Peer Commentary

A CONSCIOUS BEHAVIOURIST AND HIS CONTEXT

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B.F. Skinner's curious intellectual life — as B.J. Baars reminds us in his stimulating article — was initially marked by his infelicitous literary adventure, his subsequent drastic turn from a stream-of-consciousness perspective of maximal introspection to an apparently anti-literary stance of scientific objectivism excluding all introspection, and the radical expulsion of consciousness from the scope of scientific, i. e. experimental, psychology, a move that profoundly influenced twentieth-century research. This methodological stance may express Skinner's mental disposition for dissociation, as Baars suggests, based on a humanly common capacity to direct awareness away from specific contents and thus to have and maintain conflicting beliefs about the same referents. So on the one hand, Skinner thinks, consciousness simply does not exist, it is just a word, there is no such thing in this world, and reports about conscious experiences and mental, inner events of all sorts are just sloppy and unscientific descriptions of empirically observable forms of behaviour (but this we must forget when communicating with each other). On the other hand, consciousness indeed exists, and it gives us mental experiences of feelings, thoughts, dreams, art and poetry (but we should forget this when doing scientific research on the human psyche). Dissociation in this sense is surely a common phenomenon, and I think it is crucial to the understanding of beliefs in general, including religious and political convictions.

However, Skinner's enormous influence could evidently not be based on the same dissociation throughtout the scientific community, and especially not for the same personal reasons, perhaps centered in a traumatic rejection of literature. In order to understand how almost a century could be dedicated to (and in a sense, wasted on) anti-consciousness prejudices and provocative anti-phenomenological epistemologies — in particular, projects foregrounding either the behaviourist anti-consciousness paradigm or the psychoanalytic anti-consciousness paradigm: two versions of an equally black-box based model of the mind — we might need to take a brief look at the philosophical context.

Pragmatism has been a strong trend in Anglo-Saxon philosophy since the days of Peirce, James, Dewey, Mead, Adams. Currently, we find its style either in the form of an analytic pragmatism (C.I. Lewis) or shaped as a hermeneutic pragmatism (R. Rorty), and its echoes can even been heard in continental creations such as existentialism and deconstructionism. Its core, as I see it, is a view of human

understanding, namely of signs and their meaning, and only secondarily a conception of the mind as such. It essentially claims that the *meaning* of expressions or 'inputs' in general is practical, and consists in the practice, or behaviour, that follows from our understanding of them, rather than in the reportable inner images that form their content. It is not what we experience as the content of some expression, but rather what acts it causes us to undertake, i. e. the acts that are actualized as a consequence of our understanding of it *as being true*, that define and constitute the meaning of that expression. So semantics is interpreted in an anti-romantic framework: it is unimportant what appears to go on in our 'heart' or other pseudo-organs of our 'inner life' as we understand something; what is important is only how this something makes us act. There are many variants of this basic idea, but they all share the same underlying principle, which I have found (cf. Brandt [forthcoming]) to survive even in some prominent contemporary versions of cognitive semantics (including mental space theory), namely that *the meaning of something is the way in which it is 'true'*. From post-romantic philosophy to post-modern computer science, i. e. throughout modernism as a philosophical epistemè, as Foucault said, this idea stays stable as a dogma. According to that principle, the meaning of X is *not the concept* Y that the expression X makes people *intentionally* form, hold, and even share, in their consciousness, but instead the behaviour Z that follows X, and that X *causes*. Modernism doubts that such an intentional concept Y could be produced and shared by people at all; shared attention may exist in gesture, but its content is to be regarded as undecidable. Do we experience the same content when we communicate? Modern techno-nominalism tends to think that meaningful shared experience is impossible, since meaning is a relation holding only between external sign expressions, 'nouns', or 'symbols', and acts caused by them. This is sufficient for robotics and therefore also for 'cognitics', says the modern techno-symbolist. If humans feel, it's their problem, and it's just folk theory, he would add.

Now, according to an alternative view, which is grounded in structural semantics and, I claim, is important to consciousness studies, *understanding* the meaning of an expression, or of an event that the 'experiencer' personally participates in or witnesses, does not simply consist in drawing behavioural consequences or inferences from the truth of a set of propositions referring to it, but instead implies the mental event of representing to oneself what is signified or going on, in terms of which object categories are manifested, what sort of story is told or shown, what the characters of that story intend, believe, and feel, how the framing situation can be schematized causally, what other situations it can therefore be compared to, and what values it may exemplify.

This cognitive and phenomenological conception of semantics is based on principles of integration, categorization, schematization, and similar factors of immanent mental constructivity. There is, in this view, a *dynamic construction of meaning* immanently going on in our minds, whether we are watching the news, writing poetry, arguing about taxes in parliament, or declaring our feelings to our beloved. The condition for exploring its correlates in the neural processes of the

brain is to allow ourselves to report and model the things that happen *in* our mind. This enterprise is difficult and challenging, as literary text analysis demonstrates; but it is far from being impossible, as aesthetic criticism in general also demonstrates. The enterprise is objective in so far as what we mentally experience is *not arbitrary*: the mind is not a blank slate (cf. Stephen Pinker's recent discussion). There is a mental reality of dynamic constructivity, it has its own laws and regularities, and our familiarity with it allows us to distinguish between good and bad descriptions. It can be investigated. Consciousness is thus not, as Sartre claimed, pure 'nothingness' (*L'être et le néant*).

Where are then the clear philosophical alternatives to pragmatism and behaviourism that contemporary studies of consciousness need so dramatically? They are still unwritten. Surprisingly enough, cognitive semantics never really abandoned the framework of a truth-conditional style of thinking, even if that framework does not support its constitutive intellectual enterprise. The only gesture of escape appears to be the frequent but timid and vague appeal to 'embodiment'. However, embodiment remains precisely a behavioural notion, it is even so *par excellence*, in so far as it does not systematically entail, as Mark Johnson says, the body *in the mind* — not only as revealed by externally expressed metaphors, but also directly by imaginary body events, and as a phenomenology of the bodily imaginary would represent it.

So here is an issue to work on for all of us in the field of consciousness studies: a programme of de-problematization of 'consciousness' by finally bringing semantics out of the grip of truth-conditional philosophy.

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EXPLAINING THE ABSENCE OF CONSCIOUSNESS IN SKINNER'S PSYCHOLOGY

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Drawing on the archive left by B.F. Skinner, Baars has raised some fundamental issues about the conflict between personal and professional ambitions that led Skinner to advance a behavioural theory that expunged consciousness and denied the relevance of our conventional ideas about human freedom and dignity. Baars contends that Skinner's theory dispensed with the notion of individual responsibility and replaced it with one based on the assumption of human perfectibility through operant conditioning. Baars argues, however, that Skinner never satisfactorily resolved the tension between his early aspiration to become a novelist and use the 'stream of consciousness' literary technique to portray the essential human condition, and his wayward ambition to formulate a radical environmental theory of behaviour that minimized consciousness.

Consequently, the notions of mind and consciousness were expunged for most of the remainder of the twentieth century, even though Skinner acknowledged privately that consciousness was essential to understanding the human condition. Baars' analysis helps contemporary neuroscientists and cognitive psychologists to better understand why Skinner's views dramatically but adversely affected the fortunes of mind and consciousness as legitimate objects of scientific study and philosophical analysis.

Baars' primary argument is that Skinner was a victim of denial and self-deceit. He singles out interesting evidence from Skinner's autobiography that his uncertainty about whether to pursue a career as a novelist was intertwined with his unresolved Freudian conflicts with his father. Of course, one could argue alternatively, that Skinner was not confident enough in his writing skills to believe that he could succeed as a novelist and that practicality (and his father's advice) prevailed. There are also alternative explanations that Baars entertains, such as the 'dissociation' theory described below, to explain this apparent discrepancy between public and private selves. He is correct to point out that many other noted behaviourists found psychoanalysis compelling. But I question whether Baars' strategy is appropriate to employ psychoanalytic concepts to understand the inconsistencies between Skinner's public and private persona. This strategy would appear to undermine the possibility of explaining Skinner's personal and professional behaviour in terms of conscious motives and intentions.

One reason for my hesitation on this point is that psychoanalysis ironically furnished Skinner with a vocabulary to express his feelings about his research and career that would not call into question his sincerity or require him to explain any discrepancies between his personal and professional motives or intentions. That Skinner left a personal record that documented his conflicting feelings about his contrasting career alternatives would seem to argue against 'the possibility' Baars raises, 'that Skinner lived an elaborate pretence, seeking publicity for an ideology he knew would baffle and outrage the American bourgoisie' (p. 18 above) in order to arouse publicity. Significantly behaviourists, such as Clark Hull, were attracted to Freudian concepts, such as instincts, drives and the unconscious because these terms enabled them to explain human conduct without drawing on terms intrinsic to consciousness, such as intention, belief, will, etc. Psychologists adopted many of these terms even though they lacked good scientific support because they provided seemingly persuasive generalizations about the psychological and emotional determinants of human behaviour. Viewed in this context, Skinner's self-analysis was consistent with his public position that consciousness was not essential to an explanation of human conduct.

Baars also contends that Skinner was a radical behaviourist. However, Skinner claimed that his theory and experimental method were not based on the stimulus–response paradigm and technique of conditioning. Instead, he argued that his method of operant conditioning preserved the essential voluntary nature of human behaviour and that his notion of 'contingency reinforcement' was a pattern of habitual behaviour that grew out of the choices and behaviours initiated

by the individual. He also claimed that his theory was Darwinian and selectionist through and through. Nevertheless, I think Baars criticism is justified. Skinner failed to convincingly demonstrate that his method avoided the pitfalls of behaviourist theory. The notion is simply incoherent that humans are capable of choice but incapable of deciphering the intentional basis of their actions. Nor did Skinner make a plausible case why mind and consciousness cannot be also considered products of Darwinian behavioural evolution. Mind and consciousness are contingencies of nature whose emergence may not be fully understood but whose function in choice and deliberation would seem to be undeniable.

Baars also argues, more plausibly in my opinion, that Skinner, like many other psychologists and philosophers of that era (1950s to the early 1970s), sought an operationally neutral terminology with which to study human behaviour. This tactic unfortunately eliminated consciousness and many other terms that were indicative of human mind, thought and experience. This contributed to intellectual 'dissociations' or divisions of conscious experience, whereby many aspects of human experience, such as beliefs, memories, intentions and goals were designated subjective phenomena that were not open to scientific investigation. Perhaps, more importantly then, we need to understand why Skinner and other leading scientists who shared his views, were able to persuade so many psychologists and other social scientists to accept the narrow boundaries that were drawn regarding phenomena considered amenable to scientific inquiry.

I find Baars' analysis more plausible with regard to the positivist inclination to arbitrarily restrict the terms of inquiry than in the assertion that this was indicative of denial or self-delusion. I think that it was this semantic strategy of exclusion that enabled behaviourists and the logical positivists to justify taking an extremely limited perspective about what constitutes evidence for objective human thought and behaviour. The more phenomena they could rule out as subjective, the easier it was for them to explain thought and behaviour deterministically. Again, Freud encouraged this attitude by discrediting or discounting much of human behaviour as an exercise in self-deception.

There is a lot to be said in favour of Baars' claim that Skinner was a consummate self-promoter. He took an extreme position on the continuum of alternative theoretical perspectives about human behaviour. This drive for public acclaim was probably encouraged by Freud, whose impact on American thought and culture was so enormous that rival theorists had to work that much harder to get noticed. The attempt to stake out a position that is distinctive is not unusual among those who attain eminence in their fields. In his recent quantitative historical analysis of eminent psychologists, Dean Simonton (2002) contends that many psychologists and other scientists who have attained prominence have taken extreme positions that attract attention and stir controversy. This raises again the important question why Skinner's position became the dominant one and why so few scientists were inclined to counter his theory initially with alternative positions near the centre rather than at the extreme ends of the continuum. To answer this question would require an analysis of the role of intellectual movements in professionalization and global intellectual change that has been

attempted by Randall Collins (1998). He contends that intellectual 'attention space' is limited to a few rival theories that are advanced and expounded by dominant thinkers who attract adherents who defend a theory's conceptual boundaries from attack by those who seek to understand the underlying phenomena differently. Behaviourism's collapse then, was due more to abandonment by its adherents, who could no longer successfully sustain its key premises in the face of theoretical or empirical scrutiny, than to the discrediting of its founder.

Finally, Baars cogently reminds us of the uncanny way that provocative psychological theories are used to promote religious or political ideologies whose premises are contestable. Skinner became well known for his outspoken predictions of attaining a utopian society 'beyond freedom and dignity' that reflected his naïveté about the perfectibility of the human species and his own craving for public and professional recognition. Skinner was seeking a secular equivalent to Calvinism in which individual destiny is predetermined by a carefully crafted and bounded set of cultural contingencies, hence his injunction to go beyond freedom and dignity. Perhaps we should be grateful for Skinner's candidness about the political implications of his psychological theory, because this encouraged social scientists to critically examine and challenge his views and assumptions about human behaviour. But it also should strengthen our resolve as scientists to be leery of theorists who propound simplistic theories about human conduct that avoid addressing the fundamentally significant questions about mind and consciousness and their role in advancing freedom and dignity in a democratic polity.

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LOOK OUT FOR THE DIRTY BABY

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Back and forth swings the pendulum. It is remarkable that Baars can claim that 'many scientists now feel that radical behaviourists tossed out the baby with the bathwater' while not being able to see that his own efforts threaten to be an instance of the complementary overshooting — what we might call *covering a nice clean baby with dualistic dirt*. Yes indeed, *radical* behaviourism of Skinner's variety fell from grace some years ago, with the so-called cognitive revolution, to be replaced by a sort of *cognitivistic* behaviourism that has plenty of room for inner processes, for talking to yourself, for mental imagery, for hunches, feelings, pains, dreams, beliefs and hopes and expectations, but only so long as these are understood to be physical ('informational' or 'computational') processes that could be accomplished by the machinery of the brain. It is an interesting speculative question whether William James would have been a

wholehearted cognitivist, or whether he would have insisted that what *he* meant by the stream of consciousness had to be sharply distinguished from the streams of mere information-manipulation discernible in the activities of cortical subsystems, etc., etc. Making a home for consciousness in the brain, for a distinction between unconscious information-transformations and conscious ones, for instance, is now the work of many hands in many fields (see, e.g., Dennett, 2001, and the other essays in the special issue of *Cognition* devoted to the cognitive neuroscience of consciousness). The main methodological principle of this research is one shared with the radical behaviourists: only intersubjectively accessible data are to be admitted in this natural science of consciousness. If that allegiance, *by itself*, counts as 'behaviourism', then we should all be behaviourists, and indeed the very researchers Baars cites (Singer, Ericsson and Simon, Hilgard, Crick, and Edelman) scrupulously and unapologetically are behaviourists in this minimal sense. They *interview* their subjects, under controlled conditions, and take their reports seriously — but not as infallible guides to their subjects' subjectivity. They practice heterophenomenology, to use my awkward but precise term (Dennett, 1982; 1991; 2001) for this third-person way of taking the first-person point of view as seriously as science can — or should — take it.

As Baars notes, Skinner himself was too smart and self-observant not to be ambivalent about his own too-radical behaviourism, but he was not quite adroit enough to see his way clear to cognitivism. He knew that 'the skin is not that important as a boundary,' as he often put it, and he tried his hand at various formulations that would have permitted him to speak in good behaviouristic conscience about covert, genuinely internal episodes of behaviour that were kosher topics for science because they could be at least indirectly observed. Unfortunately, he was misled away from the main chances in this endeavour by his quite appropriate rejection of the unabashedly Cartesian formulations of his most virulent critic, Noam Chomsky, and those of his followers who vied to be the chief ideologues of cognitive science. If *that* was what cognitivism licensed, then it was better to be a radical behaviourist after all! The irony is that if Chomsky and others hadn't overstated the flight from radical behaviourism, Skinner himself might have been able to join the cognitive revolution, because he recognized, as Baars shows, the central importance for a science of psychology of making sense of the Jamesian stream of consciousness.

When Baars says that philosophers Georges Rey and Kathleen Wilkes are 'fighting the good fight after all these years' — as if they were trying to return us to Skinnerian behaviourism — he is overlooking a more charitable, and more plausible reading. Neither Rey nor Wilkes is a radical behaviourist of the Skinnerian or any other sort, so what on earth can they be doing? They are pushing the pendulum from the opposite side from Baars, to be sure, but they are trying to hold it in the sane middle ground. They are attempting to stop the pendulum from once again swinging too far in the Cartesian direction. Their denials of consciousness look daft only if you forget what some people think consciousness is! Is consciousness real? Of course it is — *as long as you don't understand it as magic* , but for some people, consciousness is magic or it is

nothing at all. For them, Rey and Wilkes provide a worthy antidote: science isn't going to find room for that kind of consciousness, in spite of first appearances. Are there really such people? Yes, many. Speaking for them, for instance, is Robert Wright:

> Of course the problem here is with the claim that consciousness is 'identical' to physical brain states. The more Dennett *et al*. try to explain to me what they mean by this, the more convinced I become that what they really mean is that consciousness doesn't exist (Wright, 2000, fn. 14, ch. 21).

In his fascinating book on the history of Indian street magic, *Net of Magic*: *Wonders and Deceptions in India*, Lee Siegel writes:

> 'I'm writing a book on magic,' I explain, and I'm asked, 'Real magic?' By *real magic* people mean miracles, thaumaturgical acts, and supernatural powers. 'No,' I answer: 'Conjuring tricks, not real magic.' *Real magic*, in other words, refers to the magic that is not real, while the magic that is real, that can actually be done, is *not real magic* (Siegel 1991, p. 425).

Those who think that the demise of 'behaviourism' is either the triumph of mystery over science, or else the rebirth of a full-fledged 'first-person science of consciousness' are mistaken. If Baars is calling for a return to a dualistic vision of consciousness, he is seriously misreading the lessons of recent scientific work on the phenomena, and if he is asking for a natural science of consciousness (or whatever we call the stage magic that happens in the brain), he is on the same team as Rey and Wilkes (and me). Baars asks rhetorically 'What would a science of human beings be like if it had no place for love and hate? If it blotted out pain and pleasure?' Skinner's response to this challenge was flawed, but on the right track: step one is to block the move that insists that *real* pain and pleasure, *real* love and hate, are phenomena that lie outside the scope of the natural sciences altogether. To some folks, this step smacks of behaviourism. So it does, and is the better for it.

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STRATEGIES FOR PUTTING CONSCIOUSNESS IN ITS PLACE

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Abstract: *In order to achieve scientific respectability, behaviourists rejected (a) a nonmaterial ontology for consciousness and (b) free will in the sense of*

volitional action by the person that lies outside a deterministic order. On failure to distinguish these metaphysical statements from metatheoretical statements, they rejected theories of interrelations among conscious states, and theories of causal control by conscious intentions — theories which in themselves entail neither a nonmaterial consciousness nor free will in the sense of indeterminism. On these confusions, protective strategies developed: (1) Redefinition of the psychological vocabulary such that it would refer to other than conscious states. (2) Treatment of consciousness as a non-causal emergent. (3) De-emphasis upon consciousness with focus upon grand claims for the action of unconscious processes. (4) Relaxation of methodological constraints so as to accommodate deeply held ideological and metatheoretical commitments. These strategies endure and are common within current and recent cognitive science.

In the target paper for this commentary, Baars makes an unusual and interesting excursion into a social constructivist (Stam, 2001) — actually personological and social — interpretation of ideological movements within the discipline. I see two theses: For a first thesis, Baars draws on the writings of Skinner, especially his autobiography, to make the case that Skinner failed to give consciousness its due, adopting a Watson-inspired behaviourism, as a way of separating himself from his failure to succeed as a stream of consciousness novelist — something that might be called 'guilt by dissociation'. The second thesis holds that 'rejection of consciousness became a core belief for academic psychologists and philosophers of the English speaking world, justifying their claim to standing among the physical sciences' (Abstract). The first thesis is fascinating, but the second is of wider significance — and the one I would examine further, in an effort to bring still more understanding to this remarkable rejection, one that has persisted in various methodological and metatheoretical strategies today. A little social constructivist thought about these matters invites more.

Central to the discipline of psychology in the behaviourist era — and I would say continuing today — have been various metatheoretical and methodological strategies designed to 'put consciousness in its place'. In the eyes of many, those strategies serve to avoid any entailment of two philosophical positions that have been deemed to be fundamentally unacceptable in science: (a) One is an ontological view that consciousness is nonmaterial, not fully reducible to, or explainable by, physical processes. (b) The other is the doctrine of 'free will', in the sense of indeterminism — the view that some conscious volitional choices occur independently of an exhaustive inventory of causal controls for that conscious volition.

Coupled with this has been the common tendency to disregard or blur the distinction between theoretical assertions that attribute causality to conscious states and the two metaphysical assertions of nonmaterialism and indeterminism. On this kind of confusion, attributing casual status to conscious states of the person implies attributing either or both nonmaterial status and indeterminism. Nevertheless, to say theoretically that conscious beliefs and volitions cause other mental states and actions, even the mentalistic metatheory that mental life consists of

a rich causal network of conscious states (Dulany, 1991; 1997; 1999; in press), makes no metaphysical claims whatsoever.

And that's just as well. (a) Nothing in the science as we know it either requires or can selectively support either a materialistic or nonmaterialistic view of consciousness. The 'hard problem' is still hard — and I would think insoluble by known scientific methodology. We learn what accompanies what. (b) The same can be said for determinism or indeterminism. By their nature, causal analyses select and abstract and can never be exhaustive, leaving the ultimate question unanswered — even if what is merely a heuristic assumption of determinism is productive.

What do I see as the strategies directed at maintaining these metaphysical positions and the metatheories with which they are confused? Strategy 1: Avoid reference of psychological terms to conscious states, by specifying other referents for the ordinary psychological vocabulary. Strategy 2: Where reference of psychological terms to conscious states is maintained, hold that these states are noncausal emergents. Strategy 3: De-emphasize the role of consciousness with claims of, and focus on, extensive domains of unconscious mental activity and control. Strategy 4: Let methodological constraints relax in order to accommodate central tenets of deeply entrenched metatheory — as elaborated earlier (Dulany, 2001, p. 6). Disciplines as social institutions, especially their publishing outlets and granting inputs, may operate protectively to characterize inadequate methods as adequate.

J.B. Watson

In his 'Behaviourist Manifesto', Watson (1913/1994), advanced the relatively temperate argument that any attempt to study consciousness presented methodological problems for the classical introspection of Wundt and Titchener and for the many already doing behavioural analysis of animals who couldn't introspect (work extensively reviewed in Watson, 1914). 'One can assume either the presence or the absence of consciousness anywhere in the phylogenetic scale,' he wrote, 'without affecting the problems of behavior by one jot or one tittle; and without influencing in any way the mode of experimental attack upon them. . . . The time seems to have come when psychology must discard all reference to consciousness . . .' (Watson, 1994, p. 249). 'Discarding reference to' is not equivalent to 'denying the existence of', but then he concludes with what eventually leads to the more extreme positions within behaviourism: 'This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier from psychology which exists between it and the other sciences' (p. 253).

After leaving academia for the less temporizing world of advertising, Watson (1924, p. 3) lays out the metaphysical claims more clearly: 'Behaviorism claims that "consciousness" is neither a definite nor usable concept; that it is merely a word for the "soul" of more ancient times. The old psychology is thus dominated by a kind of subtle religious philosophy.' And in the revised edition (Watson,

1930, p. 2), we find, 'The behaviorist holds further . . . that belief in the existence of consciousness goes back to the days of superstition and magic' — a view philosopher Gustave Bergman (1956, p. 26) characterized with that fine old philosophical term, 'silly'. Caught in the historical wake that followed, Heidbreder (1933, p. 236) put it this way (sadly) for generations that would study systematic psychology: 'On the right hand side are behaviorism and science and all its works; on the left are souls and superstition and a mistaken tradition; and the line of demarcation is clear and unmistakable.'

So there we have it: With a failure to distinguish metaphysical claims ('Consciousness as soul is nonmaterial, and thus perhaps immortal') from theoretical claims ('Conscious states function within causal networks'), psychologists if they were to be scientists could have no truck with consciousness.

Indeed other social forces may have converged. With Titchener having studied at Oxford and Leipzig, embracing a view with roots in European intellectual traditions, then establishing a 'colonial outpost' at Cornell, could behaviourism have also been in some ways a 'populist, colonial revolt'? For that matter, Watsonian behaviourism flowered during WWI, a period of strong anti-German sentiment in America, and Wundt was German and his student Titchener 'even looked like a German . . . [and] the science he labored for was German to the core [in a university] on the wrong side of the Atlantic' (Heidbreder, 1933, p. 114). The behaviourism that developed was as American as — well — agent orange, and just the thing for the conceptual defoliation their metaphysical posturing required.

With these social forces operating, we shouldn't be surprised to find Strategy 1, his famous 'peripheralism': Terms of the psychological vocabulary were now to refer, not to conscious states, but to behaviour — to muscular movement, either overt or covert. Ironically, some outsiders (even if not insiders) saw the effort gain intellectual respectability from an Oxbridge philosophical programme of the time that promoted an objective redefinition of the psychological vocabulary, a tie Baars' account also recognizes. 'Sensation', for example, was translated as 'eye movements', 'emotion' as 'response of the smooth musculature' (including the textbook favourite, 'tumescence'); and 'thinking' could be translated as any of a range of behavioural descriptions, especially as 'sub-vocal larynxation'. Indeed, prominent investigators of the time instructed their subjects to think of this that or the other, then scurried around the periphery with electromyographic recorders — evidently forgetting that 'accompanied by' doesn't translate as 'is'. (For a review, see Osgood, 1953, pp. 649–55). This work, I would think, nicely embodied Strategy 4.

B.F. Skinner.

In the first sentence of *About Behaviorism*, Skinner (1974, p. 3) did say, as quoted by Baars, 'Behaviorism is not the science of human behavior; it is the philosophy of that science.' Despite what Koch (1976, p. 454) called this 'bivocality', Skinner's radical behaviourism was both of these things, the

philosophy science and mind most systematically laid out in that work (Skinner, 1974), and the experimental analysis of behaviour and resulting behaviour theory running through Skinner's writings (e.g. 1938; 1969) and decades of studies in the movement's 'in-house' journal, *JEAB*. It was this behaviour theory that was applied in the social philosophy of *Walden Two* (Skinner, 1948; 1971) and its defence, as well as in his remarkably wide range of 'extrapolations' of behaviour theory (Skinner, 1953; 1957; 1974) — from creativity (reinforcement of behavioural 'mutation') and understanding (repetition) through the complexities of language to Hitler's irrational extension of WWII (resistance to extinction). Radical behaviourism was these other two things, too. In fact, running through descriptions of all four we can find the metaphysical position and the defensive strategies they spawned.

In that most systematic work, Skinner (1974) clearly asserts the two metaphysical views: (a) 'The position can be stated as follows: what is felt or introspectively observed is not some nonphysical world of consciousness, mind, or mental life, but the observer's own body' (p. 17). Consciousness is not nonmaterial. Later on (p. 189), he writes that (b) 'A scientific analysis of behavior must, I believe, assume that a person's behavior is controlled by genetic and environmental histories rather than by the person himself as an initiating, creative agent.' This rules out free will in the sense of a form of indeterminism.

Skinner often said that 'thinking is behaving', but the defensive strategies used are a little more complex: 'This does not mean . . . (and this is the heart of the argument) that what are introspectively observed are the causes of behavior. . . . An organism behaves as it does because of its current structure, but most of this is out of the reach of introspection. . . . At the moment we must content ourselves with a person's genetic and environmental histories. . . . What are introspectively observed are certain collateral products of those histories' (Skinner, 1974, p. 17).

On Strategy 1, 'consciousness' and other terms from the psychological vocabulary would then be redefined with reference to bodily states — the Watsonian strategy. One of his last papers (Skinner, 1989) draws on etymology to argue that terms taken as referring to feelings and states of mind were derived from labelling behaviours and external events concurrent with those various body states. On Strategy 2 he asserts that what is introspected, consciousness, is a noncausal emergent, only a 'collateral product' of environmental and genetic histories. On Strategy 3, the immediate controls of action are indeed unconscious bodily states — though so inaccessible we must look to control by environmental and genetic histories, including the immediate stimulus history. On these strategies, then, we needn't bother much with consciousness.

Despite Skinner's frequent rejection of what he called 'the inner man' and 'the inside story' (Strategy 2), he does occasionally (e.g. Skinner, 1987, p. 490) dissociate himself from logical positivists who would restrict the inquiry to the observable stimulus and response world. With the occasional lapse to be expected of any sensible person who adopts an essentially untenable position, he even acknowledges conscious control in one part of his systematic treatise (Skinner, 1974, 219–20): '[Radical behaviorism] was not designed to permit

consciousness to atrophy. What it has to say about consciousness is this: Stimulation arising inside the body plays an important part in behavior. . . . In the sense in which we say that a person is conscious of his surrounding, he is conscious of states or events in his body; he is under their control as stimuli.'

Not content with that lapse, however, he quickly returns to form (p. 221): 'Must we conclude that all those who have speculated about consciousness as a form of self-knowledge — from the Greeks to the British empiricists to the phenomenologists have wasted their time: Perhaps we must.' (For more on Skinner's ambiguous — and ambivalent — treatment of consciousness, see Natsoulas, 1978, pp. 149–51.)

Strategy 4? The fourth strategy, relaxation of methodological constraints, serves the other strategies, in particular Strategy 3, the assertion of control by unconscious processes. On this, and other versions of behaviour theory, the 'automatic action of reinforcers' provides the unconscious mechanism: It is defined as response strengthening in the absence of awareness of the reinforcement contingency or a conscious intention to act on it. The mechanism was also central to Hullian theory, and good behaviourist Tolman had clearly denied that his cognitive 'expectancies' were conscious states. Of course, neither a pigeon nor a Sprague-Dawley 'participant' was equipped for reporting anything to the contrary, and operant procedures were extended to the human level with claims that 'Very little deviation from the approach in animal work has been necessary so far to obtain lawful data in verbal behavior' (Salzinger, 1959, p. 70).

The trouble was the evidence for 'unconscious control' depended on flawed methods: assessments of contingency awareness that were subject to forgetting and social demand, and scoring insensitive to the conscious contents correlated with what the experimenter had too narrowly accepted as 'correct'. (Reviews and experimental critiques appear in e.g. Dulany, 1968; Spielberger & DeNike, 1966).

The continuing strategies in cognitive science

Baars writes that 'Behaviourism has amazing staying power. It is alive today, though less overtly.' But when this seems less overt, is it behaviourism itself that lingers — in the bones like strontium 90 — or is it something deeper and more general, even if not universal? Outside the behaviourist subculture, we don't find behaviour theory or a focus on the study of behaviour. At the 1989 APA meeting that celebrated his career, Skinner — as he has been quoted (Bales, 1990, p. 6) — said that 'So far as I'm concerned, cognitive science is the creationism of psychology'. If he had been looking more closely for continuing strategies, however, he might have liked what he saw and declared common cause instead.

Let's see. There is so much, but to be brief as I must, I will sample, summarize, and point to sources. Strategy 1? A redefinition of the psychological vocabulary? On the new computational view of mind, terms of the psychological vocabulary were to refer to a level of cognition that is apart from consciousness (Haugeland, 1978; Jackendoff, 1987; Velmans, 1991). Gardner's (1987, pp. 383–4) scholarly account said it clearly: 'To my mind, the major accomplishment of cognitive

science has been the clear demonstration of the validity of positing a level of mental representation . . . but this form of representation does not involve processes of which the organism is in any way conscious or aware.'

Within the information processing paradigm, this redefinition of the rich psychological vocabulary has amounted to a rather loose association of the term 'consciousness' with cognitive 'systems' — with a working memory or short term memory system for some (e.g. Newell & Simon, 1972), or with an attentional subsystem (e.g. Cowan, 1993), or with something among 'executive' systems and 'slave' systems (Baddeley, 1986). Indeed, I believe that Baars' (1988; 1997) 'global workspace' model becomes that kind of reassignment of reference for the psychological vocabulary when the functions of consciousness are suggested by the model rather than by a mentalistic metatheory within which the psychological vocabulary is embedded.

Expressing dissatisfaction with the 'explanatory gap', how the brain could produce the experience of seeing, O'Regan & Noë (2001) even write that 'Instead it is proposed that seeing is, essentially, a way of acting' (Abstract, p. 939). Then in (oddly) denying its characterization as behaviourist, they reassert that 'For us, in contrast, skillful activity (consisting of behavior and sensory stimulation) is the experience' (p. 1015) — a view reminiscent of an earlier blurring of the distinction between 'accompanied by' and 'is'.

And what is the force of eliminative materialism in philosophy (e.g. Churchland, 1993; Stich, 1983)? Finessing the fundamental distinction, the position takes arguments for metaphysical materialism as grounds for eliminating a vocabulary and programme of theoretical mentalism, which is misleadingly disparaged as 'folk psychology'. (It is my impression, however, that relatively few folks are acquainted with the use of linear difference equations to describe revision of belief in consciously represented causal hypotheses in response to consciously represented evidence, e.g. Carlson & Dulany, 1988). But on this view, elimination of the metatheory is evidently a way to maintain metaphysical purity.

Strategy 2? Consciousness as an emergent? On that basic computational view of mind, all real mental activity occurs within a cognitive level, a level of phenomena that run in the brain, with consciousness only a nonobligatory, noncausal emergent. With that kind of start, we shouldn't be too surprised to find Wegner (2002) recently holding that conscious will, the conscious intentions we experience, provide only an illusory sense of causal control of action. These statements are offered as themes for chapters 1 and 2: 'It usually seems that we consciously will our actions, but this is an illusion (p. 1). . . . Conscious will arises from processes that are psychologically and anatomically distinct from the processes whereby mind creates action' (p. 29). In social psychology, the counter-intuitive is highly valued. Here, too, we find a blurring of the distinction between metaphysical and theoretical assertions: 'In harmony with "dual process" models in contemporary cognitive science, which equate determined processes with those that are automatic and which require no intervening conscious choice or guidance, as opposed to "controlled" processes which do, the social cognition research on automaticity of higher mental processes provides

compelling evidence for the determinism of those processes' (Bargh & Ferguson, 2000, Abstract, p. 925).

Strategy 3 is very familiar and right in the mainstream: Keep focussed on the claims for vast regions of unconscious mental activity and unconscious control. If, as on the computational view, consciousness is only a noncausal emergent, then everything controlling is unconscious. The fashionable higher order thought theories converge: All mental states are said to be unconscious until graced by a higher order reflection (e.g. Rosenthal, 1987). Standard information processing views, too, from Bower (1975) to Baddeley (1996) leave plenty of room in that overall working memory system, outside of conscious attention, for unconscious mental activities that are unconstrained in form or complexity. In fact, on analogy to their own conscious remembrances, mainstream theorists, from Collins & Quillian (1969) onward, have placed propositional information in rich stores of unconscious enduring memory — despite the conscious and unconscious forms of memory not meeting the same functional specification.

Without elaboration here, we can also be reminded of the basic lists of standard claims: Implicit learning is unconscious learning. Implicit memory is unconscious memory. And automatic control is unconscious control. Furthermore, unconscious perception may occur in normals, distinctively identified with brain imaging, as well as in various brain pathologies (e.g. blindsight, and prosopagnosia).

Even Baars, who has done more than anyone else to revive a study of consciousness, wrote (in 1988, p. 158) that 'The bulk of spontaneous problem solving is unconscious.' On his theatre metaphor (Baars, 1997), he wrote that '. . . conscious contents are limited to a brightly lit spot of attention onstage, while the rest of the stage corresponds to immediate working memory. Behind the scenes are executive processes, including a director, and a great variety of contextual operators that shape conscious experience without themselves becoming conscious. In the audience are a vast array of intelligent unconscious mechanisms.' I believe that 'puts consciousness in its place'. With a focus on wider access and integrative functions for consciousness (Baars, 2002, p. 48), however, the unconscious is beginning to sound a little less majestic: 'Complex unconscious processes do exist in automatic functions and implicit cognition.'

Strategy 4? Has methodology relaxed to accommodate prevailing metatheories? I think it would be widely agreed that the experimental literatures of unconscious processing and control have been subject to an unusual amount of conceptual and methodological criticism, perhaps more than any others — and, I would add, they have been unusually vulnerable.

Unconscious implicit learning? But see e.g. Dulany *et al.* (1984); Dulany (1997); Shanks & St. John (1994); Perruchet & Vinter (in press). Unconscious automaticity? But see e.g. Dulany (1997); Tzelgov (1997). Unconscious perception in normals? But see e.g. Dulany (2001); Holender (1986); Merikle & Reingold (1998). Unconscious perception in prosopagnosia? But see e.g. Dulany (in press); Farah (1994). Unconscious perception in blindsight? But see e.g. Dulany (2001, in press), Perruchet and Vinter (in press). Unconscious problem

solving? But see e.g. Ericsson & Simon (1993), Mandler (1994), etc. As I have suggested before (Dulany, 1999, p. 154), if claims for the power and range of an intelligent unconscious were correct, the effects in these literatures would be too strong, replicable, and methodologically defensible for these literatures even to be controversial.

I would agree with the major thesis of Baars that something that behaviourism begot still lives. He did well to focus on its roots with Skinner and the Watsonian influence. What I believe endures, however, is a set of protective strategies that need to be more widely recognized — critically examined — and now revised or rejected. Could it be that with the ideological commitments that have existed within the discipline, these protective strategies were necessary for a revival of the study of consciousness to get a respectable start? Perhaps. But those strategies have now been challenged and rejected in some analyses (e.g. Carlson, 1997; Dulany, 1991; 1997; 2001; in press; O'Brien & Opie, 1999; Perruchet & Vinter, in press; Tzelgov, 1997). I believe that moving beyond these strategies is now the key to a fuller analysis of the richness of consciousness and its functioning — its variety of modes, the forms of contents they carry, its literal and identity codes — and its perhaps exclusive carriage of symbols of the future and past and present world beyond, as well as of itself and the self as constructed. This would be the vocabulary of a rich inventory of mental episodes, given forms by the mental operations interrelating those modes and contents.

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STRAW PARADOXES
A Commentary on Bernard J. Baars' 'Double Life of B.F. Skinner'

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Having known and worked with B.F. Skinner for the last 15 years of his life, having collected and read everything he ever published as well as a great deal of material he never published, and having edited a quarter century of his private notes into book form (Epstein, 1980), I simply do not recognize the man that Baars has constructed. Lacking concrete evidence that Skinner lived some sort of 'conflicted' or 'double' life, Baars has engaged in wishful thinking of the most extreme sort. He has constructed a stick-figure Skinner, composed largely of limbs borrowed from the corpse of John B. Watson, supplemented by fragile twigs of his own design, and held together by glue every bit as ephemeral as the 'consciousness' he so worships.

Let's cut to the chase. B.F. Skinner was probably the least conflicted person I've ever known — in addition, by the way, to being the happiest, most creative, and most productive. Baars' assertions that Skinner was 'deeply at odds with himself', that he was suffering from significant 'inner conflicts', that he was not 'at peace with himself', that he was involved in never-ending 'struggles', and that he lived an 'elaborate pretence', are patently absurd. Skinner believed in his behaviouristic credo, and he also made it work for himself in every aspect of his life. Consistent with his belief that behaviour was determined by environmental histories, Skinner deliberately and systematically manipulated his own environment to boost his creativity, maximize his productivity, and even improve his mood (Epstein, 1997). There was no trace of conflict in this process. Quite the contrary, it worked like a charm until his dying day (Vargas, 1990). I spent many

intimate moments with Fred Skinner over the years — at restaurants, movie theatres, his home and mine, by his pool, in the laboratory we created together, and so on. If there were even a trace of pretence in Skinner's public persona, I would have seen it. But Skinner was the real deal. Unlike Freud, who was unable to use his own system to spare himself the pain of his own neuroses (Breger, 2000), Skinner benefitted daily from the system of behaviour-analytic techniques he helped to create.

So how did Baars make such a huge mistake? First, he made much of a brief account Skinner gave of a year he spent after college trying to become a creative writer — a year Skinner himself labelled 'Dark'. Baars argued that because Skinner failed to write much that year, he rejected the 'subjective' life in favour of the coldly scientific. That's an overly dramatic and simplistic assessment of what happened. In fact, having spent much of his time building model ships rather than writing, Skinner simply realized, as many hopeful writers do, that he didn't have what it takes to become a professional writer. But more important for our present purposes is Baars' assertion that Skinner somehow abandoned, or at least tried to abandon, his 'subjective' side, which, as Baars uses the term, would seem to encompass all of Skinner's literary and creative interests, as well. In fact, Skinner never abandoned any of the richness of his own experience. He loved romance, fiction, TV sports, baudy limericks, magic, and Stendahl; he also freely engaged in thinking — and even in thinking about thinking — for how could he not? Skinner also loved science. Baars would have us believe that there is some 'conflict' or 'paradox' here, but there simply isn't any. One needs precision, and, yes, even special language, in the behavioural laboratory, but just because someone enters the lab on occasion doesn't mean that he or she needs to give up one jot of his or her subjective life — if, indeed, there is some way to do so! Although he only published one novel, Skinner continued to dabble in creative writing, to read great literature and participate in literature and acting groups, to play the piano and organ, and to express his creativity through invention and composition throughout his life (Epstein, 1991). Even hardcore physicists — Einstein comes to mind — lead rich, imaginative, subjective lives without apparent conflict. Why should Skinner have had a problem? Skinner rejected certain kinds of subjectivity in the construction of psychological theories, but that never led him, or me, or any other behavioural scientist I've ever known, down a path toward personal conflict.

Baars also confuses Skinner's views with Watson's. While it's true that Skinner was inspired to enter psychology by one of Watson's books, Skinner was more leader than disciple, and one of the main ways in which his views diverged from Watson's was in his serious consideration of private experience — what Baars calls 'consciousness'. Beginning with his 1945 essay, 'The Operational Analysis of Psychological Terms,' Skinner (1945a) wrote extensively about private events for nearly half a century. Baars reluctantly admits that Skinner acknowledged the existence of such experience, but he then rejects this important observation in a curious way, arguing that 'only the most careful readers of Skinner's work' were aware of the distinction Skinner drew between private

experience and 'subjectivity'. I'd argue, quite the contrary, that only the most careless readers of Skinner work could fail to recognize Skinner's deep fascination with the very phenomenon that Baars claimed he ignored. Baars seems to hold Skinner responsible for the fact that some of Skinner's critics — including Baars himself — don't seem to read Skinner's writings very carefully, but that hardly seems fair.

Baars has misinterpreted or misrepresented Skinner in many ways, large and small, but I'll mention just two more examples. First, Baars suggests that the debate between Frazier and Burris in Skinner's novel, *Walden Two* (Skinner, 1948) again exemplifies Skinner's various 'struggles'. But the serious debate about freedom and cognition in the novel was between Frazier and a third character, Castle, and no one has ever suggested that Castle's perspective matched Skinner's in any way. Moreover, the debate between Frazier and Burris (who in some sense represented two 'sides' of Skinner) was fully resolved at the end of the novel, with Frazier, the more radical of the two, winning handily. It's notable that *Walden Two* was completed in 1945, the same year that Skinner published 'The Operational Analysis of Psychological Terms'. If Skinner ever had any doubts about his brand of behaviourism — and I know of no evidence that he did — it could be argued that they were fully resolved by 1945.

Second, Baars suggests that the 'aircrib', the enclosed crib Skinner designed for his second child (Skinner, 1945b), was a tool he used to condition his daughter and that she was 'not taken out to be hugged and to play freely'. Skinner's programme of conditioning children, says Baars, 'followed a path famously pioneered by Watson'. This is so far off I barely know what to make of it. Skinner wasn't just a scientist, he was also a tinkerer and inventor, and the aircrib was simply a better crib. Parents who used aircribs — including the Skinners — hugged and played with their children every bit as much as parents who used conventional cribs (Epstein & Bailey, 1995). The crib was never used for conditioning purposes, and, unlike Watson, Skinner never recommended withholding love from children.

So what *was* Skinner's perspective on consciousness? First, as I've already noted, the experience that gives rise to the language of consciousness is quite real; Skinner never claimed otherwise. It can hardly be denied that we think, imagine, feel, and so on. Second, as Baars, a neuroscientist, would no doubt agree, this experience is a physical phenomenon; it's *activity*, mainly of the nervous system; there's no need to invoke the existence of a mental world to understand and explain conscious experience. Third, like many of his contemporaries in various schools of psychology, Skinner believed that introspection was an unreliable method for learning about human functioning. Fourth, Skinner believed that it was senseless to speak of thoughts, images, and so on, as *things.* As modern neuroimaging studies are indeed confirming, when we 'imagine', we are engaging in neural activity similar to that of seeing; we are seeing in the absence of the thing seen. An image is not an object; it's activity, or, as Skinner preferred to say, it's *behaviour.* And finally, Skinner felt strongly that we err when we assert that cognitive or neural activities are the *causes* of overt

behaviour. Activity inside our skin is part of what we need to explain, not explanatory in its own right. The causes of behaviour — all behaviour: neural, cognitive, and overt — lie, according to Skinner, in the phylo- and ontogenic histories of the organism. Looking at a phenomenon at different levels of organization can produce valuable insights, but it doesn't, said Skinner, identify causes; causation moves in only one direction, and true causes lie in the past.

Baars seems exasperated over the fact that Skinner may have been the best-known scientist of the twentieth century. He accounts for this by asserting, rather ungenerously, that Skinner was a manipulative showman, but a more realistic explanation for Skinner's fame was that his work touched so many lives. There's hardly a clinic or business or classroom in the world that hasn't benefited from operant technology.

I agree with Baars on one point: Skinner's nonscientific writings carry considerable philosophical baggage, and the baggage is more harmful than helpful at this point. The unusual circumstances that gave rise to the old turf battles between behaviourists and cognitivists no longer exist (Epstein, 1996). The time has come to put aside the isms and the polemics and to cooperate in advancing the behavioural, cognitive, and neural sciences in all their aspects.

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A SCENT OF SKINNER AT HARVARD

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As an undergraduate at Harvard in the period 1959 to 1963, I heard a good deal about B.F. Skinner. In my circle at least (math majors taking graduate courses, English majors interested in literary criticism, and various disaffected others), one mainly heard ridicule and derision, though I did know one ambitious Texan who saw great potential for mass marketing in Skinner's operant conditioning. Among the better-known figures from this circle are the philosopher Saul Kripke and the techno-futurist George Gilder.

Skinner's lab was in the basement of 'Mem Hall', then a large, aging Victorian structure (though now restored), having somewhat unpleasant associations for undergrads due to its frequent use as a site for final examinations in large courses. Skinner's lab was said to be full of hard-working rats, pigeons, and grad students, and to be pervaded by a certain unmistakable odour. Though I never visited Skinner's lab, I did notice the odour, on a visit to a different lab in the basement of Mem Hall, the more recent, more modest lab of George Miller. Miller surprised me by giving a tour of his lab, and later arranging a summer job for me as a Research Assistant in mathematical psychology at Stanford. He was very charming, and clearly proud of some recent experiments intended to demonstrate the existence of mental states. He was also excited about his recent work with Noam Chomsky on (finite state mathematical) automata for recognizing whether 'sentences' belong to a given 'language' (Chomsky and Miller, 1959). Although I was not then familiar with Chomsky's now famous, devastating 1959 review of Skinner's book *Verbal Behavior* (Skinner, 1957), I do recall thinking with some satisfaction that the handwriting seemed on the wall for Skinner's brand of extreme reductionist behaviourism. In fact, this famous 1959 paper by Chomsky and Miller formalized the main argument of Chomsky's review, the necessity of internal states for parsing formal languages.

For me personally, this meeting with Miller marked the birth of (what later came to be called) cognitive science. However, I was not sure then, and I am even less sure now, that Chomsky's disembodied anti-social theory of language is much better than Skinner's behaviourism, even though Chomsky's theory was quite solid mathematically, and has important applications to computer languages. (Of course, Chomsky has since moved through a long sequence of other theories of language, but they all have a similar formalist character.)

If I were to draw a moral, it would be much like that of Bernie Baars: In an era of huge advances in the physical sciences and mathematics, there was great pressure on the 'human sciences' to be just as rigorous and profound, and this had then, and continues to have now, a pernicious, distorting influence; even then, we called this phenomenon 'physics envy', an ironic echo of Freud's infamous concept. I think it fair to include not only Skinner's positivist behaviourism and Chomsky's 'Cartesian' Platonism, but also the logical reductionist 'Artificial Intelligence' being developed by John McCarthy and Marvin Minsky down Mass Ave at MIT, as well as the 'New Criticism' of then Harvard English Professor Reuben Brower, which excited and enraged humanities scholars of the time with its 'scientific' approach to textual analysis.

Among many more recent developments of a similar kind, I would include much of contemporary neuroscience, the more extreme versions of which I have elsewhere (Goguen, 2003) called 'neuro reductionism', and Edward Wilson's 'consilience', which attempts to reduce the humanities to evolutionary sociobiology. As Baars says, more than a hundred years have been lost by discrediting and then ignoring the rich legacy of William James. It seems to me that much the same can be said for some other great figures also associated with Cambridge, including George Santayana, Warren McCulloch, Norbert Wiener, and

Charles Sanders Peirce. Moreover, there are many more contemporary influences to which we might well pay more attention, such as actor-network theory, activity theory, ethnomethodology, and cognitive linguistics. The dangers of sacrificing our humanity to the gods of science have not diminished, though today the organs of molecular biology and computational evolution may seem larger than those of physics.

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ON B.F. SKINNER — WHO, HAD HIS THEORY BEEN TRUE, WOULDN'T *HAVE BEEN* B.F. SKINNER[1]

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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 primary 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

[1] Preparation of this commentary was supported by funds from the Committee on Research of the University of California, Berkeley. The title is a paraphrase of a toast ostensibly given on the occasion of Skinner's retirement (the story may be apocryphal, but the turn of phrase is irresistible).

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; 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 than there was 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* had been 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 hight 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 of whether such hatred is something the person may have freely cultivated, by thinking sloppily or acutely about poverty or the poor. Usually, however, when one engages in psychologizing, one suggests that the person who has certain views in virtue of a psychological condition is in the grip of that condition, not likely to be easily extricated from it, at least at the time the psychologizing is relevant to understanding what and how he or she forms opinions, develops a position on some subject matter.

Bernard Baars argues that B.F. Skinner suffered from the psychological condition of dissociative disorder (p. 24 above). In support of this diagnosis he adduces the fact, documented throughout his paper, that Skinner both denied and affirmed the existence of consciousness. He denied it, to be sure, explicitly, and affirmed it only implicitly, mainly through his reported reflections on his own life and career. To be sure, Baars shows us that Skinner didn't simply produce a contradictory position on the main topic of concern to Baars, namely, human consciousness. No, Skinner pretty diligently separated those works in which he denies the reality (or accessibility by scientific means) of human consciousness and those where he gives evidence of taking consciousness seriously (mainly his personal recollections of the content of his own thinking). Yet, Baars might have confined himself to noting that the way Skinner thought about consciousness in his professional, academic, scientific or scholarly capacity contradicted how he seemed to regard the issue when he spoke about his life and intellectual development.[2] That, surely, would be a telling point to make — it would identify a contradiction between Skinner's explicit theories and the views he held in terms of which he tried to understand himself, surely one significant instance of someone who may or may not have consciousness. (Of course, the existence of this contradiction would probably presuppose the soundness of certain methodological moves Baars embraces, such as regarding introspection a reliable source of evidence of consciousness [Baars, 1997].)

Arguably, and with Baars' full awareness, Skinner would have maintained that his autobiographical reflections that suggest that he affirmed the existence of consciousness could be rendered into terms that do not imply the existence of any conscious states but merely, as Baars' put is, instances of 'covert behaviour'.

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[2] I am aware that saying what Baars might have done, I am suggesting that Baars, as the rest of human beings not suffering from debilitating disorders, has the capacity to think otherwise from how he actually does think, to reach different conclusions from those he actual does reach. This further suggests that we are free in a way that Skinner denies, along with many others in the psychological and philosophical community. The freedom to think one way, another or not at all, implies a fundamental liberty that has seemed to be presupposed in all evaluations of human actions, including human thought (as when one criticizes a scholar, scientist or ordinary person for not having thought through, properly, some topic or drawn the correct conclusions from a line of analysis or research). For more about this, see Machan (2001).

There is a clear illustration in *Beyond Freedom and Dignity* of the translation programme that Skinner envisions:

> Consider a young man whose world has suddenly changed. He has graduated from college and is going to work, let us say, or has been inducted into the armed services. Most of the behavior he has acquired up to this point proves useless in his new environment. The behavior he actually exhibits can be described, and the description translated, as follows: he lacks assurance or feels insecure or is unsure of himself (his behavior is weak and inappropriate); he is dissatisfied or discouraged (he is seldom reinforced, and as a result his behavior undergoes extinction); he feels uneasy or anxious (his behavior frequently has unavoidable aversive consequences which have emotional effects). . . (Skinner, 1971, p. 139).

Given that Skinner was aware of the pervasiveness of the pre-scientific language of feelings, thoughts, intentions, purposes and other mentalist vocabulary, and given that he offered a prospective translation of all the terms of such mentalist language, is it still appropriate to claim that he suffered from dissociative disorder? Might the so-called disorder not more appropriately be construed as a case of advocating a slowly unfolding change of the pertinent language, from the mentalist to the behaviourist? After all, there is a lot of this going on in other spheres of human concern.

Consider that in politics we have in many places an uneasy co-existence of conflicting language — for example, that of the feudal language involving references to 'subjects', 'your highness', 'her majesty', and the like, with the more democratic or even individualist language of citizen, government by the consent of the governed, president (as presiding officer in the administration of government) and the like. Those in countries with a feudal history may no longer take the concept of 'the subject' or 'her majesty' seriously enough to remain fully loyal to it while not yet having reoriented themselves to what they may well find a more realistic, indeed proper or sound, vocabulary involving concepts that flatly deny the realities of the feudal vocabulary.

Similar changes may be in the offing, and have indeed been in the past, when the various sciences produce discoveries that would appear to overturn the implications of language used before as regards their subject matter. Sometimes, of course, the changes are misguided — given the context the earlier vocabulary could we be perfectly adequate and the claims made, when understood within that context to be well founded, could well be true.

The Popperian idea of truth, implicit in the citation by Baars, that 'The history of science … is a history of error' (p. 22 above) doesn't seem to me to be that felicitous or philosophically sound, unless one embraces a sort of Platonic conception of truth, whereby only the final, perfect statement of what is the case qualifies as a true one. If truth is not treated along these lines but given a contextualist account (Machan, 1982), whereby what is true is not some final, perfect statement of the case but one that is most up to date, well informed for the time being, then Aristotle's physics may have to be judged from the point of view of the conceptual advances that held in Aristotle's own time. It is, after all, a

rather odd notion that all that previous science, on which a good deal of successful craft and technology had been based, would be erroneous or false.

Perhaps what we need to conclude from these reflections — or, rather, from a full development of them — is not so much that Skinner had a psychological disorder but that he was struggling with the challenge of adjusting his ideas about the nature of human life with what he took to be an inadequate conception of various aspects of human life. We all may face this challenge, in smaller or large degrees, as we cope with our workaday ways of thinking and talking about reality and the various proposals to update these coming to us from scientists and other revisionists. The real issue may not be whether Skinner's quandary signified some disorder but rather whether Skinner was correct to think that his proposals as to how we need to reform our thinking were in fact correct.

It seems to me that Baars is far more convincing about the mistakes of Skinner's project than about his characterization of it as a kind of mental disorder. One important reason I see this as a more fruitful project is hinted at in Baars' own discussion, namely, that contemporary epistemology is prejudiced in favour of empiricism, which tends to favour the behaviourists' project. One might add to this that scientism itself has been prevalent since the sixteenth century, as in the attempt by Thomas Hobbes to re-conceive politics along fully reductive materialist lines (Hobbes, 1660), as well as the subsequent attempts of nearly all of the social sciences to give an account of human life in mechanistic terms.

The resulting problems are probably most evident in our efforts to reconcile social science with morality and law. In Kant this led to the postulation of a dualism of the phenomenal and the noumenal aspects of reality, with the former precluding while the latter making possible the moral dimensions of human life (Kant, 1781). In our courtrooms this is manifest in how often juries are required to struggle with conflicting claims about how a defendant in a criminal case did or did not have the capacity to do the right thing, with numerous expert witnesses from the social scientific community urging the thesis of the famous American defence attorney, Clarence Darrow, namely, that there is no crime at all, only illness leading to undesirable behaviour.

What I surmise from this is that we are still better off just concentrating on what is the better outlook on these matters, never mind whether those on any side of the dispute suffer from some kind of personal psychological disorder. By this I do not suggest that the general problem of what accounts for people being wrong (about whatever but, in particular, in areas of abstract thought, including their answers to such questions as what is human nature, what accounts for human behaviour, what is knowledge, what is the origin of life, etc., and so forth) is unimportant. It is certainly an issue that should concern everyone who dares to offer up an answer to any of these questions. Why are all those others who disagree with me so off the mark? How do we make sense of the fact that people can be wrong, when this does not appear to be a problem for other life forms?[3]

[3] I touch on this topic in Machan (2001) as well as Machan (1998), chapter 3: 'Human Action and the Nature of Moral Evil.'

Yet, it seems to me that counting on the kind of psychologizing Baars produces is very risky. It runs counter to the sensible idea that providing diagnosis requires intimate and detailed knowledge of the individual who is being diagnosed.[4] Perhaps Baars has such knowledge. But in his paper it does not appear that he has shown that what he thinks about the reasons for Skinner's mistakes and conflicts can best be explained in terms of the psychological condition of dissociative disorder.

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COMMENTARY

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The article by Bernie Baars was a complete revelation to me. I was a student at Harvard when Skinner was teaching there, and he seemed to be everywhere all the time. I walked out of a psychology class because we were expected to do unpleasant things to rodents, and there was no way I was going to do that. I suppose this had something to do with my later becoming a Freudian (a different kind of error, but that is another story). Students at the time said it was general knowledge that Skinner kept his daughter in some kind of cage (this is untrue, as Baars points out, but academic anecdotes were all the rage then) where everything was regulated according to his theories, and we all assumed she would die some kind of horrible death long before her time (I will have to check with Google to see if she grew up to become a different kind of psychologist). In other words, the word on the campus was that Skinner was some kind of nut. But he was powerful, prestigious and influential. Indeed, if Baars is right, and I see no flaw in his argument, Skinner pretty much single-handedly sent all of American psychology reeling in the wrong direction, a move from which we are still recovering even as late as in 2003.

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[4] The field of psychology, more than any other science, seems to me to require such intimate, even personal knowledge because of how essential individuality is to the conscious life and health of human beings. There are, in other words, innumerable ways of being psychologically healthy as well as ill. Perhaps this accounts for why there are so many failed attempts at identifying a school of human psychology that manages to be both correct and comprehensive.

Otherwise (to get straight away into my topic), why would it prove so difficult to discuss the topic of emotions in animals? John B. Watson (1878–1958), the founder of behaviourism, from his powerful position as professor of psychology in the Johns Hopkins University, as early as 1914 wrote, in the very first line of his book *Behavior*, that 'Psychology as the behaviorist views it is a purely experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods . . .' (Watson, 1914, p. 1). He is more explicit a few pages later: 'It is possible to write a psychology . . . and never to use the terms consciousness, mental states, mind, content, will, imagery, and the like' (p. 9). So questions as to what animals might be thinking and feeling, became the very type of question that was not permitted to be asked. Skinner later turned this into orthodoxy and none of his students, I venture to guess, ever asked, publicly, what an animal might be feeling during the many painful or boring or deadly experiments they performed upon them.

In a recent book about the remarkable abilities of an African grey parrot, Alex, Irene Pepperberg notes that

> in the 1970's, the behaviorist tradition, best exemplified by Skinner, still represented mainstream, laboratory-based behavioral science. In reaction against studies centered on anecdotal evidence and the introspective, 'mentalist' approach that characterized the late 1800's, behaviorists emphasized experimental controls and eschewed discussions of thought or mental representations, information processing, or intentional actions. Scientists canonized the cautionary tenets of Morgan and the strictures of Watson . . . according to Skinner, one needn't study a wide variety of animals, because none would react any differently from a pigeon or a rat: The rules of learning were universal (Pepperberg, 1999, p. 3).

When ethology, the scientific study of animal behaviour, came of age, those who belonged to the new discipline had to confront this doctrine, and in print at least Niko Tinbergen complied, as a famous stern passage from his 1951 book *The Study of Instinct* reminds us: 'Because subjective phenomena cannot be observed objectively in animals, it is idle either to claim or to deny their existence.' Although Tinbergen would often contradict this statement by his own behaviour and his comments about animals, the stricture became *de rigeur* in scientific circles concerned with animal behaviour. His colleague, Konrad Lorenz, agrees with the stricture only formally: there are many passages in his books that rely heavily on subjective experience, either in the animal or in Lorenz.

It is past due for a serious change. It may be that scientific turf is being protected, for to some extent we all have a need to defend what we already do. There are vested interests, not to mention inertia and scientific ossification. Women were often deliberately discouraged from entering the field, and many good minds simply picked up and went elsewhere. This era is now officially coming to a close. Philosophers such as Thomas Nagel take it as a given that one can include 'the subjectively *un*imaginable [sic] mental lives of other species, for example, in our conception of the real world without betraying their subjectivity by means of a behaviorist, functionalist, or physicalist reduction' (Nagel, 1974). At the end of an essay on other minds he is even more explicit: 'To insist in every

case that the most objective and detached account of a phenomenon is the correct one is likely to lead to reductive conclusion. I have argued that the seductive appeal of objective reality depends on a mistake. It is not the given. Reality is not just objective reality. Sometimes, in the philosophy of mind but also elsewhere, the truth is not to be found by traveling as far away from one's personal perspective as possible' (Nagel, 1986). The distinguished neuroscientist Antonio Damasio in a recent highly praised book *The Feeling of What Happens* (1999), writes that 'the conscious mind and its constituent properties are real entities not illusions, and they must be investigated as the personal, private, subjective experiences that they are.' I asked him what he thought of the possible superiority of some animal emotions, and although he found the notion intriguing, he told me that 'I am inclined to believe that animals do not have "superior" emotions. It is certainly the case however, that emotions play a greater role in animals than they do in humans, given their immense power, and given the fact that animals cannot rely on our prodigious capacities of memory and language.'

It is perhaps even stranger, when you think about it, that Skinner and Watson together were able to deflect attention away from human consciousness for nearly a hundred years. What was so threatening here? At least when it is a question of animals, one could imagine an answer: no person fifty years ago (today is a distinctly different age) would ever have even contemplated admitting that any animal could be our equal in terms of feeling — that was heresy. To be our superior (i.e., to feel feelings of friendship more intensely, as could well be the case when it comes to dogs, for example) is still more or less unthinkable. So I can understand the motivation of those animal scientists who deny emotional complexity to animals. But to our fellow humans? I could not imagine what had driven Skinner, until I read the biographical information Bernie Baars supplies. Suddenly it was all so clear: Of course, that is the explanation, I thought, as I read about his failed adolescence. You don't have to be lapsed Freudian to see the psychological connection here. The consequences were grave, as Baars points out, and we all suffer from them today.

The 'suffering' is found in areas that are still considered scientific taboos. Consider, very briefly, three: anthropomorphism, anecdotalism and sentimentalism. To be accused of any of these scientific sins was (is?) serious. You risk your reputation. Witness the savage reviews still emanating from prestigious animal behaviourists, writing in prestigious journals, when they review the work of the great Donald Griffin, the founder of cognitive ethology. What do animal behaviourists, or at least some of them, see as the problem? They maintain that anthropomorphism is everywhere and always a bad idea. To attribute to animals feelings and thoughts that properly belong only to humans, is foolish, unscientific, and — well — anthropomorphic. True, but only if one is absolutely certain that the feelings and thoughts under discussion do, in fact, belong only to humans. That is the very question that the cognitive ethologists are attempting to research. I am not talking, now, about myself, but about far more serious scientists who are pleading for renewed interest and respect for the attempt to place oneself in the mind of the animal, to see the world from the animal's point of

view. Of course that is difficult, but it is not, as some would maintain, impossible. Just see the many essays in the book recently edited by Marc Bekoff, *The Smile of a Dolphin* (2000) where many serious scientists (the table of contents reads like a who's who of the stars in the field, including such illustrious names as Richard Wrangham, Alexander Skutch, Jaak Panksepp, Barbara Smuts, Joyce Poole, Bernd Heinrich, Cynthia Moss, Jane Goodall, Roger and Deborah Fouts and Elizabeth Marshall Thomas) take very seriously the notion that animals have powerful emotions and that one way to find out about them is to use, in a constructive and knowledgeable manner, anthropomorphism. Obviously there is such a thing as bad anthropomorphism (my dog is sad because I forgot her birthday) but good anthropomorphism, based on an ever-increasing knowledge of what actually concerns the animal (and why would not physical concerns find their mental representations in the minds of animals as they do in ours?) is an aspect of observation. If you are in the field, observing, which is where you should be, you are likely to find yourself wondering what the animal is thinking and feeling. What could possibly be wrong with that? And if somebody who has been observing for longer than you comes along and tells you a good story, that is, provides an anecdotalized observation, why should you discard it? As for sentimentality, what on earth is wrong with observing with feeling, which is all a sentiment is? It is not sentimental to feel along with the animal you are watching, it is human, possibly even mammalian (what dog does not feel your pain better than Clinton?). To banish sentiments from observation is foolish and in any event, impossible to achieve, All it means is that you will stop talking about what you feel with colleagues who are not sympathetic (another sentiment).

What I still cannot understand, however, is how mainstream psychology went along with this scientific farce for so long? The psychology of Skinner is now clear to me. How, though, are we to explain the vast horde of imitators both within and without the academy? Are we like sheep (silly metaphor — sheep remember their friends for years) blindly following the flock? Was it fear? Desire for advancement? A need never to think for ourselves? I wish Bernie had suggested some answer here, but perhaps there really are not any.

Darwin did not work under any of these intellectual strictures. It is puzzling to me that we have not taken seriously the direction he showed us already in 1873 in his book *The Expression of the Emotions in Man and Animals*. He saw the continuity that *must* exist between humans and animals when it comes to consciousness and emotions. It stood to reason psychologically and anatomically (our brains, after all, are remarkably similar to most other mammalian brains) and even from an evolutionary point of view. He saw no problem in investigating the emotional life of his first son, when William was no more than a few months old. He wanted to know the origins of friendship, cooperation, love, humour, all of the questions that are still with us today. Had we followed his lead, had we not been sabotaged by Skinner and radical behaviourism, would we, today, be closer to an answer? I think that Bernie Baars would have us believe that we would, and I, for one, am ready to accept his bold, startling, and remarkably freeing, thesis. It is time to get to work!

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A DOUBLE DISSOCIATION IN TWENTIETH CENTURY PSYCHOLOGY?
A commentary on Bernard Baars: The Double Life of B.F. Skinner

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In his target article in this issue of *JCS*, Bernard Baars convincingly argues that an understanding of B.F. Skinner s personal and professional trajectory may be exemplary for the understanding of the development of twentieth-century Anglo-American psychology and philosophy. This places the narrative about a young man's unresolved tensions and unfulfilled ambitions into a richer and much more interesting context.

The account of Skinner's double life is such a good story that one is left wondering why it has not long ago become part of the standard lore of psychology, along with Pavlov and his salivating dogs in the early days of the Soviet Union; and Freud and the hysteric Viennese women during the de facto break-down of the Central Europe of the double monarchy. Like these, this tragic *bildungsroman* of how the prospective stream-of-consciousness writer in small-town Pennsylvania became a denier-of-consciousness scientist has narrative qualities of an almost mythological nature. It is story of how the world (of psychology and related disciplines) came to be the way it is.

Baars demonstrates how the scientific taboo on consciousness has to be seen in relation to a parallel focus on consciousness within other intellectual traditions in the twentieth century such as literature and arts. It seems very probable that this split is but one instance of division between the sciences on one hand and the humanities on the other which was widespread in the century that we have just left. A split, which was first described as originating in two different cultures (Snow, 1959) but which is now often seen to be part of a peculiar modern constitution (Latour, 1993; 1999) that in a highly specific way separated the world into distinct entitites such as nature and culture with the mental in strange ways located right in the middle.

No matter which perspective one accepts, this suggests that Skinner's life was, indeed, right in the vortex of processes that extends beyond the single individual. By implication, the story of psychological dissociation told by Baars could

perhaps be complemented by another one, told from a perspective of more general cultural and social processes, since the story is very much about how certain styles of thinking (Fleck, 1979) and reasoning (Hacking, 1992) became vindicatory at the expense of other perspectives.

Frederick Bartlett: Constructing memory, forgetting intentions

There are in this respect interesting parallels and contrasts to be found in the life-narrative of another of the founding fathers of psychology, Frederick Bartlett (1886–1969), who took up the first chair in psychology at Cambridge University in 1931. From this position, Bartlett resolutely promoted a pattern for psychology as experimental, practically minded, atheoretical and anti-intellectual that is still regarded as the hard-core of the discipline within most university departments in Great Britain (Costall, 1995). This understanding of psychology as a glorified button-pressing endeavour, easily integrated into the Faculty of Natural Sciences, helped to put the discipline firmly on the academic and scientific map. However, in spite of the obvious success of psychology as an established intellectual discipline, Douglas (1986) and Costal (1995) both claim that the actual content that became known as the Cambridge School was not at all what Bartlett had wanted. What had gone wrong? During his formative intellectual years, Bartlett was strongly inspired by the anthropologists W.H.R. Rivers and A.C. Haddon. They had both participated in the famous Cambridge expedition to Torres Strait in 1898 to study cross-culturally and in an evolutionary perspective the foundations of human cognition. Over the next decades they, each in their own way, attempted to formulate theoretical models that could integrate a societal and an individual level (Douglas, 1986, pp. 83–6). Along these lines, Bartlett attempted in 1913 to write a book on how eminently social processes like standardisation and conventionalisation affected individual cognition.

Like Skinner's attempt to write a stream-of-consciousness novel some ten years later, this project was never finished, and it apparently took Bartlett on the verge of a nervous break-down. He saw the limitations to the experimental models pursued by the Gestalt psychologists, who mainly worked on particular mental faculties, but it was impossible for him to turn his interest in the social processes guiding attention into actual experimental designs (Douglas, 1986). Meeting up in 1913 with the then 19-year old wiz-kid Norbert Wiener, the father-to-be of cybernetics, gave Bartlett the tools to prove how cognition was indeed an active process where 'observers constructed a terminating design before they had reached it and reported having seen detail which in fact was not there' (Bartlett, 1958, p. 142, quoted in Douglas, 1986, p. 88). However, his experimental designs, which became increasingly strict, subtle and amenable to objective scoring, effectively led him, and the discipline at large, still further away from the initial interest in the interplay between individual, social and institutional processes in perception in cognition. As concluded by Douglas: the experimental stringency required that the particular differences of emotional interest affecting each subject to be strictly excluded. The social dimension of

their experience was peeled away from the subjects (Douglas, 1986, p. 88). It was arguably this development, and the fact that the psychology, of which Bartlett became the founding father, increasingly became a technical matter of statistics and experimental design that led Bartlett to be increasingly dissatisfied with his intellectual brain-child, to the extent that anecdotally he is said to have claimed, at a reception celebrating applied psychology in Cambridge, that 'It had all gone badly wrong, *I wish I had written novels instead*' (Costall, 1995, italics added).

A double dissociation in psychology?

There is an interesting symmetry between these two men: Skinner, who tried but failed to become a novelist, and Bartlett who wished he had become one. Skinner whose life embodied a fenced off polarity between a scientific life of disciplined objectivity and an inner life of lush and conflictful subjectivity (Baars, this volume) and Bartlett: the expert on memory who himself managed to forget his own teachings. He who taught that intentions guide cognition, forgot his own intentions (Douglas, 1986, pp. 88–9). In the juxtaposition of these two founding fathers, both exposing what seem like mechanisms of dissociation, we are perhaps left with the contours of some of the practices and ideas that became central in mainstream Anglo-American psychology in the twentieth century.

There appear to have been at least two process of dissociation, this time at an epistemological level rather than at a psychological level. On one hand, a dissociation of the subjectivity of lived experience from the objectivity of observable stimulus–response paradigms. On the other hand, a dissociation of the generalizable experimental subject from those concrete, individual persons, embedded in social and cultural relationships, who volunteer to play the roles of subjects for the psychological examinations (Roepstorff, 2002). Whereas the narrative of B.F. Skinner, in Baars' excellent reading, gives us the narrative of the former, the trajectory of F. Bartlett, through the readings of Costall and Douglas, gives the other story. In both instances, we are witnessing the inner conflicts and unsolved struggles of real persons that, in a way, are much more complex than the intellectual traditions that have come to be identified with them.

Lifting the taboo: bringing within limits

Baars uses the notion of taboo to describe the role assigned to consciousness by Skinner and his followers. The word entered into English usage through James Cook's explorations in Polynesia in eighteenth century, but as with many imported concepts, the actual meaning of the word remains somewhat obscure. Based on a re-interpretation of ethnographic evidence, it has recently been suggested that the proper translation of the Polynesian word *tapu* is neither sacred nor forbidden, as is the standard English usage, but rather off limits (Keesing, 1985). Something is *tapu*, off-limits, only if some agent defines it as such, only given a certain perspective and always to someone. It cannot be *tapu* in and of itself. Being *tapu*, implies a context. A place, act or thing that is *tapu* this

afternoon from the perspective of people and in the context of a particular ritual or circumstance may be *noa* [within limits, AR] or *tapu* for different people tomorrow (Keesing, 1985).

This understanding of taboo moves the concept — and the social and cultural practices related to it — away from a sacred domain and into a somewhat more pragmatic interplay between authority, control, and accessibility. To cut a long story short, the main trend in the scientific field broadly characterized as consciousness studies has been an attempt to bring into focus some aspects of human subjectivity and experience, that had for long been considered off limits methodologically as well as conceptually. As argued by Baars (this volume) and McCrone (1999), recent developments in functional neuroimaging have been an important facilitator in this process. This seems probable, since the classic objection against introspective evidence and conscious processes was that such phenomena could not be measured. They were therefore inherently subjective and as they were bound to the individual, rendered them off limits for an objective scientific description.

However, in many brain imaging experiments, there are no discernible objective differences between the conditions as seen from a third-person outside observer. Instead, the effective contrast is established by differences in the 'script' enacted by the subject during the experiment, and this can best be validated by reports from the subjects on their emotional or attentional states during the experiment (Jack & Roepstorff, 2002; for recent examples see Blood & Zatorre, 2001; Gallagher *et al*., 2002; Lutz *et al*., 2002). We may, in other words, be witnessing a rather solid development where consciousness, understood as the acceptance and pragmatic use of a first-person perspective, is indeed increasingly used in a rather unproblematic way as a variable in cognitive experiments, publishable in mainstream scientific journals.

Are we effectively experiencing a weakening of the scientific taboo on consciousness? Perhaps consciousness is no longer a concept that scientists may talk about only through euphemisms, and it is no longer off-limits for examinations that would like to call themselves scientific. This suggests that Chalmers famous hard problem of the 1990s, which confronted the subjective first-person perspective with the objective third-person perspective, has indeed been transformed into a resource that may directly be used in generation of novel facts. Facts, which to paraphrase the epistemologist Ludwik Fleck (1979), become points of resistance that other researchers must take into account.

The second-person enigma

It seems, therefore, not unrealistically optimistic to believe that Skinner s painful dissociation is indeed being left behind — and Baars' narrative in this volume may assist in that development. But what about Bartlett s problem? What about the fact that successful paradigms for psychological experiments seem to factor out social and intersubjective elements in cognition and perception and, by extension, in consciousness? Within cognitive science and consciousness

studies, the apparently inherent communicative and intersubjective aspect of human experience, which we by analogy to linguistic terminology may call the second-person perspective, appears to be investigated even less than the first-person perspective (see Thompson, 2001, for an overview). However, current brain imaging experiments pose an interesting challenge to this dissociation as well. A detailed analysis of the actual practices and procedures involved in brain imaging experiments demonstrates that this perspective seems an almost unavoidable aspect of the experimental situation. Even when studying apparently simple stimulus–response paradigms, such as the relation between a tickle-under-the-foot and a finger-tap, intersubjectivity — in the form of a shared understanding between the experimenter and the experimental subject — is crucial for setting up the experimental situation (Roepstorff, 2001). Indeed, one of the most stunning facts about brain imaging experiments is that it is so easy to convince people that they should lie completely still in an unfamiliar environment, expose themselves to strange sorts of radiation and magnetism, and then act as if they were simple stimulus–response automata (Roepstorff, 2002). That this is a special and incredibly useful feature in human interaction is at least clear to researchers doing monkey fMRI, where performing even a simple cognitive task adequately in the scanner (Nakahara *et al*., 2002) requires many months of training the animals, while humans can simply be told what to do (Miyashita, personal information).

By an extension of Chalmers first-person hard problem, we may call this ability for humans to rapidly exchange, share and sometimes disagree on understandings of situations and models for action the second-person enigma. Factoring this perspective into cognitive brain-mapping experiments translates into embedding the objective stimulus–response setting of the behaviourists into a second-person script-report scenario (Jack & Roepstorff, 2002). Although this may not solve Chalmers hard problem, factoring-in the second-person enigma simultaneously renders that, which appeared as an insoluble problem, pragmatically useful (op. cit.) and theoretically somewhat more interesting. It takes as a starting premise what appears to be an unquestionable anthropological fact — in two senses of the word — that so much of human perception and cognition is directed against and mediated by other people (Roepstorff, 2001).

Styles of knowing for the twenty-first century?

The twentieth century was a time for creating new scientific disciplines that each — like a nation-state, the political ideal of the period — had to carve out their own unique and easily identifiable part of the total conceptual landscape. This joint reading of the personal trajectory of two founding fathers suggests that for much of Anglo-American psychology, this meant cutting out, for very sound methodological reasons, important aspects of mental life: on one hand, the subjective, individual experience, on the other, the interpersonal sharing of understanding. Instead of digging trenches, the challenge for this century may rather lie in outlining pragmatic research positions, from where one can see that

humans simultaneously and apparently seamlessly relate to and interact with three very different entities in the world: an inner self, an outer physical world, and a bunch of other subjects that also relate to themselves, to the world and to others. Theoretical positions, which try to accomplish this, seem to pop up from various corners of the established conceptual landscape such as anthropology (Ingold, 2000; forthcoming) and phenomenology (Zahavi, 2001). These approaches may give the initial contours for the styles of inquiry, reasoning and knowing, that could appear attractive and indeed vindicatory for the twenty-first century.

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A COMMENTARY
on 'The Double Life of B.F. Skinner' by B.J. Baars

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Introduction

B.F. Skinner revolutionized thinking about behaviour. Like others who challenge prevailing ideas, his writings received both praise and attacks. Along with the attacks, misconceptions sprang up. Some were passed by word of mouth, others found a home in articles, textbooks, and even encyclopaedias. Skinner's daughter Deborah, the one for whom he built the special crib he called the 'baby tender', is rumoured to have committed suicide. Since she is my younger sister, I can verify that she is alive and well. But other misconceptions are harder to put to rest. One can still find Skinner classified as an S–R (Stimulus–Response) theorist in print, although that is the position against which he fought. The article by Baars has some interesting ideas about the impact of my father's 'dark year' on his intellectual position, but it gives an inaccurate picture of Skinner's intellectual contributions and describes a different person from the father I knew. I'd like to comment on three points: (1) the break Skinner made with S–R behaviourism, (2) the role of consciousness in his analysis, and (3) what my father was like as a person. In the first two sections, I will refer to my father as 'Skinner' as befits a technical discussion.

Skinner's break with Watson's S–R behaviourism

When B.F. Skinner arrived at Harvard in the fall of 1928, he considered himself a follower of Watson and Pavlov. A science of behaviour needed to find variables of which behaviour was a function and Pavlov had shown how new S–R relationships were formed. Skinner set out to continue this line of research, encouraged not by the Psychology Department, but by William Crozier the chair of Physiology. By the end of his first year of graduate studies, Skinner began a sequence of research that was to lead him away from an S–R analysis. A fortuitous combination of shop skills, philosophical outlook, and sensitivity made his discovery possible. Skinner loved to build equipment. He was always ready to scrap an apparatus he had built when, watching his rats and looking at the records they were generating, he thought of a better way of doing things. While still a student he built at least six different kinds of apparatus, starting with runways and ending with a prototype of the experimental chamber for which he is known (Skinner, 1956; Vargas, J.S., 2002). Over those two years he became aware that what he was seeing was totally different from the S–R behaviour of Pavlov and Watson. The rat was not responding *to* a stimulus. Control over its behaviour lay in its consequences. In October of 1931 he wrote a letter to his best friend, Fred Keller. The letter is lost, but Keller's reply starts, 'The only thing that bothers me about

your very welcome and newsy letter was that talk about a brand new theory of learning' (Keller, 1931).

It was a complete shift in the thinking about where control over most of our behaviour lies. The behaviour that Pavlov described only constituted one kind of behaviour — reflexes. Skinner had stumbled on a second kind that he named 'operant'. Operant behaviour comprises most of what we do. Our operant behaviour is controlled not by a preceding stimulus, but by what follows individual actions in a process of selection by consequences. Everything we do affects our immediate environment and that impact in turn determines the way we will behave in the future. It is a fluid and continuous process.

Skinner spent the next seven years investigating the parameters of these interactions. He used rats, but was no more interested in rats than the geneticist is in fruit flies. Rats were simply a convenient species with which to find properties of behavioural interactions. (Later, in 1942, when Skinner found that pigeons could generate more data in a shorter time, he switched to pigeons.) Skinner called the differing ways in which postcedents and antecedents could be related to behaviour 'contingencies of reinforcement'. He found that antecedent stimuli gain control over operants only when paired with particular consequences for responding in their presence. He examined the patterns of behaviour produced by reinforcing only some responses, the impact of delaying the time between reinforcement and responding, and everything else he could think of. In 1938 he published *The Behavior of Organisms*, summarizing his seven years of research (Skinner, 1938).

Skinner's discoveries about the relationship between behaviour and contingencies left no room for a 'self' that decided what to do. Nor was he interested in genetics or neurological processes, both of which he considered different fields with different contributions to behavioural science. Our genetic make-up, of course, contributes to what we do, but genetics cannot explain our behaviour when, for example, we talk of 'consciousness'. Neurophysiology, Skinner would say, will someday find out what is happening when a reinforcer strengthens behaviour. Even knowing exactly how the brain functions, however, will never tell you whether your friend will go to a movie or read a book. For that level of analysis you need an analysis of contingencies. It is contingencies that determine operant behaviour, including the activities going on in the brain.

Baars makes a good point when he draws attention to the importance of language. It took Skinner many years to drop the term 'reflex' in his writings, in spite of the fact that he was writing about operants, not reflexes. He never did abandon 'response' for a single action, thus leaving the reader with an implied antecedent stimulus. It took him over a dozen years to call the process of operant conditioning 'selection by consequences', the phrase currently used. Perhaps Skinner's continued use of relics from Pavlov's and Watson's writings have affected commentators so that they, like Baars, pair Skinner with Watson, in spite of Skinner's break with the Watsonian S–R tradition. The arguments Baars makes against Watson's S–R analysis may be justified for Watson, but they do not apply to the selection by consequences of Skinner.

The role of 'consciousness' in Skinner's analysis

A scientific analysis of human conduct must find some kind of relationship between the behaviour to be explained (the dependent variable) and events that produce that behaviour (independent variables). Baars asserts that Skinner 'expelled consciousness'. Skinner expelled consciousness only from an agency role. He had thousands of hours of data demonstrating extremely precise control over behaviour by the arrangement of contingencies. Postulating an internal causal agent, such as volition, attention, or self did not add anything. His rats and later his pigeons did not behave because of 'volition'. The pigeon that pecked a key 60,000 times without getting any food didn't do so because it 'intended' to do so. All of its behaviour, including the activity in its brain, occurred because of the careful shaping it had received. Teachers use similar techniques when they reinforce student work frequently at first, and then slowly thin out the frequency of approval until the student works for long periods on his or her own.

The fervour with which Skinner argued against agencyism was more than an attempt to clear up confusion between valid and invalid variables. Looking inside for nonverifiable causes was not only futile, he argued, but harmful. Attributing behaviour to internal 'causes' is too often circular. It lulls one into complacency instead of action. For example if a teacher is told that Jack doesn't work because he isn't motivated, the observable behaviour (not working), has been restated as an internal state (not being motivated). Now the 'cause' is inside Jack. It is Jack's problem, not the teacher's. By 'expelling volition', on the other hand, the teacher is encouraged to look for factors in Jack's environment that can be changed. Perhaps Jack was never taught prerequisite skills, perhaps his work has been ridiculed, or maybe his disruptive behaviour gets more attention than doing assignments. Whatever Jack's situation, by 'expelling' these circular agents, the teacher is more likely to look for the contingencies responsible for Jack's behaviour, so they can be changed.

Skinner 'fought relentlessly' against agencyism but not against the behaviours the term encompasses. Consciousness was, for him, part of the behaviour a science needs to explain. In *Science and Human Behavior* (1953), he wrote chapters called 'Self-control', 'Thinking', and 'The Self'. In his 1957 book, *Verbal Behavior*, he spent 470 pages analysing the contingencies over what people say, write, and think. His analysis of different sources of control over verbal behaviour has been supported experimentally, and a journal based on his analysis, *The Analysis of Verbal Behavior*, is in its nineteenth year. What would have pleased him even more are the breakthroughs in teaching children with autism to communicate that have been derived directly from his analysis.

Skinner was always interested in language. Towards the end of his life he began tracing the etymology of the words we use to talk of internal feelings. He analysed over a thousand of them. To his delight he found 'they almost always began as references either to some aspect of behavior or to the setting in which the behavior occurred' (Skinner, 1989). 'Anxious', for example, comes from the root word 'choke'. 'Cognate' originally meant 'shake up'. Over time, people learned to talk about the correlates of these external events, that is, to describe

feelings and thoughts. We only learn to do so, however, when taught by those around us. Members distinguish between subtle distinctions that are important for a community, and the language grows. Skinner gives four ways in which we learn to describe internal behaviours (Skinner, 1945b; 1957). For example, seeing a child wince when eating an orange, we might ask, 'Do your teeth hurt?' Through similar interactions, the child becomes 'conscious' of what he or she is 'feeling', and acquires words to describe those states. Responding differentially to aspects of one's world, whether verbally or nonverbally, overtly or covertly, is what 'consciousness' is all about. Skinner analysed many other specific behaviours we would classify as 'consciousness' but not as causes for the behaviours used as evidence for them. As E.A. Vargas put it, 'any explanation that relates the values of an independent variable in some equational fashion to the values of a dependent variable eventually dispenses with the necessity of an agency as a causal force' (1996). Covert behaviours to Skinner were less accessible, but no different in kind than any other behaviours: 'Thinking is behaving. There is nothing inside the behaving organism but the organism itself. It is the whole organism that behaves' (*New York Times*, 1987).

Skinner as a father

One can still read descriptions of B.F. Skinner as a cold scientist who experimented on his children as if they were rats. These descriptions sadden me. He was a wonderful father. No matter how hectic his schedule or how imminent a deadline, he always found time for my sister and me. I remember as a little girl singing Gilbert and Sullivan duets with him, my father taking the lower voice and playing piano at the same time. Things would go well the first verse. But the words for the rest of the verses were printed at the end of the second page, making it difficult to read them and the piano part too. We would start the second verse, but more and more of the accompaniment would drop out. Eventually he would stop playing altogether and we would both laugh. 'Let's do the first verse again,' one of us would say, and so we would, triumphantly. When my sister and I were young, it was my father who put us to bed. Both of us devised strategies to hold him there a little bit longer after he finished the customary story. It was easy to do. He enjoyed the quiet time with us as much as we did. As we grew older, he took us to his lab on Sundays so we could play with the pigeons, explaining the basic principles of behaviour that we then used to teach our dog tricks. Over the summers he taught us to use hand tools in the shop at the back of our summer cottage. He was always building things himself. With some old pulleys he made us a 'trolley'. He strung a rope between two trees on a hill with a hand bar attached to a pulley on the rope. Grabbing the hand bar and lifting your feet would give you a wild ride down over the hill. In our early teens, my father would go on long walks with us. I remember many discussions about topics that, years later, I recognized when reading articles he had published. In spite of his commitment to his profession, his family came first. When my sister broke a leg skiing in France, he immediately cancelled all obligations and flew over to be with her.

My father did not experiment on my sister or on me. He did design a new kind of crib when my mother, pregnant again five years after my birth, expressed concerns over the dangers of the baby smothering in blankets. 'Fred,' I can hear her saying, 'can't you do something about that?' Of course, he could. The 'Baby Tender' he built had the same footprint as a standard crib, but was enclosed and heated, so no blankets were needed. Pleased with his invention he wrote an article about it and sent it to the *Ladies Home Journal*. The editor gave the article the title, 'Baby in a Box' (Skinner, 1945a). Confusion with the 'Skinner Box' followed. But the baby tender was a bed, not experimental apparatus, and got 'about the same use' as a standard crib. My sister, Deborah, had a playpen like other babies of the era.

Reflecting on my father's life I do not see the 'dissociation' of Baars's article. My father loved life and dreamed of improving every aspect of it through behavioural science. He exercised 'self-control' by adapting the environment that controlled his own behaviour. His home office has been maintained as he left it, and it shows dozens of adaptations. The desk has everything close at hand needed for writing. A light and clock turn on with a single flip of a switch. The light served as a discriminative stimulus for writing, the clock recorded his time. In my college years, when I would interrupt his early morning work, he would switch off the light and swing around in his chair to face me with a cheery 'Hello'. As I left, I would hear the click of the switch that would recreate the thoughts he was putting on paper.

Problems to my father were not deterrents, they were challenges. Do you lose the remote control for your TV? Fasten a custom-made wooden holder onto the arm of the recliner you sit in while watching. Do you forget to put stinging medicine into your eye? Fasten the little bottle to your toothbrush with rubber bands. When my father's eyesight failed, he bought a large lens a third of a meter wide supported by an arm. But the lens jiggled. So he glued wooden supports to its frame, ran nylon fishing line from them straight up to the ceiling, through screw eyes, along the ceiling to the wall, through more screw eyes, then down to weights made by casting concrete in tin cans. The lens would stay put in any position, allowing him to continue to enjoy reading.

My father had seen the harmful effects of punishment and all his life argued against it and avoided it as a method of control. He enjoyed 'fame', but was also wary of its seductive effects. He never 'took pleasure in the public image of the coldly objective professor' as Baars asserts. When making TV appearances for *Beyond Freedom and Dignity* he told me, obviously pleased, that the main reaction of audiences was, 'He seems a nice friendly man.'

When my father learned he had leukemia I took a leave of absence from my university and moved into my parents' home to spend my father's last months with him. He had no fear of death. In fact he would joke about his illness. 'I recommend leukemia as a way to go,' he'd say, 'It's not painful and you keep your wits about you.' And that he did. Eight days before his death, upon receiving the first APA lifetime achievement award, he delivered a 20 minute talk to a packed audience. My mother and I accompanied him. As we entered the huge room, the

entire audience stood up and began to applaud. The clapping continued as he was helped up stairs to the stage and to a chair. After introductory announcements that seemed interminable to my mother and me, his award was presented and it was time for his talk. He spoke without notes. His frail 86-year old body, and his poor eyesight did not hamper the force of his delivery. The themes were familiar to me: The problems facing the world — overpopulation, pollution, war — are behavioural problems. To solve them we must turn to a science of behaviour. Establishing the science of behaviour will not be easy: 'You know how difficult it has been for natural selection to be accepted. Imagine how difficult it is going to be for the individual selection by consequences of operant behavior, or the other kind of selection — the evolution of cultures — to take over the role of a creative self or mind' (Skinner, 1990).

My father spoke for almost exactly the 20 minutes he was asked to speak. He ended his talk with the last words he was ever to speak in public (Skinner, 1990):

> What I have tried to do, . . . what I have been trying to show, is how selection by consequences in the individual can be demonstrated in the laboratory . . . and to show the implications of that for the world at large, . . . Any evidence that I've been successful in that is what I should like to be remembered by. . .'

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COMMENTARY

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This is a lovely piece scholarship by Bernie Baars. It presents some fascinating and little appreciated aspects of the man behind the doctrines, and in the tradition of psychodynamically informed biographies, it helps us understand how ideas arise out of personal and conflicted life history. In this case, he outlines how behaviourist doctrines emerged from the very kind of internal struggles that B.F. Skinner disavowed as having any meaning as a scientific matter whatsoever. Quite a 180 degree 'about-face' that Skinner went from a guilty and failing novelist to someone who dissociated himself from any notion that his (or others) internal life was meaningful at all! But it is exactly the kind of life history that would make the creation and evolution of radical behaviourism more richly meaningful. Indeed, it makes the whole thing a much more human story, and for this alone, Bernie deserves much credit.

Not only did behaviourism have to 'trash' consciousness, for many reasons, but it had to remove emotion as a primary motivating force in human and animal life, assigning motivational primacy to psychology's equivalent of *ether* and *phlogiston*: the mythical process of 'reinforcement'. Of course the notion of reinforcement always begged the question as to why human beings and other organisms, sentient or otherwise, were constantly running around in hot pursuit of these 'reinforcers'. Too close a focus on this would have involved seriously addressing the problem of organismic value: how it is that biological and social value is generated 'inside' the organism. As long as the brain was dismissed as an unknowable and mostly irrelevant 'black box' this question could be effectively shunted off and marginalized, leaving radical behaviourism largely content with its pseudo-explanation ('reinforcement').

The problem of course with this radical surgery on the human psyche, excising that amorphous issue of mind and consciousness, was that *affects* are really great reinforcers, and the external triggers for affects ('reinforcements' in the environment in behaviourist lingo) have power only in proportion to their capacity to *activate* these primary internal states. Behaviourism was always backpedalling on this issue, trying to make sure that emotion, particularly its manifestation in consciousness as *feeling*, was not smuggled in the back door as a Trojan horse, opening the door for the potential sacking of the key radical behaviourist doctrine that consciousness was 'verboten'. Somehow behaviourism knew that it had to pay more than lip service to the basic issue of emotion, or else it would have no explanatory purchasing power, even in a positivistic and somewhat reductionistic American psychology landscape. (Curiously, Europeans seemed less impressed with behaviourism overall, perhaps validating their longstanding claim that they are more sensible than Americans.) Of course, without any real psychology of value or emotion, other than the pseudo-explanation of reinforcement, behaviourism had gutted the core of what it needed explain in depth to

claim any real dynamic or scientific understanding of behaviour. This denial that affects were really the great 'reinforcers', even more than the dismissal of consciousness, at least in my judgment, doomed radical behaviourism.

An interesting question about failed scientific ideas, those that in retrospect appear to be mostly ideological and 'anti-heuristic', is to what extent the creative forces behind these failed scientific ideas reflect human defensive needs or defensive distortions versus just 'bad thinking'. In other words to what extent is a fundamental conceptual error just a 'misfiring of cognitive circuits' vs. it having more affective origins? Although psychodynamic and psychoanalytic perspectives are now in many circles almost as discredited as consciousness was in the heyday of behaviourism, psychodynamics has always presented a consistent argument (with abundant data to back it up) that 'bad ideas' are not random cognitive accidents, but instead purposeful distortions. Psychodynamic psychology would argue that they stem from our ongoing need to delimit the activation of troubling and painful affective states, particularly those that are repetitively troubling and painful, in other words from maladaptive defensive structures that distort internal reality. It is not hard to imagine how a group of attachment-phobic and emotionally constricted academic males, many of whom may have been somewhat alexithymic, at a minimum mostly cut off from any awareness of their own emotional vulnerability, could rally around the banner of expunging from psychology any set of concepts that would address or acknowledge such messy and sticky emotional vulnerabilities, particularly those that might emphasize the notion of inner, private experience, and the core sense of internal vulnerability to loss, and its close relatives, guilt and shame. Such a group could be well motivated to commit themselves, hook, line and sinker to the radical behaviourist manifesto. No more needy, gooey insides centred around our need for love and connection, and our potential hurt and anger about having those taken away or frustrated in other ways. Just clean, neat, simple reinforcers. A solution tailor-made for highly cognized, somewhat alexithymic 'left hemisphere' males.

Of course, this whole line of analysis can be branded as an ad hominem argument, and as harshly attributing psychopathology where there may not necessarily have been any. This may be partially a fair criticism, but when we look at the grotesque distortions of attachment phenomena advocated by the radical behaviourists, such as the advice to seriously deprive infants and young children of love and physical affection (at great emotional cost to those children whose parents were foolish enough to believe this nonsense), one has to wonder about how much real psychopathology was behind such ridiculous recommendations. This notion was based on the absurd rationalization that it would delimit Oedipal conflicts. Those who have worked in psychotherapy with the seriously affectively deprived know that early loss makes triangular competition more, and not less, desperate. Clearly these were psychologists who had not learned much about the human psyche, their own least of all.

I think that Bernie is also correct when he states that behaviourism did not entirely die. In many ways, behaviourism is alive and well in neuroscience, particularly animal neuroscience, aka behavioural neuroscience. In behavioural

neuroscience, there is oftentimes the presentation of a 'principled agnosia' about whether species other than humans could possibly have any form of sentient or subjective experience. This extends even to a denial in some quarters that animals feel pain, or any version of prototype emotion, such as fear or anger. Clearly, there is an intrinsic epistemological barrier here, and one cannot 'know' the answer to these things in any absolute sense. But given how badly we do as a species in terms of understanding internal states in our fellow Homo sapiens, we should be understandably very cautious about attributing (or misattributing) the existence (or absence) of internal states in other species.

There are basic nosology and taxonomy issues here also, in terms of clearly distinguishing between core or primary or more affective consciousness and more extended cognitive forms of consciousness found in language competent adult Homo sapiens. Since taxonomies drive research methodologies, distinguishing between more primitive and more cognitive forms of consciousness is vitally important. In the past, a pervasive failure to observe this increasingly validated distinction seriously hampered any kind of empirically derived and evolutionarily well grounded understanding of the phenomena of consciousness. The assumption that consciousness is a more highly distributed property of animate life is driven (at least for me) from pervasive and fundamental neural and behavioural homologies that humans share with other animals, particularly mammals, and of course primates. We share an enormous degree of brainstem and mesodiencephalic architecture, considerable paleocortex, aminergic and peptidergic neuromodulatory systems, and basic affective-motivational systems with a wide variety of mammals. This (for me) is enough evidence to suggest at least a more primitive form of sentience in creatures besides Homo sapiens, but it may not be enough for others, particularly those who think that consciousness is mostly about cognition, as opposed to higher level cognitions being the last layer on the consciousness onion. To assume an absence of any kind of phenomenal experience in mammals and primates means that these multiple behavioural and neural system homologies do not generate any homologies in terms of the creation of an 'inner phenomenal space' or sentience. This can only be understood from my point of view as straightforwardly dualistic and therefore untenable.

Humans depart from other mammalian lines of evolution principally in terms of extended neocortical and prefrontal system development. These give humans vastly enhanced cognitive/conceptual abilities, particularly language, along with extended capacities for working memory, planning, and other highly cognitive aspects of executive functions/behavioural organization, extending more primitive executive functions supported the brain's prototype affective operating systems and in the basal ganglia. However, earlier anthropomorphic notions that any form of consciousness existed only in language competent creatures seem untenable and are increasingly questioned and even scientifically discredited in some circles. This older point of view also blurs above-noted critical distinctions between foundations for a basic sentience and foundations for higher cognitive processes. I would argue, in concert with leading theorists and recent investigators such as Damasio, that those higher cognitive processes rest on a primitive or

'core' or 'primary' form of consciousness. From this point of view, cognition is the latest evolutionary layer on the consciousness onion. This is explicitly different from most cognitivistic notions advocating the reverse hypothesis, namely that consciousness depends on higher cognitive processes. The preponderance of evidence favours the notion that these higher cognitive functions rest on foundations provided by the basic affective-homeostatic functions of the brain, as all cognitive activity is directed and motivated by those affective systems.

From these considerations, I would argue that the default position at this point would be to accept that other creatures at other levels of the phylogenetic scale may well have some version of conscious experience even if it is exceedingly difficult to specify its basic content, or its fine-grained phenomenal nature. While this conclusion may be intuitively appealing, particularly around empathic resonances with the affective states of mammals, some researchers believe that since we cannot confirm such conscious processes in other species to assume their existence at all constitutes 'anthropomorphism'. While sensitive to this concern, I think there is also the complementary problem of 'species-ism' with respect to consciousness, defined as the tendency to overestimate one's own species and underestimate others. These two problems (anthropomorphism and species-ism) are best understood as the 'Scylla and Charybdis' of the field, in that too much vigilance about one may mean failing prey to the other. I think that in general cognitive neuroscience, and most particularly behavioural neuroscience, have been too concerned about anthropomorphism, and inadequately concerned about species-ism.

There is additional evidence that the principled agnosia about animal consciousness in behavioural neuroscience ignores the lesion literature on consciousness, in which the evidence suggests that consciousness depends most on brainstem and mesodiencephalic systems, and much less on neocortex, with neocortex supplying highly differentiated cognitive/sensory content, but not required for pain, prototype affective states, or for a more basic version of the 'movie in the brain'. Those late arriving neocortical systems are also much less critical for a primitive kind of self, as many authors, notably Damasio (1999) and Panksepp (1998), have cogently argued in recent works. This is not the conscious 'self' or the 'self-image' existing in humans, or the autobiographical self supported in long term episodic memories, but a more primitive 'ownership' and sense of agency in which the 'movie in the brain' has a centred, 'owned' perspective, invisibly embedded in it. This more primitive aspect of self, ownership, has also been understandably much harder to research empirically, and except for the largely speculative offerings of Panksepp and Damasio, a subject about which there is even little theory. Damasio suggested that this aspect of self has to do multiple regions that dynamically map the state of the body in an ongoing manner in the brain with respect to basic visceral, homeostatic, proprioceptive, and somatosensory dimensions (the 'proto-self systems'). Damasio also suggested an additional aspect of self in that these proto-self systems then undergo ongoing correlative processing of these mappings with object mappings, with this taking place in thalamus, cingulate, and superior colliculus. This would potentially

yield the 'owner' of the movie. Panksepp thought that a primitive self structure resided in cross talk between periaqueductal gray, superior colliculus, and pontine and other brainstem motor systems.

A large part of the problem here is the high level of abstraction of these concepts, and the difficulty operationalizing them to do more rigorous empirical tests, but there is certainly face validity for the conceptualizations of both Panksepp and Damasio. There is also considerable trouble in the consciousness literature in the frequent conflating of numerous different meanings to the concept of self, some of them much more primitive, and some much more late arriving: autobiographical memory, groundedness in a body-centred coordinate system, the closely related notion of agency and ownership, self images that are consciously available for reflection and also unconsciously barred from reflection, permanently unconscious aspects of the brain's body mapping and regulation of homeostatic operations, etc., etc.

In the end, there is much similarity between these conceptions, in that this kind of primitive 'pre-conscious' self is generated by distributed processing of critical somatosensory, motor and emotional information, and in the cross talk between multiple brain systems, with brainstem components being particularly critical in my judgment. I feel drawn to the basic assumption that the cross talk must somehow involve sensory information, motor maps, goals and affective information, and body mapping in multiple modalities. I suspect that the most essential neurodynamic resonances for this are established in the brainstem regions that handle these kinds of mappings at fairly primitive and 'coarse-grained' dimensions, with cortex being a place where more detailed resonances and correlative mappings can happen. In other words, the primitive preconscious self establishes the ground out of which other integrations may emerge.

My own interests and writings of course have always gravitated toward the affective components to this complex synergistic blending, but certainly no piece of the composite can claim any kind of real hegemony. Consciousness seems to be generated in these seamless integrations that our brains achieve effortlessly, moment to moment. To be any part of the explosion of interest in this, in the (finally!) legitimate birthing of a scientific perspective on what someone once called 'the Ground of Being' is both stirring and a great privilege.

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