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### ON B.F. SKINNER — WHO, HAD HIS THEORY BEEN TRUE, WOULDN'T HAVE BEEN B.F. SKINNER<sup>1</sup>

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Psychology began as the study of consciousness. William James, in his *Principles of Psychology* (James, 1890/1980, p. 1), defined psychology as 'the science of mental life' — by which he meant *conscious* mental life. He made this proviso clear two years later in his *Briefer Course* (James, 1892/1980, p. 1), where he followed Ladd's (1887) definition of psychology as 'the description and explanation of states of consciousness as such'. But psychology was a science of mental life, and of consciousness, even before James arrived on the scene. Fechner's (1860/1966) psychophysics was primarily concerned with tracing the relations between the physical properties of environmental stimuli and the psychological properties of the conscious experiences to which these stimuli gave rise. Wundt (1874) defined psychology as 'the science of experience as dependent on the experiencing individual', as distinct from physics, which he defined as 'the science of experience as independent of the individual', and promoted introspection as its fundamental method. Boring summarized the achievements of structuralism, psychology's first 'school', in a volume entitled *The Physical Dimensions of Consciousness* (Boring, 1933).

All this began to change with Watson (1913; 1919), who argued that there was a fundamental contradiction between the focus of psychology on private experience and its claim to be a science. For Watson, the only way for psychology to become truly scientific was to abandon the mental, and to redefine psychology as a science of behaviour. By the time that the behaviourist revolution was consolidated by B.F. Skinner (1938), psychology — whether in the study of psychophysics or the analysis of animal learning — had been largely reduced to tracing the functional relations between environmental stimuli and organismal responses. Some interest in consciousness persisted in McDougall's (1923) *hormic* psychology, which argued that mental life began with a thought, continued with an intention, and ended with a feeling; among the Gestalt psychologists;

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and in Woodworth's (1938) work on the span of apprehension. Tolman's (1932) cognitive learning theory and Hull's (1943) drive-reduction theory were also mentalistic (as opposed to behaviouristic) in nature, in their respective emphasis on the role of expectations and drives in learning and behaviour, but it cannot be said that consciousness played any serious role in either approach.

Consciousness was revived as a legitimate subject for scientific inquiry only with the cognitive revolution (Baars, 1986; Gardner, 1985; but see Leahey, 1992). But even then it only crept in through the back door, implied in studies of selective attention (Broadbent, 1958; Cherry, 1953), short-term memory (Atkinson & Shiffrin, 1968; Waugh & Norman, 1965), mental imagery (Holt, 1964; Paivio, 1971; Richardson, 1969; Sheehan & Antrobus, 1972), the role of motivation, expectation, and decision-making in sensory function (Green & Swets, 1966), and even the role of surprise and expectancy in conditioning animal learning (Kamin, 1969; Rescorla, 1967; Seligman *et al.*, 1971). Moreover, equally important roles in the cognitive revolution were played by theoretical advances in which consciousness was irrelevant, such as Chomskian linguistics (Chomsky, 1957) and computer simulations of problem-solving (Newell *et al.*, 1958; Simon, 1957). Owen Flanagan (1992) has written persuasively of the positivistic reserve, piecemeal approach, conscious inessentialism, and epiphenomenalist suspicion that even today prevent cognitive psychologists and other cognitive scientists from approaching consciousness seriously. Thus, the cognitive revolution hardly made the legitimization of consciousness inevitable. As late as 1995, a world-famous cognitive psychologist proudly informed me at a cocktail party that he had written several books on cognition without once using the word 'consciousness'.

Chroniclers of both the cognitive revolution within psychology and the consciousness revolution that accompanied it often employ B.F. Skinner as a sort of negative touchstone, or talisman-in-reverse. After all, Skinner was the person who consolidated Watson's radical behaviourism, and thus ruled cognition, and consciousness, beyond the pale of scientific psychology, and he personified the radical behaviourism that held a hegemonic position within certain circles of academic psychology. It was his view of the organism as an empty 'black box', whose goal was simply to correlate environmental stimuli and behavioural responses, that made it unnecessary, not to mention foolhardy, for anyone to ask what was going on inside. His view that what was important was what people (and other organisms) *did*, as opposed to what they thought or felt, led psychology to 'lose its mind', if not its soul as well. Skinner was the 'evil demon' who had to be exorcised before psychology could be made well. Still under the sway of Freudian psychodynamics, we believe that in order to prevent ourselves from making the same mistakes again, we have to understand why Skinner came to the views he did. And, in the meantime, we pity someone who lives a dehumanized life in which experience was beyond the pale.

In his psychohistorical essay on Skinner, Baars makes much of the paradoxes of behaviourism, the torments of Skinner's 'Dark Year' of 1924, invokes the rhetoric of trauma and dissociation, and asks 'what happened when he crossed

the threshold of Memorial Hall?', and 'Was Skinner open to consciousness after all?' The answer to the first question is easy: on one occasion, he was humming Mozart; on another, he was earnestly engaged in a discussion of an orchestral concert he and his companion had both attended the previous night. And the answer to the second one is equally easy: of course Skinner was open to consciousness. He just didn't think we could have a science of it, or that consciousness played any causal role in human activity; or if it did, that the only way we could do our work as scientists is to behave *as if* it didn't. In this respect, I think, Skinner is no different from those highly respected philosophers of mind, all of whom identify themselves as cognitive psychologists or cognitive scientists, who believe that the vocabulary of neuroscience should substitute for the vocabulary of folk psychology (Churchland & Churchland, 1998); that conscious mental states are after-the-fact rationalizations (Nisbett & Wilson, 1977); that the concept of automaticity allows psychologists to become scientific at long last because their theories are now deterministic (Bargh & Ferguson, 2000); that free will is an illusion (Wegner & Wheatley, 1999), or that behaviour matters more than consciousness (Dennett, 1991).

There was probably no more contradiction between Skinner's private life and his professional activity as there is for any other academic: physicists cook hamburgers without thinking of the laws of thermodynamics, biologists clean their bathrooms without thinking of the human genome, political scientists watch *Survivor* without thinking of the vicissitudes of exit polling, and literary theorists write letters to their mothers without thinking of the interfamily dynamics in Faulkner's Yoknapatawpha novels. To say that people do things in this way is not to say that they are excessively compartmentalized or even clinically dissociated, as Baars suggests: it is simply to say that people go to work, do their jobs, come home, and do something else. People might bring some of their work home with them, just as they might skip out to get the dry cleaning between 9 and 5, but that doesn't change the basic point that there is no necessary connection between how people live their lives and what they do for a living.

Was Skinner, as Baars claims, the most famous scientist in America? Maybe he was the most famous *psychologist*, especially after the 'Baby in a Box' episode of 1945, but let's remember that Albert Einstein took up residence at Princeton's Institute for Advanced Study in 1934 — four years before publication of *The Behavior of Organisms*, (*TBO*; Skinner, 1938). *TBO* only sold a respectable number of copies after it was listed as a required text in psychology courses taught by Fred Keller, Skinner's close friend at Columbia (Hilgard, 1987). *Science and Human Behavior* (Skinner, 1953), which seems to have been Skinner's attempt to infiltrate the market for introductory psychology texts (despite the fact that it contained no reference list), languished in the shadow of competing texts (Hilgard, 1953; Ruch, 1953). Skinner held a prestigious position at Harvard after 1948, but he had an institutional rival in Tolman at Berkeley, and perhaps Hilgard at Stanford as well; and one can argue that Hull's position at Yale, where he played a central role in the interdisciplinary Institute of Human Relations, made him at least equally influential. Think, for example, of the Hull-inspired work by

Miller, Dollard, and their colleagues (Dollard *et al.*, 1939; Dollard & Miller, 1950; Miller & Dollard, 1941), and the enormous influence wielded by Hullian learning theory in the hands of Kenneth W. Spence (1956) and Janet Taylor Spence (Taylor, 1951; 1953). Baars notes the popularity (or was it just notoriety?) of Watson's and Skinner's views on child-rearing, but surely Arnold Gesell (1928) and Benjamin Spock (1946) were more influential.

Skinner's (1935) fundamental distinction between two forms of learning, Type S and Type R, did not take hold; instead, the field embraced the labels of classical and instrumental conditioning, which came from Hilgard and Marquis (Hilgard & Marquis, 1940). *TBO* gave us the notion of schedules of reinforcement, but for the remainder of his life Skinner's most salient empirical contribution was as junior author of a pioneering exercise in mathematical psychology — and the subject of that paper was even a mental state, anxiety (Estes & Skinner, 1945). The Law of Effect celebrated by *TBO* was formulated decades earlier by Thorndike (1898). It was left to others to elucidate its details (de Villiers, 1977; Herrnstein, 1970) and to relate the matching law to the economics of choice (Rachlin & Green, 1972). Skinner's emphasis on stimulus, response, and reinforcement, already undercut by the work of Tolman and his students (e.g., Tolman & Gleitman, 1949; Tolman & Honzik, 1930), was further compromised by Harlow's (1949; 1953) studies of learning set and intrinsic motivation, long before the cognitive revolution was a gleam in anyone's eye. Perhaps the final blow was the discovery of autoshaping (Brown & Jenkins, 1968; Williams & Williams, 1969) — a biological, but not a cognitive, constraint on learning.

Whereas most specialty societies and journals are founded as their topics are struggling for wider acceptance, the Society for the Experimental Analysis of Behavior, and its house organ the *Journal of the Experimental Analysis of Behavior*, were founded in 1958, long after *TBO* and well before the cognitive revolution took hold, at what should have been the height of radical behaviourism's powers. Why then? I think the answer is that while Skinner's *Verbal Behavior* (Skinner, 1957) was a useful target for Chomsky (1959), radical behaviourism had already spent itself, and psychology was eager to leave its sterile confines and ready to take up again the study of mental life. The methods of the new cognitive psychology would be behavioural, but the subject matter would be mental. Psychology would change course, away from the science of behaviour and back toward a science of mental life.

Behaviourism may have dominated American psychology at mid-century, but it was not the behaviourism of B.F. Skinner. Rather, it was a neobehaviourism, which accepted operationism and positivism, but postulated internal mental and physiological states as intervening between stimulus and response. This dynamic S-O-R psychology (Woodworth, 1921/1926), with the 'O' standing for the behaving organism itself, had its roots deeply planted in the very Chicago functionalism (Angell, 1907) that Watson explicitly, and Skinner implicitly, rejected — the functionalism of mind in body, mind in context, and mind in action.

In trying to understand why Skinner rejected consciousness as an important topic for psychology, and in exploring the consequences of this rejection for

Skinner's own life, Baars risks committing the 'psychologist's fallacy' (Dewey, 1894; James, 1890/1980) — the twin mistake of assuming that every event has a psychological explanation, and that the psychologist's explanation of an actor's behaviour is always the best one. Every adolescent struggles for self-definition, every youth has dreams, every adult experiences doubt and disappointment — in himself as in others. One doesn't have to invoke the quest for freedom and dignity to understand why an ambitious young writer-in-waiting might want to get out of Scranton — though it also has to be said that growing up in Reading, another industrial town in the Pennsylvania provinces, apparently didn't do John Updike any harm. One doesn't have to invoke the Oedipus conflict to understand why an unemployed father might feel guilty about his inability to support his son's hoped-for year as an American in Paris; why an adult child might feel guilty about living under his parents' roof in strained economic circumstances; or why his family and friends would be relieved when a college graduate finally became gainfully employed — if only as a graduate student.

And we don't have to produce any deep psychological explanation for what was, for Skinner as for Watson, a purely pragmatic decision to purge consciousness from psychology. They just didn't think that a science could be made of it. Everything else was, arguably, rhetorical flourish — an exercise in public relations, perhaps, but not necessarily reflecting deep belief. Granted, Skinner took the extra step of thinking that psychology *shouldn't* make a science of consciousness, because in his view consciousness was irrelevant to behaviour. But in the final analysis, we don't have to lay responsibility on Skinner's shoulders for the purging of consciousness from psychology, and indeed from scientific discourse. He's gone from the scene, rest his soul. But some 50 years after the cognitive revolution, we have another set of psychologists and cognitive scientists trying to do the same thing — and without any help from Skinner, thank you very much. Rather than speculate on what motivated Skinner, perhaps we ought to spend our time figuring out what motivates *them*.

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## ON BAARS' PSYCHOLOGIZATION OF SKINNERISM

*Tibor R. Machan*

... the psychologists cannot present a picture of man which patently contradicts his behavior in presenting that picture — *D. Banister*

By 'psychologization' I have in mind giving an account of a position in terms of the psychological conditions of the agent who has proposed it. This could involve arguing that someone is a Republican because of fear of poverty or hatred of the poor, or a Democrat because of envy or resentment of the rich. One implication of psychologizing is that the person's views are accounted for not by reference to a process of intellectual, rational thought — which, of course, may involve factual or logical errors — but by reference to this psychological condition. Of course, it is an open question whether the person has power over the psychological condition involved, or at least may have had power over whether to acquire this condition. So, one may argue that a Republican holds his or her political or public policy views because of hatred for the poor and leave open the issue