

necessary to measure small effects. The logistics of such trials, may impede the recording of results other than short-term survival. Yet the rationale of thrombolysis for older patients with myocardial infarction, for example, includes the hope of saving cardiac muscle to reduce the subsequent risk of chronic heart failure – a state more miserable than many people realise.

Unfortunately, once a credible trial showing a reduction in fatality has been published, placebo-controlled trials to measure other neglected outcomes become impossible. Attempts to pursue Dr McGouran's implied question of how to identify which patients will benefit, require *post hoc* subgroup analyses and are usually forbidden, even though, if treated as hypothesis-generating rather than hypothesis testing, they may prove illuminating¹. 'Trialists' have come to comprise a definable scientific specialty with a powerful statistical base. There are sound practical reasons for this, but if they cannot be persuaded to adapt their methods to ask the right questions, clinicians will be left without the right answers.

Reference

- 1 Rothwell PM, Warlow CP (on behalf of the European Carotid Surgery Trialists' Collaborative Group). Prediction of benefit from carotid endarterectomy in individual patients: a risk-modelling survey. *Lancet* 1999;353:2105–10.

JOHN GRIMLEY EVANS
University of Oxford

In response

Editor – I am grateful to Dr Daggett for his comments. I have personally received a number of letters and e-mails since the article was published, many along the lines of his letter. All have supported my concern that 'evidence based medicine' pushes us into applying population proven treatments to individuals, and that individuals suffer as a consequence. None have endorsed the opposing view. I have noticed, however, that most of the writers are of my generation and I have been a consultant for 20 years.

Data described as 'evidence based' achieve scientific respectability, when all it means is that mathematical manipulation of the figures has produced a number

previously agreed to indicate statistical significance. The important observation, as Professor Grimley Evans points out, is whether most patients will benefit from the treatment in one way or another, but evidence based analysis stops at the *p* value, usually for a single outcome. It is easy to see, in these days of doctor bashing, litigation and guidelines, how the *p* value offers a chance of a peaceful night's sleep, but first we have to forget that our primary responsibility as doctors is to our patients and not to ourselves.

Dr Jackson feels there is a better chance of benefiting from thrombolysis than winning the lottery. We should certainly hope for more favourable odds for our patients than ten million to one against, but the purpose of my paper was to question why we have moved in half a century from recommending treatment shown to benefit most patients to treatments shown in sophisticated trials to benefit only a tiny minority.

I suspect, as Dr Daggett points out, that many older physicians feel that they have been manoeuvred into prescribing drugs that they would not take themselves, and we should wonder whose puppets we have become.

RORY MCGOURAN
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Neuromythology

Editor – Thank you for publishing the views of Professor Tallis (*JRCPL* November/December 2000 pp563-7). This is a remarkably provocative article, although the pseudoscientific wrapping may have disguised the radical anti-scientific nature of the underlying message.

Professor Tallis does not believe that the mind is a function of the physical phenomena occurring in the brain and he derides as neuromythology the currently limited but ever improving scientific approach to the understanding of intelligence and consciousness.

Many of his arguments are fallacious. The problem of deriving variety from monotonous regularity does not exist. There is nothing more monotonous than the binary code that forms the basis of all computer language, yet this code can be

translated into a chess game adequate to beat all but the greatest masters.

He appears to suggest that the ability to do two things at once (avoid a bicycle whilst crossing the road) refutes any physically based theory of brain function. My cats can perform this task, possibly more dextrously than Professor Tallis, but how can this be? He does not believe conventional neuroscience can explain such actions, but reserves the magical characteristics of mind (and soul, whatever that is) for humans.

His assault on the neural theory of consciousness as savagely impoverished is quite breathtaking. Without providing any testable alternative, he locates consciousness not only outside the brain but apparently outside the physical world. I suppose this has a degree of internal consistency, in that there is no point in discussing in a scientific and rational way a belief which lies beyond the scientifically describable universe.

There is a problem whenever one tries to combine science and religion. Science in general and evolution in particular are highly convincing in their explanation of the observable universe, with no other rival in sight. Religion is not required to explain anything. I would refer interested parties to the same source as Professor Tallis' first reference – Daniel Dennett, and in particular his wonderful (if rather complex) book about evolution and the meaning of life¹.

Reference

- 1 Dennett DC. *Darwin's dangerous idea*. London: Penguin, 1995.

JOHN MAIN
*Consultant Nephrologist
Middlesbrough*

In response

Dr Main's critique of my Fitzpatrick lecture asserts that I am hostile to science. As is made plain in the final paragraph of my paper, it does not follow from my belief that neuroscience cannot explain human consciousness, that I am anti-science or even anti-neuroscience. Given that I am the author of a large number of scientific papers, it would be very odd if I were. I have, moreover, written extensively – and passionately – in defence of science against its detractors^{1,2}. What I oppose is scientism

– the belief that science can explain everything and that it can even answer metaphysical questions.

We may, therefore, turn to Dr Main's substantive criticisms of my paper. He argues that the monotonous nervous system could be the basis of the infinitely varied experience we have of the perceived world because it could 'encode' complex information in the way that monotonous binary codes of the computer do. This is a variation on the 'patterns' argument that I dispose of in my paper. The electronic activity that takes place counts as a code only if one assumes that there are conscious humans who (ultimately) decode it as meaningful information and outputs. The flow of electrons in the circuitry of 'Deep Blue' counts as chess-playing moves only in a world where conscious people, including chess masters, are already in place. Codes are second-order derivatives of consciousness and cannot, therefore, explain it. Dr Main's use of the computer analogy shows that he is still in thrall to the 'language of neuromythology' which my paper also exposes³.

Only a careless reading of my paper could conclude that I was arguing that 'the ability to do two things at once...refuted any physically based theory of brain function'. My argument is much more complex; namely that our multi-agenda-ed, multi-levelled consciousness – in which vast numbers of things have both to be kept separate and brought together (in the unified moment of consciousness) and provide each other's frame of reference (what Daniel Dennett in one of his papers recognised as the unsolved 'frame problem') – cannot be captured in neural activity. While the bicycle-avoiding skills of Dr Main's cat are admirable, I doubt that the beast could take on his/her master's nephrological duties; if he/she could, I would like to meet him/her.

Dr Main criticises me for not providing my own theory of consciousness. I willingly plead guilty to honesty. Like me, he doesn't have a 'testable' theory of consciousness. My aim in my lecture was simply (to borrow John Locke's words) that of 'removing some of the rubbish that lies in the way of knowledge' and not that of producing a theory of consciousness of my own.

Like Dr Main, I am a fan of Charles Darwin. But if Dr Main had read a couple of my books^{3,4} he would see why unquestioning faith in evolution as an explain-all may be misguided. One doesn't have to be a Bible Belt Creationist to dissent from the notion (to which Darwin would not have subscribed) that 'science in general and evolution in particular' account for the observable universe. For there is something else in that universe that they do not explain: the observer. More specifically, materialist science cannot explain why there are such things as scientists and how it is that their science is so effective.

References

- 1 Tallis, R. *Enemies of hope: a critique of contemporary pessimism. Irrationalism, anti-humanism and the contemporary counter-enlightenment (2nd ed)*. London: Macmillan, 1999.
- 2 Tallis, R. *Newton's sleep. Two kingdoms and two cultures*. London: Macmillan, 1995.
- 3 Tallis, R. *On the edge of certainty*. London: Macmillan, 1999.
- 4 Tallis, R. *The explicit animal: a defence of human consciousness (2nd ed)*. London: Macmillan, 1999.

RAYMOND TALLIS
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University of Manchester

Cancer services

Editor – I was saddened but perhaps not too surprised to find that nowhere in his editorial (*JRCPL* November/December 2000, pp515–6) does Professor Malpas mention the vital role of clinical haematologists (haemato-oncologists) in the cancer care delivery system and the ever expanding role they are expected to play to make the necessary improvements in cancer care in the UK.

A substantive proportion of lymphomas and most leukaemias are cared for by clinical haematologists. The survival figures of MRC and BNLI trials are at par with any published figures across the globe. Many clinical haematologists in the UK are serving as lead cancer clinicians.

Sadly the pivotal role of clinical haematologists has often been ignored, as the editorial reflects, and it is about time for our role to be recognised and acknowledged.

S BASU
Consultant Haematologist
Warwick Hospital

Clinical & Scientific letters

Letters not directly related to articles published in *Clinical Medicine* and presenting unpublished original data should be submitted for publication in this section. Clinical and scientific letters should not exceed 500 words and may include one table and up to five references.

The nurse endoscopy scene

Editor – The new NHS plan for reform indicates that future nursing roles will include the performance of minor surgery and endoscopic procedures¹. The nursing profession and the British Society of Gastroenterology have established a framework to support the growth of the nurse practitioner to include that of an endoscopist^{2,3}. We determined the prevalence and range of procedures currently practised by nurse endoscopists in the UK, as well as the attitude of their lead endoscopy clinicians. A postal questionnaire was sent in July 2000 to all lead clinicians in endoscopy units of UK district general (DGH) and teaching hospitals (TH) with accident and emergency, general medical and general surgical services (n=292); 176 responded (60%). Fifty-five DGHs and 21 THs employed 102 nurse endoscopists with a mean 1.3 (1–3) per hospital with a further 19 (7 TH, 12 DGH) undergoing training. Forty four (43%) performed both upper gastrointestinal endoscopy (OGD) and flexible sigmoidoscopy; solitary OGD and flexible sigmoidoscopy were performed by 17(17%) and 31(30%) respectively. Three (3%) performed full colonoscopy while 7(7%) performed all three procedures. Diagnostic procedures were also performed and included injection of ulcers (4%), dilatation of strictures (3%), PEG tube insertion (2%) and polypectomy (13%). Patient acceptability was positive in 87(89%) of units. Nurse endoscopists were integral in contributing to the reduction of waiting