

Firing pea-shooters at elephants

Julian Boon and Lynsey Gozna rail against the futility of much psychological research, with particular reference to the 'chameleon offender'

More than 35 years ago, computer scientist and cognitive psychologist Allen Newell famously asserted that 'you can't play 20 questions with nature and win' (Newell, 1973). Referencing a quiz show in which contestants attempted to guess, with up to 20 questions, an object that the quizmaster held written on a card, Newell was alluding to the near complete failure of psychological research in achieving anything approaching unified growth. There was a ceaseless stream of research into interesting facets of psychology, yet no corresponding growth that in any way coalesced into a unified science. In contrast to sciences such as physics, mathematics, chemistry and biology, the endeavour of psychology was a fruitless, pseudo-scientific one – a 'discipline' characterised by pockets of disconnected research that, although employing a multitude of researchers, is nevertheless hopelessly doomed to failure as a scientific enterprise.

Newell's assertion had a big impact at the time. For a while psychologists in the UK and US questioned the wisdom of utilising the hitherto near-universally accepted empiricist model of science as a basis for the study of psychology. However, in a remarkably short time thereafter – and without any coherent intellectual rationale for doing so – psychology reverted to type. It was business as usual. This was characterised by an expectation that canons of empiricism and large-scale data-collection would engender sound theory of

sufficient utility to underpin unified scientific growth.

In short, for a brief episode psychologists accepted there was a fundamental problem in adopting empiricism as the basis for the study of psychology. But then the issues were shoved into the 'too-difficult-to-do' drawer, and people reverted to *more*. *More* data collection, *more* careers validated in terms of the output of 'peer-reviewed' papers – regardless of the long-term credibility of their scientific utility – and *more* vain hope that one day it will lead somewhere. But it has not led to *more* in terms of a growth in bona fide scientific psychology. We should be questioning why.

Newell, however, was not alone in pointing out the futility of adopting such an approach. Other serious thinkers on the epistemology of science have repeatedly pointed out the self-serving ridiculousness of carrying on as we do (see, for example, Kline, 1988; Yuille, 1986). For decades Norman Wetherick (2003) has articulated – with razor-sharp terms and logic – how the adoption of the empiricist paradigm for researching or developing a genuinely scientific psychology has had catastrophic consequences. In so doing he has not only exposed the lamentable status quo but has also constructively advocated an alternative approach rooted in the philosophical tradition of 'naive realism' (Bhaskar, 1989, 1998). This, he has compellingly argued, would provide a way forward that could underpin a basis for a genuinely, scientific study of psychology.

However, it is not the remit of this article to yet again expound realism and its research implications in detail. Instead the aim is to take one particular domain that is truly psychological (much of what passes under the banner of psychology is not) and illustrate the impossibility of successfully studying it while using the currently adopted empiricist approach. Like Wetherick, we do not simply seek to point out that the current Emperor has no clothes and state the fact that – with notable exceptions – he has been starkers for much of psychology's 100-year-plus history. Instead we lay out suggestions as to how things *could* be productively progressed. The suggestions involve a lot more deep thinking and talent, and a lot fewer 'psychologists' collecting data. In short the thinking and theory must come first and the data acquisition be used to test the product of that thinking. This is horse about cart from the techniques that have blatantly failed psychology for more than 50 years – well before Newell's prophetic statement.

Modelling the mind

Albert Einstein asserted that the role of the scientist is to make a model of reality. In the case of psychology we are (or should be) making a model of the mind – something that itself is a continuously developing model maker. This requires the removal of the padlock on the 'too-difficult-to-do' drawer, a great deal of thinking and theoretical work, and rigorous testing of the contingent theory thereafter.

In line with the above arguments – that the thinking must come first – we suggest that there are four prerequisite steps for successfully developing a scientific understanding of psychological phenomena. Initially it is necessary to identify at the broadest level possible the core issues at play in any given domain of psychological interest. The next step is to give the utmost care in considering how the identified variables interact among and impact upon one another. After that it is

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necessary to consider the myriad of extraneous factors that also impact on the first two steps. Once a model of reality has been developed – and only then – should the model be tested empirically. This fourth stage would lead to one of two possible outcomes. The first outcome might be to test the model and find it to be of such limited utility that it is rejected outright and another model is substituted. The second would be to apply the model and commence an iterative process of refinement – retaining that which works and developing or jettisoning that which does not.

That is the way it is done in what has been called the ‘hard sciences’ and, although there are special considerations at play in psychological research (see Boon, 1998), there is ultimately no scientific reason why it cannot be done in psychology too. Did the physicists working under the Alps on the Large Hadron Collider (LHC) project test first and develop their theories from

the amassed data later? Certainly not – instead, on the basis of very profound thinking they developed the theory first and went on to test how accurate that theory was. To quote one leading physicist, referring to the Higgs boson (the most sought after particle in physics) just before the LHC was fired up:

I think it will be much more exciting if we don't find the Higgs. That will show something is wrong, and we need to think again. I have a bet of \$100 that we won't find the Higgs. (Hawking, 2008)

The underlying credo is that eventually

there will be one big unified theory – until which time competing models are refined in a bid to encompass ever more phenomena in the domain of science under consideration.

If one adopts a similar approach in psychology it quickly becomes clear that for this to be successful, it is vital that the approach adopted is *in vivo* as opposed to *in vitro*. Let's remind ourselves of the arguments for and against these respective approaches. *In vitro* research appears to have the advantages of having a tightly defined and controlled experiment. This is supposed to yield correspondingly specific and relatively unambiguous findings. The ultimate objective is for the myriad of these

in vitro experiments, to one day ‘join-up’ and develop into a unified understanding. However *in vitro* research also has its disadvantages. In order to obtain the required control, of necessity the foci of reference have to be of an ultra-limited nature relative to the overall domain of

interest. In isolating so many variables in order to maintain control over experimental focus we strangle the interpretative value of the exercise by producing findings of an extremely limited nature. In effect adopting this approach in psychology means we get to know more and more about less and less until we know pretty well everything about nothing of psychological significance whatsoever. It is therefore unsurprising that there has been no unified growth. In adopting the empiricist, *in vitro* approach we might as well be trying to stop a herd of stampeding elephants with nothing better at our disposal than firing pea-shooters at them.

The result? Peas landing randomly all over the place – with the chances of stopping even one single elephant being virtually nil.

Likewise for psychology, we see experiments all over the place yet not developing in the unified way essential for

genuine progress to be made in any science. Is it therefore surprising that we have so many conferences, so many parallel sessions, so many soon-forgotten papers, and so little truly productive output?

In contrast, proponents of *in vivo* research argue that there are major benefits to be derived from its holistic rather than piecemeal approach (see, for example, Lipinski & Hopkins, 2004; Yuille, 1986). Studying phenomena in the natural habitat, rather than in isolation amid artificial conditions of the laboratory, *ipso facto* ensures relevance to applied concerns. Furthermore, consideration of the big picture of the inter-dynamic variables ensures that the contingent research would be inherently unified. The importance of this for our own area, research in applied forensic psychology, could scarcely be more self-evident.

Yet *in vivo* research too comes with disadvantages, chief of which is that it is a lot more difficult to do. However, it can be done; and here we intentionally take one of the knottiest issues in forensic psychology to illustrate how and why it must be done.

The challenge of the chameleon

The chameleon offender (CO) is an offender who adapts psychologically over time and circumstance, continually evolving and presenting in differing ways. Accordingly, this issue provides an especially effective illustration of the difficulties involved in the pursuit of a genuinely scientific psychology.

In identifying the core issues and variables let us look at the nature of the challenges involved. It is necessary to recognise that each offence is different. It is also necessary to recognise that each offender:

- | is different;
- | may themselves be different at different times;
- | may be different across different interviews;
- | can be different across different offences;
- | may be different with different professionals; and
- | can even be different within any given interview.

To embrace all these core variables is at first sight redolent of a vaudeville act where on stage 30 or more plates are kept spinning on sticks simultaneously. Just as the last plate is set up the first ones begin to wobble and need further attention from the performer until all of them are spinning in harmony. Likewise, to have any meaningful understanding of one CO variable we must have an understanding



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of how it fits with all the others. Collectively, therefore, understanding our chameleon offender would seem to be a very formidable challenge to research, but with the aid of a coherent dynamic model it can be addressed. Furthermore, its utility can be assessed by subjecting it to the most rigorous testing of all – applying it in the field.

Unfortunately though, too few of us in psychology are prepared to simultaneously spin all those plates on the end of sticks and undertake the essential depth of scientific endeavour. In an attempt to remedy this during the past two years the model outlined in Figure 1 has been validated both through a series of interviews with various client groups in forensic settings and thereafter with forensic practitioners. In addition, it has been implemented in advanced interview training courses for police detectives who are in daily face-to-face contact with COs. These officers work in the *real* world with *real* people and as such require *real* solutions to their own professional challenges when interacting with complex individuals. Hence the provision of *real in vivo* psychology is not just a desirable option, but essential. In conducting fruitful psychological research, what is required is the methodological equivalent of highly focused and accurate heat-seeking missiles (cf. pea-shooters) that are trained on a moving target and that require constant adjustments to be computed into the remit. Here we refer to the challenge of the chameleon offender – yet as we shall see later the same point can be made for any attempt at studying psychological phenomena.

The rationale for including each of these variables is beyond the scope of this article, though for interested readers this is outlined in other papers (Boon & Gozna, 2008a, 2008b; Gozna & Boon, 2009), and the forthcoming practitioner handbook on dealing with the chameleon offender (Boon & Gozna, 2009). Furthermore, the utility of real-world research has been discussed more widely (see Gozna & Prendergast, 2008) in relation to fusing investigative and clinical domains within forensic psychology. For the purposes of this article though it is important to grasp that drawing together these core variables has only been possible by adopting the *in vivo* approach: by directly and extensively observing real-life police interviewing, together with post-conviction interviewing, and then defining the salient variables.

It should be emphasised that the

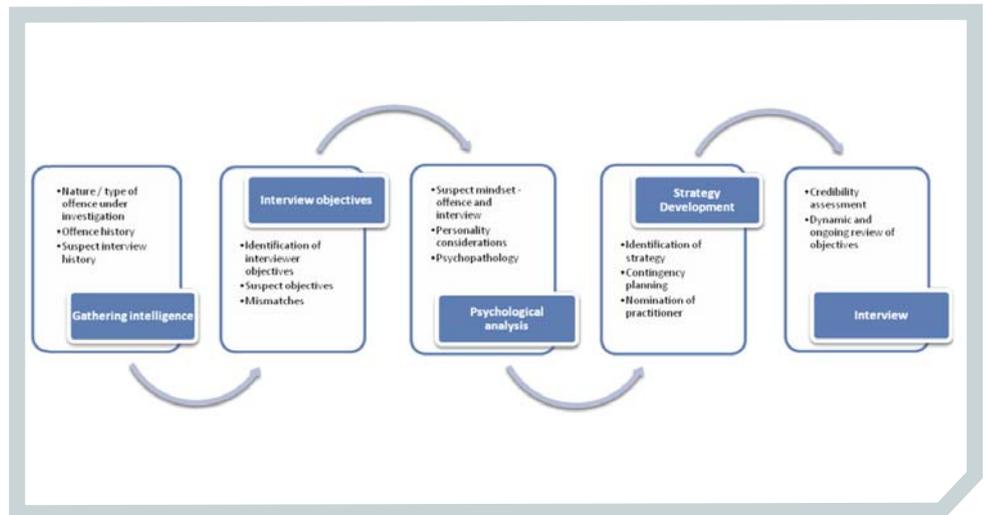


Figure 1: The bespoke chameleon model for understanding complex forensic interactions

variables are highly interactive, and in-depth training is required to master their contemporaneous handling. So the task is exponentially more challenging than the vaudevillian keeping his independently rotating plates in the air. However, once mastery of the necessary skills involved has been achieved, the rewards can be enormous – the skills to construct a proactive, bespoke approach in dealing with the unique characteristics of any given CO at any given time. In essence it allows the practitioner to get the best possible picture of the mindset of the offender, not only at the time of interview, but also at the time of the offence(s). It also permits the interviewer to engage in highly educated planning; developing focused and effective contingency plans with which to anticipate and deal with the CO's changes as the interaction unfolds.

Although highly effective for interviewing work, the use of the CO model is not confined to that domain. It has also been found useful in the fields of treatment work, risk assessment and psychological profiling. Again using the realist and *in vivo* approach has yielded psychological research with currency in diverse domains – not confined to piecemeal pockets of interest. That breadth of relevance and application is a hallmark of unified growth.

It is our submission that the development of this applied model simply could not have been conceived using the ubiquitous empiricist laboratory technique. However, while we have illustrated how a highly effective psychological model can be developed to deal with the chameleon offender, the point of this article is markedly broader than that. We could instead have similarly illustrated this by focusing on

other domains of psychological interest (in social psychology, cognitive psychology, personality theoretics, evolutionary psychology etc.). We wish to be entirely clear here. The force of these arguments is not confined to forensic psychology – they apply every bit as much to any area of psychology that is quintessentially 'psychological' (cf. psycho-pharmacology, for example).

We anticipate resistance to our critique of psychology, and the energy behind such resistance is welcomed in the form of scientific debate. Alternatively, readers may wish to wall themselves off from such debate as a result of self-defensiveness – that's obviously the easy option. Yet can we with integrity be content to maintain current sterile practices in a warped spirit of tradition simply because it is what most 'psychologists' have always done? We hope that looking toward the next 30 years will result in those of us calling ourselves psychologists beginning to observe and embrace more of the global psychological complexities and less of the minutiae; in this way working toward the production of research that has as its objective – unified growth.

In 1789, Benjamin Franklin wrote: 'In this world nothing can be said to be certain, except death and taxes.' We respectfully beg to differ. If there is no change in the current mainstream way of prosecuting our attempts at studying psychological phenomena, there is a third certainty: that the next 30 years will deliver exactly what the last 30 has done, the acquisition of masses more data that yields nothing in terms of bona fide, unified scientific growth. Ultimately, you can't play 20 questions with nature and win.

