

Drawing a line in the sand

The long-awaited report of the Mid Staffordshire Inquiry is due to be published in early 2013. The Health and Care Professions Council (HCPC) was not asked to give evidence, but we have been watching with interest and have engaged in the debates about many of the issues that have been highlighted during the Inquiry.

One of the questions we regularly ask ourselves is this: What more can we do as your statutory regulator to empower you to play a stronger role in maintaining standards, even where the culture of health and care organisations may appear to militate against this? One initiative we are supporting is the 'Big Conversation'. This has been instigated by Karen Middleton, Chief Health Professions Officer at the Department of Health in England, to encourage staff to comment on unprofessional behaviour whenever it surfaces.

Another initiative is the research we are undertaking into dispute resolution, for example between managers and staff, who may complain about each other's conduct. We need to know more about the reasons for such disputes as well as the ways in which they can be resolved.

We are already aware of the importance of supporting staff and embedding the right values into working practices. The HCPC Standards on Conduct, Performance and Ethics require those on our register to 'effectively supervise tasks that you have asked other people to carry out'. But this may not be



GEORGE PICK

enough. Supervision provides the opportunity for a professional to voice concerns about standards of care or

difficulties in doing the job. There might be questions about the need for more training, the safety of equipment, size of caseload, staffing levels and procedures. Or there might be an issue with the conduct of a colleague. The Standards are currently undergoing a thorough review with input from service users and professionals. We welcome your views on whether or not they need to be strengthened.

Another area of concern for HCPC is the adult social care workforce. The skills mix in health and social care is changing rapidly, and more health and care organisations delegate work to support workers who are not on a professional register. Support staff require appropriate education and training and supervision, but they may need to be made personally accountable too.

We are currently contributing to the debate by looking at models of regulating individuals who work in adult social care. Various options are being examined,

Minding our medication language

Further to the letter on the inaccurate use of the term 'anti-dementia' drugs (December 2012), I suggest that exactly the same arguments apply to psychiatric medications, and that our language should reflect evidence and not mislead lay people – or indeed, professionals. So called 'antipsychotics' have no specific effect on psychosis, are not a cure, and do not

rectify identified biological abnormalities. The previous term 'major tranquilliser' reflects their mode of action much better. The same is true of 'antidepressants' and of the most recent example of rebranding, 'mood stabilisers', all of which are better described simply by their drug class (SSRIs, etc.) Using these terms uncritically gives service users false hopes about what medications can achieve, and

subtly increases the pressure to take them regardless of the growing amount of evidence that in many cases they are actively damaging (Whitaker, 2010.)

Psychiatric medication does have its uses, but only if accompanied by clear messages about its limitations – it cannot heal the trauma and abuse that, as a growing amount of research indicates, plays a causal role across all

kinds of mental distress (Read & Bentall, 2012). Moncrieff (2008) has argued for a drug-centred, not disease-centred, model of medication use; in other words, one that recognises that while general effects such as sedation may sometimes be experienced as helpful, these medications do not target disease symptoms or entities. While they may bring temporary relief from overwhelming distress, they

including statutory and voluntary registers, and an Australian model that uses a statutory code of conduct and a 'negative licensing' scheme – effectively a register of individuals who are judged not fit to practise against the code of conduct rather than a register of those who are. Any care worker who is found to fall below statutory national standards of behaviour and conduct could be placed on a 'negative' register. The system could also impose a range of sanctions, for example, require specific training or supervision to be put in place. The evidence from other countries that have a similar model shows that relatively few people are stopped from working in the sector permanently. However, the statutory nature of the negative registration scheme means that there is a clear deterrent, an enforceable means of holding care workers to account. This is what is missing at the moment.

Robert Francis QC's report into Mid Staffordshire is likely to be challenging to all of us who work in health and social care, and rightly so. I would urge you to stimulate debate and discussion amongst your colleagues on these important issues, and engage with us to ensure that we continue to be a robust and responsive regulator in times of rapid change.

Anna van der Gaag
Chair, Health and Care Professions Council

do not actually 'treat' illnesses. Careful use of language will help to ensure that their benefits are maximised and the very real risk of harm is avoided.

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The male:female imbalance

I was interested to read Dr Pamela Jacobsen's letter in *The Psychologist* (January 2013) She wrote reacting to Dr John Radford's concerns about psychology moving in a 'female direction' owing to the fact that female undergraduates outnumber males 80:20. Dr Jacobsen wonders, with some exasperation, what a 'female direction' would look like and suggests that worries about the gender imbalance may be motivated by implicit sexism with 'female' being code for 'not quite as good'.

I couldn't help but raise a smile. I trained with Pam and was the only male on a course of 20 clinical psychology trainees. I was lucky enough to study and

practise alongside some brilliant, inspiring colleagues, of which Pam was certainly one, but I hope it is clear to everyone that the lack of males is a genuine problem.

For example, males are more likely to die by suicide but are less likely to engage with mental health services. Gender stereotypes about dealing with mental distress are clearly relevant here, and the visibility of male mental health professionals is likely an important way of bridging that gap. This alone seems good enough reason to promote the idea that psychology should be equally valued by people of all and every gender.

Nursing is stereotypically considered

to also be a 'feminine' profession, but nursing organisations have made a concerted effort to reach out to males and make them feel welcome despite the wider clichés. I have seen no such interest or effort in psychology.

The trick, of course, is to work in a reality where stereotypes exist without reinforcing them. So, I would hope that rather than thinking about psychology going in a 'male' or 'female' direction, we're just focused on improving the diversity of our profession. Attracting males is part of that though, and can't be ignored or sidelined if diversity is to be achieved.

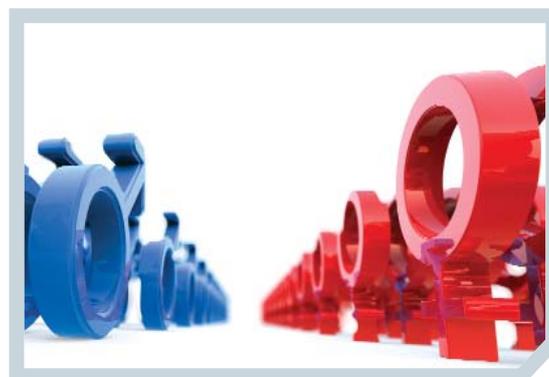
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I am sorry if my remarks in *The Psychologist* (Interview, December 2012) left Pamela Jacobsen in despair, even if quiet (Letter, January 2013). In necessarily brief responses to an interview I tried to choose my words carefully, but perhaps did not succeed. I did not 'assert' that the sex imbalance has moved the focus of the discipline in a feminine direction, I 'suspected' it. I did not suggest that 'Psychology as a title should be totally abandoned', but that it might be seen as one of a family of behavioural sciences. Even that does not cover the wide range of our discipline. Personally I prefer 'Psychology', but the public perception of that remains obstinately inadequate.

It is well established that disciplines, and the careers associated with them, are widely seen as falling on a continuum of appropriateness for, and attractiveness to, men and women. This is shown by the views of students and intending students, and by the differential rates of recruitment, which are consistent in the USA, the UK and other European countries, and China. There are several facets to this dimension of masculinity/femininity, such as

altruism/self-interest, openness/intellect, and perhaps most generally an interest in 'things' versus 'people'. It is not redundant to say that men tend to be more masculine, and women more feminine. Engineering (for example) has long been seen as more masculine, and persistent well-funded campaigns to increase female recruitment have had little effect. Psychology has changed from attracting men and women more or less equally, to the present 20:80 balance. I think it is regrettable that we recruit, not too many women, but too few men.

John Radford
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FORUM THE REAL WORLD

In his 1929 book *Civilization and Its Discontents* Freud observed that 'it is always possible to bind together a considerable number of people in love, so long as there are other people left over to receive manifestations of their aggressiveness'. Over the intervening decades, history and psychology have generated a large amount of data that speaks to this observation.

Most notably, in order to make sense of the atrocities of World War II, two sets of classic social psychological studies produced evidence that ties to an ingroup go hand-in-hand with prejudice towards outgroups. First, Sherif's Boy's Camp studies showed that close friends could become bitter enemies once their relationship was defined in terms of competing group interests. Following on from this, Tajfel's minimal group studies showed that discrimination against outgroups could arise from individuals' mere assignment to groups – however meaningless the basis for intergroup division.

The work of Sherif and Tajfel moved researchers in the social identity tradition towards the conclusion that love of an ingroup – and identification with its goals – was an essential underpinning of aggression towards outgroups. However, this analysis remained at odds with conclusions drawn from other classic studies which argued that such aggressiveness is the consequence of people's 'natural' tendency to go along with rules and roles associated with the social positions in which they find themselves. The stories that Milgram and Zimbardo told held little place for love of any form.

As it has evolved, together with colleagues like Phil Banyard, Rakshi Rath and Megan Birney, our own contribution to this debate has involved re-examining the latter paradigms in order to interrogate claims that the evils they document spring from processes devoid of thought and emotion. In our own BBC Prison Study we found that it was only when they identified with a brutal leadership that participants were willing to display the forms of enthusiasm and creativity necessary for tyranny to succeed. Our more recent work has also shown that willingness to follow the aggressive instructions of an authority is predicted by, and contingent upon, identification with it. Indeed, in both the Zimbardo and the Milgram paradigm it appears that identification with the researchers' scientific projects – and their leadership – was the crucial process that made destructive acts both meaningful and possible. In short, terror results not from carelessness but from commitment. It is a labour not of ignorance but of love.

At one level, the upshot of all this is to reveal a unity of process running through all these classic studies. This centres on the realisation that identification with an ingroup is a prerequisite for the forms of passion that justify and motivate oppression of outgroups. However, this analysis also takes us further by showing that there is no necessary link between love of 'us' and hatred of 'them'. So Freud was certainly correct to observe that these can be two sides of the same coin, but they need not be. For while social identification underpins all forms of group behaviour, work by Andrew Livingstone and others has shown that the form of that behaviour depends critically on the content of social identity.

If those we identify with espouse a vision of 'us' that requires love to be proved through hate, then we will move towards the dystopian worlds produced by Zimbardo and Milgram. But as Martin Luther King and Desmond Tutu have shown, when leaders define social identities that require love to be proved by love, this can be a royal road not only to reconciliation but also to social progress.

Steve Reicher is at the University of St Andrews. **Alex Haslam** is at the University of Queensland. Share your views on this and other 'real world' psychological issues – e-mail psychologist@bps.org.uk. An archive of columns can be found at www.bbcprisonstudy.org.

Flawed science?

The recent report of the Levelt, Noort and Drenth Committees into the fraudulent research practices of the social psychologist Diederik Stapel performs a valuable service for the academic community. In cataloguing the nature and extent of Stapel's malpractice the report draws attention to a number of challenging issues, and provides a necessary corrective to the scholarly literature by specifying the details of the research fraud in each of the publications examined by the Committees. The Social Psychology Section of the British Psychological Society welcomes this aspect of the report.

However, the report goes on to question the integrity of the field of social psychology as a whole. Despite a clear statement that '[t]he Committees are unwilling to make any statement about social psychology in general' (p.48), the verdict is nevertheless damning: 'The Committees can reach no conclusion other than that from the bottom to the top there was a general neglect of fundamental scientific standards and methodological requirements' (p.53) which extends beyond Stapel's immediate circle to encompass journal editors and peer reviewers. This leads the Committees to suggest that 'there are certain aspects of the discipline itself that should be deemed undesirable or even incorrect from the perspective of academic standards and scientific integrity' (p.54). Whilst critical reflection on scientific practice is to be welcomed, the Section has serious concerns that these conclusions are unwarranted insofar as they paint a picture of the field as seriously and uniquely compromised. As has been pointed out by Wolfgang Stroebe, Tom Postmes and Russell Spears in a recent issue of *Perspectives on Psychological Science*, there are no grounds for concluding either (a) that research fraud is any more common in social psychology as compared with other disciplines, or (b) that the editorial process is particularly poor at detecting research fraud in social psychology. In fact, they point out that fraud across disciplines is typically only identified following the actions of whistleblowers rather than through the peer review system.

Let us be clear that research fraud is beyond the pale, and that critical scrutiny and reflection is to be welcomed and encouraged. But let us be equally clear that where action is needed – such as on the impact of pressures to publish and the low value typically accorded to replications – the issues extend far wider than a single discipline. On this point, scientists in all disciplines would find much of value in the work of sociologists of science – from the classic works of Harry Collins, Bruno Latour and others – who have shown the contingent and 'messy' nature of scientific practice across the full range of disciplines. The aim of these scholars has typically not been to identify and understand 'bad science', but instead to elucidate the operation of the scientific endeavour as a social system. Promoting critical reflection on these aspects of science sheds light on the disjuncture between 'science in action' (to use Latour's term) and the official accounts one finds in the typical research report. If a genuine reflection on scientific practice is to be encouraged, it is this much more far-reaching endeavour which will ultimately serve the greater value. Steps can be, and are being, taken to make malpractice even more difficult; but the subdiscipline does not deserve the harm to its reputation that may be provoked by the careless implication of unique deficiencies.

Stephen Gibson

Honorary Secretary, BPS Social Psychology Section, on behalf of the Section Committee



We contacted Willem Levelt, Chair of the Levelt Committee, for his comment on a news piece (see pp.88–9). He offered us a sole and final rejoinder, published here in full.

Since its appearance on 28 November, *Flawed Science*, the final report of our investigation committees on the scientific fraud of the social psychologist Diederik Stapel, has triggered a flood of reactions in both the public and private domain.

The report has been praised for its thorough analysis of the complete oeuvre, 137 publications, of Stapel. The scientific record in social psychology can now be cleansed of all 55 fraudulent papers (co-) authored by Stapel, of another 10 papers with serious evidence of fraud and of 10 (at least partly) fraudulent dissertations completed under Stapel's supervision. This sets a new standard for the investigation of other cases of scientific misconduct.

The report has been also praised for its revelation and detailed analyses of scientific malpractice, aside from

straightforward fraud,

which appeared in a majority of publications co-authored by Stapel.

There was, in particular, repeated evidence of selective data manipulation: verification bias in

the design, the

statistical analysis and the reporting of the research conducted. The report noted, in addition, a general carelessness in applying basic standards of sound scientific procedure. In many cases, this negligence of scientific standards was quite manifest in the publications. Scientific colleagues let us know that they would make this analysis obligatory reading for their students or that they would use these examples in their courses on methodology and research design.

The report has also been criticised, in particular by the European Association of Social Psychologists (EASP) and by the BPS Social Psychology Section. These critical comments did not concern the facts reported, but their interpretation and inferred implications for the whole field of social psychology. The relevant facts for the interpretation were, in summary, these: The 137 publications co-authored by Stapel had gone through the hands of 70 different co-authors, some Dutch, many international. They had also been screened by a substantial number of reviewers of international, mostly leading,

journals in the field. Many of the papers, finally, had been examined by the 18 promotion committees of Stapel's PhD students. But, as the report says, 'virtually nothing of all the impossibilities, peculiarities and sloppiness mentioned in this report was observed by all these local, national and international members of the field, and no suspicion of fraud whatsoever arose'. This is remarkable at least for the many cases of manifest, often tendentious sloppiness. It is the more remarkable because increasingly Stapel's fraud became detectable, as the young whistleblowers eventually demonstrated.

The report's interpretation of these facts is two-pronged. Firstly, it states the obvious: the publications of Stapel and co-authors cannot and should not be considered as representative for the field of social psychology. The report states explicitly: 'The Committees are unable to make any statement on these grounds about social psychology as a whole', and 'the Committees are not suggesting that unsound research practices are commonplace in social psychology'. These statements have been fairly cited in public reactions to the report.

Secondly, the remarkable failure of the relevant national and international peer community to discern, over such a long period, the manifest negligence of scientific standards in the journal publications co-authored by Stapel impelled the Committees to address a more general issue. Could it be that customary procedures of enforcing and monitoring sound scientific practice in social psychology are in need of improvement? The report provides a range of concrete examples of the apparent failure of this critical function, at different levels of the scientific hierarchy far beyond Stapel's local research environment. This state of affairs was judged sufficiently serious to prompt the Committees to recommend a thorough investigation, both nationally and internationally, of these practices and review procedures.

Here, the Committees were in the good company of leading social psychologists. The report approvingly mentions various initiatives already taken since the Stapel fraud came to light in September 2011. It refers in particular to the excellent November 2012 issue of *Perspectives in Psychological Science*. The preface to that issue raises practically the same issue: 'Is there currently a crisis of confidence in psychological science reflecting an unprecedented level of doubt among practitioners about the reliability of research findings in the field? It would certainly appear that there is.' The special

issue then provides 23 papers, by leading authors, analysing these issues in social psychology and proposing various ways of improving customary practices, in particular by focusing on replication studies at all levels. This is exactly in line with the report's recommendation: 'Far more than is customary in psychology research practice, replication must be made part of the basic instruments of the discipline and at least a few journals must provide space for the publication of replicated research.'

The criticism the report received largely concerns the fact that it does not compare its findings with similar derailments in other sciences: it 'uniquely' targets social psychology, whereas a comparison with other sciences would have shown that the noticed defects are of a much more general character in modern science. This was considered to be an unfair treatment of social psychology.

True, the report does not compare the observed disquieting facts in the domain of social psychology with the situation in neighbouring or further afield sciences, either with respect to the incidence of fraud, or generally with respect to the occurrence of bad or sloppy science. It is, given the existing literature on this topic, more than likely that such a comparison would have led the Committees to the conclusion that social psychology is not unique in these respects. However, such a comparative investigation was not part of the Committees' commission. The terms of reference, specified in the opening section of the report, limit the investigation to determining which publications (co-)authored by Stapel are fraudulent and to offering a view on the methods and the research culture that may have facilitated this misdemeanour.

We were pleased to notice that, in the various responses our report elicited, the comparison to other sciences was not used as an excuse for the observed shortcomings in social psychology. Whatever the outcome of these comparisons do show, we believe it is critical that the responsible organisations and practitioners in social psychology continue to focus attention on fostering research integrity and monitoring proper research practices at all levels. If the revelation of Stapel's fraud, the report's analysis of the research culture in which it took place, and the report's recommendations to guard against such misconduct have sharpened that attention, the Committees' major efforts have not been in vain.

Pieter Drenth, Willem Levelt and Ed Noort
Chairs of the three Stapel fraud investigation committees



Tilting at Scottish windmills?

The letter by Mallory Wober, in the December issue, appears to have been prompted by a concern for the future of the UK, by a potential positive outcome of the referendum on Scottish independence. After making irrelevant comment about the origins of the fathers of Cameron and Salmond, the letter refers twice to the Scots as 'less than 10 per cent' of the British population.

The writer claims bias in what he erroneously puts as the proposed referendum question, and moreover he gives no psychological research references for this claimed bias.

A critical error is that the letter gives a misrepresentation of the referendum question as a 'statement' when it is actually a 'question'. The wording Mallory Wober gives is *not* as proposed by the Scottish Government, which is in fact: 'Do you agree that Scotland should be an independent country?' Furthermore he fails to mention

that the current proposed wording constitutes a starting point for testing by the Electoral Commission.

Reference to the saltire is misleading – suggesting it has been intensively publicised in Scotland as an 'icon of

personal identity' 'for several years'. It has been the flag of Scotland since the 9th century and was on the coinage of King David in the early 13th century. However it is a white elongated cross (adopted in the Union Jack) on a blue oblong background. To suggest that asking the Scottish people to put a black cross in a square white box (as in the filling in of any form) would bias the

response, seems incredible.

The referendum is about 'independence' not about what people believe is the 'best thing for the future of Scotland', so Mallory Wober's proposed wording is not 'fairer' as claimed, but is actually invalid. The referendum debate

will have extended over more than two years before the vote. Thus people will have made up their minds well before seeing the ballot paper, so voters are probably less likely to be influenced by ballot paper format, but this has not been researched.

A serious omission is not pointing out that the question was being referred by the Scottish Government to the Electoral Commission for Scotland for checking and modification, before it is put before Holyrood in the Spring of 2013. These facts are readily available and should be checked before passing judgement. The Electoral Commissioner has stated: 'We will assess the referendum question to see whether voters find it clear, simple and neutral. If it isn't, we'll say what needs to be done.' Parliament is committed to following that advice. The assessors will include not only the voters, but also all relevant specialists, including 'plain-language specialists' and 'academics'. Psychologists must be involved. This assessment process will take 12 weeks with 'unambiguousness' and 'avoidance of encouragement towards one response more favourably than another', being stated matters of central concern.

In many respects Mallory Wober appears to have been tilting at windmills.

Dr John W. Hinton
University of Glasgow



THE EXPERIENCE OF SUICIDE

I was interested to see that Peter Beaumont states that suicide 'is neither a crime nor a sin' (Letters, December 2012). I would beg to differ. My grandfather 'committed suicide' in the 1930s when it was indeed a crime and he would have been prosecuted had he survived. My father was also a suicide in the late 1960s when it was no longer a crime but it was a sin, in so far as the pain and poverty to my mother (no insurance pay-out) and the destruction of our family – my poor sisters suffered at school and I took the brunt of my mother's consequential inability to cope with her feelings of paranoia and feelings that other people blamed her.

My own husband took his own life in the autumn of last year after many years of mental illness. I understand that he was not a well man, but it has not stopped my pain and distress as I loved him to the end. I do not feel the paranoia my mother felt but have worried deeply that perhaps I could have done more. It has had repercussions on his son, my stepson, and deeply upset my own daughter, his stepdaughter, both of whom also loved him. I had tried to explain how such a death affected others but my husband clearly did not understand any of this and told me on a previous occasion that he could only think of himself when he reached this kind of juncture. I consider this to be much worse

than cruelty to animals – it is cruelty to human beings and I would welcome some major objective in the prevention and treatment of those with suicidal intent. It could offer hope to others before they find themselves in my situation.

This also leads me on to the letter on 'Misleading nomenclature' by Dr John Kelly (same page). My grandmother had dementia in her eighties and told me of the suffering (about which she had never previously spoken) over her husband's death in the 1930s. My mother (not related to my grandmother by blood) also has paranoid dementia, I think at least in part related to my father's death. She believes that her 'anti-dementia' drugs are all that give her hope; she would take no comfort from being told that they are palliative. What hope for me, I ask? I can only hope that by the time I am in my eighties a better solution is on offer, as I am all too aware that these drugs only offer a stay of execution.

I guess what I am trying to say is that simply looking at things intellectually is not always the complete answer – especially for those who (what to say, I am aware that 'sufferers' is regarded in the same light as 'victims') actually live through the experiences.

BPS Member (name and address supplied)

Psychologists in retirement

After 40 years in health and education and being somewhat weary of organisational life, I'm preparing to retire. I've had a varied career involving practice, training, supervision, management and research; yet I'm rather reluctant to entirely give up being a psychologist, doing things that are interesting and, I hope, useful.

I'm aware that there are options after full-time work: private practice, consultancy, sessional teaching, independent research, writing... I imagine that some of us just stop, grateful for a rest and to have more time to tend the roses/grandchildren. But are a lot of skills and knowledge being lost at this transition?

There's a paper from the American Psychological Association that addresses the issue in relation to demographic changes (Chamberlin, 2004). The higher proportion of older people means that a

larger pool of experience that is being lost to society as psychologists (and others) retire; at the time of writing it was noted there was no systematic societal or APA response to this. My information is that there is no BPS group for retired psychologists, which may indicate that all are happily engaged elsewhere.

I wonder if retired psychologists could collaborate to promote the understanding and application of psychology, with less organisational constraints or concerns about funding, career progression, etc. Not only in our primary area of expertise but also drawing on generic skill in research, analysis, communication, project management, appreciation of group dynamics and organisational politics. These are some of the topics that have come to mind:

- I people and politicians' reluctance to consider psychological knowledge;

- I the quality of decision making in public life;
- I psychological support for voluntary workers, mentors, carers;
- I attitudes to sustainability and environmental concerns;
- I facilitating partnerships across cultural domains (e.g. between science and drama).

I'm aware that I've not addressed this idea in relation to registration, insurance, ethical oversight, etc., but I'd be interested to hear from anyone who is thinking on similar lines.

Phil Topham

Bristol

Reference

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Competence to teach mindfulness

It was a pleasure to read the letter entitled 'Who is competent to teach mindfulness?' by Marika Lahtinen in the December 2012 edition. At this time of expanding interest in the implementation of mindfulness in health care the piece raised important questions about what it means to be competent to teach mindfulness and the challenges of assessing 'readiness to teach'.

The enthusiasm to implement secular mindfulness-based programmes in a range of contexts (including health care, education, business, prisons and community centres), and with a diversity of client populations within these contexts, is strong and growing. Within the UK a strong driver has been the recommendation by the National Institute for Health and Clinical Excellence (NICE, 2009) of mindfulness-based cognitive therapy (MBCT) as a National Health Service treatment of choice for people with recurrent depression. As training organisations in the UK have grown in response to this interest, there has been a concerted focus on quality and integrity in training processes, standards for teachers and mindfulness-based teaching competence. The principles underpinning the training processes at the three UK university-based mindfulness training centres – Bangor, Exeter and Oxford – are articulated in Crane et al. (2010).

The UK Network for Mindfulness-Based Teacher Trainers, which represents all the main UK training organisations, has developed and disseminated national good practice guidance on standards for mindfulness-based teachers (UK Network for Mindfulness-Based Teacher Trainers, 2011). Last year (Crane, Kuyken et al. 2012) we unpacked the aspects of competency which are particular to the mindfulness-based teaching context (i.e. embodiment of the qualities of mindfulness) and those which are generic to related contexts (i.e. relational skills, holding the group learning environment). And finally, the three universities have developed the Bangor, Exeter and Oxford Mindfulness-Based Interventions: Teaching Assessment Criteria

(MBI:TAC) Scale (Crane, Soulsby et al., 2012). Initial research on the psychometric properties of the MBI:TAC give promising indication that mindfulness-based teaching competency can indeed be assessed reliably and validly (Crane et al., in press).

All these developments do not constitute the regulation which Lahtinen mentions in her final paragraph. They do however, offer some clear frameworks and guidance which have been developed in collaboration with stakeholders and leaders in the field and will hopefully give a foundation to developments in this area going forward.

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Feeding victim mentality?

Rana & Rana's concern (Letters, January 2013) regarding over-optimism about post-traumatic growth (PTG) is mistaken. A chorus of voices since Bonanno (2004) have urged professionals not to mistake the traumatised people they see in clinics as being representative. The majority of people who experience traumatic events do not suffer from PTSD (Ahmed, 2007).

Instead of moderating their statement to say that PTSD *can* become a serious mental illness that *sometimes* leads to significant morbidity or mortality, Rana and Rana risk pathologising a normal reaction to an abnormal event. They also mislead, by omitting to mention, as closer reading of Adshad (2000) indicates, that only 'the experience of interpersonal violence increases the prevalence up to 20–30%'. Even so, over half of these people will recover without treatment! The seriousness (or not) of PTSD has to be assessed on an individual basis and not assumed or stereotyped.

When I commissioned a special issue on PTG (Altmaier & Prieto, 2013) it reminded me of a reviewer who advised against publishing a paper on PTG after sex abuse/domestic violence because it would undermine 'victim support organisations'. We need to move away from 'a damaged-for-life' victim

Professor Tanvir Ahmad Rana and Aarsal Wazir Rana's response to Stephen Joseph's article about post-traumatic growth (November 2012) goes some way to acknowledging the benefits and hopefulness behind the phenomenon. However, their letter warrants a response on three points.

Firstly, they seem to imply that the topic of post-traumatic growth comes at the expense of fully understanding the hardship of those suffering post-traumatic stress. However, the literature on post-traumatic growth, as mentioned in the article by Joseph, already makes it clear that it is not a choice between one and the other: Post-traumatic growth does not imply the absence of post-traumatic stress,

nor that once in recovery a sufferer will simply leave their traumatic experiences behind, never again to reflect on them.

Secondly, their letter stated that typically 20–30 per cent of people exposed to trauma go on to develop post-traumatic stress disorder (PTSD). While the challenges of estimating PTSD are numerous, most estimates lie at a much more conservative 7–9 per cent, with higher rates reported only following the most extreme events (Bonanno et al., 2010). Furthermore, Joseph acknowledges that PTSD sufferers are less likely to experience post-traumatic growth than those who, following a traumatic event, remain 'able to cope, think clearly and engage sufficiently in the necessary affective-cognitive processing'. To this end, the figures should encourage us, not leave us feeling helpless: If 7–9 per cent of those with traumatic experiences develop PTSD then the implication is that post-traumatic growth remains a real possibility for the majority. Even for those in the throes of PTSD, it is not a permanent state – following successful

mentality and be acutely aware that the majority of traumatised people are far more resilient than many mental health professionals recognise. Even after tsunamis and wars, most people need care and support, not therapy. PTG proponents are merely reflecting their firsthand experience of the remarkable resilience of the human spirit.

Dr Waseem Alladin
University of Leicester

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treatment they may well go on to experience post-traumatic growth.

The third, and perhaps most important point to make, is that post-traumatic growth offers a challenge to the illness ideology represented by PTSD, and as psychologists we must recognise the dangers of applying an illness ideology to mental health conditions (Maddux et al., 2004). Post-traumatic growth thus offers us a

different way to perceive the traumatic experience, whereby post-traumatic stress is shown to be a normal, natural process of cognitive adjustment, which results in positive change.

Lucinda Brabbins
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Nottinghamshire Healthcare NHS Trust

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Has your teaching, research or practice in psychology been affected by the economic climate? Get in touch if you are interested in contributing to an 'austerity psychology' discussion special – e-mail jon.sutton@bps.org.uk

Getting paid

Psychologists can do little about the scandalously poor rates of pay allowed by the Legal Services Commission. But it is not clear why they put up with being paid late.

Most psychiatrists these days seem to have terms of engagement. They deal with charge rates, cancellation fees, travelling, notice etc., etc. It is a shame more psychologists do not have the same.

One or two have confidently reported their terms to me: 'We put clearly on our invoices: This invoice is payable in 30 days' or 'We have on the invoice: We will charge 2 per cent per month for late payment'. Those terms are good guidance, but nothing more. There is a contract between the parties, that probably started when you, the psychologist, agreed to make an appointment to see the client. Unless, therefore, the solicitor knew of the terms at that time, he can simply ignore them.

All, however, is not lost. As a starting point the Law Society guidelines enjoin solicitors to make interim payments to experts. They stipulate a period of three months. Your terms of engagement might well provide for payment

in 30 days and allow yourself a discretion where solicitors have complied with the guidelines. Next you could set up an automatic diary system to warn debtors that their term of credit is due to expire shortly (giving the exact date so there is no room for manoeuvre) and add – this is the important point – that you will then be looking to them for statutory interest.

It is surprising how many debtors claim not to have heard of the Late Payment of Debt (Commercial) Act, passed in 1998. It gave creditors the statutory entitlement to both interest (currently a swingeing 8.5 per cent) and compensation (albeit modest) when bills are paid late. You may think it fair to mention the Act in your terms, but the Act incorporates the right to charge interest in your contract and is there for you when you want it.

I hope that more psychologists will start introducing terms of practice and that as a result they will get paid a bit sooner. Knowing your rights should help.

Leslie M. Wise

*Chartered accountant and barrister
See advertisement on p.87*

obituary

John Nisbet (1922–2012)

John Nisbet was born in Rosyth and died in Banchory on 5 October 2012, aged 89. He spent his whole professional career in Scotland, leading to the award of an OBE for services to Scottish education. He was a sandy-haired Scot, small in stature but with a powerful intelligence that could become overwhelming on occasions, and yet his lecturing was outstanding – clear, concise and inspirational. He once complained to me that he felt intelligent enough to see the big problems, yet not intelligent enough to solve them! That, I think, was the perfectionist in him which, allied to great energy and determination, also applied to his leisure pursuits. He held a single-figure golf handicap for 25 years, carried out a 20-mile walk and 4000-foot climb to celebrate his 75th birthday, and was still discussing a joint article with me in his final weeks.

As an RAF intelligence officer in his twenties, he passed information to the code-breakers of Bletchley Park. Afterwards he took a degree in English at Edinburgh University, before becoming a schoolteacher and taking a part-time degree in education under Godfrey Thomson. As an assistant lecturer at Aberdeen University, he completed a PhD on the effects of family environment on intelligence. Subsequently, he pioneered ways of utilising ideas from educational psychology in schools, and was awarded a Fellowship of the British Psychological Society. In 1963 he became the first Professor of Education at Aberdeen University and, during the next 10 years, his activities and influence became international. He was appointed to the SSRC, becoming chairman of its Educational Research Board, and worked in Europe for OECD and UNESCO. Over this period he wrote six books, five book chapters and 23 journal articles, and subsequently edited the *British Journal of Educational Psychology* and became Chair of the British Educational Research Association.

His main research contributions involved using theories about metacognition to show how to encourage pupils to 'learn how to learn', rather than just absorb knowledge that could become rapidly redundant. He also wrote influentially on the impact of educational research on practice, seeking to persuade policy makers that research findings should not be used to dictate specific changes in practice: their influence is subtle and long-term, changing ways of thinking to enhance the whole teaching enterprise, not to alter particular methods. That, some would argue, is a view that still needs to be heard!

Noel Entwistle

*Professor Emeritus of Education
University of Edinburgh*

NOTICEBOARD

I am a recently appointed Clinical Psychologist working in a part-time post with a newly formed Mental Health Liaison Team. The Team provide specialist frontline emergency mental health assessment for individuals across the lifespan (18 upwards) who are in acute mental health crisis, with the formulation of a management plan. It also provides advice, training and consultation to referring teams including A&E and other medical and surgical departments. I am keen to make contact with psychologists working in similar roles.

Dr James Easton

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Teachers, researchers, lecturers and students of Psychology are alerted to the online MMU Psychology Journal (Dissertations) UK. This was first published in September 2010 and the work of students graduating in 2010; 2011 & 2012 is now 'live' (see www.did.stu.mmu.ac.uk/MMU_Psychology_Dissertations_UK).

Colleagues at all UK universities are asked to make their students aware of the website.

Andy Bell

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Assisted suicide for those terminally ill is an issue that has aroused, and will continue to arouse, either empathy or alarm. Although currently illegal I sense that in the not too distant future change may be proposed to the current law. Any change might benefit from a more multidisciplinary approach than one taken purely on legal technicalities.

I am not aware of any research activity but feel sure there must be psychological interest in the issue of assisted suicide. I should be pleased to hear from psychologists interested in this topic.

David E. Broadbent

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