Diego Zilio · Kester Carrara *Editors*

Contemporary Behaviorisms in Debate

centro paradigma ciências do comportamento

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Preface

The year 2020 marked the 30th anniversary of B. F. Skinner's death, one of the most important psychologists of the twentieth century. Skinner was the central figure in the development of behavior analysis and its philosophical framework known as "radical behaviorism." Skinner's radical behaviorism generated descendants, forms of behaviorism in some ways significantly different from radical behaviorism, yet not that different so that they clearly remain within a behavioristic world view. This does not mean, of course, Skinner's behaviorism is a thing of the past. On the contrary, it still is the principal philosophy behind behavior analysis in its experimental and applied domains. The various behaviorisms discussed in this book only add to the movement as whole by showing that behaviorism is not a monolithic or static position; quite the opposite, it is a dynamic movement, changing and adapting in the face of new questions, issues, and perspectives. The death of behaviorism has been proclaimed since its early days—a "premature" assessment, to say the least—but this volume shows that behaviorism is alive and kicking, even 30 years after its main proponent passed away.

This book contains seven sections, each one dedicated to a particular variation of contemporary behaviorism: Howard Rachlin's teleological behaviorism, William Baum's molar behaviorism and multiscale behavior analysis, John Staddon's theoretical behaviorism, John Donahoe's biological behaviorism, Gordon Foxall's intentional behaviorism, Steven Hayes' contextual behaviorism or contextual behavioral science, and Emilio Ribes-Iñesta's field-theory behaviorism. Each section contains three chapters. Written by the original proponents of those forms of behaviorism, the first chapters introduce the reader to the main characteristics of each proposal. Following these, we have commentary chapters written by prominent Brazilian behavior analysts about those forms of behaviorisms. Each section ends with the proponents of those forms of behaviorism replying the commentaries.

In a sense, this book is organized in a "target paper" structure in which we have the target chapters, the commentaries, and the replies totalizing 21 chapters. More than providing to the reader an introduction to contemporary forms of behaviorism, this book also promotes debate about the main philosophical issues faced by the field of behavior analysis today—issues that can directly influence future epistemological variations in the selection process of "behaviorisms." By doing so, the book is directed not only to the present, but, more importantly, toward the future of the field.

Vitória, Brazil Diego Zilio
Bauru, Brazil Kester Carrara
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Part I
Teleological Behaviorism

Chapter 1
Teleological Behaviorism: Origins and Present Status

Howard Rachlin

Why I Am a Teleological Behaviorist

I was not a behaviorist at all until years after I received my PhD at Harvard. My undergraduate degree (from Cooper Union in New York City) had been in mechanical engineering. I worked as an engineer but hated the repetitive nature of the work. And friends of mine who were then graduate students seemed to be living a better life. So, I enrolled at the New School for Social Research (also in New York) where you could get a graduate degree by taking classes in the evening while working during the day. I originally majored in philosophy, but I found that subject to be too abstract after engineering and switched to psychology. That was around 1960; I was 25 years old.

Philosophy and psychology at The New School were heavily influenced by European refugees—phenomenologists and gestalt psychologists—hired to form the Graduate Faculty around that time. Although they had their influence on me, the courses I took were the standard ones in perception, animal learning, motivation, and physiological psychology. I loved studying at The New School. I was doing well and, after engineering, the relaxed atmosphere was just what I needed. I met my wife Nahid (now a successful novelist and short story writer) in a physiological psychology class. The teacher in that class, Marianne Simmel, had worked at Harvard. I was just about to receive a master's degree from The New School and intended to stay on for a doctorate. But she advised me to apply to Harvard. She said

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that a doctorate from Harvard was like having rich parents—it gave you an unfair advantage—and I have found that to be true.

Insofar as the amount of work required, Harvard was much more like Cooper Union than like The New School. It took me several months to fully absorb that fact, but eventually I got used to it. Besides, I was married, and the social aspects of school were less important to me than they had been. In 1962, my first year as a Harvard psychology graduate student, there were essentially two laboratories where you could work—S. S. Stevens' psychophysics laboratory and Richard Herrnstein's operant conditioning laboratory. They were located on either end of the tile-walled basement of memorial hall (a former dining hall; the basement had been the kitchen). George Miller and Jerome Bruner had joint appointments in the psychology and social relations departments and advised students, but their offices were elsewhere, and they did not take part in the daily life of the department, which Stevens ran with an iron hand. Skinner did have an office in the operant-conditioning end of the basement. He also taught a few weeks of proseminar (a required course for first-year students, in which each faculty member taught for a few weeks), but he did not directly advise graduate students, was often away, and no longer had anything to do with the laboratory—as far as we students were aware.

There were about 15 graduate students assigned desks in the warren of windowless basement rooms. They included Ed Fantino and John Staddon who had come the year before and, in my year, Billy Baum (at the desk across from mine in Stevens' end of the basement), Phil Hineline, and Al Neuringer. Peter Killeen came the year after. The atmosphere was competitive but in a good way. We argued with each other (to the point of screaming, in the case of Billy and me), but the arguments were about psychology and never personal. And we also helped each other with the heavy loads of facts and theories on which we would be frequently tested.

At first, I was more interested in psychophysics and sensation—Stevens' area—than in operant conditioning—Herrnstein's area. My first publication was a psychophysical study of the relation between subjective velocity, distance, and time. Over my 3 years as a graduate student, I had done enough psychophysics experiments for a thesis. But I also had been doing experiments in Herrnstein's lab. (Herrnstein was a kind and generous teacher and very, very smart. I believe that there is no one who can make such a judgment better than someone such as myself who knew him intimately as a student, colleague, and friend.) The excitement in the lab and the interests of most of my fellow students was centered in Herrnstein's lab, and I naturally gravitated in that direction. The title of my thesis was: Habituation to Mild Punishment in The Pigeon. (It was cited by a speaker at the Radcliffe graduation ceremony as a prime example of how we are getting to know more and more about less and less.) I was not a behaviorist. I was not interested in defending Skinner's views against Noam Chomsky and others who were attacking them. I regarded my research as a sort of game, which I enjoyed playing.

By the fall semester of 1964, all the psychology-department faculty but Herrnstein (who had just been promoted to associate professor) were full professors. There were no assistant professors, who could be relied on to teach the courses. George Miller, the chairman, convinced the dean to hire five new assistant professors at once from the current crop of psychology PhDs around the country. One of those would be an operant conditioner to be chosen by Herrnstein. Although there was a bias against hiring their own students, he felt that Harvard was so predominant in operant conditioning that an exception should be made—and so they offered me the job. I had another offer—from Columbia in New York City, where I preferred to live—but it was too tempting to stay where I was, and where I had become so comfortable, so I accepted.

The appointment was for 5 years. I still had to take my oral exam and fulfill my foreign language requirement to receive my PhD, but, since I was already hired, these became formalities. This was a lucky thing for me. The oral exam was no problem, but my foreign language skills were and are terrible (one of the reasons I became an engineer in the first place). The last phase of the language exam was supposed to be a conversation with a person fluent in the language—French in this case. Skinner was fluent in French, and he administered the exam. He would say something to me in French; I would laboriously translate it to English in my head, compose a response, translate that to French in my head, and finally say it with my terrible accent. And he would immediately respond. After about 10 min of this farce, he smiled and said, "Howie, get out of here," and I left.

So, in the fall of 1965, I began my teaching career at Harvard, which lasted for 4 years. At my first faculty meeting, I proposed getting rid of the language requirement, and Skinner supported me, but it was voted down. Herrnstein encouraged me to apply for a grant from the National Science Foundation (NSF). I did so, and the grant was funded. My research has been supported almost continuously by grants from NSF and the National Institutes of Health (NIH) to the present day.

Three events from my years as an assistant professor at Harvard, two involving Skinner, may be worth noting. One day, I looked in my mailbox and found a draft of a book he had just written (I forget which one) with a note asking for my comments. I knew he had also solicited comments from several others on the faculty but nevertheless took the assignment very seriously. I read the manuscript carefully and made several pages of comments, which I then deposited in his mailbox. About a week later, I found a note in my mailbox from Skinner's secretary saying that Professor Skinner would like to see me. I went immediately to his office. He greeted me warmly and thanked me for several of the grammatical suggestions I made. I asked him what he thought of my other comments, which mostly consisted of reservations that he was overextending findings from the animal lab to complex issues of human behavior. He rejected those. He said that I was too much of an operationist and would never have any fun as a psychologist. Remember, I was not really a behaviorist, still less a Skinnerian at that time (I call myself both a behaviorist and a Skinnerian now); so, Skinner's criticisms did not alter my behavior. I felt I was playing a game, one that I enjoyed extremely, and was willing to let NSF worry about whether and how my work could be applied in the real world.

Another interaction with Skinner from those years gives me more to regret. To quote myself (Rachlin, 1995):

> There were several points at which [the "wall" between Skinner and me] might have been breached. One of them was when I had just become an assistant professor at Harvard and Skinner had just retired from teaching. He would have been willing to conduct a small graduate seminar provided I would nominally teach the course and handle the grading and other administrative affairs. When you become an assistant professor in the same department where you were a graduate student, people tend to treat you like a graduate student. This plus my own lack of confidence resulted in an abnormal and largely self-defeating concern about status. I had written to the dean formally canceling one of my classes in retaliation for a parking ticket and now I indignantly refused to act as Skinner's TA. As I look back on the incident I see it as an attempt by Skinner to form a closer relationship with a colleague. Had he even implied such an intention I would have jumped at the chance, but I was too immature and Skinner was too "bumpy." His request to me was conveyed formally through the department chairman—and so the course was not taught. (pp. 373–374)

The third story: One day there was a knock on my office door. A Harvard Medical School student named George Ainslie wanted to see me about doing an experiment. He had been an undergraduate at Yale studying with Neil Miller and Frank Logan. Based on studies of rats running down straight alleys with food at the end, they had developed a complex equation to describe how the value of a reward decreases as its delay increases (a delay discount function). Ainslie had noticed an interesting property of this function—that preference for a larger-later reward (LL) over a smaller-sooner reward (SS) could reverse as time came closer and closer to the choice point. Thus, even though, at a distance, LL was preferred, SS would ultimately be chosen. Ainslie's idea was that, at a sufficient time prior to the LL vs. SS choice point, an animal would choose to make a response (if one was available) that would later remove SS as an alternative—thus "committing" the rat to obtaining LL. I helped Ainslie set up the experiment but with pigeon subjects pecking keys; the results were just as he predicted. This experiment formed the basis for my interest in self-control, which lasts to this day. I included a description of it in the first edition of my book, *Introduction to Modern Behaviorism* (Rachlin, 1970). Ainslie, as the reader may know, went on to write two highly insightful books about self-control: *Picoeconomics* (1992) and *Breakdown of Will* (2001).

At the end of our fourth year (1969), it became clear that none of the five assistant professors, hired for 5-year terms, would be nominated for tenure. I could have stayed on at Harvard as a "lecturer" for a further 3 years, but I felt that it would be better for my career to go someplace where my chances of tenure would be better. I had heard that the State University of New York at Stony Brook was looking for someone at the associate-professor level. Stony Brook was building a strong clinical program with a decidedly behavioral orientation. They needed someone to teach basic behavioral science. The village of Stony Brook is on Long Island, reasonably close to New York City—where I wanted to be all along. I applied for the job and was hired. If Harvard ever decided to offer me tenure, they could do it just as well from a distance as from under their noses—but they never did. I remained at Stony Brook for 35 years until my retirement as a "distinguished professor" in 2004. Although no longer teaching, I have continued to do research and am now a "research professor."

At about the time I came to Stony Brook, the "cognitive revolution" was beginning, and behaviorism began to lose its prominence in American academic psychology. (Today, aside from myself, there are no behaviorists at Stony Brook, even among the clinical psychologists, even as applied behavioral practices flourish throughout the United States and elsewhere, and the Association for Behavior Analysis International [ABAI] continues to grow.)

Still, in 1969, I was not a behaviorist. But, since behaviorism was being challenged so vehemently by the members of the cognitive revolution, it occurred to me that I ought to at least be able to defend it in arguments with my colleagues—or, if I could not defend it, then perhaps to join some of my colleagues in the experimental area who were renouncing behaviorism. I had always been interested in philosophy. As I said, I majored in philosophy at The New School before I switched to psychology. I had read *The Concept of Mind* by Gilbert Ryle (1949) and *Lectures in Pragmatism* by William James (1907/1955) as an undergraduate (as part of Cooper Union's excellent humanities program). Now I read some modern philosophers of mind, but I found their arguments unconvincing. I also read Wittgenstein's *Philosophical Investigations* (1958) which I believed and still believe is a behaviorist manifesto. Then I began to read extensively in the works of Plato and Aristotle. Despite Plato's reputation as "the inventor of the mind," I found him to be surprisingly behavioristic and eventually wrote an article to that effect called, "Maximization Theory and Plato's Concept of the Good" (Rachlin, 1985), which was published in the journal, *Behaviorism* (now, *Behavior and Philosophy*).

Between my years at Cooper Union and The New School, I had spent a semester as a philosophy student at Columbia University (I left because I needed to work full time, which could easily be done at The New School). At Columbia, I had taken a course taught by J. H. Randall, one of the world's foremost Aristotle scholars. He believed that behaviorism, as a philosophy of mind, was the modern movement closest to Aristotle. Remembering that, I began to read Aristotle. I read *Posterior Analytics, De Anima, Metaphysics, Physics*, and, most crucially, *Nicomachean Ethics*. Aristotle clearly was a behaviorist—but his behaviorism was apparently different from Skinner's. Rather than denying the usefulness of mental terms in a science of behavior, Aristotle asserted that they were crucial to such a science. However, Aristotle's idea of what constituted a science was vastly different from the efficient-cause-based sciences underlying nineteenth-century physics. His psychology was based on final causes—teleology. I found it to be compelling and now call myself a teleological behaviorist. Although Skinner certainly would have denied that his operant conditioning bore any resemblance to teleological behaviorism (TEB), there is I believe a close connection between them. I ended my essay on Skinner quoted above (Rachlin, 1995) as follows:

> What then is Skinner's lasting contribution? Not, I think, his utopian vision of a self-experimental society, nor the educational technology, nor a highly successful mode of psychological therapy based on behavioral consequences, nor the Skinner box and a host of other useful inventions, nor his contribution to pharmacological testing, nor the journals and societies based on his work, nor the individuals he has influenced, nor the fact that he has put his stamp indelibly on the face of American psychology, although all of these flow

> from his central conception. That conception and Skinner's most lasting contribution is in my opinion more philosophical than psychological. It is nothing less than a new way to look at life; in other words (words to which he would strenuously object), a new way to conceive of the soul. But I should not call his vision of the soul entirely new. The ancient Greeks, Aristotle in particular, conceived of souls as modes of living, as patterns of overt behavior of organisms, more or less complicated depending on species and individuals within species. Psychology for them was the identification and manipulation (the prediction and control) of these patterns of behavior, including one's own. To Skinner we owe the renaissance of this conception. (pp. 373–374)

The remainder of this article will be devoted to a discussion and defense of TEB.

Teleological Behaviorism

Teleological behaviorism (TEB) is a behavioral identity theory of mind. It identifies the mind not with internal events but with overt patterns of behavior observable in principle by other people. Overt behavioral patterns are said by TEB to *cause* the acts that comprise them. Thus, according to TEB, the mind may cause overt behavior. However, TEB's notion of cause differs from our usual notion; for TEB, following Aristotle (see Rachlin, 1992, 1994, 2000, 2014), the mind may be a *final* cause of behavior (an answer to the question, why?) but not an *efficient* cause of behavior (an answer to the question, how?). Of course, to explain behavior fully, one must have answers to both questions. But mental causes, the domain of psychology, are, for TEB, final causes. Thus, the notion of final causation is crucial to TEB.

Final Causes

It is possible to distinguish between two kinds of final causes: narrow final causes and wide final causes (The philosopher, J. L. Ackrill, 1980, calls them "dominant" and "inclusive" causes). Let us consider the narrow (or dominant) kind first.

Narrow final causes: Narrow final causes are congruent with the behavioral concept, "reinforcement"; when a high-valued act (such as a rat's eating a food pellet) is made contingent on a lower-valued act (such as pressing a lever), eating the food pellet is said to reinforce the lever-press (Premack, 1965). For teleological behaviorism, eating the pellet is the narrow final cause of lever-pressing. Q. Why did the rat press the lever? A. To eat the food pellet. Suppose that you put a dollar into a candy machine, press a button, and get a candy bar. The candy bar is the narrow final cause of the sequence: put dollar in slot—press button. Putting the dollar in the slot, pressing the button, and eating the candy are separate acts. Narrow final causes act in the opposite direction to efficient causes. Inserting the dollar and pressing the button cause the candy to appear in the tray (*efficient* cause). And the future appearance of the candy causes you to insert the dollar (*final* cause). Q. *Why* did you put the dollar in the slot? A. To get the candy.

W*ide final causes*: Wide (or inclusive) final causes describe the relation of abstract, temporally extended patterns of activity to the acts comprising those patterns; the pattern is said to be the wide final cause of the act. For example, performing a dance is a wide final cause of doing the steps of the dance. Q. Why are you doing those steps? A. Because I am doing that dance. Playing a sonata is a wide final cause of playing the notes of the sonata. Why are you playing those notes? Because I am playing that sonata. Why are you pitching, or catching, or running the bases? Because I am playing baseball. The pattern is the cause of the act, and the act is the effect of the pattern. All the notes of a symphony (acts) must be played before the symphony (their pattern) can be said to be played, and in that sense particular acts precede their wide final causes, but it would be more accurate to think of wide final causes as embracing their effects than as following their effects (as narrow final causes do). Just as efficient causes may form a chain, with the effect of one act causing another act, so wide final causes form nested sequences; relatively particular acts are nested within relatively abstract acts like a set of Russian dolls. Imagine that you saw a snippet of film of a man swinging a hammer. You do not know why he is swinging it. He could be aiming at someone's head. But then you see more of the film and he's hammering a nail. Why is he swinging the hammer? To hammer a nail. Then you see still more of the film, and you see he's joining one piece of wood to another. Why is he hammering the nail? To join one piece of wood to another. You see more and more of the film, and you see he's building a floor, building a house, providing shelter for his family, supporting his family, being a good husband and father, until finally (you would have had to see a film of virtually his whole life) being a good person. Each more abstract pattern is a wide final cause of more particular acts. Each more particular act is an effect of all the more abstract ones. Playing Beethoven's fifth symphony is the final cause of playing each movement. Being a good person is a final cause of being a good husband, a good father, a good friend, etc. Just as particular acts may have several efficient causes, so an act may be part of more than one pattern. For example, a single note in a Bach fugue may be part of two or three or more overlapping themes, and a single act, such as a person's crossing the street, may be, at the very same time, part of the patterns of shopping, exercising, visiting a friend, etc.

Whereas a series of efficient causes comprises a chain, effects following causes, a series of final causes comprises a set of overlapping behavioral patterns, effects fitting into causes. According to TEB, therefore, the mind (in the form of an abstract pattern of overt acts) may be the cause of an overt act. Patterns of overt acts such as wishes, hopes, intentions, perceptions, imaginations, etc., can thus cause acts. Of course, abstract patterns would not exist without their particulars; a dance, to exist, requires steps. Nevertheless, abstract behavioral patterns, such as dances, do not exist in another world or only in our heads. They exist in our world and are no less real than the acts of which they are composed. Even a step, although particular relative to a dance, is an abstract pattern relative to the individual muscle movements that make it up. That does not mean that a step is any less real than a muscle movement.

Final causes are not necessarily any less scientific, less accurate, or even less precise than efficient causes. In physics, Maxwell's equations (and all of field theory) work with final causes. According to Max Planck, a founder of quantum theory: "The *cause efficiens,* which operates from the present into the future and makes future situations appear as determined by earlier ones, is joined by the *cause finalis* for which, inversely, the future—namely a definite goal—serves as the premise from which there can be deduced the development of the processes which lead to this goal" (Yourgrau & Mandelstam, 1968, p. 165). Final causes are also fundamental in thermodynamics and in economics. The discount functions (delay, probability, and social), prevalent in modern behavioral psychology and behavioral economics are also final causes—unless one (erroneously) reifies them as neural states inside the head. Final causes are particularly suited to psychology. Self-control and social cooperation may better be seen as conflicts between abstract behavioral patterns (healthy or moral patterns) and particular acts (impulsive or selfish acts) rather than as conflicts between inner forces such as "willpower" and outer temptations. I am not arguing that just because economics or modern physics makes use of final causes, psychology should do so too. I am saying that, because wide final causes are relatively abstract, a science using wide final causes will not necessarily be less accurate or less precise than one based on relatively particular efficient causes.

Mental Terms

It is beyond the scope of this article to provide a dictionary of mental terms from the viewpoint of TEB. As examples, the following two sections illustrate TEB's approach to the terms: "perception" and "imagination."

Perception: For TEB, perception is identical to a correlation over time between a person's overt behavior and an identifiable pattern of events in the environment. Consider the following question: What is the difference between two people (say, John and Marcia), one of whom (Marcia) is stone-deaf, both sitting stock-still while a Mozart quartet is playing? A. John is hearing (i.e., perceiving) the music, whereas Marcia is not hearing it. Q. What does it mean to hear? A. To discriminate by overt acts, over a period of time, between sounds and silence. That is, a nonzero correlation exists between John's behavior and sounds (unsignaled through other senses), whereas there is no correlation (a zero correlation) between Marcia's behavior and such sounds. During the past, in the presence of sound signals, their behavior (perhaps including taking audiometric tests) differed and will differ in the future. (Consider their differing reactions to someone rushing into the room behind them yelling, "Fire!") Their identical behavior during the Mozart quartet is merely one congruent point in two drastically different correlations between behavior and sound.

It could be that Marcia's hearing mechanism is entirely normal, but she is nevertheless unresponsive to sounds. In that case we would say she was "psychologically deaf." Is psychological deafness real deafness? From the viewpoint of TEB, the answer must be "yes." What counts for deafness as for all psychological (or mental) states, for TEB, is Marcia's behavior in the long run. If she was faking deafness, then her subsequent behavior would reveal what her state really was. If, despite her normal hearing mechanism, she continued to behave all her life as a deaf person behaves, the question "Was she faking deafness or psychologically deaf?" would be entirely non-pragmatic—like whether a man and a squirrel chasing each other around a tree are or are not going around each other—and thus meaningless (James, 1907/1955, pp. 41–42).

Imagination: The teleological conception of imagination follows from that of perception. Aristotle says, "Imagination must be a movement resulting from an actual exercise of a power of sense" (*De Anima*, Book III, chap. 3, 429a). As far as the overt speech and actions of a person are concerned, imagination is the same as perception. If I am doing a good job of imagining that I smell a rose, I will behave, for a moment, just as I would behave if I actually smelled a rose. The difference between perception and imagination is that the object is present in the world during perception (the rose is there when you are sensing it), whereas during imagination the object is not present in the world (the rose is not there when you are imagining it). It is not necessary to infer that the rose I am imagining (which would be present in the world if I were perceiving it) is present inside me (as a representation, an internal image, a neural discharge, or anything else) when I am imagining a rose. When I imagine a rose, my overt movements with the rose absent are the same as those I would make if a real rose was present. In other words, all is the same in perception (or sensation) and imagination except that when I imagine the rose it is not present.

If you generally behave one way in the presence of, and another way in the absence of, red lights, you are perceiving red lights. However, if, on occasion, you behave in the absence of a red light as you normally do in its presence, you are on that occasion imagining a red light. Imagining is acting and not dreaming: Vividness of imagination is not vividness of interior image but of overt behavior. Suppose two people are asked to imagine a lion present in the room. One closes her eyes and says, "Yes, I see it, a mane and a tail, that's right, it's walking around," and so on. The other runs screaming for the door. You could say that they are both imagining a lion but in different ways. TEB puts it differently: Although they are both imagining, for TEB, they are imagining different things. The first person is not imagining a lion but a picture of a lion. The second person is imagining the lion. The location, intensity, orientation, or even the existence of an image in the head of either of them would be entirely irrelevant to the imagination of either. And neuroscience bears this out. There are no pictures in the head for either of the two imaginers to look at, and, even if there were, as Aristotle pointed out, there are no sensory receptors in the head to see them with. According to TEB, a good imagination is not just an aid or a tool in good acting. Rather, good acting *is* good imagining.

For TEB, mental terms are not just a loose way of talking about behavior, nor do they refer to covert events. For TEB, there are no inner *psychological* causes at all. It is not that the organism is in any way empty but that the molecular substrate of behavior (its set of inner efficient causes) is held by TEB to be the domain of neuroscience and not of psychology. Psychology should not rely on neuroscience but should stand on its own as a separate discipline. When some aspect of voluntary behavior is unexplained by current observation, a psychologist should look for its explanation, not more deeply in the nervous system but more broadly in time—in the behavioral history of the individual or, if still not found, in the evolutionary history of the species.

For Aristotle, understanding any act of an organism requires study of both its inner mechanisms (efficient causes) and the outer patterns into which it fits (final causes). TEB does not deny the importance of inner mechanisms—quite the contrary. But TEB views the study of inner mechanisms as the domain of neuroscience and the study of overt behavioral patterns as the domain of psychology. TEB could not and does not object to neuroscience; recent advances in this field have revolutionized our understanding of how the brain works. However, TEB does object to applying the vocabulary of psychology (i.e., mental terms) to inner events.

Nor could TEB object to the everyday-life use of mental vocabulary to refer to inner states. (In the great majority of everyday-life references to the relative motion of the Earth and the sun, the sun is said to be moving. Yet we all believe that the Earth is really revolving.) One morning, we observe a piece of behavior—a person smiles. We say that he is happy, but we do not observe his happiness. We observe only the smile and harmlessly ascribe its cause to a state of happiness within his body. For most everyday-life purposes, this is sufficient. But that smile is only one part of a pattern in his verbal and nonverbal behavior all morning. If that pattern is not there, he was not happy (says TEB, and perhaps his wife and children) regardless of his inner neural state or his own introspective report.

Internal Events in Teleological Behaviorism

We frequently do talk or picture things to ourselves. Many mental acts, especially the “stream of consciousness,” seem to be nothing but internal monologues. But can talking or picturing things to oneself or covertly humming a tune bear the burden of accounting for all mental acts including consciousness itself? TEB does not object to inferring internal speech or internal picturing from observations of external behavior. TEB objects to labeling such inferred actions with a mentalistic vocabulary. A person talking to himself or herself is like a clock with the mechanism unconnected to the hands. From the viewpoint of TEB, that person is not thinking until the mechanism meets the environment and starts to serve its function. It is true that we first learn to read aloud and only then to ourselves. But looking at a book and saying the words aloud, or to oneself, is reading only in the sense that a scanner “reads” a document. As Wittgenstein (1958) pointed out, real reading implies understanding. What is the difference between two people sitting quietly and reading *Ulysses*, one of them with a PhD in English and the other a high school dropout? In a narrow sense, these two would be behaving identically, yet they are (almost certainly) not thinking identically. That is, the patterns of their current and future overt behavior differ.

Psychological Investigation

Consider the following problem: You are a casino owner, and one of your roulette wheels is several years old. You want to make sure that it is completely fair—that, when the wheel is spun, the ball has a 1/38 chance of falling into any of the 38 holes. In theory, there are two ways you could go about determining the actual probability. You could take the wheel to a shop where they will test its balance, the trueness and equal smoothness of the wooden sides, the height and stiffness of the barriers between the holes, their curvature, depth, hardness, and so on. If the wheel passes all tests, there could still be some overlooked imbalance and some unevenness. In theory, your task would never end. In practice, you would say, at some point, it doesn't matter anymore. No gambler could possibly take advantage of the minute imbalances that remain.

A second method would be to look at the video records (that casinos typically take) of the play at the table, count the number of times the ball falls into each hole, and divide by the number of spins. You might compare the distributions of these relative frequencies over the first and second years of the wheel's life to their distribution over the last year to see if there were any changes. Because the wheel is old, it may be going out of balance, with the probabilities changing, while you are observing it. But let us assume that, as you count more and more spins, the relative frequencies of the ball landing in each hole all approach 1/38, as they did when the wheel was new. However, no matter how tightly the distribution of relative frequencies was grouped around 1/38 across holes, you could not be sure that the wheel was completely fair. As with the first method, at some point (if the relative frequencies closely approximated the desired probabilities), it would not matter. No player could possibly take advantage of whatever imbalance remained. This second method is *teleological analysis*.

Which method is more fundamental? Which gets at "true" probabilities? *Probability* is an abstract concept, not something you can point to. Proponents of the first method would say that the probabilities the casino owner is trying to determine are abstract properties of the wheel (along with those of the ball and the croupier) and that the first method, taking the wheel to the shop, is getting at the fundamental probability. Probability may be a property of the wheel, just as its shape and color are properties. According to proponents of the first method, the relative frequencies obtained by the second method would be mere reflections of the fundamental probabilities that reside in the *structure* of the wheel itself.

Proponents of the second method might say that the probabilities are abstractions of the *behavior* of the wheel (along with that of the ball and the croupier) and that the second method, looking at the wheel's history and spinning the wheel to observe its current behavior, determines, as closely as can be determined, the true probabilities. The structure of the wheel, the ball, and the croupier, these roulette-wheel behaviorists (let us call them) would say, constitute the *mechanism* behind the probabilities (in Aristotle's terms, their material and efficient causes), not the probabilities themselves. The probabilities themselves do not inhere anywhere within the wheel; they inhere in the wheel's observable behavior. Behaviorists would see the wheel's probabilities as abstractions of the wheel's behavior, just as a parabolic-like arc is an abstraction of the behavior of a baseball after being hit by a bat. You would not expect to find parabolas inside a baseball, and you would not expect to find probabilities as such inside a roulette wheel.

Now let us turn from physics to psychology. There are two methods by which mental events such as a person's *intentions* may be studied, analogous to the two ways of determining the probabilities of the roulette wheel. One way is to observe the person's behavior and infer from your observations what the inner mechanism must be to have given rise to that behavior. (This method is much like trying to infer the program of a computer by typing its keys and observing what appears on the screen.) Such an endeavor may be helped by using magnetic resonance imaging (MRI) machines to observe events going on inside the nervous system or by drawing an analogy to events measured directly in the brains of other species.

Another way to study mental events such as intentions is by teleological analysis (Rachlin, 1992, 1994, 2014). This method is analogous to the second method of determining the true probabilities of the roulette wheel—observation and analysis of patterns of behavior (including verbal behavior) over time. The fundamental meanings of mental terms, claims the teleological behaviorist, are these observable patterns; they exist on what the philosopher Daniel Dennett (1978) calls the *personal level*. Suppose John has asked Mary out for many dates. (On one occasion, he tried to kiss her, but she rebuffed him, hurting John's feelings.) Mary now wonders if John is serious about the courtship. What is his intention? Is it just casual flirtation, or something long term, or possibly marriage? For a teleological behaviorist, John's past actions (including what he says to Mary and to other people) are the only relevant data on this question because it is in those actions where his intentions reside. Good data in this regard may be difficult to obtain, and John's intentions may be obscure. But they are obscure, says the teleological behaviorist, in the same way that the roulette wheel's probabilities may be obscure—because there is not enough currently available behavior to analyze, not because John's intentions are hidden inside his head. The teleological behaviorist would never suppose that John's true intentions could be revealed by a lie detector test that measures his brain waves or his physiological responses to a series of questions, no matter how probing. However, John knows that he is trying to seduce Mary (let us assume) and, the moment after he succeeds, he will up and go. Is this intention (or for that matter, his knowledge of his intention) in his head? No, it is not. His intention is in his past behavior. The reason that John knows his own intention is not that he has access to something in his own head and Mary does not but that John has access to his own past behavior and Mary does not. In principle, John's twin sister, Jane, may know better than John does what his intentions truly are. When John tells Jane about his intentions, she may reply, "No you're wrong. She's got you in her grip with all that coyness, that playing hard to get. You'll never leave her!" And Jane may be right. One more point: John's belief and Jane's belief regarding John's intentions are discriminations among complex patterns in John's behavior. The expression of the belief in both cases may be simple assertion, but the discriminative stimuli on which the assertions are based (John's behavioral patterns) are highly complex. (Note that I am not saying that internal analysis has no place in understanding John's behavior—just that it is not the royal road to his mental state. Internal inference and analysis provide a description of behavior [simple or complex] in terms of its efficient causes; teleological analysis provides a description of behavior [simple or complex] in terms of its final causes.)

Let me add a brief personal story. Last week I was riding in a car with my wife and (adult) daughter. Out of the blue, my daughter asked, "Dad, how come you never say, 'I love you' to me?" Then my wife added, "He never says it to me either." I insisted that "I love you" is a trite and overused and therefore meaningless phrase. I added that if they didn't know I loved them from my behavior, nothing I said would make them believe it. My wife said, "Well, say it anyway." I refused and added, "But I'll tell you what I will do. I'll say, "You love me," which is neither trite nor meaningless but based on behavioral observation". Then we exchanged "You love me" all around. Cognitively we were in the same place as we would have been if we had said "I love you." That is, we all indicated that we love each other. However, in terms of emotion and meaning, we were in a much better place. We each had unmistakable evidence for what we said and could be truly believed. I invite the reader to try this with her loved ones.

The Problem of Self-Control

From a behavioral viewpoint, self-control is an *external* phenomenon—a conflict between a relatively immediate high-valued act (such as smoking a cigarette) and an element of a long-range, more abstract, high-valued pattern of acts (such as healthy behavior). When the conflict is resolved in favor of the high-valued pattern, behavior is said to be self-controlled; when the conflict is resolved in favor of the particular act, behavior is said to be impulsive. This view may be contrasted with the currently dominant view of self-control as an *internal* phenomenon. The dominant view stems from Descartes' initial conception of self-control as a conflict that takes place within the brain between a physical impulse leading to an overt act and a spiritual force, willpower, leading to a high-valued pattern of acts.

Like Descartes, many modern thinkers locate the arena of conflict between impulsivity and self-control somewhere within the brain, but for them the conflict takes place between neural impulses or endogenous hormones coming from different places (mediated by the composition, strength, and availability of different neural transmitters) rather than between a physical impulse and a spiritual force, as described by Descartes. A message from a "lower" brain area dictating a certain action (a neural impulse to smoke the cigarette) conflicts with a message from a "higher" brain area dictating an opposite action (a neural impulse to refuse the cigarette). These neural impulses (internal representations of intentions) fight it out in some area of the brain, and the behavior that emerges signals the winner.

Teleological behaviorism rejects such a view. It says that the fundamental conflict between self-controlled and impulsive actions takes place not among representations of intentions in some specific location in the brain but in the person's overt behavior over a period of time. From the present viewpoint, self-control is not a battle between internal intentions to do one thing and internal impulses to do another. It is fundamentally a temporal conflict—between behavior that maximizes value over a short time period and behavior that maximizes value over a long time.

Of course, people have intentions. (An obese person, for example, might resolve to eat less food.) They then either carry out or fail to carry out those intentions. But, according to teleological behaviorism, an intention is itself behavior—a temporally extended pattern of behavior—that may be extrinsically reinforced even when it is not carried out to its end. For example, an obese woman may be rewarded by her friends and relations for agreeing to join a weight-reduction program and rewarded again when she did join the program—independent of the other longer-term rewards contingent on losing weight. Such extrinsic rewards may increase the frequency or intensity with which she talks about her intention to lose weight, but they cannot directly increase the strength of her intention. The strength of an intention is not given by the frequency, intensity, or determination with which it is expressed or the intensity of a neural discharge or the brightness of a brain-area image in an MRI scan. The strength of an intention is rather the likelihood of its being followed; that likelihood, in turn, may be measured by how frequently prior, similar, intentions have been followed by that person in the past. Someone who promises to stop drinking now and has promised to stop drinking many times in the past but has not stopped, or has stopped for a short time, has only a weak intention to stop, unless future events prove, retrospectively, that the intention was strong. In this respect, an intention is like a probability; you can only know for sure that a coin is unbiased if you flip it a sufficient number of times and the overall frequency of heads approaches closer and closer to one-half. An apparent intention that cannot, in principle, be measured in this fashion (such as a purely internal intention) is not really an intention at all. The mechanisms underlying intentions, like the mechanisms underlying all behavior, are surely internal. But the intentions themselves occur in behavior over time, not inside the head. If they are never exhibited in overt behavior, you may want to call them potential or aborted intentions, but they are not actual, whole, intentions.

The internal and external viewpoints are not mutually exclusive. On the one hand, all complex human and animal behavior patterns are controlled by *internal* mechanisms. On the other hand, all complex human and animal behavior patterns exist because of their function in the interaction of the person with the *external* environment. The issue is ultimately semantic. Should we use the vocabulary of self-control to refer to the interaction between *internal* causes and behavior patterns (as Descartes advised), or should we use that vocabulary to refer to the function of behavior patterns in the *external* environment (as Aristotle advised)? But a semantic issue is not necessarily a pointless one. In this case, the two viewpoints have practical as well as theoretical consequences.

The Extended Self

In what economists call "public goods" situations, such as contributing to public television, voting, contributing to charities, not littering, and so on, very little or nothing is gained by contributing. Each individual benefits most by not contributing; she gets the benefits of what others contribute at no cost to her. Economic theory, based on maximization by each participant of his or her own utility, predicts that no one will contribute (this is called the "free rider" problem). Yet many people do contribute. Why? Because the unit of utility maximization is not always an individual person, bounded by her skin, but a group of individuals extended in social space—the extended self.

Corresponding to a view of the self as extended in time (the outcome of a conflict between narrower and wider temporal interests) is a view of the self as extended in social space (the outcome of a conflict between narrower and wider social interests). Whereas it may seem self-evident that people will sacrifice some part of their present good for the benefit of their future selves, it seems mysterious when people sacrifice some part of their own good for the benefit of another person. Yet, the two forms of sacrifice are equally explicable in economic terms.

Reinforcing Self-Control and Social Cooperation

The question arises: How do we reinforce self-control or social cooperation? One way to effectively reinforce both self-control and social cooperation is to require choices to be made in *patterns.* In both repeated self-control and repeated social cooperation games, subjects who are required to make a series of choices all at once tend to cooperate more and to show more self-control (Rachlin, 2000). In everyday self-control situations, this relation is fundamental. If an alcoholic is sober or drunk, drinking is always better than not drinking. But the value of a year of no drinking (or moderate drinking) is greater than the value of a year of steady drinking.

Social cooperation situations have a similar structure. The goodwill and trust of other people are vague and abstract compared to the particular and immediate rewards of defection. A person may believe that it is always better to tell the truth (and would choose to tell the truth over the next 4 years rather than to lie over the next 4 years, if such a choice could be made) but still would be tempted to lie to get out of some ticklish situation. The problem is that, in life, choices usually must be made one at a time. How do we generate a valuable *pattern* of behavior when each component of that pattern is less valuable than its alternative?

A conceivable answer to that question is that we figure it out. Somewhere in our brains is a *rational mechanism* that evaluates the pattern, organizes it, and sends out commands to the motor system to emit the pattern's components—only to be opposed during periods of temptation by contrary commands rising from below (visceral impulses). From this viewpoint, each act—self-controlled or impulsive, cooperation or defection—is the product of a battle between our higher and lower nervous systems.

An alternative view, the behavioral view, is that valuable and complex patterns of behavior may evolve from simpler ones over a person's lifetime, just as valuable and complex structures (like the eye) have evolved from simpler structures over generations. The environment (through reinforcement) selects individual acts in the same way that the environment (through survival of the fittest) selects individual organisms. *Patterns* of behavior may be selected in the same way that *groups* of individual organisms may be selected (Rachlin, 2019).

In general, it is not a good idea to make many sorts of decisions on a case-by-case basis. On a case-by-case basis, most of us would be having that second dessert, drinking that third martini at a party, and throwing that candy wrapper into the gutter and that letter from UNICEF into the wastebasket. There are often no rational justifications for doing the reverse (refusing the second dessert or the martini, holding the candy wrapper until you reach a wastebasket, sending a check to UNICEF). Rational justifications appear only for the overall pattern that has evolved by a process akin to group selection and which we follow regardless of immediate contingencies to the contrary.

No part of this process must rely on a deliberate foresighted author. As wider and wider patterns are reinforced, the units selected evolve from simpler to more complex forms over our lifetimes—just as complex structures like the vertebrate eye evolve from simpler structures in the lifetime of a species. To explain self-control and social cooperation, we do not need to imagine a creator of behavior (a rational mechanism) lodged inside each person's head.

From a behavioral viewpoint, the difficulty of explaining altruistic behavior is not intrinsically greater (or less) than the difficulty in explaining self-control in everyday life. It is not contradictory for a behaviorist to say that altruism is reinforced provided the reinforcer is understood as acting not on that act alone but on the pattern that the act is part of.

The more complex an organism's behavior, the more abstract are the principles that explain it. The life of the philosopher, Aristotle said, is the most abstract and therefore the best and freest life. For Aristotle, even though all of a person's actions are caused (by both efficient and final causes), it is still meaningful to talk of better and worse acts; it is still meaningful to talk of free acts and unfree acts. Final causes of free acts are consequences that are beneficial in the long run (to society as well as the individual), while final causes of unfree acts are consequences that may be beneficial only in the short run and possibly harmful in the long run.

The only conception of free will that remains meaningful in modern scientific psychology is this conception: When people act for the long-term good of themselves and their society, in cases where such acts conflict with their immediate and individual pleasures, they may meaningfully be said to be acting freely; they are not constrained by immediate pleasures and pains. This freedom is compatible with a determinism that sees even their choice of abstract good over particular pleasure as in principle predictable. The reason for making this distinction between free and unfree behavior is pragmatic; such a distinction is useful in developing a science of behavior.

References

Ackrill, J. L. (1980). Aristotle on eudaimonia. In A. O. Rorty (Ed.), *Essays on Aristotle's ethics* (pp. 15–34). University of California Press.

Ainslie, G. (1992). *Picoeconomics: The strategic interaction of successive motivational states within the person*. Cambridge University Press.

Ainslie, G. (2001). *Breakdown of will*. Cambridge University Press.

Dennett, D. (1978). *Brainstorms: Philosophical essays on mind and psychology*. Bradford Books.

James, W. (1907/1955). *Pragmatism and four essays from the meaning of truth*. Meridian Books.

Premack, D. (1965). Reinforcement theory. In *Nebraska symposium on motivation* (Vol. 13, pp. 123–180). University Nebraska Press.

Rachlin, H. (1970). *Introduction to modern behaviorism*. New York: W. H. Freeman and Company.

Rachlin, H. (1985). Maximization theory and Plato's concept of the good. *Behavior, 13*, 3–20.

Rachlin, H. (1992). Teleological behaviorism. *American Psychologist, 47*, 1371–1382.

Rachlin, H. (1994). *Behavior and mind: The roots of modern psychology*. Oxford University Press.

Rachlin, H. (1995). Burrhus Frederic Skinner (1904–1990). *National Academy of Sciences. Biographical Memoirs, 67*, 363–377.

Rachlin, H. (2000). *The science of self-control*. Harvard University Press.

Rachlin, H. (2014). *The escape of the mind*. Oxford University Press.

Rachlin, H. (2019). Group selection in behavioral evolution. *Behavioural Processes, 161*, 65–72.

Ryle, G. (1949). *The concept of mind*. Hutchinson.

Wittgenstein, L. (1958). *Philosophical investigations* (3rd ed.). Macmillan.

Yourgrau, W., & Mandelstam, S. (1968). *Variational principles in dynamics and quantum theory*. Dover.

Chapter 2
Purposive Behavior and Psychological Categories: Thoughts on Teleological Behaviorism

Filipe Lazzeri

Rachlin's (1994, 2014, 2021) teleological behaviorism is an approach within the behavioral research tradition in psychology. As such, it must be situated in the historical theoretical context comprising early Watsonian behaviorism (Watson, 1930), Tolman's (1932) purposive behaviorism, and Skinner's (1953) radical behaviorism, among other past and recent variations of behaviorism. Furthermore, it must be situated in the context of disputes with other research traditions in psychology, most notably cognitive psychology. Basically, Rachlin's variant of behaviorism can be understood as an attempt at enhancing radical behaviorism's problem-solving power (Lazzeri, 2017a)—that is, its effectiveness in solving empirical and conceptual problems (Laudan, 1977)—by combining a particular teleological and molar view of operant behavior with an account of psychological categories (e.g., emotions and thoughts) in terms of public patterns of operant behavior.

Teleological behaviorism goes along with several guiding assumptions (to borrow from Laudan et al.'s 1986 terminology) constitutive of radical behaviorism, the most influential theoretical framework in behavioral psychology since the 1960s. These shared guiding assumptions include those whereby the behavior of organisms, including humans, is a lawful phenomenon; what we ordinarily call actions must be fleshed out in terms of operant behavior (i.e., behavior controlled by its consequences); neurophysiological events overall can explain *how* operant behavior happens, whereas contingencies of reinforcement cum natural selection can explain *why* it happens; and the ultimate criterion for the validation of terms and claims in behavior analysis is pragmatic value, measured by effective prediction and control, among other assumptions (Lazzeri, 2017a). Teleological and radical behaviorist approaches are split on some specific issues, related to the contours of operant

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behavior, as well as the contours and place of ordinary psychological categories in behavior analysis.

In the following, I discuss three features of Rachlin's teleological behaviorism (henceforth, "TEB"), respectively: (1) its teleology; (2) its claim that psychological phenomena amount to public patterns of behavior extended in time; and (3) its support of ordinary psychological vocabulary in behavior analysis. Yet, since I have already discussed these features of TEB elsewhere (Lazzeri, 2013a, 2013b, 2017a[1]), I shall here essentially provide a brief summary of my remarks upon them.

Teleological Talk, Yes, but in Rather Straightforward Selectionist Terms

According to Skinner (1953, 1969), teleological language—the ascription of functions, purposes, or goals—connotes inverted causes. Thus, for Skinner, when we say that a function (purpose or goal—*telos*) of a given behavior is to obtain food (or that this behavior is taking place because it has such function), we are implying future events acting backward upon that behavior, as if the effects could precede the causes. For this reason, Skinner overall rejects the use of teleological language in behavioral psychology, under the assumption that the connotation of reversal causes has no place in contemporary science. Nonetheless, Skinner also suggests that teleological statements can be translated with slight changes into statements about natural selection or contingencies of reinforcement (as the case may be). Accordingly, talk of functions (purposes or goals) of a given behavior does not refer to any "property of behavior itself; it is a way of referring to controlling variables" (Skinner, 1953, p. 88). To say I was climbing a tree yesterday to (or with the purpose of) getting some fruit is at best a way of abbreviating a (possibly complex) history of reinforcement, as well as the context.

Rachlin takes issue with Skinner's rejection of teleological language, in this regard recovering a basic tenet of Tolman's (1932, 1951) purposive behaviorism: the behavior of organisms is a kind of purposive, goal-directed phenomenon, irreducible to behavior as simple motion. TEB, nevertheless, like radical behaviorism (henceforth, "RB"), completely steers clear of hypothetical inner determinants of behavior Tolman (and, of course, many others before and after him) devised, such as "cognitive maps."

Rachlin (2014, 2021) holds the view that behavior patterns are teleological in at least two different senses. First, according to him, some behavior patterns are "narrow final causes," which are those immediately contingent upon (i.e., followed or allowed by) a given behavior pattern. For example, "eating the pellet is the narrow final cause of lever-pressing" (Rachlin, 2021), in the context where eating the pellet is contingent on lever-pressing. (Lever-pressing, in this situation, can be said to have

[1] These papers, by their time, are derived from Lazzeri (2011, chap. 1; and 2015c, chap. 5).

the goal of producing pellet release.) Second, some behavior patterns are "wide final causes," which are extended in time (i.e., molar) patterns comprising less extended ones as constituents. This happens when "relatively particular acts are nested within relatively abstract acts like a set of Russian dolls" (Rachlin, 2021). Thus, Rachlin considers that building a house is a final cause of building a floor, for this is a part of the more extended behavior pattern of building a house. By its turn, the latter has its own final causes, "until finally […] [the pattern of] being a good person" (Rachlin, 2021).

With this rendering of teleological language, Rachlin follows Tolman's (1932) molarist spirit but free from Tolman's mentalism (in Skinner's sense). (Thus, by "mentalism," I mean the postulation of inner hypothetical or physiological phenomena taken as variables explanatory of *why* behavior takes place.) Rachlin seems particularly concerned with accommodating medium- and long-term reinforcing consequences, as well as extended behavior patterns as irreducible to their behavioral microconstituents ("molecules"). Like Baum (2005, 2021)—who nevertheless does not take advantage of TEB's teleology—Rachlin is critical of RB for a supposed reductive (moleculatist) stance, whereby molar behavior is analyzed not in its own but rather in terms of behavioral microconstituents, each of which is assumed to be defined by immediate consequences.

I shall not take issue with Rachlin and Baum's concern with molar behavior and molar reinforcing consequences, nor with their charge of molecularism against RB. Rather, I shall here leave open the plausibility of these molarist claims, which in part hinge on some empirical issues pertaining to the effectiveness of medium- and long-term consequences as reinforcers. Their charge of molecularism against RB is disputable, since RB seems to acknowledge molar behavior and consequences provided they are shown to obey functional relations (Skinner, 1935). This is in keeping with Skinner's guiding assumption of pragmatic value as the ultimate criterion for validation of claims in BA. Yet, it is possibly true that, in practice, RB has a predilection for molecular analysis. I shall leave open this issue in what follows.

When it comes to Rachlin's teleology, I suggest that, yes, the behavior of organisms is, by and large, a teleological, goal-directed phenomenon—it does have functions, purposes, or goals,[2] in relation to contexts—and there is no need for mentalism (in the sense previously explained) to account for this fact (Lazzeri, 2013b, 2013d, 2014). Think of a given organism's behavior and usually you will find a goal-directed phenomenon.

The concept of function (purpose or goal, which I take here as largely synonymous) has two basic connotations I would like to highlight: (a) if something X has a function F, then F is not an accidental result (functions stand in contrast with accidental, collateral results), and (b) X may have F even when some instances of X do not perform F (cf., e.g., Garson, 2016; Neander, 1991). By way of illustration, while

[2] I do not use Rachlin's preferred term "final cause," though it also means function (e.g., Ruse, 2019), because talk of "final causes" is linked to Aristotle's (*Physics*, book II) theory of the four causes. As I shall point out soon, we can make sense of function-talk without need for Rachlin's Aristotelian contrast between efficient and final causes.

climbing a tree I may accidentally break a branch and scare someone. Tree climbing, in this context, has a function of getting some fruit and not (although, of course, it could have) the function of breaking a branch and scaring. Moreover, even if some instances of this behavior do not succeed in producing food obtainment (e.g., owing to not reaching the required height or to absence of fruit), they still have that purpose.

However, there seems to be a more promising interpretation of teleological talk about behavior (in particular, operant behavior) than that provided by TB: the historic-etiological theory of functions, originally worked out by Wright (1976) and later developed into a fully selectionist view of functions by authors such as Neander (1991), Millikan (1993), and Garson (2016). For one reason, the behavior of organisms is just one kind phenomenon among many others we ascribe functions to (both in ordinary and in scientific contexts). We also ascribe functions (in the same sense, specified by [a] and [b] above) to many other traits, such as body organs, glands, and cells (e.g., lungs have a biological function of supplying oxygen to the blood; a function of the Duvernoy gland in some snakes is to secrete poison). In addition, we assign functions to cultural artifacts (spoons, pans, stoves, etc.). Function talk must be accounted for by way of a unified theory of functions or at least a comprehensive theory of functions not limited to those of behavior patterns. Now, this is accomplished by the historic-etiological theory (henceforth, "HET," briefly described below), with the possible exception of artifact functions. Rachlin's teleology, on the other hand, does not seem to have such comprehensiveness.

Wright (1976), Neander (1991), Millikan (1993), and Garson (2016) are not behaviorists, but the theory they have helped develop (HET) is not inherently committed to mentalism. Rather, it is harmonious with a non-mentalist understanding of operant behavior. According to HET (I shall here privilege Garson's 2016 recent formulation), roughly, a trait *X* (e.g., a given rat behavior) has a function *F* (e.g., obtaining food) if *X* was selected for resulting in *F*. The conditions for selection are (1) variation, (2) differential success, and (3) persistence. This means that the etiology of *X* involves (at least in part) the following: (1) a variant of X's (past) items having the relevant features for resulting in *F* and (2) conferring differential success in relation to variants of *X* that did not possess these features (e.g., lever-pressing with a given frequency resulted in the release of a pellet, whereas those without the necessary frequency didn't), (3) followed by a frequency increase (persistence) or decrease (with subsequent extinction), respectively. (I think Millikan's complementary notion of "derived proper function" must be considered in order to account for certain function ascriptions, but I shall limit myself here to the simpler cases.)

Thanks to its emphasis on selection history, HET is able to explain features (a) and (b) of function ascriptions (the function-accident contrast and the fact that function is something not always fulfilled). For example, in case an instance of lever-pressing occasionally does not have the required rate or does not occur at the right time (as the case may be), it fails to produce, though it had the purpose of producing, food release. This is because it owes its existence to a reinforcement history, and its function is a matter of having this sort of *historical* etiology. Lever-press may have the accidental result of, say, shadows projected on the floor. Despite the fact that food release and shadows are things effected by the organism, only the former result is a function of lever-pressing, because only the etiology of the former is a selection history (Lazzeri, 2013b).

In this sense, reinforcement learning bestows functions to its products (operant patterns) like natural selection to its own (biological adaptations). Both are cases of what Skinner (1981) famously called *selection by consequences*. As is well known, cultural evolution may be another case in point: it may follow parameters (1)–(3). Thus, HET comes in handy for a selectionist and non-mentalist understanding of operant behavior—upon which there seems to be large agreement between RB and TB (mutatis mutandis) (cf., e.g., Rachlin, 1991, 2014). As far as I know, Rachlin would agree that extended patterns of operant behavior are also ultimately characterized by reinforcement histories. (A caveat: I do not mean every episode of behavior is goal-directed, for some are analogous to exaptations, in Gould & Vrba's 1982 sense. Cf. Cleaveland, 2002; Lazzeri, 2013b).

Besides its far-reaching scope, another reason for embracing a historic-etiological interpretation of teleological statements about operant behavior is its congruence with current terminology in biology and its simplicity by comparison with Rachlin's interpretation. Function talk in terms of selection histories find home in contemporary biology, differently from the Aristotelian "final causes" terminology. This is because contemporary biology does not draw the Aristotelian distinction between efficient and final causes. Instead, it works only with efficient causes. When necessary, it subdivides efficient causes into distal (or ultimate) and proximate causes (e.g., Alcock, 2009; Mayr, 1961). Based on the latter distinction, we are able to express Rachlin's (1994, 2021) contrast, also made by Skinner (1990), between, on the one hand, answers to *why*-questions about operant behavior, to be found in distal, historical causes (viz., reinforcement histories), and, on the other hand, answers to *how*-questions about operant behavior, to be found in its proximate causes (viz., its physiological basis). Rachlin's Aristotelian talk of "narrow and wide final causes" is alien to contemporary biology, for, although "final cause" means function, it is linked to a distinction (between efficient and final causes) that seems no longer necessary to make sense of goal-directedness.

Therefore, I think TEB's claim that operant behavior is (in a sense) teleological, and that this requires neither postulation of reversal causes nor mentalism, is fairly right. Yet, HET's interpretation of functions seems more promising than TEB's.[3] Behavior analysis (henceforth, BA) may indeed profit from function talk to identify dependent and independent variables while keeping coherent with its basic commitments.

[3] I am aware HET is not free from objections, but there are good answers to them as well. Since it is not feasible to touch upon this debate here, the reader may have a look at Garson (2016) for an excellent survey.

Overt Operant Behavior Makes Up Psychological Phenomena, but Covert Behavior and "Private" Stimuli, Too

As is familiar in BA, RB (Skinner, 1953, 1974) acknowledges the existence not only of overt behavior but also of covert behavior, i.e., behavior unapparent in the outside body. There are reflex covert behaviors, such as heartbeat increase elicited by the occurrence of sound paired in the past with predator attack (in this case, a conditioned convert reflex). And there are operant covert behaviors, such as working out the expenses of a trip subvocally, without help of any tool or movement of the limbs. Covert behavior has the same basic properties of overt behavior (i.e., the properties which define reflexes, operants, or other recognized behavior patterns, as the case may be). The distinction between covert and overt behavior is an epistemic matter of degree of observability from a third-person vantage point, rather than an ontological distinction (cf. Lazzeri, 2013b, 2015b, 2017b; Palmer, 2009).

RB also takes into account the existence not only of external, exteroceptive stimuli, that is, visual, auditory, olfactory, etc., events in our surroundings, but also proprioceptive and interoceptive stimuli, which Skinner (1953) calls "private events." We are equipped with nerves spread in our bodies that allow us to feel, for example, the position of our muscles, tendons, and posture (proprioceptive stimuli) and conditions of our throat, stomach, and heart (interoceptive stimuli). Skinner calls these stimuli "private" because the individual has a unique contact with them, different from the contact an external observer can have. For example, with a proper instrument, many people could (in principle) have visual contact with the dry condition of person *r*'s throat (exteroceptive stimulus). Yet, it is only *r* who can feel *r*'s own throat dryness (people who could have visual, exteroceptive contact with it cannot have interoceptive contact with it). Both the exteroceptive and the interoceptive stimuli are assumed to be entirely physical (RB works under a physicalist assumption). The difference between them stems from the fact that one cannot connect his or her interoceptive nerves to someone else's body so as to literally (not metaphorically) feel the dryness felt in the throat by this individual.

Rachlin, on the other hand, eschews covert behavior and internal stimuli from his approach to psychological categories altogether. According to Rachlin (1994, 2014, 2021), psychological categories, such as sensations, emotions, thoughts, and so on, all amount to molar patterns of overt behavior. TEB does not seem to deny the existence of proprioceptive and interoceptive stimuli but clearly takes them as insignificant for the analysis of psychological categories. For TEB, nothing that happens inside the body is an ingredient of psychological phenomena—an implication that certainly sounds very odd from almost any other perspective about psychological phenomena, including RB.

When it comes to the events that for Skinner's RB are covert behaviors, Rachlin does not deny their existence either, but he views them not as behavior. Instead, for Rachlin, they are only physiological mechanisms underlying behavior (cf., e.g., Rachlin, 2014, p. 78), thereby, according to him, not being constituents of any psychological phenomenon whatsoever.

I think Rachlin's refusal to admit as behavior what Skinner calls covert behavior is definitely *not* the way forward. Covert behavior is any activity of an organism which obeys the same etiological parameters of overt behaviors, differing in that the latter is relatively observable in the external part of the body.

I can estimate the expenses of a trip out loud, with pencil and paper, or on a computer screen. In this case, my thinking process is constituted (at least in part) by a sequence of overt behaviors. Someone could watch me engaged in this thinking process and to some extent could even have a more impartial judgment as to the way I am engaged in it. I definitely agree with Rachlin, for reasons I have discussed elsewhere (Lazzeri, 2013a, 2013c, 2015a, 2015b, 2017b), overt behavior makes up different sorts of thinking processes (reasoning, remembering, imagining, etc.), as well as psychological phenomena of several other categories (emotions, perceptual processes, so-called propositional attitudes, etc.). Overt behavior is not merely an effect or signal of psychological phenomena lying behind the scene as causes. Rather, it is constitutive of psychological phenomena (a lesson both Rachlin and me, I believe, have drawn partially from Ryle, 1949, among others).

However, covert behavior, too, makes up some instances of psychological phenomena. For example, in case I estimate the expenses of a trip subvocally, without help of any tool or external bodily movement, my thinking process comprises covert behavior. This activity, like that of estimating expenses with the help of a pencil, owes its existence to a complex history of reinforcement (I had to learn many things in the past thanks to reinforcement to be capable of doing it). Therefore, this covert activity is an instance of operant behavior, unless externality is (but it should not be) taken as a criterion for an event to count as behavior.

Rachlin (2021) says: "A person talking to himself or herself is like a clock with the mechanism unconnected to the hands. From the viewpoint of TEB, that person is not thinking until the mechanism meets the environment and starts to serve its function." Yet, to keep with the example above, how come my covert activity of estimating the expenses of a trip does not meet the environment and serve its function? The activity of calculating expenses, no matter if overtly or covertly, is always situated in a given environment with demands, vicissitudes, etc., in relation to which we act under the control of historical variables. By and large, our activities always have functions, independently of their degree of observability. The activity of calculating expenses may serve functions related to the preparation for a trip. The fact that this deed happened covertly does not make any difference in so far as possession of function(s) is concerned (the function is the same for all instances of the operant pattern). Nor does it necessarily affect the fulfillment thereof; after all, it can be effective in providing a good estimation of the expenses.

It is entirely clear that TEB eschews covert reflexes (besides covert operants) from its analysis of psychological categories, but it is not if TEB takes into account at least (instances of) overt *reflex* behavior as partial ingredients of some (instances of) psychological phenomena. Indeed, Rachlin's teleology seems restricted to operants, and overall he does not mention reflexes in his analyses of psychological categories. I think instances of both overt and covert reflexes can make up psychological phenomena (e.g., Lazzeri, 2015b). The instances of behavior that compose them, I submit, can be of operant and reflex patterns, besides of tropisms (kinesis and taxes), so-called fixed action patterns, and reaction chains (encompassed by the concept of reflex broadly construed).

Now, externality by itself seems irrelevant as criterion for the applicability of the concept of behavior, ontologically speaking. The sort of etiology (or causal root) involved in the activity is what basically matters, or should matter, for it to count or not as behavior. This is because, as highlighted in the first section, behavior (in the relevant sense) is by and large a goal-directed process (i.e., it has functions, purposes, or goals), and this goal-directedness must be cached out in etiological terms. For example, whether or not something is an instance of operant behavior hinges upon whether or not it is controlled by consequences in given contexts, that is, whether or not it is something that happens because of a reinforcement history and discriminative stimulus (Lazzeri, 2013d, 2014, 2015b). The degree of observability from a third-person vantage point is just a matter of epistemic limitation, irrelevant to define the contours of behavior.

Furthermore, TEB's dismissal of interoceptive and proprioceptive stimuli in the analysis of psychological categories leaves out some ingredients of the phenomena it intends to characterize. This is especially the case of sensations, like those of dry throat and hanger pangs (interoceptive stimuli), as well as those we subtly feel in relation to the position and movement of our limbs (proprioceptive stimuli). For one reason, their character is of occurrences (things which happen in the here and now, so to speak), as opposed to a dispositional character (in Ryle's 1949 sense); that is, sensations are episodic phenomena. And at least several of them are things the organism has in specific parts of its body, often quite momentously. Public patterns of behavior do not have these features. For a second reason, as many have pointed out, sensations inherently have so-called qualitative character (e.g., the unpleasant dryness felt in the throat). There is no sensation without contact with the condition of one's own body or a part thereof, which amounts to interoceptive or proprioceptive stimulation.

So, to conclude this section, differently from TEB, RB (although with a qualification I shall point out on next section), as I understand it, considers that psychological categories refer to behavioral events that can be as follows, depending on the case: (1) operant, reflex (broadly understood, including tropisms, etc.), or aggregates of operant and reflex behavior; (2) overt, covert, or partly overt, partly covert; (3) episodic occurrences, behavioral chains, or behaviors spread in time and space; and (4) in relation to exteroceptive, proprioceptive, and/or interoceptive stimuli. RB also correctly acknowledges that proprioceptive and interoceptive stimuli can make up sensations. (To be sure, Skinner does not make such clear-cut systematization and is not always consistent with it. However, in some places, he is, as, e.g., in his analysis of emotions in Skinner, 1953, chap. 10, and of the heterogeneous category of thinking in 1957, chap. 19, to mention but a few. I have defended an approach along these lines in Lazzeri, 2015a, 2015b, 2016, 2017a, 2017b). For these reasons, I think RB (so understood) fares better than TEB when it comes to the analysis of psychological categories. TEB is simpler than RB in this regard, but simplicity is not always a virtue, particularly if it turns out to neglect the richness of the phenomena it intends to cover.

Nonetheless, by the line of reasoning here put forward, Rachlin is partially right that psychological phenomena are behavioral phenomena. TEB certainly helps clarify the overt behavioral ingredients that make up psychological phenomena of several categories, as well as some important features of self-knowledge and self-control, among other things—all of which matter so much for our scientific-philosophical understanding thereof.

Ordinary Psychological Vocabulary in Behavioral Analysis

As I have shown elsewhere (Lazzeri, 2017a; Lazzeri & Oliveira-Castro, 2010), Skinner is sometimes inconsistent as regards the analysis of psychological categories. Skinner was ambiguous between, on the one hand, (I) what we may call a "positive" approach to psychological categories along the lines described above, which acknowledges the existence of psychological phenomena of several categories as overall behavioral phenomena, and, on the other hand, (II) a "negative," eliminativist approach to psychological categories, or at least to many of them, whereby they are inherently mentalistic (in the sense mentioned in the first section) and, hence, are like outdated entities that turned out to be fictions in the history of science. This second approach is related to Skinner's well-known rejection of ordinary psychological terms for the purposes of behavior science. (For a reconstruction of Skinner's arguments for this form of eliminativism, one may have a look at Lazzeri, 2017a.)

Rachlin is closer to (I) than (II), by holding that psychological phenomena exist and are a behavioral reality. Furthermore, Rachlin (1995) goes so far as to claim the main difference between Skinner's RB and TEB lies in the issue as to "whether mental terms belong in a scientific psychology. Teleological behaviorism claims they do; Skinnerian behaviorism claims they do not" (p. 180). TEB is in part a defense of ordinary psychological terms in BA (see also Rachlin, 2012, 2021; Rachlin & Frankel, 2009).

TEB provides at least two arguments in defense of common psychological terms in BA (for details, see Lazzeri, 2015c, 2017a). According to one of the arguments, these terms are useful for identifying public patterns of behavior, because this is what they mean, when properly understood, or so TEB claims. As such, these terms are useful for behavioral prediction and control. A second argument goes as follows: Skinner's rejection of ordinary psychological vocabulary for BA's purposes has been very detrimental to its acceptance in psychology, philosophy, and the popular culture. And unduly so, since when properly understood, these terms, according to TEB, refer to behavioral phenomena, instead of inherently appealing to mentalistic hypotheses, contrary to what Skinner supposed.

In a nutshell, following here a Laudanian reading key (Laudan, 1977), TEB can be interpreted as claiming that a proper—behavioral, non-mentalistic—adoption of ordinary psychological categories for the purposes of behavioral research would increase its problem-solving power. First, it would enhance BA's already large capacity for prediction and control of complex behavior and behavior in open contexts, considered by many (though arguably oftentimes as a result of misunderstandings) as a relatively limited capacity. Second, this adoption would be useful because it would better serve a demand for explanation of phenomena dear to all, such as emotions, moods, imagination, memory, and so on.

From the "positive," non-eliminativist rendering of RB described above, Rachlin's two arguments seem correct, with the proviso, of course, that their premises concerning the contours of psychological phenomena be fixed. Covert behavior, private stimuli, etc., are also ingredients of psychological phenomena, in addition to overt operant behavior extended in time.

Conclusion

Summing up, as I see it, TEB is right that operant behavior can be characterized as goal-directed without neither postulation of reversal causes nor mentalism. However, I believe HET provides us with a better modeling of goal-directedness than Rachlin's. BA could take advantage of function talk modeled in historic-etiological terms.

TEB helps identify public behavioral ingredients that make up psychological phenomena. Yet, by dismissing covert behavior, as well as interoceptive and proprioceptive stimuli, TEB does not account for some other important features of psychological categories. Its emphasis upon operant patterns of behavior extended in time is problematic, because not only operant but also reflex behaviors make up psychological phenomena and because some instances of psychological phenomena are episodic and quite momentary.

TEB's defense of ordinary psychological categories in BA is, I think, one of its most interesting features. It is a defense based upon a behavioral, non-mentalistic view of these categories, which calls attention to the potential usefulness of such an adoption. Rachlin offers good reasons for it, except that his premises related to the ingredients that make up psychological phenomena are only partially plausible.

References

Alcock, J. (2009). *Animal behavior: An evolutionary approach* (9th ed.). Sinauer.

Baum, W. M. (2005). *Understanding behaviorism: Behavior, culture, and evolution* (2nd ed.). Blackwell.

Baum, W. M. (2021). *Introduction to molar behaviorism and multiscale behavior analysis*. Springer.

Cleaveland, J. M. (2002). Beyond trial-and-error in a selectionist psychology. *Behavior and Philosophy, 30*, 73–99. https://www.behavior.org/resources/111.pdf

Garson, J. (2016). *A critical overview of biological functions*. Springer.

Gould, S. J., & Vrba, E. S. (1982). Exaptation: A missing term in the science of form. *Paleobiology, 8*, 4–15. https://doi.org/10.1017/S0094837300004310

Laudan, L. (1977). *Progress and its problems: Towards a theory of scientific growth*. University of California Press.

Laudan, L., Donovan, A., Laudan, R., Barker, P., Brown, H., Leplin, J., Thagard, P., & Wykstra, S. (1986). Scientific change: Philosophical models and historical research. *Synthese, 69*, 141–223.

Lazzeri, F. (2011). *Funcionalismo e behaviorismo sobre predicações psicológicas ordinárias* [Functionalism and behaviorism about ordinary psychological ascriptions]. Master's thesis, University of Brasília.

Lazzeri, F. (2013a). Observações sobre o behaviorismo teleológico: Parte I [Remark on teleological behaviorism: Part I]. *Acta Comportamentalia, 21*, 241–258. http://www.revistas.unam.mx/index.php/acom/article/view/39029

Lazzeri, F. (2013b). Observações sobre o behaviorismo teleológico: Parte II [Remark on teleological behaviorism: Part II]. *Acta Comportamentalia, 21*, 391–408. http://www.revistas.unam.mx/index.php/acom/article/view/41753

Lazzeri, F. (2013c). Referir-se-ão as categorias psicológicas ordinárias a causas internas do comportamento? [Do ordinary psychological categories refer to inner causes of behavior?]. *Philósophos, 18*, 41–73. https://doi.org/10.5216/phi.v18i1.20560

Lazzeri, F. (2013d). Um estudo sobre definições de comportamento [A study on definitions of behavior]. *Revista Brasileira de Análise do Comportamento, 9*, 47–65. https://doi.org/10.18542/rebac.v9i1.2131

Lazzeri, F. (2014). On defining behavior: Some notes. *Behavior and Philosophy, 41*, 65–82. https://www.behavior.org/resources/866.pdf

Lazzeri, F. (2015a). Dynamic interactions with the environment make up our psychological phenomena: A review of Noë's *Out of our Heads*. *The Psychological Record, 65*, 215–222. https://doi.org/10.1007/s40732-014-0090-3

Lazzeri, F. (2015b). On the place of behavior in the analysis of psychological categories. *The Psychological Record, 65*, 567–577. https://doi.org/10.1007/s40732-015-0121-8

Lazzeri, F. (2015c). *Categorias psicológicas ordinárias, comportamento e análise do comportamento* [Ordinary psychological categories, behavior, and behavior analysis]. Doctoral dissertation, University of São Paulo.

Lazzeri, F. (2016). On some common objections to a behavioral approach to psychological categories. *Philosophical Psychology, 29*, 405–418. https://doi.org/10.1080/09515089.2015.1121540

Lazzeri, F. (2017a). The place of ordinary psychological categories in behavior analysis. *Journal of Mind and Behavior, 38*, 167–192.

Lazzeri, F. (2017b). Extended functionalism from a behavioral perspective. *Behavior and Philosophy, 45*, 1–21. https://behavior.org/wp-content/uploads/2018/02/BPv45_LAZZERI.pdf

Lazzeri, F., & Oliveira-Castro, J. M. (2010). Termos psicológicos disposicionais e análise do comportamento [Dispositional psychological terms and behavior analysis]. *Princípios, 17*, 155–183. https://periodicos.ufrn.br/principios/article/view/989

Mayr, E. (1961). Cause and effect in biology. *Science, 134*, 1501–1506. https://doi.org/10.1126/science.134.3489.1501

Millikan, R. G. (1993). *White queen psychology and other essays for Alice*. MIT Press.

Neander, K. (1991). The teleological notion of 'function'. *Australasian Journal of Philosophy, 69*, 454–468. https://doi.org/10.1080/00048409112344881

Palmer, D. C. (2009). The role of private events in the interpretation of complex behavior. *Behavior and Philosophy, 37*, 3–19. https://www.jstor.org/stable/41472419

Rachlin, H. (1991). *Introduction to modern behaviorism* (3rd ed.). Freeman.

Rachlin, H. (1994). *Behavior and mind*. Oxford University Press.

Rachlin, H. (1995). Things that are private and things that are mental. In J. T. Todd & E. K. Morris (Eds.), *Modern perspectives on B. F. Skinner and contemporary behaviorism* (pp. 179–183). Greenwood Press.

Rachlin, H. (2012). Is the mind in the brain? A review of *O*ut of our heads: Why you are not your brain, and other lessons from the biology of consciousness, by Alva Noë. *Journal of the Experimental Analysis of Behavior, 98*, 131–137. https://doi.org/10.1901/jeab.2012.98-131

Rachlin, H. (2014). *The escape of the mind*. Oxford University Press.

Rachlin, H. (2021). Teleological behaviorism: Origins and present status. In D. Zilio & K. Carrara (Eds.), *Contemporary behaviorisms in debate*. Springer/Paradigma.

Rachlin, H., & Frankel, M. (2009). Taking pragmatism seriously: A review of William Baum's *Understanding Behaviorism: Behavior, Culture, and Evolution* (2nd edition). *Journal of the Experimental Analysis of Behavior, 92*, 131–137. https://doi.org/10.1901/jeab.2009.92-131

Ruse, M. (2019). Why did the stegosaurus have plates, or is biology second-rate because it thinks in terms of ends? *Revista de Humanidades de Valparaíso, 14*, 9–25. https://doi.org/10.22370/rhv2019iss14pp9-25

Ryle, G. (1949). *The concept of mind*. Hutchinson.

Skinner, B. F. (1935). The generic nature of the concepts of stimulus and response. *Journal of General Psychology, 12*, 40–65. https://doi.org/10.1080/00221309.1935.9920087

Skinner, B. F. (1953). *Science and human behavior*. Macmillan.

Skinner, B. F. (1957). *Verbal behavior*. Prentice-Hall.

Skinner, B. F. (1969). *Contingencies of reinforcement*. Appleton-Century-Crofts.

Skinner, B. F. (1974). *About behaviorism*. Knopf.

Skinner, B. F. (1981). Selection by consequences. *Science, 213*, 501–504. https://doi.org/10.1126/science.7244649

Skinner, B. F. (1990). Can psychology be a science of mind? *American Psychologist, 45*, 1206–1210. https://doi.org/10.1037/0003-066X.45.11.1206

Tolman, E. C. (1932). *Purposive behavior in animals and men*. Appleton-Century-Crofts.

Tolman, E. C. (1951). *Behavior and psychological man*. University of California Press.

Watson, J. B. (1930). *Behaviorism* (2nd ed.). Norton.

Wright, L. (1976). *Teleological explanations*. University of California Press.

Chapter 3
Response to Comments of Lazzeri

Howard Rachlin

In 2002, the editors of the online journal *http://Metapsicologia.com* thought to ask some current researchers if they would retrospectively review the classics in our field. They asked me to review Skinner's (1938) "Behavior of Organisms" (*B of O*); I happily agreed. I had read pieces of this founding text but never the whole book, and I was eager to see what had held up and what had been discarded in the more than half-century since its publication. Several concepts had been discarded by Skinner himself in the intervening years. Chief among these was the "reflex reserve" which had played a major part in generating experiments and explaining experimental results in *B of O*. But one general principle, I felt, had stood up—that psychology was the study of "the organism as a whole." Here is a statement of that principle in the last chapter of *B of O*:

> The concepts of 'drive,' 'emotion,' 'conditioning,' 'reflex strength,' 'reserve,' and so on have the same status as 'will' and cognition' but they differ in the rigor of the analysis with which they are derived and in the immediacy of their reference to actual observations. In spite of the conceptual nature of many of our terms we are still dealing with an existent subject matter, which is the behavior of the organism as a whole. Here, as elsewhere in the experimental sciences, a concept is only a concept. Whether or not it is fictitious or objectionable cannot be determined from its conceptual nature. (p. 441).

If one hasn't been reading the book, the paragraph is a bit confusing. Let me spend some time discussing it, what it really means, and why I believe it is so important. Although Skinner received his PhD from the psychology department, his main thesis advisor was W. J. Crozier, a biologist known for his studies of tropisms in mammals such as mice and rats. For example, a mouse's geotropism as it interacted with its phototropism could be mathematically described by its path on an inclined plane with a light at one end. The forces on the animal were derivable from the behavior of the whole animal. Skinner's earliest experiments (many described in *B of O*) were

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of this nature. His earliest lever, for example, was a seesaw-like runway on which the rat ran back and forth. Not only was there no manipulation of the rat's insides, but there was also no speculation about what might be going on there. Discriminations were discussed, but they were discriminations in the rat's overt behavior and not anything supposed to be going on inside the rat. This may seem like standard behavioral practice, but, in the context of Hull and Tolman, two of the most prominent behaviorists of the time, it was unusual.

Another apparent oddity, then as now, is the bringing together of words such as "conditioning" with words such as "cognition" under the common label of "concepts" differing only "in the rigor of the analysis with which they are derived and in the immediacy of their access to actual observations." When I read the quoted paragraph, I tend to think of concepts such as "force" in physics, a concept in use, imprecisely, in everyday language and as precisely as currently conceivable in modern physics. Why should all the terms Skinner lists not have such double or triple meanings? When Skinner classifies them all together as "concepts," what does he mean? What does the concept "emotion" have in common with the concept "cognition"? The answer, contained in the very same paragraph, is clear. They are both patterns, more or less abstract, more or less spread out in time, more or less spread out in social extent, of *the behavior of the organism as a whole*. They are not located in another world; they are not different neural pathways entering our bodies through our senses and ending in some sensorium, out of contact with the world. In *B of O*, an emotion, like cognition, is conceived as a pattern in the overt behavior of an organism in contact with the world around it, influencing and influenced by that world.

I would like to be able to say that, after this striking, revolutionary statement of principle, Skinner supported and defended this view of psychology for the rest of his life. Unfortunately, that is not the case. Only 7 years after the publication of *B of O*, the September 1945 issue of *The Psychological Review* published a series of articles by prominent psychologists on *operationism*, then a dominant concept in the philosophy of science. Skinner's (1945) contribution to this series was titled, "The operational analysis of psychological terms" (*OAPT*). As I read it, *OAPT* is divisible into two distinct parts. The first part repeats and expands the argument from *B of O* that psychology is the study of the behavior of the organism as a whole. The second part of *OAPT* abandons this notion and considers internal responses, reinforcers, and discriminative stimuli as legitimate concepts. Thus, the second part of *OAPT* is a foundational document for radical behaviorism (*RB*).

What does a person mean when he says, "red," in the presence of a red object? Is he referring to the red object, or is he (more fundamentally) referring to some event, perhaps neural, inside his head or in his peripheral nervous system? One must ask, "Why is the person saying 'red'?" Then one can find, in the environment, the discriminative stimulus and the (social) reinforcer of this response.

But, as Skinner goes on in *OAPT*, this provision is lacking in the case of many "'subjective' terms, which appear to be responses to *private* [emphasis in original] stimuli":

> we must know the characteristics of verbal responses to private stimuli in order to approach the operational analysis of the subjective term…. The response 'My tooth aches' is partly under the control of a state of affairs to which the speaker alone is able to react, since no one else can establish the required connection with the tooth in question. (p. 275).

This is a crucial point. What is the private stimulus that acts when a person says, "My tooth aches"? Is it the (diseased) tooth or is it the ache? If we understand the private stimulus to be the diseased tooth, there is no problem with public access. A child cries, and her mother says, "Where does it hurt?" The child opens her mouth and points to her tooth (or points to her stomach, or her ear, etc.) The mother says, "Oh, you have a toothache." As experience is gained, the child learns the vocabulary of pain.

A toothache is no more problematic for learning the language of pain than a splinter in a finger. It would not be strange if a child were to say, at some point, before these discriminations were mastered, "I have a toothache in my finger." The discriminative stimulus may be wholly internal, as with a diseased tooth, or visible on the surface, as with a splinter. There is a commonality between the two as between different colored stimuli, and there may also be some neural commonality. But the *basic* commonality, the thing that makes them both pains, is not the physiological one—it is the behavioral one. In both cases, the child is harmed; in both cases, the parent (i.e., society) needs to do something about it. That is the basic commonality; that is what makes both of the child's acts pains. There is nothing in Skinner's extended discussion in *OAPT* of how terms for internal events may be socially learned, to suggest that, when a person says, "I am in pain," pain itself is the discriminative stimulus. Moreover, as Skinner says, "A similar analysis could be made of all terms descriptive of motivation, emotion, and action in general, including…the acts of seeing, hearing, and so on" (p. 279). Skinner goes on with emphasis: "*A differential reinforcement* [hence scientific understanding] *cannot be made contingent upon the property of privacy*. This fact is of extraordinary importance in evaluating traditional psychological terms" (p. 279, emphasis in original). For scientific understanding, there must be some public correlate of the internal event.

So far so good—or at least so consistent with *B of O*. But then Skinner makes an oblique reference to Pavlov and Watson. He says, "The original behavioristic hypothesis was, of course, that terms of this sort ['psychological' or mental terms] were descriptions of one's own (generally covert) behavior" (p. 280). I believe he refers here to Watson's conception of thought—that a person thinking of hammering a nail is moving her muscles as she would have done if she were overtly hammering a nail, except she's doing it covertly. People have wasted their professional lives trying to measure such covert behavior and have failed. Seen from the viewpoint of *B of O*, it is a ridiculous idea and does not stand scrutiny. Suppose the muscle was isolated and stimulated on a lab bench. Is it thinking? Can it possibly be thinking? Of course not. Perhaps what Watson really meant was that the thought resided in the proprioceptive nerves emanating from the muscle, or perhaps in the motor nerves going from the brain to the muscle, or perhaps in the brain itself. If the whole muscular circuit were isolated, could it think? If a whole human brain could be isolated and stimulated without a body, could it think? An affirmative answer to this question is given by some modern philosophers—neural identity theorists. According to the web-based Stanford Encyclopedia of Psychology:

> The identity theory of mind holds that states and processes of the mind are identical to states and processes of the brain.… Here I take identifying mind and brain as being a matter of identifying processes and perhaps states of the mind and brain. Consider an experience of pain, or of seeing something, or of having a mental image. The identity theory of mind is to the effect that these experiences just *are* brain processes, not merely *correlated with* brain processes.[1]

This is the theory that Skinner demolishes in the last chapter of *B of O*. The main problem with neural identity theory is that if the mind were *identical* with the brain (or the muscles or the proprioceptive nervous system), it would be essentially a prisoner in the body, forever out of contact with the world. Introspection would be meaningless because there is no other entity inside of us through which the brain may be viewed and no internal mirror by which the brain might view itself. Even if there were such an entity inside of us, it could only consist of more neurons—another, higher set of neurons, again supposed to be *identical* with consciousness—and so on ad infinitum.

An alternative to neural identity theory presented in *B of O* (however briefly) is what might be called behavioral identity theory (see also Rachlin, 2014). According to this theory, mental terms are abstract conceptions of overt behavior; that is what they just *are*; they have no other reality inside the head or out of it. The great advantage of behavioral identity theory is that it holds mental states to be observable—directly by other people and indirectly by the actor through reflection from the environment, including the behavior of other people. One does not need to invent a homunculus. Such a theory implies that other people may know our mental states or may know better than we do. To give a trivial example: A 5-year-old child asks me, "What's your favorite color?" I say, "red." My wife of some 57 years says, "Are you kidding? All your pants are green, all your shirts are green, all your underwear is green. Your favorite color is green." Of course, she is right, and I am wrong. The relevant data to answer the child's question, as for all my mental states, are not in my head, subject to introspection, but in the patterns of my behavior—past, present, and future, subject to direct observation by anyone close to me over a length of time. These patterns *are* my mental states; they are not just effects or outputs of my mental states. The dualist idea that overt behavior may be merely the observable effect of an internal, nonphysical mental state, which can be scientifically studied only through analysis of its overt effects, is the methodological behaviorism of Boring and Stevens, which Skinner rightly rejects in *B of O*.

In the latter half of *OAPT*, Skinner seems to have abandoned the behavioral identity theory of *B of O*. Instead he proposes a modified form of Watson's covert, neuromuscular identity theory. He considers (p. 280) the response, "red." The discriminative stimulus for that response could have been a question from a 5-year-old child. The discriminative stimulus for the response "red," as Skinner

[1] https://plato.stanford.edu/entries/mind-identity/

discusses it in *OAPT*, could have been understood to be part of a psychophysical experiment—say, the presentation of an ambiguously colored red-orange circle together with the question: "Is this circle closer to red or orange?" The situation does not demand introspection. Skinner points out that *the discriminative stimulus for the verbal response, "red," is the physical red object and not an interior image*. I emphasize this because it is just after this (valid) point where he crosses a line.

In considering the response, "I see red," as opposed to the response, "red," instead being consistent with his argument, and asking, "*Why* would a person say, 'I see red'?" and finding social discriminative stimuli and reinforcement for such a statement (perhaps the presentation of a very pale red disk and the question: "Do you see any color here?"), he says: "To see red is to react, not to red…but to one's reaction to red…. According to the present analysis it may be evoked …by *any private accompaniment* of overt seeing" [emphasis in original] (p. 281). This completely gratuitous assertion leads Skinner directly to radical behaviorism (*RB*).

Skinner speculates on some ways that external reinforcement may act on private behavior (without specifying what form, muscular or neural, and, if neural, where in the nervous system such private events take place). These speculations are vague and almost wholly based on adventitious contiguities between internal and external events.[2] I could understand them only by relating them to adventitious reinforcement as it may occur in overt behavior. For Skinner, it seems the private event is reinforced in the same way that the idiosyncratic *style* of an overt response is reinforced—for example, the batting style of a professional ballplayer: elbow up or down, bat held vertical or horizontal, etc. If the response as a whole is reinforced (if she's a good hitter), idiosyncratic aspects of her style would be reinforced as well and would persist. So might internal hitting (or hammering) conceivably be reinforced? As Skinner states (p. 280), this is only a "slight modification" of "the original [Watsonian] behavioristic hypothesis… that terms of this sort [mental terms] were descriptions of one's own (generally covert) behavior." Indeed, it is very much like Watson's conception of these terms. *And, it has all the problems of that conception.* As Skinner himself notes in this very article (p. 283), "…Watsonianism was, in fact, practically wrecked in the attempt to make [private behavior as a stand-in for mental terms] work." Indeed, there are no stimuli, no responses, and no reinforcers inside the body. As early as 1935, in his thesis, Skinner argued that even simple reflexes (e.g., food ingestion—salivation) are correlations over time between classes of external stimuli and *overt* behavior.

Does it make any sense to talk about internal stimuli, responses, and reinforcers? I think that the nub of the difference between my view of the mind and that of Lazzeri comes down to the above question. It is a question that came up very early in psychology (Dewey, 1895). As I indicated above, within a single article, Skinner took both sides of the argument, first arguing that internal stimuli, responses,

[2] Since Staddon and Simmelhag's (1971) seminal article on "superstition" in the pigeon, it has been generally accepted that an adventitious *contiguity* between discriminative stimulus, response, and reinforcer is insufficient to establish operant conditioning, which is based rather on *correlations* over time between the elements of the three-term contingency.

and reinforcers make no sense and then arguing that they do. I must confess however that with all possible good will and intellectual effort, I cannot conceive how internal stimuli, responses, and reinforcers are supposed to work. If some neural event is a stimulus, it must be a stimulus *to* something; if it is a response, it must be a response *of* something. Otherwise, there is no difference between a stimulus and a response. If a neural event is a reinforcer, it must be capable of increasing the frequency of a response on which it is contingent. These concepts thus imply the existence of some *system* that may be stimulated, may respond, and may be reinforced. I believe that such a system is an intact organism.

But the concepts of internal stimuli and responses are as clear as day compared to that of internal reinforcement. If it were possible to internally reinforce my own behavior, why should I not do it all the time? Why wait for the behavior? Or if internal reinforcement is internally contingent on an internal response, why not just keep doing the response? The idea that reinforcement can originate inside the body violates not only Skinner's own conception of how reinforcement works but also Premack's generally accepted theory of reinforcement as a contingency of a higher valued act on a lower valued act. The concept of an organism filled with covert stimuli, covert responses, and covert reinforcers makes less sense than one filled with covert hopes, fears, perceptions, cognitions, etc. Neither of these ways of thinking is useful in behavioral prediction, control, or understanding.

Internal events, events within the organism, cannot be analyzed without postulating another, smaller, organism within the larger one. I am grateful to Lazzeri for pointing to evolutionary studies. But evolution has its effects only at the border between the whole organism and the environment. I have recently argued for a view of operant behavior as an evolutionary process (see also Baum, 2016). Just as a maladaptive organism may be killed off within its ecological context so a maladaptive behavior may be extinguished within its behavioral context. Hearts, livers, and lungs evolve, but a heart or liver cannot be killed without killing the whole organism. Evolution works on populations. Individual organs evolve only as they play their part or fail to play their part in the survival or death of the whole organism. That survival or death is the bottom line in biological evolution. Correspondingly, a neural mechanism cannot be extinguished without extinguishing the overt act that it supports.[3]

To take Lazzeri's example of an internal event, consider "a dry throat." A dry throat may be viewed as a stimulus to a person's salivary gland, or it may be viewed as a response to a low level of water in the system. It is certainly an unpleasant state of affairs and may be seen as punishment for failure to fill one's canteen before a hike. But what *internal* event is it punishing? Internal mechanisms are physical systems which may be (and have been) studied in terms of efficient causes and are properly part of biology or neurobiology, not psychology. There are unquestionably feedback mechanisms within the body, but such feedback cannot reward or punish

[3] In Rachlin, 2019, I argue that individual acts and patterns of acts may evolve as do individual organisms and groups of organisms.

the behavior of an organ or set of organs. It is true that designers of neural networks speak of "reinforcement" of some set of pathways through the system. And, in their designs, "reinforcement" works to increase the survival of that set of pathways. But the reinforcement is always in the service of the network as a whole—winning at chess, Jeopardy, etc.—and makes no sense outside of that purpose. Internal stimuli, responses, and reinforcers are for behaviorists, while internal homunculi are for cognitive psychologists—a way of avoiding the real work involved in understanding (predicting and controlling) behavior.

Finally, let us consider Lazzeri's example of an operant covert behavior: "…working out the expenses of a trip subvocally, without the help of any tool or movement of the limbs." No one can deny that people talk to themselves and perform mental arithmetic. The issue is whether such behavior is usefully categorized as operant behavior. To digress for a moment, in Skinner's laboratory at Harvard, there were a group of rats, collectively called "Samson." These rats, deprived of water, would be placed, one at a time, in a wire mesh enclosure in which they had been trained to press a lever through a given angle and for which they received a small water reward. The required force on the lever could be varied by moving a weight along a slide outside of the chamber. The initial setting was at a very low force requirement, but it could be, and was, raised gradually to a value about twice Samson's weight. To the amusement of the onlookers, each avatar of Samson developed its own style of pressing the lever as the required force increased to impossible values. Some would warm up with a few light presses and then come down hard; some would crawl up on the wire mesh wall, grasp the lever in their front paws, and then try to walk downward as they pressed. The point of the demonstration was that the style of the individual rats did not matter. The essential properties of the operant (the time taken to press the lever) were the same function of the force requirement on the lever for all the rats (albeit with different parameters).

Similarly, what counts for the trip expenses are the cost of travel, the amount of money in his bank account, his other expenses, etc. *How* the man calculates his expenses for the trip—by mental arithmetic, by digital calculator, by abacus, by pencil and paper, on his fingers—is irrelevant. I know people who can perform very complex calculations "in their heads." (I do not play poker with these people.) What they do may be interesting; *how* they do it may be important for neuroscience. But, for the behaviorist, only why they do it is relevant.[4]

References

Baum, W. M. (2016). *Understanding behaviorism: Behavior, culture, and evolution*. Wiley.

Dewey, J. (1895). The theory of emotion. *Psychological Review, 2*(1), 13–32. https://doi.org/10.1037/h0070927

[4] I have had many occasions to read and respond to comments on my work. But I have rarely learned as much from such commentaries as I have from these, by Professor Lazzeri.

Rachlin, H. (2014). *The escape of the mind*. Oxford University Press.
Rachlin, H. (2019). Group selection in behavioral evolution. *Behavioural Processes, 161*, 65–72. https://doi.org/10.1016/j.beproc.2017.09.005
Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis*. Seventh printing (1966). Appleton-CenturyCrofts.
Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review, 52*(5), 270.
Staddon, J. E. R., & Simmelhag, V. (1971). The "superstition" experiment: A reexamination of its implications for the principles of adaptive behavior. *Psychological Review, 78*, 3–43.

Part II
Molar Behaviorism

Chapter 4
Introduction to Molar Behaviorism and Multiscale Behavior Analysis

William M. Baum

Brief Autobiography

I was born in New York City and grew up there. As a boy, I developed an interest in animals and their behavior. Over the years, besides dogs and cats, I kept fish, birds, turtles, and mice. When I was in high school, a family friend gave me two popular books about animal behavior, and I supplemented these with books I borrowed from the library. I conducted a study of the relation between maze-solving ability and social dominance in male mice in my bedroom, because neither facilities nor guidance was available at my school. I won a prize for my report from the New York State Board of Regents.

In my first year at Harvard College, I started concentrating in biology, because I assumed that was the right department for studying animal behavior, but in the spring of my freshman year, looking through the course catalog, I noticed "Nat. Sci. 114," which included "behavior" in its title. I went to the first class, found it interesting, and enrolled in it. The professor was B. F. Skinner. The class was divided in half: one half read the textbook, *Science and Human Behavior*, and the other half did the material as programmed instruction on teaching machines. I was in the latter group and enjoyed learning from the machines.

Now alerted that courses on behavior were available in the psychology department, the next semester I enrolled in a course taught by R. J. Herrnstein. The material interested me, and I soaked it up, but on the first exam I wrote such long detailed answers that I didn't finish all the questions. I went to see Herrnstein in his office, and he told me, "Just write faster." After that, my performance was excellent, and Herrnstein noticed. He asked me about my concentration. After I told him I was concentrating in biology, he told me I should switch to psychology, because biology

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required eight full courses, whereas psychology required only six, and I would be free to take courses in other areas.

I switched into psychology, and Herrnstein became my adviser. I took courses from him and other members of the department—including J. C. (Joe) Stevens and A. C. Catania. In those days, psychology at Harvard was considered a natural science, including sensory systems, behavior, and physiology. Avoiding the standard courses in psychology, in the Social Relations Department, suited me. In the summer after my junior year, S. S. Stevens and Joe Stevens hired me to work in the Psychophysics Laboratory. I ran experiments on sensory scaling and motor learning. In my senior year, I asked Herrnstein about doing an honors thesis. He suggested that I try an automated version of a T-maze to study choice in rats. He took me into one of the experimental rooms, showed me a relay rack, showed me how a relay worked and how to make a lockup, and then told me to learn how to program. I built a three-lever chamber with two dipper feeders for sucrose solutions of various concentrations and programmed various probabilities on the two choice levers. (The lever on the opposite wall initiated trials in which the rats pressed one of the choice levers.) My committee consisted of Herrnstein and S. S. Stevens, who grumpily acknowledged that I had done a good job and told me to get *Elements of Style*, by Strunk and White, so that I could learn to write better—in retrospect, this was a great compliment.

After graduating, I spent the summer in San Francisco painting and sculpting, and I enrolled in art school at New York University in the fall. My father, who was a painter, was dead set against my becoming an artist and urged me to choose science. By Christmas, I decided he was right. I dropped out of art school, applied for graduate school, and looked for work in science laboratories. I worked in a laboratory giving drugs to rats and extracting their pituitary glands and then in an experimental cardiac surgery laboratory.

In the fall of 1962, I entered the graduate program at Harvard University. The number of students admitted was unusually large, 12 or 13, although a few never finished. We had only two choices: psychophysics or behavior. I chose behavior, and Herrnstein became both my adviser and my mentor, and I began calling him "Dick."

The time was exciting. For me, a pivotal moment was the first time that, at one of our weekly research meetings, Dick drew on the blackboard a feedback function for a variable-interval schedule. Even before that, Howie Rachlin, Phil Hineline, and I began discussing the possibilities of the molar view of behavior, transcending momentary events and examining relations extended in time, such as between response rate and reinforcer rate. I found these discussions highly stimulating.

After completing my doctoral work, I spent a postdoctoral year at Cambridge University in the subdepartment of animal behavior studying reproductive behavior of canaries. I returned as a postdoctoral fellow to Harvard the following year, 1966. After some months, I was hired as a postdoctoral researcher with the responsibility to create a computer-based behavioral laboratory. I started teaching in 1967. In 1970, I was appointed assistant professor. From 1966 to 1970, Howie Rachlin and I were at Harvard together and collaborated.

Rachlin and I produced three papers in the molar framework, two published in (1969) and one in (1972). The 1969 paper, "Choice as time allocation," was soon declared a citation classic. In a paper that came out in 1973, "The correlation-based law of effect," I tried to lay out the basics of this new way of thinking about behavior.

In 1973, I still thought that the molar view and the molecular view of behavior could be complementary. By the mid-1970s, I began to see that the time-based view of behavior was incompatible with the traditional molecular view. A choice experiment with rats that came out in 1976 convinced me that timing activities rather than counting discrete responses made the most sense, even if counting microswitch operations provided reliable data. I reasoned that the switch operations roughly indicated the amount of time spent (Baum, 1976).

In 1975 and 1976, I spent 2 years as a senior researcher at the National Institutes of Health, at the Laboratory of Brain Evolution and Behavior, studying behavior of rats and mice living in colonies, with John C. Calhoun. After that, I went to the University of New Hampshire, where I remained until 1999, when I took early retirement and moved to San Francisco.

By 1995, I was able to articulate the concept of a temporally extended activity and the nesting of smaller-scale activities within any more extended activity (Baum, 1995a). The concept had two implications. First, I saw that the molecular view wasn't just inadequate but led to a completely implausible view of life outside the laboratory and, for that matter, wasn't much better even in the laboratory. My reasoning was buttressed by readings in philosophy, notably the writings of Benjamin Whorf and Erwin Schrödinger. Second, I began to see how the concept of scale was central to the molar view, but scale only became central in my writing in 2002, with the publication of "From molecular to molar: A paradigm shift in behavior analysis."

As the view developed further, I realized that the label "molar" was misleading, because people seemed to assume it only applied to phenomena at long time scales and couldn't apply to phenomena at short time scales. Following Phil Hineline's suggestion, I began calling it the "multiscale molar view," with the intention that I would eventually just call it the "multiscale view." I applied the concept of scale in analyzing data from experiments I was doing with Michael Davison and Carlos Aparicio at the time (Aparicio & Baum, 2009; Baum & Davison, 2004). By 2013, in a paper, "What counts as behavior: the molar multiscale view," I was able to put together the time-based view with scale, choice, and evolution.

The importance to behavior analysis of making contact with evolutionary theory cannot be overstated. Behavior analysis is properly part of biology. It is not a part of psychology but an alternative to psychology. For psychology, behavior is a superficial phenomenon that must be understood by inferences to a "deeper" level: the mind or the brain. As long as behavior is not considered a subject matter in its own right and behavioral phenomena considered secondary, a true natural science of behavior is impossible. Biologists often are naïve about the mind and consciousness, but they have no trouble thinking about behavior as real and primary. Biologists who I talk to readily accept the idea that behavior is an organism's interaction with the environment.

The organism is not the agent of its behavior but the medium of behavior. Organisms and behavior go hand in hand, because they both enhance the fitness of the genes that promote them. Organisms and behavior would not exist if the genes making for organisms were not selected by having greater reproductive success as a result of being located in organisms.

The connection to evolution and natural selection allows a rethinking of the concept of reinforcement. Once we recognize that ethologists' "fixed action patterns" are just as relevant to understanding behavior as is the notion of operant behavior, we can bring the two together, as Segal (1972) showed, with the concept of *induction* (Baum, 2012a). Events impacting fitness, *phylogenetically important events* (PIEs), induce activities that enhance good (fitness-increasing) PIEs and mitigate bad (fitness-reducing) PIEs and also induce operant activities correlated with these PIEs. The operant activities that produce or avoid the PIEs are induced along with the unconditionally induced activities. Events correlated with PIEs become proxies for them and induce the same activities as the PIEs themselves induce.

Molar Behaviorism

Behaviorism is the philosophy that underpins a science of behavior, which is usually called behavior analysis. The central premise in behaviorism is that a science of behavior is possible. If a science of behavior were impossible, behaviorism would be unnecessary.

A science of behavior could be made impossible in a variety of ways. In psychology, the supposition that behavior is not a subject matter in its own right would make the science impossible. Particularly the assumption that behavior is done by an agent—an inner self, the mind, or the brain—makes the science of behavior impossible. Indeed, any notion that behavior is caused by internal, unobservable entities, such as a person's inner intentions, beliefs, desires, or thoughts, makes the science impossible or, at least, incoherent.

Skinner (1945) made a mistake when he advanced private events to account for thoughts and feelings. He was responding to the criticism that behaviorism ignores the most important part of human life, our inner thoughts and feelings. He would have done better to question the traditional view that our behavior is caused by thoughts and feelings and to have stayed with the view that the origins of behavior (its "causes") always lie in the past and present environment. He and other behavior analysts tried to save the inferences to private events by calling them "interpretation." Such "interpretation" bears no resemblance to explanation in other sciences, which always refer to empirical relations verified in observation. Skinner's "interpretations" resemble not science but poetry or literature.

Positing private events as causes of behavior denies the science of behavior. When a dog limps and whimpers, we look for a thorn in its foot. The injury is the cause of the limping and whimpering, not "pain," not a private stimulus. Similarly, when a person limps and says, "I have a pain in my foot," the cause of the limping and saying is the injury, not a private stimulus called "pain." The temptation to attribute behavior to private stimuli derives from everyday talk about behavior, but for a science of behavior private stimuli are unobservable causes. When Jane says to her husband Tom, "I'm tired; let's go home," she is not reporting on a private stimulus; her utterance comes from a long history with such utterances and their effects (perhaps escaping from an uncomfortable situation). Verbal behavior depends primarily on the presence of a listener who is likely to respond; other aspects of the context may be important, too, but combine with the primary context. Verbal behavior, like all other behavior, occurs because of past and present environment, not thoughts and feelings. (See Baum, 2011, for further discussion of private events.)

In a science of behavior, behavioral events are natural events. Natural events are explained by their relation to other natural events. For example, an increased frequency of hurricanes in the Caribbean is related to changes in water temperature, which are related to increased global temperature (i.e., climate change). Natural events, if thought of as "caused," are caused by other natural events.

In particular, natural events are not caused by agents. Natural events just happen; they are not done by anyone. When a stone falls, it accelerates as it approaches the ground. No physicist would say the stone accelerates because it (privately) wants to reach the ground. Saying it accelerates because of gravity would also be a mistake, because the acceleration is an example of gravity, and making gravity a cause would make it an unseen agent—committing what Ryle (1949) called a "category error." Similarly, no behavior analyst should say that a rat presses a lever because it "knows" that pressing the lever produces food. The rat's pressing results from its training, which was observable, in contrast to its "knowledge," which is not. As with gravity, one could say at best that the rat's lever pressing *is* its knowing. No more than the rat are we the doers of our deeds.

Multiscale Behavior Analysis

At the beginning of the twentieth century, scientists studying behavior relied on only two concepts: reflexes and associative bonds. Both entailed discrete events and contiguity between the events. Pavlov's (1960/1927) conditional reflexes (called "conditioned" due to a translating error) depended on contiguity between a conditional stimulus and an unconditional stimulus (which he also called a "reinforcer"). Pairing the two stimuli was supposed to result in a bond between the conditional stimulus and a conditional response. Before Pavlov, nineteenth-century philosophers and psychologists considered ideas to be connected by associative bonds. The associative bond, when combined with the reflex, became a bond between stimulus and response, or an S-R bond. Ethologists invented a similar concept, in which a sign stimulus was said to "release" a fixed action pattern. Thus was born the vocabulary of stimulus, response, and reinforcer.

The early behaviorists Watson (1930) and Thorndike (2012/1911) theorized about S-R bonds. Although Watson considered S-R bonds sufficient, Thorndike added to the associative laws, such as the law of contiguity, another law, which he called the "law of effect." According to the law of effect, an S-R bond is strengthened when a satisfying event closely follows the S-R sequence.

Skinner (1938) introduced a new concept with his invention of operant behavior. In 1938, he tied it to the reflex, but he soon recognized that operant behavior cannot be characterized by S-R bonds, because no identifiable stimulus precedes each occurrence of the response. He followed with two inventions: (a) measuring behavior as response rate and (b) stimulus control. With these two new concepts, Skinner left S-R bonds behind. Instead, he thought of response rate as the primary measure of behavior and a discriminative stimulus as exerting "control" by modulating response rate. Thus, stimulus control replaced the eliciting of the response by the stimulus that characterized the reflex. Skinner's innovations pointed in a direction away from discrete responses and contiguity, but he never made a further move in that direction because he never went beyond the "operant" as a class of discrete responses or the theory that an immediately following reinforcer "strengthens" an operant response.

Critique of the Molecular View of Behavior

The view that behavior consists of discrete responses that are strengthened by closely following (contiguous) reinforcers may be identified as the molecular view of behavior (e.g., Skinner, 1948). It seems to explain the observation that response rate increases when responses produce reinforcers (e.g., food). That is about all it explains, however. It doesn't explain even the most basic phenomena in behavior analysis. For example, the molecular view cannot explain why ratio schedules maintain extremely high response rates, whereas interval schedules maintain response rates that are moderate—that is, lower but not extremely low (e.g., Baum, 1993). In attempting to explain the rate difference, molecular theorists cite differential reinforcement of relatively long interresponse times (IRTs) on interval schedules. Morse (1966), for example, showed that on an interval schedule IRTs followed by a reinforcer generally exceed IRTs not followed by a reinforcer. The reason is that the longer the IRT, the more likely an interval will have timed out during the IRT, setting up reinforcer delivery for the next response. Since IRT is the reciprocal of response rate, differential reinforcement of long IRTs explains why rate on an interval schedule should be lower than rate on a ratio schedule.

The trouble with this IRT theory is that it predicts something incorrect. If the key to lower rate on interval schedules is that the probability of reinforcer delivery increases as IRT increases, then IRTs should increase until the probability equals 1.0. For every response to produce a reinforcer, response rate on an interval schedule would have to be extremely low, but response rates on interval schedules, though lower than rates on ratio schedules, are still moderately high. When I have pointed out this theoretical failure, some molecular theorists answer by suggesting that such long IRTs would tend to increase the inter-reinforcer interval. That is so, but it is not part of the theory. In particular, because IRT is the reciprocal of response rate, and inter-reinforcer interval is the reciprocal of reinforcer rate, the suggested addition actually introduces an extended relation between response rate and reinforcer rate.

The moderately high rates on interval schedules cannot be explained without reference to reinforcer rate. When response rate is low on an interval schedule, increases in response rate produce large increases in reinforcer rate. As response rate rises to moderate levels, reinforcer rate ceases to increase. This relation is captured in the interval schedule's feedback function, which is negatively accelerated and approaches an asymptote (Baum, 1992).

Not only does the IRT theory fail to explain why interval response rates are as high as they are, it also fails even more obviously to explain the extremely high rate on ratio schedules, because in a ratio schedule, no relation exists between IRT and reinforcer probability. When one considers that the feedback function for a ratio schedule is simply an increasing straight line, an explanation in more extended terms appears. Increases in response rate always increase reinforcer rate; the only limit is the organism's ability to respond quickly. Not differential reinforcement of IRTs but differential reinforcement of response rate by increasing reinforcer rate explains the extreme response rates that ratio schedules maintain.

Another phenomenon that molecular theory cannot explain is negative reinforcement, particularly avoidance. Suppose Tom, a divorced man with a grown son, Sam, receives a phone call from Sam inviting Tom to his wedding. Tom declines the invitation because Sam's mother, Tom's ex-wife, will be at the wedding, and Tom doesn't want to see her. Thus, Tom avoids his ex-wife, but why? Declining the invitation produces no immediate reinforcer; it only insures that something will not happen. The molecular view has no way to explain this, because it cannot appeal to any immediate reinforcer, although so-called two-factor theory would postulate an implausible and invisible "fear" of the ex-wife that is reduced by the declining. Instead, we can view Tom's declining as part of an extended pattern of avoiding his ex-wife: he not only turns down invitations to events at which she will be present, but he in general avoids places where she might be. He might not always be successful, but his avoidance activities reduce the likelihood that he will have to see her.

This explanation of Tom's behavior jibes with the explanation of free-operant avoidance in the laboratory. Sidman (1966) suggested that rats press a lever that postpones electric shock because pressing the lever reduces the rate of shocks received. Herrnstein (1969) elaborated on this appeal to extended relations and pointed out the inadequacy to the molecular view as adopted by Skinner and some other behavior analysts. Baum (2020) introduced an explanation of avoidance that combines Sidman's insight with the induction of avoidance activity by the occurrences of the noxious event—avoidance is maintained by its failures.

Some behavior analysts, notably Herrnstein and some of his students (e.g., Hineline, Rachlin, and me), moved ahead in the direction that Skinner had pointed out—toward temporally extended phenomena and theories. A major step was the discovery of the matching law (Herrnstein, 1961). Generalizing this discovery leads to a law of behavior: the law of allocation.

The Law of Allocation

As Herrnstein (1961) originally presented it, the matching law stated that the proportion of behavior allocated to an alternative tended to match the proportion of reinforcers obtained by that alternative:

$$\frac{B_1}{B_1 + B_2} = \frac{r_1}{r_1 + r_2}, \tag{4.1}$$

where B_1 and B_2 are rates of behavior allocated to Alternatives 1 and 2, such as pecking at two response keys, and r_1 and r_2 are the rates at which reinforcers, such as bits of food, were obtained. Herrnstein (1970) generalized Eq. (4.1) to any number, N, of alternatives:

$$\frac{B_j}{\sum_{i=1}^{N} B_i} = \frac{r_j}{\sum_{i=1}^{N} r_i}. \tag{4.2}$$

The matching law represented a major step, because it introduced reinforcer rate as a valid independent variable for understanding response rate. Just as Skinner had recognized an extended measure, response rate, as a dependent variable, the matching law introduced an extended measure, reinforcer rate, as an independent variable, and together they indicated that behavior and its controlling relations could be seen as extended in time.

From the recognition that the matching law implies temporally extended variables and relations, only a short step was required to write matching more generally in terms of time (Baum, 1974; Baum & Rachlin, 1969):

$$\frac{T_j}{\sum_{i=1}^{N} T_i} = \frac{V_j}{\sum_{i=1}^{N} V_i}, \tag{4.3}$$

which states that the proportion of time taken up by one activity j matches V_j relative to the total of V_i across all alternatives, and each V_i is a composite measure of reinforcer variables, such as rate, amount, and immediacy, that determine the relative time. V_i may be called the *competitive weight* of activity i. It represents the extent to which inducing events (PIEs) induce activity i (Baum & Aparicio, 2020; Baum & Grace, 2020).

Equation (4.3) may be rewritten in a variety of ways (Baum, 2012b), but it is general enough for present purposes to be called the law of allocation. It has been used to explain impulsive choice (Aparicio et al., 2015) and resurgence—the reappearance of extinguished responding when an alternative activity is extinguished (Shahan & Craig, 2017). Like any scientific law, it embodies and depends upon a number of assumptions or axioms. They might be taken as guidelines for experimenting and theorizing about behavior. These were discussed less formally in an earlier paper (Baum, 2013; see also Baum, 2018).

Axiom 1: Only Whole Organisms Behave

Axiom 1 applies to all organisms: multicellular, humans, dogs, pigeons, fish, cockroaches, or hydras; unicellular, paramecia or amoebae; and archaic, bacteria and viruses. As we will see below, these are all individuals that interact with their surrounding environment.

Axiom 1 excludes inanimate things—stones, houses, automobiles, dead fish, or computers. These all undergo processes: they move, burn, break, decay, or calculate. These processes, however, do not entail interaction with the environment—the two-way street. Not all processes constitute behavior.

In associating behavior only with whole living organisms, Axiom 1 partially defines what we mean by "behavior." In addition to excluding inanimate things, it rules out behavior by parts of an organism. My heart's beating may be part of my physiology, but it is not part of my behavior. In particular, Axiom 1 denies that the brain behaves (Bennett & Hacker, 2003). Bennett and Hacker (2003) explain the logical reason that only whole organisms behave. For example:

> Psychological predicates are predicable only of a whole animal, not of its parts. No conventions have been laid down to determine what is to be meant by the ascription of such predicates to a part of an animal, in particular to its brain. So the application of such predicates to the brain … transgresses the bounds of sense. The resultant assertions are not false, for to say that something is false, we must have some idea of what it would be for it to be true—in this case, we should have to know what it would be for the brain to think, reason, see and hear, etc., and to have found out that as a matter of fact the brain does not do so. But we have no such idea, as these assertions are not false. Rather, the sentences in question lack sense. (p. 78)

What Bennett and Hacker say in this quote about "psychological predicates" applies to behavior in general, not just thinking, reasoning, seeing, and hearing. To speak of the behavior of inanimate things or parts of living things—anything other than whole living organisms—"transgresses the bounds of sense." The brain does not perceive, choose, or sense, any more than the brain can walk or talk; these are activities of whole organisms. People sometimes speak of the brain as if it behaved, but such speech constitutes only metaphorical extension and clashes with logic.

A more important reason for Axiom 1 derives from evolutionary theory. From the perspective of evolutionary theory, behavior only exists because organisms exist. Organisms exist because the genes that make for organisms reproduce more successfully than competing genes that would undo organisms—that is, the genes that produce and reside in organisms have higher fitness than any competitors. The competition continues now, just as long ago. Multicellular organisms continually face challenges by less organized life forms, particularly bacteria and viruses. These threats are countered by evolved mechanisms, such as the immune system, symbiosis with microorganisms in the gut and on the skin, and practices such as treating water before drinking it. The success of the organism-making genes relies on the organism's interaction with the environment around it, because the organism's actions change the environment in ways that are, on average, advantageous to survival and reproduction. Often the environmental changes feed back to affect the organism's further actions. The organism's actions are the organism's behavior. (See Baum, 2013, for further discussion.)

Axiom 2: To Be Alive Is to Behave

Axiom 2 says that so long as an organism is alive, it behaves continually. It immediately implies that behavior takes up all the time available. If one observes an organism for an hour, a day, or a year, one observes an hour's worth, a day's worth, or a year's worth of behavior. If behavior is allocated among various activities, those activities each take up some of the time and together take up all of the time. The key task of behavior analysis is explaining the allocation of time among all the organism's activities.

The connection to evolution further supports the central principle that behavior takes up time, because interaction with the environment can only take place over time. The phrase "momentary interaction" is an oxymoron, because interaction can only be extended. That behavior cannot occur at a moment tells us that the historical concept "momentary response" was logically and theoretically flawed.

Indeed, no activity can be identified at a moment. A snapshot of a person holding an open book tells almost nothing about what activity is occurring; the person might be reading, looking for something in the book, pretending to read, and so on. Only by observing for some time, before and after the moment, can the activity be identified as reading or pretending or something else. Similarly, a snapshot of a rat with its paws on a lever tells almost nothing of what activity is occurring; one has to see what went before and what came after to decide if the rat is pressing the lever at a high rate, at a low rate, pressing at all, exploring the chamber, or something else. (See Baum, 1997, 2013, for further discussion.)

One might assert the converse of Axiom 2 also: to behave is to be alive. Not only bacteria, which have a cell membrane, are considered alive because they reproduce and interact with the environment around them—secreting chemicals, attacking cells, and exchanging genetic material—but also viruses, naked molecules lacking any membrane, are considered alive because they reproduce and interact with the bacteria and cells they encounter. Prions, smaller protein molecules that only replicate, are not considered to be alive. Thus, behavior is inextricably tied up with life and characterizes what are considered "live organisms."

Axiom 3: Every Activity Is Composed of Parts That Are Themselves Activities

Axiom 3 introduces scale into Eq. (4.3). It says that the time taken up by any one activity may be subdivided into the less-extended, smaller-scale activities of which it is composed and that the time taken up by those parts adds up to the time taken up by the more-extended, longer-scale activity of which they are parts. If I play tennis for an hour, during that hour I am serving shots, returning shots, keeping score, exchanging remarks with my opponent, and so on. Together these activities constitute playing tennis, and together they take up the whole hour of my playing tennis. If a pigeon pecks at keys in concurrent schedules, its performance has parts: pecking at the right key, pecking at the left key, and background activities other than pecking. Its pecking might be organized into long visits to the preferred key ("fixing" on the rich key) alternating with brief visits to the non-preferred key ("sampling") plus background activities. Thus, Eq. (4.3) may apply at any time scale, to the parts of playing tennis or to the activities of a day, one of which is playing tennis, and to the allocation of pecking between keys or to the pattern of pecking and switching between keys. It may apply even at time scales of fractional seconds, to the parts of a pigeon's key peck or a rat's lever press (e.g., Smith, 1974). Axiom 3 underpins what I call the multiscale view of behavior. (See Baum, 2018, for further discussion of laws of behavior.)

The Behavior-Environment Feedback System

Some earlier papers suggested that the interaction of behavior with the environment may be compared to a feedback system (Baum, 1973, 1981, 1989, 2016). Figure 4.1 shows a diagram of the feedback system for one activity. (A more detailed presentation may be found in Baum, 1981.) The set point of the system ("competition") accords with the law of allocation; one may think of it as Eq. (4.3). It is compared with the current rate of the activity, B, and the difference or "error" equals ΔB, which is input to an environmental relation. The function g represents a feedback function—a property of the environment. The output of the feedback function, r, is a rate of consequences, PIE rate (e.g., food rate). The rate r is input to an organism-based functional relation. Some evidence suggests that this relation may be a power function, at least for relating food rate and pigeons' rate of key pecking (e.g., Baum, 2015; Baum & Aparicio, 2020; Baum & Grace, 2020):

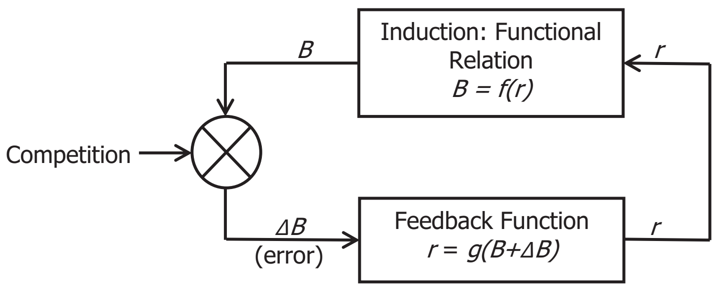

Fig. 4.1 Behavior and environment as a feedback system. The law of allocation ("competition," Eq. 4.3) determines the set point. A feedback relation in the environment translates error (ΔB) into a PIE rate (r). An induction relation through the organism translates r into rate of the operant activity (B), as, for example, in Eq. (4.4)

$$T_j = b_j r_j^{s_j}, \tag{4.4}$$

where T_j is time spent pecking, r_j is rate of food, s_j is sensitivity of T_j to r_j, and b_j is a coefficient. Equation (4.4) states that r_j induces time spent pecking according to the power s_j and in proportion to b_j. In principle, r_j need not be only rate of food; for example, r_j could represent amount of food or immediacy of food (Baum & Rachlin, 1969).

The function *f* in Fig. 4.1 may be thought of as Eq. (4.4), with B equal to T_j. The system stabilizes when ΔB equals zero. That equilibrium is often called "stable performance." Although local variation never completely ceases, allocation may be considered stable when it ceases to exhibit a trend across time.

Multiscale Behavior Analysis and Evolutionary Theory

Axiom 3 above introduces the fundamental property of scale. If driving to work is part of working, then driving to work occurs on a smaller time scale than working. One may say that working takes longer than driving to work. Driving to work is an activity composed of yet shorter activities such as driving on the highway and driving on town roads (see Wallace, 1965 for a detailed discussion of driving to work).

At the longest time scale for an individual organism, only one activity occurs. We may call it "living." Recalling the logic of evolutionary thinking, according to which multicellular organisms only exist because of the success of the genes they carry, we may conclude that living serves one function: reproducing. All other activities, whatever their scale, are ultimately parts of reproducing. In particular, surviving is often a necessary part of reproducing. Exceptions exist—for example, male mantids and spiders that are eaten by the female after copulation, providing a good meal for the female that will benefit the male's offspring. Even human beings sometimes sacrifice their own lives for the sake of their offspring.

Surviving is a necessary part of reproducing the same way that getting out a mixing bowl is necessary to making a cake; the longer-scale activity cannot be completed without it. Though necessary, however, surviving is not always sufficient for reproducing. Other parts, like mating and caring for offspring, more directly related to reproducing, must also occur. Surviving only has to provide opportunities for these other parts of reproducing on average and in the long run. Evolutionary arguments always contain this proviso, either explicitly or implicitly. A beneficial gene may be selected in a population even though some members of the population possessing the gene die without reproducing, because the gene confers advantage to offspring on average and in the long run. Similarly, an operant activity may be selected even though its consequences are sometimes bad if the consequences are better than competing variants on average and in the long run. Camping outdoors may usually be an exhilarating experience but sometimes is ruined by a rainstorm; people still go camping.

The perspective offered by evolutionary theory, that organisms exist to reproduce, may be summarized as, “Organisms are the means by which DNA makes more DNA.” It helps to answer many questions about life in general and human life in particular. For example, why do organisms age and die? Life span is tied to generation time; once a generation of parents has produced offspring, the parents may no longer have a function and, rather than live on and compete with their own offspring, they die—genes are selected that result in this built-in obsolescence. Human beings present a special puzzle: the phenomenon of menopause. In other species, both males and females continue to be fertile as long as they live, but in our species only the males remain fertile. A possible reason lies in the long period of dependence of our offspring. Rather than continuing to produce children that would compete with her other children, a woman may stop reproducing and continue living for the benefit of her grandchildren. Genes making for this pattern would be selected by the beneficial effect on the grandchildren.

Evolutionary theory helps to understand why many human activities exist that otherwise would have no explanation. Even though activities like art, music, and religion might seem to have little connection to reproducing, they can be fitted into the larger context of evolution. A highly social species like ours lived all its evolutionary history in groups, and many shared practices (i.e., operant activities), collectively known as “culture,” belong to the group. Some practices serve the individual person’s reproductive success, and some practices serve the group as a whole. Avoiding poisonous plants serves the individual, but ingroup-outgroup discrimination serves the group. Art, music, and religion may provide ways to enhance one’s status within a group and therefore open opportunities for mating and gaining resources. Practices with less-obvious function often serve the group as a means of maintaining group cohesion—for example, wearing certain tattoos or clothing, speaking a certain language dialect, and attending a certain church. Since group membership is fundamental to human life and survival, most human activities tie less directly to reproducing than to surviving.

Surviving has parts, like any other activity. The parts are not always easy to identify as such. In the past, I suggested three long-scale human activities: maintaining health, gaining resources, and maintaining relationships (Baum, 1995b, 2017). All three promote survival, and this division is useful for discussion, but these parts sometimes overlap. One usually needs to be healthy to gain resources, and sometimes resources make for good health. Earning a living by holding a job requires getting enough sleep, but having income allows one to have the shelter needed to get enough sleep. Relationships may help with gaining resources, but sometimes resources allow formation of new relationships. A friend may lend you money, but having money also may open doors that might otherwise be shut. Despite the overlap, Axiom 2 above tells us that behavior takes up all the time available and cannot take up more time than is available. The overlap, along with Axiom 2, leads to what may be called the “accounting” problem—that is, the problem of deciding when one activity begins and another leaves off in order to measure the time spent in each activity.

The Accounting Problem: Defining and Measuring Activities

In the laboratory, we can arrange conditions so as to prevent overlap between activities. We define activities so that they are readily measured. For example, research with concurrent schedules has produced support for viewing choice as allocation of time among activities. An experiment by Bell and Baum (2017) studied concurrent variable-interval (VI) variable-ratio (VR) schedules of key pecking in pigeons. Although the two types of schedules maintained qualitatively different patterns of pecking, Bell and Baum were able to measure the time spent at each alternative, and the time allocation between them provided the best description of the choice relations as relative reinforcers obtained varied across the alternatives. The accounting problem appears to be solved because no pecks are possible at one key, while pecking is occurring at the other key.

Yet, even in the laboratory ambiguity arises. As Herrnstein (1970) noted, a pigeon in an experimental chamber is not limited only to pecking keys. Every organism brings with it unmeasured activities like grooming, scratching, and exploring. That is why he added a term r_o to the version of Eq. (4.1) that described responding at a single programmed alternative. Subsequent research indicates that such "background" activities separate into those that are induced by the reinforcer (PIE, e.g., food) and those that occur independently of the reinforcer. Analysis by Davison (2004) suggests that several different background activities occur alternatively.

The accounting problem is less challenging in the laboratory than in more naturalistic settings, with humans or other animals, inside or outside the laboratory. When doing research, one must define activities so that they are mutually exclusive. Once the definitions are clear, one may tackle measurement. The best approach is to record behavior and have two or more observers code the videographic recordings (e.g., Simon & Baum, 2017). That approach, however, is labor-intensive. Another approach with humans is self-report; one simply asks a person how much time they spend in various activities, but this method relies on people to be accurate in their estimates.

Defining Activities

Skinner (1938) introduced the definition of operant activity by its function. Evolutionary theory explains why definition of behavior by function is indispensable. Since the function of organisms is to reproduce, behavior exists ultimately as interaction with the environment in the service of that function. Behavior consists of activities that serve functions that ultimately serve reproducing. Thus, when a rat's lever pressing produces food, that activity may serve the function of feeding (along with other parts, like consuming the food); pressing the lever is then part of feeding, and feeding is essential to surviving and reproducing.

In more naturalistic situations, defining activities depends on deciding which functions they serve. Depending on one's research interest, having lunch with a friend may be construed as an activity that maintains a relationship, a variant of socializing, or as an activity that maintains health, a variant of eating. A third possibility, if one wanted to separate socializing from eating, would view having lunch with a friend as multitasking. Research on multitasking indicates that it entails rapid switching back and forth between two activities (resulting in poorer performance on both than either by itself, e.g., Caird et al., 2008).

Proper definition of activities allows one to study practical problems. For example, suppose one wished to study work-life balance in someone's life. Defining "work" and "life" plausibly would be crucial. If the two activities occur in two different locations, definition might be relatively simple. Even then, however, overlap might occur, as when a person gets a work-related phone call at home or a family-related phone call at work. Defining the activities so that they are mutually exclusive might be a bit inaccurate; the more natural the setting, the lower tends to be the accuracy with which it can be studied.

Measuring Activities

For measuring activities, we gain clarity by distinguishing between episodes and constitutive parts. All activities are episodic. Suppose I drive to work every weekday. Each drive to work is an episode of the activity driving to work. All the episodes of driving to work over the course of a month or a year together constitute an aggregate, which we may liken to a population. In evolutionary theory, the aggregate of members of a species makes a population. One may be interested in the population as a whole—its size and geographical distribution—or one may be interested in the variation across the members of the population—their physical characteristics or reproductive success. Similarly, one may be interested in the population of episodes of an activity, their number or total, or one may be interested in variation across episodes, their duration or their constituent parts. If I were just interested in the aggregate, I might want to know how much time I spend driving to work. If I were interested in the variation among episodes, I might note that some of my drives to work include driving through Smithtown (a part), whereas others might avoid Smithtown. At a smaller time scale, I might be interested in the population of my drives through Smithtown—for example, some might adhere to the speed limit, whereas others might exceed the speed limit, attracting the attention of local police.

In laboratory research on behavior, populations and episodes are modeled by measuring bursts, bouts, or visits (e.g., Aparicio & Baum, 2006; Bell & Baum, 2017; Shull et al., 2001). Operant activity, like all behavior, divides into bouts interspersed with pauses that represent time spent in other activities (Davison, 1993; Gilbert, 1958). Those bouts or visits may be thought of as episodes of the operant activity, and their aggregation constitutes a population. One might examine the variation in their duration for clues to initiation and termination of the bouts, or one might examine their function, as in the pattern called "fix and sample" (Baum et al., 1999), in which operant activity fixes on the richer of two choice alternatives and takes the form of brief samples at the leaner of the alternatives.

Laboratory research also occasionally raises variation in constituent parts of patterns of operant activity. An example may be seen in food-induced activities that compete with the operant activity. These activities figure into Eqs. (4.2) and (4.3) and other expressions of the law of allocation. For example, Baum and Davison (2014) factored in induced activities in order to explain apparent deviations from the matching law. Conceiving of behavior as composed of multiple activities provides a plausible and elegant approach to measuring behavior.

The Ontological Status of Activities

Two ontological distinctions are helpful in thinking about activities: (a) between objects and processes and (b) between classes and individuals. They are not entirely independent of one another, but I will take up each in turn.

Objects Versus Processes

In everyday parlance, an object is any distinctive feature of the world that is seemingly stable—a tree, a house, a river, and a star. Their apparent relative stability translates into repeatability, as in sunsets or chemical reactions. Their repeatability arises because they may be named and classified. Atomic particles, for example, may be classified according to their energy levels. To the extent that discrete responses are treated as repeatable and classified according to fixed criteria, discrete responses are treated as objects. If a response is classified as any movement that depresses a lever a certain distance and with a certain force, the response is being treated as an object.

In contrast to the stability of objects, processes are changes through time—movement, deterioration, transformation, metamorphosis, and growth. Some objects undergo notable change and are spoken of that way, as when we speak of a child becoming an adult. When we recognize that behavior is interaction with the environment and that behavior takes time, we recognize that behavior is process. A rat's lever pressing, a child's crying, and a person's reading—these are processes, although their full definition as activities requires incorporation of their functions. The rat's lever pressing might be part of feeding, the child's crying may serve to summon a caretaker, and the person's reading might serve to inform. Thus, activities are processes.

Classes Versus Individuals

A class singles out objects or processes according to a set of defining attributes. Dog is a class of which specific concrete dogs are instances; my dog Fido is an instance. Deterioration is a class of which the wear and tear on my house and the progress of my disease are instances. Skinner's (1938) definition of an operant as a class meant that the movements that met the criteria of the class were instances of the class.

Classes cannot change and cannot do anything. They may have more or fewer instances, but they are fixed by their defining attributes. Change the force required for a lever press, and you change the class. A class cannot do anything, because it is an abstraction; only concrete particulars can do things. Dog cannot come when I call, but my dog Fido can come when I call. An operant, as a class, cannot do anything; only the concrete movements that are instances can get a lever pressed.

In contrast to classes, individuals can change while still retaining their identity. An individual could be either an object or a process. An individual is an integrated whole that functions in a definitive way, is a concrete particular, and has a beginning and end. Whereas classes have instances, individuals have parts. The relation between instance and class contrasts with the relation between part and whole; individuals are instances, but they have no instances. Individuals can be described, but they cannot be defined. Abraham Lincoln was an individual, and my dog Fido, but also the chair on which I am sitting and the Rocky Mountains; they are all concrete particulars, they all function, and they all have integral parts that function together. Although organisms are spoken of as individuals, they are not the only ontological individuals. A baseball team is an individual, insofar as the players function together and win or lose as a whole. As Ghiselin (1997) explains, species are individuals; their members are their parts, and their function is to evolve.

Processes occur in individuals. When an individual changes, the individual goes through a process. Abraham Lincoln grew from a baby into a boy and into a man. When we talk about behavior, however, our language for talking about processes may be misleading. When we say Abraham Lincoln grew, we mean only that the process of growth occurred in him. When we say that Rat 5 pressed the lever, we also should mean only that lever pressing occurred in Rat 5—yet an additional element creeps in: agency. When we say that Abraham Lincoln delivered the Gettysburg Address, from the perspective of a science of behavior, we mean that the speech delivering occurred in Abraham Lincoln. Confusion might exist if one thought that Lincoln's growing was a different sort of process from Lincoln's speech delivering because Lincoln did not *do* the former, whereas he *did* the latter—that is, the speech delivering involved agency. (See Baum, 1995c, for further discussion of agency.) Recalling that behavior is interaction with the environment, we can think of the organism as the medium for the behavior—thus, if we are trying to be precise and avoid confusion, we say the behavior occurs in the organism, even though the usual construction of saying, "The organism did such-and-such," is much easier.

As behavior, activities are processes that occur in organisms. Not all processes that occur in organisms are activities, only the ones that affect the environment. The heart's beating is not behavior, because the heart is only a part of the organism (see Axiom 1). Putting on a coat to stay warm counts as an activity, because staying warm is an interaction with the environment. Tom's avoiding his ex-wife is an activity, because it functions to keep him from seeing her.

An earlier paper (Baum, 2002) suggested that activities themselves may be seen as individuals. Like any other individual, an activity is an integrated whole constituted of parts that work together to serve a function. As cells constitute an organ, and organs constitute an organism, an activity like playing tennis is constituted of activities: serving, returning, keeping score, and so on. The activity lever pressing is both a process that occurs in Rat 5 and an individual constituted of parts that are also activities—pawing the lever, biting the lever, licking the lever, and so on. The activity baking a cake is both a process that occurs in the baker and an individual constituted of parts that are also activities—getting out a bowl, adding ingredients, mixing, and so on. Thus, an activity is both a process and an individual.

Conclusion

Behavior takes time. This fundamental principle for understanding behavior is supported both by logic and by theory. Its implications are profound. It puts aside the traditional molecular view based on discrete events and contiguity. It tells us that behavior must be understood as dynamic and extended in time, an insight that concurs with the view of behavior implied by evolutionary theory, that behavior is an organism's interaction with its environment.

Molar behaviorism and multiscale behavior analysis treat behavior as consisting of activities that are extended in time. They treat behavior at any time scale, whether milliseconds or years. An episode of an activity like a pigeon's peck, however brief, has temporal extent. Care should be taken to avoid confusing brief episodes of an activity with discrete responses; they are qualitatively (ontologically) different concepts.

Activities are processes and individuals. They function as integrated wholes and evolve through time as their parts (less extended activities) change through time and take up more or less time. Like species, activities have a beginning and may go extinct as other activities replace them—we change jobs, move to new neighborhoods, have children, and change spouses. This multiscale view applies plausibly both to behavior in the laboratory and to behavior in the everyday world.

References

Aparicio, C. F., & Baum, W. M. (2006). Fix and sample with rats in the dynamics of choice. *Journal of the Experimental Analysis of Behavior, 86*, 43–63.

Aparicio, C. F., & Baum, W. M. (2009). Dynamics of choice: Relative rate and amount affect local preference at three different time scales. *Journal of the Experimental Analysis of Behavior, 91*, 293–317.

Aparicio, C. F., Elcoro, M., & Alonso-Alvarez, B. (2015). A long-term study of the impulsive choices of Lewis and Fischer 344 rats. *Learning and Behavior, 43*, 251–271. https://doi.org/10.3758/s13420-015-0177-y

Baum, W. M. (1973). The correlation-based law of effect. *Journal of the Experimental Analysis of Behavior, 20*, 137–153.

Baum, W. M. (1974). On two types of deviation from the matching law: bias and undermatching. *Journal of the Experimental Analysis of Behavior, 22*, 231–242. https://doi.org/10.1901/jeab.1974.22-231

Baum, W. M. (1976). Time-based and count-based measurement of preference. *Journal of the Experimental Analysis of Behavior, 26*, 27–35.

Baum, W. M. (1981). Optimization and the matching law as accounts of instrumental behavior. *Journal of the Experimental Analysis of Behavior, 36*, 387–403.

Baum, W. M. (1989). Quantitative prediction and molar description of the environment. *The Behavior Analyst, 12*, 167–176.

Baum, W. M. (1992). In search of the feedback function for variable-interval schedules. *Journal of the Experimental Analysis of Behavior, 57*, 365–375.

Baum, W. M. (1993). Performances on ratio and interval schedules of reinforcement: Data and theory. *Journal of the Experimental Analysis of Behavior, 59*, 245–264.

Baum, W. M. (1995a). Introduction to molar behavior analysis. *Mexican Journal of Behavior Analysis, 21*, 7–25.

Baum, W. M. (1995b). Rules, culture, and fitness. *The Behavior Analyst, 18*, 1–21.

Baum, W. M. (1995c). Radical behaviorism and the concept of agency. *Behavior, 3*, 93–106.

Baum, W. M. (1997). The trouble with time. In L. J. Hayes & P. M. Ghezzi (Eds.), *Investigations in behavioral epistemology* (pp. 47–59). Context Press.

Baum, W. M. (2002). From molecular to molar: A paradigm shift in behavior analysis. *Journal of the Experimental Analysis of Behavior, 78*, 95–116.

Baum, W. M. (2011). Behaviorism, private events, and the molar view of behavior. *The Behavior Analyst, 34*, 185–200.

Baum, W. M. (2012a). Rethinking reinforcement: Allocation, induction, and contingency. *Journal of the Experimental Analysis of Behavior, 97*, 101–124.

Baum, W. M. (2012b). Mathematics and theory in behavior analysis: Remarks on Catania (1981), "The flight from experimental analysis". *European Journal of Behavior Analysis, 13*, 177–179.

Baum, W. M. (2013). What counts as behavior: The molar multiscale view. *The Behavior Analyst, 36*, 283–293.

Baum, W. M. (2015). The role of induction in operant schedule performance. *Behavioural Processes, 114*, 26–33.

Baum, W. M. (2016). Driven by consequences: The multiscale molar view of choice. *Decision and Managerial Economics, 37*, 239–248.

Baum, W. M. (2017). *Understanding behaviorism: Behavior, culture, and evolution* (3rd ed.). Wiley Blackwell Publishing.

Baum, W. M. (2018). Three laws of behavior: Allocation, induction, and covariance. *Behavior Analysis: Research and Practice, 18*, 239–251. https://doi.org/10.1037/bar0000104

Baum, W. M. (2020). Avoidance, induction, and the illusion of reinforcement. *Journal of the Experimental Analysis of Behavior, 114*, 116–141.

Baum, W. M., & Aparicio, C. F. (2020). Response-Reinforcer contiguity versus response-rate-Reinforcer-rate covariance in rats' lever pressing: Support for a multiscale view. *Journal of the Experimental Analysis of Behavior, 113*, 530–548. https://doi.org/10.1002/jeab.594

Baum, W. M., & Davison, M. (2004). Choice in a variable environment: Visit patterns in the dynamics of choice. *Journal of the Experimental Analysis of Behavior, 81*, 85–127.

Baum, W. M., & Davison, M. (2014). Background activities, induction, and behavioral allocation in operant performance. *Journal of the Experimental Analysis of Behavior, 102*, 213–230.

Baum, W. M., & Grace, R. C. (2020). Matching theory and induction explain operant performance. *Journal of the Experimental Analysis of Behavior, 113*, 390–418. https://doi.org/10.1002/jeab.583

Baum, W. M., & Rachlin, H. C. (1969). Choice as time allocation. *Journal of the Experimental Analysis of Behavior, 12*, 861–874.
Baum, W. M., Schwendiman, J. W., & Bell, K. E. (1999). Choice, contingency discrimination, and foraging theory. *Journal of the Experimental Analysis of Behavior, 71*, 355–373.
Bell, M. C., & Baum, W. M. (2017). Concurrent variable-interval variable-ratio schedules in a dynamic choice environment. *Journal of the Experimental Analysis of Behavior, 108*, 367–397.
Bennett, M. R., & Hacker, P. M. S. (2003). *Philosophical foundations of neuroscience*. Blackwell Publishing.
Caird, J. K., Willness, C. R., Steel, P., & Scialfa, C. (2008). A meta-analysis of the effects of cell phones on driver performance. *Accident Analysis and Prevention, 40*, 1282–1293.
Davison, M. (1993). On the dynamics of behavior allocation between simultaneously and successively available reinforcer sources. *Behavioural Processes, 29*, 49–64.
Davison, M. (2004). Interresponse times and the structure of choice. *Behavioural Processes, 66*, 173–187.
Ghiselin, M. T. (1997). *Metaphysics and the origin of species*. State University of New York Press.
Gilbert, T. F. (1958). Fundamental dimensional properties of the operant. *Psychological Review, 65*, 272–285.
Herrnstein, R. J. (1961). Relative and absolute strength of response as a function of frequency of reinforcement. *Journal of the Experimental Analysis of Behavior, 4*, 267–272.
Herrnstein, R. J. (1969). Method and theory in the study of avoidance. *Psychological Review, 76*, 49–69.
Herrnstein, R. J. (1970). On the law of effect. *Journal of the Experimental Analysis of Behavior, 13*, 243–266.
Morse, W. H. (1966). Intermittent reinforcement. In W. K. Honig (Ed.), *Operant behavior: Areas of research and application* (pp. 52–108). Appleton-Century-Crofts.
Pavlov, I. P. (1960/1927). *Conditioned reflexes: An investigation of the physiological activity of the cerebral cortex*. Dover.
Rachlin, H., & Baum, W. M. (1969). Response rate as a function of amount of reinforcement for a signalled concurrent response. *Journal of the Experimental Analysis of Behavior, 12*, 11–16.
Rachlin, H., & Baum, W. M. (1972). Effects of alternative reinforcement: Does the source matter? *Journal of the Experimental Analysis of Behavior, 18*, 231–241.
Ryle, G. (1949). *The concept of mind*. University of Chicago Press.
Segal, E. F. (1972). Induction and the provenance of operants. In R. M. Gilbert & J. R. Millenson (Eds.), *Reinforcement: Behavioral analyses* (pp. 1–34). Academic.
Shahan, T. A., & Craig, A. R. (2017). Resurgence as choice. *Behavioural Processes, 141*, 100–127.
Shull, R. L., Gaynor, S. T., & Grimes, J. A. (2001). Response rate viewed as engagement bouts: Effects of relative reinforcement and schedule type. *Journal of the Experimental Analysis of Behavior, 75*, 247–274.
Sidman, M. (1966). Avoidance behavior. In W. K. Honig (Ed.), *Operant behavior: Areas of research and application* (pp. 448–498). Appleton-Century-Crofts.
Simon, C., & Baum, W. M. (2017). Allocation of speech in conversation. *Journal of the Experimental Analysis of Behavior, 107*, 258–278.
Skinner, B. F. (1938). *Behavior of organisms*. Appleton-Century-Crofts.
Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review, 52*, 270–277.
Skinner, B. F. (1948). “Superstition” in the pigeon. *Journal of Experimental Psychology, 38*, 168–172.
Smith, R. F. (1974). Topography of the food-reinforced key peck and the source of 30-millisecond interresponse times. *Journal of the Experimental Analysis of Behavior, 21*, 541–551.
Thorndike, E. L. (2012/1911). *Animal intelligence: Experimental studies*. Forgotten Books.
Wallace, A. F. C. (1965). Driving to work. In M. E. Spiro (Ed.), *Context and meaning in cultural anthropology* (pp. 277–292). Free Press.
Watson, J. B. (1930). *Behaviorism*. W. W. Norton.

Chapter 5
The Molar View of Behavior: A Paradigm Shift in Behavior Analysis?

Carolina Laurenti

The molar view of behavior is presented as a new scientific paradigm in behavior analysis. The term paradigm does not seem to have been used in a fortuitous way. Resorting to Thomas Kuhn, Baum (2002) elucidated the magnitude of his proposal: the molar behavior-analytic view involves ontological and epistemological claims that signal incommensurable interpretations with another scientific paradigm, the molecular view of behavior. As described by the proponent, "its implications are profound" (Baum, 2021). I examine the "level of profundity" of what I consider to be some of the implications of the molar view of behavior. The molar model enables an integration between philosophy and science in behavior analysis, retrieving a genuine sense of behaviorism (high profundity). Since Skinner's view of behavior was characterized as a "molecular" one, the contrast between the molar and the molecular approaches is not so sharp when changes in the Skinnerian explanatory model are considered (medium profundity). Finally, I argue that the implications of the molar view do not seem to be that profound when behavior is understood within the framework of a gene-centered evolutionary theory.

The Molar View and Its Integrative Potential in Behavior Analysis

As a paradigm, the molar view changes the ontological conception of behavior (Baum, 2002, 2021). In the molar view, behavior is an aggregate of activities, which are processes and individuals. This means that behavior is an interaction with the

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environment that extends over time (i.e., process), whose parts (other activities) operate together in an integrated and concrete whole, serving a function (i.e., individual). As a process, behavior changes, but it does so without losing its identity (Baum, 2021). The preservation of that "identity" is understood by looking at the behavior as an integrated entity (a whole) and not at its constituent parts. It is that whole (the individual) that remains over time. Therefore, it is the operant as a process that is mutable. It is the operant as an individual (concrete) that changes the environment, preserving its "historical continuity" (Baum, 2002, p. 108).

In the molecular view, behavior is defined by the objects' ontological characteristics of stability and repeatability and by the classes' unchangeable and innocuous feature. This means that behavior consists of discrete responses susceptible of repetition that are fixed in a class when they meet the class defining attributes (Baum, 2021). If those attributes change, a new class can be identified and not a change in the same class; "the only change associated with a class is in the number of its instances" (Baum, 2002, p. 107). As an object and class, the operant is neither changeable nor changing the environment (Baum, 2021).

The molar view of behavior also produces changes in the epistemological level. In the molecular view, the *analysis* describes a relation between class and instance, which occurs in a static and atomistic record (would behavior be a sum of elements?). In the molar view, the *analysis* of behavior follows a part-whole logic that is dynamic and organic (would behavior be a *Gestalt*?). The law of allocation sums up the ontological characteristics of behavior and demarcates the research field of the behavior analyst (choice or time allocation). If behavior is an aggregate of activities (which are processes and individuals), "the key task of behavior analysis is explaining the allocation of time among all the organism's activities" (Baum, 2021). At this point, the multiscale perspective becomes pivotal in a molar view. Since it is an activity extended over time, behavior cannot be described as an activity that occurs at a moment. However, behavior can be analyzed in any time scale, going from milliseconds to years. What scale is going to be used will depend on the context of the study (inside or outside the laboratory) and on the different investigative resources for measuring activities (recording of behavior through videographic recordings, self-report, etc.), once they are clearly identified (Baum, 2021).

Although concepts only make sense within a specific paradigm, hence their incommensurable character, Baum (2021) does not only reinterpret concepts that are related to the molecular paradigm (e.g., reinforcement, punishment, stimulus control, strength of response), but he also highlights the advantages of the molar behavior-analytic view. In the experimental domain, for example, the molar view is able to explain behavioral processes that are not easily elucidated in a molecular view, such as the differences in response rates between ratio schedules (high response rates) and interval schedules (moderate response rates), as well as negative reinforcement (avoidance, in particular) (Baum, 2021). Furthermore, the law of allocation makes it possible to explain impulsive choice responses and resurgence (see also Baum, 2002, pp. 100–106).

In contrast to the molecular view, Baum (2002) argues that the molar view of behavior has a higher external validity, in the sense that it is closer to the way people talk about their lives. The image here is more of a set of rhizomes, with each activity ramifying into others, than a "time line of discrete events, one following another" (p. 111). The time spent in each one of the activities depends on the reinforcement rate that is correlated to them, among other variables (see Baum, 2002, p. 104).

A molar conception of behavior seems to be better suited to dialogue with selection as a causal mode, which is, by definition, historical and, thus, grounded on changes over time (Baum, 2021). As a science that deals with changes, evolutionary biology explains changes on species; behavior analysis explains changes on behavior. Just like species, behavior (as an individual and not as a class) changes without losing its identity, and the processes of selection are what explain those changes: "in the molar view, reinforcement is a process of selection, resembling natural selection" (Baum, 2002, p. 106).

Although Baum (2021) considers behavior analysis to be a branch of biology and not properly a psychology, molar behaviorism allows to understand psychological phenomena (e.g., thoughts and feelings) as observable activities that extend over time and not as private events. Because of its emphasis on immediate and nonhistorical causes, the molecular paradigm can resort to "interpretation" when no immediate observable cause is identified (e.g., a private stimulus known as "pain") (Baum, 2021).

The molar paradigm dismisses the notion of an agent to elucidate the active character of behavior. Behavior is not done by an agent (an inner "self" of mental or cerebral nature); behavior is "agentic" in the sense that it is the concrete (individual) activity that changes the world. The same reasoning can be applied to the notion of choice: it is not necessary to hypostatize a "self" behind choices, because behaving is choosing (allocating time among a set of activities) (Baum, 2021), and choosing is behaving (interacting with the environment).

Molar behaviorism actualizes the original notion of behaviorism: a philosophy that considers behavior to be a subject matter in its own right. Since it understands behavior as activities that extend over time, molar behaviorism "expands" the behavioral field, so that the explanation resorts to historical processes of selection, instead of inferences of immediate internal causes (e.g., private events). Likewise, this "enlargement" of the behavioral field allows complex psychological phenomena to be explained scientifically and historically *within behavior*, without appealing to another level of explanation (e.g., mental or cerebral).

A merit that deserves to be highlighted in Baum's proposal is the possibility of articulating theoretical (ontological and epistemological) and empirical aspects in a coherent whole. More specifically, the ontological notion of behavior (historical entity) is compatible with types of analysis (multiscale), behavioral laws (the law of allocation), and evolutionary theory. Thus, there is a possibility of integration between philosophy of science (molar behaviorism) and science (multiscale behavior analysis) in an evolutionary and selectionist matrix.

The Molar Behavior-Analytic View: A Post-Skinnerian Proposal?

Baum (2002, 2021) quotes B. F. Skinner as a representative of the molecular view of behavior. If "the view that behavior consists of discrete responses that are strengthened by closely following (contiguous) reinforcers may be identified as the molecular view of behavior" (Baum, 2021), and if Skinner "never went beyond the 'operant' as a class of discrete responses or the theory that an immediately following reinforcer 'strengthens' an operant response" (Baum, 2021), then the Skinnerian view of behavior coincides with the molecular paradigm.

To support this argument, Baum (2002, 2021) mentions Skinner's texts, especially from the 1930s (e.g., *The generic nature of the concepts of stimulus and response*, *The behavior of organisms*) but also from the 1940s (*"Superstition" in the pigeon*) and 1950s (e.g., *Are theories of learning necessary?*, *The experimental analysis of behavior*), which provided the theoretical and empirical basis for the formulation of concepts (e.g., response class, response rate) criticized from a molar behavior-analytic perspective (see Baum, 2002, p. 96). In addition, Baum (2002) suggests that Skinner, when compared to other authors, seems to have belatedly acknowledged (in 1981) the parallel between behavioral processes and natural selection: "Possibly Ashby (1954) was the first to recognize the parallel between reinforcement and natural selection. Campbell (1956) spelled out the idea that reinforcement is a type of selection, and R. M. Gilbert (1970) and Staddon and Simmelhag (1971) elaborated it further. Skinner (1981) himself proposed it eventually" (Baum, 2002, p. 98).

After that supposedly late acknowledgment of the parallels between natural selection and reinforcement, Skinner seems to have supported two different conceptions of the process of reinforcement, a molecular one and a selectionist one: "Instead of thinking of reinforcement as a sort of 'moment of truth', defined by contiguity with a momentary response, we may think of reinforcement as a cumulative effect, as selection through time, shaping patterns of behavior (activities) in lineages" (Baum, 2002, p. 113).

Selectionism (selection as explanatory model) is a distinctive aspect of the molar view of behavior. At some extent, both the ontological characteristics (behavior is processual and individual) and the epistemological ones (allocation) seem to be elucidated by selection as a "causal mode" (changes over time without identity lost, etc.), "a completely different and fundamentally historical type of causality" (Baum, 2002, p. 106).

Despite not being a consensual interpretation, studies indicate changes in Skinner's work ranging from mechanism to selectionism (Cruz & Cillo, 2008; Micheletto 1997; Moxley, 1999). For some interpreters, selectionism seems to have acquired sharper contours in the 1950s and 1960s (Carneiro Leão & Carvalho Neto, 2018), while for others, like Palmer and Donahoe (1992), selectionism has been present in Skinner's work since 1930.

Seeing the changes in the Skinnerian explanatory model of behavior, some of them oriented toward selectionism, what would be the scope of the molecular interpretation of Skinner's view of behavior? Would this interpretation be valid for almost the entire extent of Skinner's work (at least until 1981), as suggested by Baum? Or would it be valid only for Skinner's formulations of the 1930s and 1940s, a period in which selectionism was not yet in evidence, according to some interpreters (Carneiro Leão & Carvalho Neto, 2018; Moxley, 1999)?

It is also important to point out that conspicuous parallels between operant reinforcement and natural selection had already been made by Skinner in texts prior to the one published in 1981 (e.g., Skinner, 1953/2005, pp. 90, 222, 430; Skinner, 1968/2003, pp. 174–176; Skinner, 1971, pp. 23, 30; Skinner, 1972/1999, pp. 359–360; Skinner, 1974, pp. 114, 224). If the roots of the molar paradigm "may be traced back to the 1960s, but it became clearly visible in the 1970s" (Baum, 2002, p. 95), and if the parallels between reinforcement and natural selection – one of the distinctive characteristics of this paradigm – can already be found in Skinner in the 1950s, to what extent would the molar proposal of behavior be a "post-Skinnerian" one, in the sense of signaling a rupture with the molecular paradigm? An even more curious fact is that the classic paper quoted by Palmer and Donahoe (1992) to argue that selectionism was already present in Skinner in the 1930s, *The generic nature of the concepts of stimulus and response*, is the same that Baum (2002) used to identify Skinner as a representative of the molecular paradigm of behavior (p. 96).

If selectionism is a hallmark of the molar paradigm, and if Skinner's explanatory model can be considered selectionist (Chiesa, 1992), either Skinner shares a molar view of behavior (the question would merely be about the exact moment he started doing so) or selectionism does not seem to be a sufficient condition, despite it being a necessary one, to characterize the molar paradigm. If selectionism is not a sufficient condition to define the molar view of behavior, which would be its distinctive aspects, besides selectionism? If it is possible to conciliate selectionism with notions of the molecular paradigm (such as the notion of operant as a response class), what would be the fracture lines with the molecular paradigm?

I think that elucidating these issues could help to ascertain the relationships between molar view and selectionism (and Skinner's position in this discussion), as well as the shift paradigm brought about by the molar approach in behavior analysis, as claimed by Baum.

The Molar View in Evolutionary Theory: Revolving Around the Genes

A molar view of behavior had already been proposed by Edward C. Tolman (1932/1967). For this behaviorist, a molar definition of behavior emphasizes the emergent properties of behavior in relation to physiology. In spite of its dependence on the physiology of organisms, behavior cannot be explained in terms of physiological properties, for it requires its own concepts and principles. In Tolman (1932/1967)'s words: "behavior, as such, is more than and different from the sum of its physiological parts. Behavior, as such, is an 'emergent' phenomenon that has descriptive and defining properties of its own. And we shall designate this latter as the *molar* definition of behavior" (p. 7). Based on Tolman's definition, Skinner (1944/1989) criticized Clark L. Hull for having abandoned a molar perspective and having resorted, instead, to neurological dimensions to explain behavior: "The exigencies of his method have led him to abandon the productive (and at least equally valid) formulation of behavior at the molar level and to align himself with the semi-neurologists" (p. 288).

Despite not using the word "emergent," the "molar" view presented by Baum (2021) seems, in some sense, to be close to Tolman's, for molar behaviorism marks off behavior as primordial. In fact, it is the conception that behavior is a "subject matter in its own right" that stands at the very base of Baum's argument about behavior analysis not being a part of psychology: "For psychology, behavior is a superficial phenomenon that must be understood by inferences to a 'deeper' level: the mind or the brain." Moreover, behavior is an activity of the organism as a whole, like it was described in axiom 1, and not of parts of the organisms: "My heart's beating may be part of my physiology, but it is not part of my behavior" (Baum, 2021).

Behavior analysis is, then, part of biology, for "biologists … have no trouble thinking about behavior as real and primary. Biologists who I talk to readily accept the idea that behavior is an organism's interaction with the environment." Thus, Baum (2021) puts behavior analysis in touch with evolutionary biology and discusses behavior from an evolutionary perspective. The notion of evolution used in the interface with biology seems to be the gene-centered version of neo-Darwinism: "The perspective offered by evolutionary theory, that organisms exist to reproduce, may be summarized as, 'Organisms are the means by which DNA makes more DNA'". From this evolutionary viewpoint, "… the behavior only exists because organisms exist. Organisms exist because the genes that make for organisms reproduce more successfully than competing genes that would undo organisms – that is, the genes that produce and reside in organisms have higher fitness than any competitors" (Baum, 2021).

It is true that Baum (2021) does not disregard the role of behavior in evolution, as he says that "the success of the organism-making genes relies on the organism's interaction with the environment around it." However, behavior loses prominence in a gene-centered version of Darwinian theory, which Baum's molar view seems to rest upon. Without subscribing to the thesis of direct information transference from phenotype to genotype, there are other evolutionary approaches in which behavior plays a more relevant role in evolution (e.g., "new synthesis" (Jablonka & Lamb, 2005, 2008) or an "extended synthesis" (Pigliucci, 2009; Pigliucci & Müller, 2010)). Within the framework of these evolutionary theories, behavior can interfere in life conditions, and it can consequently affect the way natural selection operates, as described, for example, by the niche construction theory (see Odling-Smee, 2010; Shavit & Griesemer, 2011). Behavior could also have an evolutionary impact when it becomes a cultural practice or tradition, being spread along generations, affecting populations, and creating specificities that could intervene in the process of the evolution of the species (Galef Jr. & Laland, 2005; Jablonka & Lamb, 2005). Human culture, for example, due to its verbal and symbolic character, can create conditions for survival and reproduction that would possibly exist just in the context of these practices.

The issue raised by those other evolutionary perspectives is that the concept of heredity should be broadened ("there is more to heredity than genes"), covering other inheritance systems besides genetics, such as the epigenetic, behavioral, and symbolic ones (Jablonka & Lamb, 2005, p. 1). Therefore, genes would not be the center which organism and behavior revolve around. Ultimately, there does not seem to be a "center" in those theories but, instead of that, different heredity systems that are interconnect and mutually influenced by each other. It is not Darwin's theory of evolution through natural selection that is being challenged but "the prevalent gene-based unidimensional version of it. There are four dimensions to heredity, and we should not ignore three of them. All four have to be considered if we are to attain a more complete understanding of evolution" (p. 4).

If behavior analysis in not a part of psychology, for psychology deals with behavior as if it were a symptom of something "deeper" (mind or brain) (Baum, 2021), evolutionary biology, at least in Baum's definition of evolution, seems to also see behavior as a "symptom" of something "deeper," the genes. This aspect has important implications when it comes to explaining culture from an evolutionary viewpoint, as well as explaining the role of culture in evolution. Baum (2021) argues that "evolutionary theory helps to understand why many human activities exist that otherwise would have no explanation. Even though activities like art, music, and religion might seem to have little connection to reproducing, they can be fitted into the larger context of evolution." Nonetheless, in certain "cultures," there are contingencies of social reinforcement maintained by groups that are the context for choices (time allocation) that go against the individual's and the species' survival (e.g., consumerist and predatory cultural practices, certain food practices). In sum, these cultures "created" reinforcers that seem to go against survival.

On the other hand, there are also exploitative and oppressive "cultures" (e.g., neoliberal, capitalist, racist, sexist, and classist cultures) that benefit some groups to the detriment of others and that have "survived" over time. When it comes to these cultures, some privileged groups (and the individuals that belong to them) maintain health, gain resources, and maintain relationships at the expense of the exploitation of other groups.

If, as argued by Baum (2021), the molar view can also have implications on the practical life, how would it be possible to deal with cultures that not only foster (i.e., reinforce) choices that endanger the survival of the species but that also "survive" to the expense of high social inequality indexes? Considering that "the advantages to the molar view lie in its ability to integrate experimental results, in its promotion of quantitative theory, and in its applicability to everyday life" (Baum, 2002, p. 114) and that ethical and political problems are part of daily life, how can we think about these sociocultural issues from a molar view of behavior aligned with a gene-centered evolutionary theory? This is yet another set of issues that would need to be addressed in a more systematic way by the molar proposal.

Conclusion

Baum (2002, 2021) claims that the molar view of behavior is responsible for a paradigm shift in behavior analysis. Paradigm shifts are, in fact, profound. The molar approach has a (profound) potential for integrating philosophy (behaviorism) and empirical data (behavioral science) in an evolutionary framework.

The differences between molar view and molecular view lose profundity when selectionist interpretations of Skinner's view of behavior are brought to the fore. Would selectionism be a necessary condition (though not sufficient) to define the molar view? If so, what determines incommensurability with the molecular paradigm of which Skinner would be representative?

At a less profound level, the molar view, from an evolutionary viewpoint, seems to assign a secondary role to behavior, in relation to other evolutionary approaches in which behavior to have a more relevant role in evolution. When the molar view addresses more complex levels, such as the cultural one, its "profound implications" still need to be demonstrated.

If molar behaviorism and multiscale behavior analysis integrate with a gene-centered evolutionary theory, behavior analysis, understood as a part of biology, remains on the surface.

References

Baum, W. M. (2002). From molecular to molar: A paradigm shift in behavior analysis. *Journal of the Experimental Analysis of Behavior, 78*, 95–116. https://doi.org/10.1901/jeab.2002.78-95

Baum, W. M. (2021). Introduction to molar behaviorism and multiscale behavior analysis. In D. Zilio & K. Carrara (Eds.), *Contemporary behaviorisms in debate*. Springer/Paradigma.

Carneiro Leão, M. F., & Carvalho Neto, M. B. de (2018). Successive approximations to selectionism: Skinner's framework for behavior in the 1930s and 1940s. *Revista Mexicana de Análisis de la Conducta/Mexican Journal of Behavior Analysis, 1*(44), 1–24. https://doi.org/10.5514/rmac.v44.i1.65349

Chiesa, M. (1992). Radical behaviorism and scientific frameworks: From mechanistic to relational accounts. *American Psychologist, 47*(11), 1287–1299. https://doi.org/10.1037/0003-066X.47.11.1287

Cruz, R. N., & Cillo, E. N. P. (2008). Do mecanicismo ao selecionismo: Uma breve contextualização da transição do behaviorismo radical [From mechanicism to selectionism: A brief contextualization of the radical behaviorism transition]. *Psicologia: Teoria e Pesquisa, 24*(3), 375–385. https://doi.org/10.1590/S0102-37722008000300015

Galef, B. G., Jr., & Laland, K. N. (2005). Social learning in animals: Empirical studies and theoretical models. *Bioscience, 55*(6), 489–499. https://doi.org/10.1641/0006-3568(2005)055[0489:SLIAES]2.0.CO;2

Jablonka, E., & Lamb, M. (2005). *Evolution in four dimensions: Genetic, epigenetic, behavioral, and symbolic variation in the history of life*. The MIT Press.

Jablonka, E., & Lamb, M. (2008). Soft inheritance: Challenging the modern synthesis. *Genetics and Molecular Biology, 31*(2), 389–395. https://doi.org/10.1590/S1415-47572008000300001

Micheletto, N. (1997). Bases filosóficas do behaviorismo radical [Philosophical roots of radical behaviorism]. In R. A. Banaco (Ed.), *Sobre Comportamento e cognição* (Vol. 1, pp. 29–44). ARBytes.

Moxley, R. A. (1999). The two Skinners, modern and postmodern. *Behavior and Philosophy, 27*(2), 97–125.
Odling-Smee, J. (2010). Niche inheritance. In M. Pigliucci & G. B. Müller (Eds.), *Evolution: The extended synthesis* (pp. 175–207). The MIT Press.
Palmer, D. C., & Donahoe, J. W. (1992). Essentialism and selectionism in cognitive science and behavior analysis. *American Psychologist, 47*, 1344–1358. https://doi.org/10.1037//0003-066x.47.11.1344
Pigliucci, M. (2009). An extended synthesis for evolutionary biology. *Annals of the New York Academy of Sciences, The Year in Evolutionary Biology, 1168*(1), 218–228. https://doi.org/10.1111/j.1749-6632.2009.04578.x
Pigliucci, M., & Müller, G. B. (2010). *Evolution: The extended synthesis*. The MIT Press.
Shavit, A., & Griesemer, J. (2011). Mind the gaps: Why are niche construction models so rarely used? In S. B. Gissis & E. Jablonka (Eds.), *Transformations of lamarckism: From subtle fluids to molecular biology* (pp. 307–317). The MIT Press.
Skinner, B. F. (1971). *Beyond freedom and dignity*. Penguin Books.
Skinner, B. F. (1974). *About behaviorism*. Alfred A. Knopf.
Skinner, B. F. (1989). Review of Hull's principles of behavior. *Journal of the Experimental Analysis of Behavior, 51*(2), 287–290. https://doi.org/10.1901/jeab.1989.51-287. (Original text published in 1944).
Skinner, B. F. (1999). A lecture on "having" a poem. In J. S. Vargas (Ed.), *Cumulative record: Definitive edition* (pp. 353–361). The B. F. Skinner Foundation. (Original work published 1972).
Skinner, B. F. (2003). *The technology of teaching*. The B. F. Skinner Foundation. (Original work published 1968).
Skinner, B. F. (2005). *Science and human behavior*. The B. F. Skinner Foundation. (Original work published 1953).
Tolman, E. C. (1967). *Purposive behavior in animals and men*. Appleton-Century-Crofts. (Original work published 1932).

Chapter 6
Genes, Selection, and Behavior: Response to Laurenti's Commentary

William M. Baum

No one knows how the eukaryote cell came to exist, but a possible scenario may be offered. Suppose a phagocytic (ameba-like) prokaryote ingested another prokaryote but failed to digest it entirely because of a fault in the genetic material that would normally result in effective digestive enzymes. That fault (mutation) might be advantageous if it resulted in a mutualism between the two prokaryotes—ingesting and ingested. For example, if the ingested prokaryote was aerobic or photosynthetic, it would supply energy to the ingesting prokaryote, which in its turn would at least provide protection to them both. Together they might better escape predation and better utilize resources in the environment. Such a beneficial mutualism would result in a proliferation of such complex cells in competition with simpler ones.

Multicellular organisms may have resulted from a similar sort of mutualism between eukaryote cells. If two or more cells stick together as a result of glue excreted by one of them, and the whole is, for example, less subject to predation, then genes that facilitate the glue excretion would be favored by existing in such an organism and would be selected in competition with single cells and other organisms.

This last scenario may be thought of as an example of the Baldwin effect, which points to the possibility that a gene that normally has no effect on phenotype—a cryptic gene—may become advantaged in the face of a change in the environment. The gene that makes for glue excretion might do nothing until the cell happens to collide with another cell. Another example would occur among multicellular organisms if the environment changed from wet to dry. Genes that were ineffective in the wet environment might now become helpful for survival in the dry environment and be selected in the dry environment. When a drought hit the Galapagos Islands, finches with beaks that enabled them to exploit hard seeds were advantaged relative to competitors, and seed-cracking beaks were selected (Lamichhaney et al., 2016). This kind of selection occurs in the face of rare and unsystematic variation in the

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environment. When the environment varies in systematic ways, such effects permit phenotypic plasticity—variation in phenotype, for example, according to the season.

All of these scenarios involving environmental effects on genes and selection nevertheless place genetic variation and genetic inheritance at the center. To acknowledge other modes of inheritance besides genetic inheritance in no way excludes the centrality of genetic inheritance. Even in the evolution of cultural practices, which might seem to occur independently of genes, genetic selection plays an ultimate role (Baum, 2017a, 2017b, 2017c). For example, at present, most countries around the world have experienced a decline in birth rate. With few exceptions, people the world over are having fewer children, seeming to fly in the face of natural selection. If the trend continues, the world human population will soon begin to decline, because already many countries have a birth rate lower than the death rate, and more countries are going this way all the time. If the trend were to continue, the human race would eventually cease to exist. This extinction is extremely unlikely, however, because as population declines the advantages of having children will grow, and natural selection will begin to take effect. Already some countries, such as Denmark and Russia, have begun to employ incentives for couples and women to have children. Cultural practices will evolve to advantage those who have children.

All of this is to say that I don't think that "gene-centered" thinking is wrong. To acknowledge niche construction as an outcome of an organism's interactions with the environment that benefits progeny in no way denies the centrality of genetic inheritance and genetic selection. Genes that support the behavior of niche construction will be selected. If we recognize four different modes of inheritance, we do not then discard the most fundamental one of them in favor of the other three, no matter how important we think the other three are.

Acknowledging the role of natural selection in leading to the existence of organisms and their behavior gives no warrant for denying the importance of behavior. Without behavior—the organism's interactions with the environment—natural selection would be impossible. Interpreting the multiscale molar view as proposing that behavior is a "symptom" of the genes is to misread it entirely.

The multiscale molar view may be looked upon as "selectionist" thinking; that is true to an extent. It is not defined by selectionist thinking, however. The ontological and epistemological claims set it apart not only from Skinner's molecular view but also from Tolman's "molar" view. Tolman and Skinner both made moves in the direction of this multiscale molar view. Tolman's idea of "emergence" bears some relation to the recognition that behavior consists of activities that are (ontologically) processes and integrated wholes. For all his selectionism, Skinner never gave up on contiguity as the fundamental reinforcement relation, despite evidence to the contrary. Response-reinforcer contiguity, in turn, implies discrete events at moments in time. "The operant"—a noun—was a class of discrete events. Hence, I use "operant" only as an adjective, as in "operant activity."

An operant activity is an integrated whole that serves a certain function. Comparing such a behavioral whole to a Gestalt might give a sense of the "wholeness" of an activity. As an integrated whole, an activity should not be called an "aggregate." That word suggests a collection, rather than a whole with parts (Baum, 2002). From the longest time scale to the shortest time scale, every activity is a whole with parts that are themselves activities, on a smaller time scale. The part-whole relations are key.

The point I may have failed to make clear is that the selection of organism-making genes, which drove a proliferation of organisms, fundamentally affects our view of behavior. The function of an organism, as a result of that selection, is to reproduce. In this sense, it serves its genes. For behavior analysis, however, the key insight is that surviving and reproducing are the largest scale activities of the organism. Their parts are smaller-scale activities like gaining resources and maintaining health. Those activities also have smaller-scale parts and so on. Scale may be as small or as large as required.

References

Baum, W. M. (2002). From molecular to molar: A paradigm shift in behavior analysis. *Journal of the Experimental Analysis of Behavior, 78*, 95–116. https://doi.org/10.1901/jeab.2002.78-95

Baum, W. M. (2017a). *Understanding behaviorism: Behavior, culture, and evolution* (3rd ed.). Wiley Blackwell Publishing.

Baum, W. M. (2017b). Behavior analysis, Darwinian evolutionary processes, and the diversity of human behavior. In M. Tibayrenc & F. J. Ayala (Eds.), *On human nature: Psychology, ethics, politics, and religion* (pp. 397–415). Academic Press. https://doi.org/10.1016/B978-0-12-420190-3.00024-7

Baum, W. M. (2017c). Selection by consequences, behavioral evolution, and the Price equation. *Journal of the Experimental Analysis of Behavior, 107*, 321–342. https://doi.org/10.1002/jeab.256

Lamichhaney, S., et al. (2016). A beak size locus in Darwin's finches facilitated character displacement during a drought. *Science, 352*, 470–474. https://doi.org/10.1126/science.aad8786

Part III
Theoretical Behaviorism

Chapter 7
Theoretical Behaviorism

John Staddon

"Isms" are rarely a "plus" for science. The suffix sounds political. It implies a coherence and consistency that is rarely matched by reality. But labels are effective rhetorically. J. B. Watson's (1913) *behaviorist* paper would not have been as influential without his rather in-your-face term. Theoretical behaviorism accepts the "ism" with reluctance. ThB is not a doctrine or even a philosophy. As I will try to show, it is an attempt to bring behavioristic psychology back into the mainstream of science: avoiding the Scylla of atheoretical simplism on one side and the Charybdis of scientistic mentalism on the other.

John Watson was both realistic and naïve. The realism was in his rejection of the subjective, first-person accounts as a part of science. "Qualia" are not data, and little has been learned through reports of conscious experience. The naivete was in his limited view of theory: "[For human thought] the behaviorist offers a very simple, clear, straightforward scientific theory … It … hinges upon the way word habits are formed—upon word *conditioning*" (Watson, 1927, p. 158). Behaviorists who came after Watson, like Clark Hull of Yale and Kenneth Spence at Iowa, followed his preference for conditioning and animal research ("The behaviorist … recognizes no dividing line between man and brute.") but also built a corpus of largely mathematical *learning theory* that failed to live up to its promise.[1] B. F. Skinner at Harvard, a

[1] See https://www.youtube.com/watch?v=sYkmDW75MIk for a brief summary of this tradition by Gordon Bower, one of the pioneers. These neo-behaviorist theoretical models have all passed into history with few descendants. There are at least two reasons. First, the data they deal with are group averages, which are usually unrepresentative of individual subjects. Second, the models are all *local*. That is, they explain behavior on one trial, $t + 1$, in a typical trial-by-trial learning procedure, as a function of the previous trial: $p(t + 1) = F[p(t), X(t)]$ where p is response probability, X is some

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few years later, called them *methodological behaviorists*, doubled down on experimental research and essentially abandoned theory.

Theoretical behaviorism is a necessary amendment of B. F. Skinner's *radical behaviorism*. RB was wonderfully successful experimentally. The movement produced hundreds of papers reporting dozens of new phenomena. But it was much less successful in explaining them. The movement lacked, and indeed was actively hostile to, theory. Yet there were good reasons for Skinner's success.

The Success of Skinner's Method

Two technical inventions, the Skinner box and the cumulative recorder, were central. The *Skinner box* facilitated long-term (many weeks rather than a few days) automated experiments on learned behavior in individual organisms. The *cumulative recorder* showed real-time individual-subject data as opposed to the static group averages commonly reported by other researchers.

Skinner was able to demonstrate rapid learning in individual animals. The method was to present small rewards (now called *reinforcements*) right after the animal makes a desired response. The process could begin with approximations to the target behavior. Skinner called the technique *shaping by successive approximations*. The process as a whole he termed *operant conditioning*, a renaming of what was already called "instrumental learning."

Skinner recognized the spontaneity of operant behavior in advance of any reinforcement. He called such behavior *emitted* and contrasted it with the *elicited* reflex-like behavior of classical (Pavlovian) conditioning, which he called *respondent* behavior. Later work on autoshaping and "superstition" showed that Pavlovian conditioning is in fact one source for behavior that can then be conditioned operantly.

Skinner's experimental method showed that a given response, be it lever pressing by a rat or key pecking by a pigeon, need not be reinforced on every occasion. Responding can be maintained by various *partial-reinforcement* schedules.[2] Experiment soon revealed hitherto unsuspected regularities: the stable cumulative records associated with different schedules. Most important: these stable patterns could be recovered after exposure to another schedule. The typical "scallop" pattern on an FI schedule, for example, would reappear after a few days on second exposure after an intervening experience with another procedure. Behavior in condition A would be the same after different prior experiences B, C, D, etc.

Learning is, almost by definition, irreversible. The effect of treatment X will therefore be different if preceded by treatment A than if it is preceded by B. Two learning treatments cannot be meaningfully compared successively in the same

environmental event, and F is some (usually linear) function. Yet there is much evidence (e.g., Jost's law, which I discuss in a moment) that remote history does affect present behavior.

[2] The way that Skinner discovered his operant conditioning method is described in one of his most important papers: A case history in scientific method (Skinner, 1956).

subject. Most learning psychologists therefore assumed that learning must be studied by comparing groups of subjects. Yet the fact that behavior under a given reinforcement schedule is stable, the same no matter what the preceding treatment, seemed to Skinner and his followers to permit learning—operant conditioning—to be studied in single subjects Neither averaging across individuals nor comparisons between groups are required. Since the individual, not the group, is the target of all psychological investigation, and since there were known to be serious problems inferring the properties of individuals from group averages,[3] Skinner's method provided a powerful technique for understanding the effects of reward and punishment on the behavior of individual organisms.

Rate of response is visible as the slope of a cumulative record. As a subject learns a typical operant task, the slope of the record, the rate, increases: "The rate at which a response is emitted in such a situation comes close to our preconception of the learning process. As the organism learns, the rate rises." Skinner (1950) continued:

> It is no accident that rate of responding is successful as a datum, because it is particularly appropriate to the fundamental task of a science of behavior. If we are to predict behavior (and possibly to control it), we must deal with *probability of response*. The business of a science of behavior is to evaluate this probability and explore the conditions that determine it. Strength of bond, expectancy, excitatory potential, and so on, carry the notion of probability in an easily imagined form, but the additional properties suggested by these terms have hindered the search for suitable measures. Rate of responding is not a 'measure' of probability but it is the only appropriate datum in a formulation in these terms. *(p. 198)

So, response rate is useful—as what? It "is not a 'measure' of probability," says Skinner, but probability is what we should be after and response rate is our best bet.

Skinner's followers seized on the idea of response rate. They noticed that if reinforcement is available only at random times (a *random interval*, *RI*, schedule, one kind of variable interval, VI), subjects adapt by responding at a steady rate over periods of tens of minutes. Moreover, response rate rises with reinforcement rate. With these data, and Skinner's suggestion that response rate can be used as a measure of response probability, average response rate became the standard *dependent variable* for operant psychology.[4]

Pigeons, rats, and people can be easily trained to respond differentially in the presence of different stimuli, depending on consequences. If a hungry pigeon, confronted with two adjacent pecking keys, is paid off with bits of grain only for pecking the red, and not the green, key, he will soon learn to peck only the red and similarly if the payoffs are reversed. Skinner called behavior like this examples of *stimulus control.*

Skinner went on to propose the *three-term contingency* as a behavioral unit incorporating stimulus, response, and reinforcement. The idea is that reinforcing a response in the presence of a given stimulus establishes control by the stimulus of the pattern of behavior established by the prevailing reinforcement schedule.

[3] See, for example, Estes (1956) and, more recently, Staddon (2019).

[4] Skinner (1976) was not happy at the abandonment of cumulative records that followed. Once again, *averaging*—if not across subjects, within subject—seduced eager order-seekers.

Skinner called this unit the *operant*, his word for what might previously have been called a *habit.*

By inventing new concepts and renaming several old ones, Skinner created a separate terminology that helped to define a new and self-contained movement: the *experimental analysis of behavior*, aka *behavior analysis* aka *operant conditioning*.

Skinner and Theory

With the sole exception of the three-term contingency, these ideas were summarized by Skinner (1950) in a groundbreaking paper *Are theories of learning necessary?* published in 1950. He defined "theory" in an idiosyncratic way as "any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions." This definition would rule out many well-accepted theories in other areas of science. The temperature of a liquid, for example, is directly related to movement. It is not clear that the "dimensions" of temperature are the same as the kinetic energy of molecules. The spectra of hot elements—the red flame of lithium, for example—can be derived from the element's atomic properties. Again, it is not the case that the atomic properties that underlie emission spectra have same dimensions as wavelength. It cannot be right to rule out theories like this.[5]

Skinner argued that learning theories are for the most part impediments to scientific advance: "Much useless experimentation results from theories, and much energy and skill are absorbed by them" although he also conceded that "It would be foolhardy to deny the achievements of theories of this sort in the history of science." "This sort" refers to a rather opaque previous paragraph in which Skinner attempts to distinguish between "postulates," "theorems," and "theories." He admits, in a widely cited phrase, there is a "need for a formal representation of the data reduced to a minimal number of terms" but at the end of his article says that "We do not seem to be ready for theory in this sense." But we are surely ready now.

Problems with Atheoretical Behaviorism

The shaky philosophical basis for Skinner's anti-theory argument was completely overshadowed by the very compelling experimental examples he described in the rest of the 1950 article. His novel method produced strikingly orderly real-time patterns of behavior in individual organisms. He proceeded to use these data to identify

[5] There is something in physics called *dimensional analysis* which says that the dimensions (typically mass, length, and time) on both sides of an equation must match. But it is not clear that this was Skinner's meaning for "dimension."

what he called *controlling variables*, those aspects of the training procedure responsible for the observed patterns: “the independent variables of which probability of response is a function.”

When we know the controlling variables, he argued, theory is unnecessary. Defending his idea that response probability is the correct dependent variable for learning psychology, he showed that the alternative favored by reflex-type theorists, *latency*, did not behave in the appropriate way. Motivated and unmotivated animals show the same modal response latency on many tasks. Motivated animals do not respond faster, as they should if latency is an adequate measure of response strength. As hunger motivation is reduced, latencies become more *variable* however, a key difference as we will see.

In another experiment, arguing against the inhibition theory of extinction, Skinner showed that well-trained pigeons forget little even after a lapse of 4 years between successive exposures to a task. He also showed that the pattern of a cumulative record in extinction is related to the pattern built up during training. He attributed the difference between extinction of a periodic and an aperiodic schedule to *novelty* and dissipation of emotional responses. He described the method that would later be used by Guttman and Kalish[6] to measure stimulus generalization.

Skinner’s examples were striking. His conclusion was persuasive. Many readers came to accept his bold claim that theories of learning—not just the flawed theories then current but perhaps all learning theories—are not just unnecessary but impediments to progress in scientific psychology.

But Skinner’s atheoretical behaviorism is flawed in several ways, which I can best illustrate by revisiting some of his examples. Let’s look at three and see how they lead to the theory and philosophy of theoretical behaviorism.

Response Rate

Skinner (1950) wrote:

> Rate of responding appears to be the only datum that varies significantly and in the expected direction under conditions which are relevant to the “learning process…Once in possession of an effective datum, however, we may feel little need for any theoretical construct of this sort. (p. 198)

In other words, if we can find something varies in ways that we deem appropriate—a “datum that varies…in the expected direction under conditions which are relevant to the learning process”—let’s go with it. As for theory, there will be “little [or no] need” for it.

A small problem for the hegemony of response rate is that it can itself be controlled by the appropriate contingencies of reinforcement. For example, animals

[6] Guttman, N., & Kalish, H. I. Discriminability and stimulus generalization. *Journal of Experimental Psychology,* 1956, *51,* 79–88.

will learn, albeit with some difficulty, to space their pecks or lever presses 10 s apart (spaced-responding schedule) if that is a condition for reinforcement (Staddon, 2016)—and even though the "natural" rate for an equally rewarding schedule that lacks the spaced-responding requirement is much higher, perhaps 60 pecks per minute. Since rate of response, over the typical period of 30 min or so, on a spaced-responding schedule is low, then probability of response, hence response "strength," must also be low, according to one reading of Skinner—lower than on, say, a variable-interval schedule dispensing reinforcements at the same rate. This is obviously wrong. Response rate, per se, is not an adequate measure of response strength. What, then, is it?

Skinner never wrote explicitly about this issue. But an obvious rebuttal is that the spaced-responding schedule involves the discrimination of time. Response probability, hence response strength, is high at some times and low at others. It is high close to 10 s after each response and low in between, for example. Much the same is true of a fixed-interval schedule: response rate high near the time of reinforcement and low earlier in the interval. Perhaps, Skinner might respond, it is when reinforcement probability is constant, like RI schedules, that response rate is an indicator of response strength. But this uncertainty makes response rate less attractive as a universal measure of response strength than *time*. Perhaps the question should be not "How does schedule X affect response rate?" but "How does schedule X affect the temporal location[7] of behavior?"

Using time as a dependent measure also avoids a problem that is rarely addressed: over what time period (minutes? hours?) should response rates be computed—and why? In operant conditioning experiments, rates are usually computed over intervals of 30 min or more. The choice of denominator is justified not by any theoretical rationale but by the orderly functional relations that result. Using "order" alone as an experimental criterion has its pitfalls, however.[8]

Memory

Skinner never mentioned the word *memory* in the 1950 article and rarely afterward. But he did discuss *spontaneous recovery*, a paradoxical property of experimental extinction: After sufficient training, an organism responds. If reinforcement is withdrawn, responding ceases (extinction), usually within a single experimental session. But the next day, returned to the apparatus, the animal begins to respond again. Since we know (argued Skinner) that little or no forgetting should occur from 1 day to the next, this recovery of the extinguished response, an apparent forgetting of the extinction on the previous day, needs explaining.

[7] See, for example, Williams et al. (2008).

[8] See Staddon (2020), which also discusses Jost's law (below) and non-exponential habituation.

Until Skinner's paper, the standard explanation for spontaneous recovery was that during the extinction session, inhibition builds up, but by the next day it has dissipated so responding recovers, at least for a while. But Skinner already showed that mere passage of time has little effect on level of responding (although we will have reason to question that in a moment). So perhaps some other variables are operating? Skinner (1950) proposed two: *emotion* and *novelty:*

> When we fail to reinforce a response that has previously been reinforced, we not only initiate a process of extinction, we set up an *emotional response*…The pigeon coos in an identifiable pattern, moves rapidly about the cage, defecates, or flaps its wings rapidly in a squatting position that suggests treading (mating) behavior. This *competes* with the response of striking a key and is perhaps enough to account for the decline in rate in early extinction…Whatever its nature, the effect of this variable is eliminated through *adaptation*. (pp. 203–204, emphases added)

Skinner said no more than this about "emotion," but his description is interesting for two reasons. First, it involves *observation*, actually watching the pigeon subjects. This practice soon fell out of fashion in behavior analysis. Yet direct observation of behavior was later to prove critical in undermining one aspect of Skinner's approach. Second, he might have said something more about *competition*, which is apparently also involved. As it is, *emotion* is unsatisfactory as an explanation because the new process he invokes to explain its dissipation, *adaptation*,[9] is not itself explained.

But novelty is the variable Skinner thought most important: "Maximal responding during extinction is obtained only when the conditions under which the response was reinforced are precisely reproduced." First Skinner (1950) describes *stimulus generalization*, the decline in responding in the presence of stimuli different from the training stimulus. Then he goes on:

> Something very much like this must go on during extinction. Let us suppose that all responses to a key have been reinforced and that each has been followed by a short period of eating. When we extinguish the behavior, we create a situation in which responses are not reinforced, in which no eating takes place, and in which there are probably new emotional responses. The situation could easily be as novel as a red triangle after a yellow [his earlier example of stimulus generalization]. If so, it could explain the decline in rate during extinction. (p. 204)

Novelty, as subsequently precisely measured in the stimulus generalization experiments of Guttman and Kalish (1956) and many others, is the real explanation for spontaneous recovery, said Skinner. But again, this is an incomplete account, if only because we cannot easily measure the stimulus in this case. In regular stimulus generalization, to a color or a shape, for example, both physical stimulus properties and the effects of changes on responding can be measured objectively, not so in the case of extinction, the case that Skinner is attempting to explain. How exactly should "novelty" be measured? Something more is needed: a *theory* of memory, perhaps?

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[9] Emotion which competes with the learned behavior and adapts with time may seem to many readers hard to distinguish from the *reactive inhibition* that Skinner was criticizing.

A relevant theory was in fact available. At the end of the previous century, Adolf Jost (1897) proposed two memory laws, the second of which is as follows: given two associations (equivalently, habits, memories, operants) of the same strength, but of different ages, the older one will fall off less rapidly with time. Jost's law implies that the strength of a habit does not decay exponentially, by the same fixed fraction each day, because if it did, the relative strength of two memories would not change with lapse of time (Simon, 1966).

On the other hand, suppose that the strength of a habit, V_i, after time, t_i, is given by a formula like this:

$$V_i = \frac{K_i}{A + t_i}, \tag{7.1}$$

where K_i is the salience—strength—of habit i at time zero and A is a parameter representing the memory decay rate. If we look at the rate of change of V_i with time, we get.

$$\frac{dV_i}{dt} = -\frac{1}{\left(V_i + t_i\right)^2}. \tag{7.2}$$

Now, suppose that at a particular time after learning, with values t_1 and t_2, $t_1 > t_2$, representing the ages of two memories, the memory strengths, V_i, of events 1 and 2 are equal. Then the rate of decline for each strength will be given just by Eq. (7.2), with V the same for both memories. Since $t_2 < t_1$, clearly the rate of decline of memory strength will be greater for the more recent memory. Equation (7.1) is hyperbolic, but many other monotonic decreasing functions will do as well to model Jost's law.

Jost's law explains spontaneous recovery. Since the first extinction session is necessarily more recent than the many days of conditioning that preceded it, the associated behavior should lose more strength from 1 day to the next than the earlier conditioning. At the end of the first day of extinction, responding ceases, which means that the strengths of the two memories, for responding and for not responding, must be roughly equal. Once responding ceases, no further decline in the tendency to respond can occur. But the next day, the older tendency—to respond—must gain (according to Jost) over the more recent one (not responding), hence spontaneous recovery.

A model like this could make predictions about the effects of different delays before returning the animal to the apparatus and on different amounts of training on subsequent extinction.[10] If the second extinction session follows closely on the first, recovery should be less, for example. In other words, the theory draws attention to historical variables as possibly involved in recovery after extinction, a useful

[10] See Staddon (2016), *op. cit.* Especially Chap. 15.

advance over the "novelty" idea which looks only at a contemporary cause and one that is difficult to measure objectively.

A Jost's law account implies that memories *compete* in some way. The competition idea also speaks to the apparent contradiction between the very slow decay of well-learned operant behavior demonstrated in Skinner's 4-year experiment and the apparent rapid forgetting of an extinction experience illustrated by spontaneous recovery. Is bad news (extinction) just forgotten more quickly than good (conditioning)? No need to assume so if the Jost account is correct. The key is *competition* between memories. In the absence of any competition, a habit may persist for a long time, as Skinner's pigeons showed. But when the competition is weak—just one extinction session—memory for many earlier conditioning sessions reasserts itself, and responding recovers until more extinction experience has accumulated.

Hyperbolic discounting is a phenomenon much studied by behavioral economists with both human and animal subjects.[11] In a choice situation, subjects usually prefer a reward of size 2 after a delay of 10 s, say, over a reward of size 5 after a delay of 20 s, even though the *rate* of return is better for the larger, later reward. This contradicts the standard exponential discounting assumption, which assumes that rate of return is key.

It is tempting to relate hyperbolic memory decay to hyperbolic discounting in choice experiments, and there may be some theoretical link. But also involved is the fact that organisms typically *time* their responses to be proportional to the expected time of a reward. There is also some evidence that the larger the anticipated reward, the sooner animals will respond (Reid & Staddon, 1982). Offered a choice, therefore, between two stimuli, one signaling a small reward after 5 s vs one more than twice the size after 10 s, preference will be a balance between the tendency to wait a time proportional to the expected delay (which favors the smaller, sooner reward) and an opposed tendency to respond sooner if the expected reward is larger. The experimental evidence seems to suggest that the latter effect is smaller than the former. Animals are likely to respond sooner to the shorter delay, even if the associated reward size is smaller—and even if the overall rate of reward associated with the smaller choice is less than for the larger.

The Operant-Respondent Distinction

Ivan Petrovich Pavlov (1849–1936) never thought of himself as a psychologist. His pioneering work on conditioned responses like salivation (typically by a dog following several pairings between a buzzer or a metronome, say, and the delivery of food) was physiology, not psychology. The focus of Pavlov, and many who followed him, was on the reflex-like behavior maintained by *classical* or *Pavlovian* or, in Skinner's

[11] https://en.wikipedia.org/wiki/Hyperbolic_discounting

terms, *respondent conditioning*. Pavlov found that the conditioned[12] response was most rapidly obtained if the food followed closely on the stimulus (temporal *contiguity*). Subsequent work by Robert Rescorla and others showed that the key was *prediction.* The signaling stimulus need not be close in time to the reinforcer so long as it is closer than any other signal. Skinner justified his term *respondent* by pointing out that conditioned responses like salivation are products of the autonomic (involuntary), not the somatic (skeletal), nervous system. *Operant* behavior, he thought, depended on the somatic system.

But the field might have developed very differently if Pavlov and the very many others who followed him had asked the question: What is happening when I present the food not immediately but after several seconds, i.e., after a time too long to get salivation? No salivation, perhaps, but is nothing else happening? Really?

A story recounted by the great ethologist Konrad Lorenz (1969) provides a clue:

> My late friend Howard Liddell told me about an unpublished experiment he did while working as a guest in Pavlov's laboratory. It consisted simply in freeing from its harness a dog that had been conditioned to salivate at the acceleration in the beat of a metronome. The dog at once ran to the machine, wagged its tail at it, tried to jump up to it, barked, and so on; in other words, it showed as clearly as possible the whole system of behavior patterns serving, in a number of *Canidae*, to beg food from a conspecific. It is, in fact, this whole system that is being conditioned in the classical experiment.

It seems likely that some, at least, of this rich repertoire of operant behavior will appear even if the conditioned stimulus is too long to produce much salivation but is sufficiently predictive to allow the dog to anticipate food.

Another sign that something was wrong with the neat dichotomy between operant and respondent was provided by a pair of experiments: a very influential short paper by Skinner (1948b) and a longer experimental and theoretical paper more than 20 years later (Staddon & Simmelhag, 1971).[13] Here is what Skinner did in 1948. Hungry pigeons were placed in a box and given brief access to food at fixed periods—15 s for some animals and longer periods for others. This is *temporal conditioning* (a *fixed-time*—FT—schedule in operant terminology), which is a Pavlovian procedure since the animal's behavior has no effect on food delivery. Despite the absence of an operant contingency, all the animals developed vigorous stereotyped, apparently operant, activities in between feeder operations. Skinner attributed this behavior to accidental contiguity between some spontaneous behavior by the pigeon and the delivery of food: *adventitious reinforcement*.[14] Since these conjunctions were accidental, not causal, Skinner termed the activities "superstitious" and likened them to human superstitions.[15]

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[12] *Conditional* is more accurate, but *conditioned* has become conventional.

[13] Staddon and Simmelhag (1971) replicated, extended, and reinterpreted Skinner's (1948b).

[14] Indeed, he presented the experiment as a test of the adventitious reinforcement hypothesis. This is to my knowledge the only time, in any publication, that Skinner described an experiment as a test of a hypothesis.

[15] See the video snippet: https://www.youtube.com/watch?v=7XbH78wscGw in which biologist Richard Dawkins, long a foe of religion, shows a pigeon in a Skinner box. He slightly misdescribes

More than 20 years after the superstition paper, Staddon and Simmelhag repeated Skinner's experiment and observed the pigeons' behavior second by second in each interfood interval from the very beginning of training. Their aim was atheoretical. They were simply curious: let's see what happens, in detail, and let's see if the interfood interval has to be constant (as in Skinner's experiment), or can it be variable?

It turns out that variable intervals work fine; a variable-time schedule also induces "superstitious" activity. But Staddon and Simmelhag (1971) also found three things that differ from Skinner's account:

1. The activities that develop are of two kinds: *interim activities* that occur in the first two-thirds or so of the fixed interfood interval and a single *terminal response* that occurs during the last third.
2. The terminal response is either pecking or a stereotyped pacing activity obviously related to it; the terminal response does not differ from animal to animal in the capricious way implied by Skinner's account.
3. Terminal pecking often appeared suddenly after several days in which a different activity was contiguous with food at the end of the fixed-time interval training. The pecking did not develop following an accidental conjunction with food, as the adventitious reinforcement hypothesis implies (Fig. 7.1). Interim activities are rarely contiguous with food and so also cannot be explained by adventitious reinforcement.

In short, Skinner's account is wrong. The superstitious behavior he observed was not the result of accidental contiguity between an emitted behavior and response-independent food.

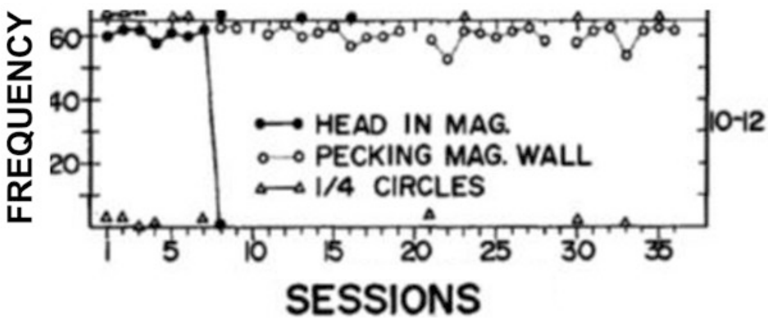

Fig. 7.1 Development of the terminal response. Activities of a single pigeon in the last 2 s of a 12-s fixed-time (Pavlovian temporal conditioning) schedule across 36 60-interval training sessions. Head-in-mag is contiguous with food for seven sessions but is supplanted by pecking in session 8 (Staddon & Simmelhag, 1971, Fig. 3)

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the "superstition" experiment but then correctly explains Skinner's (mistaken) adventitious reinforcement explanation. "Humans can be no better than pigeons" Richard concludes. Skinner's plausible though wrong account still flies phoenixlike around the Internet.

This experiment, and an earlier one (Brown & Jenkins, 1968; see also Williams & Williams, 1969) showing that naïve pigeons will learn to peck an intermittent 7-s light (conditioned stimulus: CS) that ends with free food (unconditioned stimulus: US), showed that Skinner's dichotomy between operant (somatic) and respondent (autonomic) behavior does not hold, since pecking—the prototypical operant response—can behave just like salivation, the prototypical respondent. These results demanded a revision of the standard framework for the study of operant conditioning. If pecking is both an operant and a respondent, but salivation, for example, can be classically but not operantly conditioned, if supposedly "instinctive" activities can supersede already learned operant behavior (Breland & Breland, 1961), the simple separation between classical and operant conditioning becomes untenable.

Selection and Variation

Beginning in the early 1950s, people began to point out the similarities between the learning process and evolution through variation and selection (e.g., Pringle, 1951). Recently, models explicitly analogous to gene mutation and selection by reinforcement have successfully duplicated many operant conditioning phenomena (McDowell, 2013).[16] Skinner's idea of *emitted* behavior fits quite naturally into a Darwinian scheme. Behavior varies; a variant that is contiguous with reward is strengthened and thus increases in frequency.

Unlike Darwin, Skinner had little to say about the causes and types of variation. He left the impression that variation is unstructured, "random." On the other hand, observations like Liddell's show that the repertoire from which reinforcement selects is very far from random. It is different for food than for sex or social reward, for example. Remember Lorenz's account: A dog, released after being conditioned in Pavlov's restraining harness, now, at the sound of the metronome, showed a wide range of operant-type food-related behavior in addition to salivation. Lorenz, an ethologist, identified the dog's behavior as a particular instinctive pattern. A cognitive psychologist might say that the dog is showing an *expectation* of food. A more behavioral account is that the conditioning process causes the conditioned stimulus to evoke a particular behavioral *repertoire.* The emitted behavior to which that repertoire gives rise is not at all random.

[16] See also Edelman's *Neural Darwinism:* https://www.webofstories.com/play/gerald.edelman/37;jsessionid=4B59A75EAF082B9FF369CB6D98C19671

The Repertoire

The composition of the repertoire will depend on the animal's training—learning the signal properties of the metronome—motivational state, and species. Anticipation of food will lead to a different repertoire than anticipation of electric shock. Food → vigorous activity, tail-wagging, etc. Electric shock → "freezing" and crouching—suppression of all activity. Indeed, *conditioned suppression* (CS) is the name for the shock-anticipation procedure used by Rescorla (1967) and others to establish the necessary and sufficient conditions for respondent conditioning.

The idea of a repertoire implies that some behaviors are potential, lying in wait, but ready to occur if the *active* behavior goes unrewarded. The stronger the animal's motivation and the better the predictive properties of the stimulus (how close, how big, the reward?), the more restricted the repertoire is likely to be. In the limit, if the stimulus (as in the autoshaping experiments) or the interfood interval (as in the superstition experiment) is very short, the repertoire may be limited to a single response: pecking. But if the situation is not too "hot," the repertoire will be larger.

In addition to the active behavior at any moment, a repertoire comprises *latent* or covert activities that *can* occur. This idea of a latent response should not be upsetting to radical behaviorists. It was suggested by Skinner (1948a) himself in the same year that he published the "superstition" experiment:

> Our basic datum…is the probability that a response will be emitted… We recognize …that … every response may be conceived of as having at any moment an assignable probability of emission… A *latent* response with a certain probability of emission is not directly observed. It is a scientific construct. But it can be given a respectable status, and it enormously increases our analytical power…. It is assumed that the strength of a response *must reach a certain value* before the response will be emitted. This value is called the *threshold.* (pp. 25–26, emphases added)

Skinner was writing about language and never extended the idea to the operant behavior of nonhuman animals. But his proposal is different from theoretical behaviorism in only one respect: for the ThB hypothesis, the *threshold* is simply *competition* from other latent/silent responses.

The idea that any predictive relation between a stimulus and a reward creates an expectation, equivalently, a repertoire of potential actions, answers the question I posed earlier. Imagine a conditioning situation in which the CS is just a bit too long to yield conditioning, as measured by, say, salivation, or an auto-shaped key peck. So long as the CS is still predictive (e.g., signals a shorter time to the US than other signals), the animal can still form an expectation and develop a repertoire. Members of the repertoire will be available as candidates for operant conditioning, which is to say selection by temporal contiguity. But the repertoire itself, active response excepted, will be covert and may not reveal itself at once. If the animal, like Pavlov's dog, is restrained, for example, its behavioral potential is necessarily limited. But freed from restraint, the dog shows at once the wide range of activities induced by a stimulus that signals imminent food.

Emitted responses can be induced in other ways. An unexpected reward will at once elicit a range of food-related activities, for example. Similarity of a new situation to one associated with food or a mate will similarly elicit a historically relevant repertoire.

Extinction shows the effects of relaxing selection. When reinforcement is withdrawn, the selection process ceases, and the trained response declines. But observation, and Skinner's (1950) latency data, shows that other activities, suppressed by the training schedule, now occur again. This is the normal increase in variability when selection is relaxed, either natural selection or selection by reinforcement schedule.[17] Extinction usually leads to more variable behavior.

Education

Operant learning involves both selection and variation, but almost all experimental research has been on the selection bit: the effect of contingencies of reinforcement on behavior. Unfortunately, behavior analysis has treated teaching and education in the same way. Operant reinforcement and punishment is an appropriate way to maintaining classroom discipline. The teacher is dealing with behaviors that have a nonzero *operant level*; they occur spontaneously. Her task is to increase that level for some (paying attention, doing chores, polite behavior, etc.) and reduce it for others (fidgeting, bullying, distracting other pupils). Contingencies of reinforcement do have some application here. But the *education* part of teaching is not aimed to increase the level of something already known but to bring into existence something previously unknown. The teacher must get pupils to grasp something for the first time, not just change the rate of emission of something already known.

Real teaching, imparting new knowledge and skills, is much more about *variation*, the source of a pupil's repertoire, than about selection, changing the strength of an existing behavior (Staddon, 2006). A recent review, one of many, suggests that simply rewarding answers to multiple-choice tests, Skinner's original teaching-machine approach, is not an adequate way to foster learning.[18] Many writers have described how their schooldays, perhaps at a boarding school where control by the educational environment can be very strong, provided them with an environment that fostered study, creativity, and critical thinking.[19] Creativity, etc., are not operants. They are properties of a *repertoire* of potential operant behavior. Unscientific and anecdotal as they are, these first-person accounts nevertheless give hint at what is needed if education is not to become mere schedule control.

[17] See Staddon and Simmelhag (1971) *op. cit.* p. 23 *et seq.*

[18] http://www.economist.com/printedition/2017-07-22

[19] See, for example, Richard Dawkins' account of his own public school, Oundle: https://www.theguardian.com/books/2002/jul/06/schools.news, and Alan Macfarlane's (2014) "The image of the good imperial education."

The emitted repertoire is set by processes usually studied under the rubric of classical conditioning. The repertoire depends on what the subject can expect (predictive stimulus-stimulus relations he has experienced in a given situation), on his motivation (hunger, thirst, sex, fear, etc.), and on what kind of organism he is. But the organism doesn't begin with nothing. Even without conditioning, a sheepdog, for example, knows (more or less) what a sheep is and what needs to be done about it even before he sees one. I remember my uncle's border collie "Monk," never having seen a sheep, doing his best to herd children, the next best thing, on Hampstead Heath in the middle of London many years ago. Katy Cropper, a British lady, tours country fairs with her sheepdogs that herd flocks of geese. With very little training, a puppy let off leash and, perhaps after some exploration, will return to his human companion. Unless distracted, the dog will follow his master. Much of the adult repertoire already exists in rudimentary form, needing only a little training to mature (at least in most dogs!). Humans come with repertoires like this that can be expanded (or contracted!) and directed in ways known to great teachers but still not codified by science. Education would surely benefit if much less attention were paid to selection and much more to behavioral variation.

Summary

Treating classical (respondent) conditioning and operant conditioning as different processes has taught us much about the necessary and sufficient conditions for conditioning to occur. But it has also led learning psychology somewhat astray. Learning researchers were misled by Pavlov's genius and the neurophysiological differences between typical classically conditioned responses and typical responses conditioned operantly. Salivation and lever pressing are obviously very different.

In fact classical and operant conditioning are just different faces of the same coin. Classical conditioning is a process that detects correlations between environmental features and something of value, positive or negative, to the organism. This correlation induces a repertoire from which operant conditioning can select. If the correlation is very strong and the unconditioned stimulus is imminent, then the induced repertoire may be limited—to pecking (in a hungry pigeon) or to salivation (in a restrained dog). Selection, in the sense of a response contingency, may be unnecessary. The result may look like a reflex, but isn't, although restricted behavioral options and extreme motivation may make it appear so.

If the selection is weaker, some "expectation" may still be formed, and the repertoire may comprise many responses, most of them latent. Operant reinforcement must select from this pool. If there is no reinforcement, the behaviors that comprise the repertoire will occur one after another, back and forth, each time weaker and weaker. Eventually vigorous activity may cease altogether, leaving a passive, behavioral residue.

The old Yerkes-Dodson law[20] (1908) shows that learning is fastest at intermediate levels of motivation, which suggests that the size of the repertoire is then at its maximum. As the organism learns, behavior adapts, reinforcement rate increases, and the repertoire shrinks to a class of responses defined by their consequences and controlled by a class of stimuli that are a reliable signal of the contingencies. This is Skinner's three-term operant. Another name for the operant is *state*—not internal state or physiological state or even mental state but state as repertoire controlled, in the well-trained organism, by identifiable stimuli under certain motivational conditions. (For the philosophical/logical details on state as equivalent history, see my *New Behaviorism* (Staddon, 2021) and also Staddon (2017). But these details are not necessary to see the need to add *state* to *stimulus* and *response* to arrive at an accurate picture of the behaving organism.)

Theoretical behaviorism repeals Skinner's proscription of theory. The "ism" is unfortunate because ThB is not rigid ideology that rules things out. It is theoretical but eclectic. It does require that data and theory be observable and testable by a third party. But in that sense it is just science. Concepts like *memory* and *expectation* are perfectly acceptable, just so long as they can be given some explanatory and predictive meaning.

The selection/variation view of learning implies that there is no sharp distinction between classical and operant conditioning. Operant reinforcement selects from a repertoire, just as Skinner argued. But that repertoire comes from somewhere. It has causes. One of them is stimulus-stimulus correlations detected by the processes labeled as classical conditioning. Classical and operant conditioning are a team, even if one process (a repertoire set by classical conditioning) can occasionally limit another (selection from the repertoire by response-reinforcer contiguity). Autoshaping, superstitious behavior, memory, and expectation pose problems for Skinner's radical behaviorism. They are easily incorporated by theoretical behaviorism.

References

Breland, K., & Breland, M. (1961). The misbehavior of organisms. *American Psychologist, 16*, 661–664.

Brown, P. L., & Jenkins, H. M. (1968). Auto-shaping of the pigeon's key-peck. *Journal of the Experimental Analysis of Behavior, 11*, 1–8.

Estes, W. K. (1956). The problem of inference from curves based on group data. *Psychological Bulletin, 53*, 134–140.

Guttman, N., & Kalish, H. I. (1956). Discriminability and stimulus generalization. *Journal of Experimental Psychology, 51*, 79–88.

Jost, A. (1897). Die Assoziationsfestigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen. *Zeitschrift fűr Psychologie und Physiologie der Sinnesorgane, 14*, 436–472.

[20] https://en.wikipedia.org/wiki/Yerkes%E2%80%93Dodson_law

Lorenz, K. (1969). Innate bases of learning. In K. Pribram (Ed.), *On the biology of learning*. Harcourt Brace.
Macfarlane, A. (2014). The image of the good imperial education. In M. Egan (Ed.), *The character of human institutions: Robin Fox and the rise of biosocial science*. Transaction Pub.
McDowell, J. J. (2013). A quantitative evolutionary theory of adaptive behavior dynamics. *Psychological Review, 120*(4), 731–750.
Pringle, J. W. S. (1951). On the parallel between learning and evolution. *Behaviour, 3*, 174–215.
Reid, A. K., & Staddon, J. E. R. (1982). Schedule-induced drinking: Elicitation, anticipation, or behavioral interaction? *Journal of the Experimental Analysis of Behavior, 38*, 1–18.
Rescorla, R. A. (1967). Pavlovian conditioning and its proper control procedures. *Psychological Review, 74*, 71–80.
Simon, H. A. (1966). A note on Jost's law and exponential forgetting. *Psychometrika, 31*, 505–506.
Skinner, B. F. (1948a). *Verbal behavior*. William James Lectures, Harvard University. http://www.behavior.org/resource.php?id=595
Skinner, B. F. (1948b). "Superstition" in the pigeon. *Journal of Experimental Psychology, 38*, 168–172.
Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review, 57*, 193–216.
Skinner, B. F. (1956). A case history in scientific method. *American Psychologist, 11*(5), 221–233.
Skinner, B. F. (1976). Editorial: Farewell my LOVELY! *Journal of the Experimental Analysis of Behavior, 25*(2), 218.
Staddon, J. (2006). Did Skinner miss the point about teaching? *International Journal of Psychology, 41*(6), 555–558.
Staddon, J. (2021). *The new behaviorism* (3rd ed.). Psychology Press.
Staddon, J. (2016). *Adaptive behavior and learning* (2nd ed.). Cambridge University Press.
Staddon, J. (2017). Simply too many notes. *The Behavior Analyst, 40*(1), 101–106.
Staddon, J. (2019). Object of inquiry: Psychology's other (non-replication) problem. *Academic Questions, 32*, 246–256. https://doi.org/10.1007/s12129-019-09778-5
Staddon, J. (2020). The role of theory in behavior analysis: A response to unfinished business, Travis Thompson's review of Staddon's new behaviorism (2nd ed.). *Psychological Record*. https://doi.org/10.1007/s40732-020-00409-y.
Staddon, J., & Simmelhag, V. L. (1971). The "superstition" experiment: A reexamination of its implications for the principles of adaptive behavior. *Psychological Review, 78*, 3–43.
Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20*, 158–177.
Watson, J. B. (1927). How we think: A behaviorist's view. *Harper's Magazine*.
Williams, D. A., Lawson, C., Cook, R., Johns, K. W., & Mather, A. A. (2008). Timed excitatory conditioning under zero and negative contingencies. *Journal of Experimental Psychology: Animal Behavior Processes, 34*(1), 94–105.
Williams, D. R., & Williams, H. (1969). Auto-maintenance in the pigeon: Sustained pecking despite contingente nonreinforcement. *Journal of the Experimental Analysis of Behavior, 12*, 511–520.
Yerkes, R. M., & Dodson, J. D. (1908). The relation of strength of stimulus to rapidity of habit-formation. *Journal of Comparative Neurology and Psychology., 18*, 459–482.

Chapter 8
What Is the Theory of Theoretical Behaviorism?

Carlos Eduardo Lopes

In order to understand the proposal of *theoretical behaviorism*, it is necessary to define the notion of *theoretical* that is being used in this expression. This is not an easy task, as there are different meanings of theory in the philosophical, scientific, and psychological discussions. In which sense is Staddon's behaviorism theoretical? Which theory is being defended by that proposal? I shall try to point out some directions to address these questions by examining uses of the term *theory* that seem to be involved in the proposal of theoretical behaviorism.

Theory as "ism"

Staddon has caveats in relation to the use of theory as an "ism" (as in the case of behaviorism), as he inaugurates his text criticizing it. The critical argument is that "isms" (a) sound political (which seems to restrict the use of "isms" to politics), (b) do not add anything to science (which suggests the possibility of doing science without "isms"), and (c) only have a rhetoric function (suggesting that rhetoric is "external" to science). Let us look into this.

Originally, the term "behaviorism" seems to have the function of marking off the limits of investigation and explanation of psychological phenomena—a behavioral delimitation (e.g., Watson, 1913). Different from Staddon's claims, this does not seem to be more of a political issue than a scientific one, nor something that can be disregarded in the scientific practice. The image of a scientist isolated in his laboratory, absorbedly analyzing data, contrasts with the "real scientific activity," which depends on an articulation between scientists and people from outside the

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laboratory—such as governors, military authorities, businessmen, religious leaders (see Latour, 1987/2003). In other words, besides producing and disseminating data, scientists need to get funding, recruit allies (inside and outside the scientific community), defeat adversaries, appear in the media, and occupy strategic positions in scientific politics, the kind of activities that require a fair amount of rhetoric.[1]

If scientific activity requires rhetoric, and if "isms," as Staddon pointed out, have exactly this function, the definition and self-definition around an "ism" seems to be important to science. In the case of psychology,[2] in which there are still divergences when it comes to defining its object of study, the delimitation fostered by an "ism" seems to be decisive. In this context, the use of an "ism" creates a "theoretical identity" of the defended proposal, relating it to a specific community. The solution proposed since Watson (1913), about affiliating behaviorism to natural sciences, does not seem to avoid compromising with an "ism"; on the contrary, it is the explicit adoption of an "ism"—behaviorism—what would allow the abandonment of the mentalistic psychological field.

Someone could argue that the best strategy would be actually the one described by Wittgenstein (1921/2002), about "throw away the ladder after he has climbed up it" (§ 6.54). In other words, after leaving the polemic and dissonant mentalistic psychological field, it would no longer be necessary to use "isms": we would just be another group doing science—this is where Staddon's explanation about "isms" rarely being "a 'plus' for a science" comes from. In my opinion, there are two problems with this conception.

First, it ignores all the existing controversy in the scientific field, which, far from being consensual, is paved with epistemological discussions, including, ultimately, the very definition of science (see Erduran & Dagner, 2014). These discussions are full of "isms," such as realism, constructionism, positivism, rationalism, naturalism, empiricism, pluralism, and reductionism. Apart from describing different ways of doing science, these terms also establish limits and different criticisms of the scientific knowledge. Saying that any of these classifications does not matter, that scientific practice happens without any kind of philosophy, would mean ignoring key debates. I do not think that this is Staddon's case, as he criticizes Skinner's inductivism while showing a preference for formulations of the deductive model—in the tradition of Hempel and Oppenheim (1948).

Second, those like Watson who considered behaviorism to be the only way to bring psychology to the scientific field believed that the growing adherence to this

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[1] With the exception of occupying positions, which he systematically refused to do after Indiana, Skinner was an example of the need for "leaving the laboratory" in order to institutionalize a new area of scientific research (see Cruz, 2019; Rutherford, 2000, 2004).

[2] In Brazil's case, the debate with psychology is fundamental, since a behavior analyst or a behaviorist is, above all, a psychologist. I know that in other contexts this can be quite different; that is why I want to make it clear that I am talking from my cultural perspective (Brazil). The proposal of a rupture with psychology as defended by some authors (e.g., Ulman, 1993/2004) does not seem viable to me in this cultural context, and, to be honest, I doubt whether it would be something really fruitful.

"theoretical delimitation" would make this "ism" no longer necessary. After all, if everyone is a behaviorist, that word would no longer be necessary. That was exactly what Tolman (1952/1964) advocated for at the end of his life: "today we are practically all behaviorists" (p. 305). Time proved he was wrong. The second half of the twentieth century was marked by the expansion of "isms" which were ostensibly antagonistic to behaviorism, both in natural sciences and in humanities. In a short period of time, behaviorism became *persona non grata* in some universities, laboratories, conferences, and even to the general public (little effort is required to find anti-behaviorism manifests on the Internet, in tones that go from the most moderate to the angriest).

With the expansion of this animosity, some behaviorist "deserted," taking those criticisms as self-criticism and explicitly affiliating to other "isms"; the ones who "resisted" found themselves being isolated, publishing in their own journals, attending specific scientific meetings with a hope that this would eventually change.[3] I understand Staddon's advocacy of doing "just science" in this context, but I think that it does not dismiss a theoretical marking. The abandonment of behaviorism may signal just an affiliation to a less criticized "ism," as "cognitivism" or, in order to avoid the suffix, "cognitive science" (Hatfield, 2002).

Science as Theory

Although I believe that "isms" were not and could not be abandoned, there is no doubt that this meaning of theory is not the one that plays the main role in Staddon's theoretical behaviorism. In an attempt to understand his more explicit use of theory, let us analyze the negative definition introduced at the beginning of Staddon's text, which states that theoretical behaviorism is "not a doctrine or even a philosophy" or even that it is between an "atheoretical simplism" and a "scientific mentalism."

Let us begin with the denial of philosophy or doctrine. There is here a "family resemblance" with Skinner, who repeatedly treated the history of philosophy as a prescientific thought that would end up being overcome by a science of behavior (e.g., Skinner, 1938, 1953, 1969). It all happens as if philosophy were the field of pure speculation and, as such, it were not empirically supported. Science, on the other hand, through the adoption of method, would be the field of empirically proved knowledge based on objective data.

Ironically, this conception is rooted in the work of two philosophers: René Descartes and Francis Bacon. In both cases, what was at stake was the defense of the difference between scholastic tradition philosophers' conceptions about the

[3] This narrative is accurate especially for those who had at some point the hope (or certainty) that psychology would sooner or later be converted into behaviorism (or behavior analysis). I particularly prefer Pennypacker's (2004) more thoughtful position: neither complete pessimism regarding the psychology nor Skinner's exaggerated optimism regarding behavior analysis.

world (what Descartes called "common sense") and true knowledge about nature (science).

For Bacon (1620/1944), the questioning of that "common sense" comes from the identification of *idols* that stand between intellect and nature. *Idols* are human biases (both learned and innate) that distort knowledge. The great mistake of traditional philosophy was to have ignored those biases; therefore, science should not only acknowledge *idols* but should also neutralize them by using the scientific method:

> The idols and false notions which have already preoccupied the human understanding, and are deeply rooted in it, not only so beset men's minds that they become difficult of access, but even when access is obtained will again meet and trouble us in the instauration of the sciences, unless mankind when forewarned guard themselves with all possible care against them. (Bacon, 1620/1944, § 38)

When it comes to Descartes (1641/1996), the critical argumentation stems from skeptic questions from that age, which fundamental point was to demonstrate that there is no logical guarantee that the world as we know it (or as it appears to us) has to be considered a reality. Like in Bacon, the point was to refuse the truth of a "naïve" knowledge that uncritically accepted what is perceived as a basis for science.

It is from this context that modern epistemology emerges, being understood as an effort to build scientific knowledge that was free from that "naïve realism." Those three-centuries-long discussions shaped contemporary science. For example, the description of reality given by physics does not, by any means, identify with the "immediate world"; it refers to another world, one that is *not* the "lifeworld" (*Lebenswelt*). The world of physics is one of strengths, waves, and particles, in such a way that is easily described in mathematical language. Köhler (1947/1992) illustrates this difference between those two worlds quite well:

> Centuries ago, various sciences, most of all physics and biology, began to destroy the simple confidence with which human beings tend to take this world as *the reality*. Though hundreds millions still remain undisturbed, the scientist now finds it full of almost contradictory properties. Fortunately, he has been able to discover behind it another world, the properties of which, quite different from those of the world of naïve people, no not seem to be contradictory at all. (pp. 4–5)

It can be said that the daily lived world, the one that is naively taken as the reality by common sense, is the minimum degree of theory. I would not say that it is utterly devoid of theory, because this would imply ignoring the primordial *hypotheses* or *belief* that states the world is the same day after day as it has permanence (Engelmann, 2001).[4] But this is the closest we can get to an atheoretical view (Engelmann, 1981). Consequently, it is possible to conclude that science is *always* a theoretical construction of the world, which is the same as saying that there does not exist science

[4] William James describes quite well the constitution of that fundamental "belief" in child development: "When a rattle first drops out of the hand of a baby, he does not look to see where it has gone. Non-perception he accepts as annihilation until he finds a better belief. That our perceptions mean BEINGS, rattles that are there whether we hold them in our hands or not, becomes an interpretation so luminous of what happens to us that, once employed, it never gets forgotten" (James, 1909/1970, p. 63).

without theory. The question is not whether there is theory in science or if there is not but about how theoretical science is and what are the criteria and justifications for that. I will come back to this point soon.

Following that reasoning, the maximum degree of theory would be the constitution of a world that *completely* breaks with the lived world. As demonstrated by Kant (1787/2001), the "theory abuse" is the science's great temptation: in its eagerness to solve all the contradictions of the *lifeworld*, science might end up projecting an ideal world, a world that turns out to be completely separated from what it is experienced. When this happens, we are no longer talking about science but about metaphysics instead.

To avoid this split from happening, and to avoid turning into metaphysics, modern science started demanding a mandatory relation between the "real world," the one discovered by science, and the *lifeworld*, the one of the common sense. In other words, modern science started arguing about the need for *some* empirical support for what was being theoretically affirmed, which is the same as saying that scientific knowledge must be able to make its "way back." It is important to show that what was "discovered" to be "real" still has a relation to the *lifeworld*, thus allowing us to behave in a more efficient way toward it (in terms of having a better understanding, a wider capacity of prediction and/or of operating change, and so on).

However, the meaning of "empirical" in the context of this modern epistemology needs to be clarified as well. Talking about empirical data is not the same as talking about what common sense considers to be "observable" or "visible." Science works by delimiting the *lifeworld* and fulfilling its "gaps" (or, as Köhler says, its contradictions); so when we talk about "empirical" in the scientific framework, we are not talking about a prescientific experience. It is enough to remind that, a lot of times, the scientific datum is a graphic representation or a number, which only makes sense when we know what we are looking for; someone "from outside" can easily fail to see what is there. It is the same as saying that the scientific datum is also impregnated with theory—a thesis that has already been mentioned.

Although it is always necessary to establish a "bridge" with the world of the common sense, the more theoretical the scientific proposal, the more distant the obtained data is going to be from that world. Consequently, an "economic" proposal in theory is supported by observations that are guided by a set of concepts, while a more theoretical proposal could require knowledge about differential calculation in order to access the scientific datum.

In sum, when Staddon states that theoretical behaviorism stands between Scylla and Charybdis, he is following the steps of modern epistemology, thus avoiding both the atheoretical conception of the common sense and the theoretical excess of metaphysics. Therefore, differently from what has been suggested, this is not properly a novelty, as it is exactly the same definition modernity already gave of science. What remains to be discussed, then, are the justifications for having more or less theory in a scientific proposal.

The Cognitive Status of Scientific Theories

The third meaning of theory that seems to be important to address in the proposal of theoretical behaviorism is related to the *cognitive status of scientific theories* (Nagel, 1961). The question guiding this debate is: what is the function of the scientific theories? The simplest answer (and also the most criticized) is that a scientific theory *represents* reality. As learned in the previous discussion, modern epistemology initially adopted that concept of theory: the theoretical description of the world proposed by scientists was one of a "discovery" of reality. That means that when physicists talk about particles, waves, and force fields, they are describing the world as it really *is*. The key problem is that this knowledge is indirect (or theoretical), and thus there always remains the doubt about the world really being that way. Ultimately, the skeptic question persists: since what we know about "reality" is a product of our theoretical constructions, how do we distinguish between what is merely a construction and what is an accurate representation of the world? Someone could answer: by making tests! But a test can only confirm that our theory works and not that it actually portrays the world (Rorty, 1979).

In the context of these criticisms of realism, another proposal emerges: scientific instrumentalism.[5] Opposing to realism, instrumentalism argues that theories are not portraits of reality but tools (or instruments) to deal effectively with the world. Therefore, scientific theories would no longer be compromised to represent or mirror reality but only to help us to reach our scientific goals (prediction and, in some cases, changes of the investigated phenomenon). Instrumentalism does not worry about the theory-reality relation anymore, at least not in the sense of it being a relation of representation.

The issue with instrumentalism is that, when it is taken to ultimate consequences, some inconveniences seem to arise. If the only criteria for theoretical elaboration are the effectivity of prediction and the possibility of change, *any* theory that meets those criteria would presumably be validated. Consequently, we would no longer be able to set apart what is "real" from what is "constructed." It turns out that science rarely follows this course, since it tends keep a realist discourse (or at least realism as a hypothesis; see Engelmann, 2001). Undoubtedly, this has to do with the rhetoric that science uses to convince nonscientists of the importance of their "discoveries." Would scientists manage to get financial funding if they said they *built* Higgs boson instead of saying they *discovered* it (or at least that they were trying to discover it)? Nonetheless, I do not know whether scientists themselves are instrumentalists. Maybe the realist discourse has also a motivational function for them—I

[5] There is also the descriptivism, which seems to have been adopted by Skinner in some moments (Laurenti & Abib, 2005). However, as descriptivism reduces theory to a "compendious but elliptic formulation of relations of dependence between observable events and properties" (Nagel, 1961, p. 118), it restricts theory and, therefore, moves away from the proposal of theoretical behaviorism. Hence, I will present here only the debate between realism and instrumentalism. (For additional discussion of descriptivism, see Nagel, 1961.)

think they prefer to see themselves *discovering* things and not just *building* them, as well.

Furthermore, instrumentalism could endanger the delimitation of the scientific field itself. If, on the one hand, instrumentalism leaves the scientist free to create theories and to use constructs without having to worry about a correspondence with reality, on the other hand, the distinction between science and metaphysics is no longer so easy. If theory does not represent anything, why not to embrace metaphysics, as long as it allows us to reach our goals? Ultimately, what would the difference between science and metaphysics be? Does that difference matter? When must we stop our theoretical elaboration and why? I believe that these are the questions that need to be addressed when instrumentalism is adopted.

I am not quite sure about this, but at times the arguments invoked to broaden theory in behaviorism, especially those opposite to what Skinner would have (not) done, suggest the adoption of an instrumentalism on Staddon's part, for example, when he justifies that "concepts like *memory* and *expectation* are perfectly acceptable, just so long as they can be given some explanatory and predictive meaning."

As already mentioned, denying an "atheoretical simplism" is not difficult, since all science is theoretical, but if the course followed by theorization is an instrumentalist one, avoiding "scientific mentalism" does not seem like an easy task. After all, what prevents Staddon's proposal from turning into some kind of scientific mentalism? This could be what Staddon was aiming at when he stated that "isms" do not matter. But if behaviorism does not matter, neither does mentalism; there would not be any problem with scientific mentalism and, ultimately, with metaphysics itself.

Concluding Remarks

After this brief itinerary, it is possible to reexamine some of the points signaled by Staddon. First, it does not seem proper to classify Skinner's proposal as an atheoretical one, since no scientific proposal is ever atheoretical. Only common sense is atheoretical, and, even so, this can only happen if we disregard the basic hypothesis of the world's permanence as a primitive theory (see James, 1970, 1909/1970). It would be more adequate to say that Skinner's proposal is "economic" when it comes to theory or, as some authors prefer to say, that it is parsimonious (Thyer, 2009).

Second, the empirical data that support the "Skinnerian theory" require little specialized training: to understand this data, neither calculation nor, in some cases, statistics are needed. On the other hand, a "conceptual training" is still required, since, as admitted by Skinner (1969): "The fact remains that *direct observation, no matter how prolonged, tells him* [a naïve viewer] *very little about what is going on*" (p. 9). There is no point in observing something for a long time if we do not know where to look at:

> When we recall how long it took to recognize the causal action of the environment in the simple reflex, we should perhaps not be surprised that it has taken us much longer to see contingencies of reinforcement. The traditional homocentric view of human behavior

> discourages us from looking at the environment in this light, and the facts themselves are far from obvious. (p. 9)

In the case of theoretical behaviorism, the broadening of the theory necessarily affects the datum, moving it even farther away from the *lifeworld*. That is exactly what happens when Staddon resorts to mathematical models and formulations to illustrate the potentialities of theoretical behaviorism, such as Jost's law to explain memory. This means that Skinner's and Staddon's theories can no longer be compared, since the very data supporting those theories are different, challenging the affirmation that says, "Theoretical behaviorism is a necessary amendment of B. F. Skinner's *radical behaviorism*."

Third, the fundamental question raised by a proposal of theoretical behaviorism seems to be the following: Which is the criteria used to adopt a more or less theoretical proposal? Why does Skinner prefer a "lean" proposal, while Staddon advocates for a broadening in the theory? The answer given by Staddon does not seem to suffice. If there are contradictions and gaps in the Skinnerian proposal, for example, the one about the notion of *state*—which I particularly agree with (see Lopes, 2006)—or the one about the distinction between respondent and operant, broadening the theory does not seem to be exactly a solution, precisely because it places us in "another world"; in fact, we are not solving the problem of the "Skinnerian theory" but proposing another theory in which those problems no longer exist instead.

On the other hand, the justification that the broadening of the theory makes it possible to explain things that were left out by Skinner, like memory, does not seem satisfactory either. It would be necessary to evaluate if that is a problem of the "Skinnerian theory" or if that part of the *lifeworld* was set aside simply because it does not matter in the scientific delimitation that concern us.

Finally, it would be necessary to question which are the limits of the theoretical formation. The effectivity of explanation as a criterion to broaden the theory suggests an adherence to instrumentalism. Furthermore, an affiliation to instrumentalism seems to make it harder (or even impracticable) to state that theoretical behaviorism is not a "scientific mentalism."

References

Bacon, F. (1944). Novum organum. In *Advancement of learning and Novum organum* (pp. 315–450). Willey Book. (Original work published 1620) https://books-scholarsportal-info.ezproxy.library.yorku.ca/uri/ebooks/ebooks0/apa/2011-01-12/1/200805252

Cruz, R. N. (2019). *B. F. Skinner: Uma biografia do cotidiano científico*. [B. F. Skinner: A biography of scientific daily life]. Artesã.

Descartes, R. (1996). *Meditations on first philosophy. (J. Cottingham, Trad.)*. Cambridge University Press. (Original work published 1641).

Erduran, S., & Dagner, Z. R. (2014). *Reconceptualizing the nature of science for science education: Scientific knowledge, practices and other family categories*. Springer.

Engelmann, A. (1981). O behaviorismo diante da explicação cética da ciência natural [Behaviorism in face of skeptical explanation of natural science]. *Cadernos de Análise do Comportamento, 1*, 21–27.

Engelmann, A. (2001). O meu-mundo e o resto-do-mundo. [my-world and the rest-of-the-world]. *Psicologia: Reflexão e Crítica, 14*(1), 211–223. https://doi.org/10.1590/S0102-79722001000100018

Hatfield, G. (2002). Psychology, philosophy, and cognitive science: Reflections on the history and philosophy of experimental psychology. *Mind & Language, 17*(3), 207–232. https://doi.org/10.1111/1468-0017.00196

Hempel, C. G., & Oppenheim, P. (1948). Studies in the logic of explanation. *Philosophy of Science, 15*(2), 135–175. http://links.jstor.org/sici?sici=0031-8248%28194804%2915%3A2%3C135%3ASITLOE%3E2.0.CO%3B2-E

James, W. (1970). Humanism and truth. In *The meaning of truth: A sequel to pragmatism* (pp. 51–101). University Michigan Press. (Original work published 1909).

Kant, I. (2001). In M. P. Santos & A. F. Mourujão (Eds.), *Crítica da razão pura [Critique of Pure Reason]* (5th ed.). Fundação Calouste Gulbenkian. (Original work published 1781-1787).

Köhler, W. (1992). *Gestalt psychology: An introduction to new concepts in modern psychology*. Liveright. (Original work published 1949).

Laurenti, C., & Abib, J. A. D. (2005). Instrumentalismo científico e o modelo de seleção pelas consequências [Scientific instrumentalism and the model of selection by consequences]. In H. J. Guilhardi & N. C. Aguirre (Eds.), *Sobre Comportamento e Cognição* (Vol. 15, pp. 147–156). Esetec.

Latour, B. (2003). *Science in action: How to follow scientists and engineers through society* (11th ed.). Harvard University Press. (Original work published 1987).

Lopes, C. E. (2006). Uma proposta de definição de comportamento no behaviorismo radical [a proposal of definition of behavior in radical behaviorism]. *Revista Brasileira de Terapia Comportamental e Cognitiva, 10*(1), 1–13. http://pepsic.bvsalud.org/scielo.php?script=sci_arttext&pid=S1517-55452008000100002

Nagel, E. (1961). *The structure of science: Problems in the logic of scientific explanation*. Harcourt, Brace & World.

Pennypacker, H. S. (2004). Back to the future. *European Journal of Behavior Analysis, 15*(1), 71–76. https://doi.org/10.1080/15021149.2014.11434489

Rorty, R. (1979). *Philosophy and the mirror of nature*. Princeton University Press.

Rutherford, A. (2000). Radical behaviorism and psychology's public: B. F. Skinner in the popular press, 1934–1990. *History of Psychology, 3*, 371–395. https://doi.org/10.1037/1093-4510.3.4.371

Rutherford, A. (2004). A 'visible scientist': B. F. Skinner write for the popular press. *European Journal of Behavior Analysis, 5*, 109–120. https://doi.org/10.1080/15021149.2004.11434237

Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis*. Appleton-Century-Crofts.

Skinner, B. F. (1953). *Science and human behavior*. The MacMillan Company.

Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis*. Appleton-Century-Crofts.

Thyer, B. A. (2009). Epistemology: A behavior analytic perspective. *Humana.Mente Journal of Philosophical Studies, 11*, 45–63. https://www.humanamente.eu/index.php/HM/issue/view/11/issue_11

Tolman, E. C. (1964). Edward Chace Tolman. In E. G. Boring, H. S. Langfeld, H. Werner, & R. M. Yerkes (Eds.), *A history of psychology in autobiography* (Vol. 4, pp. 323–339). Russell & Russell. (Original work published 1952).

Ulman, J. D. (2004). The Ulman-Skinner letters. *European Journal of Behavior Analysis, 15*(1), 11–19. (Reprinted from *Behaviorology,* 1993). https://doi.org/10.1080/15021149.2014.11434469

Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20*(20), 158–177. https://doi.org/10.1037/h0074428

Wittgenstein, L. (2002). In D. F. Pears & B. F. McGuinness (Eds.), *Tractatus logico-philosophicus*. Routledge. (Original work published 1921).

Chapter 9
Theory: A Response to Lopes

John Staddon

I thank Carlos Lopes for his long commentary. But I find it difficult to respond because his stance is so abstract. Misled, perhaps, by my initial throwaway comment about "isms," he lists a dizzying number of them, "such as realism, constructionism, positivism, rationalism, naturalism, empiricism, pluralism, reductionism," to which the only response is "uncle"!

I regret my comment, but perhaps I can go some way to justifying it by pointing out that "isms" proliferate only when a science is still evolving. In a relatively mature science like physics, there is no "Newtonism" or even "Laplaceism" but just "physics." "Morganism" and "Weismannism" existed in biology only as long as the facts were uncertain. Now there is just "genetics." My comment looked optimistically toward the day when there will be no neo-behaviorism, new behaviorism, or cognitivism but just *psychology*.

Lopes writes: "[T]here does not exist science without theory," implying that the "scientific method" necessarily requires some kind of theory. I tend to agree: so how does this show up in Skinner' scientific work? As I point out in a book (Staddon, 2017), there really is no algorithmic "scientific method," but we can learn something about how science and scientists work through examples. Francisco Ayala (2009) agrees: "[T] here is no better way of understanding the basic components of the scientific method, and its variations in different disciplines and peculiarities in different practitioners, than examining the work of great scientists." Charles Darwin, for example, was always just curious. As he collected specimens as a young man on his 5-year voyage around the world on HMS *Beagle*, he was looking for relationships, oddities, anything that would catch his eye. He had no theory. Then, 1 day, just 2 years after the end of his voyage, "…it at once struck me that under these

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circumstances [A Malthusian struggle for existence] favorable variations would tend to be preserved, and unfavorable ones to be destroyed . . . Here then I had at last got a theory by which to work…." Darwin had found a *process*, natural selection, by which to make sense of his mountain of empirical data. His efforts from then on would be to test his hypothesis, to see if the facts of biology, geology, and paleontology fit with the idea of natural selection. This is the general method of science: first look around, play with this and that, then form a hypotheses, and then *test it.*

B. F. Skinner has provided examples of this view of scientific method. He describes in his wonderful "Case history" paper (Skinner, 1956) how, through a combination of trial, error, and accident, he came to invent the Skinner box and stumble upon a simple schedule of reinforcement. From this came many other schedules and Skinner's idea of *contingency of reinforcement.* Reacting to Darwin's claim that he "proceeded on true Baconian principles and without any theory collected facts on a wholesale scale," Ayala writes "The facts are very different from these claims, however," and indeed Darwin himself writes elsewhere "How odd it is that anyone should not see that observation must be for or against some view if it is to be of any service." In this sense, Skinner and indeed any creative scientist do have a theory or at least a point of view: having discovered schedules of reinforcement by trial and error, he was interested in the sensitivity of schedule behavior to identifiable *reinforcement contingencies.*

Out of this work arose the "three-term contingency," the idea that learned behavior (Term 1) is established by reinforcement (Term 2) and "controlled" by a discriminative stimulus (Term 3); the three-term contingency is a simple theory. Skinner defined a class of behavior he called *operants* (aka *habits*) that fit this scheme and another class, *respondents*) that did not. The next step should be *test*, and Skinner and his collaborators did test but in a way that was Baconian rather than anything we would call hypothesis testing. Having discovered the principle of a reinforcement schedule, much research in the years following was simply to try out a range of different schedules: first fixed then variable ratio, fixed and variable interval, spaced-responding (DRL) multiple and concurrent (choice) schedules, and the like, looking always to see what contingency intrinsic to the schedule was affecting performance.

But these experiments were much more in the nature of explorations than explicit tests of theory.[1] The rather disorganized book[2] that summarized the first effort of Harvard's "pigeon lab," *Schedules of Reinforcement*, makes it pretty clear that Ferster and Skinner were just trying stuff out. As the online description says: "the book illustrates the scientific philosophy that Skinner and Ferster adopted: that a science is best built from the ground up, from a firm foundation of facts that can eventually be summarized as scientific laws." They did indeed amass a fascinating

[1] Indeed, as I point out, the only paper of Skinner's that begins with a hypothesis is his "superstition" paper (Skinner, 1948; Staddon & Simmelhag, 1971).

[2] "[T]he impact of the work was diminished by the limited analysis and interpretation of the results and elucidation of their significance in *Schedules*," writes W. H. Morse and P. B. Dews in their 1997 Introduction to a reprint of the work.

trove of behavioral data, recorded in real time via the wonderful new invention of the cumulative recorder. They write about their method:

> A more general analysis is also possible which answers the question of why a given schedule generates a given performance. It is in one sense a theoretical analysis; but it is not theoretical in the sense of speculating about corresponding events *in some other universe of discourse*. It simply reduces a large number of performances generated by a large number of schedules to a formulation in terms of certain common features. It does this by a closer analysis of the actual contingencies of reinforcement prevailing under any given schedule. (Ferster & Skinner, 1957; emphasis added)

The field of exploration was limited to operant schedules, not behavior, including instinctive behavior, in general—in accordance with Skinner's early stricture against "botanizing of reflexes." This was probably a mistake, as the operant-respondent distinction broke down after a reanalysis of "superstition": the behavior of organisms cannot be so simply partitioned.

Lopes points out Skinner's comment that direct "observation, no matter how prolonged, tells him [a naïve viewer] very little about what is going on." But Skinner's allusion here is not so much to Darwin's "observation must be for or against some view…" as to the importance of *experimental manipulation*—in this case, in the form of reinforcement schedules.

Skinner was after laws, but the best we seem to have done, after the original law of effect, is R. J. Herrnstein's *matching law*, which tells us much more about the concurrent variable-interval schedule that exhibits it than the actual choice processes that might be supposed to underly it.[3]

Nevertheless, there are glimmers of theory in *Schedules*. Several figures in the book show cumulative records of various stages as a pigeon learns a fixed-interval schedule. Ferster and Skinner even summarize the pattern of transition: a naïve animal at first responds in a burst after each food delivery; this pattern gradually changes to a steady response rate; and finally a pause develops after reinforcement and the familiar "FI scallop" results (F & S, Fig. 117). But Ferster and Skinner went no further; they made no attempt to identify a dynamic process that might account for this pattern of temporal learning. Skinner had no interest in theory of this kind, and it was Machado in 1997 who proposed the first dynamic theory of the process (see also Staddon, 2020). The topic, and the treasure trove of real-time data in *Schedules*, has been neglected since.

An obstacle to the kind of theoretical account necessary to explain things like temporal learning (as I mention in the article) seems to have been Skinner's rejection of "any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions." Yet, he was more than willing to entertain the idea of a repertoire of latent responses, albeit only in connection with verbal behavior. And he also accepted the idea of "private events" albeit in a fashion that hinted at an inner homunculus for whom these events were stimuli (Baum,

[3] See Matching law for a succinct summary.

2011, while not quarreling with Skinner's epistemology, nevertheless considers private events "irrelevant to accounts of behavior").

But Skinner's idea of latent response does have a legacy in theoretical behaviorism, in the ideas of *state* and *repertoire.* Let's begin with the operant: a response-reinforcer contingency controlled by a stimulus class. Following the logic of historical systems (Staddon, 1967; Minsky, 1969), an operant, pecking a red key, for example, can be identified as a state of the (theoretical) organism: the stimulus is the red key and the response the peck. The *state* is the property or properties of the organism, established by a *history* of reinforcement, that allow that stimulus to affect that response. (Machado's FI-acquisition model involves a sequence of states.) But of course there are other states, other responses, and other histories. During discrimination training, the organism behaves in many ways—"emits many behaviors," if you prefer. These constitute the *repertoire* from which the effective response is finally selected. Skinner, original as always, came up with the idea of *operant level* as a label for the tendency of various different activities to occur in the absence of explicit reinforcement. If we accept his 1948 suggestion, these behaviors can be divided between the *active* response, which is occurring *now*, and an indefinite number of *latent* responses, which gain or lose strength relative to the active response as it is reinforced or not.

This picture, which is in many ways just Skinner's view, slightly updated, is also the view of theoretical behaviorism, hence my claim that ThB provides a "necessary amendment" to radical behaviorism.

References

Ayala, F. J. (2009). Darwin and the scientific method. *PNAS, 106*(suppl. 1), 10033–10039.

Baum, W. M. (2011). Behaviorism, private events, and the molar view of behavior. *The Behavior Analyst*, 34, 185–200.

Ferster, C. B., & Skinner, B. F. (1957/1997). *Schedules of reinforcement.* Prentice Hall/B. F. Skinner Foundation.

Machado, A. (1997). Learning the temporal dynamics of behavior. *Psychological Review, 104*(2), 241–265. https://doi.org/10.1037/0033-295x.104.2.241

Minsky, M. (1969). *Computation: Finite and infinite machines*. MIT Press.

Skinner, B. F. (1948). "Superstition" in the pigeon. *Journal of Experimental Psychology*, 38, 168–172.

Skinner, B. F. "Superstition" in the pigeon. *Journal of Experimental Psychology*, 1948, 38, 168–172.

Skinner, B. F. (1956). A case history in scientific method. *American Psychologist, 11*(5), 221–233.

Staddon, J. E. R., & Simmelhag, V. L. (1971). The "superstition" experiment: a reexamination of its implications for the principles of adaptive behavior. *Psychological Review*, 78, 03–43.

Staddon, J. E. R. (2020). The dynamics of behavior: Review of Sutton and Barto: Reinforcement learning: An introduction (2nd ed.). *Journal of the Experimental Analysis of Behavior, 113*, 485–491.

Staddon, J. (2017). *Scientific method: How science works, fails to work or pretends to work*. Taylor and Francis.

Staddon, J. E. R. (1967). Asymptotic behavior: The concept of the operant. *Psychological Review, 74*, 377–391.

Part IV
Biological Behaviorism

Chapter 10
Biological Behaviorism

John W. Donahoe

Biographical Sketch

My path in science owes much to chance, as is true of most lives if one is honest. While in my third undergraduate year at Rutgers University studying chemistry, I happened to take a course in physiological psychology taught by Daniel Lehrman. He was at that time a new assistant professor, but Lehrman ultimately became a major figure in his field as indicated by his membership in the National Academy of Sciences and editorship of the *Journal of Comparative and Physiological Psychology*. He was a charismatic lecturer who occasionally led birding expeditions to a nearby park before beginning class. Lehrman introduced me to a field that combined my interest in science with a more fascinating subject matter—behavior. When I told Lehrman that I intended to pursue a doctorate in psychology, his only advice was to avoid departments influenced by the psychology of Clark Hull at Yale. At that time, Hull was a dominant figure in the field of learning, and learning was the dominant field in psychology.

Financial considerations were of overriding importance in my choice of a graduate program. This was the period before the launch of Sputnik goaded the government into supporting science education. I therefore restricted my search to accredited programs with low tuition and inexpensive housing. Although I was accepted into graduate programs that were generally regarded as stronger academically, I chose the University of Kentucky because it most closely met my financial criteria. Upon arrival, however, I discovered that the department was heavily influenced by Hullian psychology and that, in fact, the chairperson James Calvin had received his doctorate with Hull! So much for Lehrman's advice.

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Fortunately, there was also a new Columbia-educated "Skinnerian" on the faculty, Ernest Meyers. The following incident provides a sense of the dominance of Hullian psychology at that time: I had enrolled in a course entitled "theories of learning" that was taught jointly by Calvin and Meyers. Meyers had defined reinforcement empirically—that is, as a process whereby a stimulus that followed a response increased the frequency of that response on subsequent occasions. Calvin objected to this use of the term, insisting that the term "reinforcement" should be reserved for those events that produced "drive reduction," which was Hull's definition. In truth, Hull's view could not claim temporal priority let alone logical priority: Skinner's major work *The Behavior of Organisms* (1938) had predated Hull's corresponding work, *Principles of Behavior* (1943). Despite the orientation of the program, my graduate education was excellent in terms of "book learning," albeit deficient in laboratory experience.

During the summer following my third year in graduate school, Ernest Meyers died unexpectedly of a heart attack. There was not enough time to recruit an external replacement so I was invited to assume Meyers' teaching responsibilities while completing my dissertation. I completed graduate school at the end of that year (1958) with a degree from the Thomas Hunt Morgan School of Biological Sciences in experimental psychology with a secondary field in neurophysiology. Upon receiving the doctorate, I was appointed an assistant professor at the University of Kentucky and developed two laboratories—one in human learning, the area of my doctoral dissertation (Donahoe, 1960), and the other in animal learning. The human laboratory was housed in the psychology building and the animal laboratory in the biology building. The fact that the laboratories were in different locations turned out to be fortunate: The psychology building was soon burnt to the ground by an arsonist who was subsequently arrested when she attempted to do the same to another wooden structure, this one housing the French department. It was through this scientifically irrelevant event that I came to focus my research on animal learning.

Several other events, also largely beyond my control, contributed to the direction of my work. First, the analysis of my dissertation data required me to learn to program a mainframe digital computer, a resource that had just become available to university researchers. As a result, I was offered a joint appointment in the computing center where I acquired further skills that proved essential to my later work writing computer programs for operant experiments and for neural-network simulations with José Burgos (e.g., Donahoe et al., 1993).

Second, the Psychology Department recruited Fogle Clark, a behavior analyst whose dissertation had appeared in the first volume of the *Journal of the Experimental Analysis of Behavior* (Clark, 1958). He introduced me to the wonders of electromechanical switching circuits, which were then used for research with operant chambers. I had previously used T-mazes where I was routinely bitten by hooded rats whose superior vision was required by the nature of the project. With operant chambers, I could both minimize the chance of being bitten and, more importantly, collect data under better controlled circumstances. In addition to our separate research programs, Clark and I worked together in applied research in which we trained chimpanzees on tasks that could be performed when they were launched into space. We became involved in this work by chance: A faculty member in engineering, Karl von Lange (a former Prussian pilot with the requisite saber slash on his cheek), was a liaison between the National Aeronautical and Space Administration (NASA) and German engineers, such as Werner von Braun, who had been captured for the US rocket program..

A third event that affected the trajectory of my research occurred during a sabbatical year at the Center for Brain Research at the University of Rochester. While there, I gained further experience in neuroscience, especially electrophysiology. Paradoxically, the effect of this increased knowledge was to convince me that neuroscience was not yet sufficiently developed to make integration with behavior analysis profitable. The situation is now quite different (Donahoe, 2017).

A fourth and final fortuitous event occurred when I began to search for a different academic position. After declining an offer from another university the preceding year, I received a telephone call from my former doctoral student John Ayres (1968) informing me that the University of Massachusetts was seeking a faculty member with experience in my field. After a visit, they offered me a position in what is now the Department of Psychological and Brain Sciences where I remain as an emeritus professor.

These events guided the direction of my research together with the further good fortune of working with talented graduate students such as David Palmer (1988) who encouraged me to consider more fully the implications of basic principles for the interpretation of complex human behavior (Donahoe & Palmer, 1994/2017). My personal contributions are largely captured by Pasteur's words: "Chance favors the prepared mind" (Pasteur, 1854).

Before proceeding to the main content of the chapter, two preliminaries are in order. First, the modifier "biological" in the title of the chapter is superfluous. From the outset, behavior analysis was conceived by Skinner as "a rigorous, extensive, and rapidly advancing branch of biology" (Skinner, 1974, p. 255; cf. Skinner, 1935). Thus, behavior analysis was defined as a biological science from its inception. This stance is not surprising given that Skinner's chief mentor at Harvard was the biologist William Crozier. Second, unlike perhaps some of the other contributions to this volume, the present chapter should not be seen as a change in or a replacement of the scientific agenda envisioned by Skinner. Instead, it is a continuation of Skinner's agenda in light of recent findings in its sister discipline neuroscience. In an interview with Skinner conducted shortly before his death, Margaret Vaughan (personal communication) told me that among the future developments Skinner most fondly anticipated was uncovering the neural basis of conditioning.

The remainder of the chapter is concerned with (1) describing the type of neuroscience with which behavior analysis may have a productive relation, (2) presenting findings regarding the behavioral and neural processes of reinforcement, and (3) interpreting complex behavior through the integration of behavior analysis and neuroscience.

Prerequisites for the Integration of Behavior Analysis and Neuroscience

Skinner famously warned against potential dangers in efforts to integrate behavior analysis and neuroscience. He illustrated these dangers using the example of the decrement in responding seen in the Pavlovian conditioned-inhibition procedure, which some attributed to neural inhibition (Skinner, 1938, p. 422). Skinner argued that the observed decrement could be accommodated by the behavioral process of stimulus discrimination without an appeal to unobserved neural processes. (See Donahoe and Palmer, 1988, for a more extended treatment of the concept of inhibition.) Extrapolating from this example, Skinner drew a more general conclusion: Behavior analysis, as an independent science, need not seek explanations of behavior by moving to other levels of analysis.

The issue of "levels of explanation" is examined here using a somewhat more contemporary example. Donald Hebb proposed the following as an account of the process whereby a stimulus acquired control over behavior: "When an axon of cell *A* is near enough to excite a cell *B* and repeatedly or persistently takes part in firing it, some growth process or metabolic change takes place in one or both cells such that *A*'s efficiency, as one of the cells firing *B*, is increased" (Hebb, 1949). In more technical language, if a presynaptic neuron releases into the synaptic cleft an excitatory neurotransmitter that binds with appropriate receptors on a postsynaptic neuron and that neuron fires, then the ability of the presynaptic neuron to fire the postsynaptic neuron will increase. In short, the synaptic efficacy between the two neurons increases.

Let us examine this account to determine what addition, if any, it makes to understanding the behavioral observation that, under appropriate circumstances, temporal contiguity between a stimulus and behavior increases the ability of that stimulus to control that behavior. When evaluating any claim, it is useful to consider the status of the negation of that claim. If the negation of a claim cannot be true, then its assertion is vacuous: The claim is a truism and its assertion adds nothing to knowledge. With respect to the Hebbian claim, because changes in the stimulus control of behavior cannot possibly occur without *some* change in synaptic efficacies between neurons, the Hebbian proposal is simply a restatement in neural terms of the behavioral finding that stimulus-behavior contiguity is required for learning. The Hebbian proposal has the appearance of a contribution because it is stated in terms of physical processes (neurons and neural activity). However, the nature of those processes is unspecified, and, as such, the proposal adds little to the behavioral finding. At most, restating the behavioral observations in terms of unobserved neural processes serves as an impetus to seek those processes.

None of this is to say that direct knowledge of the neural activity mediating environment-behavior relations cannot contribute to understanding behavior—and Skinner acknowledged as much:

> A science of the nervous system will someday start from the *direct observation* (emphasis added) of neural processes. It is with such a science that the neurological point of view must

> be concerned if it is to offer a convincing 'explanation' of behavior. The correlation demanded as an explanation is with a science of neurology which completes its local references and devises techniques for the direct observation of synaptic and other processes. I am not overlooking the advance that is made in the unification of knowledge when terms at one level of analysis are defined ("explained") at a lower level. (Skinner, 1938, p. 422).

The Experimental Analysis of Reinforcement

Integrating behavior analysis and neuroscience provides a benefit that potentially parallels the earlier benefit of integrating evolution-through-natural selection and heredity (e.g., Dobzhansky, 1937, cf. Donahoe, 2003). Of this earlier synthesis, the biologist and philosopher of science Paul Gayon wrote, "If there is a key event in the history of Darwinism, it must be its confrontation with Mendelism. It was through its contact with the new science of heredity that the theory of selection became truly intelligible" (Gayon, 1998, p. 253). It was not until the genetic mechanisms of heredity were uncovered that natural selection became widely accepted within biology. A comparable situation now obtains within psychology: Although behavior analysis regards selection by reinforcement as providing the central insight into the learning of individual organisms, this view is not shared by most psychologists. If the historical parallel holds, the triumph of reinforcement awaits the discovery of its neural mechanisms. As it was with evolution through natural selection and genetics, so it may well be with selection by reinforcement and its neural mechanisms.

Behavioral Analysis of Reinforcement

Before a fruitful integration of behavior analysis and neuroscience can be achieved, an experimental analysis of reinforcement must take place within each discipline as Skinner had foreseen. In this section, the basic findings from behavior research are described, and their implications summarized. More detailed accounts are provided elsewhere (Donahoe, 2017; Donahoe et al., 1982; Donahoe & Palmer, 1994; Donahoe et al., 1993).

Two general procedures for the study of conditioning are used—Pavlovian (or respondent) conditioning and operant (or instrumental) conditioning. In the Pavlovian procedure, the experimenter arranges for an arbitrary *stimulus* to precede the reinforcing stimulus (reinforcer). In the operant procedure, an arbitrary *response* precedes the reinforcer. Although the difference in procedure instituted by the experimenter is clear, the difference in the sequence of events sensed by the organism is not. Figure 10.1 shows the events and their relation to the organism in the two procedures.

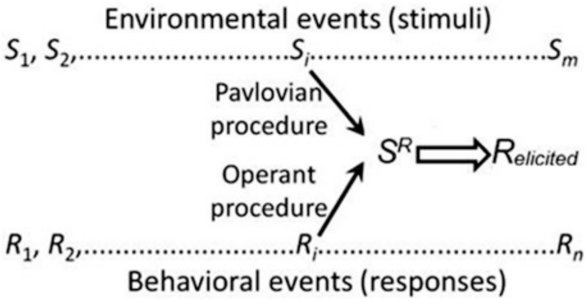

Fig. 10.1 Diagram illustrating the similarity and difference between the Pavlovian and operant procedures. In both procedures, a reinforcing stimulus (S^R) is introduced that elicits a response ($R_{elicited}$). In the Pavlovian procedure, S^R follows a specific stimulus, whereas in the operant procedure S^R follows a specific response (R_j). $R_{elicited}$ typically goes unmeasured in the operant procedure

In the Pavlovian procedure, a stimulus (S_i) precedes the reinforcer (S^R), and S_i then comes to evoke a conditioned response that in the prototypic case resembles the response elicited by the reinforcer ($R_{elicited}$). In the operant procedure, a response (R_j) precedes the reinforcer, and the operant increases in frequency. Note, however, that in the Pavlovian procedure *some* response necessarily comes before the reinforcer: Organisms are always behaving. The nature of the preceding response is, however, not controlled by the experimenter and, as such, may vary on different occasions. Similarly, in the operant procedure, *some* stimulus necessarily occurs before the operant and, therefore, before the reinforcer as well: Organisms are always sensing. This last realization comports with Skinner's observation that "discriminative stimuli are practically inevitable after [operant] conditioning" (Skinner, 1938, p. 273; cf. Donahoe, Palmer, & Burgos, 1997). In short, on every instance in which a reinforcer occurs, some stimulus and some response precede the reinforcer in both the Pavlovian and operant procedures. The difference in the procedures is not the type of event that precedes the reinforcer but the *reliability* with which a particular stimulus or a particular response precedes the reinforcer. Thus, at the *moment of reinforcement*, the organism cannot tell whether it is in a Pavlovian or an operant procedure! One implication of this insight is that the same reinforcement process must be operative in both procedures but that process must be competent to produce the very different outcomes that emerge from the different contingencies instituted by the two procedures.

Two variables have been identified as critical for the reinforcement process—temporal contiguity and behavioral discrepancy. Temporal contiguity refers to the time interval between the stimulus and response events that precede the reinforcer. The importance of temporal contiguity has long been known (e.g., Gormezano & Kehoe, 1981). In the Pavlovian procedure, the stimulus—technically, the conditioned stimulus (CS)—may precede the reinforcer by no more than a few seconds if the CS is to acquire control over the conditioned response (CR). Instances in which conditioning appears to occur over longer time intervals are treated elsewhere (e.g., Donahoe, 2017, p. 304, footnote 2). Similarly, in the operant procedure, the temporal interval between the operant and the reinforcer must also be brief if other sources of reinforcement are minimized during the response-reinforcer interval (cf. Skinner, 1938, p.53).

The second variable—behavioral discrepancy—was identified more recently. Experiments with the Pavlovian procedure and the operant procedure both indicate that, although temporal contiguity is necessary to engage the reinforcement process, it is not sufficient. In the Pavlovian procedure, if a stimulus reliably precedes a reinforcer and—as a result—already controls the CR when a second stimulus is introduced simultaneously with the first stimulus, then the second stimulus does not become a CS (Kamin, 1969). This is in spite of the fact that the second stimulus is in a temporal relation with the reinforcer that otherwise supports conditioning. Similarly, in the operant procedure if an operant is already controlled by a discriminative stimulus, and a second stimulus is then simultaneously introduced, the second stimulus does not acquire control of the operant. In summary, prior conditioning *blocks* the acquisition of a new environment-behavior relation if the same reinforcer has previously occurred in that context.

What factor is missing when the effect of a putative reinforcer is blocked by prior conditioning? To answer this question, it is necessary to unconfound the two aspects of a reinforcing event—its stimulus properties (S^R) and its elicited response properties ($R_{elicited}$). A Pavlovian procedure was devised with the rabbit using a mild electric shock to the vicinity of the eye as the eliciting stimulus and movement of the nictitating membrane as the elicited response. (The nictitating membrane, sometimes called the third eyelid, can be extended over the cornea and is present in many animals, although vestigial in humans). In the rabbit, the movement of the nictitating membrane occurs independently in the two eyes, which permits the intensity of the eliciting stimulus to remain constant but the elicited response to vary as a function of the eye region to which the shock is applied. Using a tone and a light as CSs, the study found that blocking of conditioning was eliminated if the shock was shifted to the other eye when the second stimulus was introduced (Stickney & Donahoe, 1983; cf., Burns et al., 2011). In summary, changing the elicited response eliminated the blocking of conditioning.

Subsequent research confirmed and extended these findings (Betts et al., 1996; Brandon et al., 1994). Again using the rabbit as a subject, changing the eye region to which the shock was applied when the second stimulus was introduced eliminated blocking. However, a conditioned autonomic response that did not change with a change in the shocked-eye region remained blocked. Thus, in the same animal, a change in the elicited membrane response permitted conditioning of the second stimulus, but the absence of a change in the autonomic response did not. These findings point toward a change in the *response* produced by the reinforcing stimulus as central to the reinforcement process and not the reception of the eliciting stimulus per se

The foregoing implication requires a reappraisal of the conventional account of Pavlovian conditioning that emphasizes the importance of the temporal relation between the CS and the reinforcing stimulus (the US). The conventional account is largely based on procedures in which the reinforcing stimulus and the reinforcer-elicited response occur close together in time. Under such circumstances, the effect of the CS-S^R relation cannot be separated from that of the CS-$R_{elicited}$ relation. The CS-S^R and the CS-$R_{elecited}$ temporal relations are completely confounded.

To unconfound these temporal relations, a Pavlovian procedure was devised in which the delay between the S^R and the $R_{elicited}$ was several hundred milliseconds (ms) and the duration of $R_{elicited}$ was increased to several thousand ms. These requirements were met by a procedure in which a water S^R was injected into the oral cavity of a restrained pigeon, which elicited a somewhat delayed and temporally extended swallowing response. This procedure allowed the CS to be introduced at either of three times (a) before the reinforcing stimulus (the typical forward CS arrangement), (b) after the CS but before the elicited response, or (c) after the onset but during the elicited response. Using different subjects with each of these temporal arrangements, conditioning occurred equally in all three conditions as long as the CS preceded some portion of the temporally extended swallowing response (Donahoe & Vegas, 2004). Thus, the temporal relation of the CS to the *elicited response*, and not to the reinforcing stimulus, is the critical temporal relation. The conclusion from this work is that the second factor, in addition to temporal contiguity, is a reinforcer-induced change in ongoing behavior, i.e., a behavioral discrepancy.

Unified Reinforcement Principle

Based on the preceding work, stimuli present *at the moment* a behavioral discrepancy occurs gain control over whatever responses are present in that temporal vicinity. This conception of the reinforcement process is called the *unified reinforcement principle* (Donahoe et al., 1982, 1993). It is unified in the sense that it applies equally to the Pavlovian and operant procedures. In the Pavlovian procedure, the stimuli are the CS in the context in which it appears, and the response is the reinforcer-elicited response. In the operant procedure, the situation is more complex: The stimuli are the discriminated stimulus in the context in which it appears, but there are multiple temporally proximate responses—the operant, the elicited response, and, as conditioning proceeds, the conditioned response itself. The net outcome of the operant procedure depends on interactions, if any, between the operant (R) and the elicited response ($R_{elicited}$) together with its conditioned expression (CR).

The potential complexity of the outcome of the operant procedure is illustrated by the phenomenon of autoshaping. In autoshaping, a pigeon is presented with an intermittently illuminated response key followed by the response-independent occurrence of grain. If the pigeon pecks the illuminated key, the occurrence of grain is immediate. With this procedure, the pigeon acquires the key-pecking response very rapidly, and the topography of the key-peck closely resembles the food-pecking response—the beak opens in both cases as if grasping grain (Jenkins & Moore, 1973). In autoshaping, the reinforcer-elicited response and the operant response are highly compatible (both involve pecking a localized visual stimulus), and facilitation of key pecking occurs. Contrast this outcome with that of the negative-automaintenance procedure. In negative automaintenance, the key light also precedes presentation of grain, but now a key peck *prevents* the scheduled occurrence of grain. Under this procedure, not-key pecking is incompatible with grain-elicited pecking. Disk pecking now fluctuates between periods when it occurs (generally following trials without a key peck when grain was presented) and when it does not occur (generally following trials in which a key peck did occur and grain was omitted) (Williams & Williams, 1969).

Implications of the Unified Reinforcement Principle for the Temporal Relation of the CS or the Discriminative Stimulus to the S^R-Elicited Response

As previously described**,** research indicates that the reinforcement process is instigated by a behavioral discrepancy (i.e., a change in ongoing behavior) and not by the mere occurrence of a stimulus that occasions the change. Here, we identify some implications for conditioning of this distinction. In a Pavlovian backward-conditioning procedure in which the eliciting stimulus occurs before the CS, the unified principle indicates that acquisition should not take place with short-latency, short-duration elicited responses (e.g., skeletal responses such as shock-elicited eye blinks). In this case, $R_{elicited}$ precedes the CS. However, conditioning should occur during a backward-conditioning procedure with a longer-latency, longer-duration elicited responses (e.g., autonomic responses such as shock-elicited increases in heart rate.) Here, a “backward” CS precedes or overlaps the elicited response. The temporal relation of the CS to the behavioral discrepancy and not to the eliciting stimulus is what determines the outcome of a backward-conditioning procedure. Accordingly, backward-conditioning procedures produce different effects on different response systems (e.g., Betts et al., 1996; Schneiderman, 1972; Tait & Saladin, 1986; Tait, Quesnel, & ten Have, 1987; McNish et al., 1997).

In addition to providing an account of otherwise puzzling differences in the effects of backward-conditioning procedures, the unified principle is useful in interpreting the conditioning of *behavior systems* (Silva et al., 1998). As an example, a stimulus such as food occasions a temporally extended *sequence* of responses and not a single response. In an enclosure in which food is provided to a rat, the first response is consuming the food followed by searching in that area for more food (focal search) and lastly searching more widely around the space (foraging). If food (S^R) is delivered into a food cup and a CS is presented to different subjects during only one component of the behavior system, then the CS evokes only that component of the behavior system with which it was paired. Note that this is a backward CS-US procedure but a forward CS-$R_{elicyed}$ procedure for the focal search and foraging components of the behavior system. In the conventional account, the conditioning of behavior systems implies that special conditioning principles may be required for behavior systems. However, if contiguity of the CS with a behavioral discrepancy—and not the reinforcing stimulus—produces conditioning, then these findings are not problematic.

Implications of the Unified Reinforcement Principle for Operant Conditioning

As already noted**,** the outcome of the operant procedure depends on the interaction of the operant with the elicited response and with the conditioned response. A straightforward implication of the unified principle is its treatment of punishment. In punishment, some ongoing operant is followed by an aversive stimulus, and, as a result, the operant declines in frequency. Consider a case in which lever pressing for food is subsequently also followed by shock. The behavior elicited by shock is freezing or withdrawing from the location where shock occurred. Conditioning of this shock-elicited behavior competes with lever pressing and reduces its frequency. The learner cannot simultaneously press the lever while freezing or withdrawing from the region containing the lever.

A less obvious implication of the unified principle for operant conditioning also depends on an interaction of the operant with the elicited response and the conditioned response. If the reinforcement process is initiated by the occurrence of a behavioral discrepancy, then the most temporally proximate response is the one that produces the discrepancy (i.e., the reinforcer-elicited response). The operant necessarily precedes the discrepancy by at least a brief time interval and is therefore more temporally distant. For example, suppose an experimenter arranges for lever pressing in the presence of a tone to produce food. Under this arrangement, the consummatory response (which demarks the behavioral discrepancy) is the most proximate response, and the operant (lever pressing) slightly precedes the discrepancy. Because the consummatory response is temporally closer to the discrepancy, it should be acquired slightly before the operant. As a consequence, if the CR provides stimuli to the organism, then these feedback stimuli are in a temporal position to share control over the operant with the external discriminative stimulus.

The joint control of operants by environmental stimuli and feedback stimuli arising from the CR has several effects, one of which is *revaluation*: Revaluation refers to the finding that the strength of an operant can change by virtue of manipulations involving the reinforcer that take place *outside* the conditioning procedure. Consider the following example: Lever pressing was first acquired with food as a reinforcer. Then, food was paired with shock when the lever was not present. When the lever was later reintroduced, lever pressing was reduced below its previous level. This reduction occurred even though a lever press had never been followed by shock (Colwill & Rescorla, 1985; see also Holland & Rescorla, 1975). Neural-network research that implemented the unified reinforcement principle with feedback stimuli from the conditioned response has simulated revaluation (Donahoe & Burgos, 2000).

Implication of the Unified Reinforcement Principle for Blocking

The unified principle attributes blocking to a reduction in the behavioral discrepancy that occurs when the CR precedes $R_{elicited}$. That is, the CR-$R_{elicited}$ discrepancy is less than the behavioral change produced when $R_{elicited}$ occurs alone. Research using the nictitating-membrane preparation of the rabbit shows that not only is the behavioral discrepancy smaller because of the prior occurrence of the CR but also $R_{elicited}$ is a reduced after CS presentations in which a CR occurred. On those occasional CS presentations when a CR does not occur, $R_{elicited}$ is not reduced (Canli, et al. 1992).[1]

Neuroscientific Analysis of Reinforcement

What mechanisms did evolution devise whereby the strengths of connections between neurons mediating *specific* reinforced environment-behavior (E-B) relations were modified? And are those mechanisms consistent with the behaviorally based, unified reinforcement principle? Those mechanisms must have the effect of strengthening connections along the "right" neural pathways, that is, those

[1] Two general points should be made about the present approach to the reinforcement process. First, as stated earlier, this account is consistent with Skinner's view that reinforcement operates on a moment-to-moment basis involving contiguous stimuli, responses, and reinforcers. Empirical relations observed between variables defined over more extended periods of time, that is, molar variables (e.g., Baum, 1973, 2001, 2011) may be useful for some purposes but are regarded as the cumulative effects of more fundamental, momentary processes. As an example, Skinner's interpretation of concurrent performance has received support from work showing that the molar matching relation does not occur unless switching is explicitly reinforced (Crowley & Donahoe, 2004) and that relative responding to an alternative can be manipulated by the differential reinforcement of responding to an alternative versus changing to another alternative (MacDonall, 2009). (For a discussion of this issue, see Donahoe, 2012). Indeed, a great number of molar relations are consistent with computer simulations using a genetic algorithm implementing a moment-to-moment reinforcement process (McDowell, 2013). Whether a genetic algorithm is the most behaviorally and neutrally faithful means of demonstrating the cumulative effect of a moment-to-moment reinforcement process is a separate matter. Simulations implementing stimulus-sampling theory, an approach developed by Skinner's former student William Estes, or adaptive neural networks if they incorporate a discrepancy-based reinforcement principle (Gluck, 1992; Donahoe et al., 1993) are more in keeping with the experimental analysis of behavior. Second, the chapter makes use, in part, of findings from the laboratories of Robert Rescorla and, especially, Alan Wagner and his associates. However, the present account differs from that of these researchers: Their findings are cast within the conceptual framework of association theory. That is, the findings provide the basis for *inferences* about the nature of associative processes that are assumed to underlie the behavioral observations. Inferred-process theories of this sort are inconsistent with a behavior-analytic approach on several grounds (cf. Skinner, 1950). These include the fact that behavioral observations do not sufficiently constrain inferences about underlying processes: A given behavioral finding may result from any of a large number of underlying processes. In accord with Skinner's view, if underlying processes are to be considered, then these too must be the product of experimental analysis and not inferences alone.

pathways that mediate the behavior temporally proximate to the reinforcer. A further requirement is that these same neural mechanisms must also be capable of affecting any of a large number of different pathways mediating other E-B relations when there are *different contingencies of reinforcement*. Even a relatively simple behavior such as lever pressing can be controlled by many different stimuli. Also, those same stimuli may control behavior other than lever pressing such as turning right or left in a T-maze. What are the neural mechanisms that permit reinforcers to affect a specific set of neural connections while simultaneously having the capability of affecting a wide range of different environment-behavior (E-B) relations under different reinforcement contingencies?

Dopamine and the Selection of Pathways Mediating Reinforced E-B Relations

Biologically important events such as food, water, and sexual stimulation ultimately activate neurons in the ventral tegmental area (VTA). The neural capability for widespread effects of reinforcement is enabled by the liberation and diffusion of the neuromodulator dopamine (DA) along axons that project from cells in the VTA to the prefrontal and motor cortices (among others). See Fig. 10.2. The diffusion of DA allows a reinforcer to affect a wide range of synapses and, therefore, a wide range of potential E-B relations that are mediated by pathways involving those synapses. The neural inputs from sensory areas to neurons in the prefrontal and motor cortices control behavior.

DA diffuses from varicosities along VTA axons and remains present for several seconds before being degraded (Yagishita et al., 2014). The relatively short-lived presence of DA is consistent with the contiguity requirement. DA also plays a critical role in the discrepancy requirement as noted shortly.

Figure 10.3 indicates the frequency of firing of VTA-DA neurons during an experiment with a monkey. The task was a differential conditioning procedure in which pressing one of two levers was reinforced with apple juice depending on which of two spatially separated lights was illuminated (Schultz et al., 1993, 1997). The top panel (A) shows the frequency of firing of DA neurons when the reinforcing stimulus (S^R) was presented before conditioning had occurred. Note that the baseline level of activity of DA neurons abruptly increased when the reinforcer was presented. Transition to the "bursting" mode is required for VTA neurons to liberate DA along their axons (Grace & Bunney, 1984; Grace et al., 2007; Johnson et al., 1992).

The middle panel (B) shows the frequency of firing of DA neurons after conditioning when the discriminative stimulus (S^D) was presented and a correct response was followed by S^R. The burst of firing now occurred at the onset of S^D, not S^R. This is consistent with the long-standing behavioral finding that discriminative stimuli also acquire a conditioned reinforcing function (Dinsmoor, 1950; cf. Williams,

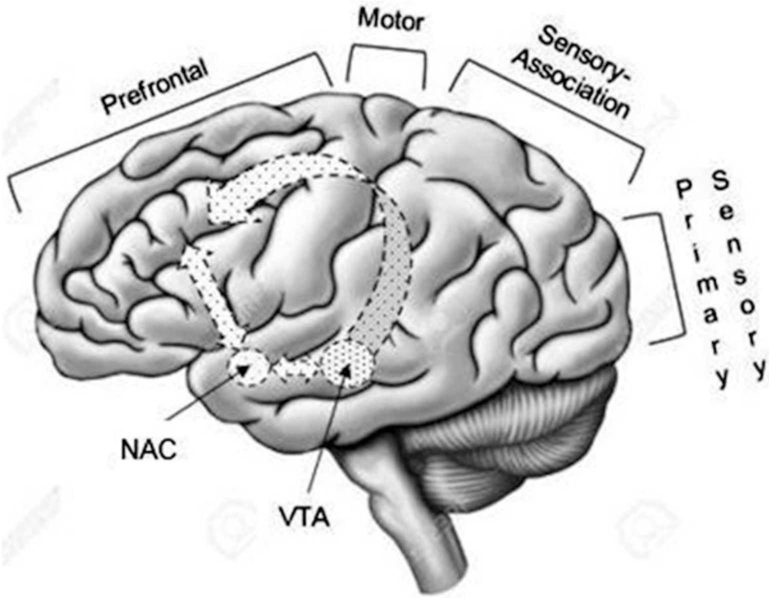

Fig. 10.2 Lateral view of the human cerebral cortex showing the major cortical regions and, using dashed lines, the subcortical structures—nucleus accumbens (NAC) and ventral tegmental area (VTA)—and pathways central to the neural mechanisms of selection by reinforcement. The divisions of the cortex designated in the diagram are for heuristic purposes only. For example, the region designated primary sensory cortex is largely the primary visual cortex, and the region designated sensory-association cortex also includes the auditory cortex of the temporal lobe

1994a, b). Thus, neuroscience indicates that both unconditioned and conditioned reinforcers liberate DA by VTA neurons. The route whereby VTA neurons are activated by conditioned reinforcers differs however. Whereas unconditioned reinforcers activate VTA neurons by pathways originating from receptors such as those for taste or sexual stimulation (e.g., Smith et al., 1996; Balfour et al., 2004), conditioned reinforcers activate VTA neurons by a less direct route involving the prefrontal cortex and the nucleus accumbens (NAC) (e.g., Pears et al., 2003; Wilson & Bowman, 2004). Based on the available evidence, the following account has been proposed (Donahoe, 2017): Evolutionary important stimuli, such as food and sex, stimulate VTA neurons by relatively direct sensory paths. This stimulation overcomes the effect of inhibitory neurons (neurons which liberate the neurotransmitter gamma-aminobutyric acid, or GABA) on VTA-DA neurons. This tonic inhibition otherwise prevents VTA-DA neurons from firing at the higher rates required for DA release. Conditioned reinforcers, such as stimuli that have been paired with food, activate excitatory neurons that arise from the prefrontal cortex and innervate neurons in the nucleus accumbens (NAC). These NAC neurons, in turn, have the effect of inhibiting the firing of the inhibitory neurons that reduce the activity of VTA-DA neurons. With the brief lifting of tonic inhibition, conditioned reinforcers allow VTA-DA neurons to fire at the higher rates needed for the liberation of DA. The net

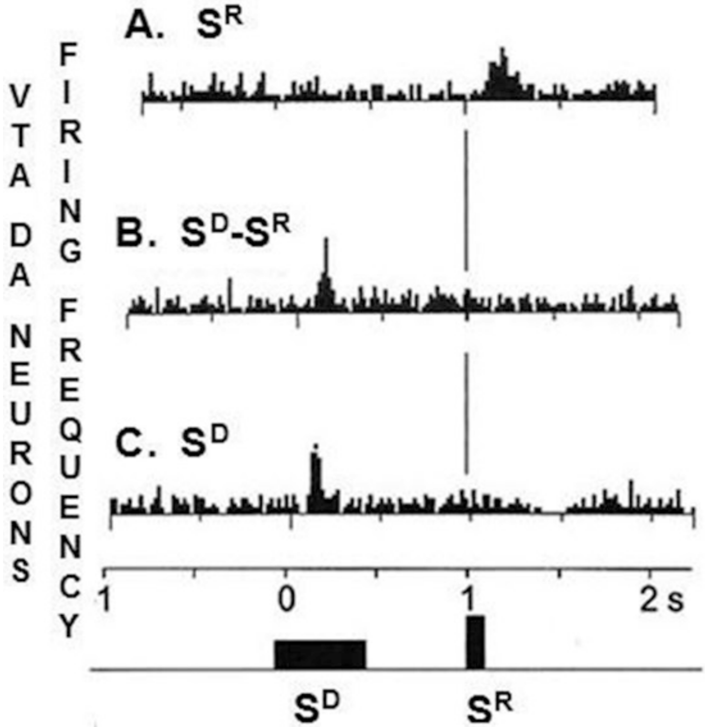

Fig. 10.3 The frequency of firing of dopaminergic (DA) neurons in the ventral tegmental area (VTA) during a differential operant-conditioning procedure. Panel A shows the DA response to the reinforcing stimulus (S^R) at the outset of conditioning. Panel B shows the DA response to the discriminative stimulus (S^D) and S^R on a reinforced trial after conditioning. Panel C shows the DA response to the S^D on a trial after conditioning on which the reinforcer was omitted (Data from Schultz et al., 1997; Schultz et al., 1993)

effect is that both conditioned and unconditioned reinforcers ultimately engage the same VTA reinforcement system but by means of different routes.

For experienced organisms in which many E-B relations have been acquired, the environment offers many opportunities for engaging the neural mechanisms of conditioned reinforcement. In this way, activities that require temporally extended sequences of behavior, such as problem-solving, are maintained by a relatively continuous stream of conditioned reinforcement, which increases as the target behavior is more closely approximated (Donahoe & Palmer, 1994/2017, p. 285 ff.). Research has shown that networks of neurons consisting of many cells can be simultaneously modified by the liberation and diffusion of DA (Athalye et al., 2018).

Panel C of Fig. 10.3 illustrates an additional finding that relates to the behavioral-discrepancy requirement. The lower panel shows the activity of DA neurons after conditioning on a trial in which S^D was presented but S^R was omitted. Note that DA activity was *inhibited* during the time when the reinforcer was scheduled to occur. If another stimulus had accompanied this S^D in a blocking design, then the reinforcer would not produce an increase in DA activity and that stimulus would not become a discriminative stimulus. The inhibition of VTA-DA neurons following the S^D is possibly due to the reinstatement of tonic inhibition following its prior suppression by the conditioned reinforcer. The inhibition of VTA-DA activity following the discriminative stimulus is the neural basis of the behavioral discrepancy requirement (Burgos & Donahoe, 2016; Donahoe et al., 1993; Waelti et al., 2001).

Specificity of the Neural Effects of Reinforcement: Long-Term Potentiation

Because of the widespread diffusion of DA in the frontal lobes, reinforcers can potentially affect a wide range of reinforced E-B relations. The specificity of action of DA to only those neurons along pathways mediating a particular reinforced E-B relation is achieved through the process of *long-term potentiation* (LTP). A given neuron has a great many receptors located within its cell membrane. For example, a single motor neuron may have thousands of receptors. When the excitatory neurotransmitter glutamate (GLU) is liberated into the synaptic cleft, it binds to receptors on adjacent neurons and may therefore activate ("fire") those neurons. In order for an E-B relation to occur, all the neurons along the pathway leading from the stimulus to the behavior must be activated. When a neuron fires, the receptors that caused the firing are thought to receive a molecular tag that persists for perhaps an hour. If DA is also present at the same time that the neuron fires, then a sequence of intracellular events takes place that permanently changes the tagged receptors and *only* the tagged receptors (Frey, 1997). Those receptors then become more responsive to the excitatory transmitter GLU. That is, they undergo LTP. It is through the process of LTP that the widespread diffusion of DA has an effect that is specific to the reinforced E-B relation. DA permanently changes only those receptors that fired the neuron, a neuron that is along the pathway that mediated the reinforced behavior. (See Donahoe, 2017 for additional references and a more detailed account of LTP as it affects the conditioning process.)

Implications of Reinforcement for Complex Behavior

The integration of the principle of natural selection with genetics is known as the modern synthesis (Dobzhansky, 1937). It provided the foundation for understanding the diversity and complexity of species. Recent work in behavior analysis and neuroscience encourages the belief that we are approaching a comparable achievement with regard to the diversity and complexity of individual behavior. Experimental analyses of behavior and neuroscience have reached the point at which Skinner believed a fruitful integration was possible. To repeat, "A science of the nervous system will someday start from the *direct observation* (italics in the original) of neural processes. It is with such a science that the neurological point of view must be concerned if it is to offer a convincing 'explanation' of behavior" (Skinner, 1938, p. 422).

The nature of the account provided by a synthesis of behavior analysis and neuroscience requires comment. First, such an account permits *interpretations* of complex behavior, not its experimental analysis. Most complex behavior is the product of a long history of selection by reinforcement, the details of which are unknown. However, many of these details are the result of experiences shared with other members of that species. In the technical sense in which Skinner used the term (Skinner, 1957), interpretations are required when present conditions do not satisfy the demands of experimental analysis but do meet two criteria: (a) The interpretation appeals to principles that are themselves the product of experimental analysis and to *only* such principles. (b) The history of the individual is likely to include events whose experimental analysis is the basis for the principles. Most scientific explanations of the real world are interpretations in this sense. Consider a boulder tumbling down a hill and coming to rest on a plain below. The relevant principles—those describing the effects of gravity, friction, and so on—are all known from prior experimental analyses, but, despite this knowledge, only an interpretation of the movement of the boulder is possible. This is because the precise values of all the relevant variables and the sequences in which the principles apply are unknown. Nevertheless, we confidently attribute the movement of the boulder to the concerted effect of the processes described by those principles. Our confidence stems from confidence in the experimental analyses upon which the principles were based. We are not temped to attribute the movement of the boulder to, for example, a spirit residing within the boulder as might our ancient ancestors.

A second characteristic of interpretations is that all of the behavioral observations on which prior experimental analyses were based may not be available when the complex behavior is interpreted. Among these missing observations are often the responses elicited by the reinforcer. For experimental analysis, observing reinforcer-elicited responses was crucial. It provided the basis for detecting reinforcer-induced behavioral change, the behavioral event most intimately related to the activity of neurons in the ventral tegmental area (VTA). From an evolutionary perspective, the changes elicited by a reinforcer were likely the behavioral phenotype on which depended the natural selection of the neural mechanisms of reinforcement. However, once the VTA mechanisms had been naturally selected, they could be exploited to serve other functions, notably conditioned reinforcement via the nucleus accumbens (NAC). VTA activity initiated via the NAC need have subtle, if any, behavioral effects. To the extent that conditioned reinforcers sustain sequences of behavior that lead ultimately to biologically important reinforcers, selection of the NAC-mediated mechanisms of conditioned reinforcement could themselves be naturally selected. Interpretations of behavior may draw upon conditioned reinforcement in interpretations of complex behavior because it is the product of experimental analyses of behavior and neuroscience and of the history of the individual.

Acquisition of Verbal Behavior

The acquisition of verbal behavior is uniquely positioned to exploit conditioned reinforcement. Consider the following: Suppose that pointing at a dog or a picture of a dog by a child is followed by approval after the parent asks “Where is the dog?” Under these circumstances, the stimulus *dog* comes to function as a discriminative stimulus in whose presence pointing at the object is reinforced. We know from experimental analysis that a discriminative stimulus can also function as a conditioned reinforcer. If the child later utters the vocal response “dog” in the presence of a *dog*, then the vocal response is automatically reinforced whether or not the parent is present (Skinner, 1957; Vaughan & Michael, 1982). When the child hears the auditory consequences of its own vocal responses, then those auditory stimuli function as conditioned reinforcers. Moreover, conditioned reinforcement for the vocal response is immediate. In addition, the more similar the stimulus produced by the child’s vocal response is to the discriminative stimulus produced by the parent’s vocalization, the greater the conditioned reinforcement. Thus, an automatic shaping process occurs. (Note that the entire process takes place without the intervention of an external agent implementing an extrinsic reinforcer.)

Verbal behavior is in an especially advantageous position to exploit *automatic shaping*: Unlike most other responses, the vocal response can immediately produce a stimulus that is similar to the one that was previously established as a discriminative stimulus. The acquisition of vocal responses of children through automatic conditioned reinforcement is facilitated by the stimulus characteristics of the vocal responses of the caretaker, so-called motherese (Cooper et al., 1997). Adult speech directed toward young children occurs at a higher auditory frequency and with a simplified structure, thereby increasing the potential similarity of the child’s speech sounds to those of the caretaker. The similarity is further enhanced by the fact that human auditory perception displays transposition, that is, sensitivity to the *relative* frequency profile of auditory patterns and not only their absolute frequency (cf. Weisman & Ratcliffe, 1992). (For a related application of conditioned reinforcement to observational learning, see Donahoe, 2010, p. 148.)

Autistic Behavior

The behavioral characteristics of persons diagnosed with autism vary widely, and for that reason the dysfunction is described as an autism *spectrum* disorder (ASD). Among its diverse characteristics are delay in language acquisition and deficits in social behavior. The occurrence of language deficits suggests the possibility that an important contributor to the disorder may be the neural mechanisms that implement conditioned reinforcement. Is there such evidence?

With the development of noninvasive technologies to monitor the function and structure of the living brain, answers are emerging. To study the effect of conditioned reinforcers on the behavior of persons with ASD, autistic subjects were placed in an apparatus that permitted indirect measures of the activity of various brain regions during a choice task (Dichter et al., 2012). When neurons are activated, their demand for oxygen increases, and this increases blood flow in those regions. The increased blood flow is detected with functional magnetic resonance imaging (*f*MRI). In an experiment in which brain activity was monitored using *f*MRI, correct choices were immediately followed by a brief view of either a dollar sign ($) or a picture. Each occurrence of the $ sign indicated that one dollar would be received at the end of the session. The $ sign was expected to function as a conditioned reinforcer. For control subjects, the $ sign activated neurons in the NAC. By contrast, for ASD subjects, activation of the NAC by the $ sign was greatly reduced. However, when a correct response was followed by a picture of an object that was known to be attention-demanding for ASD subjects, activity in the NAC increased and was the same as for control subjects. The effective pictures for ASD subjects were those of nonsocial objects such as machines, automobiles, and computers that had evoked eye-movement fixations during a pretest. In summary, the social stimulus $ functioned as an effective conditioned reinforcer for control subjects but not for ASD subjects as measured by the level of activation of neurons in the NAC.

A reduction in the responsiveness of NAC neurons provides a possible basis for interpreting the diverse behavioral deficits found along the ASD spectrum. To determine a possible origin of the reduction in NAC responsivity, a different noninvasive imaging technique was required—diffusion tensor imaging (DTI). DTI images the bundles (tracts) of association neurons within the cortex that allow neural activity from different areas of the cortex to influence one another. DTI detects the orientation of water molecules, and their orientation within a tract is not random but parallels the direction of the tract. DTI imaging revealed that the number and distribution of tracts within the prefrontal cortex to NAC were decreased for ASD subjects (Langen et al., 2012). Some of the tracts within the prefrontal cortex contain neurons that synapse on neurons in the medial prefrontal cortex, and these then project to the NAC.

Consider the possible effects of a reduction of the inputs from other cortical regions, ultimately, to the NAC. As an example, if the association neurons whose activity is normally influenced by articulatory responses are absent or reduced, then the input to NAC would be affected. As a result of the reduced activity in NAC, articulatory responses that produce speech sounds could not benefit from automatic conditioned reinforcement. Deficits in the acquisition of verbal behavior would ensue. Similar interpretations can be offered for other deficits seen along the autism spectrum. For example, if association neurons do not access areas of the cortex involved in social behavior, then the ability of social stimuli to function as conditioned reinforcers would be impaired.

The possibility that ASD may be interpreted as a dysfunction in the neural mechanisms of conditioned reinforcement suggests possible remedial actions. These actions have in common that they establish otherwise ineffective stimuli as conditioned reinforcers through their procedural association with stimuli that are able to serve this function. Among these procedures are those that produce equivalence classes, which occur with at least some ASD subjects (Eikeseth & Smith, 1992). Other procedures include Pavlovian higher-order and serial-compound conditioning (Kehoe & Morrow, 1984) and operant chaining (e.g., Kelleher & Gollub, 1962; Williams, 1994b).

Concluding Comments

A goal of this chapter has been to demonstrate that behavior analysis has produced findings that promise a comprehensive understanding of the conditioning process with both the Pavlovian and operant contingencies and that neuroscience has produced findings that complement this understanding. Together, the sister biological sciences of behavior analysis and neuroscience provide a basis for the interpretation of complex behavior that parallels the achievements of the earlier synthesis of evolution through natural selection with genetics. Darwin's proposal of natural selection offered intriguing accounts of the origin of species prior to the discovery of genetics, but most of his contemporaries were unconvinced. Faced with ignorance of the biological mechanisms that implemented natural selection and the failure of Darwin's proposed mechanism of gemmules (Donahoe, 2003), some such as Karl Pearson sought to make a virtue of necessity. That is, they argued that knowledge of the biological mechanisms was unnecessary and that a purely functional account was sufficient (Gayon, 1998). A comparable situation now exists within some quarters of behavior analysis: "Neurophysiology may be omitted … because it reveals only mechanism" (Baum, 2011, p. 119). If the parallel holds, a failure to integrate behavior analysis and neuroscience will impede the path to achieving a persuasive interpretation of complex human behavior and to developing effective means of remediating dysfunctional behavior.

The picture of complex behavior that emerges from current knowledge is that the behavior of an experienced learner is an expression of both the legacy of past environments and the discriminative effects of the present environment. The legacy of the past consists not only of prior discriminations but also of the conditioned reinforcing effects on behavior of those discriminative stimuli. As a result, a potentially rich source of conditioned reinforcement maintains and shapes present behavior, including—very importantly—covert behavior. Appeals to conditioned reinforcement are strengthened by findings that show that conditioned reinforcers ultimately activate the same VTA dopaminergic reinforcement system as unconditioned reinforcers.

Present behavior—both overt and covert—is bombarded by a rich stream of conditioned reinforcers. The failure to incorporate findings from neuroscience would have a particularly adverse effect on the interpretation of private events. Behavior analysis regards private events as not different in kind from public events but only in their observability given present technology (Palmer, 2011; Skinner, 1953). The experimental analysis of neuroscience makes this point clearly: When a reinforcer occurs and dopamine produces a change in synaptic efficacy between coactive neurons, that change takes place whether or not an observable behavior occurs. A neuron "knows" only its immediate environment. Research has shown that networks of many neurons can be organized through the release of dopamine, even without detectable public behavior (Athalye et al., 2018). In short, private neuronal behavior as well as public muscular behavior is acquired through the action of the reinforcement system.

Finally, it should again be acknowledged that even a complete experimental analysis of behavior and neuroscience—which has yet to be fully achieved—provides *interpretations* of complex behavior. Interpretations may be made more precise through quantitative models including neural networks (e.g., Donahoe & Dorsel, 1997), but they remain interpretations nevertheless. We may console ourselves, however, with the realization that interpretations are all that any science can offer when confronted with complex phenomena and historical uncertainty.

References

Athalye, V. R., Santos, F. J., Carmena, J. M., & Costa, R. M. (2018). Evidence for a neural law of effect. *Science, 359*, 1024–1029.

Ayres, J. J. B. (1968). Conditioned suppression and the information hypothesis. *Journal of Comparative and Physiological Psychology, 62*, 21–25.

Balfour, M. E., Yu, L., & Coolen, L. M. (2004). Sexual behavior and sex-associated environmental cues activate the mesolimbic system in male rats. *Neuropsychopharmacology, 29*, 718–730.

Baum, W. M. (1973). The correlation–based law of effect. *Journal of the Experimental Analysis of Behavior, 20*, 137–153.

Baum, W. M. (2001). Molar versus molecular as a paradigm clash. *Journal of the Experimental Analysis of Behavior, 78*, 338–340.

Baum, W. (2011). What is radical behaviorism? A review of Jay Moore's *Conceptual foundations of radical behaviorism*. *Journal of the Experimental Analysis of Behavior, 95*, 119–126.

Betts, S. L., Brandon, S. E., & Wagner, A. R. (1996). Disassociation of the blocking of conditioned eyeblink and conditioned fear following a shift in US locus. *Animal Learning & Behavior, 24*, 459–470.

Brandon, S. E., Betts, S. L., & Wagner, A. E. (1994). Discriminated, lateralized eyeblink conditioning in the rabbit: An experimental context for separating specific and general associative influences. *Journal of Experimental Psychology: Animal Behavior Processes, 20*, 292–307.

Burgos, J. E., & Donahoe, J. W. (2016). Unified principle of reinforcement in a neural-network model: Reply to N. T. Calvin and J. J. McDowell. *Behavioural Processes, 126*, 46–54.

Burns, R., Burgos, J. E., & Donahoe, J. W. (2011). Pavlovian conditioning: Pigeon nictitating membrane. *Behavioural Processes, 86*, 100-2-108.

Canli, T., Detmer, W. M., & Donegan. (1992). Potentiation or diminution of discrete motor unconditioned responses (rabbit eyeblink) to an aversive Pavlovian unconditioned stimulus by two associative processes: Conditioned fear and a conditioned diminution of unconditioned stimulus processing. *Behavioral Neuroscience, 106*, 498–508.

Clark, F. C. (1958). The effect of deprivation and reinforcement frequency on variable interval responding. *Journal of the Experimental Analysis of Behavior, 1*, 221–228.

Colwill, R. M., & Rescorla, R. A. (1985). Postconditioning devaluation of a reinforcer affects instrumental responding. *Journal of Experimental Psychology: Animal Behavior Processes, 11*, 120–132.

Cooper, R. P., Abraham, J., Berman, S., & Staska, M. (1997). The development of infants' preference for Motherese. *Infant Behavior and Development, 20*, 477–488.
Crowley, M. A., & Donahoe, J. W. (2004). Matching: Its acquisition and generalization. *Journal of the Experimental Analysis of Behavior, 82*, 143–159.
Dichter, G. S., Felder, J. N., Green, S. R., Ritenberg, A. M., Sasson, N. J., & Bodfish, J. W. (2012). Reward circuitry function in autism spectrum disorders. *Social Cognitive and Affective Neuroscience, 7*, 160–172.
Dinsmoor, J. A. (1950). A quantitative comparison of the discriminative and reinforcing functions of a stimulus. *Journal of Experimental Psychology, 40*, 458–472.
Dobzhansky, T. G. (1937). *Genetics and the origin of species*. New York: Columbia University Press.
Donahoe, J. W. (1960). The effect of variations in the form of feedback on the efficiency of problem solving. *Journal of Experimental Psychology, 60*, 193–198.
Donahoe, J. W. (2003). Selectionism. In K. A. Lattal & P. N. Chase (Eds.), *Behavior theory and philosophy* (pp. 103–128). Kluwer Academic Publishers.
Donahoe, J. W. (2010). Learning and behavior. In N. R. Carlson, J. W. Donahoe, C. D. Heth, C. G. Martoin, & H. Miller (Eds.), *Psychology: The science of behavior* (7th ed., pp. 122–151). Allyn & Bacon.
Donahoe, J. W. (2012). Origins of the molar-molecular divide. *European Journal of Behavior Analysis, 13*, 195–200.
Donahoe, J. W. (2017). Behavior analysis of neuroscience: Complementary disciplines. *Journal of the Experimental Analysis of Behavior, 107*, 301–320.
Donahoe, J. W., & Burgos, J. E. (2000). Behavior analysis and revaluation. *Journal of the Experimental Analysis of Behavior, 74*, 331–346.
Donahoe, J. W., Burgos, J. E., & Palmer, D. C. (1993). A selectionist approach to reinforcement. *Journal of the Experimental Analysis of Behavior, 60*, 17–40.
Donahoe, J. W., Crowley, M. A., Millard, W. J., & Stickney, K. A. (1982). A unified principle of reinforcement: Some implications for matching. In M. L. Commons, R. J. Herrnstein, & H. Rachlin (Eds.), *Quantitative analyses of behavior: Vol. 2. Matching and maximizing accounts* (pp. 493–521). Ballinger.
Donahoe, J. W., & Dorsel, V. P. (1997). *Neural-network models of cognition: Biobehavioral foundations*. Elsevier Science Press.
Donahoe, J. W. & Palmer, D. C. (1994/2017). Learning and complex behavior. (Originally published Allyn & Bacon. Boston, MA.) Third reprinting Richmond, MA: Ledgetop Publishing. http://lcb-online.org/
Donahoe, J. W., & Palmer, D. C. (1988). Inhibition: A cautionary tale. *Journal of the Experimental Analysis of Behavior, 50*, 333–351.
Donahoe, J. W., Palmer, D. C., & Burgos, J. E. (1997). The S-R issue: Its status in behavior analysis and in Donahoe and Palmer's Learning and Complex Behavior. *Journal of the Experimental Analysis of Behavior, 67*, 193–211.
Donahoe, J. W., & Vegas, R. (2004). Pavlovian conditioning: The CS-UR relation. *Journal of Experimental Psychology: Animal Behavior Processes, 30*, 17–33.
Eikeseth, S., & Smith, T. (1992). The development of functional and equivalence classes in high-functioning autistic children: The role of naming. *Journal of the Experimental Analysis of Behavior, 58*, 123–133.
Frey, U. (1997). Cellular mechanisms of long-term potentiation: Late maintenance. In J. W. Donahoe & V. P. Dorsel (Eds.), *Neural–network models of cognition: Biobehavioral foundations* (pp. 105–128). Elsevier Science Press.
Gayon, J. (1998). *Darwin's struggle for survival:* Heredity and the hypothesis of natural selection. (Translated by M. Cobb from Darwin et l'aprés-Darwin: une histoire de l'hypothèse de sélection naturalle. Paris: Editions Kimé, Paris, 1992.) Cambridge, UK: Cambridge University Press.
Gluck, M. A. (1992). Stimulus sampling and distributed representations in adaptive network theories of learning. In A. E. Healy, S. M. Kosslyn, & R. M. Shiffrin (Eds.), *From l earning theory to connectionist theory: Essays in honor of William K.Estes* (pp. 169–200). Lawrence Erlbaum.

Gormezano, I., & Kehoe, E. J. (1981). Classical conditioning and the law of contiguity. In P. Harzem, M. D. Zeiler, & Chichester (Eds.), *Advances in analysis of behavior. Vol. 2, predictability, correlation, and contiguity* (pp. 1–45). Wiley.
Grace, A. A., & Bunney, B. S. (1984). The control of firing pattern in nigral dopamine neurons: Burst firing. *Journal of Neuroscience, 4*, 2877–2890.
Grace, A. A., Floresco, S. B., Goto, Y., & Lodge, D. J. (2007). Regulation of firing of dopaminergic neurons and control of goal-directed behaviors. *Trends in Neuroscience, 30*, 220–227.
Hebb, D. O. (1949). *The Organization of Behavior*. Wiley & Sons.
Holland, P. C., & Rescorla, R. A. (1975). The effect of two ways of devaluing the unconditioned stimulus after first- and second-order appetitive conditioning. *Journal of Experimental Psychology: Animal Behavior Processes, 1*, 355–363.
Jenkins, H. M., & Moore, B. R. (1973). The form of the autoshaped reponse with food or water reinforcers. *Journal of the Experimental Analysis of behaviour, 20*, 163–168.
Johnson, S. W., Seutin, V., & North, R. N. (1992). Burst firing in dopamine neurons induced by N-methyl-D-aspartate: Role of electrogenic sodium pump. *Science, 258*, 665–667.
Kamin, L. J. (1969). Predictability, surprise, attention and conditioning. In B. A. Campbell & R. M. Church (Eds.), *Punishment and aversive behavior* (pp. 279–296). New York: Appleton-Century-Crofts.
Kehoe, E. J., & Morrow, L. D. (1984). Temporal dynamics of the rabbit's nictitating membrane response in serial compound conditioned stimuli. *Journal of Experimental Psychology: Animal Behavior Processes, 10*(2), 205–220.
Kelleher, R. T., & Gollub, L. R. (1962). A review of positive conditioned reinforcement. *Journal of the Experimental Analysis of Behavior, 5*(4 Suppl), 543–597.
Langen, M., Leemans, A., Johnston, P., Ecker, C., Daly, E., Murphy, C. M., dell'Acqua, F., Durston, S., & the AIMS Consortium, & Murp, D. G. M. (2012). Fronto-striatal circuitry and inhibitory control in autism: Findings from diffusion tensor imaging tractography. *Cortex, 48*, 183–193.
MacDonall, J. S. (2009). The stay/switch model of concurrent choice. *Journal of the Experimental Analysis of Behavior, 91*, 21–39.
McDowell, J. J. (2013). A quantitative evolutionary theory of adaptive behavior dynamics. *Psychological Review, 120*, 731–750.
McNish, K. A., Betts, S. L., Brandon, S. E., & Wagner, A. R. (1997). Divergence of conditioned eyeblink and conditioned fear in backward Pavlovian training. *Animal Learning & Behavior, 25*, 43–52.
Palmer, D. C. (1988). Blocking of the conditioned reinforcing function of a stimulus. Unpublished doctoral dissertation. University of Massachusetts/Amherst.
Palmer, D. C. (2011). Consideration of private events is required in a comprehensive science of behavior. *The Behavior Analyst, 34*, 201–207.
Pasteur, L. (1854). *Lecture*. University of Lille, 7 December.
Pears, A., Parkinson, J. A., Hopewell, L., Everitt, B. J., & Roberts, C. (2003). Lesions of the orbitofrontal but not medial prefrontal cortex disrupt conditioned reinforcement in primates. *Journal of Neuroscience, 23*, 11189–11120.
Schneiderman, N. (1972). Response system divergencies in aversive classical conditioning. In. A. H. Black & W. F. Prokasy (Eds.), *Classical conditioning II: Current theory and research* (pp. 313–376). New York: Appleton-Century Crofts.
Schultz, W., Apicella, P., & Ljungberg, T. (1993). Conditioned stimuli during successive steps of learning a delayed response task. *Journal of Neuroscience, 13*, 900–913.
Schultz, W., Dayan, P., & Montague, P. R. (1997). A neural substrate of prediction and reward. *Science, 275*, 1593–1599.
Silva, F. J., Timberlake, W., & Cevik, M. (1998). A behavior systems approach to the expression of backward associations. *Learning & Motivation, 29*, 1–22.
Skinner. (1957). *Verbal behavior*. Appleton-Century-Crofts.
Skinner, B. F. (1935). The generic nature of the concepts of stimulus and response. *Journal of General Psychology, 12*, 40–65.

Skinner, B. F. (1938). *The behavior of organisms*. Appleton-Century-Crofts.

Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review, 57*, 193–216.

Skinner, B. F. (1953). *Science and human behavior*. Macmillan.

Skinner, B. F. (1974). *About behaviorism*. Random House.

Smith, D. V., Liu, H., & Vogt, M. B. (1996). Responses of gustatory cells in the nucleus of the solitary tract of the hamster after NaCl or amiloride adaptation. *Journal of Neurophysiology, 76*, 47–58.

Stickney, K., & Donahoe, J. W. (1983). Attenuation of blocking by a change in US locus. *Animal Learning & Behavior, 11*, 60–66.

Tait, R. W., & Saladin, M. E. (1986). Concurrent development of excitatory and inhibitory associations during backward conditioning. *Animal Learning & Behavior, 14*, 133–137.

Tait, R. W., Quesnel, L. J., & ten Have, W. N. (1987). The effects of backward pairings on the conditioning of the albino rabbit's jaw movement and nictitating membrane responses. *Canadian Journal of Psychology, 41*, 271–286.

Vaughan, M. E., & Michael, J. L. (1982). Automatic reinforcement: An important but ignored concept. *Behavior, 10*, 217–227.

Waelti, P., Dickinson, A., & Schultz, W. (2001). Dopamine responses comply with basic assumptions of formal learning theory. *Nature, 412*, 43–48.

Weisman, R., & Ratcliffe, K. (1992). The perception of pitch constancy in bird songs. In W. K. Honig & G. Fetterman (Eds.), *Cognitive aspects of stimulus control* (pp. 243–262). Lawrence Erlbaum.

Williams, B. A. (1994a). Conditioned reinforcement: Neglected or outmoded explanatory construct? *Psychonomic Bulletin & Review, 1*, 457–475.

Williams, B. A. (1994b). Conditioned reinforcement: Experimental and theoretical issues. *Behavior Analyst, 17*, 261–285.

Williams, D. R., & Williams, H. (1969). Auto-maintenance in the pigeon: Sustained pecking despite contingent non-reinforcement. *Journal of the Experimental Analysis of Behavior, 12*, 511–520.

Wilson, D. I., & Bowman, M. (2004). Nucleus accumbens neurons in the rat exhibit differential activity to conditioned reinforcers and primary reinforcers within a second-order schedule of saccharin reinforcement. *European Journal of Neuroscience, 20*, 2777–2788.

Yagishita, S., Hayashi-Takagi, A., Ellis-Davies, G. C. R., Urakubo, H., Ishii, S., & Kasai, H. (2014). A critical time window for dopamine actions on the structural plasticity of dendritic spines. *Science, 345*, 1616–1620.

Chapter 11
Comments on "Biological Behaviorism"

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The relation between psychology and biology has a long and turbulent history. The path of psychology towards an independent scientific discipline was not obvious. Auguste Comte (1830–1842/1978), for example, argued that there would be no justification for a psychological science, since the phenomena supposedly studied by it would necessarily be either the object of sociology or of biology. Even after psychology had reached its independence, behaviorism often oscillates, metaphorically, between the gravitational influences of such two abovementioned great scientific fields. Some versions of behaviorism are more influenced by the social sciences, emphasizing aspects of cultural determination of behavior (George Mead, for instance). Others, in turn, are marked by the search for biological or organic behavioral or learning substrates (as K. S. Lashley).

Skinner (1969, 1974) criticized Watson's behaviorism for trying to hide his lack of data on behavior by artificially filling in the gaps with biological knowledge, especially anatomy and physiology. In fact, the book *Behaviorism* (Watson, 1970) has several chapters with basic and general information about the anatomo-physiological apparatus. However, Skinner failed to recognize in his criticism a very relevant aspect to understand Watson's proposal. To know behavior is to know the relation between a biologically constituted organism in constant interaction with its world and the environment. The way in which the environment affects the organism depends mainly on its constitution and its evolutionary history. A stimulus does not work in a vacuum. Its function is not intrinsic, but relational. The environment affects an organism who has a given physical constitution and with a certain evolutionary history. The stimulus is necessarily modulated by what happens inside the body. Therefore, for Watson, understanding how behavior works would require understanding, as a prerequisite, how the behaving organism works. By incorporating chapters on anatomy, physiology, and instincts in his book, Watson

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was assured in his theory the presence of all relevant elements in the interaction that would define the object of study itself.

On the other hand, we can discuss whether or not the biological knowledge used by Watson in his time was directly related to behavioral phenomena. In that sense, Skinner was right in saying that there was very little knowledge produced by the newly created behavioral science itself. A behavioral science would still need to be built.

The relation between Skinner and biology, in particular physiology and neurology, was often ambiguous. At the beginning of his work in the 1930s (Skinner, 1938), he started from the physiological tradition both to extract from it his first conceptual tools, such as "stimulus," "response," and "reflex," and also to define his own way of producing and validating experimental knowledge, deeply influenced by Pavlov and by the practices adopted in the Harvard physiology laboratory led by Crozier, Loeb's disciple.

Even though Skinner was influenced in many ways by the physiology of his day, he advocated an autonomous science of behavior. He defended the study of behavior in itself and not as an indication of the functioning of another level of analysis, mental or physiological (Skinner, 1938, 1974). Skinner (1953) described knowledge about the physiological apparatus as expendable in the context of behavior analysis. He predicted that physiology would be guided, concerning on what to look for in the body, by the findings of behavior analysis, but the opposite would not occur (behavior analysis guided by what physiology might discover when looking for what to investigate about behavior). Once some general principles of behavior or learning were discovered in an externalist way, physiology could seek the organic substrate of such functional relations. At other times, Skinner (1966, 1969, 1974, 1981, 1989) recognized that physiology would help to fully understand behavior functioning (for a comprehensive review on the subject, see Zilio, 2015, 2016a, b).

Since behavior analysis has established itself as an independent science and physiology has developed in technological, methodological, and conceptual terms, the initial and circumstantial reasons for the separation of the two disciplines no longer seem to exist. The contributions of Professor Donahoe's "biological behaviorism" are inserted exactly in this historical moment. As he says:

> *The present chapter should not be seen as a change in or a replacement of the scientific agenda envisioned by Skinner. Instead, it is a continuation of Skinner's agenda in light of recent findings in its sister discipline neuroscience*. In an interview with Skinner conducted shortly before his death, Margaret Vaughan (personal communication) told me that among the future developments Skinner most fondly anticipated were uncovering the neural basis of conditioning. (emphasis added)

Skinner (1953, 1966, 1969, 1974) recognized that behavioral phenomenon is a complex and continuous flow. Many things happen all the time at various levels in such an organism/environment interaction that we call behavior. By establishing two classes of behavior or two types of learning, respondent (classical or Pavlovian) and operant (or instrumental), the behavior analysis made a cut, to some extent arbitrary, in this flow. What is the response and/or stimulus to be taken for the analysis? Will it look at the antecedent or consequent events? As Donahoe points out:

> …*Although the difference in procedure instituted by the experimenter is clear, the difference in the sequence of events sensed by the organism is not* …. In the Pavlovian procedure, a stimulus (S_i) precedes the reinforcer (S^R) and S_i then comes to evoke a conditioned response that in the prototypic case resembles the response elicited by the reinforcer ($R_{elicited}$). In the operant procedure, a response (R_j) precedes the reinforcer and the operant increases in frequency. *Note, however, that in the Pavlovian procedure some response necessarily comes before the reinforcer: Organisms are always behaving. The nature of the preceding response is, however, not controlled by the experimenter and, as such, may vary on different occasions. Similarly, in the operant procedure some stimulus necessarily occurs before the operant and, therefore, before the reinforcer as well: Organisms are always sensing*. …. In short, on every instance in which a reinforcer occurs, some stimulus and some response precede the reinforcer in both the Pavlovian and operant procedures. The difference in the procedures is not the type of event that precedes the reinforcer, but the *reliability* with which a particular stimulus or a particular response precedes the reinforcer. *Thus, at the moment of reinforcement the organism cannot tell whether it is in a Pavlovian or an operant procedure!* (emphasis added)

An electric shock, for example, can be a consequence of a bar-pressing response; an eliciting antecedent of the jump response; an establishing operation that modulates the negative reinforcing value for escape and avoidance responses, including aggression; and an unconditional aversive that, when paired with a certain environment aspect, transfers the eliciting function to a conditional stimulus. In turn, a pellet of food for a private animal is a consequence for one class of response but also an eliciting stimulus for another class. The consistent presentation of the pellet preceded by a certain sound causes the acquisition of similar eliciting properties by that sound. More than that, it also makes the sound a conditional reinforcer for a certain class and discriminative for another. Multiple behavioral processes and functions occur almost simultaneously. As "behavior analysts," our training is "analytical," that is, we learn to "decompose" the world, and we learn to cut out the existing behavioral flow, seeking to identify specific patterns and differentiate them, in a sort of taxonomy of the behavior. But our cuts are just cuts. The flow remains a flow, constant and complex. It will always be possible to establish new and different ways of making a cut in the behavioral continuum (see, e.g., Baum, 2012, 2018, 2020).

Donahoe's theoretical proposal suggests a unified theory of reinforcement in which, from the point of view of neurological processes, respondent and operant conditioning would share common mechanisms. Commenting on Skinner's classic article "Selection for Consequences" for the "Canonical Papers" (special volume publisher originally in the Behavioral and Brain Sciences), Donahoe (1984/1989) says:

> Respondent and operant conditioning might best be regarded as simply different procedures for studying behavioral change, procedures that are potentially understandable in terms of a common reinforcement principle (see Donahoe, Crowley, Millard, & Stickney, 1982). (p. 38)

But such a theory would imply the dissolution or at least a revision of the consecrated distinction between the two pillars of the behavioral-analytic tradition. Skinner (1984/1989) replied at the time:

> *I do not agree that respondent and operant conditioning are best regarded as ´simply different procedures for studying behavioral change.´* As Fester and I pointed out in Schedules of Reinforcement (Ferster & Skinner, 1957), a term like 'conditioning' or 'extinction' is traditionally used to refer or two very different things: (1) the role of the experimenter or the environment in bringing about a change, and (2) the resulting change in the organism. *Donahue seems to add a third, 'procedures for studying behavioral change.'* We are concerned here with behavioral processes as they must have existed before anyone promoted them or studied them. *Whether there is a neurological principle common to respondent and operant conditioning is a question that will presumably be answered by neurologists; the two types of conditioning are still clearly distinguished by the contingencies under which they occur*. (p. 38; emphasis added)

The theoretical development and the body of empirical data of Donahoe's proposal were still incipient at that time. Furthermore, abandoning any classic distinctions is not a simple and easy task in behavior analysis, even when there are many reasons for this (see, e.g., the distinction between "positive" and "negative" reinforcement since Jack Michael's, Michael, 1975, never refuted criticism). And today, after almost 30 years, would Skinner's position still make sense? Has physiology advanced enough to prove the existence of the same organic basis for respondent and operant conditioning? Theoretical proposals in which sophisticated mathematical models are suggested have existed for a long time, such as that of Grossberg (1971), for example. Is there direct accumulated data on neurological functioning that would support the proposition of a single mechanism? Stein (1997), based on the data produced in his laboratory, suggests that it is not. For him, there are different physiological mechanisms for each type of conditioning. Donahoe (2002, 2017) and Donahoe et al. (1997) argue that it is. Is the unified theory of reinforcement supported by the direct accumulated experimental evidence so far? Is there any incompatible evidence? Are the disagreements based on the dispute between speculative models that still need empirical confirmation or on how to interpret the direct experimental evidence of physiology already effectively accumulated? The existence of physiological bases for learning, whatever it may be, does not seem to be in question now, and I believe it never has been. The critical question raised by Donahoe would be the presence of a single physiological mechanism underlying respondent and operant conditioning. This still seems to be an open question to be defined by the accumulated experimental evidence.

There are other interesting points raised in the chapter. Donahoe, like other authors, believes that the discovery of the physiological bases of behavioral principles, such as operant conditioning, could generate a positive effect for behavior analysis as a scientific discipline, increasing its public recognition. In turn, behavior analysis, strengthened and more integrated with the neurosciences, could gradually influence it. Two aspects seem relevant in this argument. First, in fact, some general principles of behavior, such as operant conditioning, discrimination, and generalization, are no longer mere theories. They have their place of phenomena empirically "proven." This was partly due to the identification of the physiological bases of such principles originally obtained in an externalist way. However, I see that such knowledge on behavior has been incorporated into the arsenal of neurosciences as mere techniques and not as a general guiding theory (see, e.g., Buccafusco, 2001). The main current theoretical orientation remains on cognitive inspiration (with all its internal variations). In this way, the possible influence that the behavior analysis would be much less than suggested. Radical behaviorism, at the height of its popularity with Skinner, inside and outside the academic world, was unable to significantly guide the neuroscience research agenda. Why now, when behaviorism influence and prestige are greatly reduced, would a behavioral model be widely adopted? There is nothing to suggest that this will change, despite the progress made internally in the behavior analysis. Mentalism won the battle against radical behaviorism as a general hegemonic theory for explaining psychological and neurological phenomena in the twentieth century. Watson and Skinner's antimentalism was never incorporated into the mainstream inside and outside psychology. The current occasional and circumscribed use of analytical-behavioral principles and procedures by neurosciences does not indicate that things will be different in the twenty-first century.

Regardless of being able to change the neurosciences in a relevant way, under what conditions would a relationship with physiology or neuroscience be beneficial for behavior analysis? Here is a second important point in this debate. Elcoro (2008) suggests some parameters that should be considered:

> …the inclusion of physiological data in behavior analysis represents an advantage if: (a) Prediction and control of behavior are increased: (b) Improvement of treatments of disorders is achieved; (c) Productive conceptual advancements are derived; (d) The focus is maintained on behavior. All these conditions do not necessarily have to all be met at once for the inclusion of physiological data to be an advantage to behavior analysis. (p. 259)

Did the physiological knowledge of the functional relationships originally established by behavior analysis increase our prediction and control? Has this knowledge led to the development of better behavioral techniques and technologies to deal with problematic behaviors? Have we had conceptual advances that resulted from this approach to physiology? If any of these aspects occurred, did it happen maintaining the behavior as an object of study in itself? Donahoe's chapter suggests positive responses for all of them, and I agree with him.

I would like to examine a last topic addressed by the author. One of the implications of a unified reinforcement theory would be the interpretation of punishment. Donahoe assumes that the suppressive mechanism observed in the punishment would be indirect, that is, other competing responses to the punished response would be elicited, and therefore the frequency of the punished response would be reduced. However, what would be the best way to explain the punishment would still be an open question. We basically have two major competing theoretical proposals. On the one hand, there is an asymmetric explanation in which the mechanisms present in strengthening (reinforcing) and weakening (punishment) the response would be different. While in the reinforcement there would be a direct effect of the consequences on the response, increasing its probability of future occurrence, in the punishment there would be no direct consequence but an indirect effect of competing responses elicited and/or negatively reinforced. Skinner (1953) and Sidman (1989) were some of the main defenders of such an asymmetric theory (see Carvalho Neto & Mayer, 2011; Holth, 2005). In this perspective, punishment would be a secondary behavioral process, an indirect effect of other variables, the weakening of the response being merely transitory. Positive reinforcement, in turn, is supposed to be a legitimate primary behavioral process with lasting or permanent effects on the likelihood of response.

An alternative way of explaining the punishment would be to consider the weakening of the response as a direct effect of the consequence, as occurs in reinforcement. Some consequences would have the function of strengthening the response and others of weakening the response. By adopting the same general explanatory mechanism, the consequence, this theory is called symmetric. Azrin and Holz (1966) were the main defenders of this theory. For them, there would be eliciting properties in both reinforcing and punitive stimuli. An electric shock and a food pellet would potentially have respondent and operant properties. However, just as we explain the strengthening of the operant response through a history of direct consequence, we should do the same to explain the weakening of the response. It is true that some aversive events can produce competing responses that indirectly suppress the punished response (as in the classic example of the conditioned suppression of Estes & Skinner, 1941). However, not every contingency of punishment shows the presence of competing responses or negative reinforcement (see, e.g., Mayer et al., 2018). Such concurrent responses are not systematically recorded and in most cases are merely inferred. The most serious case, according to Azrin and Holz (1966), would be when the “absence of a response” would be “the response” that supposedly competes with the punished response, as in the case of “passive avoidance.” Furthermore, would the effects of punishment necessarily be less lasting than those of positive reinforcement? Recent research with humans (Critchfield et al., 2003; Rasmussen & Newland, 2008) suggests that there is indeed an asymmetry but in favor of punishment.

What if the phenomenon of punishment is even more complex than we believe? What if there is not just one type of punishment but several types of punishment, with various types of associated suppressive mechanisms (direct and indirect)? What if there is a spectrum of punishments, as in a gradient between symmetrical and asymmetric mechanisms (Carvalho Neto et al., 2017)? Could physiology help behavior analysis to separate these functions and decide on the most complete explanation for punishment? I believe that in this and other cases, physiology could aid us immensely in understanding the behavioral phenomenon in all its complexity. Biological behaviorism would be most welcome.

References

Azrin, N. H., & Holz, W. C. (1966). Punishment. In W. K. Honig (Ed.), *Operant behavior: Areas of research and application* (pp. 380–447). Appleton-Century-Crofts.

Baum, W. M. (2012). Rethinking reinforcement: Allocation, induction, and contingency. *Journal of the Experimental Analysis of Behavior, 97*, 101–124.

Baum, W. M. (2018). Multiscale behavior analysis and molar behaviorism: An overview. *Journal of the Experimental Analysis of Behavior, 110*, 302–322.

Baum, W. M. (2020). Avoidance, induction, and the illusion of reinforcement. *Journal of the Experimental Analysis of Behavior, 114*(1), 116–141.

Buccafusco, J. J. (2001). *Methods of behavior analysis in neuroscience*. CRC Press.

Carvalho Neto, M. B., & Mayer, P. C. M. (2011). Skinner e a assimetria entre reforçamento e punição (Skinner and the asymmetry between reinforcement and punishment). *Acta Comportamentalia, 19*, 21–32.

Carvalho Neto, M. B., Mayer, P. C. M., & Ferreira, P. A. (2017). Simetrias e assimetrias entre reforçamento e punição: Uma proposta taxonômica (symmetries and asymmetries between reinforcement and punishment: A taxonomical proposal). *Acta Comportamentalia, 25*(1), 73–84.

Comte, A. (1978). Curso de filosofia positiva (J. A. Giannotti, Trad.). In J. A. Giannotti (Ed.), *Comte: textos selecionados* (pp. 1–39). Abril Cultural. Coleção Os Pensadores (Originally published in 1830-1842).

Critchfield, T. S., Paletz, E. M., MacAleese, K. R., & Newland, M. C. (2003). Punishment in human choice: Direct or competitive suppression? *Journal of the Experimental Analysis of Behavior, 80*, 1–27.

Donahoe, J. W., Crowley, M. A., Millard, W. J., & Stickney, K. A. (1982). A unified principle of reinforcement. In M. L. Commons, R. J. Herrnstein, & H. Rachlin (Eds.), Quantitative models of behavior (Vol. 2, pp. 493–521). Cambridge, MA: Ballinger.

Donahoe, J. W. (1989). Skinner: The Darwin of ontogeny? In A. C. Catania & S. Harnard (Eds.), *The selection of behavior: The operant behaviorism of B. F. Skinner: Comments and consequences* (pp. 36–38). Cambridge University Press. (Originally published in 1984).

Donahoe, J. W. (2002). Behavior analysis and neuroscience. *Behavioural Processes, 57*, 241–259.

Donahoe, J. W. (2017). Behavior analysis of neuroscience: Complementary disciplines. *Journal of the Experimental Analysis of Behavior, 107*, 301–320.

Donahoe, J. W., Palmer, D. C., & Burgos, J. E. (1997). The S-R issue: Its status in behavior analysis and in Donahoe and Palmer's learning and complex behavior. *Journal of the Experimental Analysis of Behavior, 67*(2), 193–211.

Elcoro, M. (2008). Including physiological data in a science of behavior: A critical analysis. *Revista Brasileira de Terapia Comportamental e Cognitiva, 10*(2), 253–261.

Estes, W. K., & Skinner, B. F. (1941). Some quantitative properties of anxiety. *Journal of Experimental Psychology, 29*, 390–400.

Ferster, C. B., & Skinner, B. F. (1957). Schedules of reinforcement. New York, NY: Appleton-Century-Crofts.

Grossberg, S. (1971). On the dynamics of operant conditioning. *Journal of Theoretical Biology, 33*, 225–255.

Holth, P. (2005). Two definitions of punishment. *The Behavior Analyst Today, 6*, 43–47.

Mayer, P. C., Carvalho Neto, M. B., & Katz, J. L. (2018). Punishment and the potential for negative reinforcement with histamine injection. *Journal of the Experimental Analysis of Behavior, 109*, 365–379.

Michael, J. (1975). Positive and negative reinforcement: A distinction that is no longer necessary; or a better way to talk about bad things. *Behavior, 3*, 33–44.

Rasmussen, E. B., & Newland, M. C. (2008). Asymmetry of reinforcement and punishment in human choice. *Journal of the Experimental Analysis of Behavior, 89*(2), 157–167. https://doi.org/10.1901/jeab.2008.89-157

Sidman, M. (1989). *Coercion and its fallout*. Authors Cooperative.

Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis*. Appleton-Century-Crofts.

Skinner, B. F. (1953). *Science and human behavior*. McMillan.

Skinner, B. F. (1966). The phylogeny and ontogeny of behavior. *Science, 153*(3741), 1205–1213.

Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis*. Appleton Century Crofts.

Skinner, B. F. (1974). *About behaviorism*. Vintage Books.

Skinner, B. F. (1981). Selection by consequences. *Science, 213*, 501–504.

Skinner, B. F. (1989). Comments. In A. C. Catania & S. Harnard (Eds.), *The selection of behavior: The operant behaviorism of B. F. Skinner: Comments and consequences* (p. 38). Cambridge University Press. (Originally published in 1984).

Stein, L. (1997). Biological substrates of operant conditioning and the operant-respondent distinction. *Journal of the Experimental Analysis of Behavior, 67*(2), 246–253.

Watson, J. B. (1970). *Behaviorism*. Norton Library. (originally published in 1924).

Zilio, D. (2015). Sobre as críticas de Skinner à fisiologia: Indicadoras de orientação antifisiológica ou contribuições relevantes? [on Skinner's criticisms of physiology: Indicatives of anti-physiological orientation or pertinent contributions?]. *Acta Comportamentalia, 23*, 465–482.

Zilio, D. (2016a). On the autonomy of psychology from neuroscience: A case study of Skinner's radical behaviorism and behavior analysis. *Review of General Psychology, 20*(2), 155–170.

Zilio, D. (2016b). Who, what, and when: Skinner's critiques of neuroscience and his main targets. *The Behavior Analyst, 39*(2), 197–218.

Chapter 12
Reactions to Commentary on Biological Behaviorism

John W. Donahoe

I am indebted to Prof. Carvalho Neto for placing the discussion of the unified reinforcement principle and its associated neural mechanisms in a more comprehensive historical/conceptual context and for raising issues regarding the treatment of punishment.

The relation between behavior analysis and its biological mechanisms has been fraught. In Skinner's effort to establish a science of behavior, some have mistakenly viewed efforts to build bridges to neuroscience as undermining behavior analysis as an independent scientific enterprise. As noted in the commentary, this is perhaps understandable at the beginning of an effort to establish a new science, but it is a misreading of Skinner's position. Behavior analysis is as independent of neuroscience as are any other biological sciences—physiology and cellular neuroscience, cellular neuroscience and molecular biology, molecule biology and genetics, and so forth. However, behavior analysis is also as *inter*dependent as are these disciplines. Skinner's point was that a science of behavior must seek regularities ("laws") at its own level of observation, not through appeals to other levels, most especially when the entities at those other levels are mere inferences from behavioral observations alone (cf., Donahoe & Palmer, 1994).

The contrary view is equally flawed—namely, that once the lower-level processes are known, the higher-level processes are superfluous (cf. Wilson, 1975). As an example, even today when much is known about genetics, inferences from genes to the structures that are built by genes elude us. As noted in a recent monograph on evolutionary theory (Bell et al., 2010; cf. Donahoe, 2012): "inferring phenotypic effects from nucleotide changes remains challenging" (p. 283) and "compared to our knowledge of genomes, our knowledge of phenotypes remains cursory" (p. 642).

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The critical role that higher-level constraints play in understanding underlying structure can be illustrated by genetic algorithms, which are used to design electronic circuits (Holland, 1992). The selection process implemented by the genetic algorithm can produce a circuit that implements a relatively simple task, for example, what are called pattern discriminations in conditioning or exclusive-or problems in logic and computer science. However, the function of the circuit is often not apparent from knowing the structure of the circuit alone. It is only after knowing the goal of the genetic algorithm that the function of the circuit becomes apparent. How much more difficult is understanding behavioral function from knowledge of neural structure alone! In the case of the unified reinforcement principle, understanding the relation between the prefrontal cortex, nucleus accumbens, and ventral tegmental area was guided by behavioral findings regarding acquired reinforcement and the separability of the discriminative and conditioning-reinforcing functions.

Turning to a different matter, the commentary raises the possibility that punishment may not be the result of a single biobehavioral process. I agree. The interpretation of punishment provided in the chapter was confined to the treatment of elicited responses from the perspective of the unified reinforcement principle. According to this principle, if a response-contingent aversive stimulus evokes behavior (such as escape responses) that compete with the operant, then the operant declines in frequency. That is, the operant is punished. However, not all aversive stimuli elicit responses that compete with all operants. As an example, if biting an object is reinforced with food and occasional electric shocks also elicit biting that same object, then operant biting is *facilitated* by shock (Kelleher & Morse, 1968). These different outcomes are facilitated because the behavioral changes elicited by the aversive stimulus necessarily occur closer in time to the discrepancy produced by the aversive stimulus than does the operant response upon which the aversive stimulus is contingent. (The temporal difference between the occurrence of the operant and reinforcer-elicited responses is also the basis for the account of revaluation provided by the unified reinforcement principle; Donahoe & Burgos, 2000). If the eliciting stimulus is aversive, then additional neural structures are recruited, but the resulting process appears functionally similar to that engaged by appetitive stimuli. The crucial additional structure is the amygdala, a complex, multi-region structure located in the temporal lobes (LeDoux, 2007). The amygdala receives inputs from a wide range of external and internal stimuli (importantly including pain-inducing stimuli). A pathway exiting the amygdala projects to the nucleus accumbens and from there to the ventral tegmental area (Root et al., 2018). A subset of the neurons in the ventral tegmental area then project back to the amygdala where they release dopamine (Tang et al., 2020). As with the dopaminergic projections to the prefrontal cortex, dopamine diffuses within the amygdala and produces long-lasting changes in synaptic efficacies between coactive neurons—those activated by sensory inputs to the amygdala and output neurons from the amygdala. These output neurons have complex effects, activating autonomic responses and inhibiting skeletal responses that produce, for example, freezing (Tovote et al., 2015). Thus, aversive stimuli may not only elicit behavior that compete with the operant, but they may also directly inhibit behavior including operants. (The foregoing account describes only a small portion of the interactions involving the various subregions of the amygdala and their connections with other structures, e.g., Jackson & Moghaddam, 2001; Janak & Tye, 2015).

A final reaction to the Commentary: I remain sanguine that a behavior-analytic account of complex human behavior, when supplemented by the relevant neuroscience, will ultimately displace current cognitive accounts. The justifications for this belief are beyond the scope of the present paper, but efforts in this direction are underway. I would especially note the critical contributions of my colleague David Palmer (e.g., Donahoe & Palmer, 1994) whereby phenomena are addressed that are conventionally denoted by such terms as attention, concept formation, memory, and language. Basic to these efforts is a recognition of Skinner's underappreciated distinction between the roles of experimental analysis and interpretation in science (Donahoe, 2004).

References

Bell, M. A., Futuyma, D. J., Eanes, W. F., & Levinton, J. S. (2010). *Evolution since Darwin: The first 150 years*. Sinauer Associates.

Donahoe, J. W. (2004). Interpretation and experimental-analysis: An underappreciated distinction. *European Journal of Experimental Analysis, 5*(2), 83–89. https://doi.org/10.1080/15021149.2004.10446387

Donahoe, J. W. (2012). Reflections on behavior analysis and evolutionary biology: A selective review of evolution since Darwin—The first 150 years. *Journal of the Experimental Analysis of Behavior, 97*(2), 249–260. https://doi.org/10.1901/jeab.2012.97-249

Donahoe, J. W., & Burgos, J. E. (2000). Behavior analysis and revaluation. *Journal of the Experimental Analysis of Behavior, 74*(3), 331–346. https://doi.org/10.1901/jeab.2000.74-331

Donahoe, J. W. & Palmer, D. C. (1994/2017). Learning and complex behavior. : Ledgetop Publishing. http://lcb-online.org/.

Holland, J. H. (1992). *Adaptation in natural and artificial systems*. MIT Press.

Jackson, M. E., & Moghaddam, B. (2001). Amygdala regulation of nucleus accumbens dopamine output is governed by the prefrontal cortex. *Journal of Neuroscience, 21*(2), 676–681. https://doi.org/10.1523/JNEUROSCI.21-02-00676.2001

Janak, P. H., & Tye, K. M. (2015). From circuits to behavior in the amygdala. *Nature, 517*(7534), 284–292. https://doi.org/10.1038/nature14188

Kelleher, R. T., & Morse, W. H. (1968). Behavior schedules using noxious stimuli. III. Responding maintained with response-produced electric shocks. *Journal of the Experimental Analysis of Behavior, 11*, 819–838.

LeDoux, J. (2007). The amygdala. *Current Biology, 17*(20), R868–R874. https://doi.org/10.1016/j.cub.2007.08.005

Root, D. H., Estrin, D. J., & Marisela Morales, M. (2018). Aversion or salience signaling by ventral tegmental area glutamate neurons. *iScience, 2*, 51–62. https://doi.org/10.1016/j.isci.2018.03.008

Tang, W., Kochubey, O., Kintscher, M., & Schneggenburger, R. (2020). A VTA to basal amygdala dopamine projection contributes to signal salient somatosensory events during fear learning. *Journal of Neuroscience, 40*(20), 3969–3980. https://doi.org/10.1523/JNEUROSCI.1796-19.2020

Tovote, P., Fadok, J. P., & Andreas Lüthi, A. (2015). Neuronal circuits for fear and anxiety. *Nature Reviews Neuroscience, 16*(6), 309–331. https://doi.org/10.1038/nrn3945

Wilson, E. O. (1975). *Sociobiology: The new synthesis*. Harvard University Press.

Part V
Intentional Behaviorism

Chapter 13
Intentional Behaviorism

Gordon R. Foxall

Overview

A Progressive Research Program

The intentional behaviorist research program has progressed from the foundation of an empirical base for the explanation of consumer choice to the development of cognitive models of consumer choice that rests on solid conclusions about what it is that makes consumer action action rather than behavior.[1] Recognition that the focus of the research program is henceforth principally on consumer action rather than consumer behavior, appreciation of the role of contingency representations in the explanation of consumption, and understanding of consumer choice in terms of the temporal considerations that underlie decision processes all indicate the progressive nature of the intentional behaviorist research program. The behavioral perspective

[1] A comprehensive account of intentional behaviorism is available in Foxall (2020), *Intentional behaviorism: a research methodology for consumer pyschology*. Cambridge, MA: Academic Press. A recent account can also be found in Foxall, G. R. (in press), in Intentional behaviorism: a research methodology for consumer psychology, L. Kahle et al. (Eds.) *The APA Handbook of Consumer Psychology*. Washington, DC: American Psychological Association.

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model (BPM) of purchase and consumption, which provides a *motif* for the intentional behaviorist research strategy, proposes that consumer choice is a function of the patterns of reinforcement and punishment which have followed consumer activity.[2] A functional analysis of consumer choice reveals an eightfold classification of the patterns of reinforcement and consumer behavior setting scope that shape and maintain consumer behavior (the contingency categories) and that the consumer situations that are the immediate precursors of consumer behavior can be defined in these terms. The model accommodates behaviorist, intentional, and cognitive perspectives to portray consumer choice, first, as the outcome of the rewards and sanctions that are the consequences of behavior and, subsequently, as a mode of human action that must be understood in terms of the desires, beliefs, emotions, and perceptions of the consumer and her intellectual functioning. Hence, the BPM provides a vehicle for the exploration of the relationships between the context in which consumer choice occurs (the contingencies of reinforcement and punishment) and the cognitive processes that underlie this choice (decision-making) via the construction of an intentional consumer situation that explains their interaction (Foxall, 2007a).

From Consumer Behavior to Consumer Action

The intentional behaviorism research program has reached an advanced phase: that of constructing and critically evaluating an intentional account of consumer choice, having identified, through the exhaustive testing of a behaviorist model of consumer behavior, the boundaries of extensional explanation (Foxall, 2004, 2016a, b).

The first phase of the intentional behaviorist research strategy, *consumer behavior analysis*, has accomplished the necessary model building, testing, and evaluation for deciding where intentional, including cognitive, explanation is essential, the form it needs to take, and the functions it needs to perform (Foxall, 2017). This stage, based on a research strategy of theoretical minimalism, continues apace for what it reveals of the relation of consumer behavior to its environmental determinants. But, at the same time, we are moving on.

The conduct of empirical research that has tested the central assumptions and explanatory modes of the behaviorist model of consumer choice has also revealed three points at which an extensional explanation of consumer choice breaks down because the stimulus field necessary to sustain it cannot be identified. These are the continuity/discontinuity of behavior across situations, the comprehension of consumer behavior at the personal level of exposition, and the delimitation of behavioral interpretations. All three of these bounds of behaviorism invite an intentional account (Foxall, 2004, 2007b, 2008, 2009, 2016b). The extensional consumer situation, conceptualized simply as the interaction of the consumer's learning history

[2] Economic behavior has provided the focus for the development of the intentional behaviorist research paradigm, but it is believed to be applicable to human behavior in general, if only because operant behavior is fundamentally economic.

and the stimulus field provided by the current consumer behavior setting, must give way to an intentional consumer situation if the explanation of consumer choice is to proceed. An essential methodological aim of the present chapter is to clarify the content and role of this *intentional* consumer situation as part of the explanatory medium that links context and cognition.

The second phase is composed of two stages, the construction of an intentional interpretation and the critical appraisal thereof, which determines whether current cognitive interpretations adequately underpin this intentional explanation. And it introduces three novel concerns.

First, the exploration of this advanced phase, psychological explanation, necessitates a shift in the conceptualization of consumer activity from behavior to action. While behavior is explicable by reference to the antecedent and consequential stimuli through which it can be predicted and influenced, action lacks such a stimulus field and is accounted for in terms of the actor's desires, beliefs, emotions, and perceptions. But this does not imply that consumer action is context-free. Rather, our concern is with how the context within which consumer choice occurs, broadly speaking what behavioral psychology calls the contingencies of reinforcement and punishment, rewards, and sanctions, relates to the mental processes that guide or at least provide the explanation for consumers' actions. The stepping-off point is the delineation of consumer choice as activity that entails temporal conflict between alternative courses of action which differ in their objective and psychological evaluation. This understanding of consumer choice is an important element in what makes action action.[3]

Second, the intentional interpretation that forms the second stage of intentional behaviorism is the construction of the consumer situation, the immediate precursor of consumer choice, in intentional terms. It, therefore, embodies the language of intentionality rather than that of extensionally described consumer behavior settings (which consist in stimulus fields) and learning histories (which somehow summate previous exposure to such stimulus fields). The construction of the intentional consumer situation requires concepts that indicate how the individual represents the contingencies of reinforcement and punishment that have provided the context for previous patterns of consumer choice and those that currently signal the probable outcomes of continued consumer actions. These *contingency representations* consist in beliefs and desires with respect to the functional outcomes of action and the perceptual experience the consumer has had of previously encountered consumer situations plus her current perceptual experience with regard to the outcomes that the present consumer behavior setting suggests will eventuate from further consumer action. The quest, therefore, involves the nature of *perceptual* contingency representation and links it to the emotional experiences consumers report based on their experience of consumer situations that portray various patterns of contingency.

[3] I do not deal with the concept of contingency representation in any detail in this abstract. A comprehensive account of its derivation and explanatory significance can be found in Chaps. 3 and 4 of Foxall (2018).

All in all, contingency representation is a second important element in understanding what makes action action.

Third, viewing consumer activity as consumer choice, defined in terms of a temporal conflict between alternative courses of action, introduces the consideration of akrasia into our subject matter and therefore a broader perspective on consumer rationality. In seeking the appropriate desires and beliefs of which the intentional consumer situation is partially constructed, this book adopts an approach which is amenable not only to the incorporation of rational propositional attitudes of this kind but also of a-rational and even irrational intentionality. Whereas earlier expositions of the intentional behaviorist research strategy have concentrated on the role of economic rationality in the explanation of consumer choice, the focus of this volume is on psychological rationality, and, given an emphasis on the consumer-as-akrates, it does not rest on the automatic assumption, common among philosophers of mind, that human action is a rational outcome of mentation. Rather, following Brakel (2009), it seeks a more rounded understanding of mental processes and their contents. The nature of the rationality (/irrationality/a-rationality) that is relevant to the intentional explanation of consumer choice is something to be further explored by reference to the structure and functioning of the cognitive procedures that underlay intentional interpretation. Moreover, psychological rationality is the third central component of what makes action action.[4]

The key to the progress of this research program, the generation of its empirical foundation, and its capacity to enhance interdisciplinary understanding of human behavior is the behavioral perspective model.

A Model of Consumer Situation

The methodology of intentional behaviorism exploits the tension between the behaviorist and cognitive perspectives, viewing each as indispensable to the other. At the heart of this intentional behaviorist research strategy is the behavioral perspective model (BPM) which can assume behaviorist, intentional, and cognitive perspectives with the aim of rendering consumer activity increasingly intelligible as its empirical base is first explored directly and then through the ascribed phenomenology of the consumer. In its contribution to the initial stage of the research program, the BPM employs a radical behaviorist depiction of consumer activity for two reasons: first, to establish the extent to which a noncognitive model can uniquely elucidate this aspect of human activity and, second, to identify the points at which such an extensional account breaks down and requires the development of an intentional theory of choice. This delineation of the BPM is based on behavior analysis, a school of psychology that relates the rate at which behavior occurs to the nature of the consequences that similar behavior has generated in the past. Behavior analysis

[4] See Chaps. 5 and 6 of Foxall (2018).

embraces a philosophy of psychology, radical behaviorism, in which the explanation of behavior involves the demonstration that it can be predicted and controlled on the basis of the environmental stimuli that precede and follow it, nothing else.[5]

This parsimonious version of the model relies on the operant "three-term contingency" of radical behaviorism which explains behavior by allusion to its predictability and modification by reference to environmental stimuli. A discriminative stimulus is a pre-behavioral event in the presence of which the individual discriminates her behavior, preforming a response that has previously been rewarded rather than one that has not. Better than "rewarded" is "reinforced" in the sense that behavior that is followed by such an event is likely to increase in frequency on future occurrences of the appropriate discriminative stimulus. Discrimination in this sense is simply an observation of an individual's behavior rather than the attribution to her of a mental operation. "Reinforcement" refers, then, to the strengthening of the behavior. Consequences of behavior that eventuate in its being performed less frequently are known as punishers; it is important to bear in mind that it is the behavior that is punished, not the individual. Reinforcers and punishers are post-behavioral stimuli, but it is their occurrence in the past, in the consumer's learning history, that accounts for their present potency in shaping and maintaining consumer activity (see Table 13.1).

Positive reinforcement is an increase in the rate of responding due to the receipt of a positive reinforcer; punishment is a reduction in the rate of responding due to the receipt of an aversive consequence. The exposition retains Skinner's (1953, 1974) terminology because it allows more subtle distinctions to be made about the environment events that control behavior. Both positive and negative reinforcements involve an increase in the rate of responding: positive reinforcement means working harder, paying more, or performing more responses to obtain the reinforcer; negative reinforcement means increasing the performance of an evasive behavior, one that allows an aversive consequence to be escaped. Punishment and omission involve a decrease in the rate of responding. Punishment is the reception of/approach toward an aversive outcome when this reduces the frequency of the behavior in question. I may still buy fresh fruit when its price increases substantially, but I buy less of it. Skinner is meticulous in using the term reinforcement for these instances rather than reward. Behavior is reinforced by an outcome that

Table 13.1 Effects of consequential stimuli on rate of responding

	Consequential stimulus	
Behavior	Positive	Aversive
Approach (generate, produce, or accept the consequential stimulus)	Positive reinforcement	Punishment
Avoidance or escape (prevent or eliminate the consequential stimulus)	(absent)	Negative reinforcement

[5] For a treatment of the behaviorist, intentional, and cognitive perspectives that the BPM elucidates, see Foxall (2016b).

increases its probability. A person can be rewarded by the adventitious receipt of a gift, say, but her frequency of behaving is not contingent upon this. The same is true of punishment: it is the behavior that is punished when its rate is reduced in the face of its being followed by certain consequences, not the person. In the analysis of consumer behavior which follows, I will use the term reinforcer to refer to consequences of behavior that increases its rate. I shall speak of emotional reward in referring to the positive emotional outcomes of behaving and receiving reinforcement. This is a subset of the reward as Skinner speaks of it. Correspondingly, emotional punishment will refer to the negative emotional outcomes of behavior.

Another type of pre-behavioral stimulus, the motivating operation, serves to enhance the relationship between a prospective response and the reinforcer which is forecast to follow its performance, making this consequence more attractive, more valuable insofar as the individual will work harder (or pay more) to obtain the reinforcer. We have seen that the three-term contingency of radical behaviorist explanation comprises a discriminative stimulus (S^D) increases the probability of a response (R) which has reinforcing/punishing outcomes ($S^{r/p}$) that influence its future rate of occurrence in the presence of the S^D. This may be augmented into a four-term contingency by the addition of an additional pre-behavioral stimulus or state, the motivating operation (MO). While the effect of a discriminative stimulus is on the probability of the response, the effect of the motivating operation is seen in its enhancing the relationship between the response and the reinforcer/punisher.

The response is known as an operant because it operates on the environment to produce consequences (in classical or Pavlovian conditioning, the response is sometimes known among radical behaviorists as a respondent; see Skinner, 1953). An operant response is, therefore, a function of post-behavioral stimuli but not as these are depicted in the three-term contingency; rather, it is a function of those reinforcing stimuli that have followed similar responses in the past and have thus become elements in the individual's learning history. The learning history is the principal explanatory variable in radical behaviorism because the pattern of prior behavior and the consequences it has generated are the means of predicting future behavior and of seeking to modify it. An operant does not properly refer to a single instance of behavior but to a class of responses, all of which generate similar consequences. As an extensional behavioral science, radical behaviorism avoids causal reference to such intentions as desires and beliefs, perceptions, and emotions.

The behavior analytic paradigm that has been described commends itself to the pursuit of theoretical minimalism because of the instrumental (or operant) conditioning on which it based: behavior is a function of the outcomes that are contingent on its performance. This is precisely the stance adopted in the study of economic behavior and is also appropriate to the study of much social behavior. Consumer choice is a function of the economic and social consequences contingent upon it and is, sequentially, controlled by those outcomes. This reliance on the explanatory device of behavior analysis gives rise to the style of theoretical minimalism most appropriate to the first stage of intentional behaviorism, namely, consumer behavior analysis. The three−/four-term contingency requires some adjustment, however, if it is to be useful for the comprehension of human consumer choice, and, even in the extensional depiction of consumer choice, there are important conceptual elaborations of traditional behavior analysis (Foxall, 2016b, Chap. 2).

Consumer Behavior Setting Scope

Except in highly restricted experimental settings, people do not respond to a single stimulus but to a selected subset of all the stimuli to which they are exposed. A consumer behavior setting, therefore, is not a single stimulus as in the three- or four-term contingency but a stimulus field, a gestalt, which shapes and maintains a pattern of consumer choice. Moreover, consumer behavior settings differ in the degree to which they encourage or inhibit a particular pattern of behavior; relatively open settings are those that permit a range of consumer behaviors to be undertaken (like being at a party), while relatively closed settings allow only one or at most a few alternative behaviors to be performed (e.g., being in the audience of an opera performance). The continuum of consumer behavior settings is, moreover, a restricted range of the entire spectrum of setting types open to humans (Fig. 13.1).

The way in which the consumer behavior setting is perceived, especially in terms of its closed-open scope, reflects individual differences in, inter alia, cognitive style, category width, and tolerance of ambiguity. But the consumer's learning history is, within the operant paradigm, the principal device for the prediction and control of

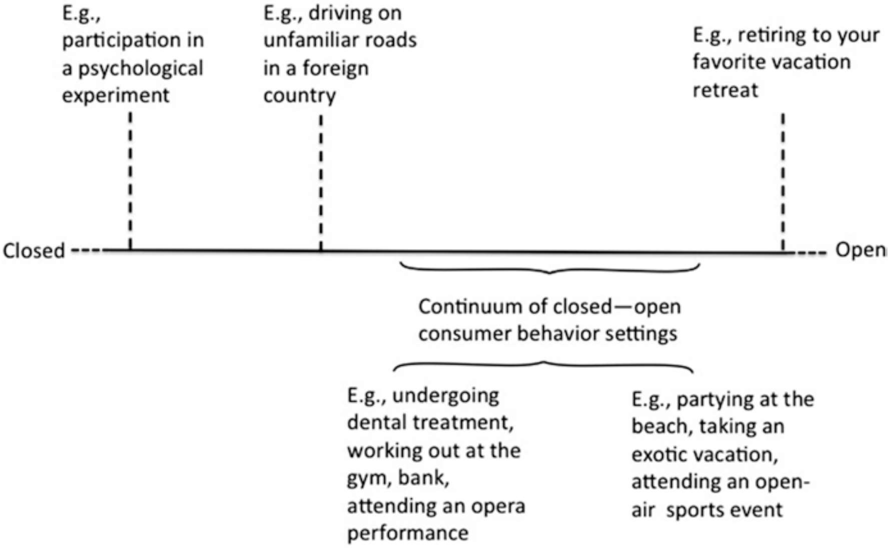

Fig. 13.1 The continuum of closed-open consumer behavior settings. The diagram is not drawn with any scale in mind but is intended to illustrate the important relationships between the absolute continuum of settings which influence human behavior and the restricted spectrum of settings that are applicable to mainstream consumer behavior. See Foxall (1990/2004, 2010b, 2016b)

their behavior in settings like the operant laboratory, a setting from which all extraneous sources of behavioral control have been eliminated so that only the uncomplicated stimulus-response-stimulus progression of operant conditioning can influence behavior. Such a paradigm scarcely suffices, however, for even the prediction and control of the human activities that comprise purchasing and consuming except in the gross terms of market aggregation. The idea of the consumer situation in consumer behavior analysis combines the concept of a learning history with that of a consumer behavior setting, the latter comprising the physical (including temporal) and social (including regulatory) stimuli that make up the immediate milieu of consumer choice. It is this context, primed and given meaning by the consumer's learning history, that is the consumer situation, the immediate precursor, and determinant of consumer behavior.

To what does the consumer's learning history refer in addition to the log of behaviors she has previously performed? It incorporates also the outcomes of those behaviors, the log of reinforcers and punishers that have followed the enactment of all those responses. The pattern of reinforcement found in the behavioral perspective model also differs from the single reinforcer or punisher depicted in this explanatory device. Reinforcers in human experience are of two kinds or sources. Utilitarian reinforcement refers to the functional benefits provided by products and services; informational reinforcement refers to the social feedback on the consumer's performance, the status or esteem that accrues to the consumer who models behaviors that are socially prescribed and approved. It is the combination of these two kinds of outcome, the pattern of reinforcement, that determines the continuity of human complex behaviors such as consumer choice. Although we shall refer to the pattern of reinforcement for simplicity of exposition, it needs to be borne in mind that consumer behavior is always punished (if only through the surrender of so valuable a means of purchasing power as money) as well as reinforced.

Pattern of Reinforcement and Operant Classes of Consumer Behavior

The enhancement of the three-/four-term contingency that provides the summative behavioral perspective model of consumer choice is shown in Fig. 13.2(a). From the model as depicted here, it is possible to derive hypothetical frameworks for the analysis of consumer choice. The first is a classification of operant consumer behaviors (Fig. 13.2b) which defines broad operant classes of consumer choice in terms of the pattern of reinforcement that maintains them. The second (Fig. 13.2c) adds the dimension of closed-open consumer behavior settings to this and defines eight categories of contingencies to which all consumer behavior can be functionally allocated. Although these are technically hypothetical interpretations of consumer behavior, they have proved remarkably predictive and robust in a wide range of empirical investigations: see Foxall (2017) for a summary.

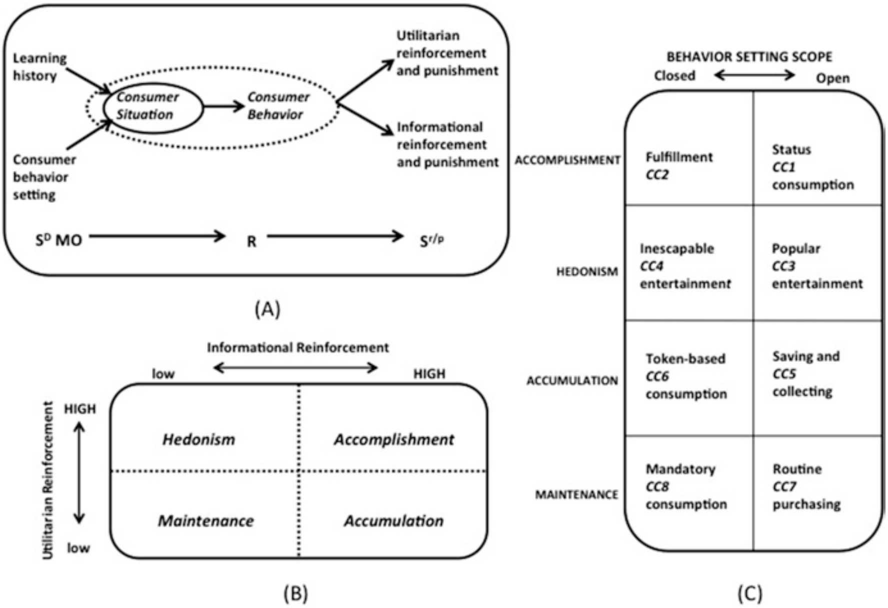

Fig. 13.2 Summary of the behavioral perspective model (BPM). (**a**) Summative behavioral perspective model. (**b**) Patterns of reinforcement and operant classes of consumer behavior. (**c**) The BPM contingency matrix. CC, contingency category. Note that all of the variables in the model and its derivative analyses are to be relatively comprehended, though for greater elegance of exposition the text refers simply to open and closed consumer behavior settings, high and low utilitarian and informational reinforcement, etc. See Foxall (2016b). *Perspectives on Consumer Choice: From Behavior to Action, From Action to Agency*. London and New York: Palgrave Macmillan

Accomplishment, maintained by relatively high levels of both utilitarian and informational reinforcement, consists of such consumer behaviors as taking high status vacations, being an early adopter of consumer innovations (in open settings), and attending personal development programs (in closed settings). *Hedonism* includes consumer behaviors maintained by relatively high levels of utilitarian reinforcement and relatively low levels of informational reinforcement. It is typified (in open settings) by watching television, attending movies, going to parties, or a springtime walk in the park and (in closed settings) by watching an inflight movie or listening to music while waiting on the telephone. *Accumulation* is consumer behavior maintained by relatively low levels of utilitarian reinforcement and relatively high levels of informational reinforcement: like saving up for a treat (open settings) and accumulating points for air travel (closed). Finally, *Maintenance* comprises consumer behaviors that are the result of relatively low levels of both utilitarian and informational reinforcement. In open settings, this might take the form of doing the weekly supermarket shop; in closed settings, this might take the form of filling out forms for a passport so that one can travel abroad.

In the extensional understanding of consumer choice, the mode adopted in theoretical minimalism, there is evidence that the ranges of behaviors encompassed by

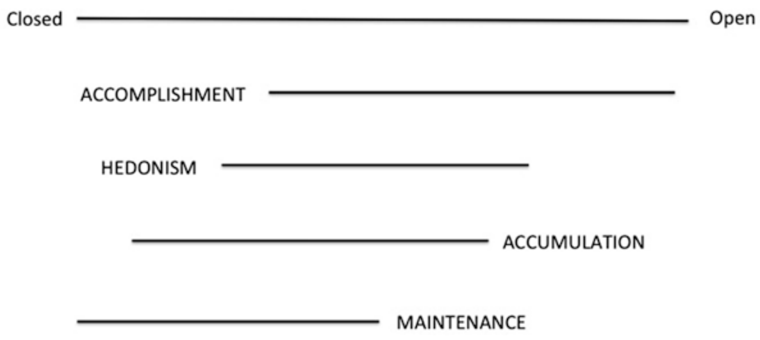

Fig. 13.3 Staggered ranges of consumer behavior setting scope. There is no attempt at definitive scale here, just an illustration of an idea. See Foxall (1999). The Behavioral Perspective Model: consensibility and consensuality, *European Journal of Marketing*, 33, 570–596

the continuum of consumer behavior analysis settings may not be of identical dimension for each of the four operant classes of consumer choice (Foxall, 1999). Figure 13.3 illustrates stylistically their relationships.

The extensional BPM has inspired a wide range of empirical research (summarized in Foxall, 2017) showing that consumers purchase quantities of utilitarian and informational reinforcement, maximizing specific bundles of these sources of functional and social benefit through the acquisition and deployment of products and services. Consumer behavior, in the sense of quantity demanded, is sensitive to changes in price but also to the amounts of utilitarian and informational reinforcement consumers acquire and consume. The operant classes of consumer choice defined in terms of pattern of reinforcement (Fig. 13.2b) and the contingency categories that appear in Fig. 13.2c are all instrumental in understanding how consumers respond to marketing variables that include not only price, to which economics largely confines itself, but the elements of branding that are central to modern marketing. Empirical research supports the structure and implications of the BPM and ensures that consumer behavior analysis is a body of knowledge that is highly germane to the development to the intentional and cognitive accounts toward which intentional behaviorism strives.[6]

Consumer Action

As has been mentioned, the intentional behaviorist research strategy has three stages: theoretical minimalism, intentional interpretation, and cognitive interpretation. These involve, first, the construct and testing of a model of consumer choice based on a behaviorist depiction that explains (predicts) consumer behavior in terms

[6] For an account of this phase of the research program, see Foxall (2017).

of the environmental stimuli responsible for the rate at which it occurs: the aim of this stage is to learn what this methodology can uniquely reveal as the mainsprings of consumer choice and also to identify the point (if any) at which this means of explanation breaks down and must be replaced by an intentional account. The stage of intentional interpretation demonstrates whether consumer activity that is not amenable to an extensional behaviorist explication can be satisfactorily accounted for in intentional terms. If this proves to be the case, the final stage, cognitive interpretation, critically examines the feasibility of the intentional interpretation as a means of understanding consumer choice. In the course of moving from the first stage of theoretical minimalism to the subsequent stages of psychological explanation, our subject matter ceases to be consumer behavior, a form of activity that is regulated by environmental stimuli, to consumer action which is conceived as resulting from the consumer's mental processes, including the perceptual and conceptual representation of the contingencies of reinforcement and punishment identified in the initial stage. A theme of the analysis undertaken in the present work is that all or virtually all modes of consumer choice involve a degree of akrasia or weakness of will marked by a tendency to reverse preferences over time in accordance with differing rates of discounting future rewards. This is an essential component of the definition of consumer choice.

Intentional Behaviorism

A Summary of the Fundamentals

The initial stage of intentional behaviorism, *theoretical minimalism*, is founded on the view that consumer behavior is shaped and maintained by its consequences, the reinforcers and punishers delivered by the products and services consumers acquire and the processes in which they consume them. Theoretical minimalism therefore entails building parsimonious, behaviorist models of behavior and testing them to destruction in order to ascertain the point at which an intentional account becomes necessary and the form it must take. When a satisfactory explanation of observed behavior cannot be made by treating it as a component of a set of contingencies of reinforcement that also includes discriminative and reinforcing/punishing stimuli, the point at which the stimulus field necessary for a behaviorist explanation is not empirically available to the researcher, psychological explanation becomes inevitable. This leads into the second stage, *intentional interpretation*, in which an account of the stimulus-free behavior proceeds by treating it as an idealized system which maximizes utility and ascribing to it the intentionality—desires, beliefs, emotions, and perceptions—necessary to render it intelligible. This idealized interpretation is cashed out in the third stage in terms of a *cognitive interpretation* that demonstrates how far cognitive processing can account for the intentional behavior proposed.[7] This three-stage procedure is the means by which intentional behaviorism interrelates the context in which consumer choice occurs—the physical and social surroundings, including temporal and regulatory influences, that comprise the stimulus field and the pattern of reinforcing and punishing consequences of behavior that regulate its rate of occurrence—to the cognitive concepts required for the explanation of behavior for which any such context eludes observation. In the course of turning to psychological explanation, the principal concern for consumer psychology has become to ascertain how the contingencies of reinforcement and punishment are subjectively processed by consumers prior to their acting, i.e., the explanation of consumer choice by reference to consumers' desires, beliefs, emotions, and perceptions.

Earlier expositions of intentional behaviorism have concentrated on desires and beliefs as the central intentional components of explanations of such behavior (Foxall, 2016a, b). In this chapter and the two which follow it, we expand this methodology by concentrating on perception and emotion. This progression is consistent with the fact that, while theoretical minimalism (leading to behaviorological explanation) is concerned with the individual's *behavior*, bodily movement or activity that results from what happens or has happened to her, psychological explanation is concerned with *action*, bodily movement which the individual performs.

A Little More Detail

Radical behaviorism is a vehicle for theoretical minimalism only so long as we are asking the basic question: what permits us to predict and control behavior, i.e., only as long as we believe that identifying the environmental stimuli that control a behavior is sufficient to explain it. We can then, at least in the closed settings of the operant chamber, discover the environmental stimuli of which behavior is a function. If, however, we ask what mechanisms would be required for an organism to respond in this way, i.e., if we seek to explain environment-behavior relationships, then we must seek a means of ascribing intentionality to the organism.

The psychological explanation that develops as a result of identifying the contributions and limitations of behaviorism has two stages. The first of these, which we have seen involves the development of an *intentional interpretation*, treats the consumer as an idealized utility maximizing system and derives the desires, beliefs, emotions, and perceptions that are required in order to account for its behavior.[8] The immediate criterion for the appraisal of this depiction is that it renders the behavior of consumers more intelligible and perhaps more predictable. The establishment of

[7] This two-stage process of psychological explanation both resembles Dennett's (1987) quest for explanation in terms of intentional systems theory and sub-personal cognitive psychology and differs significantly from it—see Foxall (2016b) for a comprehensive exposition.

[8] See Dennett (1978, 1987) for the basic methodology employed here. Foxall (2016b) treats this in detail showing where I adhere to and where I deviate from Dennett's program.

this intentional interpretation fulfills the quest for an intentional portrayal of the consumer situation: no longer simply the extensionally specified interaction of a learning history with a consumer behavior setting, the intentional consumer situation presents the framework of desires, beliefs, emotions, and perceptions that render observed consumer choice intelligible in the absence of the required stimulus field. This idealized portrayal of consumer activity is subsequently evaluated, in the course of the final stage of intentional behaviorism, which we have seen is the *cognitive interpretation*, by reference to its consistency with a broader cognitive interpretation founded upon empirical research on decision-making and action. The essence of psychological explanation, which includes both intentional and cognitive interpretations, is that it describes its subject matter using intentional idioms such as desires, believes, and perceives, as well as higher-order cognitive processes such as memory, information processing, and decision-making. Each of these intentional attitudes, as they are known by philosophers, is characterized by its being about something other than itself. The consumer desires a product, believes that she can find it at her local supermarket, and when she arrives perceives it on the shelf. Intentional objects do not necessarily exist other than as mental representations, Santa Claus, for instance, or the Golden Mountain: Brentano (1874) pointed out that intentional objects have intentional inexistence. By contrast, the extensional language in terms of which theoretical minimalism proceeds avoids intentional idioms of this kind.[9]

The theories that have provided cognitive interpretation to establish the intentional interpretation of the idealized consumer have taken three forms (Foxall, 2016b, Chaps. 8–10). The first draws upon the sub-personal realm that is the subject of neuroscience to establish the content of cognitive theories which have taken the form of dual-process theories of cognitive structure and process. This is the approach I have termed micro-cognitive psychology.[10] The second source of cognitive

[9] See, for instance, Chisholm (1957), Dennett (1969), and Searle (1983). In the context of intentional behaviorism, see Foxall (2016a, b).

[10] *Micro-cognitive psychology*. Stanovich's (2009) tri-process theory elaborates the dual-process models that separate mental processing into a system that relies minimally on working memory and which can respond rapidly to environmental events (often referred to as *system 1* or *S1*) and a system that draws heavily on working memory to produce behavioral alternatives based on consideration of the longer-term outcomes that will ensue (*S2*). I have described the tripartite theory as it may be applied in intentional behaviorism at some length elsewhere (Foxall, 2016a, b, c) and will only sketch it here. The minds posited by Stanovich and the relationships among them can be explicated in terms of a business analogy which, in the way of analogies, is not perfect but provides an initial outline. The reflective mind is the policy-making function which sets out the overarching goals of the enterprise, the styles of managerial behavior that will be employed to achieve them, the kinds of product the firm will bring to market, and the markets it will serve in order to succeed. The algorithmic mind is the strategic planning function which deliberates on how to achieve the objectives of the enterprise, the specific product markets it will enter, the composition of its marketing mixes, the permitted range of tactical behaviors it will adopt in pursuit of its strategic goals, and the product markets from which it will withdraw for the same reason. Finally, automatic mind represents the operational level of decision-making and action, which recognizes the opportunities and threats currently presented by the marketplace to which it can respond spontaneously by tacti-

theorizing appeals to the superpersonal realm of the reinforcing and punishing consequences of behavior which determine its frequency of repetition in order to set the content and form of cognitive explanation of behavior. A disciplinary base for this kind of cognitive explanation is found in theories of collective intentionality in which social groups determine for themselves what will act as reinforcers and punishers for their members. This approach I have termed macro-cognitive psychology.[11] The third approach, meso-cognitive psychology, provides necessary links

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cal action. The policy-making function of the reflective mind can overrule such tactics if it has the chance, issuing orders to the strategic function of the algorithmic mind which in turn proposes alternative courses of action and ensures that the operational level of business activities represented by automatic mind will conform to overall corporate objectives. If the operational managers of an actual firm were to respond automatically and on the basis of habit to every apparent opportunity presented by the marketplace, they might score some notable successes, but they would also on occasion land the company in deep trouble. Mostly, therefore, the managers responsible for this level are well briefed and well trained in following corporate policies, and there are mechanisms in place to ensure their conformity. The analogy is not quite accurate in this respect. Automatic mind is assumed *always* to attempt to operate on a more stimulus-response basis, automatically and autonomously reacting to the prospect of immediate gain by behaving on behalf of the entire enterprise. As may be the case in real-world business, the strategic and policy levels of supervision are not always able to countermand such behavior before it has occurred. Similarly, automatic mind's responses to stimuli must be monitored and, where necessary, either terminated before they have disastrous consequences or assuaged by complementary actions. The dual- and tri-process theories of cognitive processing brought to bear on these concerns propose that behavioral responding may be the outcome of either a mental reaction to environmental stimuli that is minimally controlled by working memory, sometimes called the impulsive system (or *S1*), or by a considered procedure in which alternative courses of action are comparatively evaluated and the one chosen that will be most effective in promoting the individual's long-term welfare (*S2*). This latter system is deliberative, sometimes known as the executive system, and may act by countermanding the impulsive system. Dual-process theorizing and research has figured in intentional behaviorism.

[11] *Macro-cognitive psychology*. This entails looking to social institutions for the sources of decision-making. Collective intentionality is an approach to the explanation of shared actions in terms of shared desires, beliefs, emotions, and perceptions. For Searle (1995), it involves deontology, the ascription of status positions and of the roles that are proper to them, and ascription of the rewards and sanctions that will be arranged to follow actions that are considered by the relevant group to be pro- and antisocial. The deontological aspect takes the form of rules that portray, usually verbally, the contingencies that connect specific actions to the situations in which their enactment will attract particular rewards and sanctions. (See Searle's exposition of collective intentionality: Searle, 1995, 2010. For other views, see, for instance, Tomasello, (2014, 2016). For further discussion in the context of intentional behaviorism, see Foxall, 2010a, b, 2016b.) The roles and actions specified in these rules require certain individuals to undertake particular functions for the execution of which they are accorded an appropriate status that is acknowledged by the entire community. Hence, a citizen who has fulfilled particular requirements such as having been successfully elected can be invested with the office of prime minister or president along with the authority and responsibilities that are deemed to go with it. Thereafter, social requirements are met by both the officeholder, who performs tasks assigned by the group and whose performance will be measured and rewarded or punished, and the rest of the community whose actions toward the person assigned to this role, such as due deference, are also laid down and rewarded or punished. Those assuming status functions enjoy *deontic powers* in the form of rights, permissions, and entitlements, but they also incur obligations and requirements (Searle, 2010). What this means is that social groups, acting collectively, have some capacity to invent for themselves the contingencies of reinforcement

among the sub-personal and superpersonal bases of these cognitive psychologies and the personal level at which behavior is conceptualized as well as the desires, beliefs, emotions, and perceptions in terms of which the behavior is explained. Theories stress the ways in which consumers' competing interests in short- and long-term satisfactions, such as Ainslie's (1992) picoeconomics, have been pressed into service for this level of exposition.[12]

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and punishment that will govern their actions, at least as far as the socially instituted and enforced informational reinforcement is concerned. (A society's capacity to influence the course of the contingencies involved in utilitarian reinforcement is, it goes without saying, more limited.) Our interest in this capacity of humans to construct contingencies and to specify the collective intentionality that will be expected of members of the social group to which they severally belong lies in there being no reason why the individual group member's mental processing of the deontic outcomes of collective intentionality should take the form only of beliefs-proper. Any individual, any given consumer, may equally fantasize about what is required in particular situations, form beliefs about what the rules are or how they may be fulfilled, and to what extent she will accord the necessary status position to those nominated to hold office. These beliefs may be accurate beliefs-proper insofar as they will lead to actions that are effective and lead to the individual's actions being rewarded and the social group as a whole prospering; if they are erroneous beliefs-proper, they may not have this effect immediately, but by their very nature such rational beliefs are likely to be soon corrected. Neurotic beliefs, however, i.e., fantasies that are reinforced by evidence gained only through psychic-reality testing, may be dysfunctional for both the individual holding them and the social group, depending on how centrally they affect the working of the group dynamic. There is a further dimension: the entire social group may entertain neurotic beliefs and religious or political ideas about how the world works that will also prove dysfunctional to the extent of causing the eradication of the society as a whole.

[12] *Meso-cognitive psychology*. The consumer's mental experience is not always a matter of the dispassionate weighing of beliefs and desires; often it takes the form of warring internal factions, what Ainslie (1992) envisions as strategically interacting interests whose distinct time frames lead to their propensity to conflict with one another. The short-range interest (SRI) seeks gratification when it is available even though it is inferior to that which is contingent on the deferment of consumption. This is the conflict between the SSR and LLR that we have already encountered. The long-range interest (LRI) is focused on the attainment of the superior but delayed reward. If we treat these interests in terms of the cognition and other intentionality they are likely to engender in the consumer, we may ask whether they represent beliefs-proper or neurotic beliefs. The intentional interpretations devised in the second stage of the employment of the intentional behaviorist research strategy must also cohere with the level of analysis at which this meso-cognitive psychology proceeds. There are several ways in which we can envision these two picoeconomic interests influence one another (Ross, 2012). Our understanding of their mutual effects reflects our assumption of whether they act contemporaneously or sequentially. Ross depicts contemporaneously interacting subagents of this kind as possessing either separate utility functions that are in conflict with each other or contrary time preferences. Each of these gives rise to its own style of economic modeling. For example, the actions of subagents with distinctly different time preferences can be related to their sub-personal neurophysiological functioning that governs their specific hyperbolic time preferences, a matter of the "competition between steeply exponentially discounting "limbic" regions and more patient (less steeply exponentially discounting) "cognitive' regions" (Ross, 2012, p. 720). This picoeconomic portrayal depends heavily on the findings of neuroeconomic experiments employing fMRI scans of humans choosing between SSR and LLR (McClure et al., 2004; for discussion in the present context, see Foxall, 2016a).

An Action Perspective

Bifurcating Consumer Activity

As a result of the theoretical thrust of the intentional behaviorist research program's having moved firmly into its second phase, that of showing what form a psychological explanation of consumer choice should take, it is now principally concerned with action rather than behavior. The adoption of an action perspective means that we are concerned primarily with activities that are performed by the consumer rather than something that happens to the consumer.[13] This focus raises a specific concern with the nature of the cognitive processes that mediate consumer action and their relationship to the ecology of consumption, something of central importance to consumer psychology. We concentrate, therefore, on how the contingencies of reinforcement come to bear on the behavior of the individual through the perceptual aspects of the felt emotion or affect that provide at least a component of her learning history. We are concerned with the contingencies as they exist within the mental processing of the individual rather than in the external environment. This entails forging links between the context of prior behavior and the cognitive framework within which it is perceived, processed, and comprehended and within which consumer action anticipates the context in which it is performed.

"Consumer activity" is understood as whatever consumers do, regardless of how it is explained. It is the activity of consumers as it would be observed by a nontheoretical watcher who took no pains to discover whether the activity resulted from things happening to the consumer or from mental operations occurring within the consumer. Consumer activity subsumes two further categories, consumer action and consumer behavior. Action is activity that is spoken of transitively, my moving my arm, rather than intransitively as my arm's moving or the moving of my arm (Hornsby, 1981). It is activity that I, the agent, bring about (i.e., transitive activity or activity$_T$) rather than something that happens to me (i.e., intransitive activity or activity$_I$) or which at least must be spoken of in these terms in the absence of a stimulus field to which the activity can be attributed. An implication drawn by some philosophers is that there is an agent (an "I") that is responsible for bringing about this movement and that the bringing about is accomplished or at least explained by mental means.[14] All of these implications of an action perspective require multifaceted philosophical discussion which is beyond the scope of this volume; the only definite implication of employing intentional language that I willingly embrace is

[13] The question of the nature of action is much more complex than my simple distinction suggests. See, as examples of some recent thinking, Dancy (2000), Hornsby (1981), Sandis (2012), the whole collection of papers in Sandis (2009), and Steward (2012). I have also discussed action and agency at greater length in Foxall (2016b).

[14] I discuss action and agency in the context of intentional behaviorism and the explanation of consumer choice at greater length in *Perspectives on Consumer Choice: From Behavior to Action, From Action to Agency* (Foxall, 2016b, particularly in Chaps. 7 and 11). See also my *Addiction as Consumer Choice: Exploring the Cognitive Dimension* (Foxall, 2016a).

that extensional language has failed to provide an explanation of observed behavior and the sole recourse is to intentionality. Making no ontological assumptions on the basis of this shift in explanation, I draw the conclusion that we have two languages, two modes of speaking about our subject matter, and two sources of explanation. But the fact remains that we have only one subject matter, what consumers *do*.

The activities we speak of intransitively, then, are behaviors; those of which we speak transitively are actions. Action and behavior can be topographically identical, and it is only as we seek to explain the activity, to trace its causation, that we switch from one appellation to the other. Behavior can be understood in terms of its biology, as is the case for taxa, or behaviorologically, as is the case for Pavlovian and operant conditioning.[15] There are more complex behaviors than these that we account for in broadly similar ways when we resort to the neurophysiology of the organism to find events that are indispensable to the performance of the behavior in question or that correlate with it in ways that make sense from our general biological perspective. We are justified in treating activities as behavior in this way if we can demonstrate that they are caused by, or a function of, environmental stimulation whether this arose, phylogenetically, in the course of evolution by natural selection or during the ontogenetic development of an individual. (See Fig. 13.4.)[16]

Action, then, is activity for which we are unable to establish antecedent stimuli that would account for it by making it amenable to prediction and control. Action must be accounted for in terms of the intentionality of the individual, her desires, beliefs, emotions, and perceptions, as we reconstruct them from our knowledge of her historical and current circumstances.[17] Having exhausted the explanation of behavior by reference to the extensional sciences of neuroscience and behavioral science, we have no alternative but to explicate any further observed behavior of the organism intentionally. Hence, while "activity" denotes either behavior or action, the topography of the activity in question may be identical whether it is viewed as behavior or action: only its explanation differs. Behavior, by contrast, *can* be traced to a stimulus field. It is only when the discriminative stimuli that would otherwise

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[15] For an account of consumer activity that treats it entirely as behavior, see my *Advanced Introduction to Consumer Behavior Analysis* (Foxall, 2017).

[16] Some authors argue that actions are the causes of activity rather than the activities themselves: so, for Dretske (1988), actions are the mental states that cause bodily movements. Others, e.g., Steward (2012), argue that there are both mental actions and physical actions. For Dretske, the content or meaning of a belief explains a movement by identifying why this mental state contributes to that movement. So the belief *that s is F* is a neurophysiological event (brain state) that, by virtue of its being selected in the course of operant conditioning, contributes to the causation of a movement because it carries the information *that s is F*. Operant selection of this kind provides the entity, here a brain state, with the function of providing the information, and this function confers upon the entity the status of being a representation. Acting "for a reason" in this way allows the explanation of the movement in terms of the content of the belief or other intention.

[17] The idea that we construct an intentional account by ascribing the mental operations the system "ought" to have given its history and circumstances is a vital component of Dennett's intentional systems theory (IST). See Dennett (1987). Intentional behaviorism makes important use of this idea in the construction of the intentional interpretation which is its second stage.

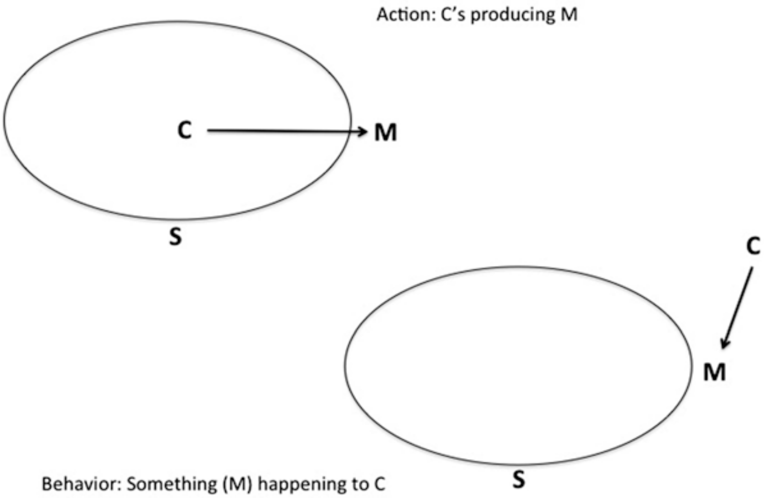

Fig. 13.4 Behavior and action. Action consists in the system's, S's, being the cause (C) of its movement: S is the origin of its own movement. This is *activity*$_T$: *S's* moving all or part of itself. Behavior consists in an external stimulus's causing S's movement: S is acted upon to move in a particular way. This is *activity*$_I$: S's being moved. See Dretske (1988, p. 3) but note that my depiction and terminology differs from his

account for an observed behavior cannot be located that the observed activity is designated *action* and a psychological explanation becomes necessary. Activity for which no such setting variables are apparent requires an intentional explanation. We have no alternative but to go beyond the extensional explanation of behavior because the necessary basis of such an explanation is absent.

Consumer Choice

Our focus on action as opposed to behavior has an important consequence. The term "consumer choice" is often used synonymously with "consumer behavior" or "consumer activity," but, in view of the emphasis on action, I should like to define it more closely. Consumer *choice* is marked by a degree of conflict between present and future activities. Should the consumer make purchases at a local convenience store which will charge more but which requires less personal effort than going to the more distant supermarket? Is the consumption of alcohol justified if the expense means eating poorly? Should the student opt for a sports event and so have less money to buy books? Each time the consumer chooses the first option, a longer-term and potentially bigger goal has to be displaced. Shopping at the supermarket, even if this requires walking there, will leave more money at the end of the week. Nutritious food is expected to ensure both a healthier life and a longer life: perhaps a single instance in which one selects an alternative product will make no difference to either, but a *pattern* of choosing the more immediate payoff may well do so. The student's future depends on studying now and therefore access to books; once again, attending one sports event may not interfere much with this, and it may even enhance the process of studying by providing a necessary diversion. But an extended sequence of such choices is likely to impede progress elsewhere. Indeed, it is this temporal conflict that transforms consumer activity, be it behavior or action, into consumer *choice.* Sometimes immediacy seems to be the sole criterion in securing the opportunity to consume. The addict's craving a substance or a behavioral outlet is a case in point even if the satisfaction of the longing fails to bring pleasure. At other times, it is relatively easy to pace our consumption. In both cases, however, it is the management of the temporal dimension that enables us to speak of consumer choice.

A Degree of Akrasia

A recurring feature of the patterns of action which illustrate consumer choice as they extend over time is the reversal of preferences. The choice inheres not just in the objective alternatives available to the consumer in the form of different external rewards: it is to be found also *within* the individual who values the rewards in varying ways over time. (Individual differences in propensity to temporal opportunities and demands stem in part from variations in learning history, personality, and neurophysiology.) It is easy enough to resolve at the start of the day that you will take a healthy walk to the supermarket and save some money, or study all day without distraction, or forgo more immediate temptations in order to eat well. As lunchtime or the study period or the opportunity to spend a relaxing evening over a drink approaches, however, it is only too easy to switch preferences in favor of the less demanding option that is currently available, even in full knowledge that this will cause a more significant long-term goal to recede further. Sometimes, we modify our preferences yet again, regretting having taken what now looks like the easy option and the consequent loss of a larger reward. Tomorrow, therefore, the cycle may well begin all over again.[18] Some consumers do, of course, choose the delayed but superior option and do so consistently. The point, however, is that most, if not all, consumer choice *can* invite weakness of will or akrasia understood as the selection of a smaller reward that is available earlier over a larger reward that will not appear for some time.[19]

[18] In the case of compulsive and addictive consumer actions, it may seem to stretch the point to speak of choice at all, but the same pattern of preference reversal, now accompanied by a striving to overcome the problem—both of which open the pattern of behavior to the charge of economic irrationality—is apparent. The possibility of at least delaying consumption remains, and the many instances in which individuals overcome addictions are testimony to the use of the term choice based as I have suggested on the underlying temporal conflict involved.

[19] See Radoilska (2013) for an interesting distinction between akrasia and weakness of will.

In affluent marketing-oriented economies, where levels of discretionary income run at very high levels, being able to choose immediate consumption rather than delay gratification can exert a strong influence on the pattern of consumer choice. As McGinn (2006, p. 50) comments, "Weakness of will is easy; it is explaining it that is hard."[20] Such behavior is said to be impulsive and is contrasted with that which ignores the immediate but inferior reward by waiting for the superior alternative, thereby exhibiting self-control. Let's not exaggerate: impulsivity does not necessarily lead to serious deleterious effects—indeed, sometimes it is a necessary interlude, adding the spice to life—but if it persists then, in broadly conceived economic terms, it can be, to say the least, suboptimal. Moreover, an intriguing facet of this kind of choice is that it is not necessarily practiced by people who are generally irrational; as Searle (2001, p. 10) puts it, "…akrasia in rational beings is as common as wine in France." But it may entail psychological as well as economic irrationality as well as a-rationality. The exploration of this theme is a central concern of this book because it is closely related to the perceptual means by which consumers evaluate the contingencies of reinforcement and punishment and to the desires and beliefs that shape perceptual experience.

This emphasis reflects that consumer behaviors form a continuum that ranges from the routine, everyday, and commonplace to the extreme, compulsive, and addictive (Fig. 13.5). The selection of a brand of a frequently used foodstuff is, at least in affluent marketing-oriented economies, an example of the former: it involves the choice of a tried, tested, and trusted item and takes place in a context of minimal uncertainty. Even this example of a consumer action may not be entirely

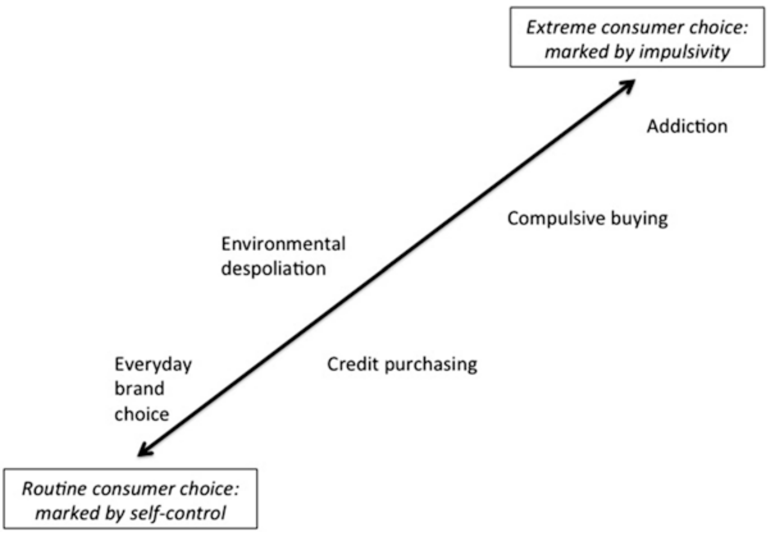

Fig. 13.5 The continuum of consumer choice. *Source*: Foxall (2010a)

[20] Indeed, for Plato, who argued that the individual who knows what is good is incapable of acting otherwise. See also Davidson (2001).

conflict-free since it may require that other purchases are delayed and raise considerations that the item purchased may not be strictly necessary given one's budget or that it might be available at a lower price at another store requiring travel. However, for the most part, routine purchasing involves a minimum of conflicting demands. At the other pole of this continuum of consumer choice (Foxall, 2010a, 2017) lie the severe compulsive consumer choices involved in addiction. Addiction defies a single-sentence definition, but it is likely to involve preference reversal, perhaps to the extent of where economic irrationality, and beyond that leading to the loss of friends, spouse, home, or job. The maladaptive behavior involved may also be exacerbated by neurophysiological overactivity (see, for instance, Ross et al., 2008; Foxall, 2016a).

Between these polar extremes, there are consumer actions such as purchasing on some form of credit because it brings consumption forward in time even though it exacts a potentially disruptive price in terms of the eventual repayments that must be made; despoiling the environment through waste disposal or using limited resources such as fossil fuels, which reduce the costs of consuming in the short term but which may be responsible for the consumer's incurring more pervasive expense at a later date; and compulsive shopping in which the immediacy of ownership is often divorced even from consumption since the outcome may well be hoarding rather than use. Apart from routine consumer choice, all of these consumer behaviors entail paying more, sooner or later, for the convenience of consuming at once, and even everyday purchasing as we have seen is open to these considerations. Moreover, as the analysis will show, routine consumer choice is wide open to the influence of the social, economic, cognitive, and neurophysiological factors that shape its more extreme versions. There is potential for *a degree of* akrasia to be present in most if not all consumer choice.

Is this to say that all or virtually all consumer action is akratic? By no means. *Addiction as Consumer Choice* (Foxall, 2016a) initiated discussion of akrasia in the context of the theory of consumer action, concentrating on the more extreme aspects of consumption that are marked by akrasia. An emphasis there is on the *economic* irrationality of extended akratic choice, and the theoretical developments to which this analysis led were generalized to the broad spectrum of consumer choice, from everyday purchasing to addiction, in *Perspectives on Consumer Choice* (Foxall, 2016b). It is hardly the case that all or even most consumer action involves weakness of will on the scale encountered in compulsion and addiction. But some *degree of* temporal discounting is common, and a good deal of consumer choice reflects psychological irrationality and a-rationality even if it is free of economic irrationality. The present work seeks, therefore, to understand better the *psychological* rationality, irrationality, and a-rationality by which consumer choice may be explained. It represents, as we shall see, a further development of the intentional behaviorist approach to the cognitive explanation of consumer choice.

Consumer choice that lacks rationality in either an economic or a psychological sense is apparent in the extremes of addiction and compulsion. Addictive consumption, for instance, involves steep temporal discounting: the addict strongly prefers immediate satisfaction to the longer-term benefits of abstinence. In addition, addicts may display economic irrationality, as when they spend large amounts of money on trying to overcome their addiction (e.g., on specialist programs and courses) only to relapse at an early opportunity. Severe addiction can be marked by disruptions to the addict's lifestyle: loss of some or all of their livelihood, home, friends, and spouse. Some of these elements of addiction, particularly the compulsion to consume immediately and repeatedly, are likely to be exacerbated by neurophysiological events that generate exaggerated rewards for the continued pursuit of the actions resulting in addiction.

It is tempting to concentrate on the role of economic rationality in consumer choice, partly because it is easier to identify objectively than are deviations from psychological rationality which may be subjective and hidden. We often hold desires and beliefs that would not be borne out if they were checked against reality, and we seek evidence that seems to bolster these mental propositions but which must be spurious. We do not necessarily advertise these thought processes to the world. The so-called gambler's fallacy, for instance, is the belief that a run of losses must be assuaged by a large success on the basis of the "law of averages." Nicotine addicts smoke what they believe to be their "last cigarette" many times. And we are all prone to the notion that making a New Year's resolution to eat less and get fit will change our behavior. In each case, such beliefs begin with a fantasy: of a large win, of a tobacco-free lifestyle, or of a future self who effortlessly pursues a lifestyle marked by eating more moderately and exercising more strenuously. Spurious evidence can be adduced in favor of all of these fantasies (see Brakel, 2001, 2009): perhaps to the effect that the betting odds have shifted in the gambler's favor, that giving up tobacco, overcoming excessive eating, and working out can be accomplished simply by summoning willpower or obtaining a gym membership. Change is possible of course, but it is not rationally predictable on the basis of these beliefs, especially if the consumer has a history—as many of us have—of repeatedly trying and failing. Although they look like genuine beliefs that guide action rationally, these fantasies-plus-evidence that never come into contact with reality may be no more than wishful thinking.

There are also consumers who are not addicted but who are open to fantasies with serious consequences not only for themselves but also for others whom they love. For example, some parents avoid immunization for their children in the belief that it will harm them. They do this in the face of demonstrated medical-scientific evidence that inoculation is actually efficacious and that rejecting it puts their own and other parents' children at risk. They may well have reasons for their behavior: information gained online from dubious sources and masquerade as "evidence" for views which are never tested against reality.[21] Less dramatically, many consumers are influenced by the strong claims of advertising and special offers and consumer deals to over-purchase and/or overconsume, backing up their actions with beliefs, not always conscious, about the social acceptability and functional benefits but especially of the self-enhancement that will follow. These consumer actions do not

[21] For a recent examination of this kind of social phenomenon, see Nichols (2017).

necessarily reflect psychological *ir*rationality, which would require consumers' adopting practices that evince a deliberate negation of common sense, but they can involve a degree of *a*-rationality where fantasized goals are shorn up by evidence that is not reality-tested.

Now, as long as one avoids seriously compulsive or addictive behaviors, this may not matter. Being somewhat lax in one's devotion to economic theory or psychotherapeutic demands is hardly a crime. But there are two instances in which attention to these deviations is called for. The first is that for addicts and near-addicts these are real problems and a theory of consumer choice should seek to understand them better. The second is that an explanation of consumer action that relies on the assumption of a high degree of cognitive rationality ought to be interested in consumer choice that is not so characterized, whether to a large or moderate extent. Many philosophical and social-scientific models of decision-making, for instance, assume a rationality that is seldom encountered in reality. The research strategy that has been adopted in devising my own explanation of consumer choice, intentional behaviorism, relies at one point, though not ultimately, on the assumption of rationality. It employs the concept of the *intentional consumer situation* as a theoretical construction that treats the consumer as a rational utility maximizer to whom, given her learning history and current circumstances, appropriate intentionality can be ascribed in order, first, to render her observed actions more intelligible and, second, in order to demonstrate that an intentional/cognitive explanation of her actions is feasible. Beyond this, it reverts to the possibility that consumer choice may not reach the requirements of optimality, but the temporary assumption of this behavioral objective nevertheless requires further attention.

At this point, it is useful to summarize the nature of akrasia in more formal terms as it is dealt with in the three stages of intentional behaviorism.[22]

Consumer Decisions in Temporal Perspective

The following account of how choice is explained, first, by behaviorists and, subsequently, by cognitive psychologists is presented as an example of how intentional behaviorism's strategy of theoretical minimalism identifies the need for an intentional explanation. As Malcolm (1977, p. 89) notes, radical behaviorism is "essentially a philosophical doctrine" that is continuous with physicalism, the view that psychology can be formulated in terms that describe physical entities (Carnap, 1959, p. 165). Psychological laws are therefore a kind of physical law, and the meaning of a sentence is its means of verification. In intentional behaviorism, this perspective is an essential starting point for the identification of when non-physicalist, intentional explanation is required and the functions it must perform.

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[22] A more comprehensive account is available in Foxall (2016b, Chap. 2).

This account centers on the activity of the consumer whose behavior exhibits a degree of akrasia and examines how she decides between immediate and delayed behavioral outcomes, be they reinforcing or rewarding and aversive or punishing. Much consumer choice entails decisions of this kind, from the preference for having a product now even though this requires a larger eventual payment for the privilege, through the disposal of waste and consumption of fossil fuels for which subsequent consumers, perhaps generations of consumers, will incur the full costs, to compulsive shopping and the familiar addictions to substances like alcohol and behavior patterns like excessive gambling.[23] The underlying distinctions among these modes of consumer choice derive from the extent to which consumers discount the future consequences of their activities. Only the most commonplace everyday purchasing of familiar brands may escape a degree of akratic preference, and even here there is the constant choice of how to obtain the funds to pay for them or whether to forgo them in order to make longer-lasting choices. I do not think it is far-fetched therefore to select this exemplar; insofar as all consumer choice entails the allocation of scarce resources among competing ends, it is all concerned with trade-offs between apparently superior and apparently inferior outcomes.

Theoretical Minimalism

Radical behaviorism avoids intentionality as a route to explanation and thus provides an ideal conceptual basis for theoretical minimalism. Hence, choice, in this paradigm, is relative rate of responding. Current accounts of akrasia, in which an individual chooses between a smaller reward available sooner (SSR) and a larger reward that will not become available until later (LLR), propose comparative evaluation of these alternatives, which appear respectively at t_1 and t_2. Moreover, they entail that these evaluations occur, first, at t_0 and again at t_1 when the selection of one option excludes the possibility of the other. The resulting explanation, which involves the decision-maker in the comparison of representations of the choices, is not a radical behaviorist explanation since it relies on the symbolic manipulation of information, mentally, neurophysiologically, or in private or public verbal behavior. Rather, it is cognitive. We need to consider how the distinct paradigms presented by radical behaviorism and cognitive psychology deal with behavior change that entails the substitution of one pattern of behavior for another. What are the elements of a radical behaviorist explanation of the kind of behavior change that is the goal of the strategies that are commonly advocated as means of overcoming akrasia?

As we have seen, the essence of radical behaviorist explanation is the insistence on extensional language to describe its subject matter and the corresponding avoidance of intentionality (Foxall, 2004). While this may not be formally enshrined as a

[23] Accounts of the *Continuum of Consumer Choice*, which summarizes this idea, can be found in Foxall (2010a, 2016b, 2017).

principle of radical behaviorism (see, however, Schnaitter, 1999), it seems to me to be its defining mark. Someone who appreciated this was B. F. Skinner (1971) who took pains to point out, for example, that when we say the fisherman spreads nets in order to catch fish, we are simply alluding to the order in which these operations occur, rather than to a pre-behavioral resolve to attain a mentally conceived goal through carrying out a causal act. In line with this, choice in behavior analysis is behavior or rather the relative rate of responding, not a matter of mental deliberation. As de Villiers and Herrnstein (1976, p.1131) put it, choice is "behavior in the context of other behavior." There is, accordingly, no room in radical behaviorism for the notion that behavior is a function of representations—in mind, or in verbal behavior, or in neurophysiology—for representation invokes intentionality in the explanation of behavior (Schnaitter, 1999).

The pre-behavioral mental representation of the environment is actually the central feature of cognitivism, where it includes both relatively simple perceptual and complex symbolic processing (de Gelder, 1996). This presents a quite distinct approach to explanation from that of behaviorism in which the effect of the environment is direct, unmediated by representations. In radical behaviorism, behavior is a function of the external reinforcing and punishing stimuli that have previously followed similar responding. The essence of this psychological paradigm is its insistence that behavior is a function of environmental variables rather than intentionality and that internal states other than physiological events (which can safely be left to the physiologist) play no part.[24] The control of behavior exerted by external stimuli can be fully described in extensional language and resort to description in terms of beliefs and desires, the stock-in-trade of intentional explanation is superfluous (e.g., Skinner, 1950, 1974). The crucial matter, as Compiani (1996, pp. 46–7) notes, is that "[t]his reasoning exclusively in terms of external parameters (stimulus and response) assumes that the processing by the system does not add anything at all to the information content of the input; that is, the performance of the system can be completely characterized externally without recourse to the internal properties of the system."

There is no place here for explanation in terms of intrapersonal desires and beliefs, attitudes, or intentions, information processing, or decision-making. My theme is how this squares with the explanation of akrasia, weakness of will. I shall examine the implications for explanation of an influential approach to akrasia which proceeds in terms of hyperbolic discounting, which I believe is ultimately cognitive, and compare it with a genuine behavior analytic approach to explanation. I shall not identify a specific source of this mode of explanation since it is pervasive in behavior analysis.

[24] Although it was always Skinner's position that the behaviorist is compiling an agenda for the research program of the physiologist by demonstrating the environmental determination of behavior, other behaviorists are more actively engaged in research that entails the neurophysiological substrates of reinforcement.

Hyperbolic Discounting

Akrasia is frequently analyzed in terms of hyperbolic discounting. In Fig. 13.6, t_1 is the initial choice point, at which Ego may choose either to take a smaller, sooner reward (the SSR) or to wait for a larger, later reward (the LLR) which will be available at t_2. At t_0 the larger reward is said to be valued more highly than the smaller. What can this mean? Neither the SSR nor the LLR is empirically available to Ego at t_0. Where can they exist in order to be evaluated? They can only exist, in a behavior analytic account, in either (a) Ego's learning history or (b) the rules with which she has been presented (or has devised for herself).[25] To say they are in the contingencies overlooks the fact that the contingencies are not empirically available to her at this point. Neither formulation actually determines where the SSR and the LLR are. If Ego's learning history is a means of predicting her future behavior, that learning history must be available to the investigator, who is making the prediction, as an

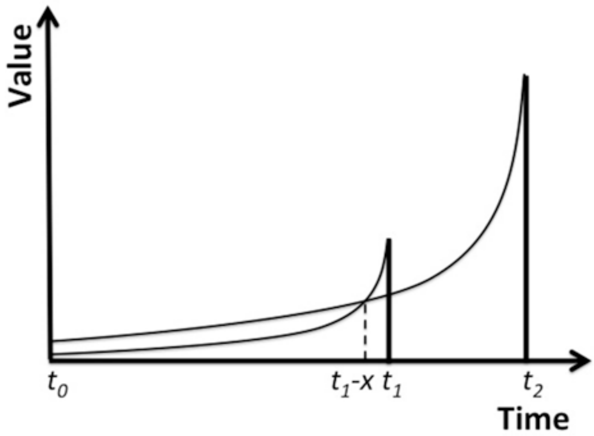

Fig. 13.6 Hyperbolic discounting and the point of decision. *Source:* Foxall (2016a)

[25] Skinner (1969) makes this important distinction. Contingency-shaped behavior is that which is explained by reference to its concomitant stimulation. An S^D or MO sets the occasion for the performance of a behavior that has previously been reinforced in similar settings. On the basis of knowledge of this stimulus field, the behavior is predictable. Most important from the point of view of intentional behaviorism is that no representation is involved in the explanation of contingency-shaped behavior. Rule-governed behavior is explained by reference to the verbal behavior of an instructor (who can be the behaver herself), giving rise to a distinction between *other*-rules, provided by another person, and *self-rules*, worked out by the individual for herself. The verbal statement is said to specify the elements of the three-term contingency: as in "When you are in the store [consumer behavior setting comprising S^Ds and MOs], please pick up some eggs [response, R], and I will make you your favorite dessert [verbal MO relating the response to a reinforcer]." The only way in which a radical behaviorist can keep such an explanation within the bounds of the operant paradigm is by assuming that the words are S^Ds or MOs that influence behavior by virtue of their having been paired repeatedly with reinforcing or punishing behavioral outcomes. If this is done, there is again no question of representation entering into the explanation.

inventory of the behaviors Ego has emitted in similar circumstances and their reinforcing (and punishing, though I am not concentrating on these) consequences: the SSRs and LLRs to which they have led. The learning history is not necessarily available to Ego in this form. But there is assumed to be a learning history of which Ego's future behavior is a function. There are behavior analysts who argue that learning history is the only explanatory variable we need to explain behavior.

But knowing these things does not allow us to locate the SSR and the LLR at t_0 in order that we can value them or, rather, know to what precisely what Ego is valuing. Whether we use Ego's learning history or Ego's rules to predict her behavior, what we actually have is a representation of the SSR and the LLR either in our inventory of her prior choices or in the track, ply, or augmental. This may enable us to predict, but it is not useful to explanation. Is the representation of which Ego's next response is a function in her memory of her past behavior and its consequences, in her neurons, or in her internal verbal behavior, perhaps as she privately repeats the rule to herself? Ego can only value the SSR and the LLR if she has a representation of them. Even if she relies on a written record of her learning history or of the rule, her comparison of the SSR and the LLR and their comparative evaluation must take place in verbal behavior, private or overt.

A similar set of circumstances obtains as Ego approaches t_1. We are told that just before the SSR becomes empirically available, its value to Ego rises dramatically, exceeding that of the LLR, and that, therefore, the SSR is likely to be chosen at t_1. Again, the SSR is not empirically available at this point – t_1 - x – but Ego is said to value it relative to another reward, the LLR, that is equally unavailable. If it is necessary to assume that the imminence of the SSR must be signaled to Ego for the closeness of t_1 to change her valuation, and if this provides some substance to its existence, it still follows that it must be compared with the as yet immaterial LLR. If Ego does not take the SSR but waits for the LLR at t_2, she is said to value the LLR higher than the SSR throughout the period under review and therefore to discount exponentially (since there is never a point at which SSR is the more highly valued).

A purely descriptive behaviorist explanation of this behavior is feasible. We can say that the impulsive consumer values the SSR more highly than the delayed LLR at t_1 and that this is an inference from, a redescription of, her actual behavior at t_1. The behavior of selecting the SSR is the valuing; the valuing is the behavior. This avoids the question of how Ego evaluates the SSR in comparison with the LLR except via a representation of the latter because it aims simply to understand the frequency of Ego's choice of the SSR as a proportion of her total number of choices of the SSR and the LLR. Choice is then defined as this rate of relative responding. This is a genuine radical behaviorist explanation since it avoids intentional idioms and is concerned only with behavior. Admittedly, it consists entirely in post hoc description, but the relative frequency of responding so obtained could be used to predict further choice. Moreover, its purpose is solely to predict and control Ego's subsequent behavior on the basis of a reconstruction of her learning history.

It may be useful to reiterate how distinct this explanation is from that which the literature of akrasia often puts forward. What is being claimed in the latter is that (a) Ego values the SSR and the LLR first at t_0 where the LLR is the more highly valued,

(b) again when the SSR and LLR curves cross just prior to t_1, at $t_1 - x$, that this is the time when Ego comes to value the SSR higher than the LLR. Comparative evaluation of this kind must take place in her private or public verbal behavior. By contrast, the radical behaviorist interpretation precludes Ego's valuing either reward at t_0 and in the interval between t_0 and t_1 because there is no opportunity for her to behave with respect to selecting one or other of the choices. Unless, that is, we take any overt verbal behavior of Ego's into consideration. If Ego tells us at t_0 that she values the LLR more highly than the SSR, is this choice behavior? Is this behavior the valuation, her comparative valuation? If so, on what is it based in the face of the SSR's and the LLR's not being empirically available and presenting themselves to Ego only in the form of representations in her learning history or rules? And if her public verbal behavior would count as her comparative evaluation, why shouldn't her private verbal behavior? We are not, after all, predicting her *behavior* now, in which case her private verbal behavior would be of no use to us since it would not be empirically available. We are only trying to understand, explain, why she behaves as she does.

The difficulty with this, from a behavior analytic point of view, is that the valuation is comparative, between the SSR and the LLR. Now the LLR can be no more than a representation at t_0 and t_1, and the SSR is a representation at $t_1 - x$. How does a representation enter into comparative evaluation if it is not a mental (private) representation? To speak in terms of representation means admitting intentionality into one's paradigm and, more than that, to ascribe causal significance to the representation. Ego's behavior in choosing the SSR earlier or waiting for the LLR must be a function of their comparative evaluation. If we say that the SSR and the LLR are representations in Ego's private verbal behavior at t_0, and between then and t_1, and that thereafter the LLR remains a representation in Ego's verbal behavior, then we make private verbal behavior intentional. If we say that the valuation takes place at $t_1 - x$, then this representative nature of verbal behavior is only confirmed. In any case, the spoken expression "I value…," whether private or public, is itself intentional. To value is a transitive verb. We always value something, and therefore our valuations are necessarily about something, intentional. These are not locutions I associate with radical behaviorist explanation.

The distinction between the explanation to which I am drawing attention and that which is a genuinely behavior analytic explanation boils down to the former's molecular analysis of choice, which ignores sequences of responses and reinforcers, and the latter's understanding of choice as a molar pattern of behavior and its consequences. We can describe the relative frequencies of behaviors that lead to the SSR and compare them to the frequencies of behavior the outcome of which is the LLR. That way we can confine our analysis to the behaviors that actually eventuate at t_1 and t_2 and ignore t_0 and the interludes among the temporal points of interest. But the question arises: how are these sequences of behavior inaugurated and how do they change? If learning history is determinative, how can behavior ever deviate from the established patterns it imposes? Yet the behavior of people who have

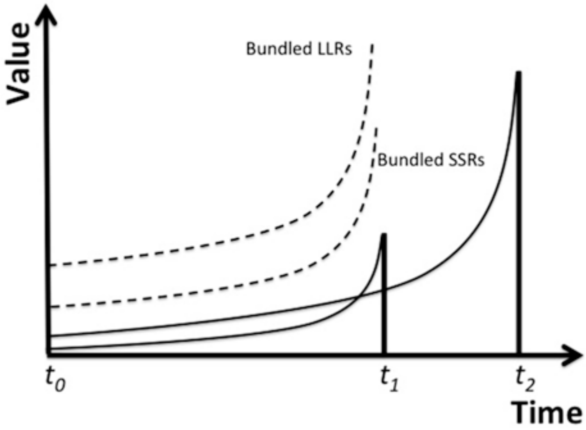

Fig. 13.7 The principle of bundling. *Source*: Foxall (2016a). *Addiction as Consumer Choice: Exploring the Cognitive Dimension*. London and New York: Routledge

apparently habitually chosen the SSR sometimes is reoriented toward long-term selection of the LLR. How does this come about?

Behavior Change

There are three sources of behavioral change: new contingencies, new rules describing contingencies, and new strategies of comparative evaluation. New contingencies need to be discovered in vivo—through exploration, we find out that the arrangement of actions and rewards has changed: for instance, that what was the LLR is now delivered at t_1, while what was the SSR arrives at t_2. This is not, however, the problem of akrasia which is our concern. Such a contingency would in any case induce exclusive choice of the SSR at t_1, and the question of comparative evaluation, however we define it, would be instantly resolved. Taking a behavior analytic perspective, we could simply monitor the behavior of Ego after the contingencies governing her behavior are modified and determine how the sequences of responses and reinforcers are functionally related. This raises interesting questions for research: Would we encounter inflexibility in her behavior after the contingencies had changed, how long would it take for the new contingencies to become operative, and how would we explain any insensitivity to the new schedule? New rules leave us in the original quandary of how to explain behavior that is a function of representations.

Strategies are means by which the akrates seeks to change behavior by modifying either the contingencies themselves or how she is thinking about them in order to ameliorate her so-called weakness of will. Most of them take on board the necessity of envisioning behavior in a molar fashion. Bundling, for example, entails bringing all the future outcomes of a stream of choices between the SSR and the LLR to a point prior to t_1 (Fig. 13.7). When the akrates is able to contemplate the sum total of SSRs and LLRs, she might reap in the course of an entire sequence of responses. While, for the hyperbolic discounter facing a single instance of choice, the SSR > the LLR at t_1, if we bundle future rewards, the sum of the LLRs > that of the SSRs. This makes it easier to commit to the sustained pursuit of LLR, and having made this selection once it becomes more probable that Ego can continue to do so on succeeding occasions of choice. But where does this bundling take place? It can only be in Ego's private or public verbal behavior. Indeed, I would venture to say that the complexity of the task of comparative evaluation which it involves would make private verbal behavior essential whether or not as a prelude to public verbal behavior. However, the required verbal behavior of whatever kind would need to be capable of holding representations of the as yet nonexistent rewards as well as calculating their relative values and of engendering consequent overt behavior. The same conclusion must be reached in respect of the other avoidance strategies. Each requires consideration of a future set of circumstances and the behavior they will generate.

Can a behavior analytic explanation be advanced for the development and implementation of strategies such as bundling—one that avoids reference to representation? We are again faced with the proposition that the decision-maker must contemplate a set of hypothetical eventualities, compare them critically, and reach a conclusion about the most rational behavior to pursue.

Psychological Explanation

This account of behavior and behavioral change stands in contrast to those offered by radical behaviorism. What form would that behaviorist explanation take? Let us put aside the possibility that thought and language are inherently intentional since they are inevitably about something other than themselves, which aboutness is what separates explanations based on them from extensional description. Let us accept that elements of an individual's locutions, private or public, may act as discriminative stimuli for her behavior. This assumption again begs the question whether discriminative stimuli are themselves intentional when they enter our explanations of behavior since they might be said to be about reinforcement or punishment. Let us put this aside too.

Let us suppose that a (more or less) akratic consumer, Ego, who, in order to escape the consequences of the over-frequent selection of the SSR, adopts the bundling strategy. Suppose further that Ego did not come upon this idea as a result of its spontaneous generation within her psyche but was informed of it by either the spoken or written word—that is, as a series of tracks and/or plys, perhaps with the odd augmental thrown in. How do these rules function to alter Ego's behavior? She must somehow compare the outcomes of her own sequence of past behaviors with the promised outcomes of a sequence of alternative future behaviors, calculating their values at the present moment, selecting one and resolving to adhere to the novel program of behavior, or—as Ainslie (1992) puts it—making a side bet with himself or herself that success will follow. We have noted that, according to this manner of explanation, Ego cannot do any of these things other than through the manipulation of representations.

If we wish to avoid this cognitive explanation, we must identify the potential discriminative stimuli and motivating operations inherent in the rules she has been given and, ultimately, show how they are consistently related to her behavior, how in other words they can be accepted as elements in three- or four-term contingencies. Such an extensional behavior analytic explanation requires that the elements inherent in rules—the sentences, phrases, words, and even phonemes within words—assume the role of discriminative stimuli and motivating operations, understood as components of the environment in the presence of which certain behaviors have been reinforced in the past. These elements cannot however assume these roles in our explanation until Ego has acquired a learning history in which they have figured and in the course of the acquisition of which they have assumed stimulus control over her behavior. We cannot just assume that because these rules are intended to persuade Ego to behave in new ways, they do so by embodying these elements of a behavior analytic account. It does not appear that there is any alternative to taking a cognitive line here.

Intentional interpretation is a depiction of the consumer treated as an idealized system that maximizes a utilitarian and informational reinforcement within the confines of her budget. The assumption is that she will do so by seeking the LLR and that doing this will be her intention at t_0. Taking the decision (at t_0) to accomplish this maximizes utilitarian and informational reinforcement at this time by optimizing the intrinsic benefits of acting consistently with one's highest good. This is consistent with her self-image and maximizes her self-esteem, again at t_0. Suppose that at t_1, however, the consumer opts to consume the SSR. In the idealized interpretation, she still maximizes utilitarian and informational reinforcement, taking the highest level of utilitarian satisfaction available at that time as well as exercising personal power of personal decision, a source of informational reinforcement. If the consumer waits patiently for the LLR at t_2 before consuming, however, she maximizes by selecting the highest level of utilitarian reinforcement at that time and also obtaining the highest level of informational reinforcement by reaping the satisfaction of having exercised self-control.

The rationale of the cognitive interpretation is to ascertain whether the intentional interpretation can be cashed out at the level of cognitive processing. Micro-cognitive psychology suggests that more impulsive decisions result from a neurophysiological system based on the limbic system, while more self-controlled decisions emerge from an executive system based on the prefrontal cortex (PFC). The relative levels of activity found in these systems determine the rate at which the individual discounts future rewards. This is consistent with the intentional interpretation: a hyperactive impulsive system coupled with a hypoactive executive system indicates a tendency toward behavior that manifests in a higher rate of discounting and selection of the SSR; conversely, a hypoactive impulsive system coupled with a hyperactive executive system is likely to eventuate in the exercise of self-control demonstrated by choice of the LLR. The intentional interpretation also squares with the kind of explanation generated by macro-cognitive psychology in terms of the collective intentionality developed within groups that evolve deontic rules that prescribe socially acceptable actions. Such actions as being patient, showing restraint, and allowing others to make choices first are generally socially rewarded, while impatience, butting in, and precluding others from making choices are proscribed and punished. The individual's history of reinforcement and punishment, including the experiences they attract as a result of the degree of conformity with rule-following or rule-breaking their actions evince, account for individual differences in the effectiveness of collective intentionality on their behaviors. Linking these two levels of exposition, the sub-personal which is associated with neurophysiology and the superpersonal which is associated with socially determined contingencies of reinforcement and punishment, is meso-cognitive psychology. Meso-cognitive psychology explains differences in the rate of temporal discounting, behavioral preferences for impulsivity, and self-control in terms of conflicting intrapersonal interests, one concentrating on short-term reward, the other on long-term reward. All of these cognitive psychologies underpin the intentional interpretation.

Conclusions: The Intentional Consumer Situation

The difference between the consumer situation in the extensional model and that in psychological explanation is that the former consists in tangible, physical rewards whether they are utilitarian or informational. They are objectively measurable and can be related systematically to patterns of consumer behavior (Foxall, 2017). Psychological explanation involves the analysis of choice by reference to desires and beliefs, emotions and perceptions, problem-solving, and decision-making rather than contingent environmental stimuli. Hence, the psychological consumer situation comprises the imagined circumstances in which consumer action occurs and the imagined consequences of acting within them; this consumer situation is posited to exist in the mind of the consumer (though it may only exist in its formal mode in the mind of the investigator) as it represents the contingencies of reinforcement and punishment intentionally and the action embedded therein (Fig. 13.8). These are future contingencies of reinforcement and punishment as putatively perceived by the consumer, perceptual contingency representations. They may not be tangible, but they have empirical substance in that they can be shown to be right or wrong and their truth value inheres in the physical, tangible rewards and sanctions with which the consumer's actions are met. A conclusion is that, in addition to the necessity of evaluating an intentional interpretation according to the requirements of cognitive

***The extensional consumer-situation*:**

Consumer behavior setting x learning history
(i.e., MOs + S^Ds) (i.e., log of prior behavior and its reinforcing and punishing outcomes)

***The intentional consumer situation*:**

Consumer behavior setting x learning history
(i.e., desires, beliefs, emotions, and perceptions representing past outcomes of consumer actions and the potential outcomes of actions performed within the present contingencies as they form the content of the consumer's intentionality)

Fig. 13.8 Comparison of the extensional and intentional consumer situations. *Note* Like the extensional consumer situation, the intentional consumer situation is centered on the interaction of the consumer behavior setting and the consumer's learning history. However, these are now portrayed as entities perceived and interpreted by the consumer's psychological processes. *Key* MO = motivating operations. S^Ds = discriminative stimuli. *Source*: Foxall (2018)

interpretation, an important criterion of accepting or rejecting the intentional interpretation is its predictive accuracy.[26]

The reason we employ a psychological explanation for consumer activities, treating them as actions rather than behavior, is that we must take the mental representation of the contingencies of reinforcement and punishment into account in the absence of any substantial extensional explanation. To invent a behaviorist explanation that consists in conjectured antecedent and consequent stimuli is the very approach that behaviorism itself has always explicitly repudiated on the basis that such putative explicatory terms are merely explanatory fictions. The construction of a psychological account of the observed activity which indicates how the individual would perceive the environment and respond to it on the basis of her desires, beliefs, emotions, and perceptions demarcates the explanation so given as of a different order from that of the empirical observation of regularities between acts of consumption and events within a controlling environment.

In doing this, we have done much more than account for bodily movements in terms of which basic actions are defined. We have ventured into the realm of what we may understand as "remote contingencies," those that exist only in the

[26] In my *Perspectives on Consumer Choice: From Behavior to Action, From Action to Agency* (Foxall, 2016b), I emphasized the difficulties of employing prediction as a criterion of the validity of an intentional interpretation. However, the development of the concept of contingency representation and the use of the success semantics of Ramsey (1927) permits a more positive appreciation of this source of validation. The need to show how the intentional interpretation is supported, where necessary, by a coherent cognitive interpretation remains.

intentionality of the consumer as she contemplates future actions—only, that is, in terms of intentional inexistence.

The development of the consumer situation in intentional terms stems from the inability of the extensional approach to the analysis of consumer behavior that has been the mainstay of the initial phase of theorizing and empirical investigation in intentional behaviorism (Foxall, 2017) to explain some aspects of the actions involved in consumer choice of this kind. Reaching this point is a central component of the intentional behaviorist methodology, for it is only when extensional explanation has been exhausted that we can know (a) what contribution to understanding consumer choice that methodology can uniquely make and (b) when, where, and how we are called upon to invoke intentional explanation. These considerations are of the utmost relevance to furthering our understanding and explanation of consumer choice conceived in terms of conflict between immediate and delayed outcomes.[27]

[27] I have written about the distinction between extensional and intentional language and explanations several times (e.g., Foxall, 2016a, b), but the following summary may be useful. Intentionality (with a "t") is simple "aboutness" and refers to the fact that some mentalistic words such as *believes* or *desires* and *perceives* or *fears* refer to something other than themselves. That is, they have an intentional object: no one just *believes*; she believes *that* such and such is the case. Similarly, we desire *that* the bus will get here quickly, say, perceive *that* the light is brighter here, or fear *that* we have failed the exam. The intentional object in each case (the bus or the light or failing) has, Brentano (1874) pointed out, intentional inexistence: it exists *in* the proposition. This is the essence of aboutness (see Brentano, 1874, pp. 88–94). It follows that the intentional object need not exist anywhere else. I can believe in Santa Claus without anyone, myself included, having the slightest notion that Santa Claus exists in the real world. If I am to behave successfully as a parent (given particular social norms), it is sufficient that he exists in my imagination and that I can talk to my kids in the knowledge that he exists in theirs too.

Intensionality (with an "s") is a linguistic phenomenon. It has implication for the way in which we employ sentences. For example, intensionality entails that the codesignative propositions cannot be substituted in a sentence that contains an attitude such as believes, desires, or feels without altering the truth value of the statement. Let me illustrate this in the case of a book called *Inside Mr Enderby*, written by Anthony Burgess under the penname Joseph Kell. Take the sentence, "*John believes that* Inside Mr. Enderby *was written by Joseph Kell.*" It is not valid to state, however, "*John knows that* Inside Mr. Enderby *was written by Anthony Burgess,*" for John may not know that Joseph Kell is Anthony Burgess. (Indeed, the editor who asked Burgess to review the book apparently did not! See Burgess (1990, p. 71) for this amusing incident.) The codesignative terms that follow "that" in these sentences are not therefore interchangeable without loss of the intentional sentence's truth value. This is not the case for sentences couched in extensional language. Changing "Anthony Burgess wrote *Inside Mr Enderby"* to "Joseph Kell wrote *Inside Mr Enderby*" does no violence to its truth value.

Another way in which intentional and extensional sentences differ is in the nature of their referring to objects. The object of an intentional sentence has Brentano's intentional inexistence and perhaps may, therefore, not exist outside that sentence, i.e., in the real world. If, speaking extensionally, I say that I am going to drive my car to Cardiff, then, if the sentence is to have any truth value, there has to be a car that is mine and there has to be a place called Cardiff that I can drive to. But if I say that I believe in Santa Claus or am seeking the Golden Mountain or praying for the Elixir of Life, the truth value of the sentence is not affected by the fact that none of these exists in a literal sense. It is crucial that a person who is to function competently and satisfactorily as a member of society understand the differences in truth value that separates extensional and inten-

If we are to understand in greater detail what is going on when a consumer selects an immediate reward that is smaller than another she could have by waiting, we have to posit that she is holding the values of both rewards in the form of perceptions and beliefs that refer to stimuli that are not empirically present at the time. The later, larger reward exists at this time only in the imagination of the consumer: she has no direct empirical access to it. Yet the consumer is able to make a decision concerning it that affects whether she accepts the smaller reward or exercises patience in awaiting the superior outcome. Similarly, if the consumer forestalls an urge to choose the smaller reward, her action is still governed by the imagined later reward on which she now places a higher value than what is currently available. This kind of understanding requires the ascription of intentionality to the consumer and ultimately a cognitive portrayal of her action. It means bringing together the *context* of behavioral responding (the contingencies of reinforcement and punishment which are the necessary tools of the extensional explanation which radical behaviorism provides) with the *cognition* that must be ascribed in order to make sense of the consumer's actions. It is the *consumer situation* that brings context and cognition together into a framework of conceptualization and analysis that can handle consumer intentionality. This book is about our conceptual move from the extensional consumer situation to the intentional consumer situation.

As the immediate precursor of consumer behavior and consumer action, the consumer situation links the stimuli that comprise the current consumer behavior setting with the elements of consumer experience of which current behavior is a function. It does so by bringing the consumer's learning history to bear on the significance of these stimuli for continued consumer behavior, the consequences such behavior is likely to yield. In the case of the extensional model of consumer behavior, the consumer situation is conceptualized as the interaction between, on the one hand, the discriminative stimuli and motivating operations that make up the physical and social setting and, on the other hand, the consumer's learning history, the sum total of previous relevant behaviors, and their reinforcing and punishing outcomes.

The extensional consumer situation, then, comprises only the interaction of the consumer behavior setting and learning history, both of which are empirically available. In this case, the extensional consumer situation can be safely assumed to provide the context of the consumer's behavior. There is no reason to adopt an intentional stance since the behavior is explicable (i.e., predictable) on the basis of the consumer situation alone. The need to link context and cognition does not arise.

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tional senses. I might spend some time imagining that the car in which I am going to drive to Cardiff is not the old rust bucket that I actually own but a sparkling new sport car. This is not a problem as long as I know I am fantasizing (or possibly speculating, hypothesizing, supposing) and that I know I must take my own car from the car park rather than the idealized alternative. Problem-solving, decision-making, and creativity all require the ability to engage in speculation, hypothesizing, and fantasizing from time to time, but all the more do they require, if they are to be successful, in our ability to switch from one of these modes of thinking and feeling to that which is governed by real-world correspondence.

This methodology is an elaboration of psychological paradigms in which behavior is predicted on the basis of the observable stimuli that surround it physically and temporally. But when such stimuli are not available to the would-be observer, it is necessary to turn to psychological explanation that employs intentional terms such as *perceives, believes,* and *desires* in order to characterize consumer action.

We have seen that in the extensional model that is a manifestation of theoretical minimalism, it comprises the interaction of the consumer's current behavior setting and her learning history. This is a conceptual definition since the consumer's learning history is often not stipulable other than in generalized terms but it is a potentially empirically specifiable entity. In psychological explanation, our focus turns from consumer behavior to consumer action, and the consumer situation comprises the intentionality (desires, beliefs, emotions, and perceptions) and cognitive decision processes that it is necessary to ascribe in order to render intelligible observed consumer activity for which no stimulus field is empirically available. The consumer situation now comprises the immediate *mental* antecedents of action.

Psychological explanation proposes two kinds of interpretation of consumer action. The first of these, the *intentional interpretation*, describes consumer situation predominantly in terms of desires, beliefs, emotions, and perceptions in order to establish an idealized consumer situation to explicate consumer action, while the cognitive interpretation appraises this in terms of what cognitive theory allows. So, the intentional consumer situation is an idealized portrayal of the context of consumer choice in which the consumer is treated as an optimizing (utility maximizing) system; it attributes to the consumer the intentionality she would have in a particular set of circumstances defined by the contingencies of reinforcement and punishment. A key consideration is how the consumer perceives the contingencies that hold the prospects for the reinforcement and punishment of her taking action in the current setting. In other words, what contingency representations would a consumer in such circumstances have to have in order to maximize utilitarian and informational reinforcement? Insofar as emotions constitute these perceptual contingency representations, this becomes: what emotional feelings would a consumer in such circumstances have to have in order to maximize utilitarian and informational reinforcement? The aim of the second kind of psychological interpretation, the *cognitive interpretation*, is to ascertain the extent to which the consumer's observed action can be understood in these idealized terms by an evaluation of this pattern of consumer action by reference to the cognitive structure and function available to the consumer.[28]

The intentional consumer situation links context and cognition by *uniting* them: for the intentional consumer situation comprises the current consumer behavior setting *as it is represented in the consumer's desires, beliefs, emotions, and perceptions* (especially insofar as these refer to the expected reinforcing and punishing outcomes of consumer action) as primed by her learning history which is itself represented by the consumer's beliefs. The desires, beliefs, emotions, and

[28] This methodology could be said to presume that perception is cognition-laden. See Foxall (2018), Chaps. 3 and 4.

perceptions (hereafter simply "the intentionality") that compose the intentional consumer situation are determined by assuming the consumer to be an economically rational system that maximizes utilitarian and informational reinforcement. What existed externally and objectively in the extensional consumer situation now are transformed into the objects of intentionality.[29] Moreover, since we resort to intentional explanation only when no stimulus field is available, no current consumer behavior setting, the objects of the consumer's intentionality must be memories and mental constructs. We aim to explain consumer action by the ascription of the intentionality the consumer *ought* to have given the behavior setting in which she is located and her learning history. An essential source of integration between context and cognition inheres in emotional responses to consumption environments. These are in turn indicative of consumers' perceptual representations of the contingencies. A central goal of the analysis that follows is to draw perception and emotion more closely into the scope of a theory of consumer choice that draws on behavioral and cognitive psychologies, behavioral economics, marketing science, and philosophy and seeks their integration as a foundation for an economic psychology of consumption. In this regard, I have introduced the concept of contingency representation as a fundamental component of the intentional consumer situation (see Foxall, 2018, Chaps. 3 and 4).

The applicability of this intentional interpretation more generally to consumer action in natural settings requires its testing via the following: first, a rigorous delineation, based on empirical observation, of the consumer action to be explained; second, predictions of consumer choice based on the ascribed intentionality, though these are likely to be somewhat simplistic and gross (the full range of desires, beliefs, emotions, and perceptions can be employed in this way); third, the subjection of the perceptual component, namely, contingency representations in the form of emotional reactions to the rigors of success semantics (clearly, this refers only to one of the components of the intentional consumer situation); and, finally, the critical evaluation of the content of the intentional consumer situation in light of theories of cognitive structure and functioning to assess the plausibility of the ascribed intentionality's availability to produce the observed action. This construction of the cognitive interpretation that establishes the viability of the intentional interpretation of consumer choice, the intentional consumer situation, finally unifies context and cognition in consumer psychology.[30]

[29] The objects of the ascribed intentionality might be said to exist in the external environment as Dretske (1995) argues, but what influences the actions of the consumer is her perception and conceptualization of the contingencies.

[30] I am clearly indebted to Daniel Dennett's (1987) exposition of his research strategy beginning with intentional systems theory (IST) which is followed by sub-personal cognitive psychology (SPCP). Elsewhere (Foxall, 2016b) I explain in some detail what the intentional behaviorism research strategy owes to Dennett's formulation and where I diverge from it.

References

Ainslie, G. (1992). *Picoeconomics: The strategic interaction of successive motivational states within the person*. Cambridge University Press.

Brakel, L. A. W. (2001). Phantasies, neurotic beliefs, and beliefs-proper. *American Journal of Psychoanalysis, 61*, 363–389.

Brakel, L. A. W. (2009). *Philosophy, psychoanalysis, and the A-rational mind*. Oxford University Press.

Brentano, F. (1874). *Psychology from an empirical standpoint*. Meiner.

Burgess, A. (1990). *You've had your time*. William Heinemann.

Carnap, R. (1959). Psychology in physical language. In A. J. Ayer (Ed.), *Logical positivism* (pp. 165–198). Free Press.

Chisholm, R. M. (1957). *Perceiving: A philosophical study*. Cornell University Press.

Compiani, M. (1996). Remarks on the paradigms of connectionism. In A. Clark & P. Millican (Eds.), *The legacy of Alan Turing. Volume II. Connectionism, concepts, and folk psychology* (pp. 45–66). Oxford University Press.

Dancy, J. (2000). *Practical reality*. Oxford University Press.

Davidson, D. (2001). How is weakness of will possible? In D. Davidson (Ed.), *Essays on action and events* (pp. 21–42). Oxford University Press.

de Gelder, B. (1996). Modularity and logical cognitivism. In A. Clark & P. Millican (Eds.), *The legacy of Alan Turing. Volume II. Connectionism, concepts, and folk psychology* (pp. 147–168). Oxford University Press.

de Villiers, P. A., & Herrnstein, R. J. (1976). Toward a law of response strength. *Psychological Bulletin, 83*, 1131–1153.

Dennett, D. C. (1969). *Content and consciousness*. Routledge and Kegan Paul.

Dennett, D. C. (1978). *Brainstorms*. MIT Press.

Dennett, D. C. (1987). *The intentional stance*. MIT Press.

Dretske, F. (1988). *Explaining behavior: Reasons in a world of causes*. MIT Press.

Dretske, F. (1995). *Naturalizing the mind*. MIT Press.

Foxall, G. R. (1990/2004). Consumer psychology in behavioral perspective. : Routledge. Reprinted 2004 by Beard Boks, Frederick, MD.

Foxall, G. R. (1999). The behavioural perspective model: Consensibility and consensuality. *European Journal of Marketing, 33*, 570–596.

Foxall, G. R. (2004). *Context and cognition: Interpreting complex behavior*. Context Press.

Foxall, G. R. (2007a). *Explaining consumer choice*. Palgrave Macmillan.

Foxall, G. R. (2007b). Intentional behaviorism. *Behavior and Philosophy, 35*, 1–56.

Foxall, G. R. (2008). Intentional behaviorism revisited. *Behavior and Philosophy, 37*, 113–156.

Foxall, G. R. (2009). Ascribing intentionality. *Behavior and Philosophy, 37*, 217–222.

Foxall, G. R. (2010a). Accounting for consumer choice: Inter-temporal decision-making in behavioral perspective. *Marketing Theory, 10*, 315–345.

Foxall, G. R. (2010b). *Interpreting consumer choice: The behavioral perspective model*. Routledge.

Foxall, G. R. (2016a). *Addiction as consumer choice: Exploring the cognitive dimension*. Routledge.

Foxall, G. R. (2016b). *Perspectives on consumer choice: From behavior to action, from action to agency*. Palgrave Macmillan.

Foxall, G. R. (2016c). Metacognitive control of categorical neurobehavioral decision systems. *Frontiers in Psychology (Theoretical and Philosophical Psychology), 7*(170), 1–18.

Foxall, G. R. (2017). *Advanced introduction to consumer behavior analysis*. Edward Elgar.

Foxall, G. R. (2018). *Context and cognition in consumer psychology: How perception and emotion guide action*. Routledge.

Hornsby, J. (1981). *Actions*. Routledge and Kegan Paul.

Malcolm, N. (1977). *Thought and knowledge*. Cornell University Press.

McClure, S. M., Laibson, D. L., Loewenstein, G., & Cohen, J. D. (2004). Separate neural systems value immediate and delayed monetary rewards. *Science, 306*(5695), 503–507.
McGinn, C. (2006). *Shakespeare's philosophy discovering the meaning behind the plays*. Harper.
Nichols, T. (2017). *The death of expertise: The campaign against established knowledge and why it matters*. Oxford University Press.
Radoilska, L. (2013). *Addiction and weakness of will*. Oxford University Press.
Ramsey, F. P. (1927/1990). Facts and propositions. *Aristotelian Society Supplementary Volume* 7: 153–170, reprinted in his *Philosophical Papers* (edited by D. H. Mellor) Cambridge: Cambridge University Press, 1990.
Ross, D. (2012). The economic agent: Not human, but important. In U. Mäki (Ed.), *Philosophy of economics* (pp. 691–736). Elsevier.
Ross, D., Sharp, C., Vuchinich, R. E., & Spurrett, D. (2008). *Midbrain mutiny. The picoeconomics and neuroeconomics of disordered gambling*. MIT Press.
Sandis, C. (2009). (Ed.)). *New essays on the explanation of action*. Palgrave Macmillan.
Sandis, C. (2012). *The things we do and why we do them*. Palgrave Macmillan.
Schnaitter, R. (1999). Some criticisms of behaviorism. In B. A. Thyer (Ed.), *The philosophical legacy of behaviorism* (pp. 209–249). Dordrecht: Kluwer.
Searle, J. R. (1983). *Intentionality: An essay in the philosophy of mind*. Cambridge University Press.
Searle, J. R. (1995). *The construction of social reality*. Free Press.
Searle, J. (2001). *Rationality in action*. MIT Press.
Searle, J. R. (2010). *Making the social world: The structure of human civilization*. Oxford University Press.
Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review, 57*, 193–216.
Skinner, B. F. (1953). *Science and human behavior*. Macmillan.
Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis*. Prentice-Hall.
Skinner, B. F. (1971). *Beyond freedom and dignity*. Cape.
Skinner, B. F. (1974). *About behaviorism*. Knopf.
Stanovich, K. E. (2009). Distinguishing the reflective, algorithmic, and autonomous minds: Is it time for a tri-process theory? In J. S. B. T. Evans & K. Frankish (Eds.), *In two minds: Dual processes and beyond* (pp. 55–88). Oxford University Press.
Steward, H. (2012). *A metaphysics for freedom*. Oxford University Press.
Tomasello, M. (2014). *A natural history of human thinking*. Cambridge, MA: Harvard.
Tomasello, M. (2016). *A natural history of human morality*. Cambridge, MA: Harvard.

Chapter 14
Behavior Analysis and Psychological Concepts: Commentary on Foxall's Intentional Behaviorism

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One of the main characteristics of any movement or proposal self-named *behaviorism* is a major concern with the old philosophical mind-body problem, particularly as it was posited by Descartes as involving an interaction where an immaterial, non-extensive, mind (or soul) influences, and is influenced by, a material, extensive, body. *Intentional Behaviorism* is no exception to this tendency. Foxall's proposal is peculiar in that it originates from the adaptation of a behavioristic position (i.e., behavior analysis and radical behaviorism) with the purpose of interpreting consumer behavior (cf. the Behavioral Perspective Model, Foxall, 1990/2004), from which it explored the limits of behavior-analytic explanation and, finding it wanting, has incorporated, in posterior stages, ascription of intentionality and cognitive explanation. Latter stages of the project have been developed in the last 20 years (e.g., Foxall, 2004), including detailed examination of predominant features of radical behaviorism, central issues in contemporary philosophy of mind, major findings and theories in neurosciences, and theoretical and empirical approaches in social-cognitive psychology. The present chapter examines central aspects of Foxall's criticism of radical behaviorism and the proposal of ascribing intentionality as a way of overcoming its explanatory limitations.

Limitations of Radical Behaviorism and Reasons for Intentional Idiom

Since its incipient stage, Foxall's model of consumer behavior has presented itself as one more alternative way of interpreting consumer phenomena with emphasis in behavior and situational variables, rather than a solution that should replace existing

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social-cognitive approaches (e.g., Foxall, 1997). The original Behavioral Perspective Model (cf. Foxall, 1990/2004, 1998) was built on adaptations of the behavior-analytic three-term contingency, including a new and useful distinction between utilitarian (i.e., directly derived from the use of product and services) and informational (i.e., socially mediated) consequences, and has been widely adopted in empirical investigations of consumer behavior (e.g., Foxall, 2016b, 2017). Although the author considers that this type of behavioral approach makes possible prediction and control of behavior, he has concluded that the framework is not capable of explaining behavior fully, especially in open settings where it is not possible to identify a controlling stimulus field.

It seems that there are two intertwined lines of argumentation. One is related to the type of explanation that behavior analysis tends to adopt in such situations, where apparently there is no identifiable event in the environment that may be said to control or influence behavior. Here, the author is particularly concerned with the absence of a discriminative stimulus that would explain, in behavior-analytic terms, the occurrence of a given response due to its previous association to responses being reinforced in its presence. In such cases, Foxall stresses that it is common to find in the behavior-analytic literature explanations grounded on possible learning experience that the individual organism might have had or on possible private events, in the form of covert responses, that might have occurred, which are said to have increased response probability. Foxall has criticized such explanatory practice, for, he has argued, there is no empirical basis to infer the antecedent or private events, as there is in non-human laboratory experiments, where the organism's previous experiences are entirely known. This type of post hoc explanations that presupposes the occurrence of events that would be necessary to explain the phenomena is seen distrustfully by Foxall, as attempts to save the theory. In several of his writings, the author even asserts that this practice represents intellectual dishonesty (cf. Foxall, 2016a, p. 26, p. 113; Foxall, 2020, p. 186, p. 217).

Very closely associated to this criticism, Foxall advances a second line of argumentation that examines the difficulties that behavior analysis faces when trying to explain behavior in the absence of discriminative events in open settings. This is based on the distinction between the extensional and intentional languages and explanations. As exposed by Foxall (2021), intentionality is related to aboutness, in the sense that some mentalistic expressions refer to objects other than themselves, such as *belief* or *desire* and *perception* or *fear.* For example, no one simply believes; the person must believe *that such and such is the case*, that is, these expressions have, in the philosophical sense, an *intentional object*. The intentional object may not exist since Mary might believe that Santa Claus is responsible for the delivery of Christmas gifts. Her belief, however, is no less veridical because Santa Claus does not exist in the "real world." Objects of extensional sentence, in contrast, must exist in order for the sentence to have truth value. Then, if Peter is described as driving his car to Cardiff, he must have a car, and there must be a place called Cardiff for the sentence to have a truth value.

Another difference between extensional and intentional language relates to the fact that intentional sentences might not display "referential transparency via substitutability of codesignatives terms" (Foxall, 2016a, p. 96). By this it is meant that an intentional sentence such as *Mary believes that that is the Morning Star* cannot necessarily be replaced by *Mary believes that that is Venus*, since Mary might not know that the *Morning Star* is the same as *Venus*. In extensional sentences, by contrast, codesignative terms can be replaced, for to assert that *That is the Morning Star* is the same as asserting that *That is Venus*. Another way to put this is to consider that whereas the truth value of the extensional statement is related to the planet Venus, an existing object in the real world, the truth value of the intentional statement is related to a fact about Mary (Foxall, 2020, p. 167).

According to Foxall, one of the main characteristics of radical behaviorism has been the exclusive adoption of extensional language, with the consequent rejection of intentional language. In his view, this produces limitations to the type of explanation that is offered by behavior analysis. After testing the limits of this kind of extensional explanations, the author defends that when it is not possible to explain behavior on the basis of antecedent and consequent events—no stimulus in the context, in open settings, not knowing the individual's learning history—then the behavior-analytic explanation breaks down, and it becomes imperative to add intentional idiom, including, for example, ascription of desires and beliefs to the person whose behavior one wants to explain.

This conclusion is derived from three main limitations associated to behavior-analytic explanation based on extensional language. The first is related to the difficulty of accounting for the continuity or discontinuity of behavior exclusively in extensional terms. According to Foxall (2016a, p. 99–100), the difficulty is to explain (1) how past events influence current behavior (e.g., how can I tell what I ate for lunch yesterday?) without some means of recording the experience; (2) changes in behavior when there is no change in the contingencies (e.g., a person that is a heavy alcohol user who reduces dramatically her drinking, a consumer that adopts a new brand to her brand repertoire); and (3) maintenance of behavioral patterns despite changes in the contingencies (e.g., an experiment participant who does not change her behavior despite changes in the contingencies). The argument is based on the assumption that the radical behaviorist account requires that a common stimulus be present on each occasion that a response is emitted. When it is not possible to detect each element of the three-term contingency, the tendency in behavior analysis is, then, to suppose that certain learning experiences occurred, or that private verbal behavior, in the form of rules, occurred, or that something occurs physiologically within the individual, which is the task of the physiologist to investigate. These are interpreted by Foxall as attempts to save the theory due to the refusal of employing intentional language.

The second important limitation pointed by the author is the impossibility of accounting for the personal level when adopting an exclusively extensional language, as does radical behaviorism. Inspired by Dennett's (1969, p. 93) ideas, by *personal level* it is meant "the level of people and their sensations and activities" rather than that of brains and events in the nervous system and rather than the environment and its reinforcing and punishing events (Foxall, 2020, p. 112). Although it is possible, according to Foxall, to study the environmental correlates of emotionality in an extensional science of behavior, it "does not embrace the unanalyzable sensation to which emotional language refers" (Foxall, 2020, p. 112). The attempt to explain first-person enunciations, such as "I am looking for my glasses," as a particular case of third-person description, such as "I can observe that I am doing the sorts of things that I have done in the past when I lost my glasses," remounts to simple translation, with no explanatory function, and to speculation about the occurrence of an untestable learning history, about which there is no available evidence. The author considers that this is not science and that the ascription of intentionality is unavoidable (Foxall, 2020, p. 119). Radical behaviorists should consider the choice between believing that "I conclude I must be looking for a book because I have observed myself systematically eyeing my bookshelves in the past, or that I simply know that I am searching for the dictionary" (Foxall, 2020, p. 121).

A third main problem related to radical behaviorist approach, according to Foxall, is the absence of limits for the interpretation of behavior outside the laboratory. There is no methodology of interpretation that defines how one can plausibly identify discriminative stimuli, operant classes, and reinforcing and punishing consequences, when analyzing complex behavior in open settings, where it is impossible to test such relations experimentally and to obtain information concerning the person's learning history and, consequently, the functions of events in the setting. The same behavior, such as *walking downstairs at home in the morning*, might be part of functionally diverse behavioral patterns, such as going to work, getting a glass of water in the kitchen, or doing stepping exercises. The author argues that "it is impossible to define the bounds of behaviorism other than by the incorporation of intentional idiom" (Foxall, 2020, p. 140).

One way of analyzing Foxall's proposal is to consider that it has, at least, two parts: a) a criticism of radical behaviorist explanation, which might be sufficient to predict and control behavior, particularly in closed, laboratory settings, but cannot fully explain behavior, and b) a defense that such limitations can be overcome by the adoption of intentional language, where one ascribe desires, beliefs, emotions, and perceptions to the person whose behavior one wants to explain. Each of these parts of the author's position will be briefly considered in what follows.

Evaluating the Criticisms of Radical Behaviorism

Foxall's criticisms touch on some important and relevant points that should be carefully considered in behavior analysis. One of them relates to the little attention that has been dedicated to the development of systematic and consistent ways of identifying and characterizing the learning history of an organism. This theme has been also stressed by Tatham and Wanchisen (1998), who called attention to the absence of systematic approaches to behavioral history. In specific experimental settings, this has been done by referring to the type of contingencies to which the organism has been exposed, for example, accurate or inaccurate instructions (e.g., Galizio, 1979), pre-extinction baseline response rates in behavioral resurgence experiments

(Shahan & Sweeney, 2011), history of exposure to specific schedules of reinforcement (Tatham & Wanchisen, 1998), and different experiences with choice and no-choice contexts (Drifke et al., 2019). The reference to learning history by specifying the contingencies to which the organism has been exposed seems to be a natural route for behavior-analytic theorizing, considering the emphasis of the approach on environmental determinants of behavior. However, considering, as stressed by Foxall, the explanatory importance of learning history in reinforcement theory, it seems useful, in most contexts, to be able to describe the learning history of the individual in terms of what the individual is capable of doing or tends to do, that is, a description of, what is called in ordinary language, abilities and propensities to do things. So, for example, in experimental settings, the description that the subject was exposed to a DRL schedule (differential reinforcement of low rates; cf. Ferster & Skinner, 1957) does not necessarily describe the behavioral patterns that the individual is likely to emit. The behavior may not have reached stable levels of performance, or the reinforcer used, or motivational operation adopted, may not have been sufficient to establish the typical DRL performance. That is, what one needs to know is that in the presence of certain events the animal tends to emit responses in low rate. Part of Foxall's criticism related to the problem of continuity of behavior may be associated to the reluctance, in behavior-analytic tradition, to use expressions concerning abilities and propensities, what philosophers have named dispositional (e.g., Ryle, 1949) and power (e.g., Hacker, 2007) concepts.

Reluctance is perhaps due to the widespread interpretation of such expressions as referring to mental events that cause what people do. But this is a mistaken interpretation of the logical use of such concepts. In ordinary language, these concepts have the function of summarizing observations of behavior and predicting certain behavior given certain conditions (cf. Ryle, 1949; Hacker, 2007, 2013). The main problem with the scientific employment of such ordinary dispositional expressions, such as ability and propensity, lies not in their referring to mysterious and unobservable events but in their vagueness. These ordinary language expressions are vague and open-textured, in the sense that the instances of behavior that are summarized and predicted by the concept might vary significantly in their different usages. Then, for example, when in ordinary language John is described as being a vain person, what is being asserted, in most contexts, is that, based upon observations or information concerning John's usual behavior, one can predict that he will likely be careful with his clothing and appearance when going to a party, or that he will frequently talk about and aggrandize his achievements in social situations, or that he will have difficulties facing criticisms directed to his behavior or deeds, or that he will tend to be exaggeratedly pleased when receiving compliments, and such like. The expression *vain* can be correctly used in ordinary language as related to all or any one of these types of occurrences or many similar others. This is what Ryle (1949) called an open dispositional concept, which may function well for its job in ordinary conversation, but is rather vague for scientific adoption, for one does not know the kind of behavior that is being summarized and predicted. In ordinary language, most dispositional psychological concepts are open (Ryle, 1949), which hinders their use for scientific purposes (Harzem, 1986). If dispositional or power concepts are closed, less vague, predicting specific behavior in specific conditions, they can fruitfully be adopted in scientific discourse.

This is what has been done in behavior-analytic research, although less extensively and systematically as one might have wanted. In behavior analysis research, there are some usages of dispositional concepts in very specific contexts such as preference for one of the alternatives in studies of choice, or impulsive or self-controlled choice patterns, or individual or group discount rates in intertemporal choice, and such like. The major point is that with this type of concept one describes what the organism is capable of doing or what the organism tends to do, that is, given certain conditions, the organism can or tends to respond in certain ways. The use of concepts describing what the individual is capable or prone to do might solve most of the problems raised by Foxall concerning the lack of continuity in behavior analytic explanation. With this, it becomes explicit that behavior analysis research is not describing only responses but also changes in repertoire of the individual, considering what in ordinary language are referred to as abilities and propensities.

Another point of criticism that is appropriately stressed is the loose treatment of "private events" as the concept has been employed in behavior-analytic circles. Sometimes it has been used as covert behavior that calls for explanation, sometimes used as stimuli that influence behavior. In this latter case, Foxall calls attention to the risk of adhering to a position that is close to "mental" causation, which would be inconsistent with radical behaviorism. This would occur, for example, when covert behavior is described as generating stimuli to subsequent behavior, in which case it could be interpreted as causing behavior. Part of the difficulties in dealing with covert behavior is due to a positive interpretation of the concept *doing in the head* or *doing mentally*, according to which the concept is taken as indicating the occurrence of unobservable responses. Having its original home in ordinary language, the concept performs a clear negative function, for it indicates the nonoccurrence of certain behaviors that occurred previously during training (cf. Ryle, 1949; Oliveira-Castro, 2000). For instance, when someone is described as making mental calculations, part of what is being said is that the person solves mathematical problems without looking up a multiplication table, without adding or multiplying the numbers on a piece of paper, without writing down or looking at the numbers on the blackboard, without using a calculator or counting fingers. The person can solve the problems without emitting any of these responses, which were necessary, and used to be emitted, in earlier stages of her training.

According to an operant approach, these responses that are skipped, as training increases (e.g., drawing bars to be added in a multiplication problem), may be interpreted as nonrequired precurrent behavior, considering that they increase, at least at the beginning of training, the likelihood of correct current responding (e.g., writing down the solution to the multiplication problem), and are not required by the programmed contingencies, that is, final responding may be reinforced even if they do not occur (cf. Oliveira-Castro et al., 1999; Oliveira-Castro et al., 2002). Moreover, these precurrent responses occur in situations where there is high correlation between the events produced by them (e.g., bars to be added in the multiplication problem, e.g., "IIII+IIII+IIII") and the stimuli in the problem situation (e.g., "3 × 4

=”), which enables the transference of stimulus function between them. In the example used here, the stimulus “IIII+IIII+IIII,” produced by the precurrent response, exerts a discriminative function in the original problem situation, for in its presence the responses of counting and writing down “12” have high occurrence probability (have been reinforced in the past). As the problem situation “3 × 4 =” is repeated and the final response “12” is reinforced, the discriminative function of “IIII+IIII+IIII” is transferred to “3 × 4,” which then functions as discriminative stimulus for the final response of the chain (writing down “12”). The precurrent response, named *auxiliary behavior* (Oliveira-Castro et al., 2002), is no longer necessary for the occurrence of the correct response and stops occurring. This is when, in ordinary language, the child is said to solve the problem *in her head* or *mentally*. The recognition of the negative function of such ordinary language concepts would encourage the investigation of the conditions under which auxiliary responses stop occurring and under which performance can be improved or disrupted. After training, the child does not draw and count bars, although one could assert that she acts *as if* she could count them, that is, she can solve the problem as if she could count bars. The expression *as if* emphasizes the metaphorical use of the expression *doing in the head*. When interpreted as performing a positive function of indicating the occurrence of unobservable responses, the expression raises several conceptual difficulties, such as metaphorical uses of stimulus and response, lack of criteria to infer private events, and possible adherence to an additive theory (cf. Oliveira-Castro, 2000).

Another criticism posed by Foxall is the absence of a personal level of explanation in the radical behaviorist approach. By personal level the author means “as the level of people and their sensations and activities,” the level at which emotion is known by the person experiencing it, which is not analyzable in terms of physiological or environmental events. In Foxall's words: “as the person who has felt pain knows what pain is, so the person whose behavior has been reinforced and punished knows what these effects are. But this knowing is unanalyzable: it is a feature of the personal level rather than either the physiological or environmental level” (Foxall, 2020, p. 112). In this context, the author criticizes the exclusive adoption of third-person description in behavior analysis, using as example Skinner's interpretation of how someone knows that she is looking for her glasses, avoiding the use of intentional language (Skinner, 1953, pp. 89-90). Again, the author calls attention for difficulties faced by behavior analysis with the interpretation of psychological concepts from ordinary language, in this case with the interpretation of goal-directed, intentional, behavior. The question of how one gets to know her own goals is not a promising question, since knowing one's goal is part of having a goal, that is, of behaving intentionally. It is understandable the reluctance to employ psychological concepts that have been poorly interpreted by philosophers and theoreticians alike, as pointed out by Hacker (2007, 2013), but its complete avoidance might not be the best way of approaching the phenomena of interest. An understanding of the usage of the concept may be helpful in dissolving conceptual confusions, overcoming theoretical difficulties, and directing sound empirical questions. Explicating the logic of the usage of the concepts in ordinary language reveals that they do not refer to unobservable events, accessible only to the person to whom the concept is attributed, nor to causes of behavior (Hacker, 2007, 2013; Harzem & Miles, 1978; Machado & Silva, 2007; Ryle, 1949).

This is why Foxall has a point when it comes to the difficulties to deal with psychological concepts. There is a tendency in behavior analysis to avoid using mentalistic concepts and little effort to understand how they function and how they are used and employed in language, that is, what the verbal contingencies for their uses are. This posture may hinder the development of certain research themes in behavior analysis. They do not refer to mental mysterious events, but have complex uses that have several other functions, some of which involve the description of capacities and tendencies of behaving (see, for instance, Ryle, 1949, and Hacker, 2013).

On the other hand, Foxall's criticisms sound too severe with behavior analysis, particularly when he suggests that some interpretations might be characterized as attempts to save the theory or intellectual dishonesty. These accusations do not seem helpful to improve the field or stimulate academic discussions since they resemble moral judgments rather than epistemological criticism. Moreover, such criticisms appear too severe when one compares this tone with that used by Foxall to refer to cognitively inspired theories in psychology and neurosciences, which, predominantly, maintain a dualist Cartesian theory where the immaterial mind has been replaced by an anthropomorphized brain, what has generated serious conceptual confusions (for systematic examinations, see Bennett & Hacker, 2003; Hacker, 2007, 2013).

Having briefly looked at the criticisms posed by Foxall, which suggest that the behavior-analytic framework might be improved, it is necessary to examine, in general terms, Foxall's proposal to overcome such limitations.

The Ascription of Intentionality

The center of Foxall's project is to add intentional language as a complement to behavior-analytic explanation. This would be done in those situations where the explanation in extensional terms, such as the typical behavior-analytic explanation, breaks down. This occurs, according to the proposal, mainly when there are no identifiable stimuli in the environment that could explain the emitted response (cf. Foxall, 2020, p. 171). Additionally, diverging from the position defended by other authors, Foxall defends that intentional description should be applied only to entities that are intensionally fluent and are not amenable to explanation via the physical or the contextual stance (Foxall, 2020, p. 171-172). Ultimately this implies that intentional explanation should be used only at the personal level and with reference to "cognitive humans" (Foxall, 2020, p. 172), which would exclude animals, inanimate objects, and parts of animals (avoiding thus the mereological fallacy pointed out by Bennett & Hacker, 2003). Then, in the absence of a stimulus field that would extensionally explain a given behavior, desires and beliefs, perceptions and emotions, would be ascribed to the person. But would intentional ascription be compatible with and complementary to behavior-analytic explanation?

Foxall's proposal, significantly inspired by Dennet, is rooted on the assumption that the distinction between extensional and intentional idioms is essential and separates the behavioral description from psychological explanation. However, as discussed by Hacker (cf. 2013), this separation might be oversimplistic when one considers that a closer investigation of the logical functions of expressions that occur as grammatical complements of some psychological verbs, or of some uses of psychological verbs, reveals a more complex picture. Several concepts, sometimes included in the category of mental or psychological, are not intentional. Sensations, such as pain, illustrate this, for they are not directed towards objects as hopes are directed towards what is hoped for. Nor can one feel a headache if there is no headache. Moreover, some cognitive verbs (e.g., know, remember, be aware of, be conscious of) are factive, that is, they have grammatical objects but their objects exist "in reality" not only "in thought." Additionally, perception verbs may be characterized as non-intentional in some of their uses, for they sometimes function as achievement verbs whose objects must occur. If John saw Mary, there must be a person called Mary; otherwise he did not see her, and he was mistaken. He might have taken her for someone else. Then, the philosophical use of *intentionality* is not necessarily a mark of the mental or psychological phenomena and is the center of a variety of puzzles and confusions dealing with the relations between thought and reality, a theme that lies beyond the scope of the present work but suggests that the route might not be the most promising one (cf. Hacker, 2013).

Epistemological Limitation or Absence of Empirical Evidence?

Another point that calls attention in Foxall's proposition is the tendency to conclude that there are insurmountable epistemological limitations in behavior analysis in contexts where absence of empirical evidence seems to be the problem. The followed line of argument establishes that in the absence of a stimuli field that might explain the occurrence of a given behavior, in behavior-analytic terms, it is imperious to adopt intentional language by ascribing to the person desires and beliefs, perceptions, and emotions. But how one would ascribe desires and beliefs? Foxall is careful about this and asserts repeatedly that the ascription of intentionality must be conducted responsibly. But what would a responsible ascription consist of, in the case of an adoption of innovation by a consumer? "Sources of intentional interpretation might include, for instance, knowledge of those elements of an innovation that ensure its more rapid diffusion . . . personality and cognitive style of the innovator . . . the nature and extent of the motivators of the innovative process . . . desires, beliefs, emotions, and perceptions in terms of which the consumer perceives her consumption history and its outcomes, the current behavior setting with its indications of the consequences that are contingent upon the execution of particular consumer behaviors, and the pattern of utilitarian and informational reinforcement that she expects to be the result of her behavior" (Foxall, 2020, p. 188). Part of this information would be obtained from verbal behavior emitted by the person requested to respond ad lib (Foxall, 2020, p. 214).

The proposal requires the collection of additional data, mainly derived from verbal behavior emitted by the person being studied. But if more data are collected would not the behavior-analytic explanation also change in order to consider the additional information? The person's reports concerning her previous experiences, her perceptions of the experimental situation, and such like, might also be considered as indicative of previous experiences, existing behavioral repertoires, and, consequently, of the functions performed by different events in the consumer setting. There is no a priori or epistemological reason, in behavior analysis, for not considering and examining verbal behavior of those whose behavior one is investigating. But there is no such obligation either, because the data that must be collected in any research depends essentially on the types of questions one intends to answer. Most examples presented by Foxall, concerning behavior-analytic speculative explanations, seem to be typical of theoretical works, where the focus is not in collecting data, or discussion sections of empirical investigations, where the focus is on interpreting findings which will be very likely the object of subsequent empirical research. In these new empirical investigations, data collection will be directed to answer the proposed speculative interpretations. Gathering more data to respond previous questions is one of the most typical characteristics of empirical science. This seems to be the typical sequence of events in empirical sciences, and behavior analysis is no exception. The theoretical interpretation advanced as attempt to explain the observed behavior is typically submitted to subsequent empirical tests.

But Foxall's criticisms are directed to those circumstances where one does not have access to additional data. To illustrate this point, Foxall (2020, p. 138) presents an example of a professor who twice a week has lunch with colleagues in the faculty club. The author cites elements in the environment, such as the notice "Faculty Club" and the time shown by the clock on the building's façade, that might work as discriminative stimuli for his entering the building and having a meal. The identification of these contingencies can form the basis of predictions of his future behavior in similar circumstances. According to Foxall this would be a typical and confirmed behavior-analytic interpretation of the professor's behavioral pattern. However, the author raises the possibility that the professor may be entering the club in order to pursue his extramarital affair with the catering manager, something he has done without his colleagues' knowledge on the remaining days of the week for the last 7 years, facts that were exposed in the tabloids later on. Based on this and other similar examples, Foxall (2020) stresses the impossibility of establishing even approximately the learning history of an adult and, consequently, it must be recognized the limitation of behavior analysis to give anything more than a plausible explanation (p. 139), which makes one conclude that "radical behaviorism has no mechanism by which to identify the context of any relevant behavior that takes place beyond the closed setting of the laboratory" (p. 145). Using Rachlin's (2000,

p. 58–59) example, of discovering what a man swinging a hammer is doing (e.g., hammering a nail, joining pieces of wood, building a house?), Foxall (2020) defends that the behavior of the builder is predictable only insofar as we ascribe to him the desire to build a house and the belief that placing this brick will lead to building a wall, that building the wall will contribute to the fabrication of a room, and so on (p. 144). But how would one know that the person has such desires and beliefs (although people usually do not believe these things about house building, they know them)?

Again, more information is needed. In order to identify someone's goals or intentions, in addition to having information concerning some aspects of the person's experiences, abilities, and motivations, one must consider the social context within which the person is behaving. This includes the kinds of behaviors that are means for what types of ends in a given society, that is, the typical behavioral patterns and respective social consequences in a community. And, of course, if the person says what it is that she is doing, the identification of her goals might become easier (cf. Hacker, 2013; Peters, 1958; Oliveira-Castro & Harzem, 1990). The identification of people's goals is part of our everyday conversation about people and part of the repertoire of any language-user adult, who is also capable of telling or refraining from telling his or her goals to someone else (Hacker, 2013). It seems that there is no reason that prevents the use of these types of information in formulating behavior-analytic interpretations of people's behavior. Based on this, one can speculate about the social contingencies to which the person is exposed, the current motivating operations that are prevalent, the person's behavioral repertoire, and such like. If the occasion demands, then the researcher, or practitioner, might look for empirical evidence that may corroborate or refute such speculations. What one would not typically do, in behavior-analytic circles, is to suggest that desires and beliefs are unobservable events that cause what the person does. The fact that these concepts have been widely interpreted, in philosophy and psychology, as the name of causative unobservable occurrences, and that they are intensively used in ordinary language, where their usage is appropriately vague and open-textured, might, perhaps, explain why behavior analysts have avoided them in their theorizing. In ordinary discourse, for example, the adequate level of description of someone's goals depends on what is expected in the context of the conversation. Answer to "what is he doing with the hammer?" can be adequately answered by "he is building a house" as well as by "he is joining wood pieces," for both may be correct, as stressed by Rachlin (2000). The context usually defines the level of analysis that is of interest to the audience, that is, the kind of answer that is likely to be socially reinforced in specific contexts. In the example of the professor and his lover, cited above, it seems that it would be equally adequate to assert that "he was having lunch with his colleagues" as well as "he was secretively saying hello to his lover." The best answer will depend on the context of the question "what was he doing?"

The importance of what the person says about her own goals is also relative to the context in which the conversation unrolls. When the behavioral patterns one observes, or is informed of, fit the known means-ends fluxes in a given society and also fit what the person says about her goals, the task of characterizing what the person is doing becomes much simpler. But, in certain contexts, what the person says is neither necessary nor sufficient to identify someone's goals (cf. Peters, 1958). This is often the case in courtrooms where defendants deny that they have committed any crime and do not reveal their motivations. Despite this, a jury, and the majority of public opinion, may reach conclusions concerning the person's motivation and past behavior. This is an extreme example to show that one cannot always rely on what people say about their intentions, desires, and, even, beliefs, as a way of identifying their intentions, desires, and beliefs. The typical case where these verbalizations are most relevant is the context of friendly and sincere conversations. In such contexts, what people say is usually sufficient to reveal their desires and beliefs, and, consequently, what they say is compatible with what they do. But the point here is to stress that this is not necessarily the case. Different social contingencies have the potential to influence the correspondence between what people say and do. The identification of the contexts in which what people say correspond to what they do is an important empirical question, one that has been neglected by most authors in cognitive social psychology for a long time.

These considerations lead to the conclusion that the untested and speculative interpretations advanced by behavior analysis may be so characterized as long as there is no reason to gather more data and information. If the questions are posed in contexts where there is relevance to find the answers, more data would have to be collected with the purpose of identifying crucial variables that might be influencing a given behavior, be them historical or contextual. This is what is done in applied settings. Interventions are based on data collection related to individual cases, firms, schools, persons, families, and so on. This is usually how empirical science and technology advances.

The Search for Intentional Objects and Representations

The emphasis on intentional idiom, as suggested by Foxall, might have the undesirable consequence of encouraging the search for mysterious objects. Because the proposal stresses the peculiar characteristics of "objects" of psychological verbs, related for example, to desires, beliefs, and emotions, which may not exist, as contrasted with the characteristics of possible "objects" of non-psychological verbs, which must exist in the "real world," it raises questions concerning the relation of intentional objects to reality and the nature of their existence. In searching for such relations, it is tempting to forget that these are grammatical objects of transitive verbs, in the case of object-accusatives, which are not to be understood as "things" in the sense that a chair or a car are said to be objects. These grammatical objects can be classified as material or intentional object-accusatives. In the case of a material object-accusative, its denotation must exist for the acceptable use of the verb in the sentence. One cannot know Jill if there is no such person, and one cannot believe a rumor if there is no rumor to believe. As for intentional object-accusatives, their denotation need not exist for the verb in the sentence to be true, since one may look for Eldorado, although it does not exist, and Mary may expect Santa Claus to visit her tonight. Other grammatical complements include nominalization- and sentence-accusatives and infinite accusative which are not objects but answers to questions (for a detailed analysis, see Hacker, 2013).

Overlooking the grammatical status of object-accusatives, philosophers have frequently instigated interpretations that they are like real objects that exist not in the world but in the mind (Hacker, 2013). In several of his works, Foxall has been careful about this issue and has attempted to make clear that his proposal is non-ontological, in the sense that intentional ascription would not refer to things in the mind that cause behavior but would be only descriptive. The suggestion had been to overlay another type of description that includes the ascription of intentionality, similarly to what Dennet has proposed (e.g., Foxall, 2016a; Foxall & Oliveira-Castro, 2009). In the present chapter (Foxall, 2021), however, it seems that the author accepts the interpretation that intentional objects exist in the mind and that the investigation should shift from the analysis of environmental contingencies to the analysis of mental representation of these contingencies. This is most clear in his analysis of hyperbolic discounting (p. 41–61). This phenomenon is usually investigated in situations of intertemporal choice, where consumers choose between one alternative that offers a smaller-sooner reward (SSR) and another that offers a larger-later reward (LLR). One of the most robust findings concerning intertemporal choice is the reversal of preference from the larger-later reward to the smaller-sooner reward, as time approaches the opportunity to obtain the smaller-sooner reinforcement. The finding has been reproduced in hundreds of experiments with different species, including animals and humans, using both real and hypothetical rewards, and indicates that a hyperbolic discount function is more adequate to describe the results than an exponential function, this latter representing the predictions from neoclassic economic model of consumer choice (cf. Mazur, 1987; Kagel et al., 1995; Rachlin, 2000).

Foxall criticizes the explanation of hyperbolic discounting found in the literature on akrasia, according to which choices are determined by the value that the individual attributes to each alternative at different points in time. According to the author, this type of explanation requires that the individual compares the two alternatives at certain moment in time in order to choose the most valued reward. Considering, however, that the alternatives are not present at the moment of choice (t_0), Foxall (2021) raises the question concerning their location. In his words: "At t_0 the larger reward is said to be valued more highly than the smaller. What can this mean? Neither the SSR nor the LLR is empirically available . . . at t_0. Where can they exist in order to be evaluated?" (p. 42). The author concludes that the only possible answer, according to behavior analysis, would be to locate the choice alternatives in the person's learning history or in learned (or self-created) rules, neither of which could serve as explanation because they are unknown and purely speculative. Based on this line of reasoning, the author defends that behavior analysis cannot avoid using intentional language and that an interpretation about the individual's representations of the contingencies must be considered in the explanation of consumer choice. Several aspects of this formulation deserve consideration.

The first one is related to the criticism towards behavior-analytic explanation of akrasia in terms of changes in values. Foxall (2021) mentions vaguely "the literature of akrasia" (p. 42) without specifying any author or particular work. Despite this, it seems possible to consider the work of Rachlin (2000) as a typical example of behavior-analytic approach to akrasia, particularly his work on self-control, where he uses the notion of increases and decreases in subjective value as part of his analysis. It seems that when "subjective value" is used as part of an explanation of the choices organisms make in intertemporal choice situations, Rachlin is asserting that preference reversal can be predicted, it is a widely observed phenomenon, replicated across a large variety of species and contexts, and that a quantitative relation, the hyperbolic function, has been shown to describe well such results. Based on this, one is not surprised to observe preference reversals when they occur. Additionally, given certain empirical evidence concerning individuals' choices in specific contexts, it is possible to make predictions concerning which alternative certain individuals or groups (e.g., children, adults, and older adults) are likely to choose under what conditions and which group show higher or lower discounting. Moreover, this type of analysis suggests the use of commitment procedures that might increase the probability of later-larger choices. Then, in such context, when one asserts that the value of one alternative increased, it seems that one is asserting that the hyperbolic function predicts a higher probability of choosing that alternative. The level of analysis is restricted to general patterns of behavior given certain conditions, typical regularities that one finds in empirical sciences. The analysis is not necessarily suitable to explain particular cases, such as John's choices of having several drinks last Monday, unless one can obtain enough data to calculate individual discount rate in a given choice context. Therefore, it would be unusual in behavior-analytic literature to explain choices as caused by changes in values, taking "changes in value" to refer to events that occur prior to choices, which cause them. However, considering that Foxall does not cite specific works, it is not possible to attempt to analyze the matter in more detail.

Another point that needs consideration in Foxall's formulation is the assumption, suggested in the chapter, that in order to choose between alternatives the person needs to make a comparison between things that are present. The author raises the question concerning the location of SSR and LLR at the moment of choice, emphasizing that the alternatives are not "empirically available" at the moment of choice. This is an unusual conception of choice. It is true that in some situations choice occurs at the physical presence of the alternative rewards, as when one chooses between two different beverages or between two flavors of ice cream. But this does not seem to be the case in most situations, where choices do not occur at the presence of the rewards but at the presence of events that have been associated with different consequences. In typical experiments of intertemporal choice with animals, for instance, at the first choice opportunity (t_0) the animal is presented with two alternative response keys, for example, a green and a red key, one of which having delivered, over several choices, a SSR and the other, a LLR. By pecking one of the keys the animal is said to have chosen one or the other reward. In this procedure, at the second choice opportunity (t_1) the animal is again presented with both alternative keys, green and red, and chooses one of them by pecking (e.g., Mazur, 1987). In this situation the choice is made in the presence of the response keys that have been associated to the SSR and LLR. The rewards SSR and LLR are not present neither at t_0 nor at t_1, but the keys associated to them are. The behavior-analytic interpretation for this is intrinsically related to the notion of discriminative stimulus, an event in present of which previous responses have been followed by certain consequences, and which acquires, on the basis of such associations, reinforcing or punishing functions, as well as the potential to alter the occurrence probability of such responses.

In choice situations with humans, these relations between events associated with the rewards and the rewards should be analogous, for there are events or things in the environment that have been associated to the consequences programmed by each alternative. When someone is asked to choose between, for instance, "U$ 100 in one month" or "U$ 140 in six months," the verbal stimuli have been associated, through a long history of training, to their respective purchase potentials and to ordinary economic rules about how to manage money. The person is not responding to empirically unavailable money amounts, but to a question concerning delayed money amounts. The posed question "Where are the U$ 100 and U$ 140 located when the question is presented to the person?" seems in need of clarification and does not seem promising for directing empirical research or interpretation. Where is any future event before it occurs? Must it be located anywhere? Why? Foxall suggests that for behavior analysts the events are in the past. But this is also a strange way to put it, for the events of the past are not in any location. Indeed, they occurred in the past. But does this mean that they are located anywhere? The question about the location of events seems to divert the focus from what seems most relevant in the explanation of this type of choice, which is the type of learning experience that might explain the observed choice patterns, as the literature on intertemporal choice has widely demonstrated with systematic empirical results.

Additionally, based on the puzzle concerning the location of the alternative rewards, not yet presented to the chooser, Foxall reaches the conclusion that the only possible answer to the puzzle is to assume that there occur mental representations of the rewards, which are part of the variables that explain individual's choice. As representations, the events are said to exist (as representations) in the present and to be located in the mind of the person who is choosing, which would solve the puzzle concerning the location of the alternative rewards. If, in the proposal of intentional behaviorism, representations are to be posited when discriminative stimuli are present, it seems even more natural to posit them when no discriminative stimulus is present in the behavior setting, a situation much stressed in the author's writings because it represents a clear point where behavior-analytic interpretation breaks down. According to Foxall, behavior analysis simply cannot explain choices in the absence of discriminative stimulus in the field and has tried to invent specific learning histories and self-created rules as attempts to save the theory. As discussed earlier, most situation where there is "absence of discriminative stimulus in the field" might be better understood if more data and information were gathered about the behavior and the circumstances where it occurred and occurs. In fact, this would also be necessary to ascribe intentionality, as proposed by Foxall. However, sometimes it will be impossible to gather more information, in which case we may never know the answer concerning the variables that influenced what the person did. But this negative conclusion would be not due to epistemological problems; it would derive primarily from the impossibility of obtaining more information. If a bird that is singing at the far end of the backyard flies away before we can take a look at it, we may never know what kind of bird it was. But this is not an epistemological mystery; it is an empirical impossibility of gathering more information (cf. Austin, 1946). Considering that the need to gather more information was discussed earlier, it might be useful now to focus on the ascription of mental representations.

The imperative of ascribing mental representations seems to be derived, at least partially, on the assumption that an adequate explanation of behavior should be based on events that are present when behavior is emitted. This represents a limiting assumption, because time intervals can be divided indefinitely, depending on the desired level of analysis. This can be illustrated by a situation where a pigeon is trained to peck either of two lateral white keys depending on the color of a central key that can be lit green or red for 2 s. If the central key is red, after it turns off, the lateral keys are lit, responses on the left key are reinforced, and there is no programmed consequence for responding on the right key. If the central key is green, after it turns off, the lateral keys are lit, responses on the right key are reinforced, and there is no programmed consequence for responding on the left key. Let us assume that, after learning this discrimination task, it takes the pigeon 0.5 s, on average, to peck the corresponding lateral key after the central key turns off. It is possible to imagine experimental manipulations that would increase gradually the time between turning off the central key and turning on the lateral keys, let us say from 1 to 400 s. Although this would be an empirical issue, let us assume that the pigeon displays perfect discriminated performance, pecking the lateral key where there is programmed reinforcement on 100% of trials, even in the 400-s delay condition. At what point, along the 1 to 400-s interval, would one consider that the response occurs in the absence of the discriminative stimulus and, therefore, requires the inference of mental representation? Taken literally, one can assert that even when the peck occurred 0.5 s after the central key was turned off, responses occurred in the absence of the discriminative stimulus. If this is so, representations would almost always have to be inferred, as in fact most cognitive theories in psychology have done (even in the case of non-human animals). There seems to be an increased tendency to infer mediating events as the interval between influencing events and behavior increases (Oliveira-Castro, 2000).

But what would be the disadvantages or problems related to inferring mental representations? Theoretically, if it is assumed that mental representations are necessary for the explanation of behavior, this would require the identification of the variables that generate or cause representations. Otherwise, one would be simply postponing explanation without identifying the variables that influence behavior (cf. Skinner, 1953). From the philosophical point of view, the attribution of representation in the interpretation of psychological phenomena has a long history of discussion, immersed in puzzles and confusions, most of which related to attempts to elucidate the relation between thought (or language, or perception) and reality. The philosophical position defending that intentionality is the mark of the mental, which has inspired Foxall's intentional behaviorism, is a central part of this web of conceptual difficulties (cf. Hacker, 2013).

Conclusions

For 40 years, Foxall has led the development of consumer behavior analysis, a theoretical framework developed to interpret consumer behavior on the basis of principles derived from behavior analysis, behavioral economics, and marketing, which has generated a wide range of international research, on a variety of relevant topics concerning consumer behavior. Despite the success of his project, the author has kept questioning the limits of the approach, particularly in its role of interpreting and explaining complex human behavior in natural settings. In this legitimately motivated academic quest, the author has identified limitations in behavior-analytic explanation, such as the difficulties of explaining the continuity of behavior and delimiting behaviorist explanation, which are related, mainly, to the behavioristic restrictions in using psychological concepts.

The proposed solution by intentional behaviorism is to superimpose the ascription of intentionality to behavior, by referring to individuals' desires, beliefs, emotions, and perceptions. The great merit of the proposal is to call attention to the importance of considering, more closely, the logic of the use of psychological expressions. The limitations pointed out by Foxall stress the need to adopt systematic theoretical treatment of learning history in behavior analysis, with the adoption of theoretical concepts related to, what in ordinary language would be called, abilities and propensities that can summarize what individuals typically do, or are capable of doing, in certain situations and predicting what they are likely to do. A better understanding of the logic of the use of these kinds of expressions (e.g., dispositional concepts or powers) in ordinary language reveals that they do not refer to unobservable mental events that cause behavior. The problem in adopting them is associated to their vagueness. If used more precisely, concepts that describe individuals' learning history and make predictions concerning probable behavioral patterns might be useful to behavioral theories.

Despite calling attention to the need of incorporating psychological concepts in behaviorism, Foxall's proposal is inspired by a philosophical tradition that has emphasized the dichotomy between intentional and extensional idiom, which tends to overlook important logical differences across psychological expressions. Many psychological concepts are not intentional and intentional concepts are of several different kinds. Beliefs are not like desires, which are both different from emotions and perceptions, differences that tend to be overlooked in the approach. The dichotomy also encourages the search for the nature and location of intentional objects, a philosophical practice that has orbited an amalgam of confused puzzles concerning the relations between thought and reality, from which representational theories of mind have evolved (Hacker, 2013). Following this philosophical trend, Foxall proposes that intentional behaviorism should consider not only the contingencies of reinforcement but also the representations the individual has about reinforcement contingencies. This approach seems to face two obstacles. The first is to establish criteria to specify when representations should be posited. The criterion, proposed by the author, based on the "absence of discriminative stimulus" has been shown to be fragile due to the relative nature of stimulus delay (i.e., as time is indefinitely dividable) and the need to gather more empirical evidence. The second obstacle is that one would need to identify the variables that generate or influence representations; otherwise one would be encouraged to elaborate post hoc explanations.

References

Austin, J. L. (1946). Other minds. In J. Wisdom, J. L. Austen, J. Austin, & A. Ayer (Eds.), *Symposium: Other minds*, *Proceedings of the Aristotelian Society, Supplementary Volumes, 20* (pp. 122–197).

Bennett, M. R., & Hacker, P. M. S. (2003). *Philosophical foundations of neuroscience*. Blackwell.

Dennett, D. C. (1969). *Content and consciousness*. Routledge and Kegan Paul.

Drifke, M. A., Tiger, J. H., & Gifford, M. R. (2019). Shifting preferences for choice-making opportunities through histories of differential reinforcer quality. *Journal of Applied Behavior Analysis, 52*(1), 227–239. https://doi.org/10.1002/jaba.515

Ferster, C. B., & Skinner, B. F. (1957). *Schedules of reinforcement*. Appleton-Century-Crofts. https://doi.org/10.1037/10627-000

Foxall, G. R. (1990/2004). Consumer psychology in behavioral perspective. : Routledge., Reprinted 2004 Beard Books, Frederick, MD.

Foxall, G. R. (1997). *Marketing psychology: The paradigm in the wings*. Macmillan.

Foxall, G. R. (1998). Radical behaviorist interpretation: Generating and evaluating an account of consumer behavior. *The Behavior Analyst, 21*, 321–354.

Foxall, G. R. (2004). *Context and cognition: Interpreting complex behavior*. Context Press.

Foxall, G. R. (2016a). *Perspectives on consumer choice: From behavior to action, from action to agency*. Palgrave Macmillan.

Foxall, G. R. (Ed.). (2016b). *The Routledge companion to consumer behavior analysis*. Routledge.

Foxall, G. R. (2017). *Advanced introduction to consumer behavior analysis*. Edward Elgar.

Foxall, G. R. (2020). *Intentional behaviorism: Philosophical foundations of economic psychology*. Elsevier and Academic Press.

Foxall, G. R. (2021). Intentional behaviorism. In D. Zilio & K. Carrara (Eds.), (Orgs.) *Contemporary Behaviorisms in debate*. Springer/Paradigma.

Foxall, G. R., & Oliveira-Castro, J. M. (2009). Intentional consequences of self-instruction. *Behavior and Philosophy, 37*, 87–104.

Galizio, M. (1979). Contingency-shaped and rule-governed behavior: Instructional control of human loss avoidance. *Journal of the Experimental Analysis of Behavior, 31*(1), 53–70.

Hacker, P. M. S. (2007). *Human nature: The categorial framework*. Blackwell.

Hacker, P. M. S. (2013). *The intellectual powers: A study in human nature*. Wiley-Blackwell.

Harzem, P. (1986). The language trap and the study of pattern in human action. In T. Thompson and M. D. Zeiler (Eds.), Analysis and integration of behavioral units (Chapter 4, pp. 45-53). Routledge.

Harzem, P., & Miles, T. R. (1978). *Conceptual issues in operant psychology*. John Wiley & Sons.

Kagel, J. H., Battalio, R. C., & Green, L. (1995). *Economic choice theory: An experimental analysis of animal behavior*. Cambridge University Press.

Machado, A., & Silva, F. J. (2007). Toward a richer view of the scientific method: The role of conceptual analysis. *American Psychologist, 62*, 671–681.

Mazur, J. (1987). An adjusting procedure for studying delayed reinforcement. In M. Commons, J. Mazur, J. Nevin, & H. Rachlin (Eds.), *Quantitative analysis of behavior: The effect of delay and of intervening events on reinforcement value* (Vol. 5, pp. 55–73). Erlbaum.

Oliveira-Castro, J. M. (2000). The negative function of "doing in the head" and behavioristic interpretations of private events. *The Mexican Journal of Behavior Analysis, 26*, 1–25.

Oliveira-Castro, J. M., Coelho, D. S., & Oliveira-Castro, G. A. (1999). Decrease of precurrent behavior as training increases: Effects of task complexity. *The Psychological Record, 49*, 299–325.

Oliveira-Castro, J. M., Faria, J. B., Dias, M. B., & Coelho, D. S. (2002). Effects of task complexity on learning to skip steps: An operant analysis. *Behavioural Processes, 59*, 101–120.

Oliveira-Castro, J. M., & Harzem, P. (1990). Level of aspiration and the concept of goal. *The Mexican Journal of Behavior Analysis, 16*, 41–53.

Peters, R. S. (1958). *The concept of motivation*. Routledge & Kegan Paul.

Rachlin, H. (2000). *The science of self-control*. Harvard University Press.

Ryle, G. (1949). *The concept of mind*. Hutchinson.

Shahan, T. A., & Sweeney, M. M. (2011). A model of resurgence based on behavioral momentum theory. *Journal of the Experimental Analysis of Behavior, 95*(1), 91–108. https://doi.org/10.1901/jeab.2011.95-91

Skinner, B. F. (1953). *Science and human behavior*. The Macmillan Company.

Tatham, T. A., & Wanchisen, B. A. (1998). Behavioral history: A definition and some common findings from two areas of research. *The Behavior Analyst, 21*(2), 241–251. https://doi.org/10.1007/BF03391966

Chapter 15
Behavior Analysis and Psychological Concepts: Reply to Oliveira-Castro

Gordon R. Foxall

Introduction

The fundamental problem which Intentional Behaviorism addresses is one that should be familiar to all behavior analysts whose work extends beyond the laboratory: the integrity of an interpretation of behavior undertaken when an experimental analysis is infeasible. Functional analysis involves the prediction and control of behavior in the relatively closed settings of the operant chamber, as well as in situations such as field experiments where the requirements of environmental control can be unambiguously observed (Skinner, 1938). But that does not mean that operant analysis is confined to such settings. As Skinner points out, notably in his analysis of verbal behavior, the principles of behavior established in these "favorable" contexts can provide "plausible" interpretations of behaviors that cannot be studied so rigorously because they lie beyond an experimental analysis (Skinner, 1957, p. 13, Skinner, 1969, p. 100, Skinner, 1984, p. 207). Just as some natural sciences like astronomy and the study of species' phylogenetic histories are not directly amenable to experimentation and yet make use of scientific principles garnered elsewhere, so behavior can be understood by reference to the contingencies of reinforcement and punishment even when experimental control of the subject matter cannot be achieved.

The point I wish to emphasize, however, is that we do not *sometimes* have to interpret our observations: rather, we *always* must do so. Even an extensional account of behavior interprets its subject matter. There are after all no stimuli and responses out there in the world; there are only events, some of which precede

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others, some of which can be said to be functions of others. There is nothing intrinsic to these events that makes them stimuli on the one hand and responses on the other. Labelling a prior event a stimulus and the latter a response is not simply a linguistic convention: it is an act of interpretation. Arguing that the responses are a function of the stimuli that have followed them in the past is similarly a matter of interpreting a series of observations to make them intelligible, to predict and attempt to influence the events labelled responses by reference to those labelled stimuli. Having acknowledged this, the point is to make explicit *how* we are interpreting our subject matter and identifying its merits and its demerits. As a methodology Intentional Behaviorism seeks to clarify on philosophical grounds the level of confidence we can accord the conclusions reached in the course of economic-psychological investigations. It is particularly concerned with the validity, reliability, and generalizability of intentional interpretations of economic behavior to which we must resort once extensional modelling has been exhausted. In the process of showing how such confidence might be achieved, it employs extensional and intentional modes of explanation for the analysis of economic, especially consumer, behavior. The ascription of intentionality in the explication of observed behavior is problematic insofar as there is no direct means of avoiding what Dennett (1969) refers to as "ontic bulge," the proliferation of intentional terms to give plausibility to that behavior. There must be checks and balances wherever an appeal is made to the intentional in order to constrain interpretation in a manner that is justified by alternative methods of investigation, analysis, and explanation. When is intentional interpretation called for, what form ought it to take, and what empirical means are available for its evaluation and progression? Intentional interpretation ought at the least to be corrigible by means that are objectively accessible to the intellectual community.

Intentional Behaviorism begins with an analysis of behavior based on the findings of a radical behaviorist paradigm. It is only when this analysis can go no further that an intentional interpretation becomes apparent and necessary. The integrity of this ensuing interpretation of such behavior, which relies on judgments about what constitutes the behavior in question and what counts as a controlling variable, has two aspects. Both involve the allusions to elements of operant explanation, i.e., the principles derived from an experimental analysis, in the course of giving an account of complex behavior, that which is not open to experimental control. The first is the delimitation of the range of current and future contingencies that can be reasonably included in one's analysis; the second, the epistemological status of references to the learning history of the individual whose behavior is under scrutiny. Behavioral interpretation ought to be subject to rigorous self-examination on the part of the investigator just as much as that which accompanies study design, data analysis, and conclusion-drawing in the case of experimental research. Non-behaviorists are as likely to encounter our work in the form of interpretations of complex behavior as in that of experimental studies and should be able to ascertain the reliability of our conclusions just as readily. While we are of course confident of the plausibility and probity of our own construals, we ought at least to be assiduously enquiring how we can ensure that our interpretations meet scientific canons of judgment as far as is possible. These are issues that require constant vigilance, so I am grateful to

Oliveira-Castro (2021) for his detailed and thoughtful response to my chapter (Foxall, 2021) and indeed to my more comprehensive exposition (Foxall, 2020). Intentional Behaviorism has now received an informed and reasoned critique, and I appreciate the opportunity to clarify my position.

Oliveira-Castro's commentary is nonetheless wide-ranging, and in this reply I concentrate on elaborating his and my outlines of the three-stage research strategy employed by Intentional Behaviorism. In so doing, I present further detail on the use in Intentional Behaviorism of both extensional and intentional reasoning, examine how the intentional behaviorist research strategy differs from both Dennett's scheme for intentional psychology and the practice of radical behaviorism, on both of which it nevertheless depends, and consider briefly some recent developments in the intentional behaviorist mode of explanation. Both of these approaches employ explanatory fiction though the latter retains the vocabulary of extensional experimentally grounded research, something that is likely to mislead. My exposition here both arises from Oliveira-Castro's commentary and allows me to respond to as many of his specific objections as space permits.[1]

[1] I should like in passing to make a general point about the range of applicability of intentional explanation. Oliveira-Castro notes what I have called "the intensional criterion," which limits intentional explanation to entities that can demonstrate intensional fluency, but omits my argument that quasi-intentional explanation can be used of other entities (Foxall, 2020). He is, therefore, incorrect in assuming that intentional explanation cannot be applied to entities other than nonverbal humans. The intensional criterion does not rule out a kind of intentional explanation for animals, for instance, insofar as it is legitimate to ascribe the desires and beliefs that would be appropriate to the animal's behavior given its history and current circumstances. The objective would be to make the animal's behavior intelligible, rather than necessarily to predict it. Apart from his insistence on the predictive criterion, this is essentially the intentional stance approach of Dennett. This approach, applied to humans or animals or machines, is a-ontological with respect to the intentional idioms ascribed: for Dennett, all there is to being an intensional system is to be predictable by this stance. Intentional Behaviorism relies on a rather different understanding of intentionality, one in which desires and beliefs actually exist in the mentality of the individual whose behavior is being interpreted intentionally. A person must actually have a desire and beliefs about how it might be brought about. The ontology of intentionality has shifted in the process of adopting this approach. First, note that what I have said about the individual actually having intentions surely is not at odds with a radical behaviorist approach. At least some schools of radical behaviorism would allow that, whatever desiring and believing, are they are *behaviors* (Skinner, 1974) and the understanding that they occur in the private behaviors of humans is therefore not different. Second, whereas radical behaviorism takes the existence of these behaviors and their import for granted and is therefore close to folk psychology in its assumptions, Intentional Behaviorism asks what empirical evidence might be adduced to indicate that the individual is capable of holding intentions to this kind (Foxall, 2020). The intensional criterion seeks to identify (a) the capacity to demonstrate that one can make verbal sense of intensional language and (b) the capacity to distinguish among the various propositional attitudes and thus to act with psychological rationality, i.e., in accordance with one's desires and beliefs. It is important that the individual be able to distinguish among fantasies, pretenses, neurotic beliefs (which are founded on fantasy), and beliefs-proper (which are reality-tested propositions about the world) (Foxall, 2017b). (a) is necessary in order to provide evidence that the individual has desires and beliefs or the appropriate private behaviors; (b), in order to assess whether her overt behaviors are consistent with these intentions. These criteria elicit very precise verbal statements; the verbal behavior in which they

However, rather than respond seriatim to Oliveira-Castro's points, I should like to organize my response by taking up one of his observations. Oliveira-Castro notes that I am inspired by Dennett, and this is an intellectual debt I am pleased to acknowledge once again. However, it is not possible to appreciate the nature of Intentional Behaviorism without understanding the important differences between Dennett's proposal for intentional psychology and the approach I am taking. I differ importantly from Dennett in accepting the reality of intentionality (over and above the realist position of his accepting centers of gravity and his later exposition of his position in "Real Patterns" (Dennett, 1991). I accept that there are both knowledge-by-acquaintance and knowledge-by-description, the first-personal subjective experience of perceptions and of desires and beliefs that is available exclusively to the individual whose experience they are. (Dennett's position seems not to be that subjective experience does not exist but that it cannot enter directly into a third-personal scientific account and is therefore not to be entertained as an explanatory device.) They are available for scientific scrutiny not directly but through their translation into knowledge-by-description, their expression in words or other media which are third-personal phenomena. Dennett does not use the expressions knowledge-by-acquaintance and knowledge-by-description, but they are I believe implicit in his exposition of heterophenomenology as the means by which such singular experience is made available to the scientific community. I do not assume that radical behaviorists are universally opposed to this distinction, though I do not understand how it is possible to speak, for instance, of subjective valuation without the notion of knowledge-by-acquaintance. Having commented on the relationship between Intentional Behaviorism and Dennett's three kinds of intentional psychology, I turn to the two themes that Oliveira-Castro notes in my chapter: the limitations of radical behaviorist explanation (what I have termed the *bounds of behaviorism*) and the case for an intentional approach (what I have termed the *imperatives of intentionality*).

Intentional Behaviorism and Dennett's "Kinds of Intentional Psychology"

Interestingly, Dennett has often been described as a behaviorist. His earliest mentors were Quine (e.g., Quine, 1960) at Harvard and Ryle (e.g., Ryle, 1949) at Oxford. Dennett argues, with behaviorists, that we have nothing other than behavior as our subject matter but that there are two ways of speaking of it. The first is the extensional way which as Oliveira-Castro notes is the manner in which I understand radical behaviorism proceeding; the other, which I argue is necessary when the extensional is exhausted, is intentional language. The subject matter has not changed; only the necessity of describing it in a different way has entered the

consist is part of the subject matter of operant psychology and provide evidence for the existence of the intentionality required and the form it takes.

picture. For Dennett there is nothing more to having a mind or being an intensional system than that one's behavior is predictable by means of the attribution of intentional states such as desires and beliefs. As a result, Dennett has been characterized as a behaviorist by other philosophers of mind. For myself, I see no need to resort to the intentional language until the extensional has been thoroughly exploited in the description or explanation of behavior; until, that is, it becomes impossible to identify empirically the elements of the three-term contingency that comprise a radical behaviorist account.

Intentional Behaviorism differs fundamentally however from Dennett's approach. Oliveira-Castro suggests that Intentional Behaviorism follows Dennett's (1981) scheme for intentional psychology. I am pleased to acknowledge (not for the first time) this intellectual debt since I would not have formulated my approach had I not become familiar with Dennett's work. However, I both use and deviate from his example, and I should like to point out the similarities and differences between Intentional Behaviorism and Dennett's program insofar as I understand it. This framework allows me to respond along the way to many of Oliveira-Castro's points.

The three stages proposed by Intentional Behaviorism may appear on superficial inspection to be compatible with the three "kinds of intentional psychology" adumbrated by Dennett (1981). Beyond initial acquaintance, however, the resemblance is largely superficial, and there are on closer inspection important differences of which this section takes note. For Dennett, an intentional system is anything that can be predicted by dint of ascribing to it the desires and beliefs appropriate to its history and current position. Being so predictable is, moreover, all that there is to having intentionality, to having a mind. The intensional stance is the device by which not only human and nonhuman animals but also computer programs and processes such as natural selection are predicted on the basis of their being imbued by the investigator with the requisite intentionality. The perspective is the third-personal level of exposition required by science. Dennett's first intentional psychology is everyday folk psychology, the attempt to make sense of ourselves and one another by reference to our assumed desires and beliefs. Going beyond this in the direction of scientific inquiry is, first, *intentional systems theory* (IST) which presents an intentional account of behavior in which the intentional system is treated as an idealized optimizer; the usefulness of this account is determined by its capacity to predict. Although the idealized interpretation incorporates the terminology of folk psychology, they are therefore more sharply defined. The intentional concepts that comprise the IST are characterized as *abstracta*, the sort of explanatory entities that correspond to parallelograms of forces or centers of gravity in physics: they may not exist in the sense in which tables and chairs exist but they are essential for conceptual understanding and prediction. Further progression toward scientific psychology is marked by what Dennett calls *sub-personal cognitive psychology* (SPCP), a level of intentional inquiry that further refines its terminology, honing them to the kinds of entity that may become the variables of psychological hypotheses. The variables in question are described as *illata*, physical entities that can be "intentionally characterized," say, electrons that can be predicted by means of the intensional stance, by the attribution to them of desires and beliefs. These are presumably the philosopher's offering of variables for psychological inquiry. No mind-body problem arises because only physical or material substance is assumed to exist; without altering this fact the behavioral scientist can discharge her tasks of prediction and explanation by accrediting to the material body that suggests an *illatum* the mental phenomena which render it predictable and otherwise useful in the task of psychological explication.

While all three of Dennett's psychologies are intentional, the first stage of Intentional Behaviorism, theoretical minimalism, requires an extensional model to be tested to destruction, initially as an important end in itself, secondarily as a means of ascertaining the necessity of turning to an intentional explanation. The initial aim does not provide a detailed learning history for specific consumers but establishes the environmental stimuli of which consumer behavior is generally a function (Foxall, 1998). In the case of economic behavior, this consists of a model of consumer choice derived from radical behaviorism, conceived as a purely descriptive approach to behavioral science and operating within the confines of Machian positivism.

As a model assuming the environmental determination of behavior, the Behavioral Perspective Model of consumer choice (BPM) portrays consumer behavior as the outcome of the consumer-situation, which forms the immediate precursor of behavioral choice. This relationship is essentially the model; see Foxall (2020) for further detail on the content of the model in the particular context to which each applies. The consumer-situation is itself the product of a consumer behavior setting that contains the discriminative stimuli and motivating operations that are the antecedents of behavior and which prefigure the consequences of such behavior, namely, the functional and social effects of consumer activity. These anticipations result from the consumer's learning history, the totality of similar behavior she has previously performed, and the rewards (reinforcers) and sanctions (punishers) it has engendered. These reinforcing and punishing consequences have the respective effects of increasing and decreasing the rate at which the behavior in question is repeated in similar circumstances. Reinforcement and punishment may also be described in neurophysiological terms which allude to the role of neurotransmitters in the regulation of behavior. The original, extensional characterization of the BPM has inspired a large body of empirical research and resulted in a subdiscipline I have nominated *consumer behavior analysis* (Foxall, 2001, 2002, 2017a) and which is confined to the investigation of the "contingencies of reinforcement and punishment" in order to account for the probability of behavior.

An Extensional Model of Consumer Choice

As an approach to explanation in economic psychology, Intentional Behaviorism takes consumption as a paradigm of behavior generally for two reasons. First, consumption currently dominates most societies, both human and nonhuman, and thereby exemplifies the whole range of activity of their members; second, consumer choice is an exemplar of all behavior insofar as it entails the attainment of rewards and simultaneously incurs sanctions. Its theoretically minimalist position is represented by the extensional Behavioral Perspective Model (BPM-E) in which the immediate precursor of consumer behavior is the *extensional consumer-situation*, the interaction of the consumer's learning history, and the current setting in which she is located. This setting is composed of discriminative stimuli and motivating operations that set the occasion for the attainment of reinforcement, and the incurring of punishment, through the purchase and consumption of economic goods. The consumer's learning history primes these elements of the consumer behavior setting, making them salient to current behavior. The reinforcing consequences of consumer behavior are of two kinds, *utilitarian* (which refers to the functional outcomes of purchase and consumption) and *informational* (which reflects the social implications of these activities). BPM-E has proven instrumental in the prediction of numerous aspects of consumer choice including product and brand selection, store patronage, and sensitivity to changes in price; research based on this model has also yielded the important result that consumers' utility functions relate their purchase choices to the attainment of bundles of utilitarian and informational reinforcement which maximize their utility subject to their budget constraints (Oliveira-Castro & Foxall, 2017; Oliveira-Castro et al., 2016a, 2016b). The extensional model has also been employed in the development of behavioral interpretations of such aspects of consumer behavior as the adoption and diffusion of innovations, ecologically sensitive consumer choice, and saving and domestic asset management (Foxall, 1996, 2017a).

In line with the rationale of theoretical minimalism that the purpose of the extensional model is to identify the limitations, if any, of the radical behaviorist paradigm in the explanation of consumer choice, the quest to find areas of consumption that are not amenable to this kind of analysis has been pursued (Foxall, 2004a, 2004b, 2007c, 2008, 2020). The explanations of consumer behaviors such as the adoption of novel brands (Foxall, 2016a) demonstrate the difficulties inherent in accounting for behavioral continuity and discontinuity; the observed inflexibility of responses to changes in schedule parameters (Foxall & Oliveira-Castro, 2009) and preference reversal (Foxall, 2016b) has been found to require a personal level of exposition for their complete explanation; finally, the behavioral interpretation of observed behavior in which expanding levels of contingency are nested within one another has been shown to require delimitation in line with what the actor can reasonably have known at the point of behaving (Foxall, 2007a, 2020). I shall comment further on these *bounds of behaviorism* in due course.

The Intentional Model of Consumer Choice

The inability of the extensional model to provide a comprehensive explanation necessitates a second kind of account which proceeds in terms of intentional language. This is the equivalent of Dennett's creating an idealized intentional account of the behavior of the individual conceived of as a "idealized system," an entity which maximizes the satisfactions it receives and can be explained in terms of the desires and beliefs that it "ought" (in Dennett's words) to have, given its history and present circumstances (what we are calling its learning history and consumer behavior setting). This is the closest point between Intentional Behaviorism and Dennett's *intentional systems theory* based on the intentional stance. The intentional Behavioral Perspective Model (BPM-I) comprehends each of the explanatory variables contained in the extensional model in an intentional manner. Learning history becomes the consumer's prior behaviors and their reinforcing and punishing outcomes *as they present themselves to the memory of the consumer*. Discriminative stimuli and motivating operations are the consumer's perceptions of the meaning of the elements of her physical and social environments as they suggest the probable outcomes of further consumer behavior as reinforcing and punishing consequences of consumer choice. What we have previously seen as the consumer's *behavior*, which is explained by the environmental stimuli that have acted upon it, becomes *consumer action*, the activity that follows from the consumer's desires and beliefs.

I am not the first by any means to note that radical behaviorists employ intentional terminology in their work. Skinner justified it in his accounts of behaviorism for a general audience (e.g., 1974), and other prominent behavior analysts have used it in their disquisitions on the philosophical bases of radical behaviorism (see Foxall, 2009). The point of Intentional Behaviorism is not to criticize this per se, but to recognize its inevitability, and to encourage discussion of the implications of the use of intentional language for the explanation of behavior. Above all, it has sought to provide means by which the intentional interpretation of behavior can be reliably constructed and appraised.

Dennett's criterion for establishing the efficacy of the use of the intensional stance to construct an intentional account of the idealized individual or other behaving entity (the "intentional system" as he terms it) is the predictive capability of the interpretation. Intentional Behaviorism has not so much argued against this criterion as suggested that predictions based on intentional interpretation are likely to be somewhat facile and to be resistant to falsification.[2] Instead it has proposed that

[2] There are several reasons why the predictive accuracy of the intentional interpretation may not be a suitable criterion for the evaluation of intentional interpretation and certainly why it cannot be the *sole* criterion. First, predictions at this point are likely to be vague generalizations based more on the extensional account of economic behavior provided by theoretical minimalism than genuinely novel insights provided by the idealized intentional account thereof. Naturally, if any such insights should be provided, they would not be neglected. Second, the point of intentional interpretation is not to predict per se but to fill an explanatory gap that extensional behavioral science is unable to address. It is this mode of explanation that requires justification. Third, the empirical investigation of the accuracy of the intentional interpretation is a subject for the extensional sciences, especially neurophysiology and behavioral science. Fourth, the intentional interpretation must first be related to the broader framework of cognitive psychology in which it is formulated: how far is the intentional interpretation consistent with the ideas of mental structure and functioning based on cognitive theory and its accompanying empirical scientific basis? (Empirical cognitive psychology is essentially based on extensional experimentation and correlative research: since its explanatory variables (which are intentional in nature) are not directly empirically available, it is necessary to

intentional interpretations be appraised in terms of their consistency with the broad corpus of cognitive theory and findings (Foxall, 2016b, 2017b). This exercise in *cognitive interpretation* is designed to lead to hypotheses which can be empirically tested within the various economics paradigms that are relevant to behavior analysis: *operant behavioral economics*, *picoeconomics*, and *neuroeconomics.*

Levels of Exposition

The models of consumer choice make reference to three levels of exposition: the personal, sub-personal, and super-personal. The personal level is that of the individual actor and is described in the intentional terminology of action resulting from desires and beliefs. This is the basis of BPM-I. The sub-personal level is that of neurophysiology. This is the basis of the understanding of economic behavior as influenced by neural structure and functioning (Foxall, 2016a) and is the subject of the nascent neurophysiological model of consumer choice (BPM-N; Foxall, in preparation). Both of these derive from Dennett's (1969) usage. The super-personal level of exposition is that of the regulation of behavior by means of its prior reinforcing and punishing consequences, the process of *operancy*. This incorporation of the super-personal level of exposition, which is not a part of the scheme advanced by Dennett, represents *the first way in which Intentional Behaviorism differs from his approach to intentional psychology*. Super-personal analysis is the basis of the extensional model of consumer choice (Foxall, 2007b), the use of which establishes the bounds of behaviorism, indicating whether and if so where an intentional interpretation is required and the form it might need to take. Hence, this level of exposition permits the search for generalizations about the breakdown of extensional explanation. The spheres of scientific endeavor which reveal the limitations of behaviorist method are:

> *Misrepresentation*, the inability to identify the stimulus field necessary to explain an observed behavior or the failure of an empirically available stimulus field to engender the behavior in question: in either instance, there is no extensional explanation for the continuity or discontinuity of behavior (Bermúdez, 2003).

> *The explanatory necessity of knowledge-by-acquaintance*, which entails the inevitability of the researcher making inferences about the private behavior of the consumer in order to explain her behavior (McGinn, 2004).

> *Delimiting behavioral interpretation*, which requires resort to intentional explanation in order to constrain the tendency of behaviorists to multiply plausible potential contingencies of reinforcement and punishment which would account in principle for an observed behavior but which are essentially untestable in empirical investigation (Foxall, 2020).

make recourse *inter alia* to measures of neuronal functioning and verbal behavior.) Fifth, the program of establishing the correspondence between cognitive theory and the intentional interpretation leads to (a) further cognitive-psychological research of this kind and (b) specific hypothesis-testing related to economic psychology via the economic disciplines of operant behavioral economics, picoeconomics, and neuroeconomics.

Although his initial presentation of the personal and sub-personal levels appears to set an unbridgeable gulf between them as levels of explanation, a major thrust of his subsequent work has been to blur this distinction and to justify the deployment of intentional language to give and account of neurophysiological and other entities that would normally be seen as falling exclusively within the purview of the physical stance. Even within his first book which has based on his doctoral thesis, *Content and Consciousness* (Dennett, 1969), blurring tendencies can be identified. In the context of Intentional Behaviorism, however, it is axiomatic that the three levels of exposition, each characterized by its peculiar mode of explanation, be kept separate. That is, the mind-body problem cannot be explained away by the notion that intentional explanation is no more than an additional layer of linguistic description imposed upon physical reality. McGinn (2004) argues not only that the difference between knowledge-by-acquaintance and knowledge-by-description is itself sufficient to make clear that there is a mind-body problem but that humans currently lack the biconditional concepts that are a necessity if this gap is to be spanned. The intentional behaviorist program proposes that a bridging concept is available in the form of "Janus-variables" which look both ways from the personal level of intentional exposition towards the super-personal and sub-personal levels.

The methodology of Intentional Behaviorism differs then in important regards from Dennett's scheme. Consistent with the employment of a super-personal level of exposition is the former's incorporation of an initial, extensional mode of explanation the operation of which, as indicated, delineates the bounds of non-intentional explanation and indicates not only the necessity of an intentional interpretation but the missions it needs to undertake. The deployment of a model of economic behavior with the objective of achieving theoretical minimalism not only suggests the form of intentional interpretation but constrains it within the bounds of what is objectively known about the factors that influence consumer choice. Precise understanding of the role of functional and social reinforcement on the rate of consumption, notably the demonstration that consumers tend toward maximizing bundles of utilitarian and informational reinforcement, militates against fanciful intentional interpretation of consumer behavior. The roles of the stimulus field in shaping consumer choice and its activation through the action of her consumption history provide an empirically tested framework of analysis for the construction of the intentional consumer-situation from which further activity is likely to stem and give some idea of the form it is likely to take. Although prediction of consumer action on the basis of these environmental variables is not the goal of the intentional interpretation, nor necessary to its evaluation, the existence of a body of knowledge compiled within a framework of extensional model building and testing gives shape to any predictions that are made on the basis of intentional reasoning and suggests lines of inquiry that would permit their empirical evaluation. *This avoidance of what can only be relatively minor, even marginal predictions made on the basis of an idealized intentional account of behavior, constitutes the second deviation of Intentional Behaviorism from Dennett's methodology.*

The third stage of the Intentional Behaviorism research strategy, *cognitive interpretation*, proposes a criterion for acceptance of intentional interpretation in the form of its revealed consistency with theories of cognitive structure and functioning. Predictive validity is nevertheless an important component of the strategy in that the procedures inherent in cognitive interpretation are expected to generate economic-psychological hypotheses for empirical testing. The relationships between the personal level of behavior or action and the sub-personal level of neurophysiological functioning are captured by neuroeconomics, that between the personal and super-personal levels by operant behavioral economics. Each of these can inspire and lead to the testing of hypotheses linking behavior to the extensional sciences that bear upon it. There is, in addition, a third form of economic analysis, *picoeconomics*, which deals with the strategic interaction of intra-personal subagents that respectively represent the pursuit of short-range and long-range interests, both of which find correlates in sub-personal and super-personal influences on action. The methodology involved in cognitive interpretation and economic hypothesis generation and testing emphasizes the second deviation of Intentional Behaviorism from Dennett's three kinds of intentional psychology framework.

The subsequent task of Intentional Behaviorism, the determination of the point at which extensional explanation breaks down, has involved the identification of three areas of behavioral science in which it is impossible to provide an account of behavior in terms of the stimulus field suggested by an extensional model. These are, first, the explanation of aspects of the continuity and discontinuity of behavior; secondly, accounting for features of consumer behavior that make necessary an account of the personal level of experience; and, thirdly, the delimitation of behavioral interpretation. Each of these "bounds of behaviorism" is associated with a corresponding "imperative of intentionality" which opens up the possibility of intentional explanation. As a result, the intentional BPM explains consumer behavior in terms of a consumer-situation comprising the consumer's perceptions of the consumer behavior setting which faces her and the reconstructed memories of her consumption history.

The use of the cognitive interpretation and subsequent economic analyses allows Intentional Behaviorism to avoid a problem that arises in Dennett's use of *illata* as the basis of SPCP. *Illata* are an attempt, though Dennett does not use this term and the mind-body problem does not arise as such in his ontology, to overcome the mind-body problem, the challenge to explain mental phenomena in a materialist universe. *Illata*, it will be recalled, are physical entities that are intentionally characterized. An example is the application of the intentional stance to, electrons or neurons, where these physical entities are predicted and partially explained through the ascription to them of desires and beliefs. Although Dennett's proposal is ingenious, the problem is that such an attribution transgresses the mereological fallacy in which attributes that properly apply only to whole systems are ascribed to its parts (Bennett & Hacker, 2003). The mind-body problem is not amenable to easy resolution, and further discussion is impossible here, but the alternative which Intentional Behaviorism proposes is the attribution to the consumer of intentional objects, i.e., mental representations of objective exchange values which provide subjective evaluations that guide behavior. *This procedure which employs cognitive interpretation and economic analyses comprises the third respect in which Intentional Behaviorism differs fundamentally from Dennett's triple model of intentional psychology.*

The Bounds of Behaviorism

If there were an extensional device that could provide scientific explanations, i.e., which are of similar rigor to those obtainable in the operant laboratory, of behavior for which no stimulus field or learning history is empirically available, then my case for the limitations of radical behaviorist explanation, the "bounds of behaviorism," would fail. Moreover, if, as Oliveira-Castro suggests, my initial evaluation of attempts to fill the explanatory gap by assuming a learning history that is essentially an extrapolation from the behavior it is intended to explain, was too severe, let me say simply that such constructions surely go beyond normal scientific practice. This is especially the case if they are offered as explanations rather than attempts to improve the empirical status of the analysis. As has so often been observed, a theory that explains everything explains nothing. Radical behaviorist interpretation must surely rest on more certain foundations, and their discovery depends on identifying the bounds of behaviorism.

The proposals suggested by Oliveira-Castro are that we take note of the "dispositions," "abilities," and "propensities" of the individual to fill the explanatory gap. He argues that:

> The use of concepts describing what the individual is capable [of doing] or prone to do might solve most of the problems raised by Foxall concerning the lack of continuity in behavior analytic explanation. With this, it becomes explicit that behavior analysis research is not describing only responses but also changes in repertoire of the individual, considering what in ordinary language are referred to as abilities and propensities. (Oliveira-Castro, 2021)

This is a sound idea though its execution would entail two consequences that Oliveira-Castro does not allude to. First, it hardly keeps the analysis on a purely extensional level since many of the concepts he advocates are intentional in nature: the concept of dispositions is the most obvious. The second problem is that all of the examples he gives assume that the investigator can observe a pattern of behavior over a period of time in order to gauge the propensities (behavioral tendencies) of the person whose behavior is to be explained. What one is observing is the intertemporal and cross-situational pattern of behavioral tendencies exhibited by the individual: in other words, her learning history! If one had the opportunity to make observations of this kind, one would not have the problem to which I allude: the *lack of a learning history!* There is no way by which we can gain knowledge of the individual's repertoire other than by observing her history of reinforcement and punishment.

Let us examine in a little more detail the possibility of explanation by dispositions as elaborated by Vanderbeeken and Weber (2002) to which Oliveira-Castro alludes. I do not attempt here a complete analysis of Vanderbeeken and Weber's scheme of dispositional behavioral explanation. I believe, however, that the idea of dispositions these authors pursue is vague: we are asked to conceive them as "properties of systems that refer to possible causal relations" (Vanderbeeken & Weber, 2002, p. 43). Then "we can explain behavior B of a system x by (i) referring to a situation of type S that triggered B, given that x has a disposition D to do B in S, or (ii) by referring to a disposition D of x to do B in S, given that x is in a situation of type S" (*ibid.*) These authors' system as they present it embraces radical behaviorism since:

> According to Radical Behaviorist explanations, a system has a disposition D to do R, due to the presence of a set of unspecified internal causal factors that are the result of a history of reinforcement. (Vanderbeeken & Weber, 2002, p. 46)

The crucial element in a dispositional explanation of behavior is the manner in which dispositions are ascribed to systems. As it stands, the formula provided by Vanderbeeken and Weber fits very well with Dennett's use of the intentional stance to ascribe the intentionality that is appropriate to a system by dint of its history and circumstances. This affinity is rendered all the closer by the recognition that explanation based on how an individual is disposed to behave is an intentional explanation. However, if the notion of causal dispositions is to remain within the ambit of radical behaviorism, then what becomes crucial is the manner of attributing dispositions. "Dispositional properties of a system state that a certain type of causal relation will take place when the system is in situations of type S" (Vanderbeeken & Weber, 2002, p. 46). This is tantamount to saying that once we know how to predict the behavior of a system in situations of a particular kind, we can predict well that it will behave similarly when next in a similar situation. The question that arises is that of what is meant by "similar."

I have no objection to the proposal of dispositions as explanatory devices per se. They are useful posits in a behavioral science which acknowledges that the behavior of an organism is necessarily accompanied by internal change, something readily acknowledged and pursued by theoretical behaviorism (Staddon, 2014; see also Kimble, 1996) and somewhat more abortively acknowledged by radical behaviorism which proposes that neurophysiological change accompanies behavioral change but then leaves the pursuit of this to an idealized physiologist. However, dispositions do not solve the problem of explaining behavior in the absence of a learning history.

First, they are in danger of being inferred from the very behaviors they purport to explain, often without further attempt to justify them. Dennett's intentional interpretation of an idealized system is at least tested by its predictive validity and leads to the development of more conceptually sophisticated intentional variables that can potentially participate in psychological research. Intentional Behaviorism tests its idealized intentional interpretation by reference to predictive competence, consistency with cognitive theories and findings, and ultimately in the testing of hypotheses derived from operant behavioral economics and neuroeconomics.

Second, their inference presupposes the very element in radical behaviorist explanation that is lacking in the interpretation of complex behavior (that not amenable to laboratory analysis), namely, a learning history. If we can study behavior in situations sufficiently thoroughly to work out what behavioral dispositions it is sensitive to, we have sufficient knowledge of the environmental determination of the behavior to enable us to explain it by reference to the discriminative stimuli, motivating operations, and reinforcing and punishing consequences in terms of which its radical behaviorist explanation proceeds; in such circumstances, there is no need of a dispositional theory unless we are moving beyond the traditional radical behaviorist criteria for behavioral explanation, namely, prediction and control.

Third, there may be more suitable (i.e., more thoroughly conceptualized and empirically tested) concepts that seek to identify and use dispositions to behave: for instance, the theory of reasoned action (Fishbein & Ajzen, 2010) and the theory of planned behavior (Ajzen, 1985). Their concepts are empirically based and their results derive from rigorous testing of verbal behavior through behavioral prediction. They are sensitive to situational influences on behavior. But they are thoroughly cognitive in their conceptual and explanatory orientations. The independent variables they employ (attitude toward the act, subjective norm, behavioral intention, and perceived behavioral control) cover all manner of "behavioral dispositions," which cohere well with a dispositional approach to radical behaviorism (Foxall, 2007a, 2020).

Finally, there is no reason these variables could not be incorporated into Intentional Behaviorism, and other theories that employ dispositions, though it is doubtful they could become part of radical behaviorism since they have been so decisively rejected. However, while Vanderbeeken and Weber (2002, p. 45) argue that the dispositional approach is "not limited to the "reinforcement thesis," by which I understand them to mean that a dispositional explanation need not involve radical behaviorism, Intentional Behaviorism is most assuredly dependent on this paradigm. Without the theoretically minimal first stage, we have no guidance on how behavior is caused under circumstances that are propitious to radical behaviorist explanation (i.e., relatively closed behavior settings). In the absence of such a layer of extensional understanding, we are at a loss to know what it is we have to explain in intentional terms. The three stages of the intentional behaviorist research strategy ensure that the use of intentional idioms is guided responsibly by reference to a strict demonstration of behavior in such settings. It is when we turn to the interpretation of behavior in relatively open settings, in which the rigorous identification of a stimulus field eludes us, that we must be very circumspect. We may refer to observed behavior in this situation as the *target* behavior, i.e., the target of our attempt to provide an account of it that is as rigorous as we can make it under the conditions that prevail. What this means is that, using data on the causation of behavior gained from settings which are as similar as possible to the target situation, i.e., more closed settings in which environmental causation can be shown, and by avoiding the use of radical behaviorist terminology in the target behavior explanation in order to avoid the implication that our interpretation is more scientifically grounded than is the case, i.e., do not invent a stimulus field or learning history; we must not assume that just anything that ensues consequentially from a behavior is its cause ("final" cause in the Aristotelian terms which Rachlin (1994) adopts). The use of intentional terminology is meant to underline this, showing that our interpretation is of a different kind from the explanation we can give of behavior in relatively closed settings. It also emphasizes that we are constructing a body of knowledge of the likely outcomes of the actor's behavior that she could conceivably have had at the time of behaving. This emphasizes the tentativeness of our interpretation and acts as an invitation for other researchers to confront and, hopefully, improve upon our surmise about the cognitive field of the actor at the time of acting.

The Appeal of "Private Events"

Oliveira-Castro agrees with me that the use of "private events" is often intentional explanation in disguise; an example is when it is used to account for covert decision-making that makes observed behavior such as schedule insensitivity intelligible (Foxall & Oliveira-Castro, 2009). The notion of "nonrequired precurrent behavior" strikes me as highly mentalistic, bordering on explanatory fiction. This is true also of auxiliary behavior, occurring "in the head" or "mentally" in ordinary language, and its "metaphorical use" in the sense of claiming the individual acts "*as if*" she were undertaking the mental operations likewise. Oliveira-Castro's idea that skipped private events are "nonrequired precurrent behavior" which increases the probability of appropriate current responding is "not required by the programmed contingencies"—even if they do not occur finally. Responding may be reinforced. With training the precurrent response, "auxiliary behavior" is no longer necessary and ceases: the person is said to solve the problem in the head or mentally. As Oliveira-Castro acknowledges, there are conceptual difficulties here.

The Appeal to "Abilities" and "Propensities"

Speaking of the absence of systematic approaches to learning history in radical behaviorism, Oliveira-Castro suggests that an analysis of "abilities and propensities" might fill this gap. But how are we to ascertain these things except by monitoring behavior? What he has to say about dispositions suggests that they are either mentalistic or so far from being empirically available to the researcher that they function in a very similar way to what the radical behaviorist refers to as explanatory fictions. But Oliveira-Castro denies that they are mentalistic. Citing Ryle and Hacker he argues that in "ordinary language," these concepts have the function of summarizing observations of behavior and predicting certain behavior given certain conditions. But he argues these are vague when it comes to their scientific deployment" and concludes that "If dispositional or power concepts are closed, less vague, predicting specific behavior in specific conditions, they can fruitfully be adopted in scientific discourse." He claims this is what behavior analysis does, albeit imperfectly. Hence:

> The use of concepts describing what the individual is capable [of doing] or prone to do might solve most of the problems raised by Foxall concerning the lack of continuity in behavior analytic explanation. With this, it becomes explicit that behavior analysis research is not describing only responses but also changes in the repertoire of the individual, considering what in ordinary language are referred to as abilities and propensities. (Oliveira-Castro, 2021)

An appeal to "abilities and propensities" appears mentalistic and, more important, requires that we know what the repertoire is: other than by observing patterns of behavior, I do not know how this is feasible and if we are able to observe a sequence of behaviors, we have a learning history.

The Imperatives of Intentionality

The Need for a Personal Level of Exposition

Oliveira-Castro readily acknowledges my point that a personal level of exposition is required. His noting the unwillingness of behaviorists to make a thorough analysis of intentional idioms and their use is valid, and I have made frequent mention of it. He writes that:

> There is a tendency in behavior analysis to avoid using mentalistic concepts and little effort to understand how they function, how they are used, and employed in language, that is, what the verbal contingencies for their uses are… They do not refer to mysterious events but have complex uses that have several other functions…. (Oliveira-Castro, 2021)

It is often true that behavior analysts avoid mentalistic concepts, but it is equally true that these concepts are often found in behavior analysis, from Skinner in his more popular works to current behaviorists in their expositions of the philosophy of behavior analysis (Foxall, 2009).

My severity with behavior analysts is closely related to Oliveira-Castro's observation. It is directed towards radical behaviorists, no doubt a small minority, in contexts where they eschew what they refer to as explanatory fictions in the form of intentional language only to propose learning histories that are derived solely from the behavior they are intended to explain, with no further attempt to access the required patterns of past behavior empirically.

Extensional Language and Intentional Language

I agree with Oliveira-Castro that the intentional extensional distinction is genuine and allows "behavioral description to be separated from psychological explanation." There is no need to justify this separation: radical behaviorism, built on the tenets of Machian positivism, is a matter of describing behavior. There is no reason why this would preclude an intentional interpretation. The distinction need not involve a difference in ontology, only in the languages employed. Neither is it a requirement that the intentional be identified with the mental, though Oliveira-Castro seems determined to equate them (p14). As Crane (1998; see also Crane, 2001, 2009, 2016) has made abundantly clear, there is no reason to accept that the intentional is the "mark of the mental" as Brentano (1973/1874) argued: pain, depression, and anxiety are all mental events that are not intentional. The same may be true of sensation and some forms of perception (see, inter alia, Searle, 1983; Strawson, 2009).

Epistemological Limitation or Absence of Empirical Evidence?

Oliveira-Castro asserts that I display a "tendency to conclude that there are insurmountable epistemological limitations in behavior analysis in contexts where absence of empirical evidence is the problem." My line of argument is, indeed, that in the absence of a stimulus field it is necessary to adopt intentional language. He asks how one is to ascribe intentionality in the case of the adoption of an innovation and says that an intentional account would require additional information in the form perhaps of the verbal behavior of the consumer. He raises the point that this additional information would equally be available to an extensional, radical behaviorist account. Of course it would. But the point is that verbal behavior is itself intentional; its use to describe behavior necessarily entails intentional explanation (Foxall, 2016b, 2020).

Oliveira-Castro asks how would one know the person had such desires and beliefs? Presumably through their verbal behavior, insofar as it can be taken as veridical. If we can know they "know" them, we can be sure they have the accompanying desires and beliefs. We can either use Dennett's strategy: that ascribing the apt desires and beliefs to them is successful if it predicts well, or—as Intentional Behaviorism makes clear—ascribe them on the basis of their specific and generic behavior.

The point about the man variously described by Rachlin (see his Rachlin, 1994) as wielding a hammer, building a wall, and so on within a framework of nested consequences is that the expanding sequence of possibly relevant contingencies (final causes that are taken as explanatory) may, if we have no means of delimiting it, ramify endlessly. Surely no one would imagine that the deployment of nuclear weapons could have been a *causal* consequence of scientists' splitting the atom. We have to limit this kind of behavioral interpretation by ascribing the knowledge the person could reasonably be expected to have had. This requires the ascription to the individual of the intentionality that could feasibly have guided her actions.

The Search for Intentional Objects and Representations

Oliveira-Castro is fearful that the search for intentional objects and representations might initiate a quest to discover "mysterious objects" which have no counterpoint in reality. In the case of hyperbolic discounting, I contend that the rewards are not available except in the mind of the individual or, if this is too great a concession to idealism, then let us say describable only in intentional terms. I think this is a fundamental difference between us. I also argue that, while some decisions may be taken instantly as long as the decision-maker has sufficient learning history to permit operant conditioning to take place, decisions typified by the kind of conflict described by Ainslie (1992) between short- and long-term interests often involve deliberation. This is especially apparent in the strategies Ainslie describes for the forestalling of temptation to select a smaller sooner (SSR) over a larger later reward (LLR), and is most notable in the case of what he calls "bundling," in which a stream of future consequences of sustained SSR choice is considered in relation to a stream of future consequences of LLR choices. The only point of this, as far as I can see, is to compare the two kinds of outcome, cognitively, and to evaluate the two kinds of consequence prior to attempting to effect behavior change. Comparison is indeed required to make such a choice. "Where is any future event before it occurs?" he asks, "Must it be located anywhere?" I believe so: even if it does not need to exist in physical form, it must be present as some kind of representation at least.

Oliveira-Castro also makes several points about the availability of the keys to an experimental animal. As far as the keys being present are concerned, they are indeed available, but my point is that the rewards themselves as physical entities are not. That is, they are not empirically available to either the actor or the investigator. They may be said to exist (a) in the mind of the actor, (b) in the rules she has been given by the experimenter, or (c) as represented by the keys. But they cannot be said to exist in the experimental space as behaviorism understands it. One of the reasons for resorting to intentional language is to emphasize that our explanations are no longer in touch with the stimuli and responses that comprise the pattern of events that make up a radical behaviorist account but, in the absence of discriminative and/or consequential stimuli, and/or responses that we can point to in order to describe or explain behavior, we have had to resort to an alternative mode of explanation. This makes clear the sort of entities we are proposing to account for the behavior—they are of a different kind from those described in extensional language and need to be treated as such; this is more straightforward than resorting to assumed learning histories.

Exemplar: The Innovative Consumer

Oliveira-Castro inquires of the nature of an explanation of innovation in intentional behaviorist terms. Consumer innovation makes a very convenient case study with which to illustrate what I have been attempting to argue in this chapter. Innovations range from those which are minimally disruptive of the consumer's present pattern of behavior (continuous innovations) to those which are maximally so (discontinuous). Line extensions and other new products that entail but a small degree of change exemplify the former, while the latter are epitomized by major kinds of novelty like cell phones and driverless cars, the opportunity to book a future space flight, or posthumous cryogenic preservation. On a continuum of innovations, of which these kinds are the polar extremes, one also encounters dynamically continuous innovations, which entail a step change in disruptiveness: for instance, a software app that provides feedback on healthy activities or a supermarket's "click and collect" operation. Consideration of these sources of originality is especially pertinent to an exposition of Intentional Behaviorism because it illustrates well the effects of behavioral continuity and discontinuity.

Innovations of various kinds are available to intermediate and final consumers, i.e., industrial users and shoppers. Many consumer innovations are fast-moving non-durables like foodstuffs; others are durable goods such as cars and washing machines that need to be replaced relatively infrequently. Industrial purchases also include non-durables such as oils that are employed in production processes and durables like capital machinery that have a long life of productive service. The following discussion is concerned predominantly with fast-moving consumer goods because the principles involving the extensional and intentional explanations of innovation are most clearly seen there but they apply more generally.

We can model the consumer's behavior in terms of three behavioral stages: *Pretrial–Trial–Adopt. Trial* entails the building of a learning history. As it continues, the consumer's behavior becomes increasingly explicable in terms of her prior purchasing and consumption responses and their outcomes. *Adoption* is the inclusion of brand in the consumer's repertoire so that repeat purchase based on some degree of loyalty is the norm (repeat may be on *every* shopping occasion for this product class or sometimes or very occasionally). The point is that her behavior has become explicable in terms of an observable learning history based on frequency of purchase. The major focus of our interest here, *pretrial*, is awareness of the brand as a new member of the product class, evaluative comparison with the brands that currently comprise the consumer's repertoire (i.e., entry to her consideration set). There is no specific learning history with respect to the innovation. The only stimuli available to her are initially neutral elements in her stimulus field, though these might be words like "New" or "Innovative" that have been linked via respondent conditioning to other brands (some of which she has purchased, many of which she has not even tried let alone adopted: so this is not determinative of her behavior).

Consumers of final products such as foodstuffs typically purchase a subset of the available brands which they see as functionally equivalent and interchangeable.

This range of brands is known as the consumer's consideration set or repertoire. Few consumers are 100% loyal to a particular brand: the majority purchase among the members of their repertoire on what seems superficially to resemble a random sequence. When a new brand in such a product class is introduced to the market, a proportion of users of that class trial it, and some of these go on to include the new brand within their repertoire. Contrary to popular understanding, a new brand in order to be successful does not need to be dramatically different from existing brands: consumers are looking for brands that perform a particular set of services. A new brand does need, however, to offer greater value for money than available items, e.g., a lower price for the same quality or enhanced performance at the same price. The consumer by definition has no learning history with regard to this brand; she has only the verbal behavior of the marketer, usually in the form of mass advertising, to inform her of the novel brand's claims. Alternatively she might simply see the new brand on the supermarket shelf and read the label. Either way the procedure is the same. At this initial stage, that of the consumer's deciding whether to trial the new item, she must determine whether the novel brand is likely to perform as well as those she is currently using: I cannot see how this can be effected other than by her comparing her experience of the brands that compose her current repertoire with the claims made by the advertiser and coming to a decision about whether to proceed. She is assessing the value for money offered by the established and novel brands and is coming to a conclusion about whether to give the new brand a chance. This is necessarily an abstract process; it need not be thoroughly explicit in the sense of requiring formal calculation and a once-for-all decision. But it must be sufficiently thorough as to ensure that when she is faced with the brand on a future shopping trip, she can pick it up or ignore it.

Being presented with a new product or brand offers an opportunity to practice behavioral discontinuity in some degree: the adoption of an altered pattern of consumption. Anyone whose explanation of behavior is confined to the extensional would point to the similarities between the situations in which a consumer has purchased prior to the appearance of the innovation and that which obtains after the new item has been launched on to the market. She can point to similarities between the discriminative stimuli and motivating operations under whose control the pre-innovation pattern of purchasing has been established. Although the new stimulus field accompanying the novel item is not identical to those previously in existence, the assumption can be made that they are sufficiently similar as to maintain the same pattern of responding which now takes in a new brand say as an item very similar to the existing brands. There is sufficient correspondence for the investigator to be able to say confidently that any new stimuli in the consumer's behavior setting are functionally equivalent to those governing her earlier behavior before the innovation was introduced. Even in the absence of experimental evidence to support this view, the purveyor of an extensional explanation can argue that a behavioral *interpretation* can be plausibly constructed on the basis of this alleged continuity. After all, new brands in established product classes must, in order to succeed, provide the same services to the consumer as do existing brands (Ehrenberg, 1988).

Alternatively, an investigator who is not satisfied with this can argue that the consumer has no learning history of using this particular brand. The verbal stimuli provided in mass advertising, point-of-sale displays, and on packaging cannot be said to be genuine discriminative stimuli and motivating operations because there has been no period of training in the course of which they have acquired the necessary functions. We may *presume* that the consumer has responded to earlier verbal claims by this or similar advertisers and therefore has a learning history of acting in accordance with instructions or promises of a similar nature to those now presented. However this is no more than surmise. How far are we willing to stretch the control of operant conditioning?

If the present stimuli (including those that comprise the current consumer behavior setting including the verbal stimuli contained in the advertiser's claims) were functionally equivalent to those controlling current choices of brands within the consumer's repertoire, the new claims would simply influence the consumer to buy the new brand, and it would automatically become part of her consideration set. The fact that this is a rare—perhaps never occurring—outcome of a new brand introduction is sufficient to suggest that the new stimuli differ in important respects from those made available in the case of established brands that compose the consumer's repertoire. The trial of a new brand is not an automatic occurrence. So the brands cannot be said to be functionally equivalent. Remember we are not saying that the test of the presence of operant principles is that the new brand immediately becomes the sole brand that the product class user considers, buys, and consumes: we are only saying that the consumer should be expected to include the new brand immediately in her consideration set if the argument is that the new brand is functionally equivalent to brands currently in use. If she does not, then the verbal claims of the new advertiser are not acting as discriminative stimuli or motivating operations. Exactly as behavior theory would suggest, they would need to be involved along with established stimuli in a training procedure which would bring the consumer's behavior under the stimulus control of the new claims. This evaluative process requires the comparison of values which are abstract.

We have considered thus far the trial of a new brand introduced into an existing product class. As one proceeds along the continuum from the continuous to the discontinuous innovation, one encounters a decreasing availability of a learning history and a more vague description of the rewarding and punishing consequences of innovative behavior on the part of the consumer. The more discontinuous the innovation the more abstract is the process. The consumer who is presented with the opportunity to prebook a space flight or reserve a posthumous place on a cryogenic preservation program clearly has no learning history and a less concrete description of the likely outcomes of consumption.

Moreover, a verbal claim in an advertisement or on a packet label is itself intentional. For a promotional claim is *about* something and the more discontinuous the innovation the more probable it is that the claim is about an entity that cannot be well-defined. To employ verbal behavior in an explanation of behavior is to employ intentionality.[3]

The lack of a learning history indicates that radical behaviorist explanation has run its course. There is no alternative but to resort to intentional language. The consumer engages in mental comparison of the new item with the members of her current repertoire, evaluating each item against her criteria of the item in use.

One can see the uniqueness of the trial stage by comparing it with the process by which a consumer determines whether she will incorporate the novel item in her repertoire, i.e., repeatedly purchase it. In this case the consumer is developing a learning history by occasionally trying the new brand. She now has her experience of the brand, i.e., how to use it in practice and the utilitarian and informational consequences of doing so. The decision to incorporate the brand into her consideration set is not instant but takes place over a protracted period in which the results of its usage are built up and the researcher has the consumer's actual learning history to work on in predicting the likelihood that she will repurchase. This is very different from the pre-trial stage in which no learning history is available and the cues available to the consumer have not become discriminative stimuli and motivating operations. The consumer's behavior here must be put down to her intentionality.

If the cues provided by the new advertising/pack were functionally equivalent to the discriminative stimuli and motivating operations that control present purchasing within the product class, the consumer would immediately add the innovation to her repertoire by beginning to purchase it at a rate similar to that of at least the least-frequently purchased item in her repertoire. She does not. She engages in pretrial behavior in which the new item is infrequently bought, most probably dropped. Since most new continuous products are adopted by only a small proportion of the existing consumerate, most fail at this stage: perhaps as many as 80–90%. Even some 50% of new products that have been through the most rigorous and exacting test marketing procedures, allowing behavioral (purchase) data to be included in projections of future sales, fail at the point of consumer acceptance (Foxall, 2015). The behavior of the consumer who *adopts* the new brand is explicable (or at least predictable) by reference to her learning history and the current stimulus field she encounters. But this is a far cry from the behavior of the consumer who is engaging in *pretrial* and *trial*.

I conclude that there is an important lack of a stimulus field for innovative behavior and that this requires explanation in terms of the consumer's desires, beliefs, emotions, and perceptions. The foregoing descriptions of the intentional BPM deals briefly with the construction of the intentional consumer-situation and, elsewhere, I

[3] A thoroughgoing radical behaviorist account of verbal behavior, proceeding in extensional terms, would deal solely in the portrayal of words (and gestures, etc.) as physical discriminative stimuli, sounds articulated by voice, pen, or machine (Foxall, 2016a, pp. 254–264, Foxall, 2020, pp. 59–61). Responses thereto would be learned in the same way as responses to any other aspect of the stimulus field. A symphony would have no meaning other than as an intraverbal sequence engendering a series of aural responses to each note or chord.

have provided much more detailed expositions of the procedures involved (e.g., Foxall, 2016a).

Conclusions

I have discussed Oliveira-Castro's comments under three headings. The first is the clarification of the nature of Intentional Behaviorism in light of Dennett's conception of three kinds of intentional psychology. The second and third follow from Oliveira-Castro's consideration of Intentional Behaviorism as a criticism of radical behaviorism explanation (the "bounds of behaviorism") and then as the claim that such limitations can be overcome through use of intentional language (the "imperatives of intentionality").

I have argued that Intentional Behaviorism differs from Dennett's proposal for intentional psychologies in three important respects. The first is the incorporation of a super-personal level of exposition, allowing the deployment of an extensional model initially to guide empirical research with the aim of indicating whether and if so where an intentional interpretation is required and the form it might take. This leads to the identification of the bounds of behaviorism and the imperatives of intentionality. This insistence on an initial extensional stage of behavioral investigation to demarcate the need for intentionality and to guide the development of an intentional interpretation avoids the *a priori* assumption that intentional explanation is inevitable. The second point of departure is found in Intentional Behaviorism's seeking the evaluation of the idealization of the consumer as an intentional system not simply by prediction but, initially, by ascertaining this interpretation's consistency with findings of cognitive psychology and, subsequently, the formulation of tests based on economic analyses, predominantly operant behavioral economics and neuroeconomics. The third is the proposal of Intentional Behaviorism that the various levels of exposition it invokes can be shown to be interdependent on the basis of the attribution of appropriate intentional objects that reflect objective exchanges.

I have argued further that, while radical behaviorism is indispensable in the initial stage of investigation, it is limited as a comprehensive methodology for the explanation of behavior. Specifically, it cannot deal on its own terms with important aspects of behavioral continuity and discontinuity, or supply the required understanding of the personal level of exposition that a complete explanation of behavior demands, or delimit its own interpretations of complex behavior. The absence of a discriminative stimulus is far from a "fragile" consideration: its reality hits at the heart of the kind of explanation to which radical behaviorism aspires (Smith, 1988, 1994; Zuriff, 1995). Intentional Behaviorism, while retaining the subject matter of radical behaviorism, extends where necessary the kind of language appropriate to behavioral explanation.

References

Ainslie, G. (1992). *Picoeconomics: The strategic interaction of successive motivational states within the person*. Cambridge University Press.

Ajzen, I. (1985). From intentions to actions: A theory of planned behavior. In J. Kuhl & J. Beckman (Eds.), *Action control: From cognition to behavior* (pp. 11–39). Springer-Verlag.

Bennett, M. R., & Hacker, P. M. S. (2003). *Philosophical foundations of neuroscience*. Blackwell.

Bermúdez, J. L. (2003). *Thinking without words*. Oxford University Press.

Brentano, F. (1973). Psychology from an empirical standpoint. London and New York: Routledge. (First published 1874).

Crane, T. (1998). Intentionality as the mark of the mental. *Royal Institute of Philosophy Supplement, 43*, 22–251.

Crane, T. (2001). Intentional objects. *Ratio, 14*, 33–349.

Crane, T. (2009). Intentionalism. In B. P. McLaughlin, A. Beckerman, & S. Walter (Eds.), *The Oxford handbook of philosophy of mind* (pp. 47–493). Oxford University Press.

Crane, T. (2016). *The mechanical mind* (3rd ed.). Routledge.

Dennett, D. C. (1969). *Content and consciousness*. Routledge and Kegan Paul.

Dennett, D. C. (1981). Three kinds of intentional psychology. In R. Healey (Ed.), *Reduction, time and reality* (pp. 37–61). Cambridge University Press.

Dennett, D. C. (1991). Real patterns. *Journal of Philosophy, LXXXVIII*, 27–51.

Ehrenberg, A. S. C. (1988). *Repeat-buying: Facts, theory and applications* (2nd ed.). Griffin.

Fishbein, M., & Ajzen, I. (2010). *Predicting and changing behavior: The reasoned action approach*. Psychology Press.

Foxall, G. R. (1990/2004a). Consumer psychology in behavioral perspective. London and New York: Routledge, Reprinted 2004 Beard Books, Frederick, MD.

Foxall, G. R. (1996). *Consumers in Context*. Routledge.

Foxall, G. R. (1998). Radical behaviorist interpretation: Generating and evaluating an account of consumer behavior. *The Behavior Analyst, 21*, 321–354.

Foxall, G. R. (2001). Foundations of consumer behaviour analysis. *Marketing Theory, 1*, 165–199.

Foxall, G. R. (Ed.). (2002). *Consumer behaviour analysis: Critical perspectives in business and management*. Routledge.

Foxall, G. R. (2004b). *Context and cognition: Interpreting complex behavior*. Context Press.

Foxall, G. R. (2007a). *Explaining consumer choice*. Palgrave Macmillan.

Foxall, G. R. (2007b). Explaining consumer choice: Coming to terms with intentionality. *Behavioural Processes, 75*, 129–145.

Foxall, G. R. (2007c). Intentional Behaviorism. *Behavior and Philosophy, 35*, 1–56.

Foxall, G. R. (2008). Intentional Behaviorism revisited. *Behavior and Philosophy, 37*, 113–156.

Foxall, G. R. (2009). Ascribing intentionality. *Behavior and Philosophy, 37*, 217–222.

Foxall, G. R. (2015). *Corporate innovation: Marketing and strategy*. Routledge Library Editions.

Foxall, G. R. (2016a). *Perspectives on consumer choice: From behavior to action, from action to agency*. Palgrave Macmillan.

Foxall, G. R. (2016b). *Addiction as consumer choice: Exploring the cognitive dimension*. Routledge.

Foxall, G. R. (2017a). *Advanced introduction to consumer behavior analysis*. Edward Elgar.

Foxall, G. R. (2017b). *Context and cognition in consumer psychology: How perception and emotion guide action*. Routledge.

Foxall, G. R. (2020). *Intentional behaviorism: Philosophical foundations of economic psychology*. Academic Press.

Foxall, G. R. (2021). Intentional behaviorism. In D. Zilio, & K. Carrara (Orgs.), Contemporary behaviorisms in debate. Springer/Paradigma.

Foxall, G. R. (in preparation). Styles of interpretation: Themes in Intentional Behaviorism.

Foxall, G. R., & Oliveira-Castro, J. M. (2009). Intentional consequences of self-instruction. *Behavior and Philosophy, 37*, 87–104.

Kimble, G. A. (1996). *Psychology: The hope of a science*. MIT Press.
McGinn, C. (2004). *Consciousness and its objects*. Clarendon.
Oliveira-Castro, J. M. (2021). Behavior analysis and psychological concepts: Commentary on Foxall's Intentional Behaviorism. In D. Zilio, & K. Carrara (Orgs.), Contemporary behaviorisms in debate. Springer/Paradigma.
Oliveira-Castro, J. M., & Foxall, G. R. (2017). Consumer maximization of utilitarian and informational reinforcement: Comparing two utility measures with reference to social class. *The Behavior Analyst, 42*, 457–476.
Oliveira-Castro, J. M., Cavalcanti, P. R., & Foxall, G. R. (2016a). What consumers maximize: Brand choice as a function of utilitarian and informational reinforcement. *Managerial and Decision Economics, 37*(4–5), 360–371. https://doi.org/10.1002/mde.2722
Oliveira-Castro, J. M., Cavalcanti, P., & Foxall, G. R. (2016b). What do consumers maximize? The analysis of utility functions in light of the Behavioral Perspective Model. In G. R. Foxall (Ed.), *The Routledge companion to consumer behavior analysis* (pp. 202–212). Routledge.
Quine, W. V. O. (1960). *Word and object*. MIT Press.
Rachlin, H. (1994). *Mind and behavior: Roots of modern psychology*. Oxford University Press.
Ryle, G. (1949). *The concept of mind*. Hutchinson.
Searle, J. R. (1983). *Intentionality: An essay in philosophy of mind*. Cambridge University Press.
Skinner, B. F. (1938). *The behavior of organisms*. Appleton.
Skinner, B. F. (1957). *Verbal behavior*. Copley.
Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis*. Prentice-Hall.
Skinner, B. F. (1974). *About behaviorism*. Cape.
Skinner, B. F. (1984). Response to Stalker and Ziff. In A. C. Catania & S. Harnad (Eds.), *The selection of behavior: The operant behaviorism of B. F. Skinner: Comments and consequences* (pp. 207–208). Cambridge University Press.
Smith, L. D. (1988). *Behaviorism and logical positivism*. Stanford University Press.
Smith, T. D. (1994). *Behavior and its causes: Philosophical foundations of operant psychology*. Springer.
Staddon, J. E. R. (2014). *The new behaviorism* (2nd ed.). Psychology Press.
Strawson, G. (2009). *Mental reality* (2nd ed.). MIT Press.
Vanderbeeken, R., & Weber, E. (2002). Dispositional explanations of behavior. *Behavior and Philosophy, 30*, 43–59.
Zuriff, G. (1995). *Behaviorism: A conceptual reconstruction*. Columbia University Press.

Part VI
Contextual Behaviorism

Chapter 16
Contextual Behavioral Science as a Distinct Form of Behavioral Research and Practice

Steven C. Hayes

Contextual behavioral science (CBS; Hayes et al., 2012; Zettle et al., 2016) is a form of behavioral thinking, composed of six primary features:

1. A refinement of radical behaviorism into a similar but distinct philosophy of science known as functional contextualism.
2. The development of Relational Frame Theory (RFT) as an approach to human language and cognition.
3. The development of a reticulated research strategy to assemble a viable long-term approach to improving the understanding of human complexity.
4. The willingness to take topographically mentalistic terms seriously if they turn out through functional contextual analysis to orient applied and basic work that is behaviorally sensible.
5. Nesting functional contextual behavioral psychology under the extended evolutionary synthesis afforded by multi-dimensional, multi-level evolution science.
6. The pursuit of models, theories, and applied program based on evolution science and behavioral principles as expanded by RFT that facilitate the prediction and influence of behavior, with precision, scope, and depth. For example, this would include the development of the psychological flexibility model as an approach to psychopathology and its treatment; the development of Acceptance and Commitment Therapy (ACT) as an approach to the modification of psychological flexibility; or the development of Prosocial, a combination of ACT, psychological flexibility, and Elinor Ostrom's Nobel Prize winning Core Design Principles for cooperation in small groups.

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In a way, it is historically backwards to speak first of CBS as a tradition or approach because as it was lived, there was no attempt to build an approach. The first use of the term "contextual behavioral science" occurred after the beginning of this millennium—whereas the assumptions, choices, theoretical developments, and data that lead to this distinct approach occurred decades earlier. It's useful to use this recent term (or "contextual behaviorism," an equivalent term that is sometimes used within behavioral psychology itself), because CBS is a large and vigorous field. Its international association (the Association for Contextual Behavioral Science; www.contextualscience.org) is apparently the largest of all of the behavioral psychology organizations, with nearly 10,000 members and 44 chapters around the world in 19 different language communities. Its journal, the *Journal of Contextual Behavioral Science*, is the most widely subscribed behavioral journal in the world, with an impact factor that rivals or exceeds that of many of the best established behavioral journals. The span of research on CBS topics covers several thousands of studies, both basic and applied. Unlike many forms of behaviorism that are defined by a single individual or a small group of scholars, CBS encompasses myriad scholars and researchers across the world. While I may have instigated CBS in the sense that my laboratory helped start ACT, RFT, functional contextualism, psychological flexibility, Prosocial, and other such areas, there are hundreds of major researchers and practitioners around the world who are actively shaping CBS and all of its major features and facets.

If the emergence of CBS is told in a linear way, three issues in the list above began the journey: items 2, 4, and 6. Traditional behavior analysis had a difficult time addressing human language. An issue of that importance demanded an answer: How can behavioral psychology address the issue of human language and cognition in a way that was more adequate to the challenge of human complexity? Early on a key choice was made: complex issues such as meaning, purpose, experience of self, or spirituality needed to be examined carefully even if the terms describing these domains were topographically "mentalistic" (Hayes, 1984). The need for a different approach to the philosophy of science underlying behavior analysis (item 1) only became clear after it was evident that RFT led to a key finding: the transformation of stimulus functions produced by relational frames. When it became clear that relational learning altered how other behavioral principles operated, then it was important to lay out very clear analytic assumptions that could help research untangle behavior–behavior relations in a systematic way (Hayes & Brownstein, 1986). ACT emerged simultaneously with RFT, but the formation of CBS as a specific scientific development strategy nested under evolutionary science (items 3 and 5) and the creation of additional applied programs on that basis (some of the elements of item 6) have only occurred within the last two decades.

In this short chapter I will begin with a précis of relational frame theory and then examine the implications of the decision to address domains that were topographically mentalistic. I will then describe functional contextualism. As that point the implications of this approach will be clearer, and we will briefly address why a reticulated research strategy is necessary, why theory matters, and why CBS is part of evolution science. Finally, the creation of CBS models and methods will be mentioned.

Why Relational Frame Theory Led to Refinement of Radical Behaviorism

Two bodies of empirical literature set the stage for RFT. The first of these was the literature on rule-governance. Behavioral researchers had long known that humans often do not exhibit patterns of responding typical of other animals when exposed to schedules of reinforcement (Leander et al., 1968), and that instructions often exert more control than direct, programmed contingencies (Lowe et al., 1978). Research in my laboratory and that of others found that relatively innocuous instructions can produce a marked tendency to persist in the face of strong contradictory contingencies (e.g., Hayes et al., 1986a; Hayes et al., 1986b).

It was not initially clear why these effects existed. A superficially satisfactory answer was to suggest that they were features of rule-governed behavior, but that only moved the mystery since Skinner never was able to define what it meant to "specify" a contingency. In Skinner's approach, the behavior of the listener was not verbal, and a verbal "stimulus" was merely the product of verbal behavior. That definition of a verbal stimulus was behaviorally incoherent, since it specified a source rather than a function.

The second body of empirical literature were the studies on "stimulus equivalence" (Sidman, 1971). In a matching to sample format, interlocking conditional discriminations (e.g., in the presence of sample A1, and comparisons B1, B2, and B3, pick B1; in the presence of sample A2 and comparisons B1, B2, and B3, pick B2; and so on) generally led to the emergence of (1) reflexivity (e.g., A1 = A1); (2) symmetry (e.g., if A1 pick B1 is trained, then B1 pick A1 is derived); and (3) transitivity (e.g., if A1 pick B1 and B1 pick C1 are trained, then given A1 pick C1 is derived and vice versa; Sidman & Tailby, 1982).

The reason that stimulus equivalence captured my imagination was threefold. First, it did not seem to fit operant or classical conditioning principles. Second, there was an apparent correspondence between the equivalence phenomena and natural language phenomena. Stimulus equivalence could be used to teach naming (Dixon, 1977); and we soon learned that it correlated with verbal performance (Devany et al., 1986) and that it developed over time much as one might expect if it was learned (Lipkens et al., 1993). Finally, it offered clues about how to think about "verbal stimuli" in a functional way if the phenomenon of equivalence could be expanded to cover a wider range of verbal relations.

Working with my colleague Aaron Brownstein, relational frame theory attempted to explain stimulus equivalence as a single learned variety of a larger set of derived relational responses (Hayes & Brownstein, 1985). RFT was the simplest possible kind of behavior analytic theory: it claimed that a phenomenon was an operant.

What was different about relational operants was (1) stimuli could be related to each other based on a history of conventional training in deriving relations, regardless of the *form* of the related events; (2) it was controlled by contextual cues that went beyond the form of the related events and thus could be arbitrarily applied (i.e., applied by social whim or convention to any set of relata); and (3) functions of relata could alter the functions of other relata in a relational network based on additional contextual cues to do so, transformed by the derived relation between them. For example, after learning that "bueno" is the same as "good" and "mal" is the opposite of "bueno," a person who could respond to "good" as a social reinforcer might react to "mal" as a punisher.

In the early days of RFT, the emergence of relational operants was simply explained by a reinforcement history but without an adequate evolutionary account. As with the behavioral account of generalized imitation (Gewirtz & Stengle, 1968; Baer et al., 1967), the claim was simply that many exemplars of a particular relational operant led to formation of a particular relational class. For example, with enough instances of bidirectional name—referent relations trained in both directions, and then in networks—a relational class of coordination or sameness ("equivalence") would emerge controlled by reliable relational cue, such as the word "is" in the sentence "____ is _____".

Because from the beginning this idea was expanded from equivalence to other types of relational operants (e.g., difference, opposition, comparison, and so on), there was a need to talk expansively but with precision about what was trained and what was derived. Relational frames were said to have three defining properties:

Mutual entailment. Mutual entailment means that if by direct training X has relation p with Y in a given context, then by derivation Y has relation q with X in that same context. Any arbitrarily applicable relation must logically entail an inverse relation: better entails worse, larger entails smaller, and so on. In some cases p and q are the same (e.g., sameness, difference, oppositeness), but often relations are not strictly symmetrical (e.g., comparatives, time).

Combinatorial entailment. Combinatorial entailment described the fundamental capacity of relational responses to combine. If by direct training X has relation p with Y and Y has relation p with Z in a given context, then by derivation X has relation q with Z, and Z has relation r with X in that same context. Combinatorial entailment differs from mutual entailment in both complexity and level of specificity. In mutual entailment, the level of precision in the trained relation between X and Y is the same as in derived relation between Y and X. In combinatorial entailment, the relation between X and Z and between Z and X may or may not be specified at the same level of precision as the relation between the bidirectional pairs. For example, if X is "more than" Y, and Z is more than Y, the relation of X and Z is unspecified (Hayes & Hayes, 1992).

Transformation of stimulus functions. If there is a derived relation between two events (say, X and Y), the functions of each may alter the functions of the other based on the underlying derived relation between them. For example, if X is "bigger than" Y, and Y is paired with shock, X may not elicit more around than Y based on a transformation of stimulus functions.

It was not argued that the relational frame was a primitive or ontological unit. Rather, these features were treated as defining features of particular relational frames because all three were necessary for even the simplest explanation of verbal stimuli and verbal rules.

Over the more than three and one-half decades since the initial description of RFT, it has been systematically extended, and its basic and applied predictions have been examined empirically (Hayes et al., 2001). The RFT literature now extends across several hundred studies, both applied and basic. Most of its central claims have by now been tested empirically. To my knowledge, all of those tests have so far been supported. Review of that literature is not the purpose of this paper, but relatively recent summaries are available elsewhere (e.g., Dymond & Roche, 2013).

It immediately became clear that RFT required a refinement of behavioral thinking itself, because relational operants operated on other behavioral processes. There is nothing in behavioral psychology or in a form of the continuity assumption that is sound in terms of evolutionary science (Hayes, 1987) that suggests that new behavioral phenomena cannot evolve in a given evolutionary branch, and the transformation of stimulus functions via relational learning is such a phenomenon.

Operant and classical conditioning are likely about 520–545 million years old based on the fact that all complex organisms that have evolved since the Cambrian period show these forms of learning (Ginsburg & Jablonka, 2010). Relational frames had to be far, far younger because with proper training it occurred readily in normal human infants (Luciano et al., 2007; Pelaez et al., 2000), but did not occur readily or perhaps at all in non-human animals (Lionello-DeNolf, 2009).

Relational framing most likely emerged as an extension of human cooperation (Hayes & Sanford, 2014). In the context of the use of vocalizations to regulate the behavior of others, social referencing (seeking of information from another individual, so as to respond to an object or event; Adamson, 1996), joint attention, nonverbal forms of perspective taking (Tomasello et al., 2005; Woodward, 2005), and skill at non-arbitrary relational learning (see Penn et al., 2008), a human speaker naming an object would be likely to receive that object if the human listener heard the name. That is, an object → symbol relation in the speaker would lead to a symbol → object relation in the listener.

Once mutuality was well established, the other defining features of relational framing—combinatorial entailment, and transformation of stimulus functions (Hayes et al., 2001)—would readily occur. Relational networks would emerge as a matter of operant learning and cultural evolution based on that learning, once derived symmetry was integrated. In the same way, a transformation of stimulus functions would be extended and contextually controlled without additional evolutionary adaptations being required. Mutual entailment, after all, already includes such functional transformations. For example, when hearing “apple” and looking for an apple, the sound has already acquired visual functions because the person can imagine what the apple looks like in its absence. Bringing transformation under specific forms of contextual control could merely involve operant abstraction.

A group with even a few speakers and listeners competent in naming (coordination framing) would be advantaged in their ability to compete with other groups due to the verbal extension of cooperation provided by these abilities. Once coordination framing was common within the group, the emergence of equivalence classes is a matter of integrating already established speaker and listener repertoires. As the initially merely cooperative community became a verbal community, other forms of social relations would provide a template for other types of relational framing. For example, if one person in the troop is bigger than another, and each is named, then a comparative verbal relation could emerge with the names themselves even though the name are never really bigger or smaller as relata (e.g., "Sally is bigger than George"). This could extend based on social whim or convention to comparisons within the troop that are arbitrarily applied (e.g., "dimes are bigger than nickels") establishing the elements of comparative relational framing within that verbal community.

The challenge of relational framing for behaviorism is that for the first time operants were formed that operated on other behavioral principles. One behavior (relating) impacted operant and classical conditioning. For example, if within the troop "x is bigger than y" by social convention and y is paired as a conditioned stimulus with aversive events via classical conditioning, x might in some conditions then produce more arousal than y. This kind of transformation of stimulus functions has been shown to occur in controlled studies (Dougher et al., 2007). It is not a form of generalization of classical conditioning because x and y need not share any physical features, nor would pairing of x and y alone ever give x *more* arousal functions via classical conditioning than y, which was directly paired with the aversive event. Rather x is more arousing than y due to a transformation of stimulus functions via relational framing—but that is another way to say that comparative relational framing has interacted with classical conditioning to produce a new behavioral phenomenon.

The goals of science from a behavior analytic viewpoint were stated by Skinner: "We undertake to predict and control the behavior of the individual organism. This is our 'dependent variable'-the effect for which we are to find the cause" (1953, p. 35). The words "and control" in the phrase "prediction and control" were meant to specify that behavior analysis sought an explanation of behavior that at least potentially allows both prediction and control simultaneously. Given that analytic goal, it would be anathema to view one behavior as *causing* another because behavior–behavior relations cannot be directly manipulated. Forgetting that point would take radical behaviorism into the path of mentalism.

Solving the challenge of behavioral "causes" could be avoided by behaviorism by simple direct contingency account of chaining and the like, but not once the transformational impact of relational operants was known. The beginning steps of a solution was to note the contextual nature of the analytic unit in behaviorism (Hayes & Brownstein, 1986). Any act in behavior analysis is examined in terms of its historical and situational context. Another way to say this is that the "act is context" is the behavior analytic unit. By extension, the impact of one behavior on another also needs to be described in terms of history and situational circumstance (the context). Thus a behavior–behavior relation requires a description of the context in which the first action occurs, the context in which the second act occurs, and the context in which the first action impacts on the second. The unit, in other word, is always the "act in context."

This emphasis on context is familiar to radical behaviorists. The four-term contingency of radical behaviorism is a dynamic spatiotemporal contextual unit, and none of the terms can be defined independently of any of the others. Radical behaviorism is so thoroughgoing in its attempt to analyze context that even the behavior of scientists as they conduct contextual analyses is to be understood through more contextual analyses (Skinner, 1945).

This use of the "act in context" as a unit raises a challenge when one behavior alters the function of another, however, and that is precisely what relational operants appear to do as an empirical fact. One problem with the "act in context" as an explanatory model is that it does not and cannot specify the scope of the act or the context. Context can proceed outward spatially to include all of the universe. Context can proceed backward in time infinitely to include the remotest antecedent, or forward in time to include the most delayed consequence. The "act" in question can vary from the finest muscle twitch to the most elaborate and extended behavioral sequence. Consequently, in behavior analysis, an operant can be of almost any size and, in principle, can be influenced by contingencies that are extremely remote or indirect. Under such circumstances, one might ask how we are to know that a particular contextual analysis (in behavior analysis, a particular "contingency analysis") is adequate?

The philosophical work of Stephen C. Pepper (1942) helped to answer this question (Hayes et al., 1988). Pepper argued that philosophical systems tend to cluster around a few distinct "world hypotheses" or "world views" and most philosophical positions can be subsumed under one or another of four adequate world views: formism, organicism, mechanism, and contextualism. Each world view is characterized by a distinctive underlying root metaphor and truth criterion. Root metaphors are well-understood, common-sense, everyday objects or ideas that serve as a basic analogy by which an analyst attempts to understand the world. Truth criteria are inextricably linked to their root metaphors and provide the basis for evaluating the validity of analyses within the root metaphor. For example, formism is based on the common-sense verbal act of naming classes of events based on their similarities (e.g., blades of grass; sheets of paper). If all analytic tasks are like that, then the analytic question is always "what repeated form is this particular event an instance of?" and the truth criterion is simple unambiguous correspondence between the names of classes of events and the formal features of the classified events. That is why formism is most frequently seen among nosologists.

The root metaphor of contextualism (another name for pragmatism) is the act-in-context, meaning the common sense historic and purposive act, like "going to the store." All acts of that kind have a history (perhaps I am going to the store because I am out of soup), a situational context (I will walk north on 1st Street to get to the grocery store), and a satisfaction (I will arrive at the store and buy the soup). If all analytic tasks are like that, then the analytic question is always "what is the history, circumstance, and satisfaction of which action?" and the truth criterion is whether the analyst also accomplished her ends (metaphorically, did the scientist also "get the soup"). Said in another way, the truth criterion of contextualism is successful working, whereby an analysis is said to be true or valid insofar as it leads to effective action, or achievement of some goal. Just as one person may be going to the store to buy soup, another to buy bread, and still another may be going to the park for a stroll, so too contextual scientists may have a wide variety of goals.

In contextualism, the context must be included in the analysis of an act, because an act out of context is not an act at all. Going to the store and taking a stroll may involve identical leg movements, but that does not mean that taking a walk in the park is the same act as going to buy food. In the same way the four-term contingency is not an assemblage because "The events participating in an operant cannot usefully be examined independently because their nature depends on their relations to the other participants" (Hayes et al., 1988, p. 101).

Contextualism can be oriented toward different goals, and thus there are a variety of scientific contextualisms (Hayes et al., 1993). Many are forms of descriptive contextualism that seek an appreciation of the participants in the whole (e.g., social constructionism, Marxism, dramaturgy, hermeneutics, and so on). Functional contextualism was special only in its goals.

Seeing Skinnerian radical behaviorism as a special form of contextualism (Hayes et al., 1988) explained its environmentalism: Only statements that point to events external to the behavior of the individual organism being studied could lead directly to the accomplishment of prediction and control. It also provided an end point for the scope of the act or the context. "Successful working" provided an answer to this question. An analysis needs proceed only to the point at which successful action can be based on it. In order for that criterion to be applied, if the goal is prediction and influence, it was necessary to link its accomplishment to manipulable events. But it was also necessary to refine that goal and state it more clearly so that it could serve as a guide to successful scientific analysis. I can predict and control the acquisition of money by holding a gun to the next passer-by and saying "your money or your life" but that does not mean I understand charitable giving.

As radical behaviorism was viewed as a variety of contextualism, prediction and control was replaced by prediction and influence—a very small, but needed step. "Control" can refer to the elimination of variability, and the pragmatic goal of the functional contextual behavioral tradition is not that so much as making a difference. The analyses that resulted had to lead to scientific ways of speaking that were precise (only certain principles can apply to given analytic tasks); they had to be broad in scope (principles needed to apply to a range of events, not only a specific instance); and they had to have scientific depth (to cohere across levels of analysis so as to lead toward a more unified fabric of science).

Thus, while there are a variety of scientific contextualisms, only functional contextualism sought "prediction and influence, with precision, scope and depth" (Hayes, 1993). Functional contextualism was said to be an approach to the study of whole organisms interacting in and with a context considered historically and situationally (i.e., it was a form of contextualism focused on the psychological level of analysis), and it was a *scientific* contextualism in the sense of being a social enterprise that had as its purpose the development of increasingly organized statements of relations among events and based on verifiable experience.

These changes were individually small elaborations, but the cumulative effect was to define functional contextual psychology as a distinct form of behaviorism that made the social/verbal nature of science more foundational, defined the psychological level of analysis, characterized its knowledge development as a form of pragmatism, and carefully specified its truth criterion.

The work on contextualism laid the foundation for CBS is a particular form of behavioral psychology, with a particular set of assumptions and purposes. Gradually, this philosophical work allowed intuitive extensions of Skinnerian thinking construed as a form of radical pragmatism to be replaced by deliberate extensions founded on a clearly stated set of assumptions.

It is possible to cast CBS as a relatively minor revision of Skinnerian thinking, but that would be correct only if you view Skinnerian thought as functional and contextualistic. That is hardly the universal foundation of behavior analyst, in part because Skinner himself was not clear. Skinnerian behaviorism contained two contradictory philosophical ideas, and these contradictions were neither noticed nor resolved.

Consider, for example, the *only* place Skinner ever clearly defined "behavior." In his treatment of the topic in the *The Behavior of Organisms*, Skinner (1938) defined behavior as "the movement of an organism or of its parts in a frame of reference provided by the organism itself or by various external objects or fields of force" (p. 6). This is a topographical and mechanistic definition. It is in no way a functional definition. Its only link to context was the frame of reference needed to define movement. It is impossible to use such a definition to directly address thoughts, or feelings, or urges. None of these are "movements."

A few lines later on the same page Skinner (1938) defined behavior as "the functioning of an organism which is engaged in acting upon or having commerce with the outside world" (p. 6). This is an entirely different approach. It is functional and explicitly contextual. It treats behavior and the context in which it occurs as an integrated phenomenon and behavior can be categorized with reference to its function or past impact. We can apply that definition with equal relevance to thoughts, or feelings, or urges, and indeed Skinner himself did so only 7 years later (Skinner, 1945) when he vigorously defended the behavioral nature of some private events.

To have two fundamentally different definitions of the target of one's science in the same paragraph is to invite incoherence. This is not the forgivable error of a young academic that was then corrected later: no well elaborated definition of behavior was ever presented by Skinner after his incoherent attempt to do so in 1938.

The same thing happened with ontological claims. On the one hand, Skinner (1953) criticized the idea that understanding stimuli required "metaphysical speculations on what is 'really there' in the outside world" (p. 138), showing in the use of scare quotes and asides that although he was a monist he was not an elemental realist: "What is lacking is the bold and exciting behavioristic hypothesis that what one observes and talks about is always the 'real' and 'physical' world (*or at least the 'one' world*)" (Skinner, 1945, p. 276, emphasis added). He did so on pragmatic grounds, which is exactly what a contextualist should do: "Responses to some forms of stimulation are more likely to be 'right' than responses to others, in the sense that they are more likely to lead to effective behavior . . .but any suggestion that they bring us closer to the 'real' world is out of place" (Skinner, 1953, p. 139). On the other hand, Skinner (1953) defined stimuli using a physicalistic and ontological definition as "energy changes at the periphery" (Skinner, 1953, p. 449). Like his treatment of behavior in 1938, this is a mess. If even terms like "real" and "physical" are to be put into scare quotes and anything one talks about is *always* in "the 'one' world" (meaning that his concern is monism, not elemental realism), then how can stimuli be glibly defined mechanistically as "energy changes at the periphery"? Skinner appreciated the a-ontological nature of evolutionary epistemology (e.g., his idea that there is a sense in which something is "true" simply because it enables effective action), but he took stands that directly contradicted these ideas.

The end result is that Skinnerian behaviorism and its advocates both overlap with CBS virtually completely at times, and are opposed to CBS in profound ways at times, depending only on the person and passage (see Hayes et al. (1988) for more examples). These inconsistencies initially drove the need for clarity about functional contextualism, but over the long run they were part of what drove CBS into its own association, with its own journal and conferences. Behavior analysis as an organized field and radical behaviorism as a philosophy of science ultimately could not adequately house CBS because it is a house divided and because the implications of RFT could only be addressed usefully if there was philosophical clarity.

Relational framing can create reinforcers, augment or diminish their impact, alter classically conditioned stimuli, or establish forms of stimulus control that did not fit any other previously identified forms. These new forms of behavior regulation (Hayes et al., 1987; Wulfert & Hayes, 1988) were both exciting and horrible news. It was exciting because a vast set of new research questions opened up, many of which led directly to questions of central importance to mainstream psychology. It was horrible because the hard won knowledge about direct contingencies that behavioral psychology had spent a good part of a century creating now had to be reworked with verbal humans. Decades of difficult experimental and conceptual work lay ahead. It was not possible to do that work inside the animal learning tradition, insofar as nonhuman animals have not yet been clearly shown to do elementary relational framing with sufficient robustness to be used as a preparation. It also could not be done inside incoherent philosophical positions.

Walking Through the Door Skinner Opened

Radical behaviorism rejected public observability per se as the defining characteristic of scientifically valid events (Skinner, 1945). Rather, observations are scientifically valid or invalid based on the contingencies controlling these observations. From a contextualistic perspective, the scientific value of an observation is ultimately determined by the degree to which it enables analytic goals to be accomplished. Public agreement provides no assurance of this:

> [Radical behaviorism] does not insist upon truth by agreement and can therefore consider events taking place in the private world within the skin. It does not call these events unobservable, and it does not dismiss them as subjective. (Skinner, 1974, p. 16)

Skinner thus opened the door to the analysis of thoughts and feelings, but nothing in his theorizing led him to view it as crucial, necessary, or even helpful to do so. He properly noted that "the initiating action is taken by the environment" (Skinner, 1974, p. 73; see also Skinner, 1984) and correctly explained that:

> It has been objected that we must stop somewhere in following a causal chain into the past and we may as well stop at a psychic level . . . It is true that we could trace human behavior not only to the physical conditions which [cause it] but also to the causes of those conditions and the causes of those causes, almost ad infinitum, but there is no point in going back beyond the point at which effective action can be taken. That point is not to be found in the psyche. (Skinner, 1974, p. 210)

All of this seems sound from a CBS point of view, but because Skinner's analysis of verbal behavior (1957) failed to appreciate that language and cognition could alter the functions of other behavioral processes, he did not feel that an analysis of thoughts and feelings would change the functional analysis of overt behavior.

The first article on ACT and RFT is arguably my article entitled "Making Sense of Spirituality" (Hayes, 1984). The first oral presentation of RFT was a year away, but this article it already contains the beginning ideas (in addition to beginning ACT ideas). In that article I argued that perhaps the most mentalistic terms we can imagine—"spirit" as opposed to "matter"—was behaviorally sensible once its basis in verbal relations was appreciated.

Now, more than three and a half decades later, it is easy to use relational frame language to describe the core of the argument: the deictic relational frames of I/You, Now/Then, and Here/There come together to form an "I/Here/Now" sense of perspective in the normal human child. Once formed we not only see and see that we see (to use Skinner's terms for self-awareness), but we see that we see from an "I/Here/Now" point of view. This perspective is based on a set of relational operants that forms the core of verbal consciousness or self-awareness, but the edges or limits of this kind of relational action cannot be consciously known, because it is central to what conscious knowing entails. Said in common sense terms, we cannot be conscious of unconsciousness. That lack of apparent edges or limits make consciousness (in this sense) seem unlimited or not thing-like—which is exactly what the term "spirit" is said to denote ("matter" is the "stuff of which things are made" and "spirit" is "not material"). Thus, "spirituality" is a sensible term for a distinct and important behavioral domain defined by a sensible functional analysis, even though it is not a technical behavioral term.

The 1984 article formed a kind of break with the Skinnerian tradition, in which literally mentalistic terms could be embraced as behaviorally useful terms if (and only if) they helped orient the scientific listener to coherent sets of functional analyses. In the years since, a considerable empirical literature has sprung up around the role of deictic relational operants in establishing a sense of self and in using that analysis to train children without a normal sense of self (see McHugh & Stewart, 2012 for a book length treatment), essentially validating that leap.

Said in another way, if "meaning is use," then we cannot look in the dictionary for approved behavioral terms, because such an approach violates a behavioral perspective on language. Yes, taken literally, the term "spirituality" contradicts the monistic assumptions of behavioral psychology, but when it is examined functionally, there appears to be good reasons for the term, based on relational learning and sense of self.

That article (Hayes, 1984) proved to be prescient in areas such as perspective-taking and deictic framing, as well as acceptance-based psychotherapy. Much of what later becomes CBS was foreshadowed there. Ironically, the core step was to take seriously a phenomenon that literally contracts behavioral assumptions. That is a step that even today most behaviorists are unwilling to make.

Recognizing the danger of embracing terms of that kind, they were given the deliberately humble name in CBS of "middle level terms." They are not technical terms, but they are viewed (with caution) as scientifically useful orientations to domains of importance in which behavioral principles apply. When a domain has been well mapped in terms of functional contextual goals (prediction and influence with precision, scope, and depth), middle level terms become shorthand guides to sets of technical analyses. If a middle level term becomes reified and disconnected to the current state of play of functional analysis, it can pose a scientific risk, and CBS scientists are constantly trying to supplant their use if possible when conducting technical studies. The expansion of CBS into applied domains, however, has been fostered by their shorthand utility in many areas.

The Reticulated Research Strategy of CBS

CBS differs from most forms of behaviorism in that its research strategy is not bottom up. Because the goal is explicitly to create a psychology more adequate to human complexity, basic and applied scientists and practitioners alike are held accountable for the adequacy of CBS principles and methods. Thus, for example, if clinical methods are inadequate and basic principles do not exist to create better ones, it is considered the shared responsibility of applied and basic workers to create the needed basic account. Similarly, if basic advancements are made, it is considered the shared responsibility of applied and basic workers to extend these developments into application.

The history of CBS as an area shows the result. Well-known clinicians do RFT research; well-known basic RFT scientists create applied programs. The truth criterion of functional contextualism gives applied and basic accounts alike a stake in the development of the field.

Theories and Models

CBS vigorously embraces the importance of theory. Theory is important for several reasons.

1. *Without theory we have no basis for using our knowledge when confronted with a new problem or situation.* Descriptions of methods or techniques, devoid of underlying theory, have little to say about novel situations, or unusual combinations of features. Theoretical understanding is needed to know how to mold our techniques and methods to specific situations.
2. *Without theory, we have no systematic means to develop new techniques and methods.* Accidents or common sense provides a poor guide to new methods. The adequacy of the precision and scope of scientific theory is tested by its ability to guide novel approaches.
3. *Without theory, areas of knowledge become disorganized and incoherent.* Without theory, scientific knowledge is a mountain of seemingly disconnected bits of information. The field becomes an incoherent mass, impossible to master and impossible to teach.

Most psychological theories are hypothetico-deductive. While theories of this kind produce a sense of scope, they tend to become increasingly focused on what is unobserved, hypothesized, or deduced, and not what is observed or manipulated. Spence, who had a major historical impact on the hypothetico-deductive model of theory, went so far as to claim that if a functional relation was "always the same … then we would have no need of theory" (1944, p. 71), adding that theoretical constructs are "guesses as to what variables *other than the ones under the control of the experimenter* are determining the response" (1944, p. 71, italics added). In his view real science and real theory was hypothetical/mediational, and its essence was focused on what you could not see and could not manipulate, and could only test by falsification. Almost by definition those kinds of theories do not orient toward manipulable variables, and thus are hobbled in terms of their practical import.

In contrast, CBS starts with *behavioral observations*—refined and precise descriptions of behavioral phenomena in well-characterized contexts. It examines the relationship between behavioral phenomena and contextual events in order to find *behavioral principles*: ways of speaking about and describing these behavioral observations precisely and yet with broad scope and depth across level of analysis that allow behavioral phenomena to be both predicted and influenced. It then applies coherent verbal networks of such principles to complex situations.

We have a *behavioral theory* when there are a) systematic and generally applicable analyses of important classes of behavioral observations b) stated in terms of coherently related sets of behavioral principles that c) allow behavioral phenomena within that class to be predicted-and-influenced as a unified goal with greater precision, scope, and depth. That is an *analytic-abstractive* theory—general functional analysis within a specific domain.

RFT is a theory in that analytic-abstractive sense. Psychological flexibility is a theory of psychopathology and its amelioration, in that same sense.

Nesting CBS Under the Umbrella of Evolution Science

The functional wing of behavioral psychology was always explicitly a form of evolutionary psychology:

> Selection by consequences is a causal mode found only in living things or in machines made by living things. It was first recognized in natural selection, but it also accounts for the shaping and maintenance of the behavior of the individual and evolution of cultures. In all three of these fields, it replaces explanations based on the causal modes of classical mechanics. The replacement is strongly resisted. Natural selection has now made its case, but similar delays in recognizing the role of selection in the other fields could deprive us of valuable help in solving the problems that confront us. (Skinner, 1981, p. 501)

CBS brings additional content to that table. CBS takes a broad focus on ontogenic development that can readily be embedded into multi-dimensional and multi-level evolution science. It is "multi-dimensional" in the view that variation and selective retention occurs in multiple strands of mutually interacting events (e.g., genetic, epigenetic, behavioral, and symbolic; Jablonka & Lamb, 2006); "multi-level" is the view that variation and selection occurs at different levels of organization, with competition and selection occurring both between and within groups (for a book length examination of the nesting of CBS under modern evolution science, see Wilson & Hayes, 2018).

By nesting CBS underneath multi-dimensional and multi-level evolution science, a natural relationship is created between contextual approaches in the life sciences. Evolutionary theory provides an unmatched degree of consilience between psychology and the rest of the life sciences that allows the "scope and depth" analytic goals of CBS to be furthered.

The Creation of CBS Models and Methods

In the world of behavior change models and methods, the practical impact of a CBS approach has perhaps been most felt in the areas of ACT and psychological flexibility. ACT is now a vast area of research encompassing nearly 400 randomized trials (see www.bit.ly/ACTRCTs for a list) and more than a thousand additional studies on open trials, single case designs, case studies, assessment, psychopathology, and processes of change. There are more than 55 meta-analyses of ACT (see http://www.bit.ly/ACTmetas for a list). There are more than 100 books on ACT in English and scores of original ACT books in other languages.

But the CBS approach is rapidly creating other forms of applied interventions and models. ACT has been combined with Lin Ostrom's Nobel Prize winning core design principles for small groups, to produce an intervention program for fostering cooperation in small groups called "Prosocial" (Atkins et al., 2019). There are powerful applied language training programs in behavior analysis based on RFT such as Mark Dixon's PEAK program (Dixon et al., 2017). Already we know that CBS thinking has added significantly to applied outcomes, even in areas in which applied behavior analysis is well established. For example, training in Skinner's verbal operants significantly increases the intellectual performance of children with developmental disabilities relative to normal programming with an effect size of $d = 0.51$, but by adding RFT training to the mix children improve much more, both relative to a traditional educational control ($d = 1.65$) and relative to traditional applied behavior analysis training focused on Skinnerian verbal operants ($d = 1.08$) (Dixon et al., 2019).

In part for such reasons, CBS is continuing to expand rapidly across the globe. Its fruits are being felt in a vast array of areas, from education to attitude change; from sports to business; from implicit cognition to social change. Even such entities as the World Health Organization now promote programs worldwide that are drawn from CBS research (e.g., Tol et al., 2020).

Summary

CBS is a distinct form of behaviorism, in terms of philosophy of science, strategy of knowledge development, concepts, models, theories, and methods. Over the last decade or two, it has been the fastest-growing wing of behaviorism across a wide range of topics and areas, whether defined by popularity, the volume of research output, research impact as measured by citations, breadth of basic findings, breadth of applied findings, or global reach. In many areas of work (e.g., psychotherapy) CBS related approaches are now the most visible face of behaviorism worldwide. For those who view Skinner's work contextualistically, CBS is an evolution and extension of the Skinnerian tradition. Others view this matter quite differently. Regardless of how it is viewed, contextual behavioral science clearly is here and here to stay as a distinct wing of behavioral science.

References

Atkins, P., Wilson, D. S., & Hayes, S. C. (2019). *Prosocial: Using evolutionary science to build productive, equitable, and collaborative groups*. Context Press/New Harbinger Publications.

Adamson, L. B. (1996). *Communication development during infancy*. Brown & Benchmark.

Baer, D. M., Peterson, R. F., & Sherman, J. A. (1967). The development of imitation by reinforcing behavioral similarity to a model. *Journal of the Experimental Analysis of Behavior, 10*, 405–416.

Devany, J. M., Hayes, S. C., & Nelson, R. O. (1986). Equivalence class formation in language-able and language-disabled children. *Journal of the Experimental Analysis of Behavior, 46*, 243–257. https://doi.org/10.1901/jeab.1986.46-243

Dixon, L. S. (1977). The nature of control by spoken words over visual stimulus selection. *Journal of the Experimental Analysis of Behavior, 27*, 433–442.

Dixon, M. R., Belisle, J., McKeel, A., et al. (2017). An internal and critical review of the peak relational training system for children with autism and related intellectual disabilities: 2014–2017. *The Behavior Analyst, 40*, 493–521. https://doi.org/10.1007/s40614-017-0119-4

Dixon, M. R., Paliliunas, D., Barron, B. F., Schmick, A. M., & Stanley, C. R. (2019). Randomized controlled trial evaluation of aba content on IQ gains in children with autism. *Journal of Behavioral Education, 2019*. https://doi.org/10.1007/s10864-019-09344-7

Dougher, M. J., Hamilton, D., Fink, B., & Harrington, J. (2007). Transformation of the discriminative and eliciting functions of generalized relational stimuli. *Journal of the Experimental Analysis of Behavior, 88*(2), 179–197.

Dymond, S., & Roche, R. (2013). *Advances in relational frame theory: Research and application*. Context Press/New Harbinger Publications.

Gewirtz, J. L., & Stengle, K. G. (1968). Learning of generalized imitation as the basis for identification. *Psychological Review, 5*, 374–397.

Ginsburg, S., & Jablonka, E. (2010). The evolution of associative learning: A factor in the Cambrian explosion. *Journal of Theoretical Biology, 266*, 11–20.

Hayes, S. C. (1984). Making sense of spirituality. *Behavior, 12*, 99–110.

Hayes, S. C. (1987). Upward and downward continuity: It's time to change our strategic assumptions. *Behavior Analysis, 22*, 3–6.

Hayes, S. C. (1993). Analytic goals and the varieties of scientific contextualism. In S. C. Hayes, L. J. Hayes, H. W. Reese, & T. R. Sarbin (Eds.), *Varieties of scientific contextualism* (pp. 11–27). Context Press.

Hayes, S. C., Barnes-Holmes, D., & Roche, B. (2001). *Relational Frame Theory: A Post-Skinnerian account of human language and cognition*. Plenum Press.

Hayes, S. C., Barnes-Holmes, D., & Wilson, K. G. (2012). Contextual behavioral science: Creating a science more adequate to the challenge of the human condition. *Journal of Contextual Behavioral Science, 1*, 1–16. https://doi.org/10.1016/j.jcbs.2012.09.004

Hayes, S. C. & Brownstein, A. J. (1985, May). Verbal behavior, equivalence classes, and rules: New definitions, data, and directions. Invited address presented at the meeting of the Association for Behavior Analysis, Columbus, OH.

Hayes, S. C., & Brownstein, A. J. (1986). Mentalism, behavior-behavior relations and a behavior analytic view of the purposes of science. *The Behavior Analyst, 9*, 175–190.

Hayes, S. C., Brownstein, A. J., Haas, J. R., & Greenway, D. E. (1986a). Instructions, multiple schedules, and extinction: Distinguishing rule-governed from schedule controlled behavior. *Journal of the Experimental Analysis of Behavior, 46*, 137–147. https://doi.org/10.1901/jeab.1986.46-137

Hayes, S. C., Brownstein, A. J., Zettle, R. D., Rosenfarb, I., & Korn, Z. (1986b). Rule-governed behavior and sensitivity to changing consequences of responding. *Journal of the Experimental Analysis of Behavior, 45*, 237–256. https://doi.org/10.1901/jeab.1986.45-237

Hayes, S. C., Devany, J. M., Kohlenberg, B. S., Brownstein, A. J., & Shelby, J. (1987). Stimulus equivalence and the symbolic control of behavior. *Mexican Journal of Behavior Analysis, 13*, 361–374.

Hayes, S. C., Hayes, L. J., & Reese, H. W. (1988). Finding the philosophical core: A review of Stephen C. Pepper's World Hypotheses. *Journal of the Experimental Analysis of Behavior, 50*, 97–111. https://doi.org/10.1901/jeab.1988.50-97

Hayes, S. C. & Hayes, L. J. (1992). Verbal relations and the evolution of behavior analysis. *American Psychologist, 47*, 1383–1395. https://doi.org/10.1037/0003-066X.47.11.1383

Hayes, S. C., Hayes, L. J., Reese, H. W., & Sarbin, T. R. (Eds.). (1993). *Varieties of scientific contextualism*. Context Press/New Harbinger.

Hayes, S. C., & Sanford, B. (2014). Cooperation came first: Evolution and human cognition. *Journal of the Experimental Analysis of Behavior, 101*, 112–129. https://doi.org/10.1002/jeab.64

Jablonka, E., & Lamb, M. J. (2006). *Evolution in four dimensions: Genetic, epigenetic, behavioral, and symbolic variation in the history of life*. MIT Press.

Leander, J. D., Lippman, L. G., & Meyer, M. M. (1968). Fixed interval performance as related to subjects' verbalizations of the reinforcement contingency. *The Psychological Record, 18*, 469–474.

Lionello-DeNolf, K. M. (2009). The search for symmetry: 25 years in review. *Learning & Behavior, 37*, 188–203.

Lipkens, G., Hayes, S. C., & Hayes, L. J. (1993). Longitudinal study of derived stimulus relations in an infant. *Journal of Experimental Child Psychology, 56*, 201–239. https://doi.org/10.1006/jecp.1993.1032

Lowe, C. F., Harzem, P., & Hughes, S. (1978). Determinants of operant behavior in humans: Some differences from animals. *Quarterly Journal of Experimental Psychology, 30*, 373–386.

Luciano, C., Becerra, I. G., & Valverde, M. R. (2007). The role of multiple-exemplar training and naming in establishing derived equivalence in an infant. *Journal of the Experimental Analysis of Behavior, 87*, 349–365.

McHugh, L., & Stewart, I. (2012). *The self and perspective taking: Contributions and applicsations from moidern behavioral science*. Context Press/New Harbinger Publications.

Pelaez, M., Gewirtz, J. L., Sanchez, A., & Mahabir, N. A. (2000). Exploring stimulus equivalence formation in infants. *Behavior Development Bulletin, 9*, 20–25.

Penn, D. C., Holyoak, K. J., & Povinelli, D. J. (2008). Darwin's mistake: Explaining the discontinuity between human and nonhuman minds. *Behavioral and Brain Sciences, 31*, 109–178.

Pepper, S. C. (1942). *World hypotheses: A study in evidence*. University of California Press.

Sidman, M. (1971). Reading and auditory-visual equivalences. *Journal of Speech, Language, and Hearing Research, 14*, 5–13.

Sidman, M., & Tailby, W. (1982). Conditional discrimination vs. matching to sample: An expansion of the testing paradigm. *Journal of the Experimental Analysis of Behavior, 37*, 5–22. https://doi.org/10.1901/jeab.1982.37-5

Skinner, B. F. (1938). *Behavior of organisms*. Appelton-Century-Crofts.

Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review, 52*, 270–276.

Skinner, B. F. (1953). *Science and human behavior*. Free Press.

Skinner, B. F. (1957). *Verbal behavior*. Prentice Hall.

Skinner, B. F. (1974). *About behaviorism*. Knopf.

Skinner, B. F. (1981). Selection by consequences. *Science, 213*, 501–504.

Skinner, B. F. (1984). Reply to Catania. *The Behavioral and Brain Sciences, 7*, 718–719.

Spence, K. W. (1944). The nature of theory construction in contemporary psychology. *Psychological Review, 51*, 48–68.

Tol, W. A., Leku, M. R., … van Ommeren, M. (2020). Guided self-help to reduce psychological distress in South Sudanese female refugees in Uganda: A cluster randomised trial. *The Lancet Global Health, 8*(2), e254–e263. https://doi.org/10.1016/S2214-109X(19)30504-2

Tomasello, M., Carpenter, M., Call, J., Behne, T., & Moll, H. (2005). Understanding and sharing intentions: The origins of cultural cognition. *Behavioral and Brain Sciences, 28*(5), 675–691.

Wilson, D. S., & Hayes, S. C. (Eds.). (2018). *Evolution and contextual behavioral science: An integrated framework for understanding, predicting, and influencing human behavior*. Context Press/New Harbinger Publications.

Woodward, A. (2005). Infants' understanding of the actions involved in joint attention. In N. Eilan, C. Hoerl, T. McCormack, & J. Roessler (Eds.), *Joint attention: Communication and other minds* (pp. 110–128). Oxford University Press.

Wulfert, E., & Hayes, S. C. (1988). Transfer of a conditional ordering response through conditional equivalence classes. *Journal of the Experimental Analysis of Behavior, 50*, 125–144. https://doi.org/10.1901/jeab.1988.50-125

Zettle, R. D., Hayes, S. C., Barnes-Holmes, D., & Biglan, T. (Eds.). (2016). *The Wiley handbook of Contextual Behavioral Science*. Wiley/Blackwell.

Chapter 17
Will the Wing Fly Away from the Body? A Commentary on Steven Hayes' Chapter, Contextual Behavioral Science

Julio C. de Rose

The invitation to write a commentary on a chapter by Steven Hayes is an honor. It is also a high responsibility and a considerable challenge. The contributions of Steven Hayes to behavioral science are highly significant. Hayes, modestly, somewhat underestimates his role in the development of Relational Frame Theory (RFT), Acceptance and Commitment Therapy (ACT), and Contextual Behavior Science (CBS) in general, picturing himself just as someone that "may have instigated CBS." He is certainly right in saying that "hundreds of major researchers and practitioners around the world … are actively shaping CBS and all of its major features and facets." I doubt, however, if anyone would question his leadership and the extent of his contribution to this endeavor. After more than three and a half decades of work by Hayes and other major researchers, RFT, ACT, and CBS in general acquired solid recognition within and outside the circle of behavioral scientists.

As Hayes observes, CBS is a vigorous field. It has its own scientific association, which may be the largest behavioral association; the Journal of Contextual Behavioral Science is probably the most subscribed among behavioral journals and has a competitive impact factor compared to other behavioral journals. Even more important are the empirical, theoretical, and applied contributions of CBS.

In most of this commentary I will focus specifically on RFT, because I am more familiar with the huge contribution of RFT to the behavioral understanding of language and cognition. The applied and philosophical aspects of CBS will be touched only briefly. For those that consider themselves contextual behavioral scientists, RFT fulfilled its promise of providing a modern post-Skinnerian approach of language and cognition, with an increasing influence on the development of educational and clinical applications. However, RFT still figures prominently in the traditional field of behavior analysis. RFT is highly represented in the more traditional behavioral journals, such as *JEAB*, *JABA*, *The Psychological Record*,

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Perspectives on Behavior Science, and *The Analysis of Verbal Behavior*. RFT researchers not only publish significant part of their work in these journals but also figure in their editorial boards and among their associate editors and editors in chief. RFT researchers are also prominent in the international associations of behavior analysis, such as EABA and ABAI. The latter has been presided recently by researchers that gave significant contribution to RFT.

Thus, it is not clear the extent to which the establishment of CBS as a distinct science severed the ties of RFT (and CBS in general) with "traditional" behavioral science. This probably depends on context, so that "traditional" behavior analysis may be sometimes in a frame of difference and even sometimes opposition to RFT, whereas in other contexts RFT may be in a frame of coordination with behavior analysis or sometimes even in a frame of hierarchy, included as part of behavior analysis. Hayes, in his chapter, remarks that CBS at times overlaps with traditional behavior analysis and at times is opposed in profound ways. I will have more to say about this along this commentary.

Even among those like myself, who are content to call themselves behavioral scientists, with no label, the standing of RFT is very high. I, for instance, may not adhere to all theoretical claims of RFT, but this approach has had a growing influence over my recent research. And I am sure the same happened with many other behavioral scientists.

The separation of CBS from behavior analysis may have some common features with the separation of behavior analysis from psychology, although I think there may be wide differences between the contexts that led to these two attempts of secession. Thus, the title of the treatise by Keller and Schoenfeld (1950), perhaps the first systematic presentation of behavior analysis, was *Principles of Psychology*. At that time, behavior analysis was viewed as a new approach to psychological science and not as a different science. It seems that behavior analysts founded their own journals and associations not because they did not want to be part of psychology, but because psychology did not want them. This is hardly the same with RFT (and probably with the other branches of CBS as well). In this case, it seems to be that CBS doesn't want to be a part of behavior analysis. I may imagine that RFT researchers may have received harsh criticism and may have had difficulties in publishing some articles in mainstream journals of behavior analysis. New theories are often received with resistance. But from the steady rhythm of publication of RFT articles in *JEAB* and other journals, one may infer that there was no systematic bias against RFT articles. On the contrary, RFT continues to grow in acceptance and influence.

But what does it mean to be or not to be part of a science? Behavior analysis differs from mainstream psychology. Does this mean that it is a different science and not part of psychological science? In this respect, I may quote the preface of Marc Richelle's very sympathetic *reappraisal* of B. F. Skinner: "The disciples of Skinner, or some of them, have had a quite distinct history in American Psychology. Among other things, they have isolated themselves from the rest of scientific psychology by creating their own journals and societies, by closing themselves to open dialogue with other trends and developing a sense of orthodoxy, which has never proved to be fruitful in the progress of a science or in the dissemination of a theory" (Richelle, 1993, p. x). Even though I consider myself a behavior analyst, I continue to view myself as a psychological scientist. I am in a department of psychology and in a graduate program of psychology. I also teach an undergraduate course of general psychology, and I do teach general psychology, not just behavior analysis. I feel that teaching and studying general psychology contributes to my work as behavior analyst, and several ideas and findings from mainstream psychology have found their way into my research. All psychologists that do empirical research today study behavior, as the leading cognitive psychologist Henry Roediger (2004) pointed out. Because I am curious about behavior, especially human behavior, and I see much human behavior around me that is so hard to explain, I don't want to isolate myself from any field of research that may increase my understanding of human behavior. I am sure Hayes does not want to isolate himself either, and his research and writings prove this. The fact that for me behavior analysis is still part of psychology whereas Hayes feels the need to separate his work not only from psychology, but from behavior analysis as well, may be explained by contextual differences. Perhaps part of the difference in context is specific to Brazil. In this country, differently than in the USA, psychology is dominated by approaches that are not based on empirical research, and most academics and practitioners are even hostile to empirical research, favoring approaches such as psychoanalysis, Marxist psychology, and so forth. In this context, there is a minority of psychologists who do rigorous empirical research, such as behavior analysts, cognitive psychologists, developmental psychologists, ethologists, etc. Regardless of wide theoretical and philosophical differences, all these groups share the view of psychology as a science based on empirical research, and, as Roediger pointed out, they do study behavior even when they theorize about the *mind*.

Therefore, if I prefer not to join most behavior analysts in their separation from psychology, I have even less reason to consider CBS as separated from behavior analysis. It is true that CBS has philosophical and theoretical differences from the rest of behavior analysis. It is probably true that these differences may have sometimes resulted in an uneasy relationship with some behavior analysts with too much "sense of orthodoxy." But they are minor differences compared to the differences between behavior analysis and the rest of psychology. I write "the rest of psychology" maintaining my view that CBS is part of behavior analysis, which is part of psychology. But this depends on the context, as I will discuss at the end of this commentary.

At this point I would like to examine some of the arguments given by Hayes to consider CBS as separate from the rest of behavioral science. Hayes admits that he and other CBS scientists have walked through the door opened by Skinner. Although a minority of behavior analysts tends to regard Skinner's writings as sacred scriptures, this is a poor judgment of the accomplishments of a scientist of Skinner's stature. Skinner opened doors, as Pavlov had opened before, and as Sidman and Hayes did more recently. These opened doors allowed scientists to explore new territory and increase our knowledge about behavior. Whoever opens a door may go groping through the new terrain and may not be able to see as clearly as others who already found the door opened and the terrain already explored to some extent. When Skinner opened a door to a functional conception of behavior, it is not surprising that he could not completely free himself from the prevalent conceptions of his formative time. Could we expect that he knew outright what behavior *is*, in order to give a proper functional definition? It may be understandable that his definition is ambiguous, partly topographical and partly functional. Hayes is not willing to "forgive" this contradictory definition even admitting that Skinner's subsequent work was much more influenced by the functional aspects of his conception, to the extent that in just a few years he defended the behavioral nature of private events and opened the way for a behavioral account of feeling and thinking.

Other important reasons that lead Hayes to want to part from the rest of behavioral science stem from empirical and theoretical advances of RFT. Advances usually require an update of old formulations, and sometimes they present a big challenge to established conceptions. Hayes argues that some discoveries of RFT may be "horrible" for behavior scientists that don't label themselves as contextual. I don't think this is to be taken literally: it is a kind of metaphorical language used by Hayes to highlight the impact of some discoveries and how they challenged established conceptions. To the same purpose, I use to speculate with my students about the reactions of Pavlov's ghost to new discoveries about conditioning: for instance, how Pavlov's ghost would have reacted when John Garcia and his associates (Garcia et al., 1966) discovered the phenomenon of taste aversion and showed that conditioning could occur across very long delays, but only with certain combinations of CS and US. Did the ghost find this horrible, because it required a change in our way of thinking about conditioning? Our knowledge about conditioning advanced significantly since Pavlov left this world, and he would probably be delighted to know how far subsequent scientists advanced through the door he opened. The discoveries of Dougher et al. (1994) and Dougher et al. (2007) are nice examples. When I tell students about these experiments, I always ask them whether Pavlov's ghost would be horrified or excited with these discoveries. If I believed in ghosts, I would bet that Pavlov's ghost, rather than finding Dougher's results horrible, would be enthusiastic about the enormous increase in the power of conditioning. We no longer need to restrict conditioning effects to directly paired stimuli! The effects of conditioning may propagate across complex relational networks. As Dougher and associates pointed out, we can now attribute a range of problem behaviors to conditioning, even when direct pairing is not involved. It is even possible that Pavlov's ghost is delighted to find that the results of Dougher and colleagues, and of RFT in general, even increased the standing of classical conditioning in current behavioral science.

And I might as well speculate about the reactions of Skinner's ghost to the discoveries of RFT. How would the ghost react to data showing that relational framing can create reinforcers, alter their impact, and spread conditioning effects through relational networks? Again, keeping in mind that the ghost is just a rhetorical device, I think his reaction would be quite predictable: he would say that science requires a disposition to accept facts even when they run against desires. When he was alive, Skinner changed his conceptions a few times, his thinking evolved as new data came about, and therefore we may speculate that now, as a ghost, he would have no problem in accepting the findings of RFT, although perhaps he could discuss the explanations.

Hayes is correct in arguing that "traditional behavior analysis had a difficult time addressing human language." I doubt that CBS is having an "easy time" addressing one of the most challenging issues in all sciences. RFT is undoubtedly making significant advances in a behavioral understanding of language. But would these advances be possible if Pavlov, Skinner, Sidman, and others had not opened doors and explored the terrain? Skinner claims to have begun the work on the book *Verbal Behavior* in 1934 (Skinner, 1957, pp. 456–457). Therefore, the more than 20 years of work in this single book serve as a measure of the "difficult time" Skinner had along the way to a functional analysis of verbal behavior. Considering that this was a very original formulation, going far beyond the behaviorist theories of the time, and considering that Skinner was just human, and not a god or a prophet, it would be virtually impossible that his Herculean work was perfect. What may surprise us, though, is how effective Skinner's account has been, despite its inevitable weaknesses.

Skinner himself (1957, p. 3) stated that "understanding" verbal behavior requires more than the confirmation of any set of theoretical principles. "The criteria are more demanding than that. The extent to which we understand verbal behavior in a 'causal' analysis is to be assessed by the extent to which we can predict the occurrence of specific instances and, eventually, from the extent to which we can produce or control such behavior by altering the conditions under which it occurs." Skinner then mentions specific *engineering* tasks that could concretely evaluate attainment of this goal. Skinner's formulation clearly contributed some of these goals, particularly the first, which is the establishment of specific verbal repertoires as end-products of education. Teaching machines and other forms of programmed instruction have been successful at all educational levels (see Holland et al., 1976; Skinner, 1968), from teaching arithmetic behavior to chimpanzees (Ferster, 1964) to teaching a second language to college students (Rocha e Silva & Ferster, 1966) and neuroanatomy to medical students (Sidman & Sidman, 1965). Not much progress occurred regarding the other goals mentioned by Skinner: the contribution of Skinner's Verbal Summator to the Freudian task of "uncovering latent verbal behavior in a therapeutic interview" doesn't seem remarkable. Skinner himself seems to have had considerable success in, as a writer, evoking "his own verbal behavior in the act of composition," but self-treatments of writer's block continue to be often unsuccessful, as demonstrated in some well-known *JABA* publications. It is curious, however, that in exemplifying pragmatic goals to his account of language, Skinner did not foresee its extraordinary success in establishing and developing language in persons with intellectual deficiency and developmental disabilities. The achievements in this area may be celebrated as one of the greatest triumphs of behavioral science, or perhaps of any science. Of course, this is the result of work of many scientists, including researchers influenced by RFT, but these achievements would hardly happen if Skinner had not opened the door to an effective account of language.

It is true that Skinner did not clarify what is meant by saying that a rule specifies a contingency. And yet he opened a door here too. In the threshold of the third decade of the twenty-first century, thanks to the contributions of about four decades of research on equivalence and RFT, we are in a much better position to understand how a rule controls behavior. Thus, Harte et al. (2020, p. 372) state that a rule "involves a relational network composed largely of equivalence relations among the words in the rule and the events to which they refer, and the sequencing of the words in accordance with temporal relations... rules or instructions involve relational networks *and* transformation of functions that provides the rule with its behavior-controlling properties."

Thus, it seems that at least two new concepts are required for the specification of how a rule controls behavior: equivalence (or coordination) and transformation of functions. This increases our understanding of the behavior of the listener, i.e., how the listener *understands* a rule, and transformation of functions may also be involved in the motivation of the listener to follow a rule, i.e., why following the rule may produce reinforcing consequences. Hayes and other exponents of RFT seem to assume that this understanding requires a change in the definition of verbal behavior. Perhaps this may be the case. Nevertheless, Skinner himself changed the definition of verbal behavior along his book. In p. 2 he defined verbal behavior as "behavior reinforced through the mediation of other persons," although he admitted that this definition needed refinements. Then, in p. 225, Skinner adds that any other person (i.e., a listener) that mediates reinforcement for verbal behavior "must be responding in ways that have been conditioned *precisely in order to reinforce the behavior of the speaker*" (italics in the original). To me this is a profound change, and introduces an element that was totally absent in the initial definition. We may consider that here Skinner reintroduces *understanding* (by the listener!) in the definition of verbal behavior, since being "conditioned precisely in order to reinforce the behavior of the speaker" may be regarded as a behavioral specification of what is meant by *understanding*. As it usually happens in science, post-Skinnerian research[1] has shown that the behavioral processes involved in "understanding" are considerably more complex than Skinner envisioned. Research on stimulus equivalence and the contribution of RFT increased (and is continuing to increase) our understanding of verbal behavior or, if you will, our understanding of language and cognition. But does this mean that behavior science advanced, or that now we have a different science and the practitioners of the new science should gather in their own conventions and publish in their own journals, with relations to "traditional" behavior analysts becoming similar to those between behavior analysts and psychologists?

I would like to close this commentary proposing that classifying something as a new science, or as an advancement of an existing science, must be regarded as an *act in context*. What is achieved in a particular context by claiming that something

[1] By post-Skinnerian in this context I mean just a temporal relation: research conducted after Skinner's.

is a distinct science, or by considering it as an advancement of the existing science, i.e., what are the consequences of such behavior?

Some consequences are visible: a new association, with more than 9000 members and 27 chapters around the world; the most widely subscribed behavioral journal in the world, with a considerable impact factor; and a very well-attended convention. My personal opinion is that some "refinements" of radical behaviorism introduced by CBS have been especially effective in reducing the resistance against a behavioral approach. An example may be the slight reformulation of the objectives of a behavioral science as prediction and "influence," rather than prediction and "control." It is my belief that this slight change helped CBS to reach a wider audience, due to the presumably aversive connotations of the word "control," and the resultant misunderstanding of the objectives of a science that intends to "control" behavior.

These may be taken as very positive consequences of the constitution of CBS as a vigorous approach. In immediate terms, it seems that both CBS and "traditional" behavioral analysis gain with this. The mainstream approach continues to benefit from the work of CBS researchers, who continue to contribute significantly to the mainstream behavioral journals as authors, reviewers, and editors. They also continue to attend the mainstream conventions, increasing the theoretical and empirical diversity of the research that is presented and debated in these meetings; and they continue to participate in the directive boards of the mainstream associations. On the other hand, it is important for researchers that share some specific interests to have specific instances of debate between themselves. If "contextual behavioral science as a distinct wing of behavioral sciences" means just this, it will probably be beneficial for the distinct wing and for the whole body. It will contribute to the goal of all behavioral scientists, which is the advancement of knowledge about behavior, in order to predict and (as I also prefer) "influence" behavior.

If, however, the "distinct wing" wants to fly away from the body and close into itself, this will hardly benefit any science of behavior, whether it labels itself as contextual or not. If behavioral scientists who regard themselves as contextual close themselves to the dialogue with "traditional" behavioral science, they will follow, as Richelle (1993) pointed out, a way that has never proved fruitful. This may be particularly dangerous for young researchers, who may be more prone to develop a "sense of orthodoxy." As behavior analysts have much to learn from mainstream psychology (and to teach also), I believe that contextual behavioral scientists will continue to have much to learn from mainstream behavior science (and much to teach as well). If young behavioral scientists who label themselves as contextual start to believe that CBS brought the light to a field in which there was only darkness before, this will be detrimental to mainstream behavioral science and perhaps even more to CBS. This certainly is not the intent of Hayes and other prominent CBS researchers, but there may be a danger if the "distinct wing" progressively flies away.

References

Dougher, M. J., Augustson, E., Markham, M. R., Greenway, D. E., & Wulfert, E. (1994). he transfer of respondent eliciting and extinction functions through stimulus equivalence classes. *Journal of the Experimental Analysis of Behavior, 62*, 331–351. https://doi.org/10.1901/jeab.1994.62-331

Dougher, M. J., Hamilton, D. A., Fink, B. C., & Harrington, J. (2007). Transformation of the discriminative and eliciting functions of generalized relational stimuli. *Journal of the Experimental Analysis of Behavior, 88*, 179–197. https://doi.org/10.1901/jeab.2007.45-05

Ferster, C. B. (1964). Arithmetic behavior in chimpanzees. *Scientific American, 210*(5), 98–107.

Garcia, J., Ervin, R., & Koelling, R. (1966). Learning with prolonged delay of reinforcement. *Psychonomic Science, 5*, 121–122.

Harte, C., Barnes-Holmes, D., Barnes-Holmes, Y., & Kissi, A. (2020). The study of rule-governed behavior and derived stimulus relatios: Bridging the gap. *Perspectives on Behavior Science.* https://doi.org/10.1007/s40614-020-00256-w

Holland, J. G., Solomon, C., Doran, J., & Frezza, D. A. (1976). *The analysis of behavior in planning instruction.* Reading, MA: Addison-Wesley.

Keller, F. S., & Schoenfeld, W. N. (1950). *Principles of psychology: A systematic text in the science of behavior*. Appleton-Century-Crofts.

Richelle, M. N. (1993). *B. F. Skinner. A reappraisal*. Psychology Press.

Rocha e Silva, M. I., & Ferster, C. B. (1966). An experiment in teaching a second language. *International Review of Applied Linguistics in Language Teaching, 4*, 85–114. https://doi.org/10.1515/iral.1966.4.1-4.85

Roediger, H. L. (2004). What happened to behaviorism. *American Psychological Society Observer, 17*(3).

Sidman, R. L., & Sidman, M. (1965). *Neuroanatomy: A programmed text* (Vol. 1). Lippincott Williams & Wilkins.

Skinner, B. F. (1957). *Verbal behavior*. Prentice Hall.

Skinner, B. F. (1968). *The technology of teaching*. Copley.

Chapter 18
The Bottom Line Is Progress: All the Rest Is Commentary

Steven C. Hayes

The Wonderous Plant

Once upon a time, long ago, a large and wonderous plant was thought to be so precious that a well-meaning group of admirers built a roof over it with a circle of seats underneath so that people could sit comfortably and appreciate its elegance. Unappreciated by them, the roof reduced the sunlight and deflected the needed rain, but for a while the plant still grew, living off splashed in rain and reflected light. Ever so slowly, each year the plant weakened. Few seemed to realize what was happening.

Loving the plant, but fearing for its life, a junior gardener talked loudly about the danger, but mostly he was ignored. One night he slipped in unnoticed, took a cutting, and planted it outside, watering it and caring for it, until it began to grow. Soon, the sun and rain provided everything it needed to prosper. It took decades, but eventually it was larger than the original plant, and notably more robust.

When the gardener returned occasionally to speak to those gathered around the mother plant about what he had happened in the years since, he was startled by the response. "Why have you separated from the mother plant?!" demanded some. "You've *ruined* the plant! Ruined it! Yes, your plant is growing but who cares about such superficial things? Your plant does not even have a roof! There are no seats!"

"It grows because there is no roof" answered the surprised gardener, "but it's a plant in its mother's lineage. It's a cutting! You or your ancestors added the roof and seats long ago—those are not the original properties of the plant. I've just figured out how to help it grow."

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"That growth properly belongs to the mother!!" they growled back. "You are a separatist and we think you pursue this because of your personal and perhaps selfish desires. *You* want to separate. And not just from the mother. You even want to separate from the earth, and sun, and water!"

The gardener sat in dumbfounded silence. He had never heard such an odd idea before. "The exact opposite is true" said the gardener very quietly. "Reaching the earth, sun, and water was my whole purpose. A plant like this can't tolerate a roof. It cuts off the sun and water it needs. Why is the health of the two plants not proof of what I am saying?"

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I welcome the opportunity to respond to Dr. de Rose's commentary. I have known Dr. de Rose (it will be hard not to call him Julio!) for over 35 years. He has made consistent contributions to basic behavioral science, and it was kind of him to take the time to share his reflections on my chapter.

Let me state forthrightly that I am puzzled—even a bit startled—by the theme of his commentary: the supposed willful and perhaps personally motivated separation of contextual behavioral science (CBS) from the behavior analytic tradition writ large, or even *psychology* itself. In this telling of the story, to quote Dr. de Rose, "Hayes feels the need to separate his work not only from psychology, but from behavior analysis as well."

I am fairly used to being scolded for supposedly separating CBS from behavior analysis, but this is the very first time in my 45 years as a professional that I have ever encountered the idea that I'm trying to separate my life's work from psychology. I'm dumbfounded by that construction because it seems so out of touch with the CBS tradition and its books, conferences, studies, labs, topics, and impact. It is also out of touch with my own history of launching or being the president of multiple mainstream psychology associations.

It does little good, I suppose, just to whisper "the exact opposite is true" but ACT is impacting evidence-based psychotherapy and RFT is impacting traditional cognitive research in ways that do not seem characteristics of mainstream behavior analysis today.

In my target chapter I walked out why I think CBS is a distinct and progressive type of behavioral thinking. That was the task given to me by the editors of this book, which is explicitly focused on "types" of behaviorism. I state quite clearly in the target article, however, that "For those who view Skinner's work contextualistically, CBS is an evolution and extension of the Skinnerian tradition." Since I have long been an advocate for viewing Skinner contextualistically (e.g., Hayes et al., 1988), in my view CBS is an evolution and extension of behavior analysis understood through that lens.

The problem is that many in behavior analysis do *not* view Skinner that way. That is both obvious and demonstrable, and it has changed behavior analysis itself in profound and apparently permanent ways. For that reason, CBS is cannot be seen as an extension of mainstream behavior analysis as it is today because behavior analysis itself has changed.

I tried to explain why that change happened in the target article. I think it is because Skinner's writings are inconsistent in certain key areas, and thus serious readers can have radically different understandings.

It is not a matter, as Dr. de Rose claims, of me being unwilling to "forgive" Skinner for his inconsistencies. I'm perfectly willing to do so, but thousands of practitioners, scholars, and researchers will continue to argue *they* are the true behavior analysts when they take positions that I believe are outright anti-behavior analytic positions. My forgiveness or lack of it will not matter a whit to these thousands.

I'm sorry that behavior analysis is in that situation, but it is. Fred Skinner will never be able to repair it.

I and many others tried for *decades* to fight for a contextualistic approach inside mainstream behavior analysis. But every year the numbers moved a bit farther away from where it needed to go, a process that has accelerated now that master's level Board Certified Behavior Analysts are the core of applied behavior analysis. Key texts began to define Skinnerian behavior analysis in ways that look and act like S-R learning theory (Cooper et al., 2007). Skinner's analysis of private events was marginalized and virtually rejected. Behavior was said to be movement in a space-time frame. Needed growth in the analysis of language did not happen in the way or at the speed needed. Clinical behavior analysis shrank almost to the point of disappearance. Work on developmental disabilities overwhelmed the field, and master's level professionalization focused on that population became ascendant worldwide. Basic behavior analysts could no longer get jobs in doctoral departments graduating basic behavior analysts.

The functional and contextualistic scientific tradition that Skinner (in my mind at least) had championed grew weaker and narrower.

Life is not infinite. We do not live forever. In order to move on, the functional contextual wing had to create a space needed to see if it could grow and prosper as what it is.

It could and did. In the target article I provided evidence that CBS is thriving and is taking behavioral thinking into many corners of the intellectual and practical world, inside behavioral psychology and within psychology and the behavioral science more generally.

Writing of that kind is difficult—it can easily seem prideful and self-congratulatory—but it is necessary in evaluating progress. That is especially true for behavioral thinking, which is a form of pragmatism. I personally can think of no better way to evaluate a line of thought than by the results it produces. In order to focus on results, results must be enumerated. And Dr. de Rose seems to agree that CBS has been unusually successful since he relists many of these points. But he never takes seriously the idea that the success of CBS is due to the steps taken that most distinguish CBS from mainstream behavior analysis. That, in my view, is an error on his part.

These steps include being clear about the type of contextualism functional behavioral thinking represents; embracing the need for a priori goals for successful working to work scientifically; emphasizing the need for precision, scope, and depth as qualifiers of the quality of principles and theories; creating a more reticulated model of scientific development instead of Skinner's bottom up model; creating a robust behavioral account of language that better addresses relational learning; carrying that account into applied areas of complex human behavior; and learning to connect in a deeper and more empirical way with evolutionary sciences.

I agree that the positive outcomes of CBS could have befallen to mainstream behavior analysis in an alternative universe, but in this universe it could not, simply because so much of mainstream behavior analysis is not contextualistic and the field has so dramatically narrowed. It is revisionist history to argue, as Dr. de Rose does, that CBS ideas have been well received by mainstream behavior analysis. Today they are reasonably well received in some corners, which is why he can make that claim, but that positive reception is decades *after* data were in hand and it is still paltry. RFT is still not on the BCBA task list. BCBAs still fear ethical complaints if they use ACT methods. Even today RFT and ACT are not thought to be part of mainstream behavior analysis. Furthermore, the exploding CBS research base is emerging largely outside of traditional behavior analytic laboratories and clinics.

But here I turn to a more important question. What is scientific progress, and how do we recognize it and create it?

There is no simple answer, but it starts with a view of what science is. I think a good working definition of science is this:

> *Science is a social enterprise that has as its purpose the development of increasingly organized statements of relations among events that allow one's analytic goals to be accomplished with precision, scope, and depth and based on verifiable experience.*

More is needed to apply that definition as a psychologist and a functional contextualist, namely, the domain of analysis and the analytic goal (Hayes, 1993). Functional contextualists define psychology as the study of whole organisms interacting in and with a context considered both historically and situationally; and they adopt the analytic goal of prediction and influence (Hayes, 1997).

When explicated that definition suggests two important areas of progress.

First, we need to recognize that science is a social enterprise. Scientific traditions that fail to attract growing scientific engagements by a community of scholars, researchers, and practitioners are traditions that are dying. Such engagement can be measured by such things as research productivity, citations, grants, students, faculty positions, conferences, special issues of journals, growth of organizations, and the like.

Yes, occasionally there are temporary exceptions when very important areas of work are played out in virtually invisibility among a few, and only later are seen to be progressive. But even then, when they *are* seen to be progressive, all of these social measures finally appear. I know of no area of science that is *known* to be important in the absence of social engagement. It appears to be necessary.

And here CBS has prospered. Work there is more highly cited, more widely funded, more widely published, with more student and faculty interest than any other wing of behavioral thinking. Anyone can go to Google Scholar, Web of

Science, Amazon book lists, association membership lists, or lists of awarded grants to confirm what I have just claimed.

Social engagement is not a sufficient measure of success, however, because of its substantive analytic goals, so a second area needs to be examined. We need to consider whether the principles, theories, models, data, and approaches in a given area are in fact leading rapidly and continuously to new, important, and interesting findings that are coherent from the point of view of the tradition. Are these principles, theories, and models able to be applied precisely and broadly, without distortion to make it so? Do they have depth—that is, do they cohere with finding at other levels of analysis? Are the analytic purposes of the field being met?

These features will vary based on the analytic goal of the field itself, but for behavioral psychology we would expect a progressive field to see increasing effect sizes, an increasing range of application, emergence of new and interesting basic questions, deepening connections to other mature area of work, the discovery of previously unknown facts, and all of this inside a "protective belt" of stability of key assumptions and analytic approaches. As measured against such criteria, the CBS research program has fared demonstrably well (Vilardaga et al., 2009). Relational operants are a fact as much as any fact is a fact in basic behavior analysis. The breadth of impact of psychological flexibility as a set of processes of change is unusual to the point of being unique in clinical psychology (Hayes, 2019). CBS ideas have been shown to be relevant to modern evolutionary science (Hayes et al., 2020; Wilson & Hayes, 2018), cognitive science (De Houwer, 2011), and positive psychology (Kashdan & Ciarrochi, 2013) among many other areas. When COVID hit, the World Health Organization distributed a successful ACT self-help program that had been tested in Uganda with South Sudanese refugees (Tol et al., 2020) on its website (https://www.who.int/publications-detail/9789240003927) as a way of addressing COVID-related stress.

In my view, CBS is a modern form of behavior analysis that is fairly closely linked to the original and broad vision of behavior analysis (Hayes, 2019). Contrary to Dr. de Rose's account, it did not so much "leave the body" as the body left it. If CBS is a "wing," it is a very large one, that is arguably as successful or more so in the two ways that scientific progress is measured than is mainstream behavior analysis itself.

The only sense in which CBS is "separate" from psychology is that its analyses go beyond the study of whole organisms interacting in and with a context considered both historically and situationally to include the analysis of small groups and cultures (e.g., Atkins et al., 2019), and to actively pursue analyses of biophysiological events relevant to psychology. Behavior analysis is similarly positioned, but in contrast to the behaviorologists and Skinner himself when he stated in the last words of the last sentence he ever wrote (1990) that "whether behavior analysis will be called psychology is a matter for the future to decide" (p. 1210), CBS is very dominantly a *psychological* approach and has actively built bridges to mainstream psychology itself.

The bottom line of science is not to be found in its politics or groupings. The bottom line is progress. On that front, CBS appears strong when examined broadly (Zettle et al., 2016). All the rest is commentary.

References

Atkins, P., Wilson, D. S., & Hayes, S. C. (2019). *Prosocial: Using evolutionary science to build productive, equitable, and collaborative groups*. Context Press/New Harbinger Publications.

Cooper, J. O., Heron, T. E., & Heward, W. L. (Eds.). (2007). *Applied behavior analysis* (2nd ed.). Pearson.

De Houwer, J. (2011). Why the cognitive approach in psychology would profit from a functional approach and vice versa. *Perspectives on Psychological Science, 6*(2), 202–209. https://doi.org/10.1177/1745691611400238

Hayes, S. C. (1993). Analytic goals and the varieties of scientific contextualism. In S. C. Hayes, L. J. Hayes, H. W. Reese, & T. R. Sarbin (Eds.), *Varieties of scientific contextualism* (pp. 11–27). Context Press.

Hayes, S. C. (1997). Behavioral epistemology includes nonverbal knowing. In L. J. Hayes & P. M. Ghezzi (Eds.), *Investigations in behavioral epistemology* (pp. 35–43). Context Press.

Hayes, S. C. (2019). *A liberated mind: How to pivot toward what matters*. Penguin/Avery.

Hayes, S. C., Hayes, L. J., & Reese, H. W. (1988). Finding the philosophical core: A review of Stephen C. Pepper's World Hypotheses. *Journal of the Experimental Analysis of Behavior, 50*, 97–111.

Hayes, S. C., Hofmann, S. G., & Ciarrochi, J. (2020). A process-based approach to psychological diagnosis and treatment: The conceptual and treatment utility of an extended evolutionary model. *Clinical Psychology Review, 82*, 101908. https://doi.org/10.1016/j.cpr.2020.101908

Kashdan, T. B., & Ciarrochi, J. (Eds.). (2013). *Mindfulness, acceptance, and positive psychology: The seven foundations of well-being*. New Harbinger.

Skinner, B. F. (1990). Can psychology be a science of mind? *American Psychologist, 45*(11), 1206–1210.

Tol, W. A., Leku, M. R., Lakin, D. P., Carswell, K., Augustinavicius, J., & Adaku, A. (2020). Guided self-help to reduce psychological distress in South Sudanese female refugees in Uganda: A cluster randomised trial. *Lancet Global Health, 8*(2), E254–E263. https://doi.org/10.1016/S2214-109X(19)30504-2

Vilardaga, R., Hayes, S. C., Levin, M. E., & Muto, T. (2009). Creating a strategy for progress: A contextual behavioral science approach. *The Behavior Analyst, 32*, 105–133.

Wilson, D. S., & Hayes, S. C. (Eds.). (2018). *Evolution and contextual behavioral science: An integrated framework for understanding, predicting, and influencing human behavior*. Context Press/New Harbinger Publications.

Zettle, R. D., Hayes, S. C., Barnes-Holmes, D., & Biglan, T. (Eds.). (2016). *The Wiley handbook of Contextual Behavioral Science*. Wiley/Blackwell.

Part VII
Field-Theory Behaviorism

Chapter 19
A Theory of Behavior or a Theory of Psychology?

Emilio Ribes-Iñesta

This chapter is composed of three sections. A first, very brief, deals with the personal biography related to the present theoretical formulation. It will help to make more understandable the changes that occurred in the process of searching for identity in psychology and knowledge of the phenomena it comprises. A second section will be concerned with the first systematic formulation of a field model, as a general theoretical proposal to study all psychological phenomena. A third will address the reformulation of such proposal, its reasons, the changes it implies, and the advantages it represents as a theoretical system.

A Personal Biography

My academic life, ever since I began as an undergraduate student at the *Universidad Nacional Autónoma de México* (UNAM) in 1960, has been a process of continuous search for psychology's disciplinary identity. At the time, psychoanalysis was the dominant conception, and, regarding experimental psychology, but not as part of the university curriculum, Hull and Piaget's orientations predominated, together with the first hints of what was shortly after baptized as "cognitive science," especially with the emergence of information theory as a model of knowledge. At the same time, were also present Gestalt psychology, Soviet psychology oriented to the problem of thought and language, and functionalism centered in the study of memory and so-called verbal learning. My efforts, apart from self-teaching actions (Ribes, 2010a, 2010b), did not achieve satisfactory results.

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Shortly after, in 1964, I had the opportunity to gradually form a disciplinary project with other university colleagues, but this time with the responsibility of sharing it, as a (premature) professor, to students at a university that was just starting its psychology program, the *Universidad Veracruzana* (UV) in Xalapa (Ribes, 2001a). During this first period in Xalapa (1964–1971) I learned, by myself and with the help of my young colleagues, that psychology was never taught or even mentioned during my time as a student. In a very short period of time, I went from psychoanalysis (my original information background), through functionalism, cognitive psychology, genetic psychology, and many modalities of behaviorism, until I finally arrived at the operant conditioning formulation, nowadays incorrectly called behavior analysis. This was part of an inquisitive process facilitated by my personal contact with psychology figures such as Harry Harlow, Daniel Berlyne, Hobart Mowrer, Teodoro Ayllon, and Sidney Bijou, among others.

My adoption of operant theory, as a reference frame for psychology, was based on four main reasons: the first had to do with the development of a methodology focused on the individual organism, ruling out statistical designs based on the assumption of the randomness of psychological behavior; the second was related to the possibility of including both animal and human behavior in its scope of analysis, including some forms of dyadic relations that intersected with social phenomena; the third was related to the possibility of extending its techniques to solving problems in social and natural environments; and the fourth and last was ideological in nature, highlighting the interdependence between behavior and environmental factors and, consequently, ruling out a solipsistic conception of the origin of psychological events, linking them with ecological determinants in animal evolution, and with the interrelationships between social productive modes and history regarding human behavior. For 15 years I thought, wrote, and investigated animal and human behavior under the categories of operant theory, trying to clarify concepts and systematically interpreting apparent empirical anomalies (Cabrer et al., 1975).

However, as I tried to deepen and extend the implications of my commitment to operant theory, doubts began to arise regarding the logical soundness of its concepts and the way in which experimental research was related to applications of such knowledge. Not only did it seem naïve to think of a behavioral technology as suggested in the early years of effervescence in behavior modification, behavior therapy, and applied behavior analysis, but also it seemed questionable to assume the possibility of extending, as simple extrapolations, so-called "principles" of behavior, to human behavior in social situations. These "principles" were nothing more than experimental operations developed in the research of animal behavior in operant conditioning chambers, with all the qualitative and quantitative restrictions it entailed.

As I have previously mentioned, my relationship with William N. Schoenfeld was decisive in this period of my academic life. Schoenfeld's interest in experimental research in classical and operant conditioning was not motivated by interests related to technological applications. Unlike Skinner, who emphasized prediction and control as scientific goals, Schoenfeld conceived scientific practice as an effort to systematize knowledge and expand its horizons by the discovery and formulation of new relations and concepts. As a part of this process, he proposed general criteria to unify classical conditioning, operant conditioning, and non-contingent reinforcement, to integrate in a continuum of behavior in time and space the situations conceived as positive and aversive, as well as several stimulus functions. In two previous writings, I have outlined his contributions and how they influenced my way of thinking (Ribes, 1996a, 2017).

Given that I have already examined these aspects in detail (Ribes, 1994, 1996b, 1999, 2004a, 2004b; Ribes & López-Valadez, 1985), I will not elaborate on the logical limitations of operant theory (and of all conditioning theory), and its operational and technological bias. It will suffice to mention that since 1975 I began to search for a new way of approaching the study of psychological behavior based on the work of J. R. Kantor (1924-1926), paying attention to contradictions, anomalies, and limitations that arose from the criticisms developed by Schoenfeld since 1956, as well as the results of experimental research using the T system, the difficulties in experimentally examining human behavior, especially its linguistic and social dimension, and the limitations and conceptual weakness shown in the applications of so-called "principles." As a result, I concluded, to the surprise of my colleagues, that operant conditioning (or behavior analysis) constituted a reductionist, linear, atomistic, cause-searching, and technologically oriented approach. Unfortunately, to this day most of my colleagues still do not understand my conclusion.

Early in 1982, I began to develop a new theoretical framework, based on a field logic, that could systematize the experimental knowledge achieved up to then, especially that arising from operant theory, but that at the same time allowed "seeing" in a new manner such knowledge and opening inquiries and applications to new problems and ways of conceiving them. This process accelerated as part of the design of an undergraduate psychology program at UNAM-Iztacala, and the multitude of doubts, proposals, and rehearsals that took place during its course with an important group of colleagues (Pérez-Almonacid & Gómez, 2014; Ribes, 2010a). The result was the publication in 1985, in collaboration with Francisco López-Valadés, of *Theory of Behavior: A Field and Parametric Analysis* (TB—*Teoría de la conducta: un análisis de campo y paramétrico*), which was in fact completed in 1983.

A Theory of Behavior: A First Step

TB was conceived as an "internal" criticism of conditioning theory, especially of its operant conception. The theoretical proposal encompassed three fundamental issues: (1) the replacement of an atomistic and causal analysis of conditioning theory with a field, deterministic and molar analysis, (2) to make explicit theory's assumptions, and (3) the formulation of a taxonomy of different kinds of functional behavior organization, beyond the operational respondent/operant dichotomy, verbal/nonverbal, and rule-governed/contingency shaped subdivisions.

Kantor proposed a field logic that assumed that components of a psychological segment, defined by the interaction of an individual organism with a stimulus object, were interdependent and, therefore, none could have causal properties over the other. In other words, in a psychological field, as in any kind of field, there are no causes and effects (neither independent/dependent variables), but only interdependent relations among the elements that make up or constitute the field. Field determinants are the interrelations among its components. No agent outside the field influences the field. Not all the components of a field have the same functional properties, and, therefore, in the case of a psychological field it is essential to specify the categories that describe these general properties. Kantor specified three categories to describe a psychological field: the *contact medium*, the contact between an organism and a stimulus object or *stimulus-response function*, and *setting factors* involving situational factors and the interbehavioral history, made up of the reactive biography and stimulus evolution.

The only contact medium identified by Kantor was what I named, for distinctive purposes, the physicochemical contact medium. With this concept Kantor followed Aristotle (*De Anima*) when he described the sensitive soul, in which the living entity, unlike the nutritive soul, incorporated only the form, but not the substance of another entity. I considered that the logics of the concept of medium required two additional types of contact media, the ecological and the conventional (at that time called normative). Kantor, in his diagram of the interbehavioral segment as a psychological field, included the contact medium within the segment, as that which enabled the contact between the organism's responses and the stimuli as properties of an object. A contact was identified as a stimulus-response function, an inseparable interaction, and the medium as those environmental conditions which made it possible. However, the logical function of the concept of contact medium contradicts the possibility of considering it as another element of the field. The physicochemical contact medium corresponds to the logical identification that, for example, light is a medium required for an object to stimulate a photically sensitive organism. In the absence of light, there is no vision, just as in the absence of an atmosphere there is no hearing, and in the absence of gravity there is no possibility of directed movement. In this way, the contact medium, multimodal in the physicochemical case, is a necessary general condition for functional contacts between organisms and stimulus objects, but it does not participate, as such, as an element of the field in which contacts take place. For this reason, the concept of contact medium, as an enabler, is empirically empty. It does not describe any object or event and therefore neither of the other functions assigned by the field concepts. For this reason, in the theory's latest formulation (Ribes, 2018), it has been proposed that the contact medium supports, surrounds, and at the same time limits each psychological field but is not a part of it since it makes it possible. This reasoning, as will be seen later, has serious implications for the very conception of psychological events. It can be said that the physicochemical contact medium allows experiencing the stimulus objects, and in this sense, it is important to stress that the physicochemical contact medium implies the potencies of a reactive and/or active organism. This medium enables de actual *presence* of both stimulus objects and the organism's reactive systems. As a complement, two additional contact media were proposed in **TB**: the ecological medium, which enables survival, and the conventional medium, exclusively human, which enables living together. Both media necessarily require the physicochemical medium, just as the conventional medium requires the ecological medium. Therefore, since media enable functional contacts, they are not mutually exclusive in a psychological field. While the physicochemical contact medium logically represents the physical and chemical conditions required for psychological behavior to occur, the ecological medium represents the biological habitat conditions, and the conventional medium represents the conditions of social formations in terms of institutions and customs.

The other two logical categories correspond to constitutive elements of the field as objects, properties, and events. One group corresponds to stimulus-response functions, and the other to what could be distinguished as dispositional or setting factors. The latter do not identify the stimulus-response function as a contact, but deal with the probability of its occurrence, in terms of facilitation or interference. These factors are part of the properties of all objects, events, and conditions of the organism, but with a function that is relative to the special circumstances in which contacts take place. Dispositional or setting properties are not immanent or intrinsic to the components that perform such function but are enhanced by the present circumstances in the organism-object interaction. The *situational* dispositional factors have to do with modal properties of stimulus objects, with chemical properties and their biological effects, with the states of the organism, and with the parameters of intensity and relative density that these properties possess at a given moment. In the case of human interactions, they are related to institutional and cultural relevance. In the case of *historical* dispositional or setting factors, these have to do with the moment that defines the initial condition of a field in terms of the organism's reactivity/activity tendencies and the functional familiarity with present stimulus objects and events. History does not act from the past but constitutes the starting point of the present field. Finally, the remaining logical function in the field corresponds precisely to its defining components: the interaction of the organism with a stimulus object (or another organism that shares those properties) as a functional relation in circumstance. The taxonomy of psychological behavior organizations constituted a tentative classification of the types of relations or interactions that make up these contacts. It is only necessary to mention that, on the part of the organism, Kantor distinguished reactive systems, responses, and the resulting response function, as well as the corresponding stimulus objects, stimuli, and stimulus function. Objects are not stimuli, and stimuli constitute changes in the properties of objects but are not independent of them, just as responses and reactive systems consist of changes in the activity of organisms and are not independent of them. This contradicts the postulation of stimuli and responses as isolated and independent events, either as instances or as classes. Psychological contacts always involve individual organisms and objects, not fictitious self-being stimulus and responses.

A second aspect dealt with in **TB** was the exposition of a set of theses that supported the theoretical perspective being adopted. Such theses included assumptions and proposals of an epistemological order, a methodological character, and a logical nature. The former referred to the conformation of its subject matter, psychological behavior, as an interdependent field of relations between the individual organism and stimulus objects. The second had to do with the corresponding methodological criteria for the analysis of the subject matter thus formulated. Finally, the logical theses posed the relationships between constitutive theoretical categories consistent with previous theses. Present space does not allow to comment in detail those theses. Those interested may review Ribes and López-Valadez (1985).

The elaboration of the taxonomy, formulated as a substantive *corpus* of the theoretical approach, obeyed two fundamental criteria. On the one hand, these taxonomic categories should systematically include the *empirical universe* of phenomena described by psychology up to that time, as well as their referents in expressions of ordinary language. It is important to highlight the empirical nature of the object to be systematized, as opposed to attempts to integrate or translate concepts as if they in fact denoted precise and identifiable phenomena and events. On the other hand, these categories were intended to develop a heuristic function, to visualize new phenomena or forms of psychological organization, not contemplated in the practice of ordinary language, nor by conceptual structures of other past or present theoretical alternatives. **TB** represented a first attempt, systematic and integrative, to seek a logic and language specific to psychology, oblivious to any eclectic or reductionist slip, putting aside models form other disciplines, avoiding the theoretical division of the psychological universe due to different conceptions of its subject matter, and taking special care not to confuse words with phenomena and events.

Based on these premises, five types of field organization were identified covering the entire universe of psychological phenomena, including animal and human behavior. The concept of contingency was central to formulating these five psychological field organizations or psychological functions, as they were called. These functions consisted of different organizations of contingency relations between the organism or individual and stimulating objects or other individuals. The psychological field was defined as a field of contingency relations. The concept of contingency was used, and I continue to use it, according to its usual meaning, and not the one incorrectly employed by Skinner (1948) and which persists within operant theory. "Contingencies of reinforcement," in English, can be understood in two ways. The first is the reinforcer being contingent (conditional) to another event (usually a response), and the second as those changes in responding taking place as an effect of the presentation of a reinforcer. The first is the correct one in the case of operant conditioning: the reinforcer (and its result, reinforcement) is an event contingent, that is, conditional or circumstantial, to the occurrence of a behavior or response that produces its occurrence. Schoenfeld and Cole (1972) described the contingency by expressing that the distribution of reinforcing stimuli is determined by the distribution of responses. In contrast, the term "reinforcement" only describes that the stimuli distribution determines differential distributions of responses. In operant conditioning, both distributions are interrelated but do not constitute a linear or unidirectional phenomenon. On the other hand, the second way of understanding the expression in English identifies that the circumstances depend on the reinforcing stimulus and, to that extent, the concept of contingency can be identified with that of consequence or subsequence of the stimulus that follows behavior. This is precisely what Skinner did when interpreting the "superstition" experiment, by stating that if the term contingency meant anything, it was a temporal relation between the response and the reinforcer. In that experiment there was no predetermined "response," but rather a pattern of movements during the interval between food deliveries. There was no contingency between such movements and the delivery of food. The contingency was temporal, that is, the occurrence of food depended, was circumstantial, conditional, over time (in fact, on the interval between food presentations).

The relations defining psychological phenomena are always characterized by being contingent, that is, circumstantial. They are not predetermined (i.e., they are not necessary by or in themselves), and, therefore, they are always conditional or circumstantial. Being contingent is always "of" or "to" something, that is, depending, being conditional or circumstantial on occurrences, on the properties of occurrences, or on the properties of another object or event. Even a temporal contingency is described by a change, in a watch or other condition, by which the dimension of "time," relative to the dimension of "space," is identified (Ribes, 1992). Contingency relations in a field are conceived as interdependent relations, that is, mutually conditional or circumstantial in their functionality. From this perspective, in **TB,** and later in another paper (Ribes, 1997), the field was described as a system of synchronic contingency relations, even when occurrences could and do occur diachronically. This analysis was illustrated with the phenomena studied using procedures of classical and operant conditioning. We will once again borrow the latter to exemplify the meaning we give to the term contingency and its synchronic character as a functional relation in a field.

The "discriminated" operant has been described by the three-term contingency relation paradigm. The discriminative stimulus sets the occasion for a predetermined response to produce the occurrence of a "reinforcing" stimulus. The discriminative properties of the stimulus can be posed only if a recurrence of the relation or of the response is observed (Skinner, 1938; Ferster & Skinner, 1957). Nevertheless, in the case of so-called verbal behavior, the reinforcer is not identified by the unilateral increase in the frequency of the response (Skinner, 1857). The contingency relation is seen as a linear, diachronic relation, in which the discriminative stimulus occurs first, then a response, and finally the reinforcing stimulus following the response, in that order, and with the expected effect in the frequency of the response or the events sequence. However, this set of events involves more than the two relations included in the three-term contingency: discriminative stimulus-response and response-reinforcer, given the previous occurrence or presence of the discriminative stimulus. From a diachronic perspective, there are more than two explicit occurrence-contingency relations: given the discriminative stimulus, the reinforcer can occur (as in the relation between stimuli in classical conditioning), and if the response occurs the reinforcer occurs. There are two occurrence-contingencies of different types: one establishes the circumstance, the other actualizes it. There is a stimulus object never mentioned in the situation: the *operandum*, usually a lever or a key. The *operandum* has stimulus properties for the occurrence of the predetermined response. Skinner (1938) himself commented that without intervention, the so-called operant level of lever pressing was above zero, that is, the rat (or the pigeon in the case of the key) presses the lever out of curiosity or accidentally. *Shaping* is a process of prompting orienting, approach, and manipulation responses that shorten the repeated occurrence of the operant response. Paradoxically, and contrary to what is claimed, during shaping, there is no operant relation in the strict sense because changes in behavior do not produce the delivery of the reinforcing stimulus. An additional response or behavioral pattern can be identified, one which has scarcely been analyzed or made explicit in the operant studies, with few exceptions, as when the so-called adjunctive behaviors are examined: the behavior of consuming food or water. In fact, food and water reinforce, because they are consumed.

Thus, there are at least four explicit occurrence-contingencies in the operant procedure: (1) the possibility of the reinforcer occurring given the brief or sustained presentation of the discriminative stimulus; (2) the occurrence of the operant instance (lever pressing or key pecking) given the outstanding presence of these stimulus objects (*operanda*) in an environment with reduced texture, and above which discriminative stimuli are usually located; (3) the occurrence of food when the operant instance (response) occurs, according to a predetermined criterion; and (4) the occurrence of the behavior of consuming the reinforcing stimulus once it is supplied as a discrete event. At the same time, in each food delivery episode, as synchronic relations between the different specified elements, other types of contingencies, which I have called *function-contingencies*, take place.

Function-contingencies, as the name implies, describe the circumstantial, conditional, or dependent functional properties resulting from occurrence-contingencies. While the latter occur in succession, diachronously, function-contingencies operate synchronously, that is, encompassing the entire episode. The properties of each component cannot be separated from those of the others. In this way, the so-called discriminative stimulus facilitates or encourages operant responses in its presence because it is related to the occurrence of the reinforcing stimulus (although this relationship is usually not very "clean" in multiple and concurrent reinforcement programs, in which a large proportion of response instances are not followed by the reinforcer in the presence of the stimulus). Similarly, lever pressing and key pecking become components of an eating behavioral pattern, as Skinner (1938) himself described his first studies (feeding reflex). The lever, the dispenser, and the food or water make up a single stimulus segment, so that, in a very general way, at least three function-contingencies may be identified in the operant procedure. When adding these contingencies to those previously indicated as occurrence-contingencies, we have that a simple discriminated operant episode includes, roughly, seven occurrence and function contingency relations, interdependent among each other. Two concepts, introduced in **TC**, are fundamental to understand the organization of a field of contingency relations: *mediation* and *functional detachment*. These concepts describe different moments in the psychological field, in terms of states and processes, although this perspective was developed later (Ribes, 2007).

Mediation refers to how the functional organization of a field of contingency relations is articulated, while functional detachment has to do with the process of continuous functional changes in the components of the field as a result of that articulation, that is, its mutual interdependence. A field component, which is always an occurrence as a change in a stimulus object or in the activity/reactivity of the individual, mediates its organization when different contingency relations become articulated by its occurrence. The absence or presence of the mediator determines how the field is functionally organized. An example is the contrast between a contingency field resulting from a classical conditioning procedure with those of an operant conditioning procedure. In classical conditioning, the conditional stimulus is originally neutral regarding eating behavior (including salivation), and when presented only elicits the orienting reflex. Its presentation as an antecedent event associated with food delivery changes the function of that stimulus, turning it into a signal for food that evokes salivation in advance as a preparatory response to chewing and swallowing. The component that articulates that the now conditional stimulus does not evoke the orienting response and instead begins eliciting salivation without the presence of food in the mouth is the unconditional stimulus. The presentation of food as a contingent event to the presence of the neutral stimulus, and its non-presentation in the absence of that stimulus, mediates the articulation and functional changes of the components in the classical conditioning procedure. The occurrence of food is contingent to the conditional stimulus (a tone), and the functional properties of the tone are contingent to the occurrence of food. In the operant conditioning procedure, on the other hand, the "discriminative" functions of the antecedent stimulus and the very occurrence of the reinforcing stimulus, water, or food, depend directly on the occurrence of a predetermined response, the operant instance. In this case, lever pressing and key pecking are the mediators of the contingency relations established by the procedure. If the contingency between antecedent stimulus and reinforcing stimulus were maintained, ruling out the occurrence of the operating instance as a criterion for the presentation of the latter, there would be a contingency relation characteristic of classical conditioning, as is the case of superstitious behavior which is not, strictly speaking, operant behavior.

Functional detachment describes the transitions in functional properties of the components that participate in a field of contingency relations. While mediation has to do with the configuration of occurrence-contingencies, functional detachment has to do with the development of function-contingencies. Functional detachment is linked to a change in the original functional properties in the individual's reactive systems, initially with biologically determined functions and, later, with functions established from their particular interaction with environmental circumstances, whether these are of an ecological or conventional nature. Functional detachment describes the functional autonomy of psychological behavior in the face of its circumstances of occurrence, based on biological behavior, as well as the continuous changes in the functions of psychological behavior with respect to previous situational circumstances. Functional detachment occurs to a different extent depending on the characteristics of the biological subsystems integrated in the different psychological reactive systems. In the classical conditioning example, salivation is a detachable component of the food ingestion response, insofar as it can take place without food being present, in the same way, that certain neurovegetative responses can occur in the absence of the stimulation that produced its occurrence integrated into a biological pattern, such as that of withdrawing a limb under an intense stimulus condition which produces a painful reaction. Historically, detachment was discussed for the first time in relation to classical avoidance conditioning. Neal Miller (1948) stated that avoidance behavior was mediated by a fear reaction, as a *detachable* component of the painful reaction to electric shock. Functional detachment, therefore, takes place initially as the occurrence of biological behavior in circumstances that are not related to its occurrence. However, functional detachment also occurs with behaviors that are not directly related to biological functions. Gestures are all the result of the detachment of movements originally used with a direct mechanical effects: pulling, pushing, kissing, etc. They occur, without the need for a direct mechanical contact, with similar or broader effects. In the case of behaviors with linguistic morphology, words, for example, are learned and expressed in the presence of a particular object or circumstance, but due to their conventional nature, they can occur in the absence of any object or circumstance, be used in other situations and with different objects, and may even become, as occurs with written texts, objects themselves. For this reason, sometimes, when reading a text, we can react as if we were in the circumstance narrated by the text. It can be said that functional detachment describes the changes in function between the various forms of activity/reactivity of the individual and the stimulus objects and events with which he/she interacts as part of various contingency relations. Psychological behavior originates and is the result of functional detachment, and, to that extent, its *meaning* (or purpose) lies in the circumstances in which it dynamically emerges from and continuously changes into. Functional detachment is the general process that identifies and characterizes psychological behavior.

In **TB**, five different types of stimulus-response functions were formulated, as were called at the time the different forms of organization of functional contacts as contingency fields. These stimulus-response functions were contextual, supplementary, selector, referential substitution, and non-referential substitution contingency fields. Unlike classifications formulated by the various conditioning theories, these types of individual-object relations were not conceived as horizontal algebraic interactions. On the contrary, they were formulated as *qualitatively* different types of behavior organization, of inclusive complexity, but in which the functional properties of the components were determined by the molar organization of the contingency field and not by fragmentary relations of some components among each other. Therefore, the inclusion of components of less complex functions in the more complex ones did not mean a simple compositional addition of elements. The complexity was given by the number and diversity of contingency relations that might be established in each type of interaction.

If we turn back to the previous example of classical and operant conditioning, conditioning theory considers them processes at the same level that interact additively or subtractively (which is how, e.g., conditioned suppression is interpreted). In **TB**, relations established by classical conditioning procedures could be classified as *contextual* interactions, in which the unconditional stimulus functions as a mediator of the contingency relations. The activity of the organism does not affect or alter the contingency relations between the conditional stimulus and the unconditional stimulus. The organism can be affected but cannot affect them. On the other hand, in the relations established by the operant conditioning procedure (and, being rigorous, we would eliminate all cases of "temporal contingency" from this procedure), an action by the organism (acting on an *operandum*) mediates the potential contingency relation between the discriminative stimulus or the simple presence of the *operandum* and the occurrence of the reinforcing stimulus. The operant response instance alters the potential contingency of the reinforcing stimulus occurrence. This contingency is only actualized if a predetermined action of the organism occurs, and the way in which such actualization takes place also depends on the temporal distribution and other aspects of the operant response. This type of phenomena is recognized in **TB** as *supplementary* interactions. The relation between stimuli in both procedures is similar: the reinforcer (or unconditional stimulus) only occurs in the presence of the antecedent stimulus, that is, the reinforcer is conditional or contingent upon the occurrence/presence of the antecedent stimulus, which receives a different name in each procedure (conditional or discriminative). The major difference is that the contingency between stimuli becomes actualized through an action of the organism in the operant situation. Although the "component" of the contingency between stimuli is similar, it has different functional properties in each situation: the effects in time (delay and duration) as well as its intermittency, have different effects in the operant situation and in the respondent. This occurs because their functional properties depend on the molar structure of the contingency relations, and not simply on the specific parameter values of the temporal contingency between the stimuli.

In **TB**, the classification of the various stimulus-response functions was based on the mediator component. Thus, in the contextual function, the mediator was the stimulus to which the response was originally made, fostering the detachment of the response from other stimulus objects or modalities. The supplementary function was conceived as a contextual relation mediated by the response of the organism. The selector function consisted of a supplementary relation mediated by the organism's response to a stimulus that conditioned contingencies moment to moment. The last two functions were exclusive to human behavior, as they required a reactive linguistic system and responses that took place in any of its three modes: observing/gesturing, listening/speaking, and reading/writing. In the referential substitutional function, the linguistic response of one individual mediated a selector relation in another individual. Finally, in the nonreferential substitutional function, a linguistic response mediated the relationship between two originally referential relationships.

For 30 years **TB** fulfilled the task of directing and guiding experimental research in animal and human behavior (Ribes, 2006). Experimental preparations were designed to evaluate the different functions in animals and humans, dozens of experiments were carried out, and new concepts and methodologies were formulated to extend the theoretical system to the analysis of individuation (development and personality), dyadic social interactions, and possible applications in the fields of health and education mainly. The theoretical analysis of field relations was deepened, and all this was materialized in a publication, in Spanish, which was called *Behavior Theory: Advances and Extensions* (*Teoría de la conducta: avances y extensiones*—Ribes, 2010b), which became known as **TB2**.

However, despite the obvious advantages of the new theoretical perspective, two types of difficulties were met. The first one had to do with operational inaccuracies in order to experimentally evaluate the selector, referential substitution, and non-referential substitution functions. The second had to do with the endurance of molecular, linear, and formalist remnants in the approach and analysis of problems investigated. As a result, and after a long process of transformation, in 2018 I published, in Spanish, *The Scientific Study of Individual Behavior: An Introduction to the Theory of Psychology (El estudio científico de la conducta individual: Una introducción a la teoría de la Psicología)*, which I will refer to as **TP**. This new formulation raised significant changes, some radical, in the way of conceiving psychology and its study.

From a Theory of Behavior to a Theory of Psychology

TP presents important changes in several aspects compared to **TB**. Some of them sponsored by the limitations found in the design and analysis of experiments, others by the need of making explicit psychology's connections with other disciplines and fields of application, others as a direct result of new investigations, some as a consequence of the discovery of the nonlinear dynamical systems analysis, and, finally, by the careful reading of Ludwig Wittgenstein and his monumental change of perspective on language as a social practice.

We will approach the analysis of **TP** in three different sections: (1) the redefinition of psychology's subject matter, (2) the reformulation of stimulus-response functions as states and transitions of functional contacts in a contingency field, and (3) the extension of the field model to the study of individuation as development (becoming), comparative analysis and behavioral styles, as well as to the multi- and interdisciplinary relations of psychology. In the final section, we will examine the role of psychological knowledge in understanding social practices.

Getting Back to Psychology's Subject Matter

TB proposed that psychology's subject matter was the interaction between an organism and its environment, later specifying that this interaction was actually with a stimulus object or event. The individuality or uniqueness of the components that made up such interaction defined the nature of psychological phenomena, an approach that was correct in principle. However, this approach neglected the incontrovertible fact that psychology must anchor its subject matter in a universe of phenomena belonging to ordinary knowledge and language practices. In fact, the historical interest in a scientific psychological discipline arose from attempts to systematize the knowledge of so-called conscious experience (Brentano, 1874/1924–1925), or to question the legitimacy of the introspective method and displace the conscious experience "outwards," in the form of observable behavior, that is, what is done and what is said (Watson, 1913). Regardless of the theoretical orientation assumed, different psychologies or psychological systems (Ribes, 2000) have accounted for and explained so-called "mental" phenomena, by identifying them with ordinary terms such as perception, sensations, imagination, memory, emotions, thinking, and so on.

Unfortunately, none of the existing psychologies have been systematically concerned with examining the functional nature of "mental" phenomena incorporated as part of ordinary language practices. Psychologies and philosophies have framed them as "mental" phenomena, despite the fact that in ordinary language practices the term "mind" is rarely used, and when used it is in fact an appropriation of expressions of particular medical and psychiatric practices. Psychologies and philosophies have incorrectly assumed that such "mental" terms in ordinary language practices refer to, denote, or report the occurrence of events not directly observable, and which are, in that sense, antecedents or determinants of those practices. This confusion about the logic of the expressions and practices of ordinary language (Ribes, 1990; Ryle, 1949; Wittgenstein, 1953) has led to the assumption that psychologies should explain the processes, entities, and events that these terms supposedly report or denote. This obligation has been assumed in three different ways. A first strategy has been to assume that these words denote processes or events that are approachable only through operational definitions, that is, by identifying them with the behavior that occurs in situations and procedures designed *ex profeso*, which has led to an unlimited proliferation of mental or psychological processes, events, and entities as a result of the also unlimited operational definitions that have been formulated. The resulting Tower of Babel has increased conceptual confusion, multiplied "processes" under the same name, without psychologists realizing that operational definitions do not denote entities or events, but only delimitate the ways in which we speak of things and act upon them (Ribes, 2003). A second strategy has been to seek changes in the central and autonomic nervous system that "correlate" with activities in various tasks, where it is assumed that these mental "processes" are involved. In this case, experimental "models" of these processes (a sort of loose operationalism) are used to assign them "material" or physical support in terms of electrochemical changes in the nervous system, explicitly assuming that the mind is a function of the brain. Finally, the third strategy, usually employed in the various modalities of "behavioral" theories, consists in translating the psychological or "mental" phenomena labels identified in ordinary language practices into concepts appropriate to such theories, for example, when talking about images as conditioned sensations, etc., thus violating the principle that two logically different languages are not directly translatable.

TP has dealt with this problem in a completely different way, under the influence of Ryle (1949), Toulmin (1953) and, especially, Wittgenstein (1953).

Initially, it is proposed that psychological or "mental" phenomena are a constitutive part of ordinary language practices and that expressions identified as psychological or "mental" do not refer to anything. Rather, as an integral, inherent, and consubstantial part of such practices, they *are* the phenomena in question. They are not reports of hidden events, nor do they name anything. They are part of the phenomena consisting of episodes and their circumstances in ordinary language practices. A certain phenomenon of "memory," which involves expressions of "remembering," *is* the episode in which such expression takes place. There is nothing external to the episode as a psychological phenomenon, nor internal events or activities in the protagonist of the expression. Psychological phenomena are not special activities or entities. They are episodic relations between individuals in ordinary language practices, and as relations, it makes no sense to predicate their direct observability (Ribes, 2001b). The elements in the relation can be observed, but not the relation itself. Psychological phenomena, therefore, do not constitute discrete singular occurrences but involve molar relations between two individuals (although sometimes a single individual can incorporate different functions in the same episode).

This approach assumes two things. The first is that ordinary language practices, made up of "mental" terms and expressions, are the original constitutive source of psychological phenomena, its "raw" material. The second is that not all practices between individuals and, therefore, not all individual behaviors or "experiences" qualify as psychological phenomena. Consequently, psychological events, contrary to what both mentalistic and behavioral psychologies suppose, do not constitute a continuum of occurrences from birth to death, but, on the contrary, consist of discontinuous episodic fragments, interpolated between intervals consisting in purely biological events, and others that correspond to episodes of ecological or social character, as will be seen later. Since the original source of psychological phenomena are ordinary language social practices, we might question the occurrence of psychological behavior in the animal world. This issue has two angles of analysis. One is that of psychological phenomenology of animals in ordinary life, and another that of the legitimacy of including the psychological behavior of animals in the scientific discipline.

The inclusion of animal behavior in psychological studies was an outcome of the impact of evolution theory and, especially, of Darwin's (1871, 1872, 1974) intuitions regarding the animal mind, formally approached by his disciple Romanes (1883/2016). Based on evolutionary thought, human mental faculties must necessarily find its antecedents in previous species of animal phylogeny. This assumption was based on two aspects. The first is that emotions in humans represent morphological vestiges of defensive and other behaviors in animals. The second is that natural selection must necessarily operate through intelligent behaviors in individuals, which allow the adaptation and survival of the species. These two assumptions undoubtedly encouraged the first studies on animal behavior and the beginning of comparative psychology (Watson, 1914; Ribes & Burgos, 2006). However, apart from this historical fact, there are two arguments that make plausible to argue that animals show psychological behavior. The first consists of an inclusive extension of ordinary language practices. The domestication of different animal species, to a greater or lesser degree, through coexistence or protected isolation, has favored human interaction with these species, speaking with or about them, as if to some extent they could understand language. Obviously, this does not happen, although different forms of communication take place between humans and animals, who seek territory, security, food, and conditions for reproduction. In these interactions, and with wide variations between different species, we attribute to animals, psychological behaviors like ours. The variety of species includes even the famous octopus that predicted the results of the world soccer championship. In non-domesticated or not captive animal species, it is not customary to think that they show psychological behavior. The second argument is logical and epistemological in nature, and belongs to the domain of scientific doing. A science's subject matter always constitutes an abstraction of the properties that are transversal, and shared in specific conditions by all the phenomena, entities, and concrete events, apparently singular, that are ordinarily known by the direct or indirect experience of others. The formulation of psychological behavior in the form of functional contacts, as circumstantial relationships between a biological individual and a stimulating object (physical, biological, or conventional), transcends the psychological phenomenology of shared practice and, to that extent, allows us to assess whether the type of relationship thus specified can be identified in species other than humans. To the extent that the reactive systems of individuals of other species show some form of functional detachment in the face of environmental circumstances, and such detachment dynamically affects their subsequent contacts, psychological behavior can be identified. Nevertheless, this is only possible in the animal kingdom. The other four kingdoms of life do not show the reactive differentiation, integration, and coordination required for functional detachment to take place. It is in the animal kingdom that this occurs, starting with the coelenterates, by the appearance of the nervous tissue in the form of ganglia, which allowed the coordination and integration of differential forms of sensory and motor reactivity. Based on these criteria, insofar as there is conformity with the functional domain defined for psychology, it can be asserted that individuals of the species that make up the animal kingdom may show psychological behavior.

Finally, it is important to point out, in this regard, the logical relevance of the concept of contact medium. This concept is a category whose logic is only applicable to psychology, and it is so for two complementary reasons.

The first has to do with the fact that psychology is the only discipline whose subject matter lacks substantial entities of its own (Ribes, 2013). Historical pseudo-solutions to this situation were to identify psychological events with mental experience or with brain structures and functioning. Both ontological attempts were reductionistic, whether monistic or dualist. Psychology is the only scientific discipline that does not study entities and their properties, as does physics, chemistry, biology, social-historical science, and linguistics. Psychological events, unless you want to reduce them to an epiphenomenon of the brain or of social practices, are not identifiable by an entity, and take place only as episodic relations in circumstance. Relations occur between entities, a biological individual, and a physical, biological, or conventional object, but such entities cannot be characterized as psychological. This is one of the reasons why historically it has been so elusive to conceive the subject matter of psychology. Its reduction to the function of the brain and the postulation of a substance such as the spirit or the mind are, with different nuances, the proposals that have been given to substantiate psychological phenomena and continue to be offered for more than 21 centuries.

The second reason is that being psychological events or phenomena an episodic relation in circumstance, the relation is restricted to biological individuals and not to species, groups, or social formations. An individual exists as a member or part of a set or group, and in the case of animals and man, individuals can only be identified in the context of a species and an ecological niche, or as member of a social formation. There are no "individuals" in or by themselves, isolated, independent of a species or a social formation. Consequently, psychological behavior can only occur embedded within interactions taking place in a species or social formation. There is no an abstract or universal individual, as a substance equivalent to the soul or spirit, that can sustain the "individual" by itself as a subject matter. That is why the contact medium, as a *logical* category, represents the conditions in an ecological medium and its species (ecological contact medium), as well as in a given social medium, its culture and its institutions (conventional contact medium), which enable the functional contacts of individuals with other individual entities. They do not represent any specific empirical universe, but the conditions that logically make such universes possible. In the first case, these conditions highlight the circumstances of survival. In the second case, these conditions highlight the circumstances of living together. For this reason, psychological phenomena intersect two different fields of knowledge, biological science as ecology and historical social science as formations organized in institutions and customs. Psychological phenomena or events can only take place as relations related to the individual within the ecological or social media.

Functional Contacts as States and Transitions of a Field

In **TP**, the concept of stimulus-response function was replaced by that of functional contact. Two reasons were responsible for this change. The first is that it is more accurate for describing the nature of relations between the individual and an object. It is not an "interaction," which is usually brief and discrete, but it has to do with a functional, non-mechanical contact, which can vary in duration and in its characteristics, including those activity patterns involved in the contact. To speak of a functional contact is to refer to a molar relation, which does not include a single reactive form or a single object, but rather a system of contingency relations in which the individual participates with respect to various objects and variations in their parameters of occurrence. The second reason is that the concept of stimulus-response function suggests, at least, a molecular covariation between a particle of the organism's activity and an instantaneous change in the object. Words are not neutral and, sooner or later, their uses attract the logic from which they come from, thereby contaminating the remaining concepts. Abandoning the concept of stimulus-response function means ruling out all influence of the atomistic, compositional, and linear logic that underlies conditioning theory. It constituted a painful, yet necessary, separation from the original formulation.

TP also considers five types of psychological behavior as fields of contingency relations. However, the types originally corresponding to the selector, referential substitution, and non-referential substitution were radically reformulated. Likewise, the conception and description of the organization of a contingency field, the synchrony of different contacts, and the characteristics of the transitions between different function-contingencies, was changed. Functional contacts, as was barely mentioned, constitute molar contingency organization systems in which participates, privileged from an analytical point of view, an individual behaving with respect to different functional dimensions made possible by his/her reactive systems. Contingency fields are examined as dynamic contingency systems regarding the individual's continuous activity in time and space, functionally identified by five molar dimensions of measurement: directionality, preference, persistence, variation, and vigor. Achievement measures, which traditionally characterize the analysis of animal and human behavior, are considered optional and complementary (Ribes, 2007).

The new proposal includes the following functional contacts: coupling-contingencies, alteration-contingencies, comparison-contingencies, extension-contingencies, and transformation-contingencies. The name of each type of contact underlines the type of functional relationship that characterizes each one. Unlike **TB**, dispositional functions are integrated as part of the field's stimulus objects (in correspondence to the states, history, and reactive characteristics of the individual). Situational factors are identified through *Dispositionally Relevant Objects* (DROs), whose dynamic characteristics vary diachronously throughout the field and may involve synchronously different stimulus objects. Historical factors, as interactive history, are identified with the contact's initial condition in the conformation of the field. Since this initial condition depends directly on the individual's reactivity/activity, history becomes apparent as a functional bias with respect to certain stimulus objects and some of their properties, a bias characterized by some, but not all, of activity's molar dimensions. History always acts in the present and changes continuously. Given the continuous and multimodal nature of an individual's activity, the analysis of "behavior" is carried out based on reactivity/activity patterns (RAPs), and not in terms of discrete response instances, which, when they occur, due to their peculiar morphology, are always considered as integrated into a functional pattern. Given that the functional meaning of all psychological behavior is its contact with some object of stimulation, directionality constitutes the fundamental dimension of analysis, upon which other measurement dimensions are complementary. It is important to clarify that, in line with what is stated in **TB**, the first three types of functional contact (coupling, alteration, and comparison) may occur in both non-human and human individuals, supported by conventional and ecological contact media (the physicochemical contact medium is a necessary condition for any type of relation); however, extension and transformation contacts can only occur in human individuals, enabled by a conventional contact medium, and as linguistic-type relations regarding both stimulus objects and RAPs.

Coupling Functional Contacts

Coupling contingencies represent the most general and simplest organization of psychological phenomena because it is found in all individuals who show psychological behavior, from coelenterates to human beings and, in the case of the latter, constitutes the dominant organization of functional contacts from birth up to the first months of life and, subsequently, constitutes the type of contact characteristic of a large part of social interrelations. Coupling contingencies consist of functional contacts in which the individual relates to occurrences in his environment that are independent of its behavior, and which cannot affect but only be affected by them. This may be attributed to two main reasons: one has to do with the fact that the individual does not possess the reactive patterns to intervene in the occurrence-contingencies that take place in his environment; another may be due to the fact that, despite the individual's reactive characteristics, the occurrence-contingencies cannot be altered because they are *imposed* upon the individual: physicochemical natural circumstances, ecological invariants in the organism's own habitat, institutional criteria, specific knowledge of the social group, or adjustment situations. In all these cases, the individual establishes functional contacts that, in one way or another, allow him to adjust to prevailing occurrence-contingencies through analogous reactive/active patterns, or in spatiotemporal correspondence to changes in the environment. Coupling functional contacts are characterized by the individual's *differential* reactive patterns to the temporal and spatial properties of the environment's occurrence-contingencies. During adjustment, changes occur in functional contacts in time and space with the relevant stimulus objects, but prominently, the individual is simultaneously exposed to the dispositional properties of objects which modulate, in every moment, the relevance and functional properties involved in occurrence-contingencies. Because of this, the mediator in coupling functional contacts is the dispositionally relevant object (DRO), a stimulus object that is not necessarily related to consummatory biological behaviors, such as eating or drinking. However, consummatory behaviors may take part in this sort of contact, especially in studies using animal subjects. Temporal parameters of occurrences always refer to their cyclicity, location in an interval, duration, and intermittency, among others. Spatial parameters, however, not only correspond to their location and extension but also encompass the point of contact with particular forms of individual's reactivity (and its body), spatial displacement, as well as reproducing or tracking other individual's reactive patterns (given an ecological or conventional medium), including articulated sounds, movements, and graphisms. In this type of functional contact, temporal and spatial dimensions of the individual's activity must conform to the temporal and spatial dimensions of occurrence-contingencies between objects and stimulus events in the environment. Coupling takes place as a functional segmentation of the individual's activity, in time and in space, in the face of ongoing spatiotemporal relations between objects and stimulus events with relevant dispositional properties. The individual, strictly speaking, must *react* to the circumstances that characterize its environment, circumstances that the individual cannot modify nor alter. Reactions, as an adjustment to circumstances preceding activity, usually include active modes as well. Adjusting to circumstances implies being differential to the conditions of their occurrence, their location, their sequencing, their availability, their correlated changes, and their dispositional relevance in terms of how they can affect the individual, biologically, ecologically, and/or socially. Therefore, coupling always constitute contacts *regarding* objects and events with dispositional properties and the circumstances in which they show relevant functional changes. These contacts can be described in terms of orientation, recognition, tracking, reproduction or repetition, and anticipated exposure to objects and events, which are often other individuals and their activities. In coupling functional contacts, the individual only determines, through his own reactivity, the relative exposure (and the effects that such exposure entails) to different circumstances in an environment of objects and stimulus events. In purely colloquial terms, we could say that the individual is only responsible for its exposure to what may or may not affect him/her. He/she can withdraw himself/herself from circumstances, not approach them, approach them occasionally or constantly, but cannot affect them. He/she can only, to put it in some way, regulate what circumstances may affect him/her and, sometimes, to what degree they do.

Functional Contacts by Alteration

In functional contacts by contingency alteration, the acting individual affects the occurrence of possible contingencies in a situation. Contingency alteration can take place in different ways. One of them is by producing direct changes on stimulus objects, either in their spatial and temporal occurrence circumstances or in their stimulus properties as events in the environment. Another way is by affecting the behavior of other individuals, producing changes in their activity through direct or indirect motor-type reactive patterns, such as when we push or run after someone. A third way, exclusive to human beings, takes place when we produce changes in other individuals and, through them, also in the physical and ecological environment, through reactive/active linguistic patterns. Not all forms of gesturing, speaking, or writing constitute contacts by alteration, nor do all motor or displacement behavior, involving actions with mechanical effects, or manipulative and fine-articulated movements. Contacts by alteration are not defined by the type of activity or behavior performed by the individual, but by the fact that its behavior changes the objects' occurrence circumstances, their stimulus properties, or the events that take place as its consequence. In the case of another individual as a stimulus object or event, the change must always be identified as a change regarding the regular occurrence-contingencies that are imposed in all interrelationships between individuals in society. Therefore, it should be emphasized that, in contacts by alteration, as its own designation indicates, contingencies are altered, that is, functional relations of interdependence between objects, events, and individuals' action/reaction patterns change. Sometimes altering contingencies requires altering objects or the state of the environment in which such contacts take place. However, altering objects or the environment's state is not equivalent to altering contingencies. Mediation in alteration contingencies consists precisely in producing changes in the circumstantial nature of occurrences as a result or effect of an activity directed at a segment of the environment. For this reason, not all the effects resulting from the activity or behavior of an individual imply alterations in occurrence-contingencies. Changes in objects and momentary changes in the behavior of other individuals can occur, without these changes constituting alterations in the conditionalities of those changes as related occurrences. One can drink water from a glass and leave it empty, without altering any occurrence-contingency, in the same way that a child can move a toy car with his hand, without altering any contingency. In one case, the weight of the glass changes and in another the position of the toy, but no occurrence-relations in which the glass or toy participate are changed. Contacts by alteration change occurrence relations between objects and events. They are not producers of changes in the state of an object. In alteration contingencies, the activity or behavior is a component of the occurrence-contingencies between two additional events, in such a way that the relationship between these occurrences is conditional by means of the form, time, and place, among other characteristics, in which such behavior or pattern of activity takes place. Contacts by contingency alteration represent a qualitatively different field organization than those mediated by coupling, and it is so for two reasons. The first is the functional characteristic of altering contingencies: the individual participates in the organization of the field, altering the occurrence-contingencies that take place and consequently contributing to additional dynamic properties of the field state configuration. The second is that the mediation articulated by the individual not only alters the occurrence-contingencies that affect him/her, but can also alter, and normally this happens in ecological and conventional contact media, the occurrence-contingencies that affect other individuals. Such changes lead to a more complex field configuration, with different and simultaneous states in equilibrium, resulting from different functional contacts by different participating individuals. The coexistence of different functional contacts in a single field, in which a contact by alteration takes place, may involve the reactivity/activity pattern (RAP) of one individual, in addition to that of another (others) individual (s) participating in the field.

Functional Comparison Contacts

Comparison-contingencies arise from individuals who are part of gregarious species, although living in a group is not a sufficient condition for this type of functional contact to emerge. Contacts by comparison consist of interrelationships dependent on relational properties, both of segments and dimensions of stimulus, and of components of the individuals' behavior patterns. Through comparison contacts, individuals distinguish between two types of constancies: among absolute changes and among relational changes. In comparison contacts, absolute properties of objects and behavior become functionally related as an outcome of relational correspondence contingencies. Relational properties do not reside *per se* in stimulus objects/events, or in the behaviors of individuals. Relational properties emerge from the individual's discriminative sensorimotor/manipulative and/or linguistic behaviors, which allows setting the relational comparability of properties, irrespectively of their absolute properties or values. In comparison contacts, unlike contacts by coupling and alteration, certain variable conditions are responded to as if they were constant. By relationally varying behavior with respect to absolute properties, the individual behaves with objects/events and the behavior of other individuals as objects relative to other objects, as events relative to other events, and as behaviors relative to other behaviors. In comparison contacts, the interrelations characteristic of contacts by coupling or alteration do not disappear. Absolute constants are necessary in order to compare relational properties. Objects and events do not cease to be differentiated as such but are *also* compared as part of a larger molar stimulus segment, in which only some properties of the objects are functional in the comparative relation. The individual's behavior is also transformed, from specific and discrete components directly related to each object/event, into a molar pattern discriminating fractional properties in objects/events to be relationally compared.

Comparison contacts involve constant relational contingencies and not constant absolute properties of objects and behaviors. Relational contingencies may encompass different types of variations, namely, (a) changes in an object or event (or behavior) maintaining the general property—not particular—as a relational criterion, (b) changes in the particular value of a property between the same or different objects maintaining the relational constancy, and (c) both cases with constant DRO or with changing DRO depending on variations in the relational property. The functionality in comparison-contingencies rests on constancy as a relation by permuting and combining absolute properties capable of varying with respect to each other, either in terms of magnitudes, objects, events, and conjugated behaviors. Comparative (or collative) contingencies should not be confused with constant relations between objects, properties, and absolute actions. In fact, the absolute properties that vary in relation are required to be comparable as relative values on the same continuum, or as a correspondence between two continua. In comparison-contingencies, at least two matches are required, whether successive or simultaneous. A single comparison between two objects and their properties constitutes only a differential contact by coupling.

In comparison contacts, functional detachment occurs both in the stimulus segment and in the reactive/active pattern. First, there is a detachment from the molar properties of the object or event as a differentiable entity, reacting only to relationally comparable micromolar or macromolecular properties, properties that constitute the contact's functional stimulating segment, involving different stimulus objects or events. Secondly, the reactive pattern is detached from particular objects/events and is segmented as a pattern in relation to concurrent properties in those stimulus objects or events. This is why mediation in the comparison contacts resides in the reactive/active pattern that relationally articulates the fractional properties of two or more stimulus objects/events, *combining* and *permutating* them as equivalent relative properties despite their absolute differences. The detachability of linguistic reactivity allows that once the comparative contact has been occurred, in successive interactions with the situation the interaction may take place as coupling or alteration contacts. During the functional detachment process, the contingency field is organized according to permutations of objects and properties adjusting to the relational contingency. Due to continuous dynamic transformations in the functional properties of objects, by permutations and conjugations taking place, relational contingencies in the field may emerge from different segments of stimulation interrelated with behavior patterns. It seems appropriate to describe the field state in terms of the *fission* of its component elements. Fission does not exclusively refer to the functional fractionation of objects and properties, but rather to the structure of the field itself, which is continuously disaggregated and reconstituted based on the resulting new comparable segments.

Extension Functional Contacts

Functional contacts by extending contingencies can only take place between people, human individuals, since the extension of contingencies between situations necessarily requires the functional detachment that linguistic reactive/active patterns allow for. These patterns, insofar as they are functional in two directions, from the one who speaks, writes, or gestures, and the one who listens, reads, or observes, always occur in both directions in all contacts by extension. There is no situational detachment of an isolated person in a situation, except in some special circumstances. Detachment occurs when another person conventionally *makes present* contingencies regarding a different situation, contingencies involved in *circumstances* not present in time, in space, or by direct perception. However, contingencies are *in fact* extended when the referred individual actualizes them by behaving in correspondence. Reference (bringing up) makes present what is not present in the situation, encouraging its inference (bringing into) through linguistic "understanding." Inference consists in actualizing a non-present referred contingency. Contingency's mediation lies in an episode between two personal linguistic patterns, those of the one who refers and those of the one who infers or understands by acting accordingly. Contacts by extension require two linguistic functions as a synchronous mediation episode and usually require two people, one who reports or mentions (brings up) occurrence-contingencies and the other who infers (brings into) function-contingencies, actualizing them by a change in his/her behavior. This double functionality can occur in a single person under special soliloquial circumstances. In extension contacts always "two" participate, either as individuals or as functions, constituting a linguistic episode of reference-inference. The integration of reference and inference as a molar pattern makes obvious sense: no one speaks or writes if not to be heard or read, and no one listens or reads if not because someone else has spoken or written. All reference is directed to someone, either immediately or mediately. Contacts by contingency extension take place, in and during the mediator episode, between referor and referred. Such contacts have extended effects but in the form of other contacts of a situational nature. Once the contingency extension has taken place, the "extended contingency" is integrated as an environment's regular characteristic. All contacts by contingency extension can be characterized as making some circumstance present through language and reacting (the same or another person) to that circumstance *as if* it were present. They do not refer to objects, people, activities, or events. They are not stories about the past or about what happens elsewhere. The "as if" of the relationship is what defines the functional characteristics of the contact. It is an "acting as if" fostered and mediated by the linguistic behavior of two individuals or by the same individual. Contacts by extension take place on a double functional level. A first extension takes place when a situational circumstance P (past or possible) becomes present in a situational circumstance C (current), and the mediating reference-inference episode is completed. A second extension takes place when the occurrence of the mediating episode actualizes different contingencies in C or N (other situational circumstances), upon which both participants will show patterns consistent with their interaction in the mediating episode. This second type of extension does not need to occur immediately, since, except in situational circumstance C, the other situational circumstances are distant in time and space. However, given the detachable and detached nature of the linguistic reactive/active pattern of contingency extension, these situational circumstances are configured as situational contingencies, whose components already show functional properties recognized in advance. The mediation process actualizes these contingencies as contingencies detached from absent situational circumstances, or from those not yet present. Therefore, a double process of detachment is established, one that takes place first while mediation occurs as a reference-inference episode, and the second that occurs later as the reactive/active pattern extended in other situational circumstances or in situation C itself, actualized as a distinct contingency circumstance. The referring-inferring pair never acts in the past, but in the present, first in the mediating episode, and later through linguistic recognition (operation rules) of circumstantial contingencies in other situations. Both types of detachment, always in the present, can only take place as conventional reactive/active patterns detachable, in principle, from the situational properties of objects, events, people, and activities.

Transformation Functional Contacts

Transformation contingency contacts represent the most complex organization of psychological phenomena and consist of interrelations between purely linguistic episodes. Transformation contacts are transitional episodes, usually long, consisting of talking or writing about how one speaks or writes in referential practices. They are reflexive linguistic episodes about the referential practice itself, and, therefore, they occur without reference to any situation, although they always occur in situation. Functional contacts by contingency transformation are transitional. Nothing is spoken or written in relation to particular situations. One speaks and writes about speaking and writing or, more precisely, about how one speaks (or writes) when one speaks (or writes) about something, including speaking or writing (e.g., reporting what was said or written).

This characteristic of contingency transformation contacts dilutes the individual in reflexive linguistic practices, not superimposed, but juxtaposed to the referential practice, so that boundaries between the individual and the stimulating objects become blurred. A reflexive episode does not entail two types of simultaneous linguistic patterns, one, the reflexive pattern, and another, the referential pattern regarding which the reflection occurs. Only one pattern occurs, the reflexive, whose stimulus object is the domain of referential patterns whose functionality is spoken, read, listened, or written about. The purely linguistic individual (from a functional point of view) unfolds itself synchronously in its activity and its effects, in the reactive/active pattern as behavior and in the conventional objects and events that result simultaneously. These conventional objects and events are the stimulus objects of reflexive reactive/active patterns. Referential and reflexive patterns never occur at the same time, but juxtaposed, as functional segments integrated into an episode. Reflexive patterns take place interspersed between referential patterns. Transformation contacts usually take place as prolonged transitional episodes, of a discontinuous oscillatory nature, and are not replicable. They take place only once. Contacts by contingency transformation could not take place without the availability of reactive systems in the modes corresponding to reading and writing. Transitional detachment, which characterizes these contacts, starts from the possibility of detaching oneself from linguistic behavior as an activity, and of relating to it as conventional stimulus objects. Transformation contacts constitute contacts between domains and/or subdomains of linguistic practices, in which contact does not take place as reactive/active patterns with a referential character, but rather people talk (or write) about how they talk and write about something in a given practical domain. They are made up of reflexive patterns to highlight that language is the only form of behavior with this property, that is, linguistic behavior can occur with respect to linguistic behavior itself. We can talk about talking but we cannot see our vision or move our movement.

Linguistic practices only make sense as parts of a functionally articulated set in a social domain. Words and expressions only make sense relative to other words and expressions *and constitutive practices*. For this reason, the linguistic reactive/active patterns that make up the practices in a functional domain always constitute patterns in an internal relation to others in that domain. These internal or mutual relationships are those that identify and "determine" their relative functionality in a certain domain. In contacts by transformation, the internal or within contingency relations between different language segments of a practical domain are changed. The transformation of the linguistic practices of a functional domain always involves the reorganization of relations that give meaning to its segments, as components that are interdependent of each other. Contacts by contingency transformation can be more appropriately described as a colloquium, that is, speaking impersonally about a topic or issue. The topics or issues are the referential practices in a domain and how they can be reorganized functionally in a domain or in relation to another domain, giving rise to new referential practices in those domains. It is not a matter of reorganizing events, things, or their properties, but rather the whole system of referential practices by which the various functional contacts in or between situations take place. The transformation consists of detaching a set of patterns from their internal relations, linking them to other patterns and to their corresponding practical domains. In transformation contacts, the field is not constituted by objects (natural or conventional), singular events and activities, but by domains or subdomains of relations between sets of referential linguistic practices. The result of a contact by transformation is a new domain with its corresponding operation rules, which is added to the domains or subdomains prior to the transformation. Detachment in the transformation contacts is a gradual, apparently discontinuous, asymmetric in speed, recursive at times, oscillatory in vigor, and a multidirectional process. Detachment starts with the identification of functional discrepancies within the referential reactive/active patterns, in some of the situations belonging to a given domain. A second stage has to do with a process in the form of a colloquium, that is, the occurrence and emergence of reactive/active reflexive patterns regarding regions of that domain, or regions between different functional domains. The transformation of the contingencies that support the relationships between patterns and practices always entails a change in the operation rules of the entire domain. Practices that used to make sense stop to after the transformation process. The domain, partially or totally, changes its functionality in accordance with the new criteria that identify the limits and relevance of possible practices, not as isolated behaviors, but as acts in interrelation with others. The practical domains vary in their nature, and this will determine the peculiarity and collective impact of the transformation process, which always constitutes an individual functional contact. Mediation takes place as a process of inquiry or search of other ways of “seeing” one’s own practice and/or that of others in a domain. This inquisitive process is nothing more than a colloquium in which one explores how to speak (or write) reflectively about one’s own referential practice in that domain. The prolonged character of the molar reorganization of the relationships between referential practices, based on how we reflexively talk about them, privileges the written mode as a way of keeping “present” the partial adjustments that occurred in the transformation process. Furthermore, writing and reading what is written while writing is the only exclusively linguistic episode taking place, setting apart the presence of situational circumstances that can “interfere” with the reflexive nature of the transformation contact. Writing also provides the recursive characteristic of the transformation process, which is not a simple recurrence, but a recurrence regarding the last transformed state to start a new stage of transformation. Recurrence regarding the last transformed state means that the partial states in a transformation contact are not repetitive, but conditions of momentary interruption of the process that, when restarted, give it its recursive character. Recursiveness implies recurrence, not as repetition, but as continuing a pattern of recurrences starting from the point where the mediation-detachment process was provisionally interrupted. Each partial state of a reflexive detachment is the new state from which a new reflexive mediation begins, until the final adjustment is reached as a transformation regarding operation rules of a complete practical domain or subdomain. When the transformation of referential practices is concluded, as a change in the criteria that relate them in circumstances, their functionality changes. Practices become relevant in situations in which they were not, and cease to be so in others where they were: one acts similarly in the face of what was previously different, and acts differently in the face of what was previously similar.

Table 19.1 describes the main characteristics of each of the functional contacts examined. For reasons of space, this description has not been detailed, but in the mentioned table the forms of mediation, field states, functional detachment, type of interactions, and adjustment criteria can be identified. At the present time, experiments are being carried out or data are being analyzed in terms of the proposed mediation and detachment relations, based on the general molar measurements, and supplemented by the analysis of nonlinear dynamic systems to identify the states and transitions of the field as spaces of states and recurrence patterns. We trust that in a couple of years the first studies carried out and examined from this new perspective will appear.

Extensions of the Field Model

The field model, as a general process theory, includes the study of universal relations and the conditions in which they take place. In the case of psychology, it is necessary to extend its logic to two domains: one, internal to the discipline, which is the case of individuation as development ("becoming") and, another, to its intersections with other scientific disciplines as multidiscipline, or with professional fields of application of knowledge as interdiscipline.

Intradisciplinary Extension

The intradisciplinary extension is directly linked to two traditional psychological topics: development (becoming) and individual differences. These two topics have, incorrectly, served as a direct foundation for theories of psychology unrelated to the analysis of general processes under experimental conditions. In **TP**, both topics have been examined as part of the process of psychological individuation, that is, how a newborn becomes a psychologically unique individual, who shares, at the same time, the same general circumstances and processes as their peers or conspecifics. The study of individuation has three aspects: the first, as a process of development, that is, *becoming* an individual in an aggregation or a group, whether ecological or social; the second, related to the conformation of individual behavioral styles, which prefigure their initial contacts with various types of contingency situations; and, the third, concerning the comparative study of the processes of individuation,

Table 19.1 Characteristics of the functional contacts

Contacts	Mediation	Field configuration	Adjustment criteria	Type of interaction	Detachment (functional segmentation)
Coupling	Isomorphism	Molding	Differentiality	Correlation	Stimulative pattern segmentation
Alteration	Operation	Amalgamation	Effectiveness	Dependence	Reactive/active pattern segmentation
Comparison	Permutation	Fission	Precision	Interchangeability	Conjugated stimulative and reactive dimensions segmentation
Extension	Transitivity	Elasticity	Congruence	Correspondence	Situational segmentation
Transformation	Reflexivity	Fusion	Coherence	Pertinence	Domain segmentation

in the context of species-ecological niche and cultural group-social formation relationships. I will very briefly review these three strands of individuation.

The concept of individual only makes sense as part of a group or aggregation. If there were no aggregations, the term "individual" would not even exist. Being an individual means having an identity and participating differentially in the activities that constitute the practice of an aggregation. It makes no sense referring to an individual, without differentiality in its recognition and in its participation in common or shared activities. Consequently, being an individual is the result of belonging to an aggregation and the criteria with which it identifies and participates. For this reason, the individual as an aggregation's functional unit is shaped as such through the action of all the individual members, a process that, by remaining indefinitely, allows maintaining the relationships between individuals that characterize the aggregation as a whole, entity or system. I will focus on the analysis of human individuation, assuming that some of its functional moments can also be extended to other animal species, but not all. The latter means that although psychological behavior can be predicated on all species on the animal scale, this is not the case with the process of psychological individuation.

Psychological individuation takes place throughout development or becoming, which begins at birth and ends with death. It is a continuous, dynamic process, comprising different domains related to survival in ecological relations, and coexistence in social relations. We will examine the process of human individuation based on what we have called the "attachment rhombus," described in Fig. 19.1. The attachment rhombus assumes an initial relation of biological dependence. In the newborn it is a biological dependency, linked to survival. Subsequently, these dependency relations become functional in their nature and may continue to be related to survival at the ecological level, or with different forms of living together in the case of human beings. The nature of the dependency changes towards functional dimensions of group practices, encouraging the individual to incorporate into them and to participate in a pertinent way. Figure 19.1 schematically describes a set of relations that are repeated in the diachrony of development and that branch off into different domains based on the contingencies that define them in their corresponding

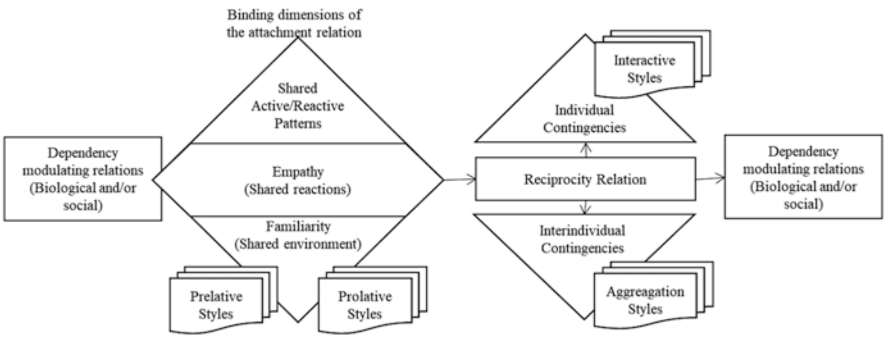

Fig. 19.1 Components of the individuation process

ecological and social environments and media. The attachment relations described do not refer, as might be incorrectly assumed, to special affective ties, although they do include what is usually conceptualized as empathy and apathy. Attachment relations constitute, as the term itself suggests, functional links, bidirectional ties between the individual and his environment and group of reference. These attachment relations *always* manifest themselves on three levels as functional relations with other individuals in the group and with characteristic objects and events of their environment. Attachment relations are shaped (a) by the establishment of shared affective reactions among individuals in the group (empathy and apathy), (b) the development of reactive systems and common functional practices (predominantly the natural language and the various technical languages in human beings), and (c) the establishment of withdrawal and approach patterns regarding other individuals, of the same or of a different species, and objects and events (familiarity and threat). These three segments of attachment relations allow identifying *other* functionally significant individuals in the group and in the environment, "others" who can be classified as their own, equal, different, and not others, differentiating the relations that can be established with each one of them.

In the case of human individuation, this process begins even before birth by the parents and relatives' willingness to incorporate the newborn into their family and cultural group. Because of this, the newborn is given a *name* that identifies and distinguishes it beforehand. The name, as a criterion for social identity, facilitates its differentiation as an individual (person) within the reference group. In the case of animals, their appearance, smell, specific sounds, and other characteristics also contribute to set identification criteria. The human neonate (and we suppose a similar process, although peculiar in every other species) initially recognizes its environment and caregivers through relations of biological dependence of feeding, cleaning, and protecting itself from changes and environmental factors, and other aspects related to survival. Sounds, smells, physical contact, tastes, and the first forms of biological contact constitute the basis upon which the neonate's attachment relationships towards its peers begin to be established (Gewirtz, 1972; Rheingold, 1963). Attachment relationships, as can be seen, are bidirectional and asymmetric. We assume that only through biological dependence relations can attachment relationships initially develop and, consequently, a process of psychological individuation. This occurs only in those species, predominantly in the classes of birds and mammals, in which the neonate requires at the time of birth, and for a time, the protection and care of adults of its corresponding aggregation. It can be assumed, based on this reasoning, that although the occurrence of psychological behavior can be predicted throughout the animal kingdom, it is likely that only in the classes of birds and mammals that the process of psychological individuation occurs, as an outcome of ecological and social processes.

Attachment relationships come together in the possibility of establishing reciprocity relations between individuals in an aggregation. Reciprocity relations must be distinguished from mutuality relationships. The latter are those that characterize the attachment process at any specific moment during development, not only in its beginnings, and which are shared with many species at the ecological level, as an evolutionary concretion of a direct or indirect symbiosis of all living organisms (Kropotkin, 1902; Margulis et al., 2000). Reciprocity relations are always episodic and between identified individuals (Ávila, 2017; Rangel et al., 2015; Ribes et al., 2010; Ribes, 2018). They are direct interactive relations that constitute the functional sustenance of exchange relations at the social level, as well as the occurrence of affective, playful, and agonist interactions. They can be additive, subtractive, or indifferent, and the conformation of patterns in interaction (RAP) will depend directly on the functional characteristics and contingencies prevailing in the corresponding domain. Parallel and subsequently to the establishment of attachment and reciprocity relations, two forms of differentiation of individuals' behavior are developed that determine their interactive uniqueness at the psychological level. These characteristics (which partially have to do with individual differences, in this case of so-called personality) correspond to behavioral styles. There are two groups of behavioral styles: those that are conformed in the absence of explicit contingencies and those that are the biographical result of consistent ways of interacting with explicit contingencies. The latter constitute interactive styles, facing two different types of contingencies: ecological contingencies and aggregation contingencies. The former begins to develop in the first stage of attachment relationships and constitute what we have called prelative and prolative styles.

Ecological interactive styles are identified as individual consistencies in the way in which an individual interacts with situations under open contingency conditions, that is, in which predetermined adjustment criteria are not established and, therefore, no specific way of behaving is established. Interactive styles are behavioral consistencies over time and in different situations with the same contingency organization. These consistencies are identified as functional behavior profiles along a gradient of contingencies that structure each interactive situation. Eight contingency situations have been experimentally studied in which interactive styles have been identified (Martínez, 2017; Ribes & Sánchez, 1992; Ribes & Contreras, 2007; Ribes et al., 2005). These situations consist of contingencies of conflict, risk, decisions, achievement persistence, ambiguity, comparison, scanning, and frustration. In all these cases, interactive styles occur under open contingencies, as explicit within-subject individual consistencies (and as differences between individuals). When contingencies are closed, that is, explicit behavioral requirements are established, differences between individuals disappear and within-subject consistencies are masked by present contingency requirements. Figure 19.2 shows the interactive profile of two different individuals, in the same situation, a risk contingency. Two evaluations under open contingencies were carried out with a one-year interval between them and using, in one case, changes between blocks of sessions and, in the other, changes between blocks within sessions. The form of the polynomial regression function was similar for each one of the participants at the two different times. Complete data can be found in Ribes and Sánchez (1992). The styles under aggregation contingencies are still in a first stage of experimental evaluation. The same occurs with the prelative and prolative behavioral styles. These, however, as already mentioned, only occur in the absence of contingencies, that is, in situations without structured contingencies, and it is the behavior of the individual that shapes them

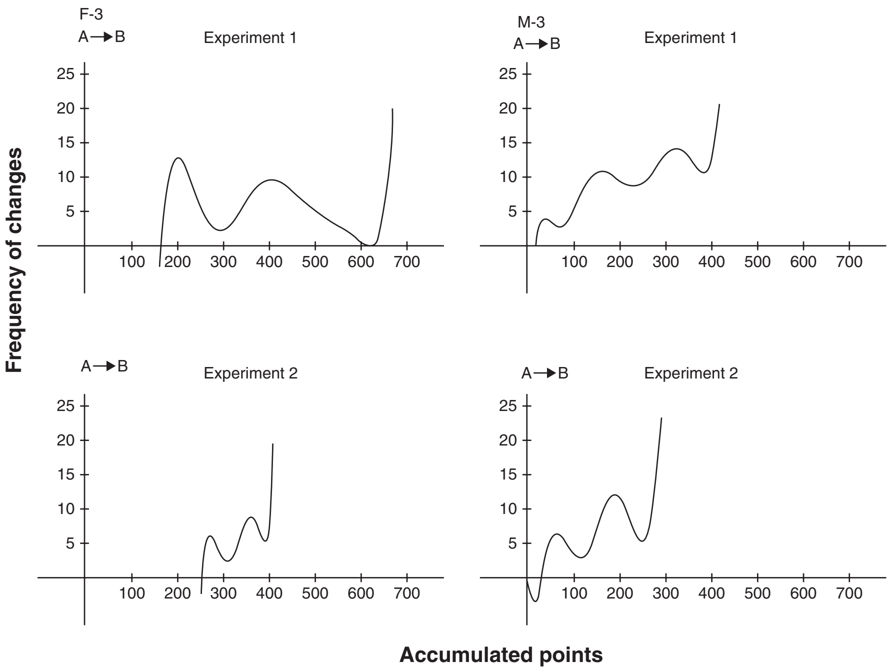

Fig. 19.2 Functional profiles for risk contingencies of two individuals. Legend: This figure shows the functional profiles for risk contingencies of two individuals in evaluations with an interval of 11–12 months. Risk is described by means of a polynomial regression with 9 degrees of freedom, showing the covariation between changes in options visited and the number of accumulated points

based on the consistent way in which, throughout his functional biography, has modulated the environment's texture and the preferences derived from contacts with such changes in texture (Gibson, 1979). There are already some preliminary results that support the possibility of identifying individual consistencies in the way of functional segmentation of the environment and the development of preference gradients regarding the modalities and properties of objects and events.

Differences examined as behavioral styles should not be confused with differences in "capacities" or "abilities," which are the result of asymmetric exposure to training and educational conditions. These differences, at least in principle, should be conceived to be susceptible of being canceled or leveled by appropriate procedures, if there are no biological deficiencies in the reactive systems. The process of psychological individuation foresees differential courses and outcomes for each individual, since even under formally identical contingencies for different individuals in the same environment, the circumstantial nature of the contacts taking place makes it impossible that these contingencies functionally operate in an equivalent manner for everyone. Nevertheless, contingencies operate in a restricted range of variation that, even when they are not "identical" for every individual in the same environment or situation, they ensure functional similarities in the interactions in which they all participate. The asymmetrical contacts of individuals with restricted *band* of contingencies explains why the development of the individuals sharing the same habitat show asymmetries in the different domains of functional interaction, and why it makes no sense to propose universal stages of homogeneous "development."

A last pertinent point regarding psychological becoming is the analysis of the emergence of the various functional contacts in individuals, resulting from the various contingencies that characterize the specific environments and groups of which they take part. **TP** does not assume, as already mentioned, universal and progressive stages identified by types of functional contact. However, it is assumed that certain types of functional contact, *specific* to each domain, may be necessary for other functional contacts to take place. However, this is an empirical and not a theoretical problem. It is a matter that must be observationally and experimentally examined, according to the possibilities of each case. In fact, the comparison of domains, functional contingencies, and reactive systems constitutes the core of the study of psychological development or becoming as individuation. Functional comparisons between species, reactive systems, domains, and contingency situations (ecological niches and cultures) represent the fundamental challenge of the study of becoming as comparative psychology. Until now, unfortunately, these analyses have been based on comparing performances under operationally similar procedures and measurement instruments.

Multidisciplinary and Interdisciplinary Extensions

Due to the very nature of its subject matter, psychology has historically met difficulties in delimiting its field with respect to those of the biological sciences and the historical social sciences, as well as with professional disciplines as education, medicine, and so on. These problems are bound both with the models, categories, and problems studied, and with the deficient definition of psychology as a discipline or interdiscipline, that is, a professional field of application. Consequently, in the case of psychology's relation to biology and social science, confusion in both directions has prevailed regarding the nature of the problems to be studied and the categories and methods to be used. While, for example, biology erroneously sets out to study the cerebral determinants of learning, thinking, memory, emotions, and other supposed psychological processes, psychology endorses concepts and models of physiology such as the reflex and imagined neural networks, among others, or empirical problems such as feeding behavior and agonistic behaviors in species. In the case of the social sciences, there are equivalent phenomena of reductionism, incorporating economic models to define or explain supposed psychological phenomena or, on the contrary, accounting for social phenomena in compositional terms by the subjective "construction" of the so-called social imaginary. Regarding professional fields, boundary conflicts and the fuzzy participation of psychology in education, health, administration, and others stand out. To the extent that in **TP** psychology's subject matter is clearly specified, it is possible to clear out the way in which psychology relates bidirectionally with other scientific disciplines and how it participates in the practical solution of social problems.

Multidisciplinary relations consist in complementary forms of collaboration between two scientific disciplines with juxtaposed empirical fields. This complementation may occur in two ways, and this determines how the multidisciplinary field is recognized. One of the disciplines raises the problems to be investigated and their theoretical foundation, while the other provides methodological resources to analyze some aspects of this problem in specific ways. In this manner, the prefix of the multidisciplinary field is identified with the theoretical discipline and the suffix, so to speak, with the discipline that complements it methodologically. In the case of psychology, its fundamental empirical juxtapositions are related to biology and sociohistorical science. Multidisciplinary fields will be identified as psychobiology or psychosociology when the neighboring disciplines provide methodologies to examine and analyze problems that are framed in psychological theory. On the other hand, multidisciplinary fields will be identified as biopsychology or sociopsychology, when the role of psychology is to contribute with analytical tools to the study of theoretical problems posed by biology and sociohistorical science. Multidisciplinary fields are not new sciences or disciplines, as is often wrongly suggested. They are always relationships between two disciplines, with complementary contributions between them. For instance, if we carefully reflect on the process of individuation, it can be quickly inferred that its experimental and observational study inevitably involves relations of a psychobiological type, when specifying characteristics and properties of the individual's ecological niche, or of a psychosociological type, when the specificity of the individual's cultural and social habitat has to be addressed. The neighboring disciplines allow strengthening the ecological or social validity (external validity) of the experimental preparations and methodologies used. In the same way, all comparative studies of individual behavior in ecological niches (including different species) or different cultural and institutional environments can be considered. The use of complementary observational methodologies (assessment or measurement instruments) is also included, such as molecular analysis of reactive systems based on electro-chemical-physiological techniques, or techniques to establish criteria for linguistic or social differentiation. In the cases of biopsychology and sociopsychology, there are outstanding examples in history: behavioral pharmacology is perhaps the most relevant in recent times, although not the only one. Pavlov's original work to explore the functional properties of the central nervous system using classical conditioning is another paradigmatic example. Sociopsychology (Ribes et al., 2016) seeks a systematic approach to the study of interindividual relationships within the framework of formal and informal institutional processes in sociohistorical science. At this point, it is worth to briefly mention that so-called social psychology and evolutionary psychology are clear examples of confusion regarding the problems specific to psychology and those belonging to the fields of sociohistorical science and biology, respectively, where models and categories of these disciplines are introduced *as if* they were characteristic of psychology.

Regarding psychology's interdisciplinary relations, it is important to point out that interdisciplines are constituted as mixed fields of knowledge application at the social level. They are not sources of knowledge but social geographies of problems that delimit, define, and direct the participation of those disciplines that can contribute to their solution. To pose psychology's potential interdisciplinary relations begins by establishing, precisely, that psychology is not a profession, but a science. To that extent, what should be examined which are the criteria and how can psychology's scientific knowledge be applied when participating in expressly professional fields. In interdisciplines, the field of application is defined as an institutionally delimited social problem, and it is not formulated based on a scientific discipline. On the contrary, scientific disciplines must adapt and adjust their knowledge to be pertinent and applicable to the interdisciplinary field. Technological and scientific disciplines, as well as artisan practices and traditional practical knowledge, also concur in the conformation of interdisciplines. Interdisciplinary fields are made up of professional disciplines. Examples are the field of health with professional disciplines such as medicine, dentistry, nursing, veterinary medicine, sanitary engineering, and traditional practices related to herbalism, and acupuncture, among others. The panorama is similar in the case of education, with the presence of pedagogy, different specialties of preschool, elementary, middle, and high school, university and technological education, audiovisual technologies, computer technologies, and architecture, to mention only a few.

Obviously, psychology also participates in these two interdisciplinary fields. However, from **TP**'s perspective, psychology is not a discipline, whose natural field is "applied psychology." On the contrary, psychology is not a profession in itself: there are no "psychological" problems in society equivalent to problems related to health, education, communication, housing, and security, among others. So-called psychological problems are personal or, at best, interpersonal problems and belong to the field of moral criteria. Psychology is an empirical science, and to that extent, its concepts are not formulated for the application of knowledge, but to understand and explain psychological phenomena. Contrary to accepted goals by behaviorists, science does not deal with control and prediction. These are goals inherent to technology and actuarial disciplines. Formulations developed by scientific theories and methodologies cannot be *directly* transferred to the natural situations in which phenomena occur (Deitz, 1978; Ribes, 1977, 1982). I have previously commented (Ribes, 2004a, 2004b) the case of applied behavior analysis as an example of assuming, incorrectly, that scientific knowledge and technological applications are similar. In the case of interdisciplinary relations, psychology must *initially* meet the criteria set by institutions regarding a social situation, even though later, like all other participating disciplines, may promote changes in the conception and solution of problems. The first step is identifying the psychological dimension of the situation to be intervened. This means analyzing the problem's functional segments in which psychological behavior is relevant, as relations between an individual and circumstances.

In the fields of education and health, the social problem is delimited and defined by the school and the health institution, respectively. The psychological dimensions in these domains have to do with *learning*, on the one hand, and with disease *risk*, *prevention*, and *amelioration*, on the other. In both cases, the participation of psychology is limited to the analysis, design, and intervention with respect to the circumstances that promote learning by individuals (and to that extent the teaching conditions involved as well), and those that prevent and reduce morbidity and mortality (including life practices and institutional care). In other words, psychology intervenes in these fields, evaluating, designing, and promoting, through direct professionals, optimal circumstances of learning conditions and the prevention and amelioration of disease or illness, so that, being a problem of collective nature, interventions can be established that cover everyone in general and each case in particular. Due to space limitations, the approach proposed by **TP** for these interdisciplinary interventions will not be described, but two schemes are presented that briefly describe them (Figs. 19.3 and 19.4). The pertinent writings can be consulted for a better understanding and a more detailed description (Ribes, 2008a, 2008b, 2018).

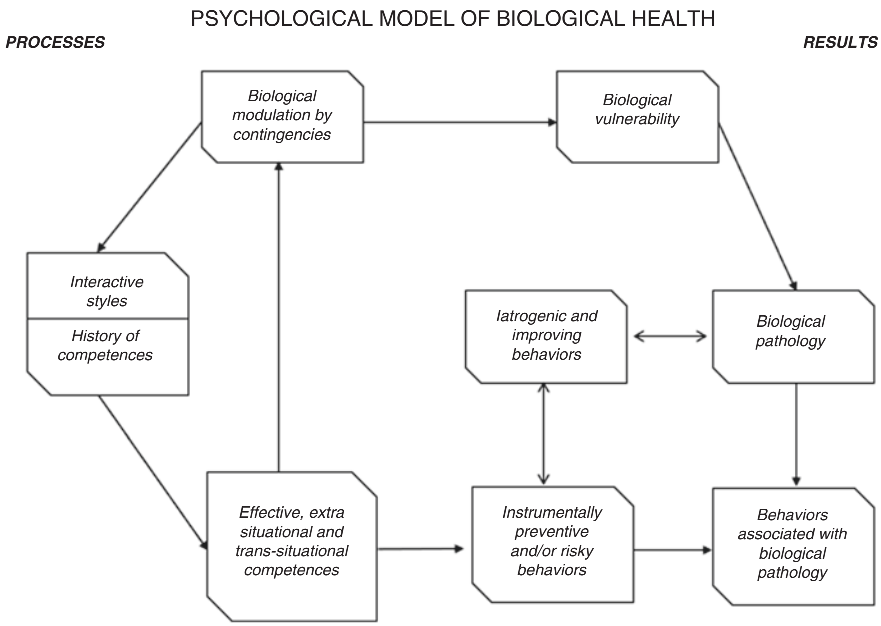

Fig. 19.3 Psychological model of biological health. Legend: This figure describes the factors involved in the individual's relations with circumstances that prevent or promote illness

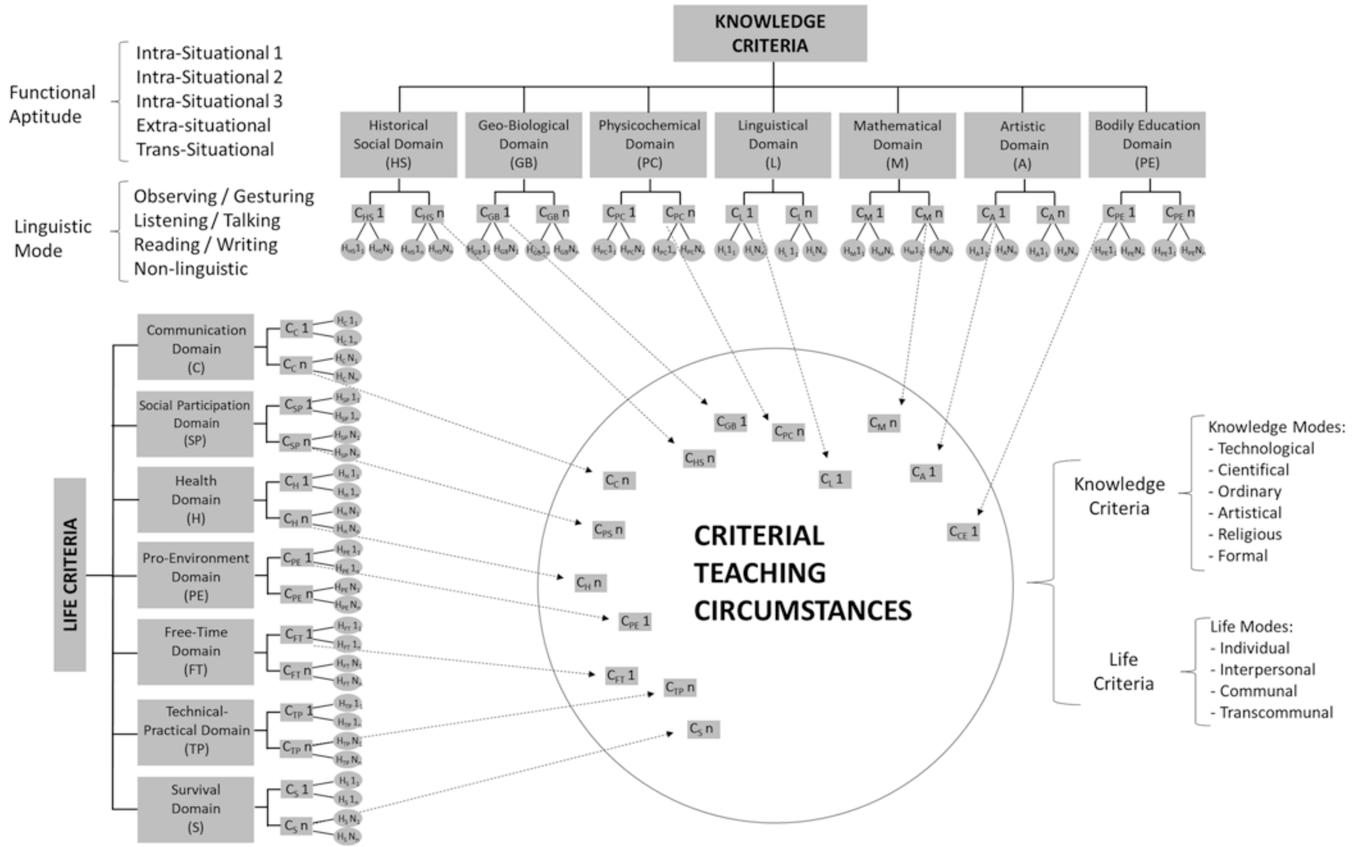

Fig. 19.4 Analysis of functional competences as a methodology for psychological intervention in school-based learning processes

Final Comments

Scientific theories and their extensions to different areas of knowledge and life do not constitute self-contained, immune, or independent systems from other social practices. Scientific theories are part of the institutional doings of social formations, and, to that extent, they are not independent of the interests and power relations that shape and characterize them (Kantor, 1963-1969; Ribes, 1986). Therefore, although we may trace the internal history of the logic, including assumptions and categories, of scientific theories, this logical plot is not independent of the historical circumstances of the social formation in which its changes and development take place. **TP** discusses some of these relations between psychological concepts and the roles they played in different historical circumstances in different social formations. The relations between psychology and social formation institutions, especially the State, are initially examined as part of the natural history of psychological concepts centered on a nuclear category: the individual. This analysis comprises three different aspects, which will be briefly described.

The first has to do with the problem of knowledge itself as a way of interrelating human collectivity with its environment and circumstances. The emergence of different modes of knowledge from ordinary language practices as the fundamental knowledge practices of all human activity is examined. The development of the different modes of knowledge (artistic, religious, ethical/juristic, formal, scientific, and technological) did not take place in a divergent and linear way, but many of them were intertwined in different ways throughout history, until recently achieving their differentiation and apparent autonomy. Each mode of knowledge is distinguished by its purposes and social validation criteria. However, in the practice of these institutional modes of knowledge, individual episodes take place in circumstance, which we may distinguish as modes of *knowing* (not of knowledge.). We suppose that, being part of the ordinary modes of knowledge, individual modes historically preceded the institutional modes. It must be emphasized, to avoid an incorrect interpretation, that individual episodes in ordinary language always occur *between individuals* and not in isolated individuals. Furthermore, we assume that the institutional criteria that delimit the "validity" of each mode of knowledge are, in fact, abstractions of each of the modes of knowing as individual episodes. This analysis presents an alternative perspective to traditional epistemologies (López-Valadez, 2017). We consider knowledge to be a social—not individual—practice, diversified by different criteria and goals, founded and based on social relations articulated in ordinary language, and that dynamically interweaves the interindividual segments of its exercise with the impersonal criteria of an institutional nature, mutually affecting each other in a complex way.

A second aspect has to do with the emergence of the concept of "individual" as a unit of social formations. This aspect is important for two reasons. One has to do with the initial occurrence of "psychological" episodes in the field of ordinary language and, therefore, the historical emergence of psychological phenomenology in the field of social life that defines *Homo sapiens* (HS) as a species. HS, unlike what current Neo-Darwinism implies, did not emerge as a privileged mutation, with a brain designed (who knows by whom) to gesticulate, speak, and associate individually with others to build a social organization. It gradually differentiated itself from other species of the same *Homo* genus, due to circumstances fostered by its life in society and the social division of labor, as a specialized form of living together, and it is under these circumstances that social practice as language emerged. Language and social division of labor, as deferred exchange, emerged at the same historical moment, and with them HS and probably the *Homo neanderthalensis* (HN) and other species of the *Homo* genus as well. The social differentiation required for the recognition of the "individual" can be located in the transition from contributive exchange communities to the first forms of non-equivalent retributive exchange, in which social classes were configured as State's segmentations (usually recognized as social formations with an *Oriental* or *Despotic* mode of production). It is with social class differentiation that the "individual" appears as responsible before the State for the proper functioning of the social division of labor and the corresponding segmental obligations. This historical fact is important because only when the "individual" is recognized as such in social practice, can episodes occur in which the functional reference has to do with individuals as such, whether in the form of self-reference or as a reference to others. Only some of these referential episodes would correspond to what we would recognize as "psychological" episodes and would constitute a differentiation of self-referred collective practices in the form of practices episodically referred to individuals. This approach assumes that psychological phenomenology is a segmentation of social practice in ordinary language and, therefore, that psychological phenomenology did not arise simultaneously with the biological differentiation of the HS species. Concomitantly, history shows how a social ideology was built regarding the individual as a social unit and his responsibility against established laws. This ideology, jointly formed in religious and political practice, was articulated through different root-metaphors in which the individual was endowed with faculties and responsibilities, which partially reflected the powers of divinity and the State, metaphors that still underlie many political, religious, and knowledge practices in today's world. Knowing and examining these metaphors is a necessary task to understand the historical development of psychology's subject matter and the way in which it has been inserted into social life in different ways, intertwined with other disciplines that have been similarly influenced. **TP** has developed an analysis of some of these root-metaphors, including those that have been provided with a "scientific" varnish through their medicalization as a social ideology.

Finally, a third aspect has to do with how psychology can help to locate individual relations in the field of ideological practices, nailed down in the form of beliefs and moral criteria. Obviously, this aspect is critical not only to understand how individuals are inserted and articulated in the constitutive dimensions of social practices, but it is also essential to assess the relevance and justification of the interdisciplinary interventions of psychology. Psychology's applications, like any other form of socially applied knowledge, are not neutral and, in the case of our discipline, have important implications regarding the so-called "clinical" applications and the analysis of social values. Social ideologies are not only “abstract” systems that support world and life conceptions, but, mainly, they are real practices in society whose function is domination in the form of the hegemony of one social class over the others. Ideologies are embodied as beliefs regarding the role and social function of every individual. The dominated classes accept the beliefs imposed by the dominant classes as facts articulated in their own practice and which, in principle, are presented as “natural” and incontrovertible. Beliefs are not determinants of practices, but the consubstantial result of them. To change beliefs, practices must change. Similarly, we may look at moral criteria in two ways. One is that of the individual who experiences moral feelings, of well-being or discomfort, because of anticipating participation in an act, of performing it, or of having performed it. Moral feelings are not a system of “rational” justifications. The latter correspond to the institutions, formal and impersonal, that value and justify or condemn the actions in question. But morality always corresponds to the individual, to the feeling that gives meaning to what he does or does not do, and has nothing to do with more or less universal norms that rationally justify, based on some principles which transcend every act, the “goodness” or “badness” of what has been done. Moral feelings are shaped in the process of individuation as part of attachment relations (empathy and apathy) and, in that sense, their origin and life course can be traced in that process.

I hope this exposition provides the necessary clues to understand the meaning of the chapter’s title. It is not a matter of exposing a biased way of understanding psychological phenomena, better or worse than the others, but of formulating the discipline’s specific and proper system, which contributes to its delimitation and relationship with other sciences. Accounts for how its subject matter can be built from practices in the real world, and how we may go back to that world to better interpret it and promote valued changes. Perhaps the moment has arrived that we no longer need to adjectivize behaviorisms and to propose a general theory of psychology, without “isms.”

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References

Ávila, A. (2017). *La reciprocidad en humanos: Análisis conceptual y experimental*. Doctoral Thesis, Universidad Veracruzana.

Brentano, F. (1874/1924–1925). *Psychologie vom empirischen standpunkt*. Vols. 1 y 2 (Edited by O. Kraus). Leipzig: Meiner.

Cabrer, F., Daza, B. C., & Ribes, E. (1975). Teoría de la conducta: ¿Nuevos conceptos o nuevos parámetros? *Revista Mexicana de Análisis de la Conducta, 1*, 191–212.

Darwin, C. R. (1871). *The descent of man and selection ibn relation to sex*. John Murray.

Darwin, C. R. (1872). *The expression of the emotions in man and animals*. John Murray.

Darwin, C. R. (1974). *Metaphysics, materialism, and the evolution of mind; Early writings Of Charles Darwin. Transcripts and comments by Paul H. Barrett with a comment by Howard E. Gruber*. University of Chicago Press.
Deitz, S. M. (1978). Current status of applied behavior analysis: Science versus technology. *American Psychologist, 33*, 805–814.
Ferster, C. B., & Skinner, B. F. (1957). *Schedules of reinforcement*. Appleton Century Crofts.
Gewirtz, J. L. (1972). *Attachment and dependency*. Winston & Sons.
Gibson, J. J. (1979). *The ecological approach to visual perception*. Erlbaum.
Kantor, J. R. (1924-1926). *Principles of psychology*. Alfred Knopf.
Kantor, J. R. (1963-1969). *The scientific evolution of psychology*. Principia Press.
Kropotkin, P. (1902). *Mutual aid: A factor of evolution*. W. Heinemann.
López-Valadez, F. (2017). *Cultura, individuo, y juegos de lenguaje: Una aproximación naturalista al conocimiento*. Universidad Veracruzana.
Margulis, L., Matthews, C., & Haselton, A. (2000). *Environmental evolution*. The MIT Press.
Martínez, D. (2017). *Un estudio experimental de la individualidad: Los estilos interactivos*. Doctoral Thesis, Universidad Veracruzana.
Miller, N. E. (1948). Studies of fear as an acquired drive. I. Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *Journal of Experimental Psychology, 38*, 89–101.
Pérez-Almonacid, R., & Gómez, A. D. (2014). *Emilio Ribes Iñesta: Una historia de proyectos institucionales de identidad disciplinar e innovación educativa*. Universidad Veracruzana.
Rangel, N., Pulido, E., Ávila, A., Ordoñez, S., & Ribes, E. (2015). Partial-altruistic Interactions as a function of reciprocity induction and written declarations. *European Journal of Behavior Analysis, 16*, 31–48.
Rheingold, H. L. (1963). *Maternal behavior in mammals*. Wiley.
Ribes, E. (1977). Relationship among behavior theory, experimental research, and behavior modification techniques. *The Psychological Record, 27*, 417–424.
Ribes, E. (1986). Historia de la psicología ¿Para qué? *Revista Mexicana de Análisis de la conducta, 12*, 443–466.
Ribes, E. (1990). *Psicología general*. Trillas.
Ribes, E. (1982). *El conductismo: Reflexiones críticas*. Fontanella.
Ribes, E. (1992). Sobre el tiempo y el espacio psicológicos. *Acta Comportamentalia*, 71–84.
Ribes, E. (1994). Skinner y la psicología: Lo que hizo, lo que no hizo, y lo que nos corresponde hacer. In E. Ribes (Ed.), *B.F. Skinner: In memoriam* (pp. 139–174). Universidad de Guadalajara.
Ribes, E. (1996a). Obituarium; William N. Schoenfeld (1915-1996). *Revista Mexicana de Análisis de la Conducta, 22*, 93–112.
Ribes, E. (1996b). Cartesian mechanics, conditioning theory, and behaviorism: Some reflections on behavior and language. *Mexican Journal of Behavior Analysis, 22*, 119–138.
Ribes, E. (1997). Causality and contingency: Some conceptual considerations. *The Psychological Record, 47*, 619–539.
Ribes, E. (1999). *Teoría del condicionamiento y lenguaje: Un análisis histórico y conceptual*. Trillas.
Ribes, E. (2000). Las psicologías y la definición de sus objetos de estudios. *Revista Mexicana de Análisis de la Conducta, 36*, 365–382.
Ribes, E. (2001a). La psicología en Xalapa: Crónica de sus inicios. *Integración, 9*, 181–212.
Ribes, E. (2001b). Los conceptos cognoscitivos y el problema de la observabilidad. *Acta Comportamentalia, 9*, 9–19.
Ribes, E. (2003). What is defined in operational definitions? The case of operant psychology. *Behavior and Philosophy, 31*, 111–126.
Ribes, E. (2004a). Concepts and theories: Relation to scientific categories. In A. Lattal & P. Chase (Eds.), *Behavior theory and philosophy* (pp. 247–264). Klüver/Plenum.

Ribes, E. (2004b). Theory, scientific research, and technical applications. In J. Burgos & E. Ribes (Eds.), *The relationships between theory, research, and applications in behavior science* (pp. 19–44). Universidad de Guadalajara.
Ribes, E. (2006). A theoretical and research program on human and animal behavior. *International Journal of Psychology, 41*, 436–448.
Ribes, E. (2007). Estados y límites del campo, medios de contacto, y análisis molar del comportamiento: Reflexiones teóricas. *Acta Comportamentalia, 15*, 229–259.
Ribes, E. (2008a). Educación básica, desarrollo psicológico y planeación de competencias. *Revista Mexicana de Psicología, 25*, 193–207.
Ribes, E. (2008b). *Psicología y salud: Un análisis conceptual (segunda edición)*. Trillas.
Ribes, E. (2010a). Remembranzas y reflexiones autobiográficas. *Revista de Historia de la Psicología, 31*, 31–50.
Ribes, E. (2010b). *Teoría de la conducta 2: Avances y extensiones*. Trillas.
Ribes, E. (2013). Una reflexión sobre los modos generales de conocer y los objetos de Conocimiento de las diversas ciencias empíricas, incluyendo a la psicología. *Revista Mexicana de Psicología, 30*, 80–95.
Ribes, E. (2017). William N. Schoenfeld (1915-1996): A busca por una teoría de campo. In D. Zilio & N. Karrara (Eds.), *Behaviorismos: Reflexöes históricas e conceptuais* (pp. 235–282). Paradigma.
Ribes, E. (2018). *El estudio científico de la conducta individual: Una introducción a la teoría de la psicología*. El Manual Moderno.
Ribes, E., & López-Valadez, F. (1985). *Teoría de la conducta: Un análisis de campo y Paramétrico*. Trillas.
Ribes, E., & Sánchez, S. (1992). Individual behavior consistencies as interactive styles: Their relation to personality. *The Psychological Record, 42*, 369–387.
Ribes, E., & Contreras, S. (2007). Individual consistencies in behavior: Achievement persistence interactions as personality styles. *Psychological Reports, 101*, 365–377.
Ribes, E., & Burgos, J. (2006). *Raíces históricas y filosóficas del conductismo*. Universidad de Guadalajara/Xalapa: Universidad Veracruzana.
Ribes, E., Pulido, L., Rangel, N., & Sánchez-Gatell, E. (2016). *Sociopsicología: Instituciones y relaciones interindividuales*. La Catarata.
Ribes, E., Contreras, S., Martínez, C., Viladrich, C., & Doval, E. (2005). Individual consistencies across time and tasks: A replication of interactive styles. *The Psychological Record, 55*, 619–631.
Ribes, E., Rangel, N., Pulido, L., Valdez, U., Ramírez, E., Jiménez, C., & Hernández, M. (2010). Reciprocity of responding as a determinant of partial-altruistic behavior in humans. *European Journal of Behavior Analysis, 11*, 105–114.
Romanes, G. J. (1883/2016). *Mental evolution in animals*. Forgotten Books.
Ryle, G. (1949). *The concept of mind*. Barnes & Noble.
Schoenfeld, W. N., & Cole, B. K. (1972). *Stimulus schedules: The T-t systems*. Harper.
Skinner, B. F. (1938). *The behavior of organisms*. Appleton Century Crofts.
Skinner, B. F. (1948). 'Superstition' in the pigeon. *Journal of Experimental Psychology, 38*, 168–172.
Skinner, B. F. (1857). *Verbal behavior*. Appleton Century Crofts.
Toulmin, S. (1953). *The philosophy of science: An introduction*. Harper & Rowe.
Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20*, 158–177.
Watson, J. B. (1914). *Behavior: An introduction to comparative psychology*. Holt.
Wittgenstein, L. (1953). *Philosophical investigations*. Basil Blackwell.

Chapter 20
A Critical Appraisal of Ribes' Theory of Psychology

Hernando Borges Neves Filho and Tiago de Oliveira Magalhães

In the 1940s, Wittgenstein wrote that "in psychology there are experimental methods and conceptual confusion… The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problem and method pass one another by" (Wittgenstein, 1958, 232e). More recently, Machado et al. (2000) criticized psychologists for this same tendency of overemphasizing empirical over conceptual and theoretical research. Ribes (2006) advances a similar criticism: "Contrary to our pragmatic culture, advances in psychology do not necessarily depend on empirical accumulation of evidence, especially when it is based upon conceptual misunderstandings. The critical revision of prevailing assumptions about human behavior may be a more adequate strategy to formulate meaningful questions" (p. 121). Based on this conception, the Theory of Behavior, later reformulated as Theory of Psychology (TP), put forward by Emílio Ribes-Iñesta is a *tour de force* of theoretical work, firmly grounded in vast empirical research on human and animal behavior that intends to do away with the plethora of conceptual confusion inherent in the academic psychology.

TP proposes promising solutions to important issues that behaviorism has faced since its first days. One of them is the relation between the technical language used by psychologists and the ordinary means of talking about behavior, especially the linguistic expressions usually acknowledged as description of an inner world of subjective phenomena. Under the influence of Wittgenstein, Ribes rejects a common perspective among psychologists: eliminativism. According to eliminativists, ordinary language houses some inadequate conceptual frames, and science must eliminate those frames and replace them with new ones, in order for our knowledge

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to develop (Magalhães, 2017). This stance is explicitly adopted by Skinner (1938), who attributes several problems to the terms we ordinarily use to describe behavior. The replacement of these terms with a set of sharp and directly-related-to-data expressions would be necessary to increase our ability to predict and control behavior.

This replacement, however, is not entirely viable. Ordinary language is everywhere, and scientists are not immune to the conceptual difficulties inherent in its use. Given that, for Ribes "psychology must anchor its subject matter in a universe of phenomena belonging to ordinary knowledge and language practices" (2021). According to his perspective, it is not correct to say that under the skin of each person there is a private world that can be described by language. For Ribes, as well as for Wittgenstein, "psychological or 'mental' phenomena are a constitutive part of ordinary language practices and that expressions identified as psychological or 'mental' do not refer to anything" (2021). Psychological phenomena cannot be understood as discrete stimuli or responses; it has to be interpreted as complex relations extended in time. Thus structured, TP is an interesting alternative to Skinner's theory, avoiding important philosophical problems imposed by the notion of private events, a notion that preserves some Cartesian conceptions of mind (Tourinho, 1994).

Another virtue of Ribes's system is the considerable effort made to avoid some reductionist core conceptions of behavior science. The conceptual frame proposed by TP intends to overcome the traditional understanding of behavior in terms of stimuli and responses, proposing a more complex and sophisticated set of categories. The functional conception of stimulus and response, as articulated by Skinner (1938), was already intended to emphasize the relational nature of behavior. However, the use of the terms stimuli and responses foments some misunderstandings. As explained by Ribes (1997), following Kantor (1924), technical behaviorist language frequently confuses stimulus object, stimulus, and stimulus functions. Among other things, a more precise classification could be helpful to distinguish the various stimulus functions between each other and to more accurately understand the relations between psychology and other sciences, mainly biology and sociology.

Instead of the predominant atomistic approach, TP puts forth a field conception, based primarily on molar analysis, rather than the fragmentary analysis of simplified and insufficient samples of behavior that employs "the operational respondent/operant dichotomy, verbal/nonverbal, and rule-governed/contingency shaped subdivisions" (Ribes-Iñesta, 2021). Instead, TP includes five types of "functional contacts," intended to describe different groups of relations characterized by distinct levels of complexity: coupling, alteration, comparison, extension, and transformation. Coupling-contingencies are the most general and simple kind of contact, present in all the species that show psychological behavior, and transformation-contingencies are the more complex ones, restricted to purely verbal relations.

Under the operant-respondent dichotomy, there is a tendency to classify behavioral phenomena using disjunctive reasoning: i.e., if this is an operant sample of behavior, then it is not a respondent one (Domjan, 2016). Ribes's TB, and later TP, rejects that dichotomic way of thinking, recognizing that "A relatively simpler psychological organization is incorporated into higher-order functions. Therefore, the degree of simplicity of a function is equivalent to its relation of 'inclusiveness' with other functions" (Ribes & López-Valadez, 1985, p. 116, translated by the authors). The emergence of a new type of contact expands the possibilities of relations between the members of a field and maintains possibilities already present in the preexistent kinds. This feature establishes a quite distinct way of conceiving the different levels of complexity displayed by behavior. Linguistic phenomena, for instance, should not be described as a kind of behavior of greater complexity, apart from reflex or common instrumental responses. In TP taxonomy, language may be present since the simplest type of functional contact, coupling-contingencies, even if the most sophisticated possibilities emerge only with extension and transformation contacts. Some of our simplest reactions, like seeing an object as a fruit, a mere action of perceiving, are functionally related to linguistic conventions with which we interact in social contexts. The act of seeing an object, as a perceptual process, may not be an operant response, in Skinnerian terms, but this is not a good reason for claiming that the response lacks verbal character, regardless of the definition of verbal behavior proposed by Skinner (1957). TP seems to offer a more appropriate alternative to conceive the total role of language in human behavior.

The rejection of the atomistic way of building a conceptual frame, adding simple elements step by step, makes TP adopt a more assorted set of criteria, whose underlying logic is not always clear. To illustrate that, let's look at the distinction between the three first types of functional contact. What distinguishes coupling-contingencies from alteration-contingencies is that, in the later but not in the former, the subject is able to modify, in a molar sense, the contingencies disposed by the environment in which behavior occurs. The third type of functional contact, comparison-contingencies, differs from alteration-contingencies in quite different way: it is not based on absolute properties of the objects but on their relational properties. "In comparison contacts, absolute properties of objects and behavior become functionally related as an outcome of relational correspondence contingencies" (Ribes, this volume). It is hard to understand why those three types of contact would form a single line of progressive increase in complexity and not, for instance, a matrix organized under the two criteria proposed. Apparently, the ability to modify the contingencies and the ability to grasp relational properties are distinct and mutually independent. So, it seems possible to conceive, for example, functional contacts in which (1) the subject cannot modify the contingencies and acts purely based on absolute properties; (2) the subject can modify the contingencies and acts purely based on absolute properties; (3) the subject cannot modify the contingencies and acts based on relative properties; and (4) the subject can modify the contingencies and acts based on relative properties.

The last two types of functional contact, extension-contingencies and transformation-contingencies, are characterized as fields in which linguistic relations are indispensable. *Language* is a core notion in TP, but Ribes does not offer a technical definition of it. Apparently, the word is used with its ordinary meaning. Ribes (1999, 2018) presents sharp criticisms of the theories of verbal behavior (Skinner, 1957), stimulus equivalence (Sidman, 1994), and relational frame theory (Hayes et al., 2001) that expose the difficulties of theorizing on such a broad set of phenomena like the ones embraced by the word *language*. Thereby, the author one more time aligns with Wittgenstein (1958), recognizing that finding a definition is apparently impossible, since the functions of what we call language are "highly diversified in their effects on social life: to name, to describe, to ask, to communicate things, to teach, to learn, to do things, to reject, to look for, to invent, and so on" (Ribes, 2006, p. 116).

However, in having reference as a core concept in the characterization of extension-contingencies, TP departs from Wittgenstein. The departure is understandable, given that Wittgenstein was not interested in creating theories, as his intention was purely to clarify the use of language. Ribes, in its turn, seeks to systematically explain behavior. With that purpose, the author articulates a concept of reference, understood as something that expands the field of contact making present what is not present in the situation. It is important to highlight that this is not an attempt to reduce all the linguistic practices to one of its aspects. The concept of reference is, in TP, just one technical term, between others, developed according to the necessities of theoretical explanation, and not a name for the supposed essence of language.

Even if it does not try to reduce language to reference, it is possible to articulate a Wittgensteinian criticism of how TP approaches linguistic phenomena. Emphasizing the ability to make present what is not present, through reference, TP apparently attributes disproportional weight to semantics over, for example, syntax. The referential function is, certainly, one of the most important features of language practices, but the way human beings make references and inferences would not be possible without the peculiar grammatical relations underlying speech. Incorporating considerations on the syntactic aspects of language could be one way to supplement TP's conception of linguistic practices and consequently its conception of human behavior.

As a Theory of Psychology, Ribes' TP offers interesting possibilities of research through the application of the field model logic to many other topics besides language. Its comprehensiveness is evident for virtually any subject matter of interest to any behavioral or cognitive science. To illustrate this, here we briefly exemplify the pertinence of TP as (1) a guide for research of individual differences within signature testing of comparative animal cognition and (2) a way to understand the pervasiveness of fake news in today's human culture and society.

Testing complex behavioral patterns in different animal species is a constant trend in studies of animal cognition and comparative psychology, especially those that employ problem-solving methodologies, both in the wild and in controlled environments (Holth, 2008; Taylor, 2014; Neves Filho, 2018). Signature testing is a general term used in animal cognition research to describe a battery of behavioral and cognitive tests designed to measure different repertoires in individuals of different species, in a comparative fashion. By identifying and measuring the similarities and differences in repertoires and how different animals of different species learn, solve or do not solve problems, use tools, perform complex discriminative tasks, and so on, it is possible to investigate the different ways evolution produces specialized behavior observed in different species today, taking into account the ecological niche of each one. Tool use, for example, is a complex, naturally occurring behavior that is found in primates, corvids, and dolphins, to name a few (for a review, see Shumaker et al., 2011), and these species perform these functionally similar, complex behaviors in dramatically different ways, contexts, and environments.

This phenomenon of different phylogenetic histories (i.e., different species) producing functionally similar complex behavior in different environments (e.g., tool use in the wild) is known as convergent evolution (van Horik et al., 2011; Roth, 2015), with signature testing as a way to empirically test and observe the acquisition and function of these similar complex behaviors in different species. TP, especially the underlying assumption that full psychological individuation (not to be confused with psychological behavior) is reserved to some species (the *Homo sapiens* being the explicit example), poses an interesting empirical question. Signature testing could include testing this assumption in many empirical ways, as it has overlap many questions currently being asked in ethology and comparative animal cognition, such as why some group of primates show tool use in the wild and others don't (Cardoso & Ottoni, 2016; Gruber et al., 2010; van Schaik et al., 1999), or why even different groups of the same species of primates show variations of skill techniques of tool use (Fox et al., 2004).

Efficient tool use in groups of primates and birds such as New Caledonian crows (*Corvus moneduloides*) is generally observed in some individuals of the group, but not others (e.g., Neves Filho et al., 2016; Neves Filho et al., 2019). This proficiency in tool use in the wild is cross generational, so there is a process of learning between members of the group involved, some of which are more involved than others (e.g., juveniles, Fragaszy & Visalberghi, 2004). Additionally, proficient tool users can be treated differently by group members (Stammbach, 1988; Neves Filho et al., 2011), and there have been even gender differences in tool use and function documented (Falótico & Ottoni, 2013). These data taken together permit us to visualize how tool use in the wild, when unevenly distributed in a population, can be a possible origin of specialized individual differentiation, given different consequences for different members of the group—a group that, in the case of primates, already has a structural hierarchy of members.

TP offers a chance to look at all these currently available data on comparative animal cognition through a new lens. Moreover, the notion of behavioral styles also seems to be a promising tool for understanding the variety of possibilities found in this field. Another important improvement proposed by TP is adding new dimensions to the description of behaviors, like directionality, preference, persistence, variation, and vigor, all which make possible to describe behavior in a more detailed and refined way. All of these contributions could aid us in drawing a "map" of what individual and social processes are involved in the making of an individual, and how and where these processes, or rudimentary versions of these processes, can be found in the animal kingdom.

Our second point should be of special interest for behavior analysts of the Skinnerian tradition who study ethics, cultural, and social behavior. There is a growing and necessary call among behavior analysts for a "behaviorism for social issues," and many interesting and important studies are being published showing a variety of examples where a behavioral perspective can help understand and intervene in a social matter (e.g., Abdala et al., 2020; Lemos & Todorov, 2020; Medeiros & Haydu, 2018; Saini & Vance, 2020; Valderlon & Elias, 2019). Recently, a pervasive, widespread, and global issue has taken prominence in academic and common discourse: fake news, or the effects of disinformation.

"Fake news" was the word of the year 2016 (Sample et al., 2020). It is a term used to describe information that is false, inaccurate, or literally made up, fake. A simple example would be to affirm that 2 + 2 = 5, an affirmation that any person with basic arithmetical knowledge could dispute and claim as false. However, the origin of the dispute resides in the "knowledgeable person," or in the previous information and experience of that person. And recent large-scale studies showed that people attribute information as "fake news" largely biased by political, ideological, and moral backgrounds (Axt et al., 2020; Guess et al., 2019).

These studies, and all the first-hand evidence laid by the damaging effect of the spread of fake news, disinformation, and all sorts of conspiracy theories during the COVID-19 pandemic—something that is not exclusive for this pandemic (e.g., Larson, 2018)—suggest that it is not a simple case of "showing the truth," or to inform or educate someone against fake news (Salvi et al., 2021). In fact, studies show that once disinformation is spread in a group of people, fact checking is mostly ineffective to change what these people say, think, and do about it (Green & Donahue, 2011). This is a large-scale social issue, turbocharged by the real-time accessibility of communication and information exchange due to mobile phones and the Internet. This is something that we are still beginning to grasp as a society, but that has already had major impacts on democracies and in sanitary policies during the pandemic (Barua et al., 2020; Carr et al., 2020; Galhardi et al., 2020; Moscadelli et al., 2020). Throughout the Internet, falsehood spreads faster than the counterproofs, and reality is fragmented into different world views, beliefs, and moral practices that affect how people behave in everyday life (Hopp et al., 2020).

Much can be done locally to educate, inoculate against, or mitigate the effects of disinformation (e.g., Couto et al., 2020; Tibério et al., 2020); however to understand the origins, dispersion, creation, uses, and misuses of fake news, it is necessary to understand the moral or political biases surrounding the production, dispersion, and acceptance of disinformation. Understanding the origins and how moral practices and group dynamics are affected by the dispersion of disinformation is essential to tackle this problem. Ribes's TP frames ideology and morality right into a historical, behavioral, and field perspective that sheds light on how these phenomena control real day-to-day behavior of verbal humans in a given community. As Ribes (2018) puts it:

> Ideological practices are not separate from social practices in its various levels. The ideological is part of the social, arises as part of it and functions as part of it. It is not an addition, but is inherent to life shared in society. (p. 626, translated by the authors)

In this sense, since people tend to accept more disinformation if it is aligned to their backgrounds, and the Internet permits a fast and real-time exchange of information, a multitude of ideological and moral practices are being reinforced by disinformation, producing a fragmented society that is incapable of a common ground consensus in a variety of topics. In this scenario, it is imperative to understand how these processes occur, because they are already being exploited (de Guarda et al., 2018). By understanding the roles played by moral and ideological practices in the acceptance and spread of disinformation, it is possible to better prevent or intervene and correct the disinformation (e.g., Feinberg & Willer, 2013; Jones & Song, 2013). TP understanding of morality and ideological practices and its dynamics in a group of individuals can serve as a comprehensive guide for all behaviorists interested in this and other social issues.

Ribes TP offers a comprehensive framework to understand the origins of psychological phenomena from individual differentiation to moral and ideological practices of humans in the twenty-first century, and by doing this, it urges a modern interdisciplinary approach to this understanding, unique among behavioristic approaches to interdisciplinary practices.

Behaviorisms that emerged and were established throughout the twentieth century mostly assumed a revolutionary (e.g., Watson, 1913) or defensive (e.g., Skinner, 1977) stance on the methodologies and theories of animal and human behavior, mostly because the behavioristic view breaks with the traditional mentalistic explanations, being revolutionary, or because behaviorists, as outsiders who breaks with the *status quo* of mentalism, must defend its subject matter, behavior, as a valid object of scientific scrutiny, defending this point of view by producing conceptual work and empirical data to back up this assumption (Boakes, 1984).

This general position of behaviorists as outsiders or a defensive, yet prolific, group of researchers probably was not an easy context to stimulate interaction with other sciences. In comparison, cognitivists of the cognitive revolution of the twentieth century (see Hobbs & Chiesa, 2011, for a discussion about the pertinence of the use of "revolution" to describe this historical event) readily stated that their field and all cognitive phenomena are interdisciplinary, gathering immediate attention, collaboration, and insertion in fields such as neurosciences, computer sciences, linguistics, and others (Gardner, 1987; Miller, 2003). To establish and develop an interdisciplinary agenda was probably a more suitable survival strategy, since cognitivism eclipsed behaviorism in the second half of the twentieth century (Virués-Ortega & Pear, 2014).

Learning from these past experiences, TP carefully delimits the object of study of psychology, radically distinguishing it from the ones of other sciences, but this posture, as we will show, does not deny the relevance of multidisciplinarity. The main feature of psychology's object of study is the singularity that takes place when an individual develops a set of peculiar characteristics that cannot be explained as properties shared by the other members of the species:

> The psychological phenomenon is identified with the relationship and not with the organism. The organism, as an object of knowledge, defines the domain of biological science. It is important to specify that not any relationship between the organism and other entities typifies a psychological phenomenon, since ecological relationships are part of biology and not psychology. However, ecological relationships identify the activities of organisms as a species, that is, as a biological collective, and these activities are shared by all members of

> the species. There is no singularity of the activity of an organism in the ecological domain. All organisms of a species share the same activity and are not distinguishable from each other. The change in activity occurs as a change in the activity of the species. On the other hand, the psychological relationship is specific to each individual because the psychological is identified precisely from the uniqueness of the relationships between an individual and other entities (Ribes, 2018, p. 123, translated by the authors).

TP also points to singularity in order to distinguish the object of psychology from the object of social sciences. In this case, the difference comes from the institutional, impersonal character of the relations studied by social sciences:

> A relationship is psychological when it occurs with respect to an object, event or another individual or person, that is, when the relationship between these two elements always includes a person and the other element may or may not be another person, they are the only components that define the relationship and its course. In contrast, in a social relationship, the defining relationship takes place as an institutional relationship. It is a self-referred collective practice, which is presented as relationships between individuals, interindividual or interpersonal, in which these relationships are always located as circumstantial variants of a custom or convention. (Ribes, 2018, p. 128–129, translated by the authors)

There always was a fertile ground of interactions between psychology and other sciences, especially biology and social sciences. But, to avoid conceptual and methodological confusion, it is necessary to elucidate the precise role each part should play.

According to TP, psychology is not a profession, but a science. Its role is not to seek prediction and control of phenomena, but to understand and explain them. The different fields of application are characterized by the mixed use of scientific knowledge coming from different disciplines in order to deal with social demands. The employment of scientific knowledge in interdisciplinary fields is not a mere transposition of the formulations of the scientists; it involves a large amount of adaptation of those formulations to the specific features of the different services. For this reason, Ribes adopts the quite radical (yet totally pertinent) assumption that “there are no ‘psychological’ problems in society equivalent to problems related to health, education, communication, housing, and security, among others. So-called psychological problems are personal or, at best, interpersonal problems, and belong to the field of moral criteria” (this volume). Psychology contributes to solve problems in interdisciplinary applied fields, like health and education, but psychology is not defined by specific pragmatic demands. A possible evidence of that is what happens in the treatment of individuals diagnosed with atypical development, in which several distinct professions, like occupational therapists, speech therapists, behavior analysts (psychologists), and physical educators, utilize behavior-analytical knowledge associated with knowledge and practices from other disciplines in order to help patients improve their life quality (Smith, 2014).

In his characterization of the limits of psychology, Ribes adopts a normative, rather than a descriptive stance. The meaning of the word *psychology* is not explained in terms of the practices of people who use it, as would be recommendable according to a Wittgensteinian point of view, but in terms of rigorously delimited objects. One problem with a counterintuitive normative like that is the elevated risk of not being taken into account by the scientific community. Skinner’s (1981) Selection by

Consequences Model postulated a clear-cut division of behavioral phenomena between biology, behavior analysis, and anthropology. According to it, biologists should study species-specific behavior, and behavior analytics should study the ontogeny. Fortunately, biologists ignored that guideline and have done substantial work on the different forms of ontogenetic development across countless species (e.g., Alcock, 2001). Obviously, the isolation of Skinnerian behavior analysis from other sciences cannot be totally attributed to the adoption of that model, but it is reasonable to say that that way of understanding the relations between behavioral sciences did not foment multidisciplinary collaboration. This observation does not imply that Ribes's proposal will have the same destiny. But it seems cautious to analyze the case. The different sciences already have their practices of talking about what they study, and how. The successful introduction of a new characterization, based on theoretical considerations of a scientist from another area, demands a careful "persuasion strategy," taking into account the preexisting linguistic practices (probably, morality and ideological practices also play a role here).

We can speculate that a possible obstacle to the adoption of TP by other scientists or by psychologists could be the extension and complexity of its conceptual paraphernalia. The high response cost involved in assimilating such an extensive technical vocabulary almost makes the reader recognize virtues in the reductionist approach targeted by the author. Another stylistic feature that makes it difficult to understand the proposal is the parsimony with which examples are given. When exposing the five kinds of functional contact in this volume, Ribes illustrates each type with very brief descriptions of situations, what is understandable given the limited space. But one can experience similar difficulty in more extended expositions of TP, like Ribes (2018), in which, sometimes, the examples of the concept explained are presented only after several pages of purely abstract reasoning.

Grounded on rigorous conceptual and theoretical reasoning, TP undoubtedly shows remarkable explanatory power. Due to the rich resources it offers to a detailed description of behavior, TP can be compared to a very high definition set of photographs. This virtue, however, brings with it a relevant limitation: the photographs are probably too heavy, and its processing is not viable or too slow in most devices. Thus, it would not be surprising that people continued to choose the low-resolution photographs of atomistic theories instead of their more costly alternatives. In order to accomplish its social function of providing scientific interpretations to be adapted and used in the interdisciplinary fields, TP has to find a way to present more accessible images of itself.

References

Alcock, J. (2001). *Animal behavior: An evolutionary approach*. Sinauer Associates.

Abdala, M., Leite, F. L., & Neves Filho, H. B. (2020). Análise interpretativa do planejamento de contingências em malha cicloviária. *Acta Comportamentalia, 28*, 339–355. Retrieved from http://www.revistas.unam.mx/index.php/acom/article/view/76767

Axt, J. R., Landau, M. J., & Kay, A. C. (2020). The psychological appeal of fake-news attributions. *Psychological Science, 31*(7), 848–857. https://doi.org/10.1177/0956797620922785

Barua, Z., Barua, S., Aktar, S., Kabir, J., & Li, M. (2020). Effects of misinformation on COVID-19 individual responses and recommendations for resilience of disastrous consequences of misinformation. *Progress in Disaster Science, 8*. https://doi.org/10.1016/j.pdisas.2020.100119

Boakes, R. (1984). *From Darwin to behaviourism: Psychology and the minds of animals*. Cambridge University Press.

Cardoso, R. M., & Ottoni, E. B. (2016). The effects of tradition on problem solving by two wild populations of bearded capuchin monkeys in a probing task. *Biology Letters, 12*(11), 20160604. https://doi.org/10.1098/rsbl.2016.0604

Carr, P. R., Sanchez, S. L. C., & Daros, M. A. (2020). Citizen engagement in the contemporary era of Fake News: Hegemonic distraction or control of the Social Media context? *Postdigital Science and Education, 2*, 39–60. https://doi.org/10.1007/s42438-019-00052-z

Couto, K. C., Lorenzo, F. M., Tagliabue, M., Henriques, M. B., & Lemos, R. F. (2020). Underlying principles of a Covid-19 Behavioral Vaccine for a sustainable cultural change. *International Journal of Environmental Research and Public Health, 17*, 9066. https://doi.org/10.3390/ijerph17239066

de Guarda, R. F., Ohlson, M. P., & Romanini, A. V. (2018). Disinformation, dystopia and post-reality in social media: A semiotic-cognitive perspective. *Education for Information, 34*(3), 185–197. https://doi.org/10.3233/EFI-180209

Domjan, M. (2016). Elicited versus emitted behavior: Time to abandon the distinction. *Journal of the Experimental Analysis of Behavior, 105*(2), 231–245. https://doi.org/10.1002/jeab.197

Falótico, T., & Ottoni, E. B. (2013). Stone throwing as a sexual display in wild female Bearded Capuchin Monkeys, Sapajus libidinosus. *PLoS One, 8*(11), e79535. https://doi.org/10.1371/journal.pone.0079535

Feinberg, M., & Willer, R. (2013). The moral roots of environmental attitudes. *Psychological Science, 24*(1), 56–62. https://doi.org/10.1177/0956797612449177

Fox, E. A., van Schaik, C. P., Sitompul, A., & Wright, D. N. (2004). Intra-and interpopulational differences in orangutan (Pongo pygmaeus) activity and diet: Implications for the invention of tool use. *American Journal of Physical Anthropology, 125*(2), 162–174. https://doi.org/10.1002/ajpa.10386

Fragaszy, D., & Visalberghi, E. (2004). Socially biased learning in monkeys. *Animal Learning & Behavior, 32*, 24–35. https://doi.org/10.3758/BF03196004

Galhardi, C. P., Freire, N. P., & Minayo, M. C. de S. & Fagundes, M. C. M. (2020). Fact or Fake? An analysis of disinformation regarding the Covid-19 pandemic in Brazil. *Ciência & Saúde Coletiva, 25*, 4201–4210. https://doi.org/10.1590/1413-812320202510.2.28922020

Gardner, H. (1987). *The mind's new science: A history of cognitive revolution*. Basic Book.

Gruber, T., Reynolds, V., & Zuberbühler, K. (2010). The knowns and unknowns of chimpanzee culture. *Communicative & Integrative Biology, 3*(3), 221–223. https://doi.org/10.4161/cib.3.3.10658

Guess, A., Nagler, J., & Tucker, J. (2019). Less than you think: Prevalence and predictors of fake news dissemination on Facebook. *Science Advances, 5*(1), eaau4586. https://doi.org/10.1126/sciadv.aau4586

Jones, M. D., & Song, G. (2013). Making sense of climate change: How story frames shape cognition. *Political Psychology, 35*, 44–476. https://doi.org/10.1111/pops.12057

Green, M. C., & Donahue, J. K. (2011). Persistence of belief change in the face of deception: The effect of factual stories revealed to be false. *Media Psychology, 14*(3), 312–331. https://doi.org/10.1080/15213269.2011.598050

Moscadelli, A., Albora, G., Biamonte, M. A., Giorgetti, D., Innocenzio, M., Paoli, S., Lorini, C., Bonanni, P., & Bonaccorsi, G. (2020). Fake News and Covid-19 in Italy: Results of a Quantitative Observational Study. *International Journal of Environmental Research and Public Health, 17*(16), 5850. https://doi.org/10.3390/ijerph17165850

Hayes, S. C., Barnes-Holmes, D., & Roche, B. (2001). *Relational frame theory: A post-Skinnerian account of human language and cognition*. Plenum Press.

Hobbs, S., & Chiesa, M. (2011). The Myth of the "Cognitive Revolution". *European Journal of Behavior Analysis, 12*(2), 385–394. https://doi.org/10.1080/15021149.2011.11434390

Holth, P. (2008). What is a problem? Theoretical conceptions and methodological approaches to the study of problem solving. *European Journal of Behavior Analysis, 2*, 157–172. https://doi.org/10.1080/15021149.2008.11434302

Hopp, T., Ferrucci, P., & Vargo, C. J. (2020). Why do people share ideologically extreme, false, and misleading content on social media? A self-report and trace data–based analysis of countermedia content dissemination on Facebook and Twitter. *Human Communication Research, 46*(4), 357–384. https://doi.org/10.1093/hcr/hqz022

Kantor, J. R. (1924). *Principles of psychology*. Alfred Knopf.

Larson, H. J. (2018). The biggest pandemic risk? Viral misinformation. *Nature, 562*(7727), 309–309. https://doi.org/10.1038/d41586-018-07034-4

Lemos, R. F., & Todorov, J. C. (2020). Promoting intersectoral action to address chronic poverty and social exclusion. *Behavior and Social Issues, 29*, 35–51. https://doi.org/10.1007/s42822-020-00029-9

Machado, A., Lourenço, O., & Silva, F. J. (2000). Facts, concepts, and theories: The shape of psychology's epistemic triangle. *Behavior and Philosophy, 28*, 1–40.

Magalhães, T. (2017). Sobre certas dissimilaridades entre as Investigações filosóficas de Wittgenstein e o behaviorismo radical de Skinner. *Princípios: Revista de Filosofia, 24*(43), 175–225.

Medeiros, D. M. S., & Haydu, V. B. (2018). Interpretação Ambiental à luz dos princípios da Análise do Comportamento: Contribuições para Educação Ambiental. *Perspectivas em Análise do Comportamento, 9*, 43–59. https://doi.org/10.18761/PAC.2017.012

Miller, G. A. (2003). The cognitive revolution: A historical perspective. *Trends in Cognitive Sciences, 7*, 141–144. https://doi.org/10.1016/S1364-6613(03)00029-9

Neves Filho, H. B. (2018). *Criatividade: Suas origens e produtos sob uma perspectiva operante*. Imagine Publicações.

Neves Filho, H. B., Assumpção, A. P. B., & Barros, R. S. (2011). Engenharia social em macacos-prego em cativeiro: Um possível programa de enriquecimento ambiental. In *Conference in 41ª Reunião da Sociedade Brasileira de Psicologia (SBP)*. Brasíl. Retrieved from https://www.researchgate.net/publication/311946337_Engenharia_social_em_macacos-prego_em_cativeiro_Um_possivel_programa_de_enriquecimento_ambiental

Neves Filho, H. B., Carvalho Neto, M. B., Taytelbaum, G. P. T., Malheiros, R. S., & Knaus, Y. C. (2016). Effects of different traininghistories upon manufacturing a tool to solve a problem: Insight in capuchin monkeys (Sapajus spp.). *Animal Cognition, 19*, 1023–1045. https://doi.org/10.1007/s10071-016-1022-1

Neves Filho, H. B., Knaus, Y. C., & Taylor, A. H. (2019). New Caledonian crows can interconnect behaviors learned in different contexts, with different consequences and after exposure to failure. *International Journal of Comparative Psychology, 32*, 1–16. Retrieved from https://escholarship.org/uc/item/85b0q1r9

Ribes, E. (2006). Human behavior as language: Some thoughts on Wittgenstein. *Behavior and Philosophy, 34*, 109–121.

Ribes, E. (1997). Causality and contingency: some conceptual considerations. *The Psychological Record, 47*(4), 619–635. https://doi.org/10.1007/BF03395249

Ribes, E. (1999). *Teoría del condicionamiento y lenguaje: Un análisis histórico y conceptual*. Taurus.

Ribes, E. (2018). *El estudio científico de la conducta individual: Una introducción a la teoría de la psicología*. El Manual Moderno.

Ribes, E., & López-Valadez, F. (1985). *Teoría de la conducta: Un análisis de campo y Paramétrico*. Trillas.

Ribes-Iñesta, E. (2021). A theory of behavior or a theory of psychology? In: D. Zilio, K. Carrara (Eds.) *Contemporary behaviorisms in debate*. Springer, New York.

Roth, G. (2015). Convergent evolution of complex brains and high intelligence. *Philosophical Transactions of the Royal Society of London. Series B, Biological Sciences, 370*(1684), 20150049. https://doi.org/10.1098/rstb.2015.0049

Saini, V., & Vance, H. (2020). Systemic racism and cultural selection: A preliminary analysis of metacontingencies. *Behavior and Social Issues, 29*, 52–63. https://doi.org/10.1007/s42822-020-00040-0

Sample, C., Jensen, M. J., Scott, K., McAlaney, J., Fitchpatrick, S., Brockinton, A., Ormrod, D., & Ormrod, A. (2020). Interdisciplinary lessons learned while researching fake news. *Frontiers in Psychology, 11*, 537612. https://doi.org/10.3389/fpsyg.2020.537612

Salvi, C., Iannello, P., Cancer, A., McClay, M., Rago, S., Dunsmoor, J. E., & Antonietti, A. (2021). Going viral: How fear, socio-cognitive polarization and problem-solving influence Fake News detection and proliferation during COVID-19 Pandemic. *Frontiers in Communication, 5*, 562588. https://doi.org/10.3389/fcomm.2020.562588

Shumaker, R. W., Walkup, K. R., & Beck, B. B. (2011). *Animal tool behavior: The use and manufacture of tools by animals (revised and updated ed.)*. John Hopkins University Press.

Sidman, M. (1994). *Equivalence relations and behavior: A research story*. Authors Cooperative.

Skinner, B. F. (1938). *The behavior of organisms*. Appleton Century Crofts.

Skinner, B. F. (1957). *Verbal behavior*. Appleton Century Crofts.

Skinner, B. (1977). Why I am not a cognitive psychologist. *Behavior, 5*(2), 1–10. Retrieved from http://www.jstor.org/stable/27758892

Skinner, B. F. (1981). Selection by consequences. *Science, 213*(4507), 501–504. https://doi.org/10.1126/science.7244649

Smith, T. (2014). Behavior analysts can be interdisciplinary too: A review of Durand's Autism Spectrum Disorder. *Journal of Applied Behavior Analysis, 47*, 866–879. https://doi.org/10.1002/jaba.157

Stammbach, E. (1988). Group responses to specially skilled individuals in a Macaca fascicularis group. *Behaviour, 107*(3–4), 241–266. https://doi.org/10.1163/156853988X00368

Taylor, A. H. (2014). Corvid cognition. *Wiley Interdisciplinary Reviews: Cognitive Science, 5*(3), 361–372. https://doi.org/10.1002/wcs.1286

Tibério, S. F., Mizael, T. M., Luizm, F. B., Rocha, C. A. A., Araújo, S. A., Santos, A. M., Tehoch, G. B., Guarnieri, L. P., Fonseca Júnior, A. R. F., & Hünziker, M. H. L. (2020). A natureza comportamental da pandemia de covid-19. *Revista Brasileira de Análise do Comportamento, 16*, 57–70. https://doi.org/10.18542/rebac.v16i1.9098

Tourinho, E.Z. (1994) *Behaviorismo Radical, Representacionismo e Pragmatismo: Uma discussão epistemológica do pensamento de B. F. Skinner*. Tese de Doutoramento, Instituto de Psicologia da Universidade de São Paulo.

van Horik, J., Clayton, N. S., & Emery, N. J. (2011). Convergent evolution of cognition in corvids, apes and other animals. In T. K. Schackelford & J. Vonk (Eds.), *The Oxford handbook of Comparative Evolutionary Psychology*. Oxford University Press. https://doi.org/10.1093/oxfordhb/9780199738182.013.0005

van Schaik, C. P., Deaner, R. O., & Merril, M. Y. (1999). The conditions for tool use in primates: Implications for the evolution of material culture. *Journal of Human Evolution, 36*(6), 719–741. https://doi.org/10.1006/jhev.1999.0304

Valderlon, Y., & Elias, L. R. (2019). The Bolsa Família program and cultural design: Behavioral interpretations of cultural interventions. *Behavior and Social Issues, 28*, 114–126. https://doi.org/10.1007/s42822-019-0003-9

Virués-Ortega, J., & Pear, J. (2014). A history of "Behavior" and "Mind": Use of behavioral and cognitive terms in the 20th century. *The Psychological Record, 65*(1), 23–30. https://doi.org/10.1007/s40732-014-0079-y

Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20*, 158–177. https://doi.org/10.1037/h0074428

Wittgenstein, L. (1958). *Philosophical investigations*, 3rd ed., (tr. G. Anscombe). London: Macmillan.

Chapter 21
The Definition of Psychological Behavior and the Adequacy of Theoretical Concepts Are the Fundamental Issues: A Reply to Neves Filho and Magalhães

Emilio Ribes-Iñesta

I would like to say that I agree with many of the comments to this chapter by my colleagues Neves Filho and Magalhães and that I feel flattered by their general evaluation about the relevance of the field theory I have advanced during the last 35 years. Some of the difficulties involved in understanding some aspects of the proposal result from the limitations of space, on one hand, and from the novelty of the technical terms and assumptions for a reader coming from a different conceptual tradition. The same occurs to novel readers of operant theory.

I will focus on three issues in relation to the comments to this chapter. The first one concerns with how to conceive the subject matter of psychology and the nature of functional contacts in a field. The second deals with the concept of language in my proposal, and very especially the concept of referential behavior. Finally, the third will be related to the issues of multidiscipline, interdiscipline, and the ideological substrate of practical relations between individuals, which encapsulate psychological behavior.

Psychological Behavior

To talk about psychological behavior is not a redundancy. The term "behavior" is not exclusive of psychology, although, erroneously, psychologists have assumed so. "Behavior" is a term used in ordinary language in relation to good, bad, effective, or ineffective deeds of individuals. This use of the term *includes* the outcomes and functional circumstances of such individual acts. "Behavior" in ordinary language

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is equivalent to acts and functional actions (as fractions of acts). The term "behavior" is used by all the empirical sciences: physics, chemistry, biology (ecology included), social science (history and law included), and linguistics, additionally to psychology (and not all "psychologies," regrettably). In all these sciences, behavior is used as an extension of the ordinary language term but restricting the entities which manifest the specific kind of behavior or activity conceptually framed by each discipline. "Behavior" is used to refer to the activity of elementary particles, cosmic bodies, chemical molecules, cells, body organs, body functional systems, species, habitats, groups, institutions, law-related acts, markets, political corporations, linguistic practices, and, of course, individual acts. The psychological character of "behavior" is restricted to acts or actions of individual organisms of the Animal Kingdom, including mankind, based upon the development of differential sensitive and motor reactional systems and their coordination by some sort of neural tissue. Nevertheless, not all individual behavior of animals and men can be considered psychological. Rather, psychological behavior occurs only when some special functional changes take place in the biological and/or social behavior of individuals.

The different subject matters of empirical sciences are not capriciously established. They consist in analytic abstracted segments of reality such is interacted with in and by means of ordinary language practices. The universe of things and events conforming "reality" or the "world" is looked at in terms of progressively (and more restricted) complex layers of organization. Concreteness of things and events is overcome by fragmenting them according to selected properties, and scientific concepts denote these properties as the subject matter of every science: atoms, molecules, cells, social formations, and language structure. The density in "reality" of properties being identified and analyzed decreases as complexity increases, in such a way that the subject matter of physics encompasses all existing things and events, whereas the subject matter of linguistics covers only a small period of mankind, beginning with the invention of writing. The historical problem of psychology regarding the formulation of a consensual subject matter is rooted in the fact that "psychological phenomena" lack any substance whose properties can be abstracted and analyzed (Ribes, 2019). The invention of the "mind" and its several components or equivalents (consciousness, cognition, etc.) represents a historical attempt to fill this gap, under the influence of religious and political institutions concerned with social power and domination.

The viewpoint I have exposed is that psychological behavior is not an autonomous phenomenon or event, but that is a special kind of functional relation coextensive to bio(eco)logical and social behavior, in the case of human beings. Psychological behavior can only be predicated of animals (from coelenterates to *Homo sapiens*). As Kantor (1924–1926) advocated, psychological behavior consists of a relation (stimulus-response function) between an individual organism and particular stimulus objects and events (including other organisms). This relation takes place in a field conformed by the functional contact of the organism and the stimulus object or event, as well as by relevant setting factors (historical and situational) to the extent of the possibilities allowed by a contact medium. I shall not extend myself in the examination of the concept of a psychological field. Rather, I shall concentrate on the concept of functional contact. A functional contact is not a punctate event in which an individual and a stimulus object or event coincide. It is rather an episodic organization of interdependent contingencies and setting factors (Ribes, 1997), in which the individual organism is the target of analysis in its relationship with available stimulus objects and events. Psychological behavior is always an episodic functional transition of bioecological and/or social behaviors. As previously exposed (Ribes, 1997), all interactions between an individual and other individuals and stimulus objects and events in the environment are framed by initial occurrence contingencies, that is, by the interdependence of events taking place under given circumstances in a situation. Occurrence contingencies also include the individual's behavior. Psychological behavior emerges when the behavior of the individual develops new functional relations, different from those prevailing in the initial occurrence contingencies. The transition between two sets of occurrence contingencies (which always include the individual's behavior) may be identified as function-contingencies. Function-contingencies are the outcome of the functional detachment of behavior with respect to occurrence contingencies. Once the transition through new function-contingencies has concluded in the form of a different stable set of occurrence contingencies, psychological behavior as an episode ends, and begin to operate different occurrence contingencies involving bioecological and or social behaviors.

Psychological behavior consists in the functional detachment of individual bioecological and social behaviors regarding prevailing, standard, stable occurrence contingencies in a situation. Thus, psychological behavior, contrary to what is advocated by all psychological theories, is not continuous during the lifespan of individuals. It is a discontinuous set of functional detachment processes, always framed and coextensive to the occurrence of bioecological and social behavior which, undoubtedly, are continuous. The concept of functional detachment was originally used by Neal Miller (1971) and O.H. Mowrer (1960) in relation to the acquisition of fear as a drive. They pointed out that the biological response of pain when electric shock was applied had a detachable component, "fear," that could occur in the absence of shock and in presence of a previously "neutral" stimulus. This functional detachment of a component of biological behavior explained the establishment of discriminated avoidance and escape. The same could be said about classical salivary conditioning, in which a component of the eating response is detached and occurs in anticipation to food in the presence of a previous "neutral" stimulus event, the conditional stimulus. In the case that in the natural environment the contingency relation between both neutral stimuli regarding shock and food were consistent and permanent, the behavior thus developed would become a new bioecological behavior, often called "instinctive" in animals and "customs" in humans. Psychological behavior would take place only during the "conditioning" episode.

Functional detachment is equivalent to psychological behavior, which paradoxically is a behavior that always occurs, as a "substantial" act, in the form of bioecological or social behavior. I have proposed five different types of functional contacts identifying the same number of types of psychological behavior organization as field contingencies. This taxonomy is based upon the kind of functional detachment of individual behavior (biological and/or social) that takes place in a situation.

Coupling consists in a functional detachment of a biological or social behavior pattern from the (organically, ecologically, or socially determined) stimulus object before which usually occurs, by an extension of the behavior to other stimulus objects and events based upon the coincidence of spatiotemporal parameters of contingencies. *Altering* consists in a functional detachment of the behavior patterns from the spatiotemporal contingencies between stimulus objects and events in a situation by changes mediated by ongoing behavior. *Comparing* consists in the functional detachment from any absolute property of stimulus objects or events by conjugating behavior discriminating patterns to relational properties permutable among objects. *Extending* consists in the functional detachment of the present situation and ongoing contingencies by actualizing different situational contingencies previously occurred, that might occur, or that are occurring elsewhere. This functional detachment is only possible by means of linguistic behavior patterns given its conventional nature. *Transforming* consists in the functional detachment of the conventional contingencies relating referential behavior in a functional practical domain. It involves exclusively linguistic behavior contingencies, irrespectively of any particular situation. These functional contacts describe a continuum of functional detachment, involving increasingly complex contingency fields. Although it is assumed that to develop a more complex functional contact is required to be able to perform according to less complex ones, the latter are not compositional components of the former.

Language Is Not Verbal Behavior

Since the publication by Skinner (1957) of his book on *Verbal Behavior*, so-called behavior analysts have erroneously equated language (and linguistic behavior) with verbal behavior. Verbal behavior deals with speech or talking but does not encompass all kinds of linguistic behavior. Other active linguistic modes are gesturing and writing, without considering the reactive modes of observing, listening, and reading. Speaking is the most frequent kind of linguistic behavior, but nothing else. Functional properties of the other modes cannot be reduced to those of speech. In contrast to bioecological behavior, consistent in varied patterns of bodily bound sensitive and motor reactivity and activity, linguistic behavior, in each of the three active modes, involves special morphologies articulated by conventional criteria: sound articulation and patterning (talking), signs and graphic characteristics (writing), and movement and expressive features (gesturing). Kantor (1936) distinguished between alive and dead language. Alive language corresponds to actual linguistic behavior, whereas dead language has to do with outcomes and vestiges of such practical behavior in society. Talking or speaking in situation is alive language, while records of such talking, like transcriptions or tapes, are dead language. The same applies to writing and gesturing. Writing, should be highlighted, is the unique mode of linguistic behavior that results in the actualization of stimulus objects in the form of texts, graphics, and symbolic representations. Psychological behavior is related to both kinds of language in two different ways: individual linguistic behavior, as an activity, represents alive language, and dead language products may work as linguistic stimulus objects. In any case it is important to stress that, both in history and in ontogeny, after gesturing, speech or talking was and is the first form of socially acknowledged linguistic behavior and that writing emerged as the transcription of speech sounds through signs. Words appeared with writing. Writing is not symbolic behavior *per se*. Therefore, it is a mistake to look for grammatical rules governing speech, since grammar appeared with writing, and only can be applied to written texts. It should be kept in mind that linguistic behavior modes are relatively independent from each other.

Wittgenstein (1953) emphasized the difficulty in defining language since any kind of devisable social practice is permeated and articulated *in* and *through* language. He comments that to imagine a language is to imagine a form of life, since language does not occur in a vacuum. It is always part of an activity between individuals forming part of a social practice. Language alone as strings of sounds or signs would be meaningless. Language is always interwoven with social practice, and none of both language and social practice would be conceivable without each other. In fact, the history of the first human social formations is bound to the emergence of language. Language could not emerge either as a mere product of biological evolution. Language and society were "born" together. Because of this it is misleading to isolate so-called "verbal" behavior from social behavior, and to assume that individual linguistic behavior is equivalent to language. These conceptual mistakes are rooted in the generalized assumption that language is a biological outcome of evolution, and, therefore, as a brain function, it is an individual competence of the members of the species, *Homo sapiens*. Quite the opposite, language is a practical social activity, and not a brain "secretion" shaped by contingencies in the environment. Human life is organized *in* language, *with* language, and *by means of* or *through* language, including not only our social behavior but also our bioecological behavior and the qualities of our habitat.

Language, as previously mentioned, consists in different active and reactive modes, always integrated in social activities between individuals that involve all sort of doings. Linguistic behavior, then, may be considered as the individual component of social behavior, consisting in conventional reactive-active patterns that involve the various modes in which language takes place. In the human being, linguistic behavior integrates bioecological behavior as a distinctive difference with respect to animal behavior. Newborn become human individuals by participating in linguistic interactions with their kin or caregivers. During the first weeks of life, babies are vulnerable biological beings. Attachment and socialization results from the interaction with adults, and their increasing selective responsiveness to linguistic patterns (talking and gesturing) accompanying physical and biological contact during feeding, cleaning, dressing, and playing. Responsiveness initially consists in reactive patterns including motor, orienting and expressive responding to the adult. "Language development" is identified when the infant becomes active, uttering sounds, initiating approaches, and establishing reciprocal interactions of several sorts. This is considered the breaking point of language development in the infant and the beginning of an ever-time increasing social participation. Thus, even in early life stages, human behavior is functionally mediated by linguistic interactions and criteria, in such a way that it seems meaningless to distinguish among linguistic and non-linguistic human behavior just based on phonological (and graphological) morphologies. There is no question about language consisting mainly in arbitrary articulated sounds and their graphic signs. But it is also out of doubt that articulated speech and writing signs would never arise and developed independently of the practical social activities of individuals in which, as behavior, became and are sensical.

The conventional nature of language behavior is related to two factors. The first one is that arises as an outcome of practical activity *between* individuals. It is not the outcome of a previous agreement, that without language could not be achieved. It is the outcome of practical concordance. Agreement may take place afterwards through language. The second factor is the arbitrary morphology of language behavior, which is developed (and informally trained) without correspondence to any biological bound vocal reaction. This characteristic allows the detachability of language behavior from any object, action, situation, or physicochemical condition and gives account of the wide variety of natural languages or idioms. As Kantor (1936) pointed out, language syntaxis is nothing else than the peculiar style in which every linguistic community speaks and writes. The conventional nature of language behavior and its functional detachability affords individuals to share their activities and products in different situations, times, and places, without limiting their interactions to ongoing circumstances and conditions of a particular situation. This is the foundation of economic life as exchange relations resulting from specialized division of labor and socially delimited appropriation of goods and services (Ribes, 2001). Since language fosters that individual shares circumstances and outcomes, it is not surprising that language behavior in ordinary language practices in society consists mostly in referential behavior.

Reference means that when talking we always talk *about* something (not necessarily about things) *to* somebody (and in special circumstances to ourselves) while being engaged in some sort of shared activity or interaction. Speaking (and obviously gesturing and writing) is not a mere denotative or descriptive accompaniment of the reactions and actions taking place when individuals behave. As an accompaniment, language would be dispensable or redundant. Semantics, following an ancient tradition rooted in Augustine of Hippo, has postulated that the meaning of words consists in its "reference" as correspondence with an object. But semantics, as all the analyses of dead language, is based on words and sentences divorced from actual circumstances in which an individual speaks or writes while behaving and, therefore, is a misleading logic to follow in the understanding and explanation of social and psychological language behavior. The difficulties of this kind of analysis becomes especially evident when gesturing "meaning" is restricted to ostensive pointing. Since language practice, as referential behavior, always takes place in a social context, immediate or delayed (especially in writing), the meaning or sense of what is being gestured, spoken, or written consists in the way linguistic behavior is used, that is, its function interwoven with activities regarding other individuals in specific situations. This is what Wittgenstein (1953) emphasized when pointing that the meaning of a word or expressions is the way it is used in context. And it cannot be other way since one of the first obvious characteristics of word and expressions (even in dictionaries) is their multivocity, that is, no word or expression has a unique meaning. This feature is inherent to language behavior detachability.

Ideological Practices and the Complementation and Application of Scientific Knowledge

"Belief" is not a psychological term and beliefs are not things, events, or entities. As terms, beliefs pertain to the logic of dispositional categories (Ryle, 1949), framing propensities, tendencies, and biases. Beliefs can be identified only from consistencies in the behavior of individuals. But such consistencies involve more than one behavior and a particular situation. To believe something is to behave consistently according to interrelated practices which, as a set, provide meaning or sense to each other. Wittgenstein (1969) approached beliefs from the perspective of language games. Beliefs are a result of the practical learning of language games, which consist in the diversity of acting forms that have social sense or meaning because of language and through language. Wittgenstein (1969, 114) pointed out that "If you are not certain of any fact, you cannot be certain of the meaning of your words either." Language games are learned as practices related to interconnected facts. Consistent relations between practices and facts ensure that what it is done makes sense, and such consistencies become the rules of the language games. Therefore, beliefs are always beliefs about consistencies and not about isolated facts: "When we first begin to *believe* anything, what we believe is not a single proposition. It is a whole system of propositions (Light dawns gradually over the whole) …" (Wittgenstein, 1969, 141). "The child learns to believe a host of things, i.e., it learns to act according to these beliefs. Bit by bit these form a system of what is believed and in that system some things stand unshakeably fast and some are more or less liable to shift. What stands fast does so, not because it is intrinsically obvious or convincing; it is rather held fast by what lies around it" (Wittgenstein, 1969, 144). Social life implies the continuous learning of new and different doings by individuals, and to the extent that these doings become part of the social practice of individuals, it is believed in what is being done. Actions and words acquire a sense or meaning as facts. Not being sure of what is being done implies not being sure of the meaning of words either.

Beliefs are the outcome of practices having factual justification according to a given social context. Beliefs derive from senseful social doings and therefore consist in confirming or denying that something senseful or senseless may occur or not. Rumors and post-modern fake news are examples of beliefs expression, and of the inducing effects that may take place in related circumstances in which facts sustaining the beliefs may be absent. We believe in what we do. It would be absurd to do something and do not believe in that doing. Since beliefs are the outcome of social practice, we believe what is said if its coherent with it. We do not doubt about historical facts or about the existence of other countries, for instance. When coherence between facts in social practice is lost, then we doubt, but as Wittgenstein pointed out, doubt comes after belief. Consistently related doings are the ground of beliefs, and to change beliefs it is necessary to change the practices that justify them, or to induce doings that show different connections between facts related to usual doings.

To believe something is tantamount to accept it. I cannot believe something and reject this belief. We accept what we believe through the social practices and doings that sustain our beliefs. This accounts for why we may conceive beliefs as the "glue" of ideological practices. Ideological practices consist in individuals accepting their life conditions and doings according to the life conditions and practices of those who benefit from them. Even though there is no explicit statement in this regard, operant theory assumes a compositional perspective of social behavior kindred to liberalism. Liberal political theories advocate that society is the outcome of individuals' association to ensure survival and that the State is nothing more than an agreement between individuals to prevent inequity due to scarcity, violence, and coercion. Following Skinner's proposal of designing society and cultures (Skinner, 1948, 1961), it has been naively expected—or hoped—that it is possible to achieve a "non-aversively controlled" society by means of reinforcement contingencies engineering (Skinner, 1971). Nevertheless, mankind history daily shows that this is not the case and that social formations are organized and ruled according to the prevailing economic production-appropriation mode (tributary, slaver, feudalist, or the several kinds of capitalism: trading, industrial, monopolist, or financial). Social phenomena and processes cannot be reduced to the additive or subtractive composition of individual acts. Neither can be ignored that the behavior of individuals is always under the context and influence of social class interests and that, to that extent, it is not politically neutral. It has always been an active or a passive component of the power relations that originate in and sustain social domination of some classes over others in mankind history.

Vagueness and confusion regarding psychology's subject matter has propitiated the adoption of theoretical models and assumptions stemming from other disciplines, which are not exempt of ideological interests and influences. Prominent examples are the selectionism of the new synthesis in evolution and econometric models for animal and human behavior (Baum & Kraft, 1998; Kraft et al., 2002; Naour, 2009; Skinner, 1966). Animal behavior under operant or instrumental conditions has been interpreted as a minimal economic system. However, rats and pigeons in the experimental apparatuses have warranted their daily food and water in the living cages, irrespectively of their performance under food or water reinforcement, and the experimental environments have nothing to do with free living habitats shared with other species. Reinforcers or reinforcement cannot be assigned any value, since they are not involved in a trade exchange: food and water in the experimental chambers are not merchandise, have no exchange cost, nor produce any profit (Commons et al., 1987; Hursh et al., 1988; Collier et al., 1992). Such analogies and comparisons are out of place. The same can be said from the incorporation of logical models of equivalence to deal with language behavior and traditional phenomena conceived as problem-solving, reasoning, and thinking under the dubious label of rules-governed behavior (Hayes, 1991; Sidman, 1992).

The adoption of models foreign to psychology has been confounded with multidiscipline and interdiscipline relationships when logical intrusion and reductionism are really taking place. In the case of selectionism and econometric models based on marginalism and an ideal free market, ideological bias is evident, and in the case of equivalence models a return to formalism and rationalism may be observed. True multidiscipline must consider two conditions: first, the specificity of each discipline which enters in relation, and second the role that performs each discipline in the multidisciplinary enterprise, either regarding the theoretical question or the methodological and informative contribution. Never the theoretical problems of a discipline should be replaced by models or issues from other disciplines, as usually occurs in psychology. When model-invasion occurs, it indicates the confusion and logical mixture prevailing in the discipline. Unfortunately, this is the case in psychology and in so-called behavior analysis or operant theory. In addition to the formulation of a logical model specific to the subject matter of psychology, it is required a sufficient knowledge of the theoretical status of neighboring disciplines, in order to prevent the "pick-up" temptations resulting from "accidental" discoveries. In the case of interdisciplinary relations, these are determined by the social problem demanding attention and solution. It is not a matter of applied or translational science as it is fashionable to say in present days, but a matter of identifying in the social demand an individual behavior dimension in which psychological *theoretical and experimental knowledge* can contribute to in conjugation with other disciplines and practical social wisdom. So-called applied behavior analysis has only a family resemblance with experimentally based behavior theory and runs under different trails since many years ago (Ribes, 2004).

Final Comments

Ordinary language is concerned with living practices in the world, whereas scientific language is concerned with finding what the world is made of and how it works. Each kind of language pursues different types of knowledge goals and criteria, and they are not replaceable by each other. Questions regarding daily social life between individuals and their world are the domain of ordinary language and resorting to particular causes and reasons is the adequate way to explain and understand it (Ribes, 2020). In contrast, scientific language consists in a technical language, based on ordinary language, that analytically crosses through concrete entities, events, and conditions, abstracting their common functional properties. Although the raw material of science comes from ordinary language practices, scientific facts, objects, and events do not correspond directly to those involved in daily circumstances and episodes. The formulation of a theory requires being able to map all concrete phenomena "contained" in ordinary language, while new concepts (and the correlative methods) are formulated to deal with their abstracted, generic, transversal properties. And this is an effortful recursive, long-term enterprise, in which it is necessary to be self-critical, and sometimes to move backward in order to choose new ways of understanding and being coherent. That is the challenge.

References

Baum, W. M., & Kraft, J. R. (1998). Competition, travel, and the ideal free distribution. *Journal of the Experimental Analysis of Behavior, 64*, 227–245.

Collier, G., Johnson, D. F., & Morgan, C. (1992). The magnitude of reinforcement function in closed and open economies. *Journal of the Experimental Analysis of Behavior, 67*, 81–89.

Commons, M. L., Mazur, J. E., Nevin, J. A., & Rachlin, H. (1987). *Quantitative analyses of behavior (Vol. V). Effect of delay and intervening events on reinforcement value*. Lawrence Erlbaum.

Hayes, S. C. (1991). A relational control theory of stimulus equivalence. In L. J. Hayes & P. N. Chase (Eds.), *Dialogues on verbal behavior* (pp. 19–46). Context Press.

Hursh, S. R., Rasleav, T. G., Shurtleff, D., Bauman, R., & Simmons, L. (1988). A cost-benefit analysis of demand for food. *Journal of the Experimental Analysis of Behavior, 50*, 419–440.

Kraft, J. R., Baum, W. M., & Burge, M. J. (2002). Group choice and individual choices: Modeling human social behavior with the ideal free distribution. *Behavioural Processes, 57*, 227–240.

Kantor, J. R. (1924–1926). *Principles of psychology*. NY. Alfred Knopff.

Kantor, J. R. (1936). *An objective psychology of grammar*. Principia Press.

Miller, N. E. (1971). *Selected papers*. Atherton-Aldine.

Mowrer, O. H. (1960). *Learning theory and behavior*. John Wiley.

Naour, P. (2009). *E.O. Wilson and B.F. Skinner: A dialogue between sociobiology and radical behaviorism*. Springer.

Ribes, E. (1997). Causality and contingency: Some conceptual considerations. *The Psychological Record, 47*, 619–639.

Ribes, E. (2001). Functional dimensions of social behavior: Theoretical considerations and some preliminary data. *Mexican Journal of Behavior Analysis, vol., 27*, 285–306.

Ribes, E. (2004). Theory, scientific research. and technical applications: How related in operant psychology? In J. Burgos y E. Ribes (Eds.), The relationships between theory, research, and applications in behavior science. (pp. 19–44). : Universidad de Guadalajara.

Ribes, E. (2019). El objeto de conocimiento de la psicología como ciencia: Relación sin "cuerpo-substancia". *Acta Comportamentalia, 27*, 463–480.

Ribes, E. (2020). Sobre la explicación y su relación con los distintos modos de conocimiento. *Acta Comportamentalia, 28*, 223–236.

Ryle, G. (1949). The concept of mind. New York, NY: Barnes & Noble.

Sidman, M. (1992). Equivalence relations: Some basic considerations. In S. C. Hayes & L. J. Hayes (Eds.), *Understanding verbal relations* (pp. 15–27). Context Press.

Skinner, B. F. (1948). *Walden two*. The MacMillan.

Skinner, B. F. (1957). *Verbal behavior*. Appleton Century Crofts.

Skinner, B. F. (1961). The design of cultures. In *Cumulative Record* (pp. 36.01–36.12). Appleton Century Crofts.

Skinner, B. F. (1966). The phylogeny and ontogeny of behavior. *Science, 153*, 1205–1213.

Skinner, B. F. (1971). *Beyond freedom and dignity*. Alfred Knopff.

Wittgenstein, L. (1953). *Philosophical investigations*. Basic Blackwell.

Wittgenstein, L. (1969). *On certainty*. Basil Blackwell.

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