

Is the 'cognitive revolution' a myth?

Sandy Hobbs thinks the term is inaccurate and unhelpful; Jeremy Trevelyan Burman disagrees

Sandy Hobbs

The term 'revolution' implies the dramatic overthrow of something previously dominant, either political (e.g. Russian) or intellectual (e.g. Copernican). The 'cognitive revolution' refers to the overthrow of behaviourism, which supposedly dominated psychology in the early 20th century, during which time the study of cognitive processes was largely ignored. This is historically false.

Although behaviourism had many supporters from the 1920s onwards, other approaches (e.g. gestalt and psychoanalysis) flourished alongside it. It was not a monolithic movement: witness the rival learning theories of Guthrie, Tolman and Hull. Lovie (1983) and Dewsbury (2000) show that research on attention and animal cognition was common at the time. It is true that since the 1950s many psychologists see themselves as 'cognitivists', but this cannot reasonably be called a 'revolution'. There continue to be different theoretical perspectives that flourish side by side within psychology. The most relevant 'non-cognitivist' approach in this context is radical behaviourism, a movement that has grown since the 1950s, the very time the cognitive revolution supposedly took place. Radical behaviourists exist in relative isolation from other psychological approaches. For example, there is little cross-referencing (Coleman & Mehlman, 1992).

Leahey (1992) has called the revolutions in American psychology

'mythical'. Why do some psychologists spread myths about their own subject? I can offer a couple of hypotheses. Many of us would like an agreed unified paradigm for research and theorising in psychology. This has not happened, but to refer to a 'cognitive revolution' may allow proponents of cognitivism to pretend that theirs is such a paradigm. Some people have origin myths that account for the existence of their particular tribe. Samelson (1986) argued that to present Comte as the founder of social psychology was such an origin myth. Perhaps the notion of a cognitive revolution is an origin myth too.

Jeremy Burman

There is a distinguished and eminently worthy tradition in the history of psychology of correcting falsehoods, exaggerations, and myths. Indeed, in my own area, one of the most important examples of this 'debunking' rebuts the 10 errors most commonly found in readings of Jean Piaget's theory of developmental stages (Lourenço & Machado, 1996). But in making claims of 'correction' it is one thing to point to a mistranslation or a neglected text. It is quite another to argue the inexistence of a complex social movement.

In making this case, Sandy Hobbs uses



terms like 'historically false', 'flourished' and 'origin myth'. More than a problem of semantics, the kind of thinking typically associated with such language can be historically problematic. In complex cases, it is often associated with the premature articulation of a conclusion; anticipating a body of evidence rarely explored in sufficient depth or detail. The frame it provides also biases the subsequent discussion toward an overly simplistic question. In this case: Do you believe that there was a cognitive revolution, or not?

It is certainly the case that a body of critical work supports the 'no revolution' hypothesis. As Hobbs notes, Leahey (1992) did conclude – on the basis of an analysis of the meaning of Kuhn's term 'scientific revolution' (Kuhn, 1962/1993, 1987/2000) – that no such thing had occurred in psychology. A citation analysis conducted by Friman et al. (1993) then supported this conclusion, as did a more recent philosophical examination by O'Donohue et al. (2003).

Nevertheless, many authors of similar persuasion have argued that something definitely did happen, whether 'whatever it was' meets Kuhn's criteria or not. For example: Greenwood (1999) concluded that the change we label 'a revolution' occurred as a result of the replacement of operationalised variables (behaviourist descriptions) with hypothetical constructs (cognitive causes). Mandler (2002) suggested that 'the revolution' was really 'a return' to pre-behaviourist attitudes and trends, supported by imports from Germany, Britain, France and Switzerland (including Piaget).

Of course, none of this addresses the underlying historiographical issues. It merely shows that the question itself is more complicated than one of bipolar belief: there is no 'yes' or 'no' answer. And although we cannot omit findings simply because they are inconvenient, we certainly can hope that – contrary to Hobbs's suggestion – there is more to the cognitivist position than the unifying cry, 'With us or against us!'

references

- Baars, B.J. (1986). *The cognitive revolution in psychology*. New York: Guilford Press.
- Berlyne, D.E. & Piaget, J. (1960). *Théorie du comportement et opérations [Theory of behaviour and operations]*. Paris: Presses Universitaires de France.
- Boden, M.A. (2006). *Mind as Machine: A history of cognitive science*. New York: Oxford University Press.
- Brock, A.C. (Ed.) (2006). *Internationalizing the history of psychology*. New York: New York University Press.
- Cellérier, G., Papert, S. & Voyat, G. (1968). *Cybernétique et épistémologie [Cybernetics and epistemology]*. Paris: Presses Universitaires de France.
- Chiesa, M. & Hobbs, S. (2008). Making sense of social research: How useful is the Hawthorne effect. *European Journal of Social Psychology*, 20, 29–44.
- Coleman, S.R. & Mehlman, S.E. (1992). An empirical update (1969–1989) of D.L. Krantz's thesis that the experimental analysis of behavior is isolated. *Behavior Analyst*, 15, 43–49.
- Cornwell, D. & Hobbs, S. (1976). The strange saga of little Albert. *New Society*, 35, 602–604.
- Cornwell, D. & Hobbs, S. (1984). Behavioral analysis of metaphor. *Psychological Record*, 34, 325–332.
- Dewsbury, D.A. (2000). Comparative cognition in the 1930s. *Psychonomic Bulletin and Review*, 7, 267–283.
- Friman, P.C., Allen, K.D., Kerwin, M.L.E. & Larzelere, R. (1993). Changes in modern psychology: A citation analysis of the Kuhnian displacement thesis. *American Psychologist*, 48(6), 658–664.
- Goldstein, J. (1984). Foucault among the sociologists: The 'disciplines' and the history of the professions. *History and*

Sandy Hobbs

I began my argument by stating what I took to be the normal meaning of the term 'revolution', then made a case for the view that it was an inappropriate word to apply to the history of 20th-century psychology. Jeremy Burman does not deal with the term 'revolution' but instead objects to other phrases I use, 'historically false', 'flourished', 'origin myth'.
Historically false I employed to describe the belief that behaviourism for a time dominated psychology and cognitive



processes were ignored. He does not counter the evidence I cited and indeed cites additional evidence in favour of my case. *Flourished* I applied to the development of radical behaviourism since the 1950s. Burman offers no evidence to the contrary. *Origin myth* I offered as a hypothesis, asking whether the position of cognitive psychology might be analogous to the widespread misapprehension about the origins of social psychology. It may not be, but Burman does not provide any counterargument. His response is the vague phrase 'something definitely did

happen'. However, he does not explain why it is appropriate to call that 'something' a revolution.

There is nothing in Burman's reply to make me question my original position. Throughout the 20th century, and into the 21st, different paradigms for psychology have existed side by side. In these circumstances, to employ the term 'revolution' to any movement at any time seems inappropriate. Burman suggests my approach is associated with 'premature articulation of a conclusion'. Surely it is those who proclaim a 'revolution' who are premature.

Jeremy Burman

The evidence does indeed seem to support the re-examination of histories celebrating the 'cognitive revolution'. However, the 'no revolution' position runs contrary to the prevailing viewpoint. Therefore, in the resulting debate, explaining our collective and continuing ignorance requires more than a hypothesis supported by an appeal to authority.

In the study of history, there is a pejorative term for inadequately supported contrarian conjecture: 'negationist revisionism'. The distinction

between this and the 'good kind' of revisionism (which I previously called 'debunking') relates ultimately to the treatment and reporting of evidence. This point was made powerfully in *Denying History*, published in 2000 by Michael Shermer and Alex Grobman:

...real revision – the modification of history based on new facts or new interpretations of old facts – is not only a legitimate activity of historians' profession, it is a necessary tool...
 The prevailing viewpoint on any

historical topic, including the origins of Western philosophy, can be questioned and plausibly revised when the participating scholars play by the rules of science, logic, and reason. That is, as long as scholars put their claims forward as testable hypotheses, then those hypotheses can be weighed against the evidence and accepted or rejected in relation to other interpretations. (p.238)

The key question, moving forward, is how to do that in this case.

Sandy Hobbs

For over 30 years, I have advocated precision and caution in debate (e.g. Cornwell & Hobbs, 1976; Cornwell & Hobbs, 1984; Hobbs, 2002; Hobbs & Chiesa, 2003; Hobbs & McKechnie, 2007), so I should be upset to find myself described as 'debunking' and engaged in 'negationist revisionism'. However, Burman's treatment of me as a 'contrarian' is based on his unsubstantiated assumption that my views conflict with a 'majority'. What

population does he have in mind? How did he classify their opinions?

In any case, I would prefer if Burman were to deal with the specifics of my case rather than subject my position to broad and vague generalisations. He does not seem to grasp that I am arguing that the phrase 'cognitive revolution' is inappropriate and misleading. To aid clarification I shall restate my case in separate points which I invite him to

"Surely it is those who proclaim a 'revolution' who are premature"

answer one by one.

- A. The term 'revolution' implies the overthrow of a regime.
- B. 'Cognitive revolution' implies the overthrow of a hegemonic behaviourism.
- C. Behaviourism was never dominant in psychology to the exclusion of other perspectives.
- D. Cognitive issues were never excluded from mainstream psychology.
- E. The term 'cognitive revolution' is thus inappropriate.

In the paper that drew me into this

Theory, 23(2), 170–192.
 Green, C.D. (1996). Where did the word 'cognitive' come from anyway? *Canadian Psychology*, 37(1), 31–39.
 Greenwood, J.D. (1999). Understanding the 'cognitive revolution' in psychology. *Journal of the History of the Behavioral Sciences*, 35(1), 1–22.
 Hobbs, S. (2002). New sociology and old psychology. In B. Goldson, M. Lavalette & J. McKechnie (Eds.)

Children, welfare and society (pp.29–41). London: Sage.
 Hobbs, S. & Chiesa, M. (2003). Errors of omission and commission: The analysis of misrepresentation in secondary sources. *History and Philosophy of Psychology*, 5, 46–56.
 Hobbs, S. & McKechnie, J. (2007). The balance model reconsidered: Changing perceptions of child employment. In B. Hungerland, M.

Liebel, B. Milne & A. Wihstutz (Eds.) *Working to be someone: Child focused research and practice with working children* (pp.225–231). London: Jessica Kingsley.
 Kuhn, T.S. (1993). *The structure of scientific revolutions* (3rd edn). Chicago: University of Chicago Press. (Original work published 1962).
 Kuhn, T.S. (2000). What are scientific revolutions? In J. Conant & J.

Haugeland (Eds.) *The road since structure* (pp.13–32). Chicago: University of Chicago Press. (Original work published 1987)
 Leahey, T.H. (1992). The mythical revolutions of American psychology. *American Psychologist*, 47, 308–318.
 Lourenço, O. & Machado, A. (1996). In defense of Piaget's theory: A reply to 10 common criticisms. *Psychological Review*, 103(1), 143–164.

discussion (Chiesa & Hobbs, 2008), we argued that ‘Hawthorne effect’ does not help to increase our understanding of social interaction. Similarly, I suggest that Burman would be more likely to clarify the ‘something’ to which he applies the term ‘cognitive revolution’, if he were to study it without the distraction of such an inappropriate label.

Jeremy Burman

Although the burden of supporting the ‘strong claim’ was his, let’s conclude the debate simply by doing as Hobbs suggests.

Points A and B. These are unnecessary. ‘Revolution’ already has a meaning for debates regarding the cognitive revolution, supplied by readings of Thomas Kuhn’s early work. The histories I reviewed earlier argued – supporting Hobbs’ argument – that this particular case does not meet the criteria necessary for a Kuhnian revolution. So what’s the problem?

Point C. In North America, contrary to Hobbs’ assertion, behaviourism was indeed dominant. As George Miller explained in an interview with Bernard Baars: ‘The power, the honors, the authority, the textbooks, the money, everything in psychology was owned by the behaviorist school’ (Baars, 1986, p.203). That there were other approaches that existed alongside the behaviourist is relevant only if one takes an exclusionary Kuhnian (paradigmatic) perspective. The desire to preserve the problematic term – ‘cognitive revolution’ – is therefore not a reflection of behaviourism’s provisioning psychology with a scientific paradigm, followed by its replacement, but of behaviourism’s lost dominance in the material support of psychology as a profession. (There is a vast, and still-growing, literature on what it means to be

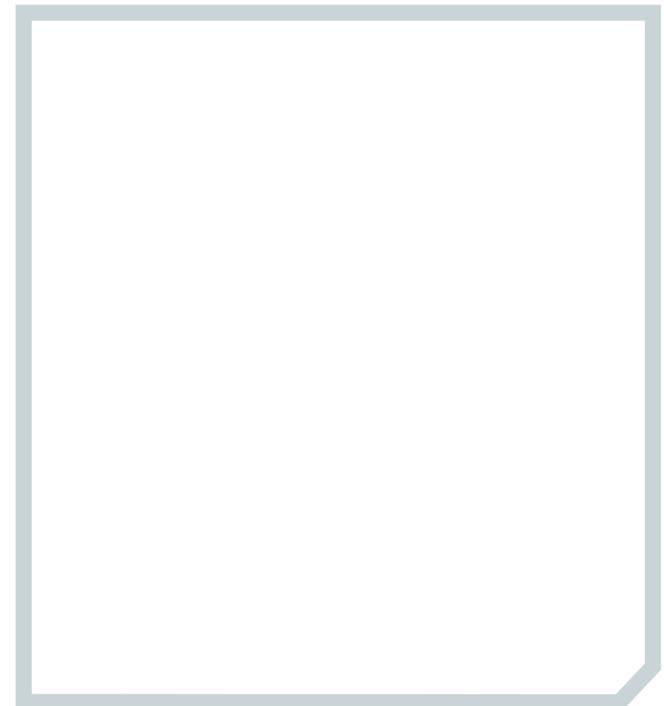
a ‘profession’. Goldstein, 1984, outlined a particularly promising approach.) From the North American perspective, therefore, Hobbs’s proposal that no such shift in resources occurred is contrarian; the resulting history, revisionist. But I accept that this may not be so in Europe (see, for example, Baars, 1986, p.259).

Point D. Under behaviourism, whatever it is that happens between stimulus and response was black boxed. During the ‘revolution’, however, these phenomena came to be associated with ‘the cognitive’ (Green, 1996). Yet the recognition of the importance of explaining behaviour was not unique to North America.

In Europe, for example, Piaget attempted – in collaboration with Daniel Berlyne – to bridge the gap between behaviourism and what became the cognitive approach (Berlyne & Piaget, 1960). Piaget’s group’s later forays into cybernetics, similarly, sought to produce a functional model of learning that would be consistent with both perspectives (Cellérier et al., 1968). Their synthesis was then later re-imported to America, via Seymour Papert’s influence at MIT, and itself became the dominant approach in research on artificial intelligence until the late-1980s (see Boden, 2006, pp.912–916).

Hobbs’s point E must therefore be rephrased and made more specific: the term ‘cognitive revolution’ is indeed inappropriate in describing a global

“it draws attention to a shift...in what it meant to ‘do science’...”



Is ‘cognitive revolution’ an inappropriate label?

phenomenon (cf. Brock, 2006). But such labels can be usefully applied in specific regions where different national psychologies have followed their own developmental pathways. In cases where perspectives from ‘foreign’ national psychologies were imported, issues of translation – not only of language, but also of context and intent (i.e. matters of implication) – must be considered in examining the contents of any resulting change.

Contrary to Hobbs’s basic premise, therefore, the term ‘revolution’ itself can indeed be useful. Insofar as it draws attention to a shift in the dominant meta-theory guiding what it meant to ‘do science’ in a specific place and time, and thus highlights shifts in implication for would-be translators (and interpreters) of the meanings of the results of that science, it can serve as a helpful signpost. Its use also affords opportunities for doing histories of the discipline (e.g. social and institutional histories, analyses of influence, the search for invisible colleges, etc.), rather than just histories of psychological facts discovered in a social and political vacuum.

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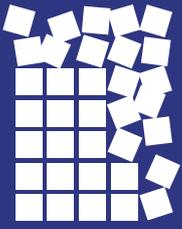
Lovie, A.D. (1983). Attention and behaviourism – Fact and fiction. *British Journal of Psychology*, 74, 301–310.

Mandler, G. (2002). Origins of the cognitive revolution. *Journal of the History of the Behavioral Sciences*, 38(4), 339–353.

O’Donohue, W., Ferguson, K.E. & Naugle, A.E. (2003). The structure of the cognitive revolution: An examination from the philosophy of science. *Behavior Analyst*, 26(1), 85–110.

Samelson, F. (1986). History, origin myth, and ideology: The ‘discovery’ of social psychology by Auguste Comte. In K.S. Larsen (Ed.) *Dialectics and ideology in psychology* (pp.14–29). Norwood, NJ: Ablex.

Shermer, M. & Grobman, A. (2000). *Denying history*. Berkeley, CA: University of California Press.



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