SIR,—The report to the MRC on the Clinical Research Centre is admirable. It analyses the factors that have prevented the Northwick Park partnership of clinical research centre and district general hospital realising its full potential. A strong recommendation is made to start afresh with the creation of a national clinical research centre based on a service hospital with support from basic science and having a commitment to the postgraduate training of clinical scientists. There seems no alternative to siting such a centre either at the Royal Postgraduate Medical School, which has a very successful record, or at a reconstituted and augmented Northwick Park centre. For various reasons, not least of economy, the former would appear the better choice, but the decision requires detailed analysis.

The main fault at Northwick Park has undoubtedly been the existence of two insufficiently coordinated communities of scientific and NHS clinicians—in brief, one of the many examples of the besetting British sin of compromise. I suppose that as a member of the MRC in the 1960s I must bear a small measure of responsibility for this unhappy state.

Any new venture must have a strong unified control in which the heads of the clinical divisions are clinical scientists. The senior staff must be selected on the basis of high clinical and scientific ability. Less senior staff down to the senior house officer level must participate actively in clinical work as well as undertaking, at the appropriate level, scientific work. Individual remunerative private practice is quite inappropriate to such an institution. Postgraduate teaching is important but must be restricted to a relatively small number of carefully selected students who participate in the work of the centre and who have promise of becoming clinical scientists. This activity could be served by having an establishment large enough to include these students as junior staff members.

The government of this centre will require to be unique. Overall control should be vested in a senior clinical scientist answerable to a small board consisting of a lay chairman and nominees of the NHS, the MRC, and the university. Answerability to parliament and access to funds will have to be arranged. Such a centre will constitute an "Academic Peculiar" in much the same way as Westminster Abbey enjoys unique status as a "Royal Peculiar."

The attempt to implement the proposals in this report will certainly arouse much fierce opposition. The word "impossible" will be on many lips. Many friendships will languish.

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Blood flow in the skin of the foot related to posture in diabetes mellitus

SIR,—We were interested to read the paper by Dr G Rayman and colleagues (11 January, p 87). Although their findings may be relevant to the development of neuropathic oedema, we believe that certain aspects of their paper require some further discussion.

Laser Doppler flowmetry, with which they measured blood flow, is very similar to the long established technique of photoelectric plethysmography, which we have used to study changes in blood flow close to subcutaneous injection sites of various drugs in normal and diabetic subjects.[1-3] It is not clear whether the use of a coherent (laser) light source has any advantages over photoelectric plethysmography, which uses non-coherent visible or infrared radiation.[4,5] Although both techniques are ideally suited to assessing relative changes in superficial blood flow, neither can measure blood flow in absolute units because of individual variation in the optical properties of the underlying tissues, non-uniform fixation of the probe, and so on. Therefore, although it is legitimate to examine relative changes in blood flow within groups of subjects, comparisons of absolute blood flow between groups (as in their fig 2) are of doubtful validity. This is particularly important when one group of subjects is susceptible to factors that could affect the reflected laser Doppler signal; in this case glycosylation of connective tissues or even thickening of the capillary basement membrane in patients with neuropathy might be relevant.

None the less, the laser Doppler flowmetry data were supported by the higher resting skin temperature in the neuropathic patients, as has been reported.[6] It would be interesting to know whether there was any correlation between basal skin temperature and laser Doppler output, and also whether skin temperature was affected by dependency of the legs in the same way as the laser Doppler data.

It is important to know whether these findings were reproducible and consistent, as measurements of superficial blood flow are notoriously susceptible to interference from many sources.[7]

As the authors point out, laser Doppler flowmetry (like photoelectric plethysmography) is sensitive to total blood flow—that is, that in the capillaries and that bypassing the capillary bed through arteriovenous anastomoses. Changes in capillary flow cannot be distinguished from changes in shunted flow, and any discussion based on these studies about the contribution of shunting to apparent increases in flow can therefore be only speculative.

A minor point is that the changes in blood flow may not be restricted to the skin; as far as we know the precise range of the laser Doppler probe has not been established, but photoelectric plethysmography seems to be sensitive to changes in blood flow in subcutaneous tissue as well as in the skin.[2]

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**The authors reply below.—ED, *BMJ*.

SIR,—We thank Drs Williams and Pickup for their interest in our recent paper and for the opportunity to clarify the advantages of laser Doppler flowmetry.

Laser Doppler flowmetry and photoelectric plethysmography are similar in that both techniques rely on back scattered light. The fundamental difference is that the laser Doppler flowmeter produces a signal proportional not only to the number of red cells in the volume of tissue that the light penetrates but (in contrast to photoelectric plethysmography) also to their integrated velocity (derived according to the Doppler principle from the frequency shift in the back scattered light).[1] The signal is thus proportional to the flow rate of red cells, an assertion that until now has relied on data obtained in vitro by passing blood of different packed cell volumes and at different velocities through models of the microcirculation.[2] Recently, however, Tyml and Ellis have shown in vivo that flowmeter output correlates well with temporal variations in superficial microvascular flow by simultaneously assessing red cell perfusion directly using videomicroscopy.[3] Furthermore, synchronous measurement of laser Doppler flowmeter output and capillary red cell velocity by videomicroscopy gives broadly comparable results in response to a variety of physiological stimuli.[4] In contrast, there is no similar validation, using direct techniques for determining microvascular flow, for photoelectric plethysmography. Of more practical concern is the fact that photoelectric plethysmography does not mirror changes in rate of blood flow produced by venous occlusion or dependency, the response we were studying. The congestion of the tissue segment—that is, the increase in the number of cells contained but not their velocity—results in a higher plethysmographic signal, whereas all other validated techniques show a fall in blood flow in response to such manoeuvres.

We accept that laser Doppler flowmetry output cannot yet be expressed in absolute units of blood flow, but it is difficult to believe that the data in fig 2 in our paper do not represent higher blood flow in the diabetic group, firstly because of the degree of difference observed in the light of the inherent variability in skin blood flow outlined by Drs Williams and Pickup; secondly, as skin temperature was higher in the diabetic group; and, thirdly, in view of the likelihood that the imaginative effects of the proposed changes in tissue would result in a lower rather than a higher signal.

Resting skin temperature was correlated with $\log_{10}$ of laser Doppler flowmeter output ($r = +0{\cdot}82$, $p<0{\cdot}001$). Skin temperature fell on dependency in both groups, but the difference in fall was not significant, not surprisingly, as skin temperature lags behind changes in blood flow.

We are aware of the psychic and environmental influences on skin blood flow, and painstaking efforts were made to ensure comparable conditions. A measure of reproducibility is provided in the paper, the normal subjects who were indirectly heated constituting a second control group. Before heating their percentage fall on dependency was virtually identical with that achieved in the first control group (20·9% (SD 13%) *v* 18·1% (SD 11·9%)). Further studies, as yet not submitted for publication, confirm the impairment we have shown in the diabetic groups.

Drs Williams and Pickup are right to emphasise that our data do not tell us whether the higher blood flow seen in the diabetic group represents capillary flow or shunted thermoregulatory flow, or both. We are repeating the study and synchronously measuring toe nailfold capillary flow velocity by television microscopy and laser Doppler flowmetry, which should clarify this important issue. The consequences of either component being increased would be the same—namely, increased capillary pressure on dependency—the potential mechanism of oedema formation and the main point of our paper.