

# ‘Introspectionism’ and the mythical origins of scientific psychology

Alan Costall

*Department of Psychology, University of Portsmouth, Portsmouth, Hampshire PO1 2DY, UK*

Received 1 May 2006

---

## Abstract

According to the majority of the textbooks, the history of modern, scientific psychology can be tidily encapsulated in the following three stages. Scientific psychology began with a commitment to the study of mind, but based on the method of introspection. Watson rejected introspectionism as both unreliable and effete, and redefined psychology, instead, as the science of behaviour. The cognitive revolution, in turn, replaced the mind as the subject of study, and rejected both behaviourism and a reliance on introspection. This paper argues that all three stages of this history are largely mythical. Introspectionism was never a dominant movement within modern psychology, and the method of introspection never went away. Furthermore, this version of psychology’s history obscures some deep conceptual problems, not least surrounding the modern conception of “behaviour,” that continues to make the scientific study of consciousness seem so weird.  
© 2006 Elsevier Inc. All rights reserved.

*Keywords:* Introspection; Introspectionism; Behaviourism; Dualism; Watson; Wundt

---

## 1. Introduction

Probably the most immediate result of the acceptance of the behaviorist’s view will be the elimination of self-observation and of the introspective reports resulting from such a method. (Watson, 1913b, p. 428).

The problem of consciousness occupies an analogous position for cognitive psychology as the problem of language behavior does for behaviorism, namely, an unsolved anomaly within the domain of an approach. (Shallice, 1972, p. 383).

Psychology cherishes many favourite stories about what Ebbinghaus (1908, p. 3) referred to as its “short history.” Unfortunately, most of the more engaging ones are untrue. These include Watson’s rapid and long-lasting conditioning of a phobia in ‘Little Albert,’ Lloyd Morgan’s rejection of ‘anthropomorphism,’ and how the workers in the famous Hawthorne experiments kept increasing productivity, regardless of the experimental conditions, just because they thought the researchers and management were being so nice to

---

*E-mail address:* [alan.costall@port.ac.uk](mailto:alan.costall@port.ac.uk)

them (Bramel & Friend, 1981; Costall, 1993, 1998; Harris, 1979; Samelson, 1980). Psychologists are not alone in creating such myths, and the inaccuracies and outright inventions of ‘textbook histories’ are not just a question of carelessness. These fictional histories help convey the values of the discipline, and a sense of destiny (Brush, 1974; Kuhn, 1968; Samelson, 1974).

Some of the more important elements of psychology’s fictional history have coalesced into a comprehensive and highly persuasive myth about how scientific psychology came into being in the first place, and how it came to be how it is now. ‘Introspectionism’ plays a leading role in this fictional history.

Many introductory textbooks begin with the following historical sketch:

- (1) Psychology, as instituted in the universities, began as the study of mind, based, almost exclusively, on the method of introspection.
- (2) In reaction to the blatant unreliability of the introspective method, behaviourism then redefined psychology as the study of behaviour, based, primarily, on the objective method of experimentation.
- (3) In reaction to the limited research agenda and theoretical bankruptcy of behaviourism, the ‘cognitive revolution,’ in turn, restored the mind as the proper subject of psychology (but now with the benefit of the rigorous experimental and statistical methods developed within behaviourism).

This three-stage history takes the form of a thesis, antithesis, and synthesis. Here is an early example from an introductory textbook by Donald Hebb:

If Watson’s work is seen as [a] house-cleaning operation . . . , its importance becomes clearer. In the first place, he was right about rejecting introspection as a means of obtaining factual evidence; it is certainly true that one often knows much of what goes on in one’s own mind, but there is an element of inference in this knowledge that we do not yet understand clearly (i.e., it is not factual evidence) and little agreement can be obtained from introspective reports. In 1913 the whole case for mental processes seemed to depend on introspection; if it did, the case was a bad one, and “mind” had to be discarded from scientific consideration until better evidence could be found. . . Paradoxically, it was the denial of mental processes that put our knowledge of them on a firm foundation, and from this approach we have learned much more about the mind than was known when it was taken for granted more or less uncritically. (Hebb, 1966, p. 5–6).

Here is a more up-to-date example, from the thirteenth edition of *Hilgard’s introduction to psychology*:

Because psychologists were growing impatient with introspection, the new behaviorism caught on rapidly . . . . The modern cognitive perspective is in part a return to the cognitive roots of psychology and in part a reaction to the narrowness of behaviorism and the S–R view . . . . Like the 19th century version, the modern study of cognition is concerned with mental processes such as perceiving, remembering, reasoning, deciding, and problem solving. Unlike the 19th-century version, however, modern cognitivism is not based on introspection. Instead, it assumes (1) only by studying mental processes can we fully understand what organisms do, and (2) we can study mental processes in an objective fashion by focusing on specific behaviours (just as behaviorists do) but interpreting them in terms of underlying mental processes (Atkinson, Atkinson, Smith, Bem, & Nolen-Hoeksema, 2000, pp. 12–13).

This three-stage history is also to be found in the more specialized literature, as in John Anderson’s widely used textbook, *Cognitive psychology and its implications*:

Just because introspection proved to be unreliable did not mean that it was impossible to develop a theory of internal mental structure and process. It only meant that other methods were required. In physics, for example, a theory of atomic structure was developed, although that structure could only be inferred, not directly observed. But behaviorists argued that a theory of internal structure was not necessary to an understanding of human behavior, and in a sense they may have been right . . . . A theory of internal structure, however, makes understanding human beings much *easier*. The success of cognitive psychology during the later part of the twentieth century in analyzing complex intellectual processes testifies to the *utility* of postulating mental structures and processes. (Anderson, 2000, p. 10; emphases added)

Finally, here is an example of one of the major pioneers of the cognitive approach, George Miller, repeating the same basic story:

*The cognitive revolution in psychology was a counter-revolution.* The first revolution occurred much earlier when a group of experimental psychologists, influenced by Pavlov and other physiologists, proposed to redefine psychology as the science of behavior. They argued that mental events are not publicly observable. The only objective evidence available is, and must be, behavioral. By changing the subject to the study of behavior, psychology could become an objective science based on scientific laws of behavior. ... If scientific psychology were to succeed, mentalistic concepts would have to integrate and explain the behavioral data. (Miller, 2003, pp. 141–142; *emphasis added*)

There are several problems with this simplified version of our disciplinary history. First of all, an increasing number of psychologists are becoming dissatisfied with this account because they no longer regard the third stage of this history as the culmination it once had seemed. The ‘mind’ restored to science by cognitive psychology is coming to appear disappointingly elusive: it is largely unconscious, and perhaps nothing more than a convenient theoretical ‘postulate’ (as the references to “easier” and “utility” in the above quotation from Anderson, would seem to suggest). As Neisser has explained, he deliberately avoided any discussion of *consciousness* in his classic text, *Cognitive psychology* (1967), precisely because he could see that it would not be sufficient merely to treat consciousness as yet another “stage of processing in a mechanical flow of information” (Neisser, 1976, p. xiii).

For many, therefore, a *fourth* stage in psychology’s historical progress seems long overdue—the full-blooded return of consciousness as a proper subject for psychological research, through the reintroduction of first-person methodologies. The return of consciousness would then, many now believe, mark the true *culmination* of psychology’s destiny as the science of *mind*.

There are further problems with this three-stage history, in addition to simply stopping too soon. Many other significant fields of psychology are excluded. As a result, the textbooks, having rehearsed this history, have then to start all over again in order to include other important historical issues, such as the impact of Darwinian theory on early psychology, the rise of psychoanalysis, developmental psychology, and social psychology, and, indeed, ‘applied psychology,’ including mental testing, clinical and educational psychology.<sup>1</sup>

The really big problem with this three-stage history, however, is that it is wrong. The status of each of the three stages is largely mythical, and all this mythical history has helped conceal the dualistic assumptions behind both the behaviourist and cognitive ‘revolutions.’ As I shall be trying to show, the early debates about introspection as a method in psychology were a good deal more subtle and insightful, and also much less decisive, than the textbooks might have us believe. Those early debates identified fundamental issues that have been obscured behind all this phoney history. Yet these issues need to be remembered and resolved if modern psychology is to achieve any revolutionary break with behaviourism.

## 2. The cognitive revolution

Our emphasis was upon processes lying immediately behind action, but not with action itself. On the other hand, we did not consider ourselves introspective psychologists, at least not in the sense Wilhelm Wundt defined the term, yet we were willing to pay attention to what people told us about their ideas and their Plans. How does one characterize a position that seems to be such a mixture of elements usually considered incompatible? When we stopped laughing it suddenly occurred to us that we were *subjective behaviorists* (Miller, Galanter, & Pribram, 1960, p. 211).

<sup>1</sup> Several of the very recent textbooks either omit the long established historical introduction altogether, or include, instead, a section on the different “perspectives” within psychology, with the three-stage account nevertheless surviving as the only sustained historical narrative.

I will be approaching the ‘official’ three-stage history of psychology backwards, from the present to the past, since that is how fictionalized disciplinary histories are themselves constructed—as a justification and celebration of the *status quo*.

Cognitive psychology has for many years been presenting itself as the *revolutionary* alternative to mechanistic, stimulus–response behaviourism. Yet this revolutionary talk is difficult to reconcile not only with its acknowledgement of perhaps *too* many pioneers (such as, Bartlett, Piaget, Tolman, and the Gestalt psychologists), but also with the prior existence (seldom acknowledged) of an alternative “cognitive psychology” based, not on computer metaphors, but on physicalist and cybernetic models (see Dupuy, 2000; Costall, 2003a). More importantly, however, the new, supposedly revolutionary cognitivism has remained, in many fundamental respects, an extension of the traditional behaviourist framework it claims to have undermined (see also Costall, 2004; Costall & Still, 1991; Knapp, 1986; Leahey, 1992). And this matters a great deal, because cognitive psychology is no longer a circumscribed field of research, but a general approach that has come to dominate mainstream psychology:

Friendship has become social cognition, affect is seen as a form of problem-solving, new-born perception is subsumed under a set of transforming rules, and psychoanalysis is reread as a variant of information processing. Cognition, the feeble infant of the late Fifties and early Sixties, has become an apparently insatiable giant. (Kessen, 1981, p. 168).

Many of the pioneering figures in the new cognitive movement did start out with radical intentions. Their early writings still convey a vivid sense of intellectual excitement, if now also poignancy. For, as many of the early pioneers insist, mainstream cognitive psychology lost its way (Bruner, 1988, 1990; Neisser, 1997; Broadbent, 1980). The cognitive psychologists themselves quickly became caught up in psychology’s persistent anxiety about method. They “stopped looking at the world too early and tried to be scientists . . . they got precision at the cost of validity” (Jenkins, 1986, p. 254).<sup>2</sup> Despite great hopes, the new movement failed to create an effective challenge to the methodological imperatives already established by the behaviourists. These include the stimulus–response formula, the hypothetico-deductive method, and a commitment not simply to methodological behaviourism but also to a thoroughly dualistic conception of *behaviour*.<sup>3</sup>

(1) *Commitment to stimulus–response psychology*. Far from replacing the ‘stimulus–response’ scheme of Watsonian and neo-behaviourism, cognitive theory has largely been an elaboration of that scheme. The textbooks and the more advanced literature are all agreed that the proper ‘task’ of psychology—one supposedly rejected by behaviourism—is to explain what intervenes between the so-called stimulus and response. Thus, Kihlstrom, having repeated the usual story about the limitations of Wundt’s introspectionism and its replacement by behaviorism, goes on:

Beginning in the 1950s . . . psychology abandoned a radically behaviorist point of view in what has since come to be known as the “cognitive revolution.” Cognitive psychology comes in various forms, but all share an abiding interest in describing the mental structures and processes that *link environmental stimuli to organic responses* and underly human experience, thought, and action (Kihlstrom, 1987, p. 1445 *emphasis added*).

In fact, cognitivism is no more than the “flip-side” of stimulus–response behaviourism, where “mental processes” are defined as whatever “is left over after one tries to stuff all psychological phenomena into the S–R box” (Reed, 1997a, p. 267). It is not even the case that cognitive psychology’s *mediational* version of S–R behaviourism is new. That kind of behaviourism was well established by the 1930s and 1940s (e.g. Hull, 1943).

<sup>2</sup> Along with James Jenkins, several contributors to Baars’ steadfast celebration of the supposed revolution expressed *deep* misgivings about the current state of cognitive psychology (Baars, 1986). To judge from his book and his later comments on the triumphs of cognitivism, Baars either failed to notice what they were telling him or refused to believe them (see also Baars & McGovern, 1994).

<sup>3</sup> A striking example of this unquestioning commitment to the standard methodology is the way that American and British developmental psychologists turned to Piaget’s work as a refreshing alternative to the standard approaches to child development, but then became totally obsessed with the issue of whether his claims could stand up to the long established standards of experimental psychology (see Costall & Leudar, 2004).

The revolutionary rhetoric about the overthrow of behaviourism has helped conceal the widespread retention of this amended form of stimulus–response theory—and *methodology*—within modern psychology. Relatively recent changes in terminology have also served as an effective distraction: the replacement of the terms ‘stimulus’ and ‘response’ not only by the computer jargon of ‘input’ and ‘output,’ but also by the language of ‘independent’ and ‘dependent’ variables. As Winston (2004) has explained, this terminology of ‘variables’ is of recent origin within science and is almost unique to psychology, having been initiated by R. S. Woodworth from the 1930s onwards. Despite its apparent theoretical neutrality, this talk of variables is also theoretically loaded, since, as Danziger (1997, p. 171) has put it, “analysis in terms of variables has become a way of eliminating questions of meaning from the explanation of human conduct.”

(2) *The hypothetico-deductive method.* Cognitive psychology also retains an enthusiastic commitment to the hypothetico-deductive methodology developed with the neo-behaviourism of the 1940s, leading to the same introverted research that used to characterize ‘learning theory.’ Neisser has been pointing out the futility of this approach for some time:

The activity that dominates cognitive psychology today is not empirical exploration but something quite different: namely, the making and testing of hypothetical models. . . . research should always begin with a theory; not just any theory, but a specific model of the internal processes that underlie the behavior of interest. That mental model is then tested as thoroughly as possible in carefully designed experimental paradigms. . . . The aim of the research is not to discover any secret of nature; it is to devise models that fit a certain range of laboratory data better than their competitors do. (Neisser, 1997, p. 248).

(3) *Methodological behaviourism.* The most important, though largely unacknowledged, legacy of behaviourism is the profound *disjunction* of *mind* and *behaviour*. As Harnad has approvingly noted, cognitive psychologists “are really just a species of behaviourist. They know (or ought to know) that their *data* can still only be what the organism (or machine) *does*” (Harnad, 1985, p. 901). Unfortunately, given the dualistic premises common to classical behaviourism and modern ‘cognitive behaviourism,’ the connection between the data supposed to be available to the psychologist and the supposedly inaccessible realm of mind is remarkably shaky. The behaviourists’ objectivist conception of “behaviour” is maintained within mainstream cognitive psychology, and this leads to an extremely tenuous connection, therefore, between the available ‘behavioural data’ and claims about mind.

To be blunt, modern cognitive theory is mired in dualism. Now, most cognitive psychologists would, of course, be puzzled, indeed outraged, by such an accusation. They would protest that they are not committed to an ontological dualism of mind, on the one hand, and body and behaviour, on the other. Yet, they, nevertheless, keep backing themselves into epistemological and methodological dualisms because of their retention of a Watsonian conception of behaviour. This conception of behaviour not only entails the assumption that our understanding of other people’s intentions, feelings, and so forth, can only be based on *inferences*, but also—given the assumed *logical disconnection* between behaviour, and intentions, etc.—such inferences lack *any* premises.<sup>4</sup>

Curiously, this problem of bridging the gulf between the supposedly available ‘data’ and knowing about other minds is recognized, even relished, by cognitive psychologists when they are talking about how ‘people’ (i.e. *non-psychologists*) make sense of one another. As Alan Leslie (1987, p. 422), one of the main instigators of the Theory of Mind approach, has put it: “It is hard to see how perceptual evidence could force an adult,

<sup>4</sup> The still dominant computer metaphor of cognitive theory continues to be widely regarded as a serious challenge to dualism since “brain and mind are *bound* together as computer and program,” or hardware and software (Johnson-Laird, 1988, p. 23; emphasis added). But the metaphor proves to constitute a perverse kind of reaction, and a strange kind of bond. How is it that cognitivists can also claim that theirs is “a science of structure and function divorced from material substance” (Pylyshyn, 1986, p. 68)? They have been so enthralled by the software or program aspect of the computer metaphor, that they have hardly bothered to spell out what precisely the *hardware* is supposed to represent, not least, whether it refers to the mind, the brain, or the body. Either way, this hardware, when it is not completely lost in thought, is no more than a stimulus–response interface. Certainly, some theorists have invoked aspects of the hardware as part of the computer metaphor, such as the central processing unit, memory stores, and buffers. Yet it is the *ideal* of a computer as a ‘general purpose machine’ that formally underpins the supposed separability of software and hardware. And, according to this ideal, the hardware (as mind, brain or body) can have no explanatory relevance at all (see Costall, 1991a).

let alone a young child, to invent the idea of unobservable mental states.” Yet, although this mysterious gulf between observable behaviour and unobservable mental state is presented as an intriguing and fundamental research ‘problem’ *within* psychology, there is seldom any serious discussion of the methodological implications for how psychologists themselves are supposed to be able to deal with this same, supposedly awesome gulf (see Costall & Leudar, 2004; Leudar & Costall, 2004). Yet the profound chasm presupposed by cognitivist metatheory, between what we can *directly* know about another person (their “behaviour”) and that person’s intentions, feelings, and innermost thoughts, has to exist just as much for the *psychologist* as for everyone else. However, as I shall now try to explain, this stark antithesis of behaviour and ‘mind’ within psychological theory is recent, and, in large part, a legacy of Watson’s reaction against ‘introspectionism.’

### 3. The behaviourist revolution

What, if anything, was there really for Watson (1913a, 1913b) to overthrow in his behaviourist manifestos, when he insisted that psychology should reject the method of introspection and become the science of behaviour? According to most contemporary commentators, very little:

Behaviourism, in 1912–1914, was a ‘youth movement.’ Watson was a young man, and his followers were mostly in the younger generation . . . In their enthusiasm they exaggerated the revolution . . . The actual revolution in psychological research was slight. Objective work continued; introspective work continued. (Woodworth, 1931, p. 62)

Watson’s critics were well aware that “objective work” was well established within experimental psychology long before Watson decided to stir things up, such as the research on reaction times, psychophysics, and remembering (O’Donnell, 1985). More than thirty years before Watson’s first behaviourist manifesto, John Dewey had already stressed the important new direction taken by the “New Psychology”: in the light of Darwinian theory, there was now an insistence upon “the impossibility of considering psychological life as an individual, isolated thing developing in a vacuum,” and there was also a new *method*—experimentation now “*supplemented and corrected* the old method of introspection” (Dewey, 1884, pp. 285 and 282; *emphasis added*).

It is simply not the case, therefore, that the psychology of the late nineteenth and early twentieth centuries was dominated by introspectionism. For example, Scripture’s (Scripture, 1895/1907) very successful textbook, *Thinking, feeling, doing: An introduction to mental science*, first published in 1895, makes very little reference to introspection, and is full of highly technical studies of people and animals *doing* different (and sometimes rather unusual) kinds of things, including people conducting music, preparing to throw a punch, and setting off on a sprint, and even a dog settling down to think (see Figs. 1–4). You can see why Seashore (1930, p. 247), a one-time student of Scripture, complained that the research “seemed nearer to telegraphy than psychology.”

When Watson declared the *impending* behaviourist revolution, he could point to the *existing* animal research as a model of good scientific practice. However, as many of Watson’s critics kept insisting, objective (i.e. non-introspective) methods were already well established in human experimental psychology as well. And Watson himself, despite his *revolutionary* claims, was perfectly happy to draw attention to a large range of other “flourishing” areas of human psychology where introspection was, again, hardly an issue:

What gives me hope that the behaviorist’s position is a defensible one is the fact that those branches of psychology which have already partially withdrawn from the parent, experimental psychology, and which are consequently less dependent upon introspection, are today in a most flourishing condition. Experimental pedagogy, the psychology of drugs, the psychology of advertising, legal psychology, the psychology of tests, and psychopathology are all vigorous growths. (Watson, 1913a, p. 169)

Samelson (1981), in a detailed discussion of Watson’s promotion of behaviourism stressed the lack of substance behind Watson’s revolutionary programme. At the outset, there was not even any new, distinctive human experimental research to serve as an exemplar of the new research movement. Nevertheless, according to Samelson, Watson did bring about a radical reversal in what psychologists came to regard as *direct* evidence:

In the earlier phase we find again and again the statement that the introspective method constitutes direct and immediate contact with the subject matter, while what we now mean by objective observation

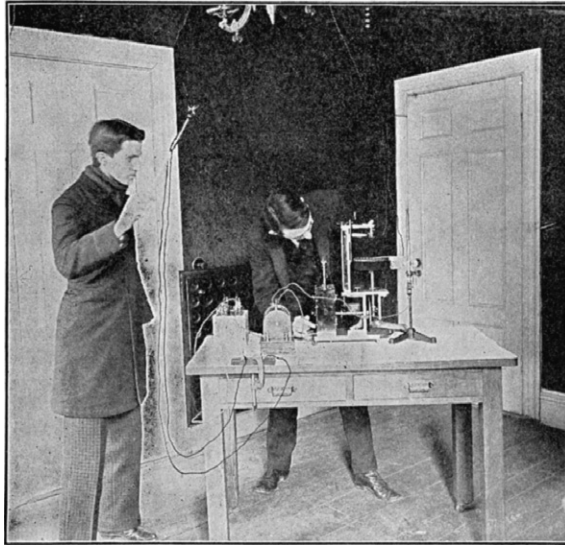


Fig. 1. Taking an orchestra leader's record with the electric baton.

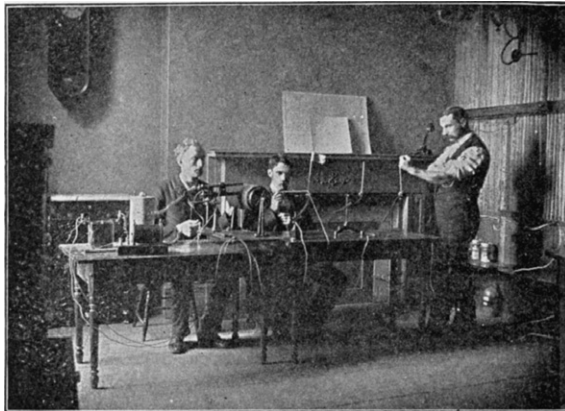


Fig. 2. Measuring how rapidly a pugulist thinks and acts.

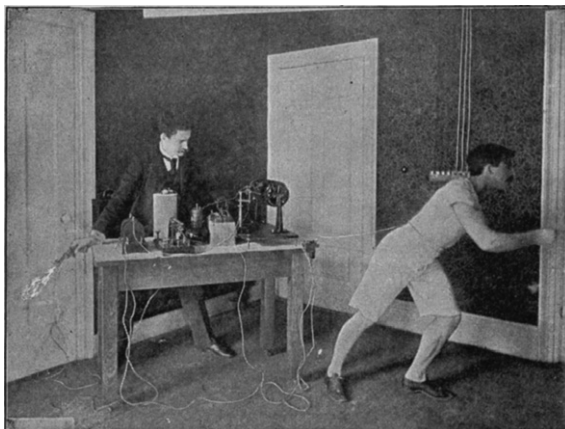


Fig. 3. Measuring a runners reaction time.

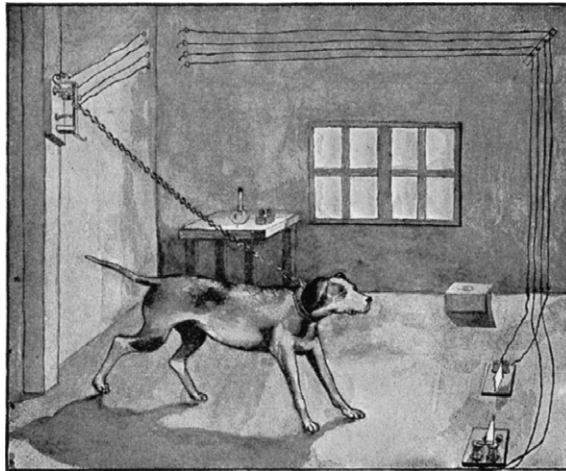


Fig. 4. Measuring how fast a dog thinks.

was then only an indirect or mediate one.<sup>5</sup> After the revolution, the meanings are reversed: objective observation is the direct contact, while information obtained through introspection, if not altogether impossible or irrelevant, is at best indirect, a tenuous base for fragile inferences from questionable verbal reports. (Samelson, 1981, p. 415).<sup>6</sup>

Samelson's claim is puzzling. First of all, concerning the initial status of introspective data, it is clear that many of the committed proponents of the method were already well aware of its limitations, and were going to great lengths to refine the conditions of testing, and training the subjects. Furthermore, there were theoretical developments within psychology that had already set substantial limits on the scope and apparent 'transparency' of introspection as a method.

Among these was the issue of the unconscious. The research on hypnosis, multiple personality, and other altered states of consciousness (well underway before Freud's appearance on the scene) was enough, in itself, to challenge the transparency of mind to the introspective gaze:

...conscious thought is merely the surface foam of a sea where the real currents are well beneath the surface. It is an error, then, to suppose that the 'secret behind' a man's actions lies in those thoughts which he (and he alone) can 'introspectively' survey. (Holt, 1915, p. 88).

The question of the unconscious, and its implications for introspection, had, in fact, already arisen some time before in nineteenth-century perceptual theory with Helmholtz's influential claim that perceiving is a process of unconscious inference, and hence could not be directly examined. As Helmholtz himself put it, "in this case we are concerned with mental operations about which introspection is utterly silent and whose existence is to be inferred, rather, from physiological investigations of the organs of sense" (Helmholtz, translated in Meyering, 1989, p. 182).<sup>7</sup>

Furthermore, there were also radical developments in developmental and social psychology that challenged the primacy of reflective self-consciousness, and the self-contained ego of Cartesian theory. According to the

<sup>5</sup> At this point, Samelson cites Angell (1906, p. 4) and Carr (1926, p. 7) as examples.

<sup>6</sup> In an account of the remarkably slow acceptance of Watsonian behaviourism, Samelson (1985) presents a much more nuanced account of Watson's impact.

<sup>7</sup> Both Meyering (1989) and Reed (1997b) have argued that it was Helmholtz's insistence on the unconscious status of mental processes that provided the basis of modern psychology. Meyering celebrates this move, Reed deplored it. Following Helmholtz's lead, cognitive psychologists have managed to conjure up a whole universe of "unconscious psychological processes," thus rendering, it would seem, the method of introspection as very limited in scope. However, the existence of these "processes" remains, in the end, a matter of theoretical faith.



social psychologists, such as Baldwin, Cooley, Dewey, and Mead, the individual self, far from constituting the starting part of psychology, had to be understood as a social construction:

The individual is found to be a social product, a complex result, having its genetic conditions in actual social life. . . . *It is now the problem to find any knowledge that is psychologically private, not to find knowledge that is common and public.* . . . The knower . . . starts with what his and his neighbor's experience in common verify, and only partially and by degrees does he find himself and prove himself to be a relatively competent independent thinker. (Baldwin, 1909, p. 211; emphasis added).

Here is John Dewey also stressing the *derivative* nature of 'privacy' but this time with specific reference to introspection:

When the introspectionist thinks he has withdrawn into a wholly private realm of events disparate in kind from other events, made out of mental stuff, he is only turning his attention to his own soliloquy. And soliloquy is the product and reflex of converse with others . . . . If we had not talked with others and they with us, we should never talk to and with ourselves. (Dewey, 1925/1958, p. 170).

So, contrary to the first part of Samelson's claim about Watson's true revolutionary status, there was plenty already going on in psychology to raise serious doubts about the traditional Cartesian view of introspection as direct and incorrigible.

Turning now to Samelson's second claim about the new status of *behaviour* (as opposed to introspection) as *direct* evidence, it is certainly true that Watson, having regarded behaviour as impossibly *indirect* evidence of mind and consciousness, opted for behaviour, instead, as the subject matter of psychology. But it was not long before the majority of the behaviourists were treating behaviour as *indirect* evidence about *internal structures*, real or hypothesized, that were supposed to 'generate' the observed behaviour. And by the time cognitive psychologists were beginning to make inferences about *mind* from behaviour, their data constituted very indirect evidence indeed. For despite the remarkable ambivalence of Watson and his followers about what they meant by 'behaviour,' the official scientific line was that it was nothing but "colourless movement" (Hull, 1943, p. 25).

Harvey Carr, one of Watson's teachers, was one of the many critics who had challenged Watson's claim that behaviour had never previously been the focus of study within American psychology. He insisted, therefore, that 'objectivism' was a more appropriate term than 'behaviourism,' since the essence of Watson's approach was "not a distinction of subject matter (behavior) but the objective view from which it is studied" (Carr, 1915, p. 309).

Daston (1992) has argued that the modern scientific ideal of objectivity as "aperspectival" is relatively new. It only became influential during the nineteenth century, when science was becoming highly international and conducted on an almost industrial scale, and the relationships among scientists themselves were becoming more distant and impersonal:

Aperspectival objectivity was the ethos of the interchangeable and therefore featureless observer—unmarked by nationality, by sensory dullness or acuity, by training or tradition, by quirky apparatus, by colourful writing style, or by any other idiosyncrasy that might interfere with the communication, comparison and accumulation of results. (Daston, 1992, p. 609).<sup>8</sup>

The ideal of standardization was central to the debates about the unreliability of introspective psychology. Not only did the method of introspection seem to defy standardization, but the very idea of 'aperspectival *introspection*' surely sounds like a serious contradiction in terms. Yet, it is important to remember that stan-

<sup>8</sup> In the case of psychology, at least, this retreat into objectivity was surely also linked to other concerns. Bakan has identified several important ones, including the experience of living with strangers in the new big industrial cities (Bakan, 1966), which Watson himself stressed, e.g. Watson (1924, p. xi), and also religion, which Watson certainly did *not*: "The Protestant ethic was associated with an intense psychological separation of individual from individual. It had a theology which suggested that the thoughts, feelings, and wishes of each individual were a matter between himself and God alone, and not a matter for another man to concern himself with. It tended to substitute formal and contractual forms of relationship for intimate interpsychic contact. A too great interest in the inner life of another person not only exceeded the bounds of formal relationship, but was also a reminder of the odious Confessional of the Catholic Church. At the same time the Protestant ethic was associated with a vaulting thrust to master the world through industry and through science." (Bakan, 1969, p. 39).

andardization became just as much an issue within the study of *behaviour*. For, if we are to understand behaviour as both meaningful and historically situated, then we remain in the realm of hermeneutics. Watson's escape from this problem was, in effect, to 'depsychologize' behaviour.

It is no longer easy for most modern psychologists to understand what really happened. The textbook histories are no help, since they do not challenge Watson's account of the introspectionist psychology he claimed he was displacing, so that something along the lines of his behaviourism seems a reasonable first step, at least, towards something better. However, there was also a profound transformation in the meaning of 'behaviour,' underway before Watson entered the scene, that eventually gained currency well beyond the confines of experimental psychology.

Until the late nineteenth century, the term 'behaviour' was applied primarily to people, and how they conducted themselves in public in a proper and orderly way (see Ardener, 1973; Williams, 1983, pp. 43–45; see also Leahey, 1993, on the wider cultural changes involved in this shift of meaning). This normative, moral sense still persists in relation to the word 'misbehaviour' and the injunction 'behave!'—which is hardly a command just to 'do' anything! In the nineteenth century, 'behaviour' came to be used in popular scientific writings in a metaphorical way to convey the idea that even physical processes, such as chemical reactions and planetary motions, also proceed in an orderly fashion.

The extension of 'behaviour' to animals occurred mainly in the context of experimental research designed to demonstrate that animals were not really as intelligent as they might first appear.<sup>9</sup> As a result, instead of 'behaviour' conferring dignity upon animals, the term itself became further debased. It continued to refer to activity that was public, and hence observable, and rule-based, *but now with reference to causal laws* rather than social conventions and expectations. In this way, 'behaviour' eventually became largely synonymous in mainstream psychology with physical movement, and hence applicable, indifferently, to objects, animals, and people.

On the face of it, Watson's own conception would seem to be extremely inclusive: "anything the organism does—such as turning toward or away from the light, jumping at a sound, and more highly organized activities such as building a skyscraper, drawing plans, having babies, writing books, and the like" (Watson, 1930, p. 6; see Kitchener, 1977, for an extensive account of the different and often ambiguous meanings of, 'behaviour' in behaviourist theories). The problem is that Watson was not just using polemics to promote his career, but making a career out of polemics. And with his "blunderbuss polemical methods" (Harrell & Harrison, 1938, p. 402) he was reacting against almost *everyone*, not just the 'introspectionists' in relation to consciousness, but also the pragmatists, the functionalist psychologists, and Gestaltists with their emphasis upon purpose and meaning. His conception of behaviour was thus set up in opposition not only to consciousness, but also to *purpose, meaning, and value*.<sup>10</sup>

I have no sympathy with those psychologists and philosophers who try to introduce a concept of "meaning" ("values" is another sacred word) into behavior. (Watson, 1920, p. 103; see also, Watson, 1930, p. 1).<sup>11</sup>

<sup>9</sup> Lloyd Morgan's *Animal Behaviour*, published in 1900, is probably the very first book with such a title, and its title must have had the kind of shock effect on contemporary readers that, say, talk of 'chimpanzee politics' has now. In fact, Lloyd Morgan devoted several pages of the introduction of his book to the different current meanings of the term "behaviour" in an attempt to justify the unusual title of his book (Costall, 1998). When Maurice Parmelee published his behaviourist text, *The science of human behavior* (1921) he too saw the need to provide at least a brief explanation of the ambiguity of the term 'behaviour': "The word 'behavior' has a variety of meanings. Sometimes it is used in a very limited sense to refer to matters of deportment and etiquette. Sometimes it is used in a very broad sense to refer to the mode of acting of not only living beings, but also inanimate things such as an engine or a waterfall. *Recently, however, there has been growing up a so-called "science of animal behavior" which has given to the word a very definite meaning* (Parmelee, 1921, p. 1; emphasis added)."

<sup>10</sup> Gestalt psychology and the "so-called functional psychology" were, as Watson (in his usual diplomatic way) put it, "illegitimate children of introspective psychology. Functional psychology, which one rarely hears of now, owed its vogue to considerable patter about the physiologically adaptive functions of the mind. The mind with them is a kind of adjusting guardian angel (Watson, 1930, p. 1)."

<sup>11</sup> In contrast to Watson, the pragmatists and functionalist psychologists continued to use the term behaviour in its original sense, and thus interchangeably with action and conduct as implying orientation to a goal. As Morris (1970, p. 43) himself has pointed out, the classic paper by John Dewey (1896) on the "The Reflex Arc Concept in Psychology," published some time before Watson's manifestoes, was, in effect, a rejection of what Watsonian behaviourism eventually came to stand for.

Many of Watson's early critics readily detected the blatant dualism behind his apparently no-nonsense, materialist psychology:

... in so far as behaviorists tend to ignore the social qualities of behavior, they are perpetuating exactly the tradition against which they are nominally protesting. To conceive behavior exclusively in terms of the changes going on within an organism physically separate in space from other organisms is to continue the conception of mind which Professor Perry has well termed "subcutaneous". This conception is appropriate to the theory of existence of a field or stream of consciousness that is private by its very nature; it is the essence of such a theory. (Dewey, 1914/1977, p. 445).<sup>12</sup>

Edwin Holt's objection to the Watsonian behaviourists was shorter but also very much to the point: "They are to-day in danger of making the materialist's error, of denying the *facts*, as well as the theory, of consciousness" (Holt, 1915, p. 206).

#### 4. 'Introspectionism'

We finally come to 'introspectionism,' the first stage in psychology's three-stage origin myth. I put the term 'introspectionism' in scare quotes because, as a supposedly dominant, psychological movement, it is largely Watson's own invention. Nevertheless, the term is useful in distinguishing *introspection* as a method from the real or imagined historical contexts and psychological and philosophical assumptions that have been associated with its deployment.

In relation to experimental psychology, the systematic use of introspection was an early twentieth-century development promoted by, among others, Külpe and Marbe, in Germany, Binet in France, and Titchener in America. It was, therefore, in relation to Watsonian behaviourism, a relatively recent development, and neither a long established nor a dominant paradigm. American experimental psychology had been previously based largely on "objective" studies of reaction time, psychophysics and memory, and continued to be so. Presumably, because he had failed to convince his American peers that introspectionism was rampant all around them, Watson came to characterize this approach, instead, as an essentially alien, *un*American activity stemming from Leipzig. As Watson's student, Mary Cover Jones (1974, p. 582) put it, Watsonian behaviorism was exciting to the younger generation because it "shook the foundations of traditional European-bred psychology."

Interestingly, I have managed to find very few references to Wilhelm Wundt in the early publications relating to introspection and behaviourism (e.g. Cattell, 1904, p. 597; Thorndike, 1915, p. 463). Watson makes no mention of Wundt in his first article on behaviourism (Watson, 1913a). There are some brief references to him in Watson's second paper on behaviourism (Watson, 1913b, p. 425) and in his first book, *Behavior: an introduction to comparative psychology* (1914, p. 21), but these references concern a rather specific point about Wundt's theory of affection. Only later did Watson decide that Wundt was going to be the one to take the blame for being the original perpetrator of introspectionism, and also, along with William James, one of its main exponents:

<sup>12</sup> Similar points were raised by critics from quite different backgrounds, such as Judd, perhaps Wundt's most loyal American student, and the Soviet psychologist, Rubinstein:

I think the writers who have recently used the word "behaviorism" to express their spleen against introspection, have run away with a word to which they have absolutely no right. The plain implication of the word behaviorism is that there is in the world something directly antithetical to behavior which needs to be banished from psychology. The psychologist most completely devoted to introspection does not for a moment overlook behavior. The pages of all the early introspectionists confirm this statement. (C. H. Judd, cited in Roback, 1964, p. 249)

The behaviorist argument against consciousness [Watson, 1914] is based on the introspectionist view that one must either accept the data of consciousness completely or exclude them completely; one cannot change the concept of consciousness. On the basis of this—introspective—understanding of the psyche, combining idealism and mechanism, behaviorism reduces man to the set of his responses to the environmental stimuli. (Rubinstein, 1937/1987, p. 13).

They maintain that the starting point in psychology is the study of one's own mind. You are supposed somehow to halt from moment to moment your ordinary daily activities and to analyze the accompanying "mental states" in terms of 'sensations,' 'images,' and 'the affective tones' present. (Watson, 1924, p. xii).<sup>13</sup>

Now there has been plenty of material available to English readers about Wundt to correct the over-simplified characterization of him as an introspectionist. Several of his books have long been available in English translation; there are several well informed early accounts of his work (e.g. Judd, 1897; Heidebreder, 1933; Flugel, 1951; Humphrey, 1951; Klein, 1970); and there are also a large number of more recent, revisionist historical treatments of Wundt, many occasioned by (and often undermining) the centenary celebrations in 1979 of his 'founding' of experimental psychology (e.g. Blumenthal, 1975, 1979, 1985, 2001; Danziger, 1979, 1980a, 1980b).

As Heidebreder (1933, pp. 93–94) long ago pointed out, Wundt denied that the experimental method had wide application within psychology (e.g. Wundt, 1897, p. 23). Furthermore, his conception of the "psychological experiment" was radically different from the ideal of experimentation that has eventually come to dominate psychology, not least regarding the epistemological status of the 'subject' and the power relation between subject and experimenter (Danziger, 1985). The 'subject,' in the Wundtian experiment, was not 'naive' but highly trained, and also fully knowledgeable about the real point of the research. So, to the extent that Wundt was the father of experimental psychology, his offspring was *not* experimental psychology as we now know it.

In fact, Wundt's main interest eventually turned to a cultural psychology<sup>14</sup> employing the methods of history and anthropology (see Ermarth, 1978; Judd, 1932). According to Wundt (1916, p. 3), the subject of this cultural psychology would be those "mental products . . . created by a community of human life" and hence could not be accountable in terms of "merely individual consciousness, since they presuppose the reciprocal action of many." Wundt (1916, p. 3) makes it very clear that he regarded introspection as having limited value within cultural psychology, since "Individual consciousness is wholly incapable of giving us a history of the development of human thought, for it is conditioned by an earlier history concerning which it cannot itself give us any knowledge."<sup>15</sup>

Most remarkably, as the supposed founder of introspectionism, Wundt also had serious reservations about the method of introspection, in its more elaborate forms, even within *experimental* psychology, "Introspective method [*introspektive Methode*] relies either on arbitrary observations that go astray or on a withdrawal to a lonely sitting room where it becomes lost in self-absorption" (Wundt, 1900, p. 180, translated in Blumenthal, 2001, p. 125). In fact, he was one of the harshest critics of the introspectionist research conducted by two of his most famous students, Titchener and Külpe (see Wundt, 1907; Humphrey, 1951; Blumenthal, 2001).

Danziger has examined the research conducted in Wundt's laboratory and concluded that Wundt relied primarily on objective methods, mainly time measurements and straightforward qualitative judgments of presented stimuli. According to Danziger, "introspection," as such, was seldom used, and then in the following limited ways: "(a) Attempts to explain individual differences in the objective data, which was of course a matter of no systematic interest in Wundt's laboratory; (b) checks on the effectiveness of experimental manipulations, e.g. in regard to levels of attention" (Danziger, 1980b, p. 115; see also Blumenthal, 1985, p. 31).

In short, the textbook accounts of Wundt are misleading. But it would be wrong, in the light of all this revisionist history, simply to opt for an alternative caricature. First of all, although Wundt set limits on the scope of experimental psychology, there is little reason to suppose that he entirely rejected the value of

<sup>13</sup> Titchener made a big point of stressing Wundt's founding role. Although this was an act of homage, it was not necessarily an entirely selfless act. Until Wundt made himself unpopular in America because of his support for the German cause during the War, he was an influential figure, and so could bestow credibility on Titchener's own introspective psychology. Many of the leading American psychologists at the time had, along with Titchener, studied with Wundt. Edwin Boring, Titchener's student, and eventually American psychology's 'official' historian, later presented Titchener's version of Wundt as definite historical fact,—even though (as Boring himself put it) "when Titchener wrote about Wundt, he seemed to be writing about *himself*" (Boring, 1927, p. 504; emphasis added).

<sup>14</sup> I have adopted Robinson's translation of Wundt's term, *Völkerpsychologie* (Robinson, 1982, p. 147).

<sup>15</sup> Greenwood (2003) has recently pointed out that Wundt's arguments against the use of introspection within cultural psychology primarily related to the study of historical change, occurring over long periods of time, involving not only many different people but different generations. Wundt, Greenwood argues, probably would have had no similar objection to the use of introspection to help understand the local interactions between individuals that sustain and transform cultural traditions.

experimental studies in relation to either thinking or social psychology (see Moustgaard, 1990, p. 63; Greenwood, 2003). Furthermore, despite his rejection of the introspective methods developed by Titchener and Külpe, Wundt did not reject the use of introspection in the sense of self-report or subjective report. He regarded it as entirely appropriate to obtain experiential reports under carefully controlled, imposed conditions:

By the objective method, I have never meant a method that was objective in the sense that it excluded self-observation. To demand such a method in psychology, would be tantamount to demanding my esteem for nonsense” (Wundt, 1887, p. 304, translated in Moustgaard, 1990, p. 64).

Why has the myth of Wundt as the arch-introspectionist persisted, not only in the introductory textbooks (see Brock, 1993, for a review), but also the advanced literature, including many of the recent publications on consciousness (e.g. Adams, 2000; Blackmore, in press, p. 37; Dennett, 1991, p. 44; Güzeldere, 1997, p. 13; Kihlstrom, 1987, p. 1445; Hooker, 1996, p. 184; Leahey, 1984; Lundin, 1984; Rosenthal, 1998; Varela, 1996; Varela, Thompson, & Rosch, 1991; Vermersch, 1999; Weidman, 1999, pp. 11 and 33). If one of the roles of fictional history, including ‘father figures,’ is to socialize and inspire new members of a discipline, why misrepresent a founding father in such an unflattering way: as someone foolish enough to establish the new science of psychology with what the textbooks insist was such an obviously hopeless and eventually abandoned methodology? In Watson’s case, the answer is simple: he was intent upon distancing himself from almost everyone else in the discipline, and discrediting them as hopelessly subjective in their methods, or effete, or both. But why did the cognitive psychologists, despite their apparent disdain for behaviourism, take the self-serving history of the behaviourists for granted, merely to add their own self-serving appendix? I think it reflects a fear of acknowledging the fact that psychology, and science in general, is a *human* enterprise. A serious understanding of the *historicity* of psychology as a scientific discipline (see Connelly & Costall, 2000) would expose the unresolved issue of subjectivity behind the modern conceptions of both mind and behaviour, and indeed behind the project of science itself. Cognitive psychology shares mainstream behaviourism’s terror of subjectivity—at least in relation to *method*. Yet, even though the issue of subjectivity can be very effectively concealed, it will not go away. Even Watson, despite all his fear and loathing, had to face the fact that consciousness is, after all, “the instrument or tool with which all scientists work” (Watson, 1914, p. 176).

## 5. Guilt by association: Introspection vs Introspectionism

Having misrepresented Wundt as the founder of ‘introspectionism,’ the textbooks go on to present accounts of its demise that are both dishonest and glib. They are dishonest because they fail to mention that introspection as a technique has continued to be used effectively in psychology (and beyond) in a variety of guises. And they are glib because most of them focus on the single issue of unreliability, and treat even that in a remarkably superficial way. In fact, the issue of unreliability was by no means as straightforward or decisive as the textbooks imply, and it was not the only reason that introspection became such a contentious matter in the early part of the twentieth century. The method of introspection was caught up in a number of wider agendas, and, as I shall now try to show, largely a victim of guilt by association.

### 5.1. *The reliability and validity of introspection*

There have been several principled objections to the use of introspection, many of which had been expressed long before Watson’s own critique. There was Comte’s argument that introspection requires us to split ourselves in two, as both knower and known (see James, 1890, Vol. 1, p. 188). (It seems that Comte, the dogmatic founder of positivism, never found himself in two minds about anything.) Then there was the moral and political objection, voiced by Dilthey, that, in retreating into ourselves, we are denying our situation within the world and within history, where any true understanding about ourselves is to be found (see Ermarth, 1978).

The most influential ‘in principle’ argument against introspection has a long history. The very act of introspection would distort its object. Here is an early statement of the problem by the Scottish philosopher, David Hume. His proposed alternative might easily be mistaken for an anticipation of Watsonian behaviourism, if we were to forget the profound transformation undergone in the meaning of “behaviour” since Hume’s time:

... 'tis evident this reflection ... would so disturb the operation of my natural principles as must render it impossible to form any just conclusion from the phenomenon. We must, therefore, glean up our experiments in this science from a cautious observation of human life, and take them as they appear in the common course of the world, by men's behaviour in company, in affairs, and in their pleasures. (Hume, 1739-40/1969, p. 46).<sup>16</sup>

Catching fleeting mental processes “on the wing,” as William James put it, does seem “like seizing a spinning top to catch its motion, or trying to turn up the gas quickly enough to see how the darkness looks” (James, 1884, pp. 14 and 3). Instead of trying to freeze such fleeting events, an alternative would seem to be to try to recall them after they had occurred. This was the strategy adopted in the early twentieth century, by the researchers at Würzburg in their studies of imageless thought. Yet, their “method of retrospection,” in addition to the usual problems of conflicting reports, also became subject to serious principled criticism. The Belgian psychologist, Albert Michotte (1907), who had been using the method himself, argued that the *failure* of observers to report images or sensations retrospectively could hardly be taken as conclusive: absence of evidence is not evidence of absence. Michotte lost all faith in the method and for several years turned to quite different areas of research (see Costall, 1991a, 1991b, 2003b; Michotte, 1954/1991).

The textbook histories usually just mention the fact that introspection yielded inconsistent results, and then conclude, on this basis alone, that the method was clearly invalid. Yet, even by the standards of fictionalized, disciplinary history, this is not good enough. Failures of agreement among researchers are hardly unique to introspection or to the discipline of psychology, and do not necessarily imply a lack of scientific rigour or honesty, or even an eventual “dead-end.” Observation in science, as William James calmly pointed out, is hardly ever a simple affair:

... *introspection is difficult and fallible; and .. the difficulty is simply that of all observation of whatever kind.* Something is before us; we do our best to tell what it is, but in spite of our good will we may go astray, and give a description more applicable to some other sort of thing. The only safeguard is in the final *consensus* of our farther knowledge about the thing in question, later views correcting earlier ones, until at last the harmony of a consistent system is reached. (James, 1890, Vol. 1, pp. 191–192).<sup>17</sup>

The issue of variability among introspective observers is further complicated by the fact that the experiences of the ‘subjects’ may *really* differ. Individual differences are, after all, a basic fact of life. Indeed, individual differences in self-consciousness later became a topic of research with important clinical implications (see Buss, 1980). With a nice touch of wit, Margaret Floy Washburn took up the issue of individual differences in her review of Watson's first book on behaviourism (Watson, 1914). First she teased Watson by pointing out that he must have surreptitiously engaged in some kind of introspection in order to convince himself that imagery was nothing more than kinaesthetic sensations deriving mainly from his vocal apparatus. She then continued:

We are not justified in saying that it was bad introspection on his part: some minds may indeed be so poorly furnished with certain elements of enjoyment that their possessors live in a world divested of the glow of inner color and the harmony of inner sounds. But the more fortunately endowed will reproach them for making their individual limitations the universal law. (Washburn, 1922, p. 212).

Finally, to make matters even more complicated regarding the credibility of introspective data, reliability and replicability were not the only issues. There was also the question of *validity*. As the Gestalt psychologists, such as Köhler, were insisting, why should ‘findings’ obtained through the highly analytical use of introspection be regarded as fundamental, given the highly artificial conditions under which the results were obtained:

<sup>16</sup> A similar criticism was made by Brentano, to what he termed “inner observation”: “If someone is in a state in which he wants to observe his own anger ranging within him, the anger must already be somewhat diminished, and so his original object of observation would have disappeared. The same impossibility is also present in all other cases. It is a universally valid psychological law that we can never focus our attention upon the object of inner perception.” (Brentano, (1874/1995), p. 30). For summaries of the criticisms of introspection by Lange, Comte, and Maudsley, see Brentano, (1874/1995), pp. 31 et seq., and also Wilson (1991) on Mill and Comte.

<sup>17</sup> Dewey later made a very similar point in reaction to Watson criticisms about the difficulty of engaging in introspection (Dewey, 1918/1977, p. 449), and this point has also been well made in the recent literature by Kusch (1999, pp. 240–242).

When I apply the Introspectionist's methods I often find the same experiences as he does. But I am far from attributing to such facts a rare value as though they were more "true" than the facts of everyday experience. (Köhler, 1929/1957, p. 52)

### 5.1.1. *Mental atomism*

Many of the important criticisms of introspective psychology were directed not against the use of self-observation *per se*, but against the assumptions and prejudices that came to surround it in its various applications. One of the more conspicuous of these assumptions was 'mental atomism,' the idea that consciousness consists of "complexes that are constituted of patterns of sensory elements" (Boring, 1961, p. 215).<sup>18</sup>

Sensory atomism did have some influence within early twentieth-century psychology, usually in association with a supplementary assumption, the 'constancy hypothesis,' the assumption that local stimulation (for example, to the retina) always produced a constant corresponding local sensation (see Gurwitsch, 1955). Nevertheless, there is no inevitable *logical* or even *historical* link between the use of the method of introspection and mental atomism. After all, the introspective studies conducted by the Würzburg psychologists challenged the idea, central to the positivism of the time, that consciousness was a concatenation of elementary sensations. And James was not rejecting introspection, but rather its prejudiced application, in his famous discussion of the stream of consciousness. He was appealing to introspective data as the basis of his criticism of the prejudiced disregard within the current introspective psychology of the *relational* and the *vague*:

What must be admitted is that the definite images of traditional psychology form but the very smallest part of our minds as they actually live. The traditional psychology talks like one who should say a river consists of nothing but pailsful, spoonsful, quartpotsful, barrelsful and other moulded forms of water. Even were the pails and pots all actually standing in the stream, still between them the free water would continue to flow. It is just this free water of consciousness that psychologists resolutely overlook. Every definite image in the mind is steeped and dyed in the free water that flows around it. With it goes the sense of its relations, near and remote, the dying echo of whence it came to us, the dawning sense of whither it is to lead. (James, 1884, pp. 16–17.)

### 5.1.2. *The "professed dogmatic intolerance" of introspective psychology*

Although, contrary to Watson's claims, introspective psychology was hardly the dominant approach claimed by Watson, he was certainly not alone in expressing concern that the more extreme proponents of introspectionist psychology were denying any serious place for behavioural studies in psychology. However, most of the critics saw no need to abandon introspection as a method. They were objecting instead to what Dodge described as "the professed dogmatic intolerance of introspective psychology . . . *the dualistic dogma that alone is mental reality which is given in introspection*" (Dodge, 1912, p. 216; *emphasis added*). Their point was that the study of behaviour would complement introspection, and even improve the method by correcting its "obvious limitations" (Dodge, 1912, p. 217).<sup>19</sup>

Dodge went on to claim that the "dogmatic introspectionists" believed that, with the use of introspection, "knowledge of our own mental life was immediate and adequate" (see Dodge, 1912, pp. 218). Yet there could not have been so many psychologists, at the time, who really would have claimed that introspection, as such, was transparent and unproblematic. Many were working very hard, as Dodge himself acknowledged, to develop "an introspective *technique*" (Dodge, 1912, p. 219; *emphasis added*).

<sup>18</sup> As Boring (who studied with Titchener) put it, a "complete knowledge of the psychology of sensory data would be an approximately complete knowledge of consciousness" (Boring, 1933, p. 98). Boring identified Wundt as the main proponent of mental atomism (e.g. Boring, 1961, p. 215), but this is misleading (see Blumenthal, 1975, p. 1083; Danziger, 1980c, p. 79). Furthermore, the claim that consciousness is 'sensuous' is not logically tied to sensory atomism (see Gibson, 1966; Thinès, Costall, & Butterworth, 1991).

<sup>19</sup> The same point was later made by Merleau-Ponty working within a different tradition, but also very well informed about Watson's behaviourism and his critique of introspection: "There is no reason either to reject introspection or to make it the privileged means of access to a world of psychological facts" (Merleau-Ponty, 1942/1963, p. 183).

### 5.1.3. *Two sides of the same coin*

The crucially important point being made by many of the critics of *both* “dogmatic introspectionism” (to the extent it existed) *and* Watsonian behaviourism was that they were hardly the complete opposites they might at first seem. The reason the Watsonian behaviourists rejected introspection as a proper scientific method was identical to the reason why the dogmatic introspectionists dismissed the study of behaviour as proper psychology. Both were arguing from the same premise, the *antithesis* of behaviour, on the one hand, and mind and consciousness on the other. Both were committed to an overly subjectivized conception of subjectivity, and an overly objectivized conception of behaviour.

However, introspection would be not just unreliable but entirely impossible, if there were no shareable criteria (c.f. de Laguna, 1927, p. 126). First of all, if these “inner states” were so ‘inner’ and private in a Cartesian sense, there would be no basis for acquiring and sharing a language of such psychological states in the first place.<sup>20</sup> Second, the psychological experiment (in whatever form) itself depends on an effective social coordination between experimenter and subject, as a means of negotiating the very meaning of the experimental task at hand. If introspection really concerned essentially private states, there would be no basis for any agreement about what would *count as introspection*, and, as Brock (1991, 1993) has been pointing out, certainly no possibility for the rise of *schools* of introspectionism. Despite the famous dispute between the Cornell and Würzburg researchers, there was agreement within these different groups. Finally, there manifestly do exist shareable, interpersonal criteria for assessing subjective reports, and hence reports about supposedly “essentially private states” do *not* have to be taken at their face value. It is on such a basis, after all, that the empirical research has tested not just the consistency but also the validity of introspective reports. If we truly had nothing to appeal to beyond the introspective evidence itself, there would be no way to determine whether the *majority* of such reports are right or indeed *unreliable* (cf. Nisbett & Wilson, 1977). In short, in the absence of shareable criteria for validation we would be faced with the considerable mystery of how the various practices of introspection, reliable or otherwise, ever came into being.

## 6. The dead end of introspective psychology?

According to the textbooks, and also most of the revisionist histories, introspective psychology proved a “dead end” (Danziger, 1990, p. 42). I have already discussed the issue of unreliability, and pointed out that a number of other issues were also involved in the debates within early twentieth century psychology about the value of introspection, and I have tried to show that those debates hardly proved conclusive, in terms of the logic of the arguments, one way or another. But there was also the issue of practical insignificance. Interestingly, it is in relation to introspective psychology that Neisser first invoked the term “ecological validity.” In his own version of the three-stage history of psychology, Neisser argued that introspective psychology was rejected both for its unreliability and its irrelevance: “Narrow, overly rational, applicable only to laboratory situations, it lacked any clear account of how people interact with the world” (Neisser, 1976, pp. 2–3). I will return to this point about irrelevance later, but here simply note that even Titchener, despite his dim view of psychology as a mere technology (including Watson’s behavioural engineering, see Titchener, 1914, p. 14; 1929, pp. 66–67), was happy to point out that introspectively-based psychology had already been effectively applied to pedagogy, psychopathology, and even advertising (Titchener, 1914, pp. 8–9).

The trouble, in the end, with the textbooks when they are rehearsing the old story about the demise of introspective psychology, is that they seldom bother to explain what they mean by “introspection.” The term can apply to a wide range of techniques, from the reporting of whether a sound, for example, is below or above the threshold of hearing, to the more elaborate versions involving the retrospective reporting of the process of problem solving to the search for the supposed basic elements of sensory consciousness. I have chosen to use the term in an inclusive way because I believe introspection *even in its least elaborated forms* continues to pose a very awkward problem for the majority of modern psychologists, and one that they keep failing to confront. How are self reports—either denigrated as ‘introspections’ or else re-branded as ‘verbal

<sup>20</sup> The classic examination of this issue within philosophy occurs in Wittgenstein’s *Philosophical investigations* (1953). I am aware of very little discussion or even acknowledgement of this issue, in, for example, the Theory of Mind literature. See Skinner (1945, 1957), for one of the most extensive treatments within psychology of how a language of psychological terms is possible (see also Costall, 1980).



reports’—possible? To address that basic problem effectively, psychologists will need to be more honest with themselves about their past.

Introspectionism was never the dominant force claimed by the textbooks. But, furthermore, the claims about the demise of the introspective method are highly exaggerated. Introspection, even in some of its more full-blown versions, never went away. Some versions of the method along with their results continued—and continue—to be taken seriously within the discipline. For example, the introspective techniques developed by the Würzburg psychologists were later deployed by Otto Selz in his research on problem solving (van Strien & Faas, 2004; van Strien, 2004). As late as the 1950s, Robert Leeper drew extensively on the early Würzburg studies in his chapter on “cognitive processes” for the prestigious *Handbook of Experimental Psychology* edited by Stevens (Leeper, 1951). Introspective techniques have also found a very important place within clinical psychology. Kroker has recently examined the career of Edmund Jacobsen, a student of Titchener’s, and how he had applied Titchener’s introspective method in his progressive relaxation technique to help patients deal with sources of tension within their own bodies (Kroker, 2003).

Introspection has often also been used, in a more informal, even furtive, way, as a source of preliminary insights, as in, for example, the artificial intelligence research on problem solving, etc (see Crosson, 1985).<sup>21</sup> As van Strein, has recently argued, there are also affinities between Ericsson and Simon’s (1984) work on the use of ‘think-aloud protocols’ to study thinking, and the earlier introspective research on thinking (van Strien, 2004; van Strien & Faas, 2004).<sup>22</sup> Given the important (if still controversial) status of their research, and also of Herbert Simon as one of the few psychologists to be awarded the Nobel Prize, it is surely very odd indeed that no mention is made of their endorsement of self-observations in the textbook accounts of the fate of introspective psychology:

... verbal reports, elicited with care and interpreted with full understanding of the circumstances under which they are obtained, are a valuable and thoroughly reliable source of information about cognitive processes. (Ericsson & Simon, 1980, p. 247).

However, there is another important reason for taking introspection more seriously. It is an important aspect of people’s lives beyond the very limited confines of psychological research, within, for example, religious and meditational practices, literature, and our own attempts to make sense of ourselves. As a subject of study, these practices and their conditions of possibility surely deserve careful investigation (see Vermersch, 1999).

## 7. The end of history?

With the rise of the new consciousness studies, a new mistake is just waiting to be made, a new *four-stage* history of scientific psychology. The first three-stages will be familiar: (1). *Introspectionism*: psychology as the science of the mind; 2. *Behaviourism*: the rejection of introspection as a method and the redefinition of psychology from a science of mind to a science of behaviour; (3). *Half-baked Cognitivism*: the rejection of behaviourism’s conception of psychology as the science of behaviour, and the return to a science of mind. To these three stages, there would then be added this fourth and final stage: (4). *True Cognitivism*: the return of *consciousness* as a proper subject of psychological study, through the return of ‘introspection,’ and the ultimate fulfilment of the goals of the early introspectionists.

This four-stage account would, as I have been trying to argue, not only perpetuate a good deal of mythical history, but would once again postpone the serious conceptual work that needs to be done before we can bring

<sup>21</sup> Much of what once would have counted as “introspection” simply became rebranded as “verbal report.” A distinction between the two is seldom forthcoming. Hebb’s textbook is an exception. Having dismissed “introspection” as a hopeless method, it immediately goes on to recommend “verbal reports” as a “sensitive indicator of what is going on inside the subject” (Hebb, 1966, p. 6). The *big* difference is, according to Hebb, that making “verbal reports” (as opposed to “introspecting”) involves nothing but *movements* “produced by the chest muscles which control breathing, by changes in the tension of the vocal cords, and by movements of the lips, tongue and jaw” (Hebb, 1966, p. 6).

<sup>22</sup> As Danziger (2004) has recently noted, Ericsson and Crutcher (1991) not only contrasted introspection and verbal reports, but also linked their use of protocols to Watson’s use of protocols in his studies of thinking. However, Herbert Simon, himself, in an interview with Baars (1986, pp. 365–66), insisted upon a close relationship between his own self-report studies and Otto Selz’s early introspective studies.

consciousness properly back onto the psychological agenda. What we lack, at least within mainstream psychology, is a sensible, non-dualistic account of what is *really* involved in the various socially shared practices of self-observation. After all, as Dewey pointed out, “if we had not talked with others and they with us, we should never talk to and with ourselves” (Dewey, 1925/1958, p. 170).

## References

- Adams, W. A. (2000). Introspectionism reconsidered. Presented at “Towards a science of consciousness (Tucson 2000), April 10–15, 2000, Tucson Convention Center, Tucson, AZ.
- Anderson, J. R. (2000). *Cognitive psychology and its implications* (5th ed.). New York: Worth.
- Angell, J. R. (1906). *Psychology* (3rd ed.). New York: Holt.
- Ardener, E. (1973). ‘Behaviour’: a social anthropological criticism. *Journal of the Anthropological Society of Oxford*, 4, 152–154.
- Atkinson, R. L., Atkinson, R. C., Smith, E. E., Bem, D. J., & Nolen-Hoeksema, S. (2000). *Hilgard’s introduction to psychology* (13th ed.). Fort Worth, TX: Harcourt College Publishers.
- Baars, B. J. (1986). *The cognitive revolution in psychology*. New York: Guilford Press (pp. 239–252).
- Baars, B. J., & McGovern, K. (1994). How not to start a scientific revolution. *Contemporary Psychology*, 39, 370–371.
- Bakan, D. (1966). Behaviorism and American urbanization. *Journal of the History of the Behavioral Sciences*, 2, 5–28.
- Bakan, D. (1969). *On method: toward a reconstruction of psychological investigation*. San Francisco: Jossey-Bass.
- Baldwin, J. M. (1909). The influence of Darwin on theory of knowledge and philosophy. *Psychological Review*, 16, 207–218, p. 211.
- Blackmore, S. (in press). *Conversations on consciousness*. Oxford: Oxford University Press.
- Blumenthal, A. L. (1975). A reappraisal of Wilhelm Wundt. *American Psychologist*, 30, 1081–1088.
- Blumenthal, A. L. (1979). The founding father we never knew. *Contemporary Psychology*, 24, 547–550.
- Blumenthal, A. L. (1985). Wilhelm Wundt: Psychology as the propaedeutic science. In C. Buxton (Ed.), *Points of view in the modern history of psychology* (pp. 19–49). New York: Academic Press.
- Blumenthal, A. L. (2001). A Wundt primer: The operating characteristics of consciousness. In R. W. Rieber & D. K. Robinson (Eds.), *Wilhelm Wundt in history: The making of a scientific psychology* (pp. 121–144). New York: Kluwer Academic/Plenum Publishers.
- Boring, E. G. (1927). Edward Bradford Titchener 1867–1927. *American Journal of Psychology*, 38, 489–506.
- Boring, E. G. (1933). *The physical dimensions of consciousness*. New York: Century.
- Boring, E. G. (1961). The history of introspection. In E. G. Boring (Ed.), *Psychologist at large* (pp. 211–245). New York: Basic Books.
- Bramel, D., & Friend, R. (1981). Hawthorne, the myth of the docile worker, and class bias in psychology. *American Psychologist*, 36, 867–878.
- Brentano, F. (1874/1995). *Psychology from an empirical viewpoint*. London: Routledge.
- Broadbent, D. E. (1980). Autobiography. In G. Lindzey (Ed.), *A history of psychology in autobiography* (Vol. 7, pp. 39–73). San Francisco: Elsevier.
- Brock, Adrian. (1991). Imageless thought or stimulus error? The social construction of private experience. In W. R. Woodward & R. S. Cohen (Eds.), *World views and scientific discipline formation* (pp. 97–106). Netherlands: Kluwer.
- Brock, A. (1993). Something old, something new: the reappraisal of Wilhelm Wundt in textbooks. *Theory and Psychology*, 3, 235–242.
- Bruner, J. (1988). Founding the center for cognitive studies. In W. Hirst (Ed.), *The making of cognitive science: Essays in honor of George A. Miller* (pp. 90–99). Cambridge: Cambridge University Press.
- Bruner, J. (1990). *Acts of meaning*. Cambridge, MA: Harvard University Press.
- Brush, S. G. (1974). Should the history of science be rated ‘X’?. *Science* 183, 1164–1172.
- Buss, A. H. (1980). *Self-consciousness and social anxiety*. San Francisco: Freeman.
- Carr, H. A. (1926). *Psychology*. New York: Longmans Green.
- Carr, H. A. (1915). Review of J.B. Watson (1914). Behavior: an introduction to comparative psychology. *Psychological Bulletin*, 12, 308–312.
- Cattell, J. McK. (1904). Conceptions and methods of psychology. *Congress of Arts and Science, Universal Exposition, St. Louis*, 5, 593–604.
- Connelly, J., & Costall, A. (2000). R.G. Collingwood and the idea of an historical psychology. *Theory and Psychology*, 10, 147–170.
- Costall, A. (1980). The limits of language: Wittgenstein’s later philosophy and Skinner’s radical behaviorism. *Behaviorism*, 8(2), 123–131.
- Costall, A. (1991a). Graceful degradation: cognitivism and the metaphors of the computer. In A. Still & A. Costall (Eds.), *Against cognitivism* (pp. 151–170). London: Harvester-Wheatsheaf.
- Costall, A. (1991b). General introduction. In G. Thines, A. Costall, & G. E. Butterworth (Eds.), *Michotte’s experimental phenomenology of perception* (pp. 1–12). Hillsdale, NJ: Erlbaum.
- Costall, A. (1993). How Lloyd Morgan’s canon backfired. *Journal of the History of the Behavioral Sciences*, 29, 113–124.
- Costall, A. (1998). Lloyd Morgan and the rise and fall of ‘animal psychology’. *Society and Animals*, 6(1), 13–29.
- Costall, A. (2003a). Le préjugé du monde all over again. Review of Jean-Pierre Dupuy. ‘The mechanization of the mind: on the origins of cognitive science’. *Theory and Psychology*, 13, 421–423.
- Costall, A. (2003b). Meeting behaviourism half way: Michotte and the revolt against positivism. In U. Savardi & A. Mazzocco (Eds.), *Figura e sfondo: Temi e variazioni per Paolo Bozzi* (pp. 131–147). Padova: Coop. Libreria Editrice Università di Padova.

- Costall, A. (2004). From Darwin to Watson (and Cognitivism) and back again: the principle of animal-environment mutuality. *Behavior and Philosophy*, 32, 179–195.
- Costall, A., & Leudar, I. (2004). Where is the ‘theory’ in theory of mind? *Theory and Psychology*, 14, 625–648.
- Costall, A., & Still, A. (1991). Cognitivism as an approach to cognition. In A. Still & A. Costall (Eds.), *Against cognitivism: Alternative foundations for cognitive psychology* (pp. 1–6). Hemel Hempstead: Harvester-Wheatsheaf.
- Crosson, F. J. (1985). Psyche and the computer: integrating the shadow. In S. Koch & D. E. Leary (Eds.), *A century of psychology as a science* (pp. 437–451). New York: McGraw-Hill.
- Danziger, K. (1979). The positivist repudiation of Wundt. *Journal for the History of the Behavioural Sciences*, 15, 205–230.
- Danziger, K. (1980a). The history of introspection reconsidered. *Journal of the History of the Behavioral Sciences*, 16, 241–262.
- Danziger, K. (1980b). Wundt’s psychological experiment in the light of his philosophy of science. *Psychological Research*, 42, 109–122.
- Danziger, K. (1980c). Wundt and the two traditions of psychology. In R. W. Rieber (Ed.), *Wilhelm Wundt and the making of a scientific psychology*. New York: Plenum.
- Danziger, K. (1985). The origins of the psychological experiment as a social institution. *American Psychologist*, 40, 133–140.
- Danziger, K. (1990). *Constructing the subject: historical origins of psychological research*. New York: Cambridge University Press.
- Danziger, K. (1997). *Naming the mind: How psychology found its language?* London: Sage.
- Danziger, K. (2004). Concluding comments. In A. C. Brock, J. Louw, & W. van Hoorn (Eds.), *Rediscovering the history of psychology* (pp. 207–231). New York: Kluwer Academic.
- Daston, L. (1992). Objectivity and the escape from perspective. *Social Studies of Science*, 22, 81–128.
- de Laguna, G. A. (1927). *Speech: its function and development*. New Haven: Yale University Press.
- Dennett, D. (1991). *Consciousness explained*. London: Allen Lane.
- Dewey, J. (1884). The new psychology. *Andover Review*, 2, 278–289.
- Dewey, J. (1896). The reflex arc concept in psychology. *Psychological Review*, 3, 357–370.
- Dewey, J. (1914/1977). Psychological doctrine and philosophical teaching. In S. Morgenbesser (Ed.), *Dewey and his critics* (pp. 439–445). New York: Journal of Philosophy, Inc. [First published in the *Journal of Philosophy Psychology and Scientific Methods*, 1914, 11(19)].
- Dewey, J. (1918/1977). Concerning alleged immediate knowledge of mind. In S. Morgenbesser (Ed.), *Dewey and his critics* (pp. 446–452). New York: Journal of Philosophy, Inc. [First published in the *Journal of Philosophy Psychology and Scientific Methods*, 1918, 15(2)].
- Dewey, J. (1925/1958). *Experience and nature*. New York: Dover [Based on the Paul Carus lectures of 1925].
- Dodge, R. (1912). The theory and limitation of introspection. *American Journal of Psychology*, 23, 214–229.
- Dupuy, Jean-Pierre. (2000). *The mechanization of the mind: On the origins of cognitive science*. [trans. M. B. DeBevoise.] Princeton, NJ: Princeton University Press.
- Ebbinghaus, H. (1908). *Psychology: an elementary text-book*. (Max Meyer, trans.) Boston: D.C. Heath & Co.
- Ericsson, K. A., & Crutcher, R. J. (1991). Introspection and verbal reports on cognitive processes—two approaches to the study of thinking: response to Howe. *New Ideas in Psychology*, 9, 57–71.
- Ericsson, K. A., & Simon, H. A. (1980). Verbal reports as data. *Psychological Review*, 87, 215–251.
- Ericsson, K. A., & Simon, H. A. (1984). *Protocol analysis: Verbal reports as data*. Cambridge: MIT Press.
- Ermarth, M. (1978). *Wilhelm Dilthey: The critique of historical reason*. Chicago: University of Chicago Press.
- Flugel, J. C. (1951). *A hundred years of psychology 1833–1933: with additional part on developments 1933–1947*. London: George Duckworth & Co. Ltd.
- Gibson, J. J. (1966). *The senses considered as perceptual systems*. Boston: Houghton-Mifflin.
- Greenwood, J. D. (2003). Wundt, Völkerpsychologie, and experimental psychology. *History of Psychology*, 6, 70–88.
- Gurwitch, A. (1955). The phenomenological and the psychological approach to consciousness. *Philosophy and Phenomenological Research*, 15, 303–319.
- Güzeldere, G. (1997). The many faces of consciousness: a field guide. In N. Block, O. Flanagan, & G. Güzeldere (Eds.), *The nature of consciousness: Philosophical debates* (pp. 1–67). Cambridge, MA: MIT Press.
- Harnad, S. (1985). Aspects of behaviourism. *Times literary supplement*, No. 4298 (August 16th), 901.
- Harrell, W., & Harrison, R. (1938). The rise and fall of behaviorism. *Journal of General Psychology*, 18, 367–421.
- Harris, B. (1979). Whatever happened to Little Albert? *American Psychologist*, 34, 151–160.
- Hebb, D. O. (1966). *Textbook of psychology*, (2nd. ed.).
- Heidbreder, E. (1933). *Seven psychologies*. New York: Century.
- Holt, E. B. (1915). *The Freudian wish and its place in ethics*. New York: Macmillan.
- Hooker, C. A. (1996). Toward a naturalized cognitive science: a framework for cooperation between philosophy and the natural sciences of intelligent systems. In W. O’Donohue & R. F. Kitchener (Eds.), *Philosophy of psychology* (pp. 184–206). London: Sage.
- Hull, C. L. (1943). *Principles of behavior*. New York: Appleton-Century.
- Hume, D. (1739–40/1969). In E. C. Mossner (Ed.), *A treatise of human nature*. London: Penguin Books.
- Humphrey, G. (1951). *Thinking*. London: Methuen.
- James, W. (1884). On some omissions of introspective psychology. *Mind*, 9, 1–26.
- James, W. (1890). *Principles of psychology* (Vol. 1). New York: Holt.
- Jenkins, J. J. (1986). Interview with James J. Jenkins. In B. J. Baars (Ed.), *The cognitive revolution in psychology* (pp. 239–252). New York: Guilford Press.
- Johnson-Laird, P. (1988). *The computer and the mind*. Cambridge, MA: Harvard University Press.
- Jones, M. C. (1974). Albert, Peter, and John B. Watson. *American Psychologist*, 29, 581–583.

- Judd, C. (1932). Autobiography. In C. Murchison (Ed.), *A history of psychology in autobiography* (Vol. 2, pp. 207–235). Worcester, MA: Clark University Press.
- Judd, C. H. (1897). Wundt's system of philosophy. *Philosophical Review*, 6, 370–385.
- Kessen, W. (1981). Early settlements in new cognition. *Cognition*, 10, 167–171.
- Kihlstrom, J. F. (1987). The cognitive unconscious. *Science*, 237, 1445–1452.
- Kitchener, R. F. (1977). Behavior and behaviorism. *Behaviorism*, 5, 11–71.
- Klein, D. B. (1970). *A history of scientific psychology: Its origins and philosophical backgrounds*. London: Routledge & Kegan Paul.
- Knapp, T. J. (1986). The emergence of cognitive psychology in the latter half of the twentieth century. In T. J. Knapp & L. C. Robertson (Eds.), *Approaches to cognition: contrasts and controversies* (pp. 13–35). Hillsdale, NJ: Erlbaum.
- Köhler (1929/1957). *Gestalt psychology*. New York: Liveright.
- Kroger, K. (2003). The progress of introspection in America, 1896–1938. *Studies in the History and Philosophy of Biology and Biomedical Science*, 34, 77–108.
- Kuhn, T. S. (1968). Science I: the history of science. In D. L. Sills (Ed.), *International encyclopedia of the social sciences* (Vol. 14, pp. 74–83). New York: Macmillan.
- Kusch, M. (1999). *Psychological knowledge: A social history and philosophy*. London: Routledge.
- Leahey, T. H. (1984). Behaviorism. In R. J. Corsini (Ed.), *Encyclopedia of psychology* (Vol. 1, pp. 130–133). New York: Wiley.
- Leahey, T. H. (1993). A history of behavior. *Journal of Mind and Behavior*, 14, 345–354.
- Leahey, T. H. (1992). The mythical revolutions of American psychology. *American Psychologist*, 47, 308–318.
- Leeper, R. (1951). Cognitive processes. In S. S. Stevens (Ed.), *Handbook of experimental psychology* (pp. 730–757). New York: Wiley.
- Leslie, A. (1987). Pretense and representation: the origins of theory of mind. *Psychological Review*, 94, 412–426.
- Leudar, I., & Costall, A. (2004). Special issue: theory of mind. *Theory and Psychology*, 14(5), 573–757.
- Lundin, R. W. (1984). Structuralism. In R. J. Corsini (Ed.), *Encyclopedia of psychology* (Vol. 3, pp. 374–375). New York: Wiley.
- Merleau-Ponty, M. (1942/1963). *The structure of behaviour*. (A. L. Fisher, trans.) London: Methuen.
- Meyering, T. C. (1989). *Historical roots of cognitive science: The rise of a cognitive theory of perception from antiquity to the nineteenth century*. Dordrecht: Kluwer Academic Publishers.
- Michotte, A. (1907). À propos de la “méthode d'introspection” dans la psychologie expérimentale Concerning “the method of introspection” in experimental psychology. *Revue Néo-Scholastique*, 14, 507–532.
- Michotte, A. (1954/1991). Autobiography of Professor A. Michotte van den Berck. In G. Thinès, A. Costall, & G. E. Butterworth (Eds.), *Michotte's experimental phenomenology of perception* (pp. 24–49). Hillsdale, NJ: Erlbaum [Translation by A. Costall of A. Michotte “Autobiographie de M. Le Professeur A. Michotte van den Berck. *Psychologica Belgica*, 1954, 1, 190–217].
- Miller, G. A. (2003). The cognitive revolution: a historical perspective. *Trends in Cognitive Sciences*, 7, 141–144.
- Miller, G. A., Galanter, E., & Pribram, K. H. (1960). *Plans and the structure of behavior*. New York: Holt, Rinehart & Winston.
- Morgan, C. L. (1900). *Animal behaviour*. London: Edward Arnold.
- Morris, C. (1970). *The pragmatic movement in American Philosophy*. New York: George Braziller.
- Moustgaard, I. K. (1990). *Psychological observation and description*. Norway: Sigma Forlag.
- Neisser, U. (1967). *Cognitive psychology*. New York: Appleton-Century-Crofts.
- Neisser, U. (1976). *Cognition and reality*. San Francisco: W.H. Freeman.
- Neisser, U. (1997). The future of cognitive science: an ecological analysis. In D. M. Johnson & C. E. Erneling (Eds.), *The future of the cognitive revolution* (pp. 247–260). New York: Oxford University Press.
- Nisbett, R. E., & Wilson, T. D. (1977). Telling more than we can know: verbal reports on mental processes. *Psychological Review*, 84, 231–259.
- O'Donnell, J. M. (1985). *The origins of behaviorism: American psychology, 1870–1920*. New York: New York University Press.
- Parmelee, M. (1921). *The science of human behavior*. New York: Macmillan.
- Pylyshyn, Z. (1986). *Computation and cognition*. Cambridge, MA: MIT Press.
- Reed, E. (1997a). The cognitive revolution from an ecological view. In D. M. Johnson & C. E. Erneling (Eds.), *The future of the cognitive revolution* (pp. 261–273). New York: Oxford University Press.
- Reed, E. S. (1997b). *From soul to mind: The emergence of psychology, from Erasmus Darwin to William James*. Yale, CT: Yale University Press.
- Roback, A. A. *History of American psychology*. New York: Collier-Macmillan.
- Robinson, D. N. (1982). *Toward a science of human nature: Essays on the psychologies of Mill, Hegel, Wundt, and James*. New York: Columbia University Press.
- Rosenthal, D. M. (1998). Introspection. In R. A. Wilson & F. C. Keil (Eds.), *The MIT encyclopedia of cognitive science* (pp. 419–421). Cambridge: MIT Press.
- Rubinstein, S. L. (1937/1987). Problems of psychology in the works of Karl Marx. *Studies in Soviet Thought*, 33, 111–130 [First published in *Sovetskaja psichotekhnika*, 7(1), 1934. Trans. T.J. Blakeley].
- Samelson, F. (1974). History, origin myth and ideology: “Discovery” of social psychology. *Journal for the Theory of Social Behaviour*, 4, 217–231.
- Samelson, F. (1980). J.B. Watson's Little Albert, Cyril Burt's twins, and the need for a critical science. *American Psychologist*, 35, 619–625.
- Samelson, F. (1981). Struggle for scientific authority: the reception of Watson's behaviorism, 1913–1920. *Journal of the History of the Behavioral Sciences*, 17, 399–425.
- Samelson, F. (1985). Organizing for the kingdom of behavior: accademic battles and organizational policies in the Twenties. *Journal of the History of the Behavioral Sciences*, 21, 33–47.

- Scripture, E. W. (1895/1907). *Thinking, feeling, doing: An introduction to mental science* (2nd ed., rev.). New York: G.P. Putnam's Sons [First edition published in 1895].
- Seashore, C. (1930). *Autobiography*. In C. Murchison (Ed.), *A history of psychology in autobiography* (Vol. 1). Worcester, MA: Clark University Press.
- Shallice, T. (1972). Dual functions of consciousness. *Psychological Review*, 79, 383–393.
- Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review*, 52, 270–294.
- Skinner, B. F. (1957). *Verbal behaviour*. London: Methuen.
- Thinès, G., Costall, A., & Butterworth, G. (1991). *Michotte's experimental phenomenology of perception*. Hillsdale, NJ: Lawrence Erlbaum.
- Thorndike, E. L. (1915). Watson's "behavior". *Journal of Animal Behavior*, 5, 462–467.
- Titchener, E. B. (1914). On "Psychology as the behaviorist views it". *Proceedings of the American Philosophical Society*, 53, 1–17.
- Titchener, E. B. (1929). *Systematic psychology: Prolegomena*. New York: Macmillan.
- van Strien, P. J. (2004). Paris, Leipzig, Danziger, and beyond. In A. C. Brock, J. Louw, & W. van Hoorn (Eds.), *Rediscovering the history of psychology* (pp. 75–96). New York: Kluwer Academic.
- van Strien, P. J., & Faas, E. (2004). How Otto Selz became a forerunner of the cognitive revolution. In T. C. Dalton & R. B. Evans (Eds.), *The life cycle of psychological ideas: Understanding prominence and the dynamics of intellectual change* (pp. 175–201). New York: Kluwer Academic/Plenum Publishers.
- Varela, F. (1996). Neurophenomenology: a methodological remedy for the hard problem. *Journal of Consciousness Studies*, 3, 330–350.
- Varela, F., Thompson, E., & Rosch, E. (1991). *The embodied mind: Cognitive science and human experience*. Cambridge, MA: MIT.
- Vermersch, P. (1999). Introspection as practice. *Journal of Consciousness Studies*, 6, 17–42.
- Washburn, M. F. (1922). Introspection as an objective method. *Psychological Review*, 29, 89–112.
- Watson, J. B. (1913a). Psychology as the behaviorist views it. *Psychological Review*, 20, 158–177.
- Watson, J. B. (1913b). Image and affection in behavior. *Journal of Philosophy, Psychology, and Scientific Methods*, 10, 421–428.
- Watson, J. B. (1914). *Behavior: An introduction to comparative psychology*. New York: Holt.
- Watson, J. B. (1920). Is thinking merely the action of language mechanisms?. *British Journal of Psychology* 11, 87–104.
- Watson, J. B. (1924). *Psychology from the standpoint of a behaviorist* (2nd ed.). Philadelphia: J.B. Lippincott.
- Watson, J. B. (1930). *Behaviorism* (Second revised ed.). New York: Norton.
- Weidman, N. M. (1999). *Constructing scientific psychology: Karl Lashley's mind-brain debates*. Cambridge: Cambridge University Press.
- Williams, R. (1983). *Keywords*. London: Flamingo, Fontana Paperbacks.
- Wilson, F. (1991). Mill and Comte on the method of introspection. *Journal of the History of the Behavioral Sciences*, 27, 107–129.
- Winston (2004). Controlling the metalanguage: authority and acquiescence in the history of method. In A. C. Brock, J. Louw, & W. van Hoorn (Eds.), *Rediscovering the history of psychology* (pp. 53–73). New York: Kluwer Academic.
- Wittgenstein, L. (1953). *Philosophical investigations*. Oxford: Blackwell.
- Woodworth, R. S. (1931). *Contemporary schools of psychology*. London: Methuen & Co.
- Wundt, W. (1887). Selbstbeobachtung und innere Wahrnehmung. *Philosophische Studien*, 4, 292–309.
- Wundt, W. (1897). *Outlines of psychology*. (C. H. Judd trans.) Leipzig: Wilhelm Engelmann.
- Wundt, W. (1900). *Die Sprache*. Leipzig: Engelmann.
- Wundt, W. (1907). Über Ausfrageexperimente und über die Methoden zur Psychologie des Denkens. *Psychologische Studien*, 3, 301–360.
- Wundt, W. (1916). *Elements of folk psychology: Outlines of a psychological history of the development of mankind*. (E.L. Straub trans.) London: Macmillan. [German edition first published in 1912].