

The behaviorist myth and the problem of behavior control in economics¹

by

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“if practitioners are reluctant to make use of the history and methodology professional literatures, but still wish to use arguments of this character, they might simply do so in a more implicit manner by adopting and modifying methodology and history arguments they perceive to be ‘in the air’ according to their more immediate needs”

(Davis, 2006)

Introduction

There seem to be obscurities about the meaning of behaviorism and its connections to economic theory, especially in recent discussions about “economics and psychology”. This essay aims at exploring that subject, and analyzing the possibility of defining such a thing as a “behaviorist choice theory”. It does so by presenting the main traits of behaviorism as conceived by historians of psychology, and contrasting that view against that of economists. It highlights the role of “control” in defining behaviorism, and claims that rather than affecting choice theory, behaviorism influenced unorthodox writings by economists involved in early institutionalism and the progressive movement of the 1890s-1920s.

Section 1 begins by presenting a sample of the arguments about behaviorism currently held by economists. According to such views, consumer choice theory – especially that following Samuelson’s revealed preferences – became behaviorist during the 1930s as it got rid of psychic (i.e. unobservable) elements like “marginal utilities” and “substitution rates”. Scholars involved in advancing behavioral economics usually claim to react against that “behaviorist turn” by providing choice theory “with more psychologically plausible foundations” (Angner and Loewenstein, 2012, p. 642).

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Section 2 presents parts of the history of behaviorism (i.e. by historians of psychology) and claims that its main aim was to control and predict behavior, which was by no means equivalent to just “observing behavior” as (mostly behavioral) economists seem to believe. For behaviorists, behavior resulted from the interaction of an organism with its environment. J.B. Watson, B.F. Skinner and their followers developed a behavior control paradigm by claiming that manipulating environments led to shaping behavior. They attempted to use their science to improve society – both during the progressive era and throughout the 1970s – in ways which were fundamentally opposed to the ordinalist project of deducing preferences and utility functions from choice.

Section 3 claims that the ordinalist program was a pole apart from behaviorism, even methodologically speaking (i.e. considering their common references to P. Bridgman’s operationalism). The fact that economists like Samuelson attempted to operationalize their concepts, does not imply that choice theory became close to behaviorism. Mainstream economists still tend to conceive of behavior as being essentially purposive, deliberate, and based on the mentalist notion of preferences. They also tend to refuse behavior control epistemologies and thus the core of behaviorism.

Section 4 concludes by pointing out a series of behavior control elements in unorthodox writings by early institutionalists like T. Veblen, W.C. Mitchell, and J.M. Clark. It compares such views with those of behavioral economists, in order to demonstrate that economics is still fundamentally at odds with the idea of considering behavior as being non-purposeful and resulting from control. It is claimed that in order to qualify as economic, a theory must explain behavior as resulting essentially from volition and choice.

1. The “behaviorist turn” in economics

The so called “behaviorist turn”, initiated during the 1930s, is critically discussed by economists who are interested in further relating economics to psychology. These (usually behavioral) economists interpret behaviorism as being a “dark” episode in the history of the relationship between the two disciplines. In addition, behaviorism is considered with skepticism by economists who study verbal behavior through surveys – like, for instance,

those interested in analyzing happiness (i.e. subjective well-being data). This section describes these ideas about the influence of behaviorism and economics. In doing so, it shows that the subject is much more complex than what appears in the current literature about economics and psychology.

Psychology “in” and “out” of economics

As noted by Hands (2010), there “seem to be two popular views on the matter within the contemporary literature”: (1) that during the ordinal revolution “psychology was driven out of consumer choice theory and that was a good thing” and (2) that “psychology was driven out of consumer choice theory and that, we now realize, was a bad thing” (Hands, 2010, p. 634). These two views confront “those who are broadly supportive of rational choice theory” (i.e. ordinalists), with “contemporary experimental and behavioral economists who are (to some degree) critical” (*ibid.*). Hands (2010) identifies the following historical narrative as supporting that debate:

“In simplified form, the standard story of consumer choice theory is that psychology came into economics during the neoclassical revolution of the 1870s, and remained in for the period of cardinal utility theory, but then was driven out during the ordinal and revealed preference revolutions [...]. If one extends this story forward to the current time, then it appears that yet another change—in this case a change back to the explicit consideration of psychology—may be underway.” (*ibid.*, p. 635)

According to a recent survey by Angner and Loewenstein (2012), the change back to considering psychology has been happening since the 1980s as economists have increased the “explanatory and predictive power of economic theory” by giving it foundations which are “consistent with the best available psychology” (Angner and Loewenstein, 2012, p. 642). This new literature supposedly stands “in opposition to neoclassical economics, which was heavily influenced by behaviorism and associated doctrines, including verificationism and operationalism” (*ibid.*).

In exploring the history of economics and psychology, Angner and Loewenstein show that “before the emergence of behaviorism” (*ibid.*, p. 644), both classical economists and “early neoclassicals” – in addition to instinct psychologists like William James [1842-1910] – were “comfortable with talking about mental states and other unobservables” (*ibid.*). In line

with Hands' "standard story", they show how that position changed along Watson's attack to the use of introspection and mentalism:

"The emergence of behaviorism – marked by the appearance of John B. Watson's article 'Psychology as the Behaviorist views it' [1913] – included an attack on both heavy reliance on introspection and references to mental states. Behaviorists like Watson argued, first, that all scientific methods should be public (thereby rejecting the use, e.g., of introspection), and second, that a science of behavior should focus on behavior only (thereby avoiding references to unobservables such as beliefs, desires, plans, and intentions) [...]. These ideas are clearly present in the writings of the postwar neoclassical economists as well." (*ibid.*, p. 647)

In general, the rise of behaviorism is associated by economists to explanations about how neoclassical economics "escaped from" psychology (Giocoli, 2003) – or to use Hands' terms, about how psychology was driven "out". It is usually argued that "postwar neoclassical economists wanted to gain distance from psychology of all kinds, objected the notion that economics should make reference to conscious states, and rejected the idea that introspection was a scientifically acceptable means to explore such states" (*ibid.*).

Behaviorism as understood by economists

It is worth noting that the history of economics and psychology described in this section, has been written mainly by economists. It appears within surveys about behavioral economics, experimental economics and other subfields, as, for instance, those by Lewin (1996), Rabin (1998), Giocoli (2003), Bruni (2004), Sent (2004), Asso and Fiorito (2004), Bruni and Sugden (2007), and Angner and Loewenstein (2012). It offers quick reconstructions of the history of the two disciplines (particularly loose concerning psychology), and gives only partial accounts of "introspection", "behaviorism", the "cognitive revolution", and several other elements of that history.

S. Lewin's (1996) article can be claimed to be among the first to explore "the historical roots of today's disagreements" about economics and psychology (Lewin 1996, p. 1294). It is a quite clear example of the above-mentioned literature, and proceeds through the following "synopsis":

“A behaviorist movement arose in economics, as theorists attempted to free economics of all psychological elements. This movement contributed to the replacement of the older theory of cardinal utility, with the new notion of ordinal preferences. Later, the theory of revealed preferences eliminated the need to interpret even ordinal preferences psychologically. Preferences were transformed from ‘metaphysical’ entities into scientifically valid, truly empirical objects derived solely from behavior” (*ibid.*, p. 1295)

That narrative about the history of economics and psychology makes part of the now “traditional interpretation” according to which “the final step in the escape from psychology was completed by Samuelson’s attempt to reconstruct consumer choice theory along completely behaviorist lines” (Hands, 2010, p. 636). Moreover, and in addition to that “traditional interpretation”, there is a second strand of literature referring to the impact of behaviorism in economics. Proposed by economists willing to study subjective outcomes from surveys, that narrative opposes the “behaviorist belief” (Angner and Loewenstein 2012, p. 649) according to which observing market transactions or choices, is “the only valid method to collect information about preferences” (*ibid.*)³.

I have elsewhere written about how trusting what people do rather than what they say, has conditioned the history of the use of surveys in economics (Edwards 2012). Economists involved in such research often refer to A. Sen’s (1973) writings about “non-verbal behavior” being the only valid source of information about a person’s preferences. In Sen’s (1973) terms: “that behavior is a major source of information on a person’s preferences can hardly be doubted, but the belief that it is the only basis of surmising about people’s preferences seems extremely questionable” (Sen, 1973, p. 258). In qualifying the revealed preferences approach, he refers to behaviorism as follows:

“There is an old story about one behaviorist meeting another, and the first behaviorist asks the second: ‘I see you are very well. How am I?’ The thrust of the revealed preference approach has been to undermine thinking as a method of self-knowledge and talking as a method of knowing about others. In this, I think, we have been prone, on the one hand, to overstate the difficulties of introspection and communication, and

³ Add note about the history of the use of self-reports in economics (Edwards, 2012).

on the other, to underestimate the problems of studying preferences revealed by observed behavior” (Sen 1973, p. 258)

Extended to the more general use of surveys in economics, Easterlin (2004) considers behaviorism as being the “typical” economic attitude of avoiding the use of verbal statements:

“The typical economist’s view, encapsulated in the concept of ‘behaviorism,’ was put succinctly as follows by Victor Fuchs, president of the American Economic Association in 1995: ‘Economists, as a rule, are not concerned with the internal thought processes of the decision maker or in the rationalizations that the decision maker offers to explain his or her behavior. Economists believe that what people *do* is more relevant than what they say’” (Easterlin, 2004, p. 21)

For Easterlin, the economists’ predisposition against using subjective facts does not respond to “uncertainty as to their robustness”, but to the “disciplinary paradigm of behaviorism” (Easterlin, 2004, p. 31). However, and in line with most behavioral economists, he perceives “recent signs that the discipline may be gradually turning away” from that paradigm (*ibid.*).

Layard’s *Happiness: Lessons from a New Science* (2005), develops a peculiar account of the history of economics and psychology, which is reproduced here as a final example of what economists seem to think of behaviorism and its impact on economics⁴:

“The [GNP] concept was developed in the 1930s [...]. But very quickly it got hijacked to become a measure of national welfare, and nations now jostle for position in the national income stakes [...]. This hijacking was inevitable once economics had been captured by behaviorism in the 1930s. It is actually a rather sorry tale. In the late nineteenth century most English economists thought that economics was about happiness [...]. Their system was not fully operational, but it was a forward-looking agenda. It was also in tune with late nineteenth century psychology like that of William James, who was actively studying the strength of human feelings. Then psychology turned behaviorist. Along came John Watson and Ivan Pavlov (followed

⁴ Add note about the economics of happiness (Edwards, 2009).

by Skinner), who argued that we can never know other people's feelings, and all we can do is to study their behavior [...]. So behaviorism became the intellectual climate, and in the 1930s it took over economics. This led to a much narrower concept of happiness [...]" (Layard, 2005, p. 133)

As in most of the literature about economics and psychology, Layard points out quick connections between elements like "behaviorism", "John Watson", "William James", and "Skinner". As the following text shows, economists seem to have developed an image of behaviorism, which is inconsistent with what behaviorists did.

About the complexity of the history of economics and psychology

It must be noted before proceeding that the complex character of the history of economics and psychology has been identified (albeit not clarified) by some of the authors involved. A. Sen (1973), for instance, claimed that the real interest of revealed preferences came from the "skillful use of the assumption that behavior reveals preference", rather than from explaining "behavior without reference to anything other than behavior" (*ibid.*).

Lewin (1996) also claimed that the meaning of behaviorism and its relation to economics was "far more complex" (*ibid.*, p. 1307) than usually understood. While pointing out the "absurdity of behaviorist mainstream economics" Lewin showed that even Samuelson ignored the work of behaviorist psychologists:

"Behaviorist mainstream economics was doomed to fail, for the theoretical practice of 'behaviorists' such as Samuelson contradicted their own professed methodological views. [...] if economists were to become behaviorists, they had to do so wholeheartedly and actually learn from the work of behaviorist psychologists. But even as they reformulated preference theory so as to make its behavioral implications more explicit, these mainstream economists nevertheless ignored the work of behaviorist psychologists. They continued to obtain their assumptions from introspection or a priori deduction, rather than looking to rigorous experimental results as their own behaviorist methodology indicated that they should." (Lewin, 1996, p. 1318)

Along the same line, Hands (2010) shows that psychology was never as "out" of economics as economists seem to claim: not only did Robbins know that the "forward-looking notion of purposive behavior" was "not susceptible of observation by purely behaviorist methods"

(Hands, 2010, p. 640), but also Samuelson himself would have dropped the idea of developing a “strictly operational theory of consumer behavior”, shortly after his 1938 paper (*ibid.*, p. 641)⁵.

Last but not least, and in addition to the “in”/“out”/“back” account of the relationship between economics and psychology, there is another interesting narrative referring to the early institutionalism of T. Veblen, W.C. Mitchell and J.M. Clark. Angner and Loewenstein (2012) show that these institutionalists were among “the earliest and most vehement critics of ordinalist tendencies” (*ibid.*, p. 652). That account suggests that while neoclassical economists were getting economics rid of psychics, institutionalists “believed that it would be a mistake for economists to ignore psychology” (*ibid.*). Institutionalists like Mitchell, they claim, “believed that the incorporation of a more plausible psychology would make for better economics” (*ibid.*, p. 653).

As the following sections show, many of the complexities mentioned in this subsection become clearer by taking into account the problem of behavior control.

2. Including behavior control in the history of economics and psychology

Understanding the historical process leading Watson to write “Psychology as the behaviorist views it” (1913) is essential for grasping the meaning of behavior control. Similarly, later and more sophisticated versions of behaviorism, such as Skinner’s radical behaviorism, can be better understood by being analyzed through the problem of control in the history of psychology⁶.

J.B. Watson’s functionalism: from animal psychology to behaviorism

There is quite a consensus among historians of psychology about the fact that, despite German influences, functionalism, the main force shaping American psychology arose “not

⁵ Asso and Fiorito (2004) also qualify the “Slutsky school” as a form of “behaviorist mainstream economics”, influenced by “Frank Knight’s famous critique of the ‘Slutsky School’ in demand theory (Knight, 1944)” (Asso and Fiorito, 2004, p. 465). For Knight, the “diminishing ‘coefficient of substitution’ of one good for another” was a “purely behavioristic principle” (Knight, 1944, p. 289).

⁶ This section is based on the following writings about the history of psychology: Boring (1950, 1961, 1964), Madden (1965), Cravens and Burnham (1971), Daston (1978), Ash (1980), Herman (1995), Mills (1992, 1998), Capshew (1993, 1999), A. Rutherford (2003, 2006, 2009), Mandler (2007), Igo (2008) and Staddon (2014).

within psychology itself but within American society from about the 1880s onward” (Mills, 1998, p. 2). E.G. Boring (1950) explains the functionalist character of American psychology as follows:

“If influenced by your culture, you conclude that you should devote yourself to the description of nature, content to say *what* happens and *how* it happens, without asking the question *why*, then you are concerned with structure, are working in the descriptive tradition [...]. But if you ask *why*, if you try to understand causes, then you are interesting yourself in capabilities, in capacities, and are being a functionalist. It is as natural to be a functionalist as it is to want to predict, to be more interested in the future than in the past, to prefer to ride facing forward on the train. The future concerns you because you think you might change it if you have the ability. The past has gone by, lies there open to description but unalterable.” (Boring, 1950, p. 551)

The first school of functionalist psychology (i.e. the study of the adaptive role of mind in animal life) was established at the University of Chicago. Animated by John Dewey [1859-1952], George Herbert Mead [1863-1931], Addison Webster Moore [1866-1930] and James Rowland Angell [1869-1949], American school was quite immediately acclaimed by James:

“Chicago has a School of Thought! [...]. It coincides remarkably with the simultaneous movement in favor of ‘pragmatism’ or ‘humanism’ [...]. It probably has a great future, and is certainly something of which Americans may be proud.” (James, 1904, pp. 1, 5)⁷

The way in which historians of psychology consider behaviorism is interesting as it takes into account the connections between James, Dewey, Angell and Watson, rather than just differences between James and Watson, which is what economists tend to highlight. The Chicago school had a clear impact on the process leading to behaviorism (e.g. Watson studied philosophy under Dewey, and had Angell as supervisor)⁸, and consequently Watson’s approach was clearly functionalist. According to his 1913 paper, psychology

⁷ Add note about James and the development of functionalist psychology... Another department of functional psychology was settled at Columbia by James McKeen Cattell [1860-1944], Edward Lee Thorndike [1874-1949] and Robert Sessions Woodworth [1869-1962] (Boring, 1950).

⁸ Add note about Watson’s education.

should be based on the methods of comparative psychologists, by performing experiments in which the “entire life history of [the] subjects [was] under careful control” (Cravens and Burnham, 1971, p. 647). Watson “assumed that young animals had at their disposal a vast array of random movements and that habits emerged from that pool via a process of selection” (Mills, 1998, p. 57). From there, he aimed at developing psychology into an “experimental branch of natural science” (Watson, 1913, p. 158), the main goal of which was “the prediction and control of behavior” (*ibid.*). The following text extracts the essence of Watson’s ideas:

“I feel that behaviorism is the only consistent and logical functionalism [...]. The psychology which I should attempt to build up would take as a starting point, first, the observable fact that organisms, man and animal alike, do adjust themselves to their environment by means of hereditary and habit equipments [...]. Some time ago I was called upon to make a study of certain species of birds [...]. In order to understand more thoroughly the relation between what was habit and what was heredity in [their behavior], I took the young birds and reared them. In this way I was able to study the order of appearance of hereditary adjustments and their complexity, and later the beginnings of habit formation [...]. Had I been called upon to examine the natives of some of the Australian tribes, I should have gone about my task in the same way [...] if I had been called upon to work out the psychology of the educated European, my problem would have required several lifetimes. But in the one I have at my disposal I should have followed the same line of attack [...]. My final reason for this is to learn general and particular methods by which I may control behavior.” (*ibid.*, p. 167)

So more than just attacking introspection – or the study of mental states – for Watson, the study of behavior was important so as to be consistent with the functionalist (i.e. useful) character he sought for psychology:

“If psychology would follow the plan I suggest, the educator, the physician, the jurist and the businessman could utilize our data in a practical way, as soon as we are able, experimentally, to obtain them. Those who have occasion to apply psychological

principles practically would find no need to complain as they do at the present time.”
(*ibid.*, p. 168)

It is also interesting to realize that despite the title of his 1913 paper: “Psychology as the behaviorist views it”, historians of psychology consider that Watson’s “published animal work shows no trace of a behaviorist position” before at least 1917 (Mills, 1998, p. 57)⁹. Historical accounts by Cravens and Burnham (1971) or Mills (1998), draw the distinction between comparative psychology and behaviorism based on whether the experimental findings were socially applied. The “socially oriented and crudely speculative” behaviorism of Watson seems to have emerged only alongside his work on “the problem of human instincts in human infants” (Cravens and Burnham, 1971, p. 646), The following series of extracts describes Watson and Morgan’s (1917) “Emotional reactions and psychological experimentations”. They are intended to exemplify the following: (1) that emotional reactions (i.e. mental states) were not avoided but actually analyzed through behaviorist methodologies; (2) how those emotions arose from behavior; (3) how by attaching emotions to behavior psychology can be made useful; and (4) that behavior control was already employed in business practices.

- (1) “After observing a large number of infants, especially during the first months of life, we suggest the following group of emotional reactions as belonging to the original and fundamental nature of man: *fear, rage, and love* [...]. We use these terms which are current in psychology with a good deal of hesitation. The reader is asked to find nothing in them which is not fully statable in terms of situation and response. Indeed we should be willing to call them original reaction states, X, Y, and Z. They are far more easily observed in animals than in infants. While we do not claim that this list is complete, we do claim that our own observation of the first few months of infancy has not yielded any larger number” (p. 165)
- (2) “An individual hampers my use of my arms and legs, constrains me, holds me badly when dressing me, etc. (original conditions for arousing rage) – shortly the mere sight of that individual arouses the rage components. Finally an entire

⁹ According to Mills (1998), by 1913 Watson was, “quite explicitly, not calling for the creation of a new version of experimental psychology. Instead, he was asking his colleagues to apply the rigorous methods of experimental animal psychology to their work” (Mills, 1998, p. 65).

stranger whose behavior is even slightly similar to that of the first individual may set off the responses” (p. 169)

- (3) “emotions furnish the ‘drive’ for many forms of activity” (p. 170) [...] “If emotions can be attached [...], and if such attachment can be made to serve a useful end such as that of helping individuals to form necessary but prosaic habits, interesting outcomes may be expected from our work” (p. 172)
- (4) “many drives have been hit upon in a practical way already, by business houses such as threatening discharge (fear), by ridicule (rage), and by getting the individual attached to the ‘house’ through ‘loyalty’ (love)” (p. 174)

Watson and Morgan’s paper is a good example of the early behaviorism, which declined abruptly together with Watson’s academic career during the early 1920s (Mills, 1998, p. 75)¹⁰. Eventually, Watson’s approach, which was characterized by promoting the social application of the experimental findings, remained only a “far cry from [the] highly sophisticated and technical work” (*ibid.*) of later behaviorists like Skinner.

Skinnerian radical behaviorism

From the late 1930s to the 1970s, “neobehaviorists” produced “highly sophisticated and, in some cases, comprehensive psychological theories” (Mills, 1998, p. 4). Of these, Skinner’s were the most visible as he applied experimental findings to the study of popular subjects like baby care, infant teaching and social philosophy through mass media¹¹. As for the earlier behaviorists, behavior, for Skinner, was strictly connected to the growth of habit structures. Those structures were conceived of as resulting from repeated stimulus-response-reinforcement chains, or, in Skinnerian terms, the “history of past reinforcements” encountered by an organism in its interaction with the environment¹².

¹⁰ Add note about Watson’s career, and that of some other early behaviorists, like Guthrie, Weiss, Meyer and Kantor (Mills, 1998, p. 76).

¹¹ Add Skinner note.

¹² The history of past reinforcements was one of the main concepts manipulated by Skinner. Because that history supposedly shaped behavior patterns, Skinner aimed at showing how “seemingly cognitively controlled behaviors could be patiently shaped” (Mills, 1998, p. 124). Radical behaviorists, like Skinner, considered that “to want or desire something [was] to seek that which [had] secured positive reinforcement in the past” (*ibid.*, p. 139). Similarly, to “intend to do something”, could be conceived of as to “be guided by one’s history of past reinforcements” (*ibid.*, p. 139).

Skinner's machine – the Skinner box (Figure 3, below) – was an instrument providing controlled environments in which the initially random behavior of animals under experimentation (mainly hungry rats and pigeons) could be positively reinforced by providing them food through a mechanism. In that way, desired behaviors were obtained by designing different “schedules of reinforcement”. That “operant conditioning” process of behavior control was documented in the “behavior records” provided by the Skinner boxes.

Skinner's “system of behavior” was thoroughly presented in *The behavior of organisms* (1938). However, during and after World War II, Skinner became increasingly popular due to his efforts at applying his knowledge to human affairs. He aimed at developing social technologies by which his science could serve society.

In 1945, for instance, Skinner produced an “Air-Crib” or “mechanical baby tender”: a “labor-saving invention” designed for “the problem of the nursery” (Skinner, 1945, p. 29)¹³, and in 1948 he spread his ideas to the general public in *Walden two*, a utopian novel about an experimental community designed to deal with postwar problems like “the exhaustion of resources, the pollution of the environment, overpopulation, and the possibility of a nuclear holocaust, to mention only four” (Skinner, 1948, p. vi). According to Capshew (1993), that book represents the “first step in Skinner's public transformation from experimental psychologist to social philosopher” (Capshew, 1993, p. 836).

Skinner claimed that society should be built from “semantically transparent” concepts and values, which were useful for creating “thoroughly pragmatic” “value systems” (Mills, 1998, p. 153). In arguing that the sciences and technologies of behavior lacked progress compared to that of the natural sciences¹⁴, he showed how behavior “could be changed by changing its consequences” through operant conditioning: i.e. the use of positive

¹³ The “air-crib” was a “closed compartment about as spacious as a standard crib” where temperature and humidity were controlled and the passing air filtered so that the baby in care could be freed from clothing (except diapers), bedding, and excessive bathing so that mothers could gain “freedom for other activities” (*ibid.*, p. 32). The Air-Crib was by no means designed to be a “Skinner box”, for there were no experimental reinforcement devices attached to its mechanism. “Baby in a Box” was however the title under which Skinner's “inexpensive mechanization of baby care” (*ibid.*, p. 34) was presented in the *Ladies Home Journal*.

¹⁴ while “Aristotle could not have understood a page of modern physics or biology”, Socrates and his friends would have “little trouble in following most current discussions of human affairs” (Skinner, 1971, p. 6)

reinforcements (Skinner, 1976, p. viii)¹⁵. He thus proposed to control human behavior by using neither coercive measures nor punishment:

“To induce people to adapt to new ways of living which are less consuming and hence less polluting, we do not need to speak of frugality or austerity as if we meant sacrifice. There are contingencies of reinforcement in which people continue to pursue (and even overtake) happiness while consuming far less than they now consume.” (Skinner, 1976, p. x)¹⁶

However, Skinner’s proposal was widely debated and strongly criticized (A. Rutherford, 2000, 2003, 2009). For Skinner, the negative reception of his ideas proved the “uneasiness with which” government was viewed when attempting to control behavior by means of positive reinforcements instead of punishments (Skinner, in Rogers and Skinner, 1956, p. 17). He claimed that the hostility to his ideas revealed a generalized social misunderstanding of concepts like “volition”, “free-will” and “freedom”. Because all “men control and are controlled”, the question to ask was not how freedom should be preserved, but “what kinds of control [were] to be used and to what ends” (*ibid.*). Freedom, or in Skinner’s terms, behavior that “feels free”, was *always* “the product of a history of conditioning” (Skinner, 1964, p. 483):

“The problem is to free men, not from control, but from certain kinds of control, and it can be solved only if our analysis takes all consequences into account. How people feel about control, before or after the literature of freedom has worked on their feelings, does not lead to useful distinctions.” (Skinner, 1971, p. 42)

¹⁵ *Beyond Freedom and Dignity* (1971) – a best-seller and Skinner’s “most widely debated book” (A. Rutherford, 2006, p. 204) – was “socially” rather than “scientifically” contested. His detractors did not charge against its “scientific validity”, but against its “serious affront to traditional value systems”: “renouncing the freedom, dignity, and autonomy of human beings” (A. Rutherford, 2000, pp. 385-386). According to historians of psychology, that episode represents a “distinctly illustrative” example “of the processes through which psychological science and its products” are “shaped, regulated, and modified by the society in which they are embedded” (A. Rutherford, 2006, p. 218).

¹⁶ Although *Walden two* did not “sell well” until the end of the 1960s (A. Rutherford, 2000, p. 382; Mills, 1998), over 2 million copies were sold during the 1970s, promoting vivid discussions around the social use of behavior technologies (*ibid.*). In the introduction of its 1976 edition, Skinner acknowledged that his utopian novel was influenced by his own post-war problems. The world, he wrote: “was beginning to face problems of an entirely new order of magnitude [...]. Physical and biological technologies could, of course, help [...]. But that would happen only if human behavior changed, and how it could be changed was still an unanswered question” (Skinner, 1976, pp. vi-vii).

3. Were the ordinalists behaviorists?

No. This section shows that even by methodological standards, what economists proposed during the 1930s and 1940s was far removed from behaviorism. Economists seem to think that the ordinalist turn was behaviorist mainly because of Samuelson's references to "operationalism"¹⁷. But behaviorists like E.C. Tolman or B.F. Skinner applied Bridgman's methodology within programs which were fundamentally different from that of Samuelson and his followers.

Samuelson's operationalism and the human guinea-pig

It is clear that Bridgman's methodology was a reference for Samuelson as it was for behaviorists like Tolman and Skinner (Boring, 1950; Mills, 1992, 1998; Hands, 2004). According to Hands (2001), Samuelson's "stated economic methodology" was both "operationalist" and "descriptivist" (Hands, 2001, p. 61):

"The core operationalist idea is that a question has *meaning* only if there exist a set of operations that will provide a definitive answer to it. Correspondingly, a concept or term is *operationally meaningful* if it can be characterized by a particular set of operations, and the meaning of a concept or term is *defined* by that set of operations [...]. A second feature of Samuelson's methodological position (also shared with Bridgman) is a *descriptivist* view of scientific theories; scientific theories *merely describe* the empirical evidence and do not go beyond the evidence to *explain* any deeper, underlying, or hidden causes of the phenomena." (*ibid.*, pp. 62-63)

In a series of writings, Hands (2001, 2004, 2010, 2014) has shown that even though Samuelson aimed at deriving operationally meaningful theorems in his 1938 paper (*ibid.*, p. 63), he eventually changed "the methodological target in the period between the publication of the original paper in 1938 and (say) the late 1940s" (*ibid.*, p. 67):

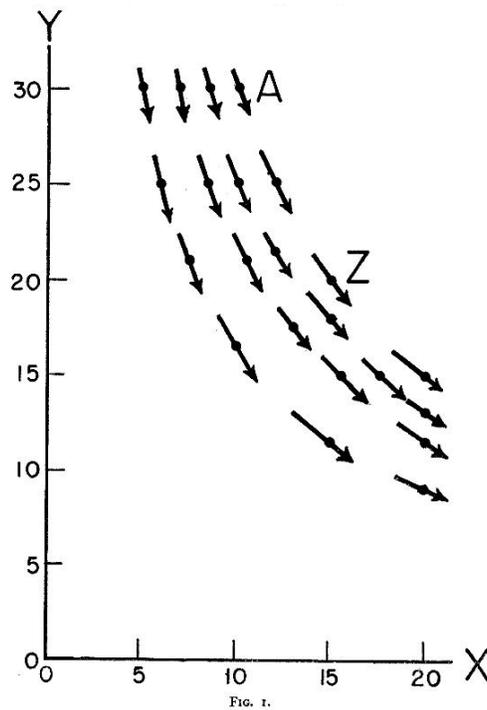
"In the beginning the goal was clearly to *purge* the mentalistic concept of utility and *replace it* with something that was scientifically more acceptable (something more operationally meaningful) [...]. Samuelson did *not* use the word *preference* (or any

¹⁷ See Lewin (1996), Giocoli (2003), Hands (2010), and their references, for accounts of the ordinalist turn by I. Fisher, W. Pareto, E. Slutsky, and J. Hicks and R. Allen, and P. Samuelson.

surrogate for it) in the original paper (or in *Foundations*) [...]. Samuelson circa 1938 was (only) concerned with observable *behavior* and eliminating (not revealing) *unobservable intentional* concepts from the theory of consumer choice. The problem for the consistency of his methodological position is that later he did change to talking about *revealing preferences*” (*ibid.*, pp. 67-68)

Hands (2014) identifies a “Das Paul Samuelson Problem” and shows that there is a “stark difference” between Samuelson’s “Note on the pure theory” (1938) – which attempted to drop off “the last vestiges of the utility analysis” (Samuelson, 1938, p. 62) – and what he attempted around a decade later: i.e. to “infer whether a given batch of goods is preferred to another batch” (Samuelson, 1948, p. 243).

FIGURE 1 (Samuelson, 1938, p. 244)



It is now clear that the “human guinea pig” introduced by Samuelson in 1948 (*ibid.*, p. 250) was part of an “operationalist-inspired” project by a young economist, which was not only based on the mentalist concept of preference, but which also lacked of solid empirical

foundations. Samuelson's "guinea-pig" was only theoretically supposed to reveal its "preference pattern" through "market behavior" (*ibid.*, p. 243)¹⁸.

According to Hands, it is "well-established" – at least among historians of economics – that "the operationalist-inspired project of basing demand theory on revealed preferences was a failure from a variety of different perspectives" (Hands, 2004, p. 962):

"Not only did the original project of purging preference and utility from economic analysis fail but the later goal of providing a practical way of 'revealing' those preferences was also unsuccessful even on its own (revised) terms. Whatever the reasons are that economists believe in demand theory, the claim that revealed preference theory has provided consumer choice theory with incorrigible operational/empirical foundations doesn't seem to be – or certainly shouldn't be – one of them." (*ibid.*)

In addition to that, recent research about the history of expected utility theory shows how the concept of cardinal utility was rehabilitated during and after the 1950s, within research programs which were unrelated to behaviorism (see Moscati's essay in this volume).

P. Bridgman and the pragmatic tradition

It has been argued that something similar to the revealed preferences episode happened in psychology at the same time. In Hands' (2001) terms:

"It is interesting, though admittedly getting a bit ahead in our story, that precisely the same movement seemed to take place with respect to operational concepts in psychology during the same period [...]. What happened over time though, as with Samuelson, was that the new and ostensibly more operational concepts ended up

¹⁸ Samuelson's (1948) argument relied indeed on Gestalt psychology rather than behaviorism. When developing "the numerous little arrows" (Figure 1) he "observed" into "behavior curves" (i.e. revealed preferences indifference curves), he referred to the "well known observation of Gestalt psychology that the eye tends to discern smooth contour lines from such a representation" (*ibid.*, p. 245). From there, he proceeded by pointing out "an exact mathematical counterpart of this phenomenon of Gestalt psychology" (*ibid.*).

being used to defend and put a new scientific sheen on the traditional concepts, rather than as a replacement for them” (Hands, 2001, p. 68)¹⁹

That way of reading the impact of Bridgman’s methodology on both economics and psychology would seem to support the view that mainstream economics became behaviorist along the ordinalist/operationalist turn. Hands claims that, at least in principle, for both “Samuelson and the behaviorists, science exclusively involve[d] the theoretical redescription of given empirical observations, and since purpose and intention are not observable in this sense, they ha[d] no place within science” (*ibid.*, p. 961).

In addition, however, Hands (2004) shows that the diversity of operationalist approaches in the social sciences was “much greater than commonly recognized” (*ibid.*, p. 954). He highlights John Dewey, for whom “the main operationalist message” was “precisely the opposite of the message promoted by Samuelson in economics and by mid-century behaviorists in psychology” (*ibid.*), and in line with philosophers who see the “link between Bridgman’s operationalism and the pragmatic tradition” (*ibid.*, p. 959), he shows that for Dewey, scientific operations were “directed and purposeful”, and also “intelligent precisely because they serve[d] human designs” (*ibid.*, p. 961):

“The task of pragmatic reason is not to discover the essence or true nature of the objects of inquiry but rather to be successful in the active interaction with nature, and that success requires anticipation, deliberation, and intentional operations [...]. For Dewey ‘the evidence’ is not simply ‘given’ by nature; it is always interest laden and a product of active human operations” (*ibid.*, p. 960)

It is thus possible to emphasize “how *diametrically opposed* Dewey’s version of operationalism” was to the “project envisioned by Samuelson and various behaviorists within psychology” (*ibid.*, p. 961)²⁰:

¹⁹ Or yet in different terms: “in psychology, as in economics, the initial, quite radical operationalist ideas eventually came to serve as little more than a ‘reassurance fetish’ [...] for mainstream methodological practice” (Hands, 2004, p. 958, reference omitted)

²⁰ Hands’ account remains however restricted to just two different uses of Bridgman’s methodology. On the one hand, that related to Dewey who saw science “as something uniquely and enthusiastically human” (*ibid.*, p. 962), and on the other, that of “Samuelson and the others” who aimed at “precisely the absence of the human, the disinterestedness of [their scientific] method” (*ibid.*).

“Dewey employed the concept of operations to *give purpose and intentionality a legitimate role in scientific inquiry, while Samuelson and others employed it to get purpose and intentionality out of science*” (*ibid.*)

The operationisms of E.G. Boring, E.C. Tolman, and B.F. Skinner

Quite unsurprisingly – given the diversity of psychology as a research field – Bridgman’s methodology served multiple purposes among behaviorists, as references to “operationism” (i.e. the psychologists’ adaptation of Bridgman’s approach) spread throughout psychology even beyond behaviorism. Bridgman’s methodology provides indeed “the major connecting thread” between behaviorism and cognitive psychology: “cognitive psychologists are as deeply committed to operationism, and thus to positivism, as the neobehaviorists were” (Mills, 1998, p. 191).

According to historians of psychology, operationism was introduced by E.G. Boring (who was not a behaviorist), and students of him like B.F. Skinner and S.S. Stevens (Green, 1992; Mills, 1998)²¹. Mills (1998) shows that, for psychologists, operationism meant redefining “mentalist concepts in some objective way” (Mills, 1998, p. 86), which involved producing “causal accounts” of behavior. In order to produce such accounts, behavior should be under the careful control of the experimenters:

“An operationist believes that to understand is to give causal accounts that leave no room for the action of forces lying outside the physical realm. We can give causal accounts only if we can control the situations in which identifiable phenomena occur [...]. One limited one’s observations to dependent variables, which can be defined as physicalistically defined outcomes (behaviors produced in carefully specified conditions). One stringently controlled the situations producing those outcomes by devising procedures for eliminating or randomizing the effects of various background variables. One also controlled the conditions instantiating ‘hidden’ factors such as motives, expectations, values, attitudes, and the like. Then one studied the effect of manipulations of the strength of variables triggering action under pre-specified conditions (that is, one instantiated independent variables). One then said that one had

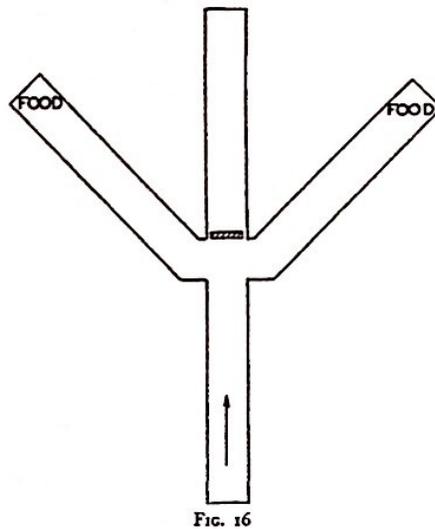
²¹ Add note about the operationisms of E.G. Boring and S.S. Stevens.

provided a causal account when the obtained outcomes matched the predicted outcomes” (*ibid.*)

Controlling for “hidden factors” was thus essential for producing operational behavior accounts, which was what behaviorists like Tolman and Skinner aimed at. Moreover – and in line with Dewey’s operationalism (Hands, 2004) – these behaviorists clearly worked with evidence which was the “product of active human operations” (Hands, 2004, p. 960).

Tolman, who was arguably the least behaviorist among behaviorists, is nowadays considered a pioneer in cognitive psychology (see Staddon 2014)²². His approach has been classified both as “purposive behaviorism” and “cognitive behaviorism” (Kimble *et al.*, 1991), for his theory included the “treatment of purpose” and was influenced by Gestalt psychology (Mills, 1998, p. 94). Tolman’s views “derived directly” from the New Realism he learned at Harvard from E.B. Holt and R.B. Perry (*ibid.*, p. 33), which “crucially differentiated his theory from Watson’s” (*ibid.*, p. 95)²³. However, just like radical behaviorists, Tolman kept the concept of learning at the core of his program. One could never observe “raw purpose”, but only “raw action” (*ibid.*). Purposive behavior was thus produced, and “emerged as a function of training” under the careful control of the experimenter.

FIGURE 2 (Tolman, 1938, p. 22)



²² Add Tolman note.

²³ Add note about Tolman’s education as influenced by the New Realists.

Tolman's instrument was the rat maze (e.g. Figure 2), and thus Tolman's observations were produced by running such animals through multiple kinds of mazes. He aimed at understanding "why rats turn the way they do, at a given choice-point in a given maze at a given stage of learning" (Tolman, 1938, p. 1), and in doing so he developed the concept of "cognitive map". Finding that rats produced cognitive maps of mazes, implied that they were able to learn the configuration of a maze even in absence of rewards (i.e. Tolman's "latent learning"). Conceiving of cognitive maps reveals the influence from Gestalt psychology (Hothersall, 1995) that made Tolman's approach different from that of radical behaviorists like Skinner.

And it was indeed Skinner who "took the final step needed to produce the operationism of modern psychology" (Mills, 1992, p. 76) by applying operationism in the most radical way" (*ibid.*). He eliminated "motivational states" (i.e. Skinner's "hidden factors") by introducing a concept of "drive", which was defined "in terms of operations carried out by an experimenter" (Mills, 1992, p. 76). The drive "hunger", for instance, was defined as the "reduction of an animal's body weight to 80% of the free-feeding level or placing the animal on a 23 1/2-hour feeding schedule" (*ibid.*). Motivational states were thus replaced with operations that could "be very precisely correlated with behavioral outcomes (such as the rate and the pattern of bar-pressing or key-pecking in a Skinner box [e.g. Figure 3])" (*ibid.*). Because Skinnerian behaviorists had precise control not only over the tasks performed by the animals, but over all the "relevant aspects of [the] animal's lives", they could predict, "no matter in what laboratory [...], precisely what the outcome of those tasks would be" (Mills, 1992, p. 77).

FIGURE 3 (Skinner, 1938, p. 49)

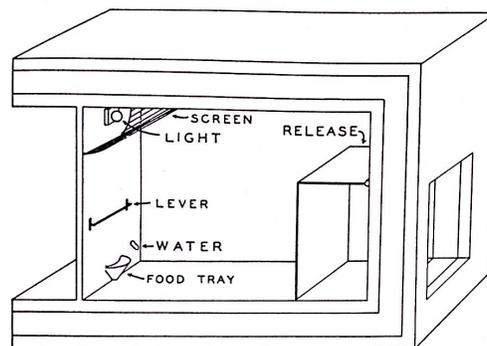


FIGURE I
A TYPICAL EXPERIMENTAL BOX

Skinner's aim was achieved by "treating operationally defined constructs as the means for producing desired forms of behavior" (Mills, 1998, p. 101). But even though Skinner's program effectively avoided mentalism, it arguably failed because of defining drives "solely in terms of the operations carried out by the investigator" (Mills, 1992, p. 77):

"a Skinnerian experiment became no more than an empirical check on the internal consistency of the concepts. No possibility remained for discovering anything that was not stipulated in advance" (*ibid.*)

Whatever the methodology, however, one of the main traits of behaviorism, as proposed by Watson and Skinner, was the avowed interest in "developing behaviorist-based social technologies" (Mills, 1998, p. 102). Based on that fact, Mills (1998) claims that postwar behaviorism had a two-tiered structure, which was connected by Bridgman's methodology and the commitment to control:

"Once operationism had reached its full development in psychology, a two-tiered differentiation of researchers was possible. On the one hand, there was a role for the Tolmans, whose primary interest was theoretical (but who were always aware of the possible practical applications of their work). On the other, we had those (especially Skinner and his followers) whose primary interest was in developing behaviorist-based social technologies. A commitment to social control linked the two tiers of behavioral science" (Mills, 1998, pp. 101-102)

4. Discussion: behavior control elements in economics

It has been argued throughout this text that, despite claims to the contrary, utility theory has always been far removed from behaviorism. Alternatively, behavior control elements can be found in the institutionalism of T. Veblen, W.C. Mitchell, J.M. Clark, L.K. Frank, and M.A. Copeland, as well as in some current forms of behavioral economics. These last ones are interesting, for they explicitly avoid dropping purposiveness and volition, while acknowledging that decisions are determined (i.e. controlled) by the contexts in which they happen.

Institutionalism and the backward art of spending

Accounting for “extravagant” (i.e. not rational) spending behavior was one of the main elements of the early institutionalism of Veblen (1898, 1899), Mitchell (1910, 1912, 1914) and Clark (1918). That program was based on the naturalist idea that man is an agent that acts “in response to stimuli afforded by the environment in which he lives”, and that like other animal species, he is “a creature of habit and propensity” (Veblen, 1898, p. 188). These institutionalists were critical towards the approach they referred to as “neoclassical economics” (Veblen, 1900), or “mechanics of self-interest” (Mitchell, 1910). They claimed that economists should not build their theories of behavior out of “a few principles of human nature” (Mitchell, 1910, p. 97), but rely on the psychology of those who had “specialized in that field” (Clark, 1918, p. 4). Unlike the deductive accounts of utility theorists, these institutionalists aimed at approaching functionalism: a “positive science of conduct” that will give birth to “an evolutionary natural history of mind” (Mitchell, 1910, p. 100)²⁴.

For Mitchell, neoclassical economics: i.e. the mechanics of self-interest, had strictly nothing to say regarding the arts of spending. Alternatively, he considered McDougall’s (1908) functionalist approach as adequate to explain such behavior:

“the assumption of rationality fits the activities of consumption nowhere outside of economic treatises [...]. Passing whims, carelessness about prices, ignorance of qualities, obstinate preference for old ways are left wide scope. In McDougall’s terms, habit, suggestibility, and the instincts of emulation and imitation must be brought in, if we are to account for our own subservience to fashion, our conspicuous waste, and our slovenly dependence on the advertiser. The assumption of rationality is inadequate to explain the facts.” (*ibid.*, p. 200)

²⁴ The main reference for institutionalists was McDougall’s *Introduction to Social Psychology* (1908): “From Mr. McDougall’s standpoint the simple psychological premises of [the] mechanical type of economics are wholly inadequate, if not radically mistaken [...]. For the mechanics of self-interest, like its prototype, rational mechanics, does not profess to take into account complex reality [...]. While economists of that mental bent which is peculiarly sensitive to the claims of logical order and precision have been perfecting the mechanics of self-interest, their colleagues of a realistic turn have sought to keep economic science in close touch with economic life. To men of the latter temperament, logical precision smacks more of scholasticism than of science when attained by sacrificing faithfulness to fact.” (*ibid.*, pp. 109-110) Add note on Mills (1998, p. 39).

While these institutionalists initially proposed studying human behavior “from the evolutionary view-point, in the light of functional psychology” (*ibid.*), they eventually turned to behaviorism, just like most functionalists did²⁵. Mitchell (1914) claimed that economics should cease being “a mechanical study of static equilibria under non-existent conditions, and become a science of human behavior” (Mitchell, 1914, p. 47). Such arguments proliferated during the interwar period, especially by Mitchell (1925), L.K. Frank (1923, 1924a, 1924s, 1925) and M.A. Copeland (1925, 1926, 1951), who promoted a sort of “behaviorist institutionalism” (Asso and Fiorito, 2004; M. Rutherford, 2000, 2002): a quantitative approach attempting to replicate the work of American psychologists who were “moving rapidly toward an objective conception and a quantitative treatment” of human behavior (Mitchell, 1925, p. 6):

“Their emphasis upon stimulus and response sequences, upon conditioned reflexes; their eager efforts to develop performance tests, their attempts to build up a technique of experiment, favor the spread of the conception that all of the social sciences have a common aim – the understanding of human behavior; a common method – the quantitative analysis of behavior records; and a common aspiration – to devise ways of experimenting upon behavior.” (*ibid.*)

About behavioral welfare economics and “nudging”

Moving back towards the beginning of this essay, it must be noted that behavioral economists are not only “motivated by the belief that people often fail to act rationally”, but they “have always been interested in how people’s decision making can be improved” (Angner and Loewenstein, 2012, p. 677). Among these, Thaler and Sunstein (2003, 2009), explicitly attempt to apply elements “borrowed from psychology” to figure out how to possibly improve spending behavior (Thaler and Sunstein, 2009, p. 20)²⁶.

Interestingly, they do so borrowing elements from behavior control, just like those present in the institutionalism of Veblen, Mitchell and Clark (who also explored the arts of spending), and also related to the programs developed by Watson, Skinner and their followers. Thaler and Sunstein’s *Nudge* (2009) points out “choice architectures” as being

²⁵ Add note about *The battle of behaviorism* (Watson & McDougall, 1928)

²⁶ Add note about “Libertarian paternalism” (2003), *Nudge* (2009), and the behavioral economics literature.

“nudges”, which may help people improve their behavior. While admitting that “there is no such a thing as a ‘neutral’ design” (i.e. no neutral “choice architecture”) for the contexts “in which people make decisions” (*ibid.*, p. 3). They, however, claim that people should be “free to choose”! (*ibid.*, p. 5).

Just like Skinner (1971) did, *Nudge* claims that “better governance requires less in the way of government coercion and constraint” (*ibid.*, p. 15), which should be replaced by “incentives and nudges” (*ibid.*). Surprisingly, however, Thaler and Sunstein make no reference to the literature on behavior control, although admittedly using elements “borrowed from psychology” (*ibid.*, p. 20). Following Davis’ (2006) account (an extract of which opens this essay), one may interpret that absence as resulting from a lack of interest in “engaging the relevant professional literatures in depth” (Davis, 2006, p. 12). Or is it that by presenting their views in terms of “libertarianism”, “choices” and “nudges” they aim at belonging to mainstream economics?

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