



## TO THE EDITOR...

Letters should be marked clearly 'Letter for publication in *The Psychologist*' and addressed to the editor at the Society office in Leicester. Please send by e-mail if possible: [psychologist@bps.org.uk](mailto:psychologist@bps.org.uk) (include a postal address). Letters over 500 words are less likely

to be published. The editor reserves the right to edit, shorten or publish extracts from letters. If major editing is necessary, this will be indicated. Space does not permit the publication of every letter received. Letters to the editor are not normally acknowledged.

## The root of all evil?

**D**AVID Miller and Greg Philo illustrated a number of issues psychology should be addressing in their article 'Silencing dissent in academia: The commercialisation of science' (May 2002). Their invitation to discuss the impact of commercial science on our discipline is timely, given parallel debates elsewhere (Concar, 2002). In particular, we need to focus on the issue of what is funded, by whom, and what they do with our research. Unfortunately Miller and Philo's definition of what

constitutes 'commercialisation' was too broad and at times confusing, which detracted from the very real concern about the way corporations are remaking how we see health, well-being and 'normality'.

At a time when funding is scarce, it is not surprising that people accept corporate funding of research (myself included). And with companies now offering funding to areas that have a lengthy history of neglect, it's hard to say 'no' to money. For example, in my specialty (sex research), we've been ignored, patronised and

underfunded for decades, so it's not surprising people now jump at anything that can pay and make them feel validated at the same time. Unfortunately there's a price to pay for this, the main one being that certain corporate sources will use the work that paid us and made us feel worthwhile to make claims beyond the data, distort results, and gag academics. Although Miller and Philo did acknowledge these problems, they could have gone further to offer some practical ideas about how we might do something about them.

Their attempt to cover too many issues at once also confused the very serious issue of bias and research funding. Their move to include swipes at both psychologists in the media and contemporary social science debates meant it was unclear what part of 'commercialisation' we should take issue with. Talking to the media is not, after all, the same as being funded by a company who is using you to do their PR, thinly disguising it as research. True, the rise of 'reality TV' and the treatment of participants in these shows do need to be addressed by the BPS, but I feel this is a separate topic to that raised by their original theme of 'commercialisation of science'.

Appearing in the media doesn't necessarily mean

'commercialisation'; indeed, if we are being required to bring psychology to society, I am not sure how else we are meant to do this. The BPS could improve the way it supports and monitors psychologists who deal with the press, yet Miller and Philo didn't particularly add to that issue.

More worrying, given their suggested expertise, was their association of commercialisation with what they dismissed as the 'dead ends of postmodern psychology'. I find it astonishing that they made this claim, given the positive impact those utilising postmodern approaches have made to highlight and campaign against biased corporate-funded research (see Kaschak & Tiefer, 2001; and [www.fsd-alert.org](http://www.fsd-alert.org)).

We need to deliver research that is not just of a high standard, it also should be honest. I feel we should now build on the ideas presented by Miller and Philo to ensure that our research is more transparent, and to campaign for better funding for what we do.

**Petra Boynton**  
*Department of Primary Care and Population Sciences*  
*University College London*

### References

- Concar, D. (2002, 16 March). Corporate science v the right to know. *New Scientist*, pp.14–16.  
Kaschak, E. & Tiefer, L. (Eds.) (2001). *A new view of women's sexual problems*. New York: Haworth Press.

**M**ILLER and Philo made some excellent points in their recent article ('Silencing dissent in academia: The commercialisation of science', May 2002). Whilst I didn't agree with all of it – I don't think, for example that post-structuralist critical research is always a dead end – what they say has major implications for the place of psychology in teaching, training and research.

One area they identify is the influence of commercial and other interests in research. There is huge involvement of drug companies in funding research and conferences in medicine and psychiatry, which has led to enormous criticism and actions like the demonstration outside last year's Royal College of Psychiatrists Annual Conference by critical mental health professionals, Mad Pride and other users of mental health services. In September the *British Medical Journal* and another 13 of the world's leading medical journals published strongly worded editorials outlining steps they were taking to limit the influence of drug companies on research. The *British Journal of Psychiatry* now includes a space for 'declaration of interests' at the start of every article published. But how does psychology fare?

When I rang to enquire what was the BPS policy, I was passed from person to person. There did not seem to be a coherent Society policy on this or, if there is, it is carefully hidden. The Society should develop a clear and well-publicised policy on the limiting of commercial and other interests in all spheres of psychology, including research.

**Dave Harper**  
*School of Psychology*  
*University of East London*

See p.342 for further discussion on 'media ethics', and p.346 for a 'Counterpoint' to Miller and Philo's article.

## Support for EMDR

I AM glad to see EMDR discussed within these pages ('In the blink of an eye', March 2002; 'Counterpoint: Emperor's new clothes?'; and Letters, May 2002). Before adding my comments, I'd like to make two general points.

First, when asked to justify my therapeutic practice to reasonable fellow citizens it is not my current finger-waving

which embarrasses me but my long-ago participation in aversion therapy for gambling and token economies for anorexia. Both treatments were trumpeted as based on sound scientific principles. I am sorry I believed such garbage and ashamed I took part. One day I may decide EMDR is garbage too, but I doubt I'll need to be ashamed of having done it.

WRITE in reply to Robert A. Forde's amusing but misguided parody of EMDR (Letters, May 2002). I would encourage him to provide some initial strong evidence of RUNT's (rolled-up newspaper therapy) clinical efficacy and research status. Then I would be interested to begin my own investigation, as progress in psychotherapy, and better results for our patients or clients, may be at stake.

I would want to concentrate on the most tightly designed, controlled research studies, and to investigate whether RUNT has been endorsed by well-respected and renowned scientists in various related fields such as psychiatry, neurobiology and trauma, and by influential institutional bodies. I would be alert to the views of proponents of psychoanalysis, CBT, attachment theory, etc. who had looked into the new approach in depth, and had experience in its use. The views that would carry most weight would be those of clinicians and researchers known to have approached RUNT with at least the same degree of scepticism that Forde has applied to EMDR, but, of course, with a fair and open mind. There is a difference between scepticism and obscurantism. I would also want to take a good look at its theoretical basis, current debate, and areas of ongoing and proposed research.

If I found myself satisfied to a high degree on these fronts, I would like to learn more and to experience RUNT for myself, first as client and then cautiously as practitioner. If the results seemed favourable, I would want to continue further, still with an open mind and perhaps join the body of researchers. I would recognise that RUNT would already have come at least as far in providing a rationale than any other method of practising psychotherapy. I would recognise that RUNT would have needed to provide more evidence of its effectiveness than is usually required in the psychotherapy arena, as it appears so unusual. The biggest risk to a practitioner may be one of ridicule from one's peers. Unfortunately, if one is not willing to take that risk, sufferers of the ravages of PTSD and other miseries may lose out.

I believe EMDR is way ahead of your RUNT, Mr Forde.

For those with open and curious minds, see [www.emdria.org](http://www.emdria.org) and [www.emdr-practitioner.net](http://www.emdr-practitioner.net).

**Nel Walker**

56 Melrose Place  
Watford

Second, I am a long-time fan of both Occam's Razor and Sagan's baloney detection kit, and never venture into foreign conceptual territory without them. But I am equally mindful that our natural attachments to the familiar may prompt us to refuse the venture altogether, thereby avoiding the bother of challenging our habitual, cherished notions and practices. Parody, for example, is an excellent means of deflating grandiosity: but it also enables nimble-wittedness to masquerade as intellectual incisiveness. Other much-loved ways of avoiding new ideas are 'It's only common sense really', 'You must think I'm daft' and 'Of course, we've all been doing it for years already'. Sound familiar?

I must admit that when I first read about EMDR I was horrified, disbelieving and even disgusted at the idea that severely traumatised people should be obliged to put up with such self-evidently absurd shenanigans. Six years on I know if I ever need treatment for a severe trauma reaction, I will seek EMDR as a key component of that treatment.

Of course it does not help in

every situation. Nothing does. As always it would be good to know what are the crucial variables which differentiate those for whom it does absolutely nothing and those for whom it does the trick. Until we know, I will continue to use it with people referred to me after cognitive therapy, exposure and person-centred counselling have been unable to help them conquer their intrusive memories and trauma-generated negative self-schemas.

Not infrequently the shift from past-fixated self-loathing to future-minded self-affirmation during a session is so elegant and apparently inevitable (one client called it 'the melting of the glacier') that the client and I are both left bemused, and grateful. Such jewelled moments may of course happen at any time, in or out of therapy, but they seem more common during EMDR sessions than any other kind of meeting. Put simply, the procedure has transformed my practice.

**Brenda Roberts**  
*Hove Polyclinic*  
Hove  
East Sussex

STEPHEN Joseph ('Counterpoint: Emperor's new clothes', May 2002) responds critically to Shapiro and Maxwell's March article on EMDR. Unfortunately this article contains inaccuracies, omissions and recycled polemic that do not address the questions posed by the EMDR literature.

Joseph says that the International Society for Traumatic Stress Studies (ISTSS) guidelines recommending EMDR as an effective treatment for PTSD, are based on EMDR being compared with no treatment. Wrong. The 'active control'

section of the guidelines lists four studies, all showing significant advantages for EMDR over its active comparisons. Joseph cites the Davidson and Parker meta-analysis showing EMDR is as effective as exposure. In that case his comparison of EMDR's effectiveness to Mesmer's animal magnetism therapy must also apply to exposure.

Joseph quotes McNally as stating that 'the novel component of EMDR (eye movements) adds nothing to the traditional imaginal exposure component'. Leaving aside a weight of evidence that eye movements do contribute to reductions in PTSD distress and

imagery (e.g. Andrade *et al.*, 1997) – a fact partly acknowledged by Joseph later on, EMDR is the *antithesis* of ‘traditional exposure’. In EMDR exposure is short and dosed and incorporates a free associative element alien to exposure. A whole body of research calls for *prolonged* exposure in cognitive-behaviour treatments, and indeed, according to Marks *et al.* (1998), EMDR should sensitise rather than desensitise its recipients. If EMDR is exposure therapy, then psychoanalysis must be too. As Marks noted, ‘many therapies can be equally effective – it does not mean they are caused by the same mechanism’.

Joseph quotes the Davidson and Parker meta-analysis as suggesting that EMDR is *no more* effective than exposure. He ignores the larger meta-analysis in this area (Van Etten & Taylor, 1998) looking at all PTSD treatments, and concluding that ‘EMDR is an effective psychotherapy for PTSD and is more efficient than other treatments’. In fact, several recent studies show greater efficiency for EMDR in direct comparisons with exposure (Ironson *et al.*, 2002, is the latest).

Finally, Joseph revives the old argument that EMDR is ‘pseudoscience’ – because it was introduced ‘without the

appropriate evidence’. EMDR was introduced in 1989 as an experimental procedure – until 1995, after multiple studies had been carried out. There are currently more studies on EMDR and PTSD than any form of cognitive behaviour therapy, which is why the ISTSS, the American Psychological Association and Department of Health have acknowledged it. Those whose approaches are challenged by EMDR’s evidential success should focus arguments and research on why a procedure so alien to prolonged exposure is so effective and efficient, and not seek to diminish it by recycling spurious claims and comparisons.

Some clues to EMDR’s effectiveness may lie in the independent work of Andrade *et al.* (1997) and replicators, showing that eye movement

reduces the vividness of emotive imagery in trauma clients. Clues may also lie in exciting new neurobiological work (e.g. Stickgold, 2002).

**John Spector**  
Consultant Clinical Psychologist  
Watford General Hospital

**References**

Andrade, J., Kavanagh, O. & Baddeley, A. (1997). Eye-movements and visual imagery: A working memory approach to the treatment of post-traumatic stress disorder. *British Journal of Clinical Psychology*, 36, 209–223.

Ironson, G.I., Freund, B., Strauss, J.L. & Williams, J. (2002). Comparison of two treatments for traumatic stress. *Journal of Clinical Psychology*, 58, 113–128.

Marks, I., Lovell, K., Noshirvani, H., Livanou, M. & Thrasher, S. (1998). Treatment of PTSD by exposure and or cognitive restructuring, a controlled study. *Archives of General Psychiatry*, 55, 317–325.

Stickgold, R. (2002). EMDR: A putative neurobiological mechanism of action. *Journal of Clinical Psychology*, 58, 61–76.

Van Etten, M. & Taylor, S. (1998). Comparative efficacy of treatments for PTSD. *Clinical Psychology and Psychotherapy*, 5, 126–144.



No easy ride for students

**J**AMES Saunders ('Approval disapproved', April 2002) appears unnecessarily dismissive of the difficulties faced by some students in gaining two signatures to support their applications for membership to the Society. As Chair of the Student Members Group when this proposal was introduced, I think it's appropriate to say why so many students have faced problems.

First, not all students attend an institution, many pursue their courses by distance learning – the Open University being an example. It is evidently not so easy for them to find two signatories when they don't attend 'a department full of psychologists'.

Secondly, perhaps Saunders was not aware

that to support an application, the signatories must themselves be members of the Society. Unfortunately it is not as easy as one might imagine to find two Society members in any given department.

Thirdly and perhaps most importantly, the SMG regarded this as an unnecessary barrier to membership. We hope this has been overcome now this more sensible system has been introduced, replacing the need for two signatures with a department 'stamp'. Given the Society's aims to boost membership to 50,000 by the year 2006, any impetus to that endeavour should be welcomed, I suggest.

**P. Alex Linley**  
Department of Psychology  
University of Warwick

Researching complementary medicine

**A**S a psychologist and chairman of the Research Council for Complementary Medicine (RCCM), I read Adrian Furnham's article (May 2002) with interest. I support the view that psychologists have important skills and experience that can contribute to research in complementary and alternative medicine (CAM). I would also suggest that this extends to the teaching of research skills for the development of research-aware and competent CAM practitioners. Research in CAM requires investment, the development of a research community and the dissemination of research literature.

The Department of Health is beginning to address the investment issue by funding a number of research posts through the recently announced National Award Scheme ([www.doh.gov.uk/research/rd3/workforcecapacity.htm](http://www.doh.gov.uk/research/rd3/workforcecapacity.htm)). The RCCM has established a CAM Researcher Network, a 'virtual' community of people from a range of disciplines who can share methodological experience and provide a database of 'expertise' for journal and funding council reviews. The RCCM Centralised Information System in Complementary Medicine database has a specialist thesaurus of CAM terms which makes searches for research more comprehensive and specific.

The work of Adrian Furnham and his colleagues will make a significant contribution to the published research in complementary medicine. But access to such research and the development of a CAM research community are paramount in this exciting and developing area.

**Janet Richardson**  
Research Council for Complementary Medicine  
27a Devonshire Street  
London W1

## Incompetence of the competence industry

**T**HE anonymous letter headed 'Too much power already?' (February 2002) points to one of the most pervasive problems in modern society, namely how to deal with professional incompetence. A couple of years ago *The Psychologist* published a short article of mine highlighting some of these issues in relation to education and counselling ('Ethical dilemmas', August 2000).

The problem of professional incompetence has been thrown into relief in two chapters (by Tony Becher and Irene Ilott) in a recent book. Becher gives a dramatic example: 'The anaesthetist (prospective whistle-blower) was sacked; the surgeon was allowed to go on

killing people' (Becher, 2001) Peter Faire underlines much the same set of issues in connection with his stepson's dyslexia in the March issue of *The Psychologist*. No one who has any local or central government money is allowed to criticise the doings of their fellows or those of the government or the local authority in public.

It is this observation and fear of pervasive professional incompetence, coupled with the feeling of futility of trying to do something about it on an individual basis, that has fuelled the vast multibillion dollar international competence specification and testing movement.

Unfortunately, this movement entirely fails to address the kinds of



### How to deal with professional incompetence?

incompetence that people are concerned about. The components of competence that are insisted upon and checked in the course of these activities are not the obverse of the kinds of incompetence that people are concerned about. It is impossible, through the arrangements that are being

promoted, either to nurture high-level competence or to constrain the pervasive incompetence that so many of us are so acutely aware of. Yet the specification and development of the organisational arrangements and staff-appraisal procedures that are required to raise levels of competence is a job for psychologists. It is one indication of our pervasive collective incompetence that, by and large, we have not been doing that job.

**John Raven**  
30 Great King Street  
Edinburgh

#### Reference

Becher, T. (2001). The incapable profession. In J. Raven & J. Stephenson (Eds.) *Competence in the learning society*. New York: Peter Lang.

## Dyslexia – Children still lost in the maze

**A**S a dyslexic parent of dyslexic children, Chair of my local Dyslexia Association and a qualified dyslexia tutor, my reaction to the article 'Dyslexia – Seeking help to negotiate the maze' by Peter Faire (March 2002) contrasted markedly with the reaction of 'surprise' described by Kairen Cullen (Letters, May 2002): Faire merely catalogued my daily experience of this maze.

Cullen's first point is that working in partnership with parents, as encouraged by the new Code of Practice, is central to the educational psychologists' role. In fact, the new Code dangerously places increased responsibility with our schools for identification of, and provision for, special educational needs despite the fact that few teachers, as Faire noted, have any expertise in the area of dyslexia. The child is only likely to see an educational psychologist after experiencing severe 'failure' over a protracted period. By the

time of the first meeting, the whole family's levels of misery, fear and stress are already often painfully high rendering a true 'partnership' unlikely.

Once this first meeting date finally arrives one should indeed hope that the issues would be taken as seriously as Cullen believes. However, the 1999 DECP working party report, though it may well have been the result of much serious consideration, offers a definition of dyslexia that is woefully inadequate, being based solely on difficulties with 'accurate and fluent word reading and/or spelling' that are 'severe and persistent'. As pointed out by Ann Cooke (2001), this definition militates against early identification and any prospect of proactive prevention of failure, while potentially reducing numbers quite drastically, but artificially, by excluding the many children and adults with dyslexia who read quite well 'at word level'. Can we be expected to take seriously a definition akin to

'word blindness' that the last 30 years of research has shown to be grossly inadequate.

Finally, Cullen refers to the 'complexity and breadth' of the assessment process and calls for 'thorough' assessing, but this is surely rendered almost redundant by a minimal definition that makes no reference to aetiology; there is rarely much evidence of either complexity or thoroughness in the LEA reports I see, which often do little more than quantify low attainment.

More importantly, the 'simple and limited' question of 'does this young person have dyslexia or not?' is precisely the one that needs answering (even 'appears to have difficulties which may be consistent with a diagnosis of dyslexia' would do!) before we can start to provide some useful help and rebuild a child's self-esteem. False positives are barely an issue as there are, to my knowledge, no documented cases of damage being caused by a multisensory programme

geared to individual need and a bit of dyslexia-friendly classroom teaching. False negatives however, even if only implied by equivocal psychobabble, can cause untold damage. The wish to avoid labelling is also a poor defence when we all know that the classic alternatives to dyslexia are 'lazy', 'thick' or both.

As Lindsay Peer comments (Letters, May 2002) educational psychologists are, of all the professions, best placed to lead improvements in our provision for dyslexia. If they don't respond soon, I'm afraid they will be sending an open invitation to the lawyers to take over the job.

**Jennie Murray**  
19 Paris Avenue  
Newcastle  
Staffordshire

#### Reference

Cooke, A. (2001). A view from the chalkface: A critical response to the British Psychological Society Working Party on Dyslexia. *Dyslexia: An International Journal of Research and Practice*, 7, 47–52.

## Barking up the right tree on decision making

**D**IANA Barker (Letters, May 2002) correctly points out the importance of tracking outcomes after a decision has been made. Contrary to implication, though, this has not been ignored by psychologists. Rather, it was not something that Clare Harries and I chose to emphasise in the special issue on decision making ('Decisions, decisions', February 2002).

However, I am mystified by the assertion that psychologists should focus on tracking rather than 'barking up the wrong tree

trying to 'rationalise' the decision-making process'. This statement ignores several points that Clare Harries and I made in our article 'How rational are we?' (February 2002).

First, timely and accurate outcome feedback is often unavailable (e.g. jury decision making), which is an obvious major problem for tracking. Second, contrary to trying to 'rationalise' the decision-making process, much of our article reviewed evidence for the effectiveness of simple decision strategies. Third, we also pointed out that the

effectiveness of decision-analytic techniques, though widely assumed, is largely untested and even doubted by some.

However, we did also say that analytical thought may well be important in many decision environments, as may some form of decision education. Thus I disagree with Diana Barker when she says:

'None of us needs training in decision making. What we need is help after a decision has been made.' This is like saying my decision to jump out of an aeroplane without a parachute is irrelevant, as long as I can get help with regard to the approaching ground.

**David Hardman**

*Department of Psychology  
London Guildhall University*

## I'm not one to complain, but...

**I**N 'Public health and the gendered body' (April, 2002) Catherine Swann makes a brief mention of the 1998 Acheson Report, which shows that men have much higher mortality rates than women. Rather than dwell on this, she goes straight to the global scene, presenting selected examples of women being worse off (e.g. anaemia).

As a campaigner for more action on men's health, I am familiar with the practice of swamping evidence of male disadvantage in our own society with worldwide statistics said to prove 'female oppression'. But the figures presented by Acheson show that in all social classes, male mortality exceeds female, to such an extent that, in Acheson's own words, 'the least well off women still have a lower mortality rate than the most well off men'.

He goes on to observe that the greater association of male mortality with socio-economic factors (the gender divide increases significantly as one progresses down the social scale, so that the poorer males are dying off at almost twice the rate of the richer women) has important policy implications. He suggests that efforts to decrease socio-economic inequalities would impact more on men.

He further suggests policies of gender-specific initiatives to

embrace conditions that are not common to men and women. In the four years since there have been plenty of gender-specific policies for women (as before) but few for men. In 1992 the then Chief Medical Officer, Sir Kenneth Calman, made a plea that action on men's health be made a priority. Ten years on, the Health Action Zones and all the other busy initiatives have done little for men and boys.

Campaigning at the sharp end – confronting the people in preventive medicine who are supposed to be putting up posters in surgeries and hospitals to encourage men to seek screening, and so on, but manifestly failing to do so – I still encounter the same old excuse: that it is a waste of time as men are not interested in their health anyway.

We have just about the lowest expenditure on health relative to gross national product in Europe. If men complained more and demanded the same health care that woman get, the system would be overloaded and tax increases would follow. There is an economic incentive to let misguided male stoicism continue.

**William Coulson**

*United Kingdom Men's  
Movement  
35 School Street  
Syston  
Leicestershire*

### INFORMATION

■ I AM a Chartered Psychologist actively involved in **Riding for the Disabled**. I would like to communicate with other psychologists who are involved in this or similar activities. (There are legal constraints that require us to continue using the word *disabled*. We don't regard our riders that way.)  
**Brian Osman**  
*mail@osbox.freemove.co.uk*

■ I AM an assistant psychologist working in **child and adolescent mental health**. I am currently researching a project looking at availability and quality of information aimed at children and parents for child and adolescence mental health services. I am **keen to establish links with other people working in the same field**.

I am also working towards helping to set up a support/therapy group for young children whose parents are divorced/separated. Has anyone out there done something similar or know of anyone who has?

**Suzanne Parry**

*Child and Adolescent Mental Health  
Services  
St Michaels Hospital  
Holly Lodge  
Lichfield WS13 6EF*

■ I GRADUATED from Bristol University with a 2:1 and am currently working in London with mentally disordered offenders within a rehabilitation setting. I am

**looking for voluntary work experience within a forensic setting**, either within a prison or a medium/low secure unit for a couple of hours a week or a day a week.

**Laura Parsloe**

*E-mail: laura\_parsloe@hotmail.com*

■ I AM carrying out a systematic review for Cochrane on the **effectiveness of therapy for stroke patients with apraxia**. As part of the review I will implement an extensive database search of all the published trials. To avoid publication bias, I would appreciate it if people could contact me if they are aware of any unpublished controlled trials on apraxia.

**Carolyn West**

*Human Communication and Deafness  
Faculty of Education  
University of Manchester  
Oxford Road  
Manchester M13 9PL  
Tel: 0161 275 8286; fax: 0161 275 3373*

■ I AM exploring options for establishing **counselling facilities for the fire service**. I would be interested to hear from psychologists with experience of the fire or other emergency services who could help identify the principles of best practice.

**Sue Kerfoot**

*Clinical Psychologist  
Tel: 01283 585291; e-mail:  
norman.kerfoot@advance-  
consultancy.com*