Against cognitive

NORMAN WETHERICK *hates mindless data collection – 'Who's asking the big questions?'*

FIFTY years ago, I embarked on a philosophy degree hoping for an answer to a central question. How, by purely natural means, could beings have evolved in a physical universe that were capable of understanding it? Beings capable of generating all our sciences, political and social systems, works of art and literature. The usual answer was (and often still is) that all this is made possible by supernatural intervention. God gives us minds/souls which, being immaterial (i.e. non-physical), can have any properties we like. Such an answer is utterly unacceptable to me.

I was disappointed to find that the question was not addressed in British philosophy departments at the time. I discovered 20 years later that it was addressed by philosophers on the continent of Europe, but by then I had concluded that psychology promised more and had taken another first degree (in 1960). My hopes appeared reasonable at the time. Though a proportion of published studies in psychology were intended to solve practical problems, many were not. They appeared to be meant as contributions to the solution of my central problem – the development of a natural scientific understanding of the nature of human nature.

My undergraduate studies in psychology were concerned almost wholly with the behaviour systems of the American theorists – Hull, Tolman, Guthrie, and so on – but 1960 marked the end of all that. There followed an era of cognitive experimental psychology in which, I think, the object of the exercise was forgotten. Experimental studies came to be valued for the precision of their design and the sophistication of their statistical analyses – not for their contribution to our understanding of the phenomena. Preliminary conceptual analysis was neglected, perhaps due to the fact that it is easy to devise interesting (but ultimately uninformative) experimental projects, when both experimenter and subject are intelligent human beings. Real science is not so simple.

What have we actually learnt?

What we used to call the short-term memory remains a topic of interest in cognitive psychology. Why is that? Early on it appeared that the length of the list of digits a person can recall accurately after one hearing is a good indicator of general intelligence. That seemed to show that the capacity of some kind of short-term store might be important. But it was also established that this 'capacity' depends on the material to be recalled. Seven items (plus or minus two) for digits, fewer for letters, many more for connected prose. Material, moreover, does not have to pass through a short-term store to get to the long-term memory; nor does it have to do so on the way from the long-term memory to expression in verbal or other behaviour (e.g. we do not compose sentences in our heads before uttering them). Renaming the topic 'working memory' has (perhaps inadvertently) made the position clear.

Everything that is of real psychological interest is now a function of the central executive, about which not much is known except that it may be located in the frontal lobes. Temporary stores for verbal or visual material are sometimes required but these are surely of minor importance. Recently, an additional store – the episodic buffer – has been postulated, whose function is 'to combine information from the LTM with that from the slave systems' (Baddeley, 2001). All this makes the central executive look more than ever like what we called the mind, all those years ago. What has been gained?

If one seeks an approach to the topic that does justice to its inherent conceptual complexity, there is Varela (1999). His study of 'present-time consciousness' calls on state-of-the-art mathematics and neurophysiology to begin elucidation of the many vital functions of the 'working memory' that go beyond remembering a telephone number long enough to dial it. Its function is to relate what we already know about what predicts what in the external world to our present desires and capacities, so as to maximise by our behaviour our chances of satisfying the former and, ultimately, of surviving. For this purpose a moving time sample is required which includes a few moments ago and what seems likely to happen in the next few moments – what William James called the 'specious present', the span of time covered by a single act of awareness. In work on the so-called central executive there is rarely any reference to what is happening there or why – except in the vaguest possible terms.

Studies of perception began even earlier than those of short-term memory. Investigators were fascinated by visual illusions, which appeared to show that intelligent people could make apparently stupid observational errors and could not avoid them by conscious effort. I periodically ask colleagues in this area what they have actually learnt about visual perception, but get no satisfactory reply. In 1913 Wolfgang Köhler asked 'How is it that visual illusions are pervasive but do not prevent a successful interaction with the environment?' He continued: 'This fact might easily be explained if we regard as...the biologically primary reality, not sensations but, for the most part, things.' (Translation by A.C. Zimmer, cited in Albertazzi, 2001, pp.259–260.)

WEBLINKS

Francisco Varela's home page:
http://web.ccr.jussieu.fr/varela/index.html
Francisco Varela's obituary: *http://psyche.cs.monash.edu.au/v7/psyche-7-12-thompson.html*
The experience and perception of time:
http://plato.stanford.edu/entries/time-experience/
Johnson-Laird and Byrne's mental models website:
www.tcd.ie/Psychology/Ruth_Byrne/mental_models

psychology

I recently attended a public lecture by Professor Richard Gregory, who considered several of these illusions and told us how our susceptibility to them could be explained by his theory of perception. He was unwilling to suggest any answer to Köhler's question but it may of course be answered simply by pointing out that the visual illusions are almost never encountered in interaction with the environment, only in the perception of two-dimensional representations. The latter have not been around long enough (20 to 30 thousand years) to influence the evolution of the visual system, which evolved to perceive the three-dimensional world of things, as Gibson has shown (e.g. Gibson, 1986). Organisms encountered no survival contingencies that depended on not being misled by a visual illusion – if they had done so, the system would have evolved to cope. No significant advantage would accrue to us, even if we were never ever misled!

'It is easy to devise interesting (but ultimately uninformative) experimental projects... Real science is not so simple'

So have psychologists simply been wasting their time chasing this particular illusion? Recent, useful, research in the area includes Aglioti *et al*. (1995). There are in fact two visual systems in the brain: one regulating behaviour and one, conscious experience. The former is not misled by visual illusions (not, at least, by the Titchener circles) but the latter is. The conscious experience system has evolved only so far as was necessary for its purpose.

Most perceptual theorists continue to insist (irrationally in my opinion) that the business of perception is to make models of the real world (who or what evaluates the models is never specified). But if we do not 'perceive' the real world, if we really do have nothing but models to go on, then of course thinking can only proceed by manipulating these models. Mental models theory (Johnson-Laird, 1983) purports to account for all kinds of rational thinking but offers no evidence that anyone consciously employs models – they must be supposed to operate in the unconscious. And, of course, no account is given of who or what evaluates the models: establishes that they are correctly formed, that there are the right number of them, and so forth.

I would argue that mental models theory has been a fertile source of experiments but not of understanding. The argument was for a time conducted through experiments involving Knights and Knaves. In this scenario, everyone on an island is either a Knight or a Knave. They are indistinguishable in appearance but Knights utter only true propositions and Knaves only false ones. The problem is to decide whether a speaker is a Knight or a Knave by what he says. But all parties to the argument for and against mental models overlooked the implications of the fact that no one on the island can say 'I am a Knave'. Knaves may utter the words as part of a conjunction ('I am a Knave and B is a Knave', meaning 'we are both Knaves') as long as the other part of the statement is untrue. But they cannot use the words as part of a disjunction ('I am a Knave or B is a Knave or both'), since the first part of the disjunction would then be true. For Knights the reverse is the case: the conjunction would be false, but the disjunction could be true as long as B was in fact a Knave. Many of the problems employed can be solved by considering only one assertion (not two or more), and people naturally find these problems easier – this has nothing whatever to do with the presence or absence of mental models.

Wason's (1966) selection task assumes that people should employ the propositional calculus (which is a logician's technical device, not a theory of thinking). If one goes to the park to see whether all the swans are white, the propositional calculus appears to suggest that one should examine all the swans (to make sure than none of them is non-white) and all the non-white things (to make sure that none of them is a swan). This is impractical since there are too many of the latter. Most investigators would be worried by the fact that 95 per cent of participants give what Wason regards as the wrong answer. But the answer they give is in fact correct; it follows that there is no problem here worth investigation. Useful work on the relation between logic and psychology is thin on the ground but I have hopes for Wetherick (2002).

Answer the question

I am not an applied psychologist, and it may be that cognitive psychology has made contributions of value in that area. My objection is to its failure to contribute to pure psychology – the science. Experimental cognitive psychology has lost sight of the object of the exercise (a scientific understanding of human nature) and has turned instead to details of experimental procedures that may once have had that object in mind – but only when first introduced, fifty or a hundred years ago.

Some say that current practice is required by empiricist scientific theory: that the scientist is required to collect data, and theories will 'emerge'. But collecting data is just too easy in psychology, and there is no limit to the amount that can be collected. This version of empiricism is, in any case, wrong – it was abandoned by the other sciences a century or more ago. I have even seen it suggested that experiments may be regarded as ends in themselves but that is, in my view, a status reserved for works of art. What is important about a scientific experiment is that it should try to answer a significant question. Experiments in cognitive psychology usually fail this test.

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