

## Medical academia is failing patients and clinicians

### The neglect of basic observational clinical research

**Stroke Prevention Research Unit, University Department of Clinical Neurology,  
Radcliffe Infirmary, Oxford OX2 6HE**

Peter M Rothwell

*professor of clinical neurology (peter.rothwell@clneuro.ox.ac.uk)*

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[193;38;155;587;171](#)

The need to facilitate clinical research is widely recognised[1] [2]—particularly translational “bench-to-bedside” research and randomised controlled trials of interventions. Indeed, the influential report of the Academy of Medical Sciences on *Strengthening Clinical Research* concentrated purely on these two areas.[2] However, there is another even more neglected field of clinical research, which arguably has greater potential to improve clinical outcomes, certainly in the short term: basic observational research necessary for effective clinical practice.

It is common to find, for example, that although much is known about the molecular pathology of a condition, little is known about the reliability of clinical diagnosis, or about prognosis and the factors that affect it. Yet, these issues are of most concern to patients and clinicians: How certain can you be about the diagnosis, doctor? What are the risks of a poor outcome over the next few weeks/months/years? How do my particular characteristics influence the likely outcome? The trial showed that this treatment is likely to do more good than harm on average, but are the risks worth taking in my case?

The report by Whiting and colleagues of a systematic review of studies of the diagnostic/prognostic value of magnetic resonance brain imaging (MRI) in patients with possible multiple sclerosis illustrates the problem (p 875).[3] In agreement with a previous systematic review,[4] they found many small studies with poor methodology and inadequate follow-up, and they conclude that the prognostic value of MRI has been overestimated. Although they did not have individual patient data and so could not stratify results by important covariates such as age or fully take into account the effect of the number and location of MRI lesions, and accepting that imaging technology has evolved during the past two decades, the findings should nevertheless worry patients and practising neurologists.

Many other examples exist in neurology alone where a lack of basic data on diagnosis or prognosis is the main barrier to effective treatment in routine practice. For example, we know

little about the reliability of clinical diagnosis of transient ischaemic attack (TIA) and virtually nothing about the long term prognosis of several common TIA-like presentations, such as isolated vertigo, isolated diplopia, or transient speech arrest.[5] Some clinicians treat these events as TIAs, possibly unnecessarily exposing patients to potentially dangerous interventions, such as carotid surgery or warfarin, whereas others do not diagnose TIA, possibly missing an opportunity to prevent stroke. Moreover, until very recently no reliable data existed on the early risk of stroke in patients with definite TIA. A risk of 1-2% at 30 days was widely quoted, and clinical services were organised accordingly, but it is now clear that the true risk is about 10% at 7 days[6] [7] and that simple risk scores can identify individuals with a 7 day risk of up to 30%.[8] Armed with these basic data, clinicians can now begin to treat patients appropriately.

Other recent examples in neurology of the enormous value of such basic observational research include the first ever large study of the accuracy of early recognition and diagnosis of meningococcal meningitis,[9] and the first reliable data on the risks of major congenital malformations due to different antiepileptic drugs in pregnancy.[10] Many other equally important and tractable clinical questions remain unanswered.

It is hardly credible that the basic observational research described above, which could and should have been done decades ago, is only now being published. In each case, countless patients have suffered from incorrect diagnoses and delayed or inappropriate treatment. If the research necessary to improve diagnosis and prognostication is easy and relatively cheap to do, which it usually is, then failure to gather the necessary data is indefensible. It would, for example, be inexcusable for the airline industry not to bother studying the diagnostic and prognostic reliability of routine aircraft safety inspections. Yet we fail to apply the same standards in medicine. Remarkably, the neglect of this research element of our duty of care is greatest in the case of potentially dangerous interventions. We know next to nothing, for example, about the predictors of major bleeding on warfarin, or the characteristics that make it likely that the benefits of endarterectomy will outweigh the risks in a patient with asymptomatic carotid stenosis.

The lack of basic clinical research on the issues that matter most to patients and practising clinicians inevitably calls into question whether medical academia, as currently constituted and funded, is properly fit for purpose. Basic biological research and bench-to-bedside translation are obviously important, but why has so much critically important basic clinical research not been done? Whatever the causes (some possible ones are given in the box), medical academia must improve its performance or, less preferably, be forced by politicians

to prioritise appropriately. The recent emphasis on the development of clinical research is welcome,[2] as are the recent UK Department of Health proposals for future research funding,[11] although there are potential pitfalls.[12] Greatest of these is the tendency for clinical research to be defined too narrowly as being only bench-to-bedside translational research, large scale epidemiology, and pharmaceutical trials, with the lowest hanging fruit—observational research necessary for effective clinical practice—continuing to be neglected.

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### **Why does academia neglect basic clinical research?**

Too few practising clinicians in decision making positions in academia

Non-clinical or non-practising researchers underestimating the need

Underestimation of the immediate improvements in outcomes such research can produce

Overoptimistic expectations of the clinical benefits of basic biological research

Loss of traditional teaching hospital consultants (doing research based on their routine practice)

Loss of the capacity of NHS to facilitate long term follow-up studies

Increased bureaucratisation of clinical research

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Conflict of interest: None declared.

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