal immunoassayable parathyroid hormone was higher in patients who subsequently had primary non-function of a renal graft than in those whose transplant functioned. They hypothesised that parathyroid hormone may have a nephrotoxic effect on the transplanted kidney. This conclusion is unwarranted.

It is inappropriate to use an assay of C terminal parathyroid hormone in an attempt to represent biologically active parathyroid hormone concentrations in patients with renal impairment.[1] The raised mean C terminal parathyroid hormone concentration in each group reflects poor renal clearance of C terminal fragments. The authors did not show that the two groups had comparable renal function preoperatively. A method using an "intact molecule" of parathyroid hormone is the best indicator of parathyroid activity in renal disease, and this was not attempted.

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1 Anonymous. Measuring the PTH level [Editorial]. *Lancet* 1988;i:94-5.

SIR,—Dr Z Varhese and others (6 February, p 393) base a hypothesis on an unpaired t test significance of $<0{\cdot}01$. To make things even worse one of the samples has a skewed distribution. The group that had primary function had a mean immunoassayable parathyroid hormone concentration of 1760 mg/l with a standard error of the mean of 330. As n was 26 the standard deviation must have been at least 1650. Negative values of immunoassayable parathyroid hormone concentration have not been reported, so a substantial number of patients with primary function must have had concentrations overlapping those in the primary non-function group. If the sole evidence for the hypothesis rests on the unpaired t test I find it difficult to accept, particularly as the effects of anaesthesia and surgery on circulating immunoassayable parathyroid hormone concentrations have been ignored.

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AUTHORS' REPLY—We have no argument about the relative merits of measuring intact parathyroid hormone or N terminal immunoassayable parathyroid hormone fragments to assess parathyroid function in any medical condition. The point of our paper was to suggest the possibility of an interaction between parathyroid hormone and a transplanted kidney resulting in primary non-function.

Previously we have used the antiserum AS211/32 to measure the suppressibility of parathyroid glands when infusing calcium into patients undergoing maintenance dialysis. Because of the shortage of this antiserum we used a C terminal assay. We investigated a group of patients who had no residual renal function and who had been on dialysis for varying times. We have not failed to show that the two groups had comparable renal function preoperatively. Perhaps Dr Gunn is confused on this point.

The clearance rate for C terminal parathyroid hormone should be similar in both groups. Furthermore, there should be a substrate-product relation between the parent compound and its fragments. Parathyroid hormone and its various fragments have this relation in end stage renal failure, but it is preferable to measure the intact or N terminal parathyroid hormone when available.

We find it difficult to accept the various points raised by Dr Shaldon. The assay was carried out on samples collected before renal transplantation and the effects of surgery and anaesthesia are not applicable. When a non-parametric analysis (Mann-Whitney test) was used to allow for the few high immunoassayable parathyroid hormone concentrations in the primary function group the difference between the two groups was more significant ($p<0{\cdot}003$, 95% confidence interval of 443 to 2377). The median for the primary function group was 1012 ng/l compared with 2860 ng/l for the primary non-function group.

Factors other than the immunoassayable parathyroid hormone concentration influence the incidence of primary non-function, so it is not unexpected that some patients with primary non-function have immunoassayable parathyroid hormone concentrations overlapping those in the primary non-function group. This does not invalidate our observation that patients with primary non-function had much higher immunoassayable parathyroid hormone concentrations.

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False negative colposcopic cervical biopsy

SIR,—We find it surprising that Dr M Jarmulowicz and others (13 February, p 499) were surprised by our high number of false negative results of colposcopically directed cervical biopsies (16 January, p 172). They expressed an interest in our negative biopsy rate.

During the study 1316 women were referred for evaluation of persistent cytological abnormality of the cervix. A biopsy specimen was taken from 1020 patients, in whom the whole of a colposcopically abnormal transformation zone was visualised. In 986 of these each biopsy specimen was considered adequate for histological assessment. Our paper reported the outcome in 132 of these patients in whom initial biopsy did not confirm the presence of disease. Thus our rate of negative biopsies during the study was 13%. Singer *et al*, reporting from the same unit as Dr Jarmulowicz and his colleagues, quoted a negative colposcopic biopsy rate of 23·9% in a group of patients evaluated in 1982.[1] The same unit now quotes a negative biopsy rate of 4·6% (13 February, p 500). Further comment is not possible because they did not give their false negative rate.

Dr Jarmulowicz and colleagues suggest that a possible explanation for our high negative rate could be the pathologist's inability to recognise, or reluctance to report, "minimal change papillomavirus infection." Details of grade of cervical intraepithelial neoplasia and papillomavirus changes were not given in our short report, but it would be naive to assume that the pathologist who reported these cases, with more than 20 years' experience in gynaecological pathology, ignored or failed to recognise histological evidence of papillomavirus infection. We also find that a high proportion of our cervical biopsy material contains histological features which may reflect the presence of human papillomavirus infection. The evidence implicating koilocytotic atypia as a reliable histological index of papillomarvirus infection is strong, although not conclusive.[2-5] The evidence implicating other "minimal change" histological features such as binucleation, multinucleation, single giant nuclei, and individual cell dyskeratosis is weaker. In our unit these features are reported when present, but we would urge caution in interpreting such features, without koilocytotic atypia, as diagnostic of papillomavirus infection until more conclusive evidence is available to confirm the association.