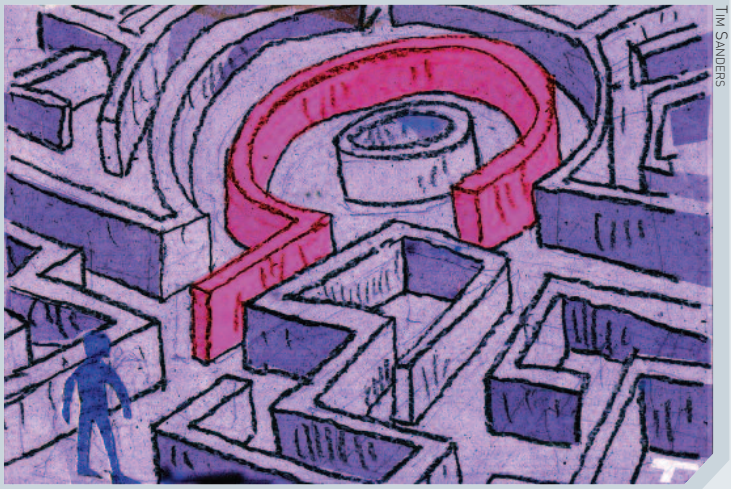
The moral maze of public protection


TIM SANDERS

Anna van der Gaag, Chair of the Health and Care Professions Council, is absolutely right to draw attention to the need for a comprehensive understanding of individual circumstances when making a judgement about a professional's fitness to practise (Letters, July 2013). She refers to an independent report commissioned by the HCPC on expectations of trust, as seen by both members of the public and professionals, and this makes very interesting reading. The report states that non-professionals 'found it difficult to define what the public may wish to be protected from'. Likewise, professionals were concerned with defining what constitutes harm, and felt that most issues could be dealt with internally. The consensus seemed to be that allegations of a lack of fitness should be considered on a case-by-case basis. This approach seems incompatible with Dr van der Gaag's idea of HCPC standards as a 'Highway Code'. This code is not a guide but a requirement. On the whole, the courts are not sympathetic to a motorist who pleads mitigating circumstances after breaking the code. The speed limit is the speed limit, even if you are driving to A&E.

A complaint against a professional is analogous to a situation of marital breakdown. The therapist/mediator should listen to all sides of the story, preferably with the protagonists in face-to-face interaction. The 'truth' of the matter is usually grey rather than black and white. In the methodology of the HCPC research, opinions were sought through scenarios involving a professional who had clearly breached rules of conduct. In other words, guilt was assumed. Respondents were, nevertheless, remarkably fair-minded: '...members of the public were most concerned to explore why a (fictional) scenario may have happened and, in particular, to identify possibly mitigating factors. Many expressed a wish to assist a health professional in improving.'

Dr van der Gaag states that the HCPC does consider allegations case-by-case,

Dear Lisa and everyone else like us

In response to Lisa Molloy's letter ('The future we want?', July 2013). Not only are you not alone, but the pattern you describe has devalued more than a generation of hard-working, dedicated and creative psychologists. I am you but 10 years further down the line, and the perspective from down here is far worse.

Like you, I applied myself, immersed myself in a complete love of psychology and worked like a Trojan. Like you, I applied for psychology positions during and after university, but without careers guidance, clear paths for progression or frankly an interest from anyone in the field I could not line up work. A PhD I was told was essential. So despite great personal and financial hardship I did a PhD. Again applying myself at all costs I graduated, unlike most, in the allocated three years with a PhD from the prestigious St Andrews University. My PhD is one whose content I am still proud of, unlike many. But yet again I was cast adrift, in a rapidly changing psychology accreditation and degree system.

Without a mentor or supervisor I could gain no starting footing in the area of health or counselling and did not have the new basic but specific undergraduate degrees now being required for placements. I, like you, applied for everything employing ingenuity, reframing and frankly downright logical reasoning, but got nowhere. I cannot begin the explain the depths of effort the dedication, the dogged determination and the personal costs I have gone through attempting to get a psychology career off the ground.

With no means of financial support I was forced to apply for any paying job that my transferable skills (the invention of this phase will haunt me to my grave) could be shoe-horned into. Without

contribute

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However, see **www.thepsychologist.org.uk** to contribute to our discussion forum (members only).

which it may well do at the investigatory stage. However, unlike the situation in family therapy, the professional is assumed guilty until proven innocent. Any 'case to answer' allegation is made public one month before a final hearing and is only removed from the website if it is shown to be unfounded. The process from the beginning is overtly adversarial, with legal representation on both sides. There is no sharing of information except in so far as the HCPC must state what the allegations are. A defendant's lawyers may then challenge them and the HCPC may be required to clarify or hand over evidence. The allegation is essentially taken over by the HCPC, and the complainant may not even be present at the final hearing.

This is far removed from an even-handed consideration of individual circumstances. What remedy is there for false allegations? Or suppose a person complains about a psychological assessment that has contributed to a child being taken into care? Vested interests might only be revealed, if at all, at the final hearing. There appears to be no penalty for a complainant who sets out to be vindictive. It is far too costly to ask for a judicial review of a judgement. The HCPC says that it aims to protect the public but only a very small minority of successful 'cases to answer' emanate from the public. The majority are made by employers, and some arrive anonymously.

The HCPC website also links to an excellent report entitled *Alternative Mechanisms for Resolving Disputes: A Literature Review*, which emphasises 'learning from past errors in order to improve the quality of future practice'. The report, dated 2011, recommends that a mediatory approach could be taken immediately after an allegation has been received, and following an allegation that is upheld. In her introduction, Dr van der Gaag states: 'We do not yet know how alternative dispute resolution will be used in our regulatory process but we are committed to undertaking further work to explore the use and value of mediation in an HPC [as it then was] context.' We are waiting to hear what further steps have been taken in this direction.

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WHY BLOG?

We are interested in the reasons **why psychologists blog**, and equally, why they don't. We'd like to better understand what people teaching, studying and practising psychology think about the blogging process and how you are using it either in your work or personal life, or perhaps you use it in both spheres. On the other side of things, we'd also like to know more about why psychologists read blogs and what sort of content you're consuming.

We are therefore seeking the opinions of academic/research psychologists, practitioners, lecturers, teachers, undergraduate and postgraduate psychology students, and anyone else who identifies themselves as a psychologist. We are inviting you to complete a short, anonymous online survey to share your views at: http://bit.ly/1cqzHqe

We are also looking to undertake content analysis of psychologists' blogs, you can provide your blog address at the end of the survey if you are interested. We have received ethical approval for this project from Oxford Brookes University (ethics registration number: 130744).

We hope to report the qualitative and quantitative findings of our blogging survey in a future edition of The Psychologist, along with reflections from a blogging workshop at the PsyPAG 2013 Annual Conference led by Dr Christian Jarrett and sponsored by The Psychologist and Research Digest.

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going into details, as Scotland is a small place and that geography comes with its own unique difficulties which add ten-fold to that of normal psychology career progression, I have been adrift ever since – and not for the want of serious paddling! Although I thought I chose wisely (and I did out of the options I had) that first job was the start of a series of short-term 'use-abuse and throw away the postholder' positions. If I had a pound for every time I'd been promised a future and the start of something great then I'd be rich. I definitely am not.

Not only have I spent the last decade and a half giving my all to every new job every few months, spent every other minute job hunting for the 'next big opportunity' but I've also dedicated myself desperately and tirelessly in vain to start a career in psychology. I have applied myself and psychology in such ingenious ways in unrelated jobs that over the years I have still managed to gain my chartered status and my Associate Fellow status, and I religiously pay my annual dues. I have never given up, psychology is in my soul but I fear it will never be my role.

To what avail these efforts, degrees and accolades? With such hard-worked distance travelled since my original degree and dreams, I have no doubt that what I have not already achieved will become far less possible with each passing week. Indeed, was there ever any window of opportunity for me? I say no, I certainly would have found it if there was. I am not now in middle age suddenly going to become employable in a psychological role when I haven't had a single opportunity in two decades of full-on endeavour.

The cost: a life I have not had time to live or enjoy quite simply due to the tireless belief that psychology can be my career; and a series of unrewarding, poorly paid jobs none of which needed, paid for or celebrated a psychologist. The casualty is me.

To be frank, Lisa, our past successful seniors had a utopia of luck and opportunity to thank for their satisfying and influential positions. Every successful psychologist I know has nothing like the qualifications and experiences I have and openly admits to never having had to really try. Such opportunity and creativity has been regulated and classified away over the last couple of decades and will never return.

In medicine every graduate is valued and guaranteed a job, so surely something as important as the study of psychology, which impacts every part of human life, deserves to be considered this worthwhile too. The casualty is the future of psychology, and me and you, and you – what a waste.

Dr Lorraine Paterson
Davenport

DCP, diagnosis and DSM

I would agree with Richard Pemberton that the Division of Clinical Psychology (DCP) Position Statement on Classification is a more balanced document than reported in the media (Letters, July 2013). I would also agree that there are inherent problems with existing systems of psychiatric classification. Given that, by definition, psychiatric diagnoses are used in the absence of identifiable physiological abnormalities, deciding what is an 'illness' and what is a normal reaction inevitably involves the use of arbitrary cut-off points, thereby laying the system open to the kind of criticisms made by the DCP (along with many others).

However, there are some aspects of the position statement that I would question.

Firstly, it lumps together a variety of very different conditions, such as schizophrenia, ADHD and conduct disorders. While many do claim a biological basis for schizophrenia (*pace* Mary Boyle and others), few would make the same claim for, for example, conduct disorders. Pemberton says that we do not conceptualise 'other' responses to life events such as bereavement as diseases, but this begs the question, assuming that it has been clearly demonstrated that all conditions given a psychiatric label are nothing more than reactions to life events. In the case of schizophrenia (or the phenomena associated with this diagnosis), despite Boyle's thorough examination of the issue, it is actually very hard to make a convincing case for it being entirely due to adverse experience. Childhood abuse has been linked to a wide variety of conditions in addition to schizophrenia, including depression, eating disorders, personality disorders and substance abuse; if biological predisposition plays no part in any of these, there would need to be specific types of adverse experiences that are unique to each condition, otherwise it is impossible to explain why some people develop one condition and some another. As far as I know, no evidence has ever been produced to support this.

Secondly, the paper confuses the stigma arising from a psychiatric diagnosis with that which arises from the condition itself. It is a sad fact of life that people perceived as different have always tended to be rejected by society, and the 'insane' were stigmatised long before Bleuler and Kraepelin gave their condition a medical label. However, the implication in the DCP paper is that, were it not for psychiatric diagnoses, the stigma would not exist.

It is still possible that some kind of neurophysiological abnormalities may be found to be the cause of at least some psychiatric conditions; the fact that these have not yet been discovered should not be so surprising given the huge complexity of the human brain. Chris Frith, for example, presents a plausible model of this kind to explain the phenomena associated with schizophrenia, generating testable hypotheses, some of which have been supported by research (Frith, 1992). I believe the DCP should continue to be open to such possibilities, while continuing to draw attention to the weaknesses in existing diagnostic systems.

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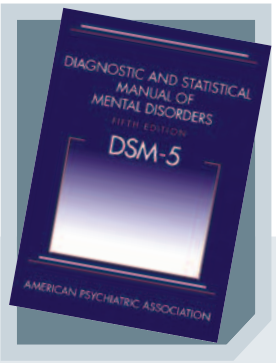

The DCP has recently made a well-considered attempt at communicating a position on diagnosis, formulation, and related clinical issues. It has generated further debate much of which is helpful and thought provoking. However, aspects of the debate appear stifled and unhelpful, especially concerning the legitimacy of diagnosis, the role of 'biological' factors, and the role and activities of non-psychologist colleagues.

As psychologists we rightly advocate psychological formulation as a prime means of understanding clinical information, identifying helpful interventions, and communicating with others. We have trained for this, we train others, and we are rightly concerned about the sufficiency of psychiatric diagnosis and the associated classification systems. But as a profession we risk being seen as unreasonably intolerant if we caricature how diagnosis is used and those who use it (usually psychiatric colleagues). A diagnosis can provide a shorthand for communicating a case-essence. It does not necessarily imply permanence, or the necessity for pharmacological or indeed any other clinical intervention (including psychological or psychosocial). Arguably it is used more pragmatically now than previously and can be valued by service-users. It can be a starting point for them.

We should also be cautious about unreasonably criticising the work of colleagues. I work on care pathways for people with neurological conditions or injuries (including some who have no 'diagnosis', neurological or psychiatric). Much first-line psychological care is provided by neurology colleagues or specialist nurses. Some is provided by GPs. Psychiatrists are occasionally involved. Sometimes people get to see a clinical psychologist/ neuropsychologist, but few are available. I am concerned that our rightful professional esteem for our own role sometimes appears unmatched by a respect for non-psychologist colleagues who attempt a different contribution to care of the 'psychological'. In my experience, non-psychologist colleagues are not indiscriminate dispensers of pharmacology, nor are they blind to psychological processes and the influence of adverse life events or circumstances.

It is important to our professional credibility not to be seen to deny or unduly minimise the role of biology in some conditions, nor to overstate others' emphasis of it. I know of few psychiatric colleagues who use contentious diagnoses (e.g. schizophrenia, bipolar disorder, personality disorder, conduct disorder) in a way that excludes the importance of the past and present experience of the client, either in understanding their current state or in its development over time. Equally, I thankfully know of no psychologists that still insist on a singular psychogenic or social-environmental cause for some conditions including autistic spectrum conditions, Tourette's syndrome, or many other conditions that can involve neurocognitive or neurobehavioural disability. Adverse life events do not necessarily induce emotional, behavioural or cognitive difference (from what is normal for self or others – although of course they often do). Conversely, emotional, behavioural and cognitive difference does not necessarily imply a root-cause in adverse life events (or that 'distress' is necessarily present).

There are now a growing number of conditions thought to have a possible 'biological' contribution; some of the diagnoses referred to as 'functional' in the

The spate of comment and correspondence arising from the DCP Statement on Diagnosis, while important for many obvious reasons, fails in one very noteworthy way, that of contextualising diagnosis within health care, particularly the mental health sector. The issues are not just about diagnosis. As the Society's guidance statement on the Electronic Health Record (BPS, 2011) makes clear, psychiatric diagnoses are used for many different purposes within mental health, including policy, research, and service evaluation, as well as the day-to-day clinical uses and the fact that the terminology is entrenched in the language of professionals, the media and the public. The critics of such diagnoses – and these include psychiatrists – apart from seemingly overgeneralising their criticisms to all diagnosis, nevertheless do a service in pointing out that the basis for using such diagnoses within clinical practice is flawed. There is abundant evidence that this is so, summarised for instance in my recent paper (Berger, 2013), prepared partly to provide a background and context for the DCP Statement. Hence, the challenge to psychologists and other critics is: 'Can you do any better?' As my paper notes, there are some options in development, but it is not clear that these will satisfy the diversity of functions served by psychiatric diagnosis, nor will they necessarily satisfy those service users who may want such a diagnosis.

The most significant feature of the context is the ongoing central drive towards the national implementation of what are now called the Integrated Digital Care Records (IDCRs). These rebranded Electronic Health Records are intended to serve within organisations and sectors (Healthcare Trusts and the NHS in England), as well as across sectors (health, social care and possibly even education – for instance in relation to children's social services), and for the lifetime of the individual. The IDCRs, like their predecessors, will, in addition to core clinical content, also have to contain all the information required for the diverse purposes noted above. IDCRs (and paperless or paper-light systems that provide them), are a salient goal of government policy. Indeed, such systems are well established in many mental health services, and are becoming ubiquitous. In this context, if something is not in the electronic record, it effectively does not exist, with many serious ramifications. This being the case, there is a further question: 'How might psychologists or other practitioners want their activities represented in IDCRs if these are not to be focused around psychiatric diagnosis?'

An approach to answering this question is developed in the second part of the Berger (2013) paper, accompanied by practical illustrations of what is entailed. In essence, it suggests that we need to develop psychology models and datasets that capture the perspectives of psychologists relevant to their activities, that are specifically intended for inclusion in IDCRs. This is a complex, demanding and ongoing challenge. For instance, such a model must incorporate the implications of epigenetic studies pointing to biological anchors linking early experiences to later-appearing psychological difficulties, and to the recent developments in extinction research (for further references, see Berger, 2103). Proper salience must also be given to social factors in the emergence and maintenance of individual difficulties. Although focused on clinical psychology in the first instance, the proposals also serve as a basis for the other practitioner disciplines wishing to have their contributions recognised within the domains of their activities.

It would be easy to dismiss the issues raised here, and in the broader debates underpinning the previous correspondence, as matters that are relevant only to some practitioners. That would be a serious error. For all, the advent of electronic records will have profound consequences – they will contain information about us, among other things our medical histories and our sometimes deep and sensitive personal concerns and difficulties. These records will influence how we are seen and what will happen when we want, or access, services in health, social care or education. The information they contain will also increasingly be used to determine which services merit funding. In all these core services and in other areas, there are crucial psychological perspectives and resources. There should thus be a space where psychological perspectives relevant to the purposes of such records can be identified and be used for the benefit of the subjects of these records.

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Declaration of interest: The author is a member of the DCP Working Party that produced the Statement on Diagnosis. He does not benefit financially from sales of the Paper.

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DCP position statement may in due course come to have revised understandings. Either way, psychologists have a promising role in contributing to the quality of life of people with these conditions and we risk unnecessary clinical and professional compromise (and marginalisation) if we are perceived as denying the possibility of 'biology' in these or as oversimplifying the complexities in understanding them (it is of course a simplification to even talk of biology without deconstructing what that means, be it genes, body, brain, or the many interactions between all of these and the environment).

We have a distinct and challenging role as psychologists. We should avoid it becoming further complicated by perceived premature or misplaced arguments. Otherwise we risk being seen as more conceptually flawed and clinically unhelpful than the targets of our discontent in the first place.

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As one of the few British psychologists to have contributed substantially to the DSM-5 and to be actively involved in the revision of the International Classification of Diseases (ICD-11), I thought it might be helpful to record my experience of this process.

Since the publication of DSM-III in 1980 the principle has been that disorders are to be described as clearly as possible on the basis of their symptoms and not linked to specific aetiologies in the absence of compelling evidence. Thus, although the new manual undoubtedly introduced an explicitly medical rather than psychosocial focus, there was no necessary implication that mental disturbances are a subset of physical diseases; rather, in the words of DSM-III, 'Each of the mental disorders is conceptualized as a clinically significant behavioral or psychological syndrome'. Although some clinicians and researchers do adhere to a predominantly

FORUM GREEN SHOOTS

Robert Worcester, who founded the opinion polling organisation MORI, once described a hierarchy in what can be measured from the public. At the top, in his view, are opinions: 'the ripples on the surface of the public's consciousness – shallow and easily changed'. Then there are attitudes: 'currents below the surface, deeper and stronger'. Finally, deeper still, lie society's values, the 'deep tides of public mood – slow to change, but powerful'.

These distinctions are relevant to psychology research. The public are often asked what they think about environmental issues, and we often see a picture of widespread concern. When the Department for Transport (DfT, 2011) carried out a national survey they found 69 per cent of people said they were worried about climate change, and 66 per cent agreed this was the result of human activity.

But it is hard to escape the contradiction here: if people are so concerned, why is there no real evidence of people avoiding flights, shunning New Zealand apples or reducing their driving? It's not because they feel their actions will have no effect – the same DfT survey specifically asked about that, and people said their actions make a difference. So... where are these actions?

There are many possible explanations here – not least the risk of socially desirable responding when asked about a charged issue, and theory of planned behaviour fans would no doubt mention intention–behaviour gaps. But let's consider something more psychologically interesting – cognitive availability biases. Viscusi and Zeckhauser (2006) gave people a survey of environmental concern, but prefaced some copies with a passage of text about climate change. The people who got this version made significantly more extreme judgements about the extent of future global warming. Even more strikingly, Joireman et al. (2010) found that exposure to words like 'sunny' or 'sweat' – or even warm weather on the way to the laboratory! – could increase people's judgements about the risks of climate change.

I'm not for a moment suggesting that these effects are the only issue in explaining the gulf between what people say they believe and their actions, and there are many known complexities (e.g. Corner et al., 2012). Rather, I'm suggesting that, as known psychological mechanisms such as the availability bias can change people's judgements about an important social issue so easily, these judgements look a lot more like the ripples on the surface of the public's consciousness than they do the deep tides of public opinion. As such, is there a case for moving from explicit ratings to more subtle, implicit measures of environmental attitudes and beliefs if we really want to know what the public think? Certainly, psychological studies make it pretty clear that we take a risk if we base major governmental policy on what the public say they believe to an opinion pollster.

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biological model, the contribution of psychosocial factors, including cultural factors, is explicitly recognised by the editors of DSM-5 (Regier et al., 2009) and appears in many places within it. Moreover, DSM-5 introduced for the first time a category of trauma- and stressor-related disorders to identify conditions where adversity is considered to be a significant cause as opposed to one risk factor among many. The ICD-11 proposals contain a similar section. Both manuals also now recognise the occurrence in a trauma context of symptoms such as auditory hallucinations that have traditionally been seen as exclusively psychotic in nature. In my experience of the DSM and ICD there were many robust, well-informed, and sophisticated discussions that attempted to reflect the available evidence as accurately and helpfully as possible, while bearing in mind the deficiencies in the knowledge base.

In my own field, post-traumatic stress disorder, DSM-5 has chosen to increase the number of symptoms whereas ICD-11 proposes to greatly reduce the number (Maercker et al., 2013), an approach that is closer to my own views about the best way forward (Brewin et al., 2009). The ICD-11 approach is largely in the service of clinical utility, attempting to make the recognition and treatment of these disorders easier and simpler. In most countries psychiatric disorders are managed with minimal specialist mental health resources. In these contexts there is a clear need for a common and comprehensible diagnostic system to guide thinking, raise awareness, and educate clinicians and patients. Diagnosis itself need not entail the adoption of a narrow, non-psychological approach, or be necessarily disempowering, although obviously it has been and in some cases continues to be applied in an unthinking and potentially harmful way. This is surely more of a problem with some disorders than others, and can be addressed without doing away with diagnosis altogether.

We have been here before. DSM-I and DSM-II incorporated broad, aetiologically-defined entities that were continuous with normality, based on the idea that the total personality and life experiences of the person had to be understood to provide a context for symptoms (Mayes & Horwitz, 2005). But this approach was heavily criticised for being too subjective, unscientific, and overly ambitious in terms of its ability to explain and alleviate psychological disturbance, and these problems were instrumental in bringing about the introduction of diagnostic categories in DSM-III. By acknowledging their own limitations, diagnostic manuals can do much to emphasise the complexity of psychiatric disorders and about the need always to consider the interplay of biological and psychosocial factors. Perhaps they are like many of our institutions – occasionally baffling and frequently disappointing – but we should be wary of rejecting them without good evidence that there is a better alternative.

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Known and unknown unknowns

The June edition of *The Psychologist* highlighted a number of issues that seem to have a grip on psychology, and appears to reduce the capacity for free thinking within it. Firstly, if we make a leap of faith and combine what we learn from David Dunning's work ('The paradox of knowing') with some famous words from Donald Rumsfeld it is clear that psychology, because it is the study of the mind by human minds, remains not only with many 'known unknowns' but also many 'unknown unknowns'!

Yet psychology, especially published psychology (which, of course, becomes what psychology 'is') does not like 'known unknowns' and, I would suggest, has an allergic reaction to anything approaching 'unknown unknowns'. When studying for my master's a research assistant stated that she 'hated Freud' because he was too wishy-washy and provided unfalsifiable theories; not surprising when this was a similar view held by many of the university staff. So, if a graduate research assistant has been indoctrinated into this view by a number of faculty staff, what does it say for the future of psychology? Yes, Freud was wrong about some things, but he got a few things right and has been quite influential in not just modern psychology but modern life as a whole.

So, how does psychology start to deal with both known and unknown unknowns? Is it capable of doing so and, if it did, what would it mean for psychology? Albert Einstein was a patents clerk when he had some good ideas about energy and stuff. Srinivasa Ramanujan has been described as a mathematical genius but received no formal training in mathematics and did the majority of work alone. How would these two individuals be viewed by the psychological community if they were psychologists (some would probably say they would not be able to be called psychologists if they operated in the same way as they did in their fields!)? Yes, scientific study has a place, and a very important place, but should that be to the detriment of free-thinking and embracing what cannot be quantified?

We are human after all and we all would do well to remember Thomas Kuhn's thoughts in *The Structure of Scientific Revolutions* that the acceptance or rejection of a particular paradigm depends on human preferences as well as the evidence available. What affects someone's 'human preferences' is wide and varied, but could include future income and status ('If I don't get published I won't attract more funding'), vanity ('Being published boosts my ego'), and self-belief ('My reading of the data is correct, not others'); and being able to unpick just those three points and their effect on psychology would probably be more difficult than deciding whether DSM-5 is a help or hindrance for mental health!

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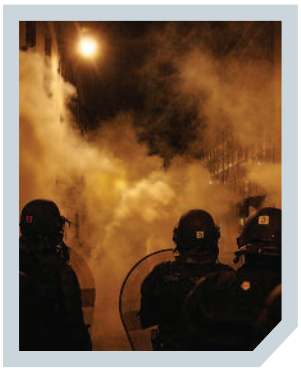

New insight into rumblings of discontent?

A personal research project has been fuelled by the observation that there are more instances of rioting and disorder shortly before earthquakes than would be expected by chance.

I was so intrigued by this little known phenomenon that I began to compile lists of earthquakes and riots for the land area of England and Wales between 1980 and 2012. There may be questions about what is or is not a riot, but my research has found 68 significant widely reported riots during the period in question.

The hypothesis I have tested is: 'The distribution of riots before earthquakes is significantly different to the distribution of riots after earthquakes'. My forthcoming e-book presents statistical tests showing a probability of less than 1 per cent that the hypothesis is not true. This is very low and contrasts sharply with the usual perception that there is no association between the incidence of riots and earthquakes.

This is not an unknown phenomenon: over the years, snakes, dogs, birds and insects have reportedly exhibited unusual behaviour shortly before earthquakes. My main question is why would humans be any different, even if the effects are less obvious?

There may be speculation about how the changes in stress prior to earthquakes could result in the variations of riot distribution and how this may be indicative of a human response to seismic effects. Can we 'feel' remote physical stress such as increases in tectonic stress prior to earthquakes?

Other primary effects of seismicity are thought to be variations of electricity, electromagnetism, hydro-geochemistry, atmospheric conditions, ground gas emissions and gravitational potential. The participation of one or more of these effects could equally explain the human response to seismic activity and this is discussed more fully in my book.

One aim of this letter is to find out if my database of riots would be of interest to the psychological community. I imagine that a website presenting my database may be of some potential benefit to future research projects. A web-based database would also provide an opportunity for the public at large to suggest additions for verification if gaps are identified so that the list can develop as time moves on.

I look forward to any feedback this letter may generate regarding the further development of a database of riots and the implications of my research findings. *Gravity and Mind – Human Response to Tectonic Stress* is available in e-book from Amazon.

Alan Watson
Aylesbury, Buckinghamshire

NLP – Google, rabbit holes and out-of-the-box thinking

Although I do not wish to comment on the scientific basis of neuro-linguistic programming (NLP) as discussed by Bruce Grimley (Letters, July 2013) I did want to caution about the use of Google hits as a form of evidence base. I concur wholeheartedly with Bruce's assertion that 'Google search is not the most scientific way to conduct research', but I thought I would bring up an interesting point or two, as well as some interesting (fun) data on comparative hits.

As most readers will be aware, Google search works on a keyword extraction algorithm, therefore searching for 'NLP' will bring up any word, phrase, or term

with NLP in it, whether related to neuro-linguistic programming or not; for example, there is currently a global research effort into studying how natural language processing (NLP) can be applied to sentiment analysis to improve its accuracy, as most sentiment analysis tools work on keyword extraction algorithms that are not very sensitive to the regional, national and cultural inflections and nuances of language. This is also how Google search is being improved. Therefore, if you enter 'NLP' into Google you get 56,400,000 hits; however, if you enter 'neuro-linguistic programming' you only get 3,060,000 hits whereas if you enter 'natural language processing' you get 18,100,000 hits. Therefore the lesson is to conduct a more specific search of terms you are interested in and don't assume that a certain term, acronym or abbreviation that you are aware of is the only one that exists.

For comparison (and fun) I also searched for 'pseudoscience' (2,340,000 hits), 'homeopathy' (15,400,000 hits), 'black magic' (369,000,000 hits), and even 'Google search' (6,350,000,000), so I'm not convinced that the number of Google hits for NLP is meaningful when discussing the rigour of scientific disciplines, or when compared to other topics on the internet (e.g. 'cats in hats' received 28,600,000 hits)… but I do not wish to go down that particular 'rabbit hole' (23,700,000 hits).

Dr Neil Verrall
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I am an NLP Master Practitioner and use NLP techniques in my work as a part-time therapist. I also use key NLP concepts such as neurological levels, meta-programmes and meta-states in my efforts to construct a theoretical framework – 'Integrated SocioPsychology' – to align and integrate the behavioural sciences.

So I have some considerable sympathy with Bruce Grimley's frustrations (Letters, July 2013) about what I can only call the rather 'sniffy' attitude of psychological academia towards neuro-linguistic programming.

Unfortunately, NLP – or, rather the way it is taught and promoted – is often its own worst enemy. While Grimley does quote some interesting examples of attempts to look at NLP in a more 'scientific' way, all too often many in the NLP 'communities' tend to go in the transpersonal direction and thus eschew empiricism. 'NLPers' themselves are all too often guilty of an equally 'sniffy' attitude towards academia and its demands for rigorous scrutiny.

To name some 'hard truths' which make it hard for academics to take NLP seriously:

- There is no underpinning principle – no paradigm – on which NLP is based. It is a ragbag of models and techniques largely 'stolen' from the likes of early cognitive psychologists like George Miller, linguists such as Alfred Korsybzki and genius mavericks like Gregory Bateson.
- It completely ignores the biological approach. Everything in NLP can be 'fixed' through some psychological exercise or other. So no account is taken of genetic predispositions, of neurotransmitter fluctuations or variations in brain structure, etc., etc.
- There is no overt, publicly accountable attempt to understand why NLP techniques work for some people but not others. (Personally, I think the suggestibility of the 'client', perhaps as measured by Hilgard's Stanford Hypnotic Susceptibility Scale, may be a critical factor. However, I'm not aware of any research directly linking NLP therapy success to suggestibility.)

Perhaps for me the single biggest concern with regard to NLP is that many trainers encourage their Practitioners to try out advanced and powerful techniques – highly potent for some, at least! – with no more understanding of psychology than 20 hours practitioner training (20 hours was the old INLPTA criterion; but, with the entry of Paul Daniels, etc., into the training field, seven days and advance materials seem increasingly to be the norm). Nor, from my experience of NLP trainers, does there seem to be much imperative to get a grounding in psychology. An NLP Practitioner or Master Practitioner qualification seems to be all the psychology NLP trainers seem to think their students need before turning them loose on the world. It's a bit like giving a Ferrari to someone who's just passed their driving test.

It may turn out fantastically well or it may result in complete disaster – at a very accelerated pace!

As Grimley points out, NLP does have a great deal to offer – though it lacks cohesion as a supposed discipline and there many dubious aspects to some of the cultures practising it. But, to steal from the title of Grimley's letter, does academia have to have 'all-or-nothing thinking about NLP'? NLP founders Bandler and Grinder cherry picked what they felt would work for them from the various schools of psychology and then they and other 'NLPers' developed them – in some cases way beyond their original forms. Why can't academic psychologists pick from the NLP menus, test, evaluate and incorporate what enlightens and/or what works? There is no hard-and-fast imperative that psychological academia either has to accept NLP *in toto* or reject it *in toto*.

We need some out-of-the-box thinking to take from NLP what can truly benefit and ignore the more fantastical and ill-founded claims.

Keith E Rice
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Advice on media ethics

In response to Phil Boyes' letter ('Working with media agendas', July 2013), as Chair of the Society's Media Ethics Advisory Group, I would like to expand a little on the important points that were raised.

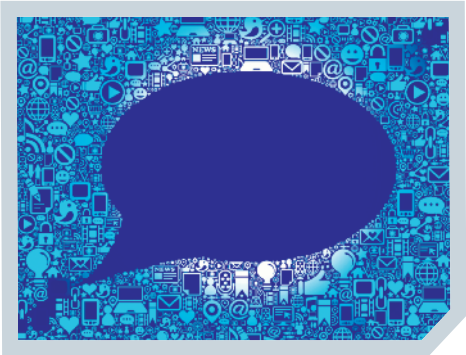
Phil is right to say that engaging with the media is an excellent opportunity to promote psychology and individual research. After all we hope our research will better inform society at large.

However, as Phil has said, sometimes we also have to try to engage with the media (particularly programme makers) in recognising and resolving what we identify as ethics issues in how they treat topics and participants, or to cease our engagement because no common ground can be found.

The Society's Media Ethics Advisory Group was established just over three years ago to do just this. We are a small group of members with many years' media experience in a range of programmes. We were formed from members of the Ethics Committee and the former Media and Press Committee following an unprecedented number of concerns over the nature of some 'reality' television programmes. As well as engaging with programme makers and broadcast companies, we have also played a central role in developing and promoting proposals for new legislation to better safeguard children in performances.

When the Society's communications team is contacted by, in particular, television production companies, there is an initial discussion about what the programme makers are planning, and any ethics concerns are also raised. The communications team explains the ethics help that is available through our advisory group.

We work on a rapid-response basis to get in touch with the production company as quickly as possible to help with more expert ethics advice on a one-to-one basis.



At this point I would like to thank my colleagues on the group for their work. These enquiries can come at any time and overall colleagues have been able to respond quickly to point out problems, help find ethical solutions and in some cases even persuade the company not to pitch that programme idea to programme commissioners, or to explore alternative means of achieving the production aims.

Communications is about engaging and getting good evidence-based research out there, in ways that a broad public can understand. It is also about upholding the ethics standards of our profession, and communicating why a programme idea fails an ethics test, and indeed may be potentially harmful and distressing for participants and/or viewers.

Some of the evidence to the Leveson Inquiry reminds us how much distress can be caused by unethical media practices. Our advisory group works with and on behalf of the Society to intervene at the earliest possible stages in productions to minimise risks, and to promote the key ethics principles of respect for the autonomy and dignity of participants, scientific value, social responsibility and beneficence.

John Oates
Chair of the BPS Media Ethics Advisory Group

obituary

William T. Powers (1926–2013)

'Behaviour is the control of perception.' This tenet is at the heart of the pioneering work of William ('Bill') T. Powers, who died on Friday 24 May 2013 in Lafayette, Colorado, USA. Bill was born in Illinois in 1926 and grew up with a fascination for science that took him from the domains of astronomy, through control engineering, to his theory of living systems and social science – perceptual control theory (PCT). Bill worked tirelessly to share his vision with anyone who tried to grasp it.

He told us that when he accompanied his mother on her work in psychiatric wards he encountered many diverse and bizarre expressions of the human mind and he wanted to understand them. He later regarded these as signs of conflict between underlying layers of homeostatic control systems within the nervous system. Spurred on by the cybernetics movement of the 1940s, Bill began work on his unified theory of control in living organisms.

The field of psychology owes an enormous amount to Powers' theory, that began in 1960, but is probably best known for his 1973 book *Behavior: The Control of Perception*. Thomas Kuhn, Professor for the Philosophy of Science, once stated it as 'among the most exciting I have read in some time; the achieved synthesis is thoroughly original'. During the 1980s, this book informed the development of Carver and Scheier's work on self-regulation and Vallacher's theory of action identification. In recent years, Timothy Carey used PCT to develop the 'transdiagnostic' psychological therapy, Method of Levels, which has been evaluated and disseminated over the last decade.

Yet this work reflects a small part of Bill's contribution to scientific knowledge as a whole. It is possible to track the influence of PCT to sociology, within the development of Heise's affect control theory and Burke's identity theory. A wider impact of PCT is emerging year-on-year as new applications are published. To take two diverse examples, a trial of a PCT-based traffic incident control system in Dalian, China, improved accuracy and response time, and a PCT-based intervention in schools across North America improved student attendance and achievement. The world has lost one of its greatest thinkers and many of us have lost a cherished friend and collaborator.

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