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The Place of Ordinary Psychological Categories in Behavior Analysis

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This paper discusses the place of ordinary psychological categories (OPCs) — i.e., terms from "folk psychology" (e.g., "intention," "fear," "thinking") — in a science of behavior, especially in behavior analysis. Based upon Laudan's problem-solving metA–Theoretical framework, I begin by laying out some of behavior analysis's standard guiding assumptions, as found in Skinner's works. Second, I provide a semi-formal reconstruction of Skinner's arguments against OPCs in a science of behavior. His assumption that OPCs are "mentalistic" — i.e., connote real or hypothetical inner phenomena explanatory of why we behave the way we do — is here highlighted, as well as disputed, by drawing upon conceptual analyses worked out by Ryle, among others. Third, I contrast Skinner's view (radical behaviorism) with Rachlin's (teleological behaviorism), a recent alternative behavioral standpoint on OPCs (similar to radical forms of 4E approaches). According to Rachlin, once the non-mentalistic meaning of OPCs is fully grasped, a proper use thereof turns out compatible with and useful to behavior analysis for picking out dependent variables. Finally, although going along with the thesis that OPCs are not mentalistic, I take issue with certain features of Rachlin's account; among other things, with his dismissal of covert behaviors in the analysis of OPCs. Nevertheless, his claim for the possible significance of OPCs in behavior analysis — more exactly, the thesis that a proper adoption of these categories therein would enhance its problem-solving power — is supported.

Keywords: behavioral science, psychological catgories, folk psychology, behaviorism

Are ordinary psychological categories — i.e., terms from so-called folk psychology (related to emotions, cognitive process broadly understood, personality traits and so on) — consistent with and potentially useful to behavior analysis

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and a science of behavior more generally? According to standard behavior analysis, the answer is negative. In this paper, I shall address this issue and support a positive answer to it in four steps.

Thinking along Laudan's (1977, 1981) problem-solving model of the structure and dynamics of scientific theories, the issue at hand hinges upon considerations of problem-solving effectiveness of a given research tradition; in this case, of behavior analysis, part of a larger science of behavior comprising congenial neuroscientific, ethological, and anthropological studies (cf. Skinner, 1981, 1990). Thus, I aim to argue that a proper adoption of ordinary psychological categories (OPCs) in behavior analysis would enhance behavior analysis's problem-solving effectiveness. According to Laudan, the problem-solving effectiveness of a scientific research tradition is measured by the number and importances of both the conceptual and empirical problems it solves, subtracted by the number and importance of conceptual problems and empirical anomalies it faces.

By taking advantage of Laudan's model, (1) I lay out behavior analysis's standard guiding assumptions — ontological, methodological, and axiological guidelines — most significant to the issue at hand. These guidelines, it will be shown, underpin behavior analysis's usual dismissal of OPCs. Now, there is unequivocal consensus in this tradition on the centrality of several of Skinner's works from the 1930's until the 1980's (such as *The Behavior of Organisms; Science and Human Behavior;* and papers collected in *Cumulative Record*) in setting up these guidelines, known, roughly speaking, under the label of "radical behaviorism." This consensus is witnessed, for instance, by textbooks (e.g., Catania, 2012; Chiesa, 1994; Moreira and Medeiros, 2007; Pierce and Cheney, 2004) in the area. In keeping with this consensus, my reconstruction of behavior analysis's standard guiding assumptions is fundamentally based upon Skinner's works.

Subsequently, (2) I provide a reconstruction and assessment of Skinner's view on the meaning of OPCs and their place in behavioral science. Part of this reconstruction is made in semi-formal terms. That is, I make explicit each main premise of his arguments and the connection among the premises, by taking advantage of some rudiments of classical logic. As will be shown, Skinner does not have an unambiguous view on the meaning of OPCs. Yet, when it comes to the place of OPCs in behavioral science, his position is clear-cut, to wit: a qualified form of behavioral eliminativism (cf. Collins, 2007; Lazzeri and Oliveira–Castro, 2010; Rey, 1997), whereby, roughly speaking, by and large these terms are explanatory fictions and should have no place in behavioral science. I shall highlight and cast doubt upon Skinner's assumption that OPCs are "mentalistic" terms (i.e., that they connote, even though mistakenly, inner determinants explanatory of why we behave the way we do), by drawing upon Ryle (1949) and others.

Then, (3) I shall contrast Skinner's radical behaviorism with Rachlin's (1994, 2014) teleological behaviorism, an alternative behavioral standpoint on OPCs which calls Skinner's eliminativism into question as well. Rachlin's teleological

behaviorism and Foxall's (2004, 2007) intentional behaviorism are currently the two main contenders to radical behaviorism when it comes to OPCs in behavior analysis. I have chosen to discuss Rachlin's approach here partly because it is more clear-cut than Foxall's.[1] According to Rachlin, once the meaning of OPCs is fully grasped — which, he claims, comes down to patterns of overt behavior over time — the use thereof for identifying phenomena of widespread interest in our daily lives turns out to be compatible with and useful to behavior analysis on different counts.

Finally, (4) I take issue with certain features of Rachlin's account, among other things, with his dismissal of covert behaviors in the analysis of OPCs. Nevertheless, his claim for the possible significance of OPCs in behavior analysis, like Deitz and Arrington's (1984) "call to cognition" for behavior analysts, is here supported. Thus, thinking along Laudan's (1977) model of the dynamics and structure of scientific theories, I argue there are good reasons (distinct from Foxall's, 2004) to expect that a certain application of OPCs in behavior analysis would increase its problem-solving effectiveness; but this application should rest upon a more accurate behavioral picture of OPCs (which I endeavor to outline elsewhere; see, in particular, Lazzeri, 2015a, 2015b, 2016).

As far as I know, this is the first article in the English language extensively discussing both Rachlin's approach to the meaning of OPCs *and* their place in a science of behavior. In addition, as far as I am aware, this is the first comprehensive survey and discussion in English of Skinner's eliminative arguments employing semi-formal logical techniques *cum* elements of Laudan's approach to the structure and dynamics of theories.[2]

Given the prevalence of cognitivist models in behavioral science nowadays, one may ask why I have chosen to discuss behaviorist foundations. I could mention several reasons in this regard; here are two. First off, as will become more evident later on, I think behavior analysis has theoretical resources to model psychological categories — including the category related to cognitive process (e.g., thinking, remembering) — more accurately and parsimoniously than symbolic–computational models (cf., e.g., Lazzeri, 2015b). Second, in recent years, there has been a considerable interest among cognitive scientists in alternative approaches to mind–behavioral relations, called 4E (embodied, embedded, extended, and enactive) approaches. Radical forms of 4E approaches, which propose doing away with symbolic–computational models and call attention to the behavioral constitution of psychological phenomena (e.g., Barrett, 2011; Chemero, 2009; Hutto and Myin, 2013; Noë, 2009), bear some resemblances to avowed behaviorist ones (cf. Barrett, 2015; Charles, 2014; Chemero, 2013; Lazzeri, 2015a; Hackenberg, 2014).

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[1] I discuss Foxall's intentional behaviorism in Lazzeri (2015c).

[2] For the use of Laudan's model to study other aspects of behavior analysis, see Morris (1992).

This signals a possible closer approximation between behavior analysis and cognitive science further down the line.

Behavior Analysis Standard Guiding Assumptions

Laudan (1977) distinguishes two sorts of guiding assumptions (in Laudan et al.'s [1986] terminology) that make up a scientific research tradition: (1) ontological ones, i.e., guidelines regarding what things exist in the research domain, as well as the general properties they have (in other words, the conceived types of dependent and independent variables and their features); and (2) methodological ones, i.e., prescriptions of methods (e.g., inductive or hypothetical–deductive ones) to be adopted in the research domain. In Laudan (1984) a third sort of guiding assumption is discerned, to wit, (3) axiological ones, which are the cognitive values and aims (e.g., approximation to the truth, predictability) to be adopted.

According to Laudan's (1977) model, a scientific research tradition (a unit of analysis similar to Kuhn's [1970] "paradigms" and Lakatos' [1978] "research programs") is not defined by inflexible, immutable guiding assumptions. Rather, a research tradition has an historical identity, which can change over time. By competing with one or more traditions in the same field of study (e.g., behavior analysis versus psychoanalysis and cognitive–computational psychology), the guiding assumptions of a research tradition can change in order to improve the tradition's relative problem-solving effectiveness. The guiding assumptions that make it up are broadly accepted by researchers in the tradition as "sacrosanct" during a particular time period. However, a few of these assumptions are occasionally disputed by some theoreticians working in the tradition, believing changes in the assumptions are needed to improve the tradition's problem-solving effectiveness.[3] In behavior analysis, for instance, some of the standard guiding assumptions set forth by Skinner's radical behaviorism are disputed by Staddon's (2001) "theoretical behaviorism" and Rachlin's (1994) "teleological behaviorism" — the latter, as I will discuss later on, by suggesting, among other things, the use of OPCs for picking out dependent variables.

Let me, then, bring to the fore behavior analysis's standard ontological, methodological, and axiological guidelines most relevant to the issue at hand. They will be laid out immediately below in a list format and succinctly.[4] Skinner's view of OPCs and their place in a science of behavior, it will be highlighted shortly, is anchored upon these assumptions.

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[3] For details, see Laudan (1977, 1981) and Laudan et al. (1986).

[4] For more details, besides Skinner's own works, one can have a look at Chiesa (1994), Delprato and Midgley (1992), and Smith (1986).

Ontological Guidelines

(a) Behavior is lawful, that is, determined by conditions amenable to scientific control. Although some behaviors may not seem lawful, they must be assumed to be (Skinner, 1953, p. 6; 1947/1961a).

(b) Behavior, the dependent variable, is determined ultimately by three sorts of (independent) variables: phylogenetic ones, acting via the organism's genes; the organism's individual learning history; and culture. From the 1960's on, Skinner speaks of these three levels along selectionist terms: natural selection, operant selection, and cultural selection, respectively (cf., Skinner, 1969, 1971, 1974/1976, 1981, 1990).[5] He understands the two former levels in the Darwinian sense of selection, i.e., as involving variability, differential success and retention as necessary and sufficient conditions (e.g., Skinner, 1974/1976, 1981).

(c) Behaviors can be classified into respondents (or reflexes) and operants (e.g., Skinner, 1938, p. 20; 1953). Respondent behavior is a function of changes in conditions of the environment immediately before the behavior's occurrence and which induce the behavior's occurrence with high probability. It is either unconditioned, i.e., shaped in the species phylogeny; or conditioned, shaped by Pavlovian conditioning. Operant behavior, on the other hand, is a function of the consequences it produces. That is, it is the product of operant conditioning (reinforcement and punishment processes), which is an extension of natural selection in the ontogeny and has the same structure thereof (e.g., Skinner, 1953, 1974/1976).

(d) Neurophysiological structures and events fill the temporal and spacial gaps between the learning (operant and classical conditioning) processes and the behaviors stemming from them (cf., e.g., Skinner, 1953, p. 54; 1974/1976, pp. 236–238, 274; 1975, p. 43). To this extent, neurophysiological structures and events can explain *how* behavior happens, while variables from the three levels of selection of behavior explain *why* they happen (cf., e.g., Skinner, 1990, p. 1208).

(e) Some behaviors are covert, that is, not observable from a third-person look at the external part of the body (e.g., increase of pulse rate elicited by the presence of a predator, as a result of classical conditioning; working out "in the head" the cost of a travel, as a result of reinforcement history). What an organism does may not comprise external movements and still count as behavior, provided what the organism does stems from the same sorts of processes (i.e., obeys the same laws) that give rise to overt behaviors (cf., e.g., Skinner, 1953, p. 257ff; 1974/1976).

(f) Some stimuli take place inside the organism's body. These are stimuli we feel (e.g., dry throat, heart beating fast). The environment is not only what is outside the body, but rather everything that can have a (discriminative or eliciting) stimulus function. Stimuli can be exteroceptive, proprioceptive, or interoceptive.

[5] In Skinner (1953), however, we already find parallels between operant learning and natural selection.

In any case, they are physical phenomena (cf., e.g., Skinner, 1953, p. 261–262; 1974/1976, 24–25).

(g) States and events of the body which the organism feels or introspectively accesses are not causes of the organism's behaviors. Rather, in Skinner's (1974/1976) words, "feelings are merely collateral products of the conditions responsible for the behavior" (p. 52); and "[a]ll that a person comes to know about himself with their help [i.e., the help of interoceptive and proprioceptive systems] are just more stimuli and responses" (p. 238; cf. also, e.g., Skinner, 1969, pp. 257–260, pp. 267–268; 1971, pp. 12–13; 1975, pp. 43–44; 1989, pp. 13, 18).

(h) Some operant behaviors do not stem from direct contact with reinforcing or punishing variables, rather being governed by rules (e.g., instructions, sayings). Rule-governed behavior is a verbal behavior under the control of rules as discriminative stimuli, through reinforcement of rule-following (cf., Skinner, 1969, pp. 133ff).

Methodological Guidelines

(i) Behavior can be studied as a subject in its own right, rather than as an indirect means of finding out about other sorts of events in the organism's body (cf., e.g., Skinner, 1938, pp. 3–4, p. 418ff; 1955/1961b).

(j) Behavior analysis must discover behavioral laws though experimental inquiry, consisting of functional analysis; i.e., manipulation of independent variables, and observations of resulting changes in the organism's behavior as dependent variables. Well-established relationships between independent and dependent variables are functional relations, the fundamental target of behavior analysis's research (cf., e.g., Skinner, 1938, pp. 8–9, 433–435; 1953, pp. 23, 35; 1947/1961a, p. 225).

(k) Behavior analysis is to proceed via inductive methods to the discovery of functional relations, privileging the direct manipulation of variables in the environment. It should steer clear of hypothetical–deductive methods (cf., Skinner, 1938, pp. 44, 437; 1969, pp. viii–xii).

(l) Behavior analysis has also an interpretative, non-experimental part. Some variables lie beyond experimental control, most notably when it comes to behaviors outside the laboratory. To deal with such cases, behavior analysis needs to resort to interpretation work, always grounded upon the principles found out in the experimental context (e.g., principles related to schedules of reinforcement and stimulus control), as well as upon the ontological assumptions of the experimental context (e.g., Skinner, 1955/1961b, pp. 204, 206; 1957, pp. 10–12; 1971, pp. 22–23; 1974/1976, pp. 21, 194, 251–252).

(m) The vocabulary of behavior analysis must be limited to as few terms as needed, and these terms should have demonstrated "experimental reality." That is, terms that make unnecessary distinctions for predicting and controlling

behavior must be avoided; and terms from ordinary language can have a descriptive role in behavior analysis provided they are experimentally validated (cf. Skinner, 1938, p. 42).

(n) The use of vocabulary referring to a level of analysis other than that of behavior in its own right must be avoided in behavior analysis, no matter if this level of analysis is real (such as the physiochemical level of analysis) or hypothetical (such as a symbolic–computational level of analysis). Here included are OPCs, which, according to Skinner (as I shall highlight below), refer to a non-behavioral, hypothetical level of reality: "Traditional concepts are based upon data at another level of analysis and cannot be expected to prove useful. They have no place in a system derived step by step from the behavior itself" (Skinner, 1938, p. 441; see also Skinner, 1938, pp. 418ff; 1950, pp. 193–195; 1989, p. 18).

Main Axiological Guideline

(o) The ultimate goal and success criterion of a behavioral science should be the prediction and control (the latter in the sense of change, extinction, or generation) of behaviors, by means of functional analyses (cf. Skinner, 1953, p. 35; 1957, p. 12). As Skinner (1955/1961b) put it, the aim of behavior analysis is:

> to discover the functional relations which prevail between measurable aspects of behavior and various conditions and events in the life of the organism. The success of such a venture is gauged by the extent to which behavior can, as a result of the relationships discovered, actually be predicted and controlled. (pp. 203–204)

Skinner's Stand on OPCs in a Science of Behavior

I shall now reconstruct Skinner's view on the meaning of OPCs and his arguments against them in a science of behavior. It will be shown that Skinner does not have an unambiguous view of the meaning of OPCs (cf. Charles, 2011), save when it comes to the issue of the application of OPCs in a science of behavior. In this regard his position is clearly a form of behavioral eliminativism. By and large, he rejects the use of OPCs in a science of behavior (as will be shown shortly), although with a caveat: "A few traditional terms may survive in the technical language of a science, but they are carefully defined and stripped by usage of their old connotations" (Skinner, 1989, p. 18; cf. also Skinner, 1938, pp. 7–8).

Skinner on the Meaning of OPCs

Skinner's take on the meaning of OPCs is not entirely uniform. Occasionally Skinner seems to recognize that several of these categories have corresponding realities of a behavioral or physiological nature. However, other times he puts forward a behavioral eliminativism, as will be here explained. I shall call the

former approach "Skinner's positive approach," while the latter "Skinner's negative approach."

Skinner's positive approach. On the one hand, on some occasions Skinner seems to acknowledge the existence of emotions (e.g., fear, anger, contempt), moods (e.g., sadness, happiness), appetites (e.g., thirst), processes of perception (e.g., seeing, smelling), imagining and remembering, and several other phenomena we refer to by means of OPCs. In various passages of *Science and Human Behavior* and *About Behaviorism*, among other works, Skinner offers analyses of several of these phenomena as behavioral or (as the case may be) physiological phenomena. In any case, he rejects that they are "initiating causes" of behaviors, i.e., that they explain why we behave the way we do. Just to give an example, Skinner says of emotions: "We define an emotion … as a particular state of strength or weakness in one or more responses induced by any one of a class of [e.g., reflex] operations" (1953, p. 166). A person with fear of dead birds, for instance, is someone for whom probably

> the unexpected sight of a dead bird elicits very considerable reflex responses — blanching, sweating, change of pulse rate, and so on, as well as various expressions executed by the musculature of the face and body. If this were the extent of the phobia, we could give a complete description of it as a set of conditioned reflexes evoked by sight of a dead bird, but there are other important effects. The behavior of escape will be very powerful. Some of this — such as turning or running away — may be unconditioned or conditioned very early in the history of the organism. Some of it — calling upon someone to take the bird away, for example — is obviously of later origin. The rest of the repertoire undergoes a general change. If our subject was in the course of eating his dinner, we observe that he stops eating or eats less rapidly. If he was engaged in some other task, we observe a change which might be described as "losing interest." (1953, p. 167)

Roughly speaking, at such moments Skinner looks like Ryle (1949) talking about an OPC straightened out by behavioral principles, in keeping with guiding assumption (l) above (related to behavior analysis's interpretative dimension). The reality of emotions, moods, and so on is acknowledged, but understood fundamentally as interactions with the environment, instead of as inner causes of these interactions.

Skinner's negative approach. On the other hand, oftentimes Skinner claims that overall OPCs amount to outdated "explanatory fictions," meaning that attributions of mental phenomena in general refer to mistakenly supposed non-behavioral "initiating causes" of behavior (cf. Lazzeri and Oliveira–Castro, 2010). Accordingly, on this view OPCs are analogous to theoretical terms that have been abandoned in the history of science, such as "caloric," "ether," and "phlogiston," and their fate should be the same in behavioral science:

> To spend much time on exact redefinitions of consciousness, will, wishes, sublimation, and so on would be as unwise as for physicists to do the same for ether, phlogiston, or *vis viva* …. Old ways of speaking are abandoned with regret, and new ones are awkward and uncomfortable, but the change must be made. This

> is not the first time a science has suffered from such a transition. There were periods when it was difficult for the astronomer not to sound like an astrologer … and when the chemist had by no means freed himself from alchemy. We are in a similar stage in a science of behavior, and the sooner the transition is completed the better. (Skinner, 1974/1976, pp. 21–22)

In other words, according to this interpretation of OPCs, these categories are inherently mentalistic. That is, they refer to *supposed initiating causes of behavior (causes that give origin to them and explain why they happen) located inside us* (cf. Moore, 1990, 2013). They, Skinner suggests, are at odds with what truly explains why our behavior happens, viz., histories of natural selection, classical conditioning, reinforcement, punishment and related processes, as pointed out above — especially guiding assumptions (b), (c), (d) and (g). Thus, OPCs come down to an erroneous conception of the causes of behavior, so that the effort to translate them in behavioral science would be similar to physicists and biologists translating outdated theoretical terms already relinquished from their sciences (cf. also Skinner, 1958/1961c, p. 256).

Skinner offers several diagnostics of the mentalism inherent in OPCs. For example, he avers that since the role of the environment often is not clear in the determination of our actions, we tend to attribute our behaviors to hypothetical inner causes in terms of the vernacular. Since our actions do not stem from the current environment alone, but rather to a great extent from historical processes discontinuous in space and time — guiding assumptions (b) and (c) — it is easy to neglect these causes and hypothesize surrogate ones (cf., Skinner, 1971, pp. 16–18; 1974/1976, pp. 57–59; 1990, p. 1209). We may take our feelings as initiating causes, when in reality — as discerned in guiding assumption (g) — they are collateral effects of our interactions with the environment (cf., e.g., Skinner, 1971, pp. 15–16; 1974/1976, pp. 52–53).

The upshot is that sometimes Skinner suggests that OPCs mean behavioral and physiological realities, thereby recognizing the existence of emotions, cognitive events (broadly understood, i.e., uncommitted to a symbolic–computational modeling thereof) and so on (Skinner's approach), whereas other times he puts forward a sort of eliminativism (Skinner's negative approach). Differently from the brain-centrist variety of eliminativism (Churchland, 1988; Feyerabend, 1963; Rorty, 1965), it is a behavioral eliminativism, since it favors replacing OPCs mostly for a behavioral-level (and partly neurophysiological) vocabulary. This negative approach is particularly clear in Skinner's arguments against OPCs in a science of behavior, to which I shall now turn.

Skinner's Arguments Against OPCs in a Science of Behavior

Skinner offers at least four arguments for the thesis I shall henceforth call (~OPCs), which is implied by guiding assumption (n) above:

(~OPCs) By and large OPCs should not be used in a science of behavior.

The arguments are that OPCs: (i) amount to an erroneous conception of behavior; (ii) encumber the prediction and control of behavior; (iii) lead to wasteful research; and (iv) are incongruous with scientific vocabulary. (Importantly, Skinner does not extend [~OPCs] to ordinary, non-scientific contexts. Cf. Skinner, 1989, p. 18.) In the following, I endeavor to reconstruct the arguments analytically.

(i) *The argument that OPCs amount to an erroneous conception of behavior.* One of Skinner's arguments for (~OPCs) goes thusly. Ordinary psychological categories are references to hypothetical inner causes purporting to explain why a particular behavior happens. Still, things that actually explain why a particular behavior happens lie elsewhere (viz., conditioning histories, schedules of reinforcement, and so on) — as seen in the ontological guidelines above. Hence, OPCs amount to an erroneous conception of behavior. They are like outdated and abandoned theoretical terms in the history of science (phlogiston, etc.). Thus, (~OPCs) [cf. also Lazzeri and Oliveira–Castro, 2010].

This argument is put forth in the passage quoted above from Skinner (1974/1976, pp. 21–22 (where he writes "To spend much time on exact redefinitions …"). Also in Skinner (1971), wherein he calls into question the persistence of "this prescientific way" of talking in the scientific domain of human behavior (psychology, economics, anthropology and so on):

> Physics did not advance by looking more closely at the jubilance of a falling body, or biology by looking at the nature of vital spirits, and we do not need to try to discover what personalities, states of mind, feelings, traits of character, plans, purposes, intentions, or the other perquisites of autonomous man really are in order to get on with a scientific analysis of behavior. (p. 15)

In semi-formal terms:

(i.i) OPCs are mentalistic terms (i.e., they refer to supposed states and events inside the body that explain why we behave the way we do). [Premise]
(i.ii) What explains why behaviors happen lies elsewhere from where mentalism implies. [Premise, related to behavior analysis's standard ontological assumptions]
(i.iii) If (i.i) and (i.ii), then OPCs amount to an erroneous conception of the causes of behavior [Premise]
(i.iv) OPCs amount to an erroneous conception of the causes of behavior. [From (i.i)–(i.iii)]
(i.v) If (i.iv), then (~OPCs). [Premise]
∴ (~OPCs). [From (i.iv) and (i.v)]

(ii) *The argument that OPCs encumber the prediction and control of behavior.* A second argument for (~OPCs) advanced by Skinner is similar to (i) but relies

more directly upon (o) mentioned above, behavior analysis's main axiological assumption (whereby its goal and success criterion should be prediction and control). This argument, which is the most frequent one among the four under scrutiny, goes as follows: the use of OPCs in a science of behavior encumbers the prediction and control of behavior. These categories divert the scientist's attention from the determinants of behavior to the organism's insides. They mislead the scientist into thinking that determinants thereof lie inside the organism, thereby making the scientist feel satisfied with explanations that actually do not trace behavior to the organism's history of interaction with the environment. The practice of explaining behavior along mentalistic terms "is dangerous because it suggests that we have found the cause and therefore need search no further" (Skinner, 1953, p. 31; cf. also Skinner, 1950, p. 194; 1971, pp. 12–13; 1988; 1990, p. 1209; see also Moore, 1990). Skinner makes clear his critique is not against the unobservable character of causes adduced by mentalistic explanations:

> [S]upported by the false sense of cause associated with feelings and introspective observations, mentalism has obscured the environmental antecedents which would have led to a much more effective analysis.... The objection to the inner workings of the mind is not that they are not open to inspection but that they have stood in the way of the inspection of more important things. (1953, p. 182)

> The nature of any real or fancied inner cause of behavior is not at issue; investigative practices suffer the same damage in any case....Inner entities or events do not"cause" behavior, nor does behavior "express" them. At best they are mediators, but the causal relations between the terminal events which are mediated are inadequately represented by traditional devices. Mentalistic concepts may have had some heuristic value at one stage in the analysis of behavior, but it has long since been more profitable to abandon them. In an acceptable explanatory scheme the ultimate causes of behavior must be found *outside* the organism. (1958/1961c, pp. 252–253; italics in the original)

In semi-formal terms:

(ii.i) If (i.i) and (i.ii) [i.e., if OPCs are mentalistic terms, but what actually explains the origin of behaviors lies elsewhere from where mentalism implies], then OPCs encumber a more efficient prediction and control of behavior. [Premise]
(ii.ii) (i.i) and (i.ii) are the case. [Premise]
(ii.iii) ∴ OPCs encumber a more efficient prediction and control of behavior. [From (ii.i) and (ii.ii)]
(ii.iv) If (ii.iii), then, if a science of behavior must aim at predicting and controlling behavior, then (~OPCs). [Premise]
(ii.v) A science of behavior must aim at predicting and controlling behavior. [Premise, related to behavior analysis's standard guiding axiological assumption, (o) above]
∴ (~OPCs). [From (ii.iv) and (ii.v)]

(iii) *The argument that OPCs in a science of behavior lead to wasteful research.* A third argument for (~OPCs) put forward by Skinner relies more directly upon the methodological assumptions he suggests, especially (k): the prescription of inductivism. According to this argument, OPCs in a science of behavior lead to wasteful research. Theories adopting them tend to be overthrown, and research associated with them end up establishing little or no result, in spite of much time and effort expended.

> Research designed with respect to [a mentalistic] theory is … likely to be wasteful. Much useless experimentation results from [mentalistic] theories, and much energy and skill are absorbed by them. Most [mentalistic] theories are eventually overthrown, and the greater part of the associated research is discarded. (Skinner, 1950, p. 194)

According to Skinner (1950, pp. 194–195), this is because mentalistic theories imply hypothetical–deductive procedures ("stages of hypothesis, deduction, experimental test, and confirmation"). If we did not have a productive alternative, then the use of OPCs for behavioral research could be justified. Yet, Skinner (1950) claims, we do have one, viz., the inductive method, centered upon the direct manipulation of variables, which, he believes, is a more economic and effective means of finding out laws of behavior. In semi-formal terms:

(iii.i) The use of OPCs in a science of behavior depends upon the adoption of hypothetical–deductive procedures. [Premise]
(iii.ii) If (iii.i), then the adoption of OPCs in a science of behavior leads to wasteful research. [Premise]
(iii.iii) ∴ The adoption of OPCs in a science of behavior leads to wasteful research. [From (iii.i) and (iii.ii)]
(iii.iv) The adoption of inductive method yields a more effective means of finding out laws of behavior. [Premise, related to behavior analysis's standard methodological assumptions, especially (k)]
(iii.v) If (iii.iii) and (iii.iv), then (~OPCs). [Premise]
∴ (~OPCs). (From [iii.iii]-[iii.v])

(iv) *The argument that OPCs are incongruous with scientific vocabulary.* Finally, a fourth argument Skinner advances for (~OPCs) goes thus: by and large OPCs do not meet what we should expect of a scientific vocabulary. Influenced by Bacon (1620/2000) and Mach (1919) [cf. Smith, 1986, p. 259ff; 1995; also Skinner, 1987], Skinner holds that a scientific vocabulary must be as economic as possible (have as few terms as possible), and contain only terms having demonstrated experimental realities, i.e., terms referring to events or relations among them effectively observed by way of experiment (e.g., "reinforcement") [as seen in the methodological assumption (m)]. Ordinary psychological terms, however, are countless, and

several of them overlap one another (e.g., "to like," "to want," and "anger," do not differ significantly from "to appreciate," "to desire," and "irritation," respectively). They are vague, imprecise terms, and overall they imply distinctions we should not expect to find counterparts of in nature (cf. Skinner, 1938, pp. 7–8, 41–43; also Baum and Heath, 1992, p. 1314). In Skinner's (1938) words:

> The vernacular is clumsy and obese; its terms overlap each other, draw unnecessary or unreal distinctions, and are far from being the most convenient in dealing with the data. They have the disadvantage of being historical products, introduced because of everyday convenience rather than that special kind of convenience characteristic of a simple scientific system.... There is only one way to obtain a convenient and useful system and that is to go directly to the data. (1938, p. 7)

In semi-formal terms:

(iv.i) If OPCs were to have a place in the vocabulary of a science of behavior, then they would be in a parsimonious vocabulary, and we should expect them to have demonstrated experimental realities. [Premise, related to methodological assumption (m)]
(iv.ii) OPCs are not a parsimonious vocabulary, and we should not expect OPCs to have demonstrated experimental realities. [Premise]
∴ (~OPCs). [From (iv.i) and (iv.ii)]

An Appraisal of Skinner's Stand on OPCs in a Science of Behavior

Once one goes along with Skinner's suggested guidelines and his negative approach on the meaning of OPCs, (~OPCs) ensues. I shall take issue with (i)–(iv) fundamentally by disputing the negative approach.

Arguments (i) and (ii) clearly hinge upon the premise that OPCs are inherently mentalistic (premises [i.i] and [ii.ii] in the reconstruction thereof above. Like typical dualistic (e.g., Descartes on the *Meditations*), neurophysiological (e.g., Armstrong, 1968) and cognitive–computational views of OPCs (e.g., Fodor, 1975; Neisser, 1967), Skinner thinks these terms connote non-behavioral inner states and events that explain why we behave the way we do (cf. Aizawa, 2015). The difference, it goes without saying now, is that he denies there are corresponding *denotata* for such connotations: he takes up an eliminativist stance.

I think we should resist this premise.[6] As Deitz and Arrington (1984) suggest (see also Deitz, 1986, 1988), there are good reasons to doubt the apparently intuitive idea that OPCs are mentalistic, and to rather embrace a portrayal of OPCs more akin to Skinner's positive approach (in the sense previously explained). In

[6] This is an important point of convergence, I believe, between behavioral approaches such as the one I favor and some radical forms of 4E approaches, such as Hutto and Myin's approach (2013).

other words, there are reasons to believe OPCs are actually harmonious with Skinner's guiding assumptions. Granted, as Skinner illuminatingly pointed out, our culture inherits deplorable mentalistic ways of understanding our conduct (e.g., to ascribe outstanding skills to a person's "geniality" as something mysterious the person is endowed with). There is no doubt that mentalism underwrites some usages of OPCs. Yet, the logical geography (to borrow Ryle's [1949] terminology) of OPCs (i.e., the basic nuances related to their employment in ordinary contexts) is consistent with a behavioral account thereof, regardless of some claims to the contrary (see Lazzeri, 2016). Ryle (1949) and Wittgenstein (1953), among others, provided analyses of OPCs revealing these terms are actually at odds with mentalism (although they did not use the terminology of "OPCs" and "mentalism"). Thus, OPCs are not to blame for mentalism. As Deitz and Arrington (1984) put it:

> Wittgenstein showed us that when cognitive terms are examined within the language-game that is their original home, they are found to be neither mentalistic nor dualistic; certainly they are not "computeristic." They describe behavior in context.... While Skinner's … criticism of cognitive psychology is correct, it rests upon agreement that cognitive terms mean what cognitive psychologists say they mean.... Cognitive terms are more amenable to use by behaviorists ....[I]t is behaviorists who can most properly study what those terms actually refer to. This not only legitimizes behaviorism as a study of cognition, it also opens up vast new areas of interesting research for behaviorists. (p. 12)

Deitz and Arrington's claim is focused upon cognitive terms from folk psychology (such as "belief," "reasoning," "remembering"), but I believe their claim can be generalized to other OPCs (terms for emotions, personality traits and so forth). Since I have extensively argued elsewhere for the thesis that OPCs are coherent with a positive behavioral approach to them (Lazzeri, 2015a, 2015b, 2016, Lazzeri and Oliveira–Castro, 2010), I shall here limit myself to provide a summary of some main inter-related points.

Consistency with the mereology of OPCs. As Ryle (1949) and Wittgenstein (1953) remarked (see also Bennett and Hacker, 2003), in general OPCs make up sentences that are applied with sense only to entire living beings. For instance, it is the dog (as a whole) that is hungry, not its brain (or any other part of the organism); it is the owl that cares for its young, not its brain. Similarly, in general, behaviors are things exhibited by the organism as a whole. The logical geography of these concepts reveals that just like we cannot find activities of barking, digging, bar pressing, or hooting in a part of the organism, so we cannot find hunger, love, reasoning, and so on inside the organism, contrary to the mentalist suggestion.

Consistency with the abstract character of OPCs. As a matter of logical geography, phenomena such as desire, fear, and reasoning are not the sort of thing one can move from one place to another or be some centimeters distant from other things. So are behaviors, taken singularly (e.g., the occurrence of an eye-blink

reflex), in chains (e.g., bar-pressing), or molar aggregates spread out in time and space (e.g., traveling) [Lazzeri, 2015a, 2015b]. On the other hand, some entities privileged by mentalistic accounts, such as hypothesized symbols and symbolic processes in the brain (or even in the extraneural structures of the extended mind view), do not seem to share this feature.[7] They are conceived to be stored and retrieved in the brain; accordingly, they could, for example, be distant from the ears and the feet.

Consistency with the criteria for mental attributes. As Ryle (1949) greatly elaborates and Deitz and Arrington (1984) highlight, we decide whether or not our descriptions formed by OPCs are true according to factual or presumed past and future occurrences of relevant behaviors in relation to the environment. If an owl persistently shrieks at and attacks someone who approaches its nest, we may say it is angry and desiring to protect its young. In the absence of such or comparable behaviors and contexts, it simply does not make sense to ascribe these attributes. This is inconsistent with mentalistic intuitions, whereby those attributes are things like neural or symbolic processes in the owl's head, which could take place without behaviors. People can hide some of their intentions and other mental attributes (i.e., attributes in terms of OPCs), but that is consistent with a behavioral understanding thereof: when this happens, either some overt behaviors that make up these phenomena are concealed from others (in space and time), or else they amount to chains of covert behaviors, as the case may be.

Consistency with the episodic and dispositional features of OPCs. Some ordinary psychological verbs, such as "to reason about" and "to remember," denote activities the organism engages in — they are episodic terms. If somebody thinks about how much money she will spend on a holiday travel, she engages in mathematical tasks with fingers, pencil and paper, a calculator, or in the limit covertly altogether. In any case, such mathematical tasks comprise behaviors stemming from a history of reinforcement for this sort of task, to which we are usually introduced early in life. The typical mentalist, however, implies that besides engaging in these behaviors the person is engaged in some inner non-behavioral activity of symbol manipulation or the like for which these behaviors are only auxiliary. For this reason the mentalist is accused of either falling into an infinite regress (Ryle, 1949) or (by Skinner [1969] himself) of implying homunculi.

Some OPCs, such as "to intend to" and "to be angry with," do not denote things the organism does (e.g., differently from remembering, which is an activity; to be angry is not something we do). They are dispositional, rather than episodic, terms. That is, they form sentences which mean that if certain contexts or situations were the case, then certain behaviors would be likely to occur (Ryle, 1949). Someone who intends to be a professional philosopher may be swimming at this moment,

[7] Cf. Clark and Chalmers (1998).

which means that she reads and discusses philosophical ideas etc. in some contexts that afford these behaviors. Dispositional psychological terms, thus, are amenable to a behavioral approach too — as references not to individual activities or chains of activities, but rather to aggregates thereof spread in time and space.

This goes to show that OPCs are not mentalistic. If I am right, arguments (i) and (ii) do not go a long way. For premise (i.iv) [i.e., that OPCs amount to an erroneous conception of the determinants of our conducts], as well as premise (ii. iii) [i.e., that OPCs encumber a more efficient prediction and control of behavior], hinge upon Skinner's reading of OPCs as tantamount to a mentalistic vocabulary.

Now, what about arguments (iii) and (iv)? Prima facie, Skinner's view of OPCs as mentalistic terms underpins (iii.i), the premise that adopting OPCs in behavioral science depends on acceptance of hypothetical–deductive procedures. If OPCs were supposed to refer to inner phenomena hypothesized as initiating causes of behavior, taking them in would irremediably involve speculations beyond the behavioral-level of analysis. As I have suggested, there are good reasons to call this premise into question, from which it does not follow (iii.iii), that OPCs in a science of behavior lead to wasteful research. In Arrington and Deitz's (1986) words: "[O]nce the behavioral basis of cognition is recognized, behaviorists can embrace the study of cognition without sacrificing any of their scruples" (p. 104).

If (iii.i), appearances to the contrary, does not hinge upon Skinner's view of OPCs as mentalistic, (iii.iii) does not follow either. We can grant that even a behavioral understanding of OPCs along the lines of Skinner's positive approach could imply some degree of hypotheses testing if OPCs were used in behavior analysis. Still, that would not be too different from terms commonly used in behavior analysis for identifying its acknowledged dependent variables. In a behavioral understanding, OPCs by and large refer to such dependent variables (and their relations to independent variables) at the behavioral-level of analysis. Their adoption would not be more wasteful than the currently acknowledged terminology in the area, provided they are likewise properly delimited. This said, although we are to keep up the focus on the direct manipulation of variables, the distinction between theory and observation may not be as clear-cut as Skinner supposed. Observation is to a greater or lesser extent embedded in theory, as philosophers of science are found of pointing out (e.g., Chalmers, 2013). A strong, excluding dichotomy between inductive and hypothetical–deductive methods is not satisfactory.

When it comes to argument (iv), it may seem hard to resist premise (iv.i), which implies that the vocabulary of science, particularly an experimentally driven science, must be parsimonious and have experimental counterparts in reality. Indeed, I do agree it is reasonable to pursue this methodological rule in a science of behavior to some extent. Ordinary psychological categories are in fact elastic, rather than precise concepts. Yet so are many of the concepts we use to pick out molar behaviors and behavior patterns in behavior analysis itself;

for instance, concepts we use to speak of certain behaviors as being of foraging, aggression, building a house, or teaching (all of them used to identify behaviors in Pierce and Cheney, 2004). Furthermore, as I have argued, OPCs refer likewise to behavioral events as much as the currently employed vocabulary for picking out dependent variables. Hence, OPCs can have (if not in practice at least in principle, as the case may be), counterparts in experience. I submit that a *modified version* of Rachlin's two arguments for OPCs in behavior analysis, which I shall reconstruct in the following, strengthen this point.

Rachlin's Teleological Behaviorism

Rachlin on the Meaning of OPCs

Rachlin states the main thesis of his teleological behaviorism as follows: "[M]ental terms refer to overt behavior of intact animals. Mental events are not supposed to occur inside the animal at all. Overt behavior does not just *reveal* the mind; it *is* the mind. Each mental term stands for a pattern of overt behavior" (1994, p. 15; italics in the original). Thus, according to this approach, OPCs refer to patterns of behaviors observable in the external part of the body. For example, an organism's hunger is understood as a pattern entirely composed of overt behaviors that tend to favor eating certain sorts of food and avoid situations that usually delay feeding. In this view, even sensations are patterns of overt behavior. So, if I scratch an itch, my scratching behavior reveals the itch itself.

Rachlin (e.g., 1994, p. 28; 2012a, p. 10; 2012b, p. 135) explicitly disallows Skinner's idea of covert behaviors. Rachlin apparently thinks that the acceptance of covert behaviors in the analysis of OPCs is tantamount to accepting these categories as references to private phenomena, not differently from mentalistic approaches. He seems to suggest that what Skinner calls covert behavior should actually not count as behavior, for the reason that it is not in direct contact with reinforcing or punishing consequences (cf. Dougher, 2016).

Rachlin (1994) presents his approach by contrasting efficient and final causes. He claims behavior patterns and thereby mental attributes are final causes, while neurophysiological phenomena are efficient causes of behavior. Final causes, he says, exhibit the logic of *fitting into*, whereas efficient causes exhibit that of mechanistic, "billiard-ball" relations. The logic of fitting into is that of an element forming a larger (more extended in time) pattern. For instance, the behavior pattern of playing a concerto movement is understood as a final cause of playing each of the movement's subparts. The existence of these subparts as components of the concerto movement depends upon the existence of the larger whole they make up over time. Some neurophysiological structures and processes support the musician's behavior, but what defines this behavior as that of playing a certain concerto movement lies elsewhere.

These teleological remarks are intended to support a sort of molar operant analysis of OPCs. As Rachlin (1995a, p. 116) says, "The behavioral definition in each case rests on common consequences of the behavior — common contingencies," and:

> Aristotle's classification of movements in terms of final rather than efficient causes corresponds … to Skinner's conception of an *operant* as a class of movements with a common end…. This Aristotelian conception, the *operant* … shifts the focus of behavioral investigation away from efficient causes … toward final causes — contingencies of reinforcement. (Rachlin, 1994, pp. 83–84; his italics)

Thus, Rachlin understands final causes of behavior as molar contingencies of reinforcement. This interpretation of final causes entails that behavior patterns, hence mental attributes (remember that for Rachlin "Each mental term stands for a pattern of overt behavior"), are aggregates of (for Rachlin only overt) behaviors under contexts to which these behaviors become associated by virtue of the consequences they produce. They are not defined by the properties of the organism's body, but rather by the consequences behaviors produce which change the probability of their occurrence in similar contexts. Apparently, Rachlin gives no relevance to reflexes in the analysis of OPCs.

Rachlin's Claim for OPCs in Behavior Analysis

Rachlin (1995b) states that "The crucial issue between the two [teleological behaviorism and radical behaviorism] is whether mental terms belong in a scientific psychology. Teleological behaviorism claims they do; Skinnerian behaviorism claims they do not" (p. 180). Rachlin presents at least two arguments for the thesis I shall henceforth call "(OPCs in behavior analysis)":

> (OPCs in behavior analysis): OPCs should be accepted in behavior analysis for picking out dependent variables.

His two arguments are that OPCs: (v) refer to behavioral-level phenomena, besides being useful for prediction and control; and (vi) have a cultural appeal that must be taken into account, on pain of marginalization of behavior analysis.

(v) *The argument that OPCs refer to behavioral-level phenomena, besides being useful for prediction and control.* Rachlin (1988, 1995b) suggests that, since the meaning of OPCs comes down to operant patterns of overt behavior, OPCs can be useful for prediction and control in behavior analysis. Accordingly, they are actually consistent with behavior analysis's guidelines.

In their review of Baum (2005), who shares Rachlin's emphasis upon patterns of overt behavior extended in time but accepts (~OPCs), Rachlin and Frankel (2009) claim:

> Baum does not go far enough in the behavioral analysis of everyday language.... But a developed science of behavior will eventually have to confront the pragmatic fact that life in the modern world would be almost impossible without the behavioral distinctions that those terms [i.e., mental terms] make.... We believe that it is possible to avoid mentalism without banishing mentalistic terms from a science of behavior. Skinner ceded such terms to less pragmatic, more mentalistic psychologies; this led people to believe that a behavioristic science could not deal with the aspects of their lives that most concerned them when the exact opposite was and is the case. As this book so elegantly shows, Baum's molar, functional view enables a deep understanding (in terms of behavioral prediction and control) of all aspects of life, including ones usually regarded as mentalistic. (pp. 136–137)

In other words, once the behavioral character of OPCs are grasped, they turn out compatible with and useful to behavior analysis. Compatible because they do not violate behavior analysis's basic assumptions. Useful because they can help us distinguish molar dependent variables of widespread interest, which otherwise could hardly be identified.

In semi-formal terms:

(v.i) OPCs refer to patterns of overt behavior and are useful for predicting and controlling behavior. [Premise]
(v.ii) If (v.i), then, provided OPCs are used correctly, then (OPCs in behavior analysis). [Premise]
∴ Provided OPCs are used correctly, then (OPCs in behavior analysis). [From (v.i) and (v.ii)]

(vi) *The argument that OPCs have a cultural appeal that should be taken into account.* Rachlin holds that Skinner's eliminative stance toward OPCs leads many people into thinking that behavior analysis neglects the phenomena OPCs refer to (that is, moods, perception, thinking, memory and so on), which are of great interest in their lives. Rachlin (2012b) denies that "it is better after all to eliminate all mental terms from our scientific vocabulary," on the grounds that "it is the acceptance of [this idea] … by behaviorists that has led to the marginalization of behaviorism within academic experimental psychology and its demonization within philosophy" (p. 135). There is also a glimpse of this argument in Rachlin and Frankel's (2009) passage quoted above ("this led people to believe that a behavioristic science could not deal with the aspects of their lives that most concerned them").

In semi-formal terms:

(vi.i) Let's suppose ~(OPCs), i.e., denial of (OPCs in behavior analysis). [By hypothesis]

(vi.ii) If (vi.i), then behavior analysis will not be willing to study mental phenomena. [Premise]
(vi.iii) Behavior analysis will not be willing to study mental phenomena. [From (vi.i) and (vi.ii)]
(vi.iv) If (vi.iii), then behavior analysts will be accepting the marginalization of behavior analysis. [Premise]
(vi.v) ∴ Behavior analysts will be accepting the marginalization of behavior analysis. [From (vi.iii) and (vi.iv)]
(vi.vi) Behavior analysts should not accept the marginalization of behavior analysis. [Premise]
∴ (OPCs in behavior analysis). [From (vi.i), (vi.iv), (vi.v), *reductio*]

An Appraisal of Rachlin's Approach

I agree with teleological behaviorism that OPCs should be accepted in behavior analysis for picking out behavioral variables of interest. Nonetheless, I take issue with four features of this approach when it comes to its claims about the meaning of OPCs (cf. Lazzeri, 2015b).

(1) I submit that Rachlin's dismissal of covert behaviors is mistaken. Skinner's recognition of covert behaviors [guiding assumption (e) above] is reasonable as well as fundamental for a thorough analysis of OPCs.[8] For instance, if one is at this moment working out a multiplication without exhibiting any overt behavior, one is engaging in an episodic activity nevertheless. Such an activity is at least part of what we mean by saying that the individual is thinking, reasoning, or calculating. Interpreting all mental phenomena (i.e., phenomena we refer to by means of OPCs) as covert behaviors alone is misguided, since this is incoherent with their basic nuances, as it should be clear from my remarks in the section "An Appraisal of Skinner's Stand on OPCs in a Science of Behavior." However, it is quite another thing to claim that mental phenomena are sometimes (partly or entirely) made up by covert behaviors and, therefore, are sometimes (partly or entirely) inner.

Furthermore, as Dougher (2016, pp. 261–262) has argued, even if operant covert behavior is not in direct contact with reinforcing and punishing consequences, it is surely indirectly influenced by them. For example, covert verbal behavior may be influenced by verbal reinforcing or punishing stimuli. A child's behavior of sub-vocally working out a mathematical problem can be influenced

[8] A caveat: if a behavior is covert, it does not mean that it is entirely unobservable. To be sure, it can be measured and observed, at least to some extent, by proper neurophysiological instruments (cf. Ortu, 2012; Silva, Gonçalves, and Garcia–Mijares, 2007). Covert behaviors, as I understand them, obey the same laws of overt behaviors, but are not observable on the external part of the body (see Lazzeri, 2015b).

by contingencies such as a teacher's reinforcement for providing the right answer. Curiously, save by Rachlin's confinement to external events, this line of reasoning is consistent with Rachlin's own molar understanding of contingencies, whereby the probability of occurrence of a given response can be determined by consequences that have not taken place immediately after the response.

(2) As I mentioned above, some OPCs are episodic. In other words, some mental phenomena take place in the here-and-now, instead of spread out in different moments. When we say that someone is working out a multiplication (be it covertly or overtly), we mean that the person is *engaged now in a momentary activity* with the function of solving the multiplication. One's ability to calculate can be understood as a behavior pattern, which has a dispositional character. What we have here, however, is the exercise of the ability, which is something episodic (like swimming right now is different from the ability to swim). Teleological behaviorism, as it stands, does not seem to grasp the episodic nuances of such and similar cases, rather implying that every OPC is dispositional.

(3) Rachlin does not seem to give any importance to kinds of behaviors other than operants. I suggest that reflexes — here including kineses, taxes, modal action patterns and reaction chains as forms of reflexes — are the raw material of mental phenomena as well. For example, a maggot's (taxis) behavior of turning right, given bright light in the opposite direction, composes an instance of wanting protection against the threat. These other sorts of behavior are criteria for attributions of mental phenomena as much as operant behavior.

(4) Last, like Skinner in his positive approach to OPCs, I believe we should not dismiss the possibility that some OPCs refer partly to neurophysiological events besides behaviors. Particularly when it comes to sensations (by which I mean things we feel in parts of the body, such as pains and tickles), one can hardly not take into consideration physiochemical events to fully account for them. Otherwise the qualitative properties ontologically inherent to sensations are left out.

If am correct, then (v), Rachlin's first argument for (OPCs in behavior analysis), is on the right track, with a caveat. Premise (v.1) [whereby OPCs refer to patterns of overt behavior] should give place to a *more accurate behavioral picture of OPCs*, which, among others things, considers not only overt behaviors, but also covert ones, as well as episodic behaviors, and not only molar patterns thereof.

Finally, concerning (vi), Rachlin's second argument for (OPCs in behavior analysis), I believe it is safe to say that Skinner's eschewal of OPCs has played a role in the marginalization of behavior analysis. The fact that other research traditions in psychology, as well as common-sense, attach great importance to the modeling of different sorts of mental phenomena, increases the importance of this modeling for behavior analysis, as Laudan's (1977) problem-solving view of scientific theories predicts. (In Laudan's terminology, this comes down to an external conceptual problem for behavior analysis.) Hence, Rachlin is right that the adoption of OPCs in behavior analysis would increase its acceptance.

Yet, I would raise one important caveat to the argument (vi): it goes a long way provided it is built upon a proper behavioral understanding of OPCs as premise. If Skinner's negative approach were right, we would have reason to hold (~OPCs), notwithstanding the unpopularity of this stance. It would make little sense to give in to the appeal of OPCs if OPCs amounted to a mentalistic conception of behavior; for in that case they would be irreconcilable with behavior analysis's ontological assumptions.

Wrap Up

After bringing to the fore some of behavior analysis's standard guiding assumptions, this paper distinguished between two different stances Skinner takes toward OPCs: a positive one, which acknowledges the reality of referents of sentences couched in OPCs as behavioral and physiological events; and a negative one, whereby OPCs are mentalistic. The latter underwrites Skinner's four arguments for (~OPCs), to wit: (i) OPCs amount to an erroneous conception of behavior; (ii) they encumber the prediction and control of behavior; (iii) they lead to wasteful research; and (iv) they are at odds with scientific vocabulary.

Skinner's assumption that OPCs are mentalistic is highly doubtful. The logical geography of OPCs is actually harmonious with a behavioral modeling of them, and thereby with behavior analysis's guiding assumptions in general. Once (along with Skinner's positive approach) their behavioral character is recognized, OPCs turn out compatible with behavior analysis's (ontological, methodological, and axiological) guiding assumptions.

Accordingly, OPCs, once properly understood, should find place in behavior analysis. For one reason, OPCs turn out potentially useful for picking out behaviors and patterns thereof which otherwise could hardly be identified. For instance, how could we pinpoint a sad mood, as a singular phenomenon, by identifying several actual and potential behaviors involved, without saying the organism is sad? For another reason, a proper adoption of OPCs by behavior analysis would solve a considerable (external) conceptual problem (in Laudan's sense), related to the enormous value most people in our culture and other scientific research traditions attach to these categories. It makes perfect sense to expect from a behavioral science research tradition models of such hugely important phenomena as emotions, moods, and reasoning processes.

Rachlin's teleological behaviorism holds a similar view to the one here favored, but with some significant differences. Rachlin claims that OPCs mean patterns of overt behavior, and offers two reasons for the acceptance of OPCs in behavior analysis: (v) since they refer to variables behavior analysis recognizes, they are congenial with and useful for the goals of this research tradition; and (vi) they have a cultural appeal that must be taken seriously in a science of behavior. Four features of teleological behaviorism are found wanting: (1) Rachlin's dismissal

of covert behaviors; (2) his neglect of the episodic character of some sentences formed by OPCs; (3) his consideration only of operant behavior and neglect of reflexes; and (4) his dismissal of physiological events as possible components of some mental phenomena (we could hardly account for sensations, in particular, without taking physiological events as their constituents). Thus, (v) and (vi) can be much improved with a more accurate behavioral modeling of OPCs that takes (1)–(4) into account (cf. Lazzeri, 2015b).

A conceptually sound and cautious adoption of OPCs by behavior analysis would enlarge behavior analysis's already great problem-solving effectiveness. The standard eschewal of OPCs in behavior analysis has left to many people the detrimental impression that it is a psychology without mental phenomena. Actually, behavior analysis holds promise as, among other things, a parsimonious and empirically adequate (as this term is used in philosophy of science) understanding of mental phenomena, congenial with OPCs logical geography.

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