

No Need for Private Events in a Science of Behavior: Response to Commentaries

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At the outset of any discussion of private events in a science of behavior, we should recognize a few basic points. First, *private* means, by definition, unobservable by another. As scientists who study the behavior of organisms, we are almost always the other, apart from the subject of study. We cannot observe private events in the subject, by definition. Any private events we may speak of, then, are guessed at, inferred, hypothetical constructs. The guesses and inferences we might make are unverifiable and likely to be unreliable.

Second, if we ask a human about private events (or interpret public behavior as indicative of private events in a nonverbal organism), we encounter other sources of unreliability: error and deceit. Whatever a person may say, even if the person is honest, he or she may change stories depending on circumstances, and beyond that, people often lie when queried about private matters. The utterances have effects on people who hear them, but they throw no light on any private events they may seem to be “reporting.” Thus, first-person verbal accounts cannot boost the reliability of measurements of private events.

Where does that leave a behavior analyst in relation to private events? The third point is that because whatever we say or hear about private events is unreliable, they cannot be measured, and, thus, they have no place in a science.

Yet, many behavior analysts, including four of the five of these

commentators, are reluctant to come to this obvious conclusion. Most likely, the reason is that everyone who speaks the English language *knows*, by way of folk psychology, that everyone has private events (i.e., thoughts and feelings). In the Middle Ages, and until at least the 18th century, hardly anyone doubted, for the same reason, that everyone had a soul that departed when the person died. We now may doubt that this is so, but it seemed self-evident, the same way that everyone’s possession of private thoughts and feelings today seems self-evident.

The main point of my article is that, whether or not anyone has private events, and whether or not anyone has a soul, neither private events nor souls belong in a science of behavior. The reason is simply that they cannot be measured, and science deals with data, with measurable events. (Although, if one followed Rachlin’s, 2011, lead, then they can be measured, but as extended patterns of public behavior.) Even in sciences with well-developed theories (e.g., physics), theoretical predictions must be tested against measurable phenomena. From a pragmatic point of view, the reason for excluding unmeasurable events is that they provide no useful advance toward improving our understanding of behavior; instead, they offer all manner of opportunities for obfuscation and confusion.

In the article, however, I show that the omission of private events is no loss, because behavior is extended in time, and once we shift to talking about extended activities (instead of discrete responses), we discover that

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no need arises for private events. Indeed, for all practical purposes and for a science grounded in evolutionary theory (as behavior analysis ought to be), private events are irrelevant.

Palmer (2011) and Marr (2011) defend the inclusion of private events in behavioral accounts on familiar grounds: that *interpretation* is a vital part of science. To this, I reply, "Are we practicing science, or are we telling fairy tales?" Marr goes so far as to say that scientific accounts of behavior are "anemic." By what standard? If we say that the moon is a chunk of rock, that might seem to negate a lot of poetry, but the understanding of the phases of the moon, its rising and setting, its effects on tides, and so on would seem to offer a richness that matches the expressions of poetry. So too with accounts of behavior. When we see a person behaving honestly, we need not appeal to some inner virtue, as a poet might. We enrich the account by looking at the effects of the honest behavior on people and society around the individual and by understanding its origins in a history of interaction with the social environment. I hardly think that is "anemic." Moreover, Marr fails to distinguish between literature and science. He writes, "Joyce's *Ulysses*, for example, a novel largely comprised of 'interior monologues,' and considered thereby 'realistic' by some critics" (p. 214), as if this work of literature is in any way touched by exclusion of private events from a science of behavior. Does he see no difference between the goals of literature and the goals of science? If no difference exists, then we are in big trouble.

We need to distinguish among prediction, scientific explanation, and speculation. The example of Neptune's prediction and discovery is instructive, but not for the reasons that Palmer gives. Neptune's existence was predicted by a well-developed quantitative theory of the solar

system and moving bodies in general. When we see phenomena outside the laboratory, as in astronomy or everyday life, we may explain them by bringing to bear scientific concepts derived from laboratory research. So, we understand earthquakes as arising from the collision of tectonic plates and tides as arising from the gravitational force of the moon. That is scientific explanation. But interpretation, not as Palmer defends it, because he conflates prediction and scientific explanation with interpretation, is simply speculation, having a basis neither in well-developed theory nor in laboratory research. Speculating about other creatures' private events is as unconstrained as speculation about any other unverifiable entities. Does thinking have the same properties as lever pressing? Who knows? That each person possesses a soul that leaves at death seems to jibe pretty well with the facts, but it is just interpretation. It has no connection to theory or laboratory research. That objects possess phlogiston, a substance of negative weight that leaves objects when they burn, is also a classic example of interpretation. Such speculation gives science a bad name.

Palmer and Marr both bring up the idea of mental computation, as if the solution of a puzzle somehow necessitated private events. The examples that they give are instructive, too, for the puzzles may be solved in a variety of ways. We could explain to the solver how to solve the puzzle, but we will never know how the person solved it, and possibly even the solver will not be able to say. What we know is the posing of the puzzle and its solution, and any speculation about private events in the solution is futile.

Speculating about private events is no better than speculating about souls or essences. One may make up private stimuli, covert responses, and even covert reinforcers, but such make-believe entities have none of

the properties of their measurable counterparts; or rather, they can have any properties whatsoever, and any “explanation” that incorporates them gives only the appearance of a basis in research. One may make stuff up, but it will be tied neither to theory (as prediction) nor to established research (as explanation). Would research into the properties of the soul likely be productive? Palmer and Marr appeal to theoretical predictions and explanations in the physical sciences as if they were comparable to speculating about private events. The guess about Neptune and the appreciation of electron flow stem from celestial mechanics and atomic theory, both well developed. No such well-developed theoretical framework supports the imagining of private events in other organisms, human or not. Contrary to Palmer, their imagining is “for free,” because no theory constrains them. How long does it take to have a private thought? Would I be enriching the account of a pigeon’s key pecking if I imagined the pigeon was thinking about pecking the key when it was standing in front of it? It might be fun, but it wouldn’t be helpful.

Palmer and Marr repeat the same nonsense that we heard from Skinner: that private events are just like public events, except they are private. The reason this is nonsense is that we can never know whether they are just like public events, because they are private. Their being private is the whole problem. It is just like saying, “This fake diamond is just like a real diamond, except that it is fake.” The whole problem is that it is fake.

Unobserved is not the same as *private* if better instrumentation is possible. Neptune was not private (contra Palmer); it only required more effort and better instrumentation to be seen. Environmental history may be unobserved, but it is not private (contra Hineline, 2011), because it might be observed in a better

preparation, and because history might be inferred from knowledge and theory about the effects of history on behavior. The antiprivacy machine, were it possible, would only make for confusion and would in no way remove the speculative nature of inferring private events in everyday life. If a person denies committing a public event, as in Palmer’s example, we have any number of other people to corroborate the event, but if the antiprivacy machine were to say, “I shot the sheriff,” who is there to corroborate the thought? The antiprivacy machine would not be “instrumental amplification” that “renders ... behavior observable” (p. 204); the machine’s output is public, but the events remain private and uncorroborated. To be sure, some events within the skin are like Neptune: neural events, glandular secretion, and muscular contractions. These are measurable and thus potentially public. Thoughts, feelings, and sensations, however, unlike these events, are not located within the skin and cannot finally be made public.

The antiprivacy machine raises the specter of “truth by agreement,” which some behavior analysts apply as a pejorative label. Both Marr and Palmer bring up Robinson Crusoe, the model of the lone scientist, finding truth, but agreeing with no one. If I were studying the behavior of scientists, the last scientist I should pick would be Robinson Crusoe, because scientific behavior of any note occurs in an environment that includes other scientists. We examine one another’s data and writings, just as we are doing here, in this exercise about my article. Skinner (1945) labeled truth by agreement “sterile,” but I challenge anyone to describe science *as it occurs* as anything other than a social enterprise. I was taught in graduate school by my mentor, Richard Herrnstein, that no research is complete until it is published, and I unhesitatingly pass this advice on to my students. No doubt the “truth by

agreement” and “sterile” labels point to a danger; that a sort of “group think” can prevent an individual scientist from entertaining new approaches to a subject. Indeed, Catania (2011), Marr, and Palmer appear to be victims of just such a constraint. Because they are wedded to an older paradigm, they find it difficult or impossible to consider seriously a molar view of behavior.

Hineline distinguishes between what he calls *analysis* and *interpretation*, but he fails to distinguish between what I am calling *scientific explanation* and *interpretation*. I agree wholeheartedly that experimental and empirical analysis yield knowledge and theory that can then be extended to explain natural phenomena that either cannot be studied for practical reasons or have not yet been studied. Such scientific explanations must be distinguished, however, from speculation about events that cannot be observed or measured that might masquerade as explanation. Like Palmer, Hineline fails to distinguish between unobserved and unobservable. Neither environmental history nor Neptune is private; they may be unobserved, but they are not unobservable. To guess at the history of an individual that might lead to current behavior is far different from speculating about private events that might lead to current behavior. Environmental events are, by definition, observable; the phrase *private environmental event* is an oxymoron.

Marr tries to make a distinction between direct and indirect observation, but he fails to make it precise, and if he did try to, we would see that the distinction is a red herring. All observation takes place within the context of some kind of theory. If I understand what Marr is getting at, I would argue that all observation is indirect. Measurement without theory is impossible, because we nearly always use instruments to measure, and we invent the categories of things to be measured in the first place. His

example of electron flow, like Palmer’s example of Neptune, depends on a well-developed theory for meaning, measurement, and prediction. Electron flow fits into a theoretical framework; private events remain wholly speculative.

Palmer misread my point that evolutionary theory implies that behavior consists of interaction with the environment. In doing so, he claims that most human behavior is excluded from natural selection. I doubt he really means to suggest that human behavior is exempt from evolutionary theory. To do so would be to deny a huge body of research and theory by biologists, anthropologists, psychologists, economists, and political scientists. The word *interaction* covers a lot of ground. The environment affects behavior, and behavior often changes the environment. That change in environment feeds back to affect behavior, and so on. Is human behavior affected by reinforcers and punishers? Isn’t *that* interaction with the environment? Where does Palmer imagine that susceptibility to consequences comes from, if not from natural selection? Those ancestors in the prehuman population that were little interested in food, shelter, predators, injury, or mating are no longer represented because they had fewer offspring than those that were more interested.

Catania supports Skinner. No one should be surprised, given his history of reinforcement. However, I was criticizing certain views about private events, some of which Skinner held, not criticizing only Skinner’s views. Thus, Catania’s appeals to authority are often wide of the mark. Moreover, the reader should beware that he rewrites sentences I wrote to say things I did not say. I believe I presented reasoned arguments and drew conclusions. Unless a reasoned conclusion is “just an opinion,” I was not expressing just opinions.

Here is the gist of one of my arguments. Dualism must be exclud-

ed from any science, because the existence of two kinds of stuff creates the intractable problem that the influence of one kind of stuff on the other remains forever mysterious. The mind-body problem is one example of this intractability. Hence, we must shun mind and mental entities in our technical accounts of behavior. Given this argument, what should we say about private events? What we should say depends on whether private events constitute a second kind of stuff or not. If private events are forever private, not susceptible to being made public by any known means, then they raise the same intractable problem as the mind-body problem, except that the mysterious connection is between private events and public events; we have no idea how one could affect the other. If private events are really private, then we have two options: to shun them in our technical accounts of public behavior or to redefine them as patterns of public behavior, as Rachlin advocates. That is a reasoned conclusion, not an opinion.

Catania plays around with the words *inner* and *in* to create statements that I never made. The words have multiple usages. I meant to associate *inner* with the phrase *inner world*, as in the dualistic dichotomy between inner world and outer world. If that was unclear, perhaps it will be clear now. The word *in* requires special care, because *in* everyday speech we say that a person is *in* good health or that Jane is *in* love, which differ from the usage that Tom is *in* the garden or the brain is *in* the body. The last usage is the one appropriate for events internal to the skin that with instrumentation can be converted to public events. The former usages might be appropriate for thoughts, sensations, and feelings, but because instrumentation cannot turn these into public events, the latter usage will not work for these.

Hineline, too, fails to distinguish between *inner* or *internal* and *private*. People's verbal behavior may include terms that seem to be occasioned by *inner* or private events, but their utterances differ only as to whether they are occasioned by physical events actually within the skin, such as stomach contractions, or imaginary events, such as *frustration*, not actually within the skin. Both sorts of utterance, however, are socially useful and occasioned by the presence of sympathetic or otherwise receptive listeners. Perhaps I failed to make clear that observable events within the body, such as events in the nervous system, which researchers are increasingly making public, are in no way problematic for behavior analysis (although they cannot substitute for environmental history, and, if they are said to cause behavior, would need to be explained by environmental history). Problems arise when events that are private and unsusceptible to being made public are invoked to try to explain public behavior.

Instead of addressing the problematic nature of private events for technical accounts of behavior, Catania focuses on what he takes to be my deviation from Skinner's account of everyday utterances that include private-event terms (i.e., "references" to putative private events). Most of the views Catania attributes to me I never said, so I will try to clarify. I think that Skinner was largely correct in his account of the origins of verbal behavior in general and talk of private events like thoughts and feeling in particular. No doubt the verbal community shapes the talk by reinforcing some utterances and punishing others. If a child has a visible injury or is crying, people around are apt to talk about pain and hurt, and the child will imitate this, and the child's talk about pain and hurt will be reinforced. We fall into error, however, if we interpret this as the child introspecting, reporting, or dis-

criminating some inner stimulus. If we do so, we invoke a hidden cause with only a mysterious connection to the utterance. The valid part of Skinner's account is the shaping of the utterances; a discrimination is involved, but it is based on the persons around the child (whether they have been sympathetic or not) and other surrounding circumstances (e.g., the presence of peers who would ridicule the utterance). The discrimination might also be based partly on sustaining an injury, but that is not the whole account, because the absence of a sympathetic audience will prevent any verbal behavior of the sort. How Catania concludes that I think history to be unimportant, I cannot guess; in the molar view, it is all-important.

Catania gets both Watson and Whorf wrong. Watson (1913) was no methodological behaviorist. He rejected introspection as a method from the beginning. Skinner coined the term to apply to Stevens and Boring, who did think of introspection as a method of studying subjective experiences. Watson did not affirm the existence of a mental world, as Catania says. He argued that references to the mental world had no place in the technical vocabulary of a science of behavior. Whorf (1956) argued that language determines reality, not the other way around. That is a summary, and doesn't capture the strength of his arguments. To a behaviorist, Whorf's arguments can be translated as saying that verbal behavior is shaped, and the verbal behavior that a given community shapes determines what the members of that community can talk about. The criticism of mentalism that I quoted follows this line of argument (p. 187).

Catania and Marr, like Palmer and Himeline, accept *interpretation* as a part of the practice of behavior analysis; what I said above in response to Palmer and Himeline applies also to the comments of Catania

and Marr. In addition, the elaboration on *interaction* above might allay Catania's concern that I omitted to talk about ontogeny in connection with evolution.

Catania and Marr both were aggrieved by my saying that some behavior analysts bring private events forward in a misguided attempt to reassure critics that behaviorism offers an account of mental life. Presumably Catania and Marr number among those behavior analysts, but they both are aggrieved because they see it as a criticism of Skinner, whom they seem to regard as almost infallible. I didn't make it up; in the quote from *About Behaviorism*, he says as much (Skinner, 1974): "A science of behavior must consider the place of private stimuli as physical things, and in doing so it provides an alternative account of mental life" (pp. 211–212). Moreover, in response to the scandalized remarks of both, I would remind Marr and Catania that Skinner's 1945 paper was given at a symposium in which that criticism of behaviorism was made and to which Skinner was, in part, responding. So, yes, one of his goals in that paper was to reassure everyone that behaviorism is not bereft of a treatment of mental phenomena. Marr's rambling discussion of what would be true if private events didn't exist (something I never said) seems to stem from the same misguided attempt, except that Skinner understood that verbal behavior, including references to private and mental events, is shaped by the verbal community, regardless of whether private or mental events exist. What exists, for sure, is the verbal behavior, and that makes poetry and other literature possible.

One final note about Catania is that I know only the public parts of his behavior. His private events may or may not exist. Thus, I am free to doubt that he has any private events at all.

A final note about Marr's commentary: I found it "a great muddle, full of confusions, conflation, and contradictions, strange broad-brush accusations and misattributions, vague assertions and assumptions, unfounded conclusions, and a view of behavior that fails to address adequately a significant portion of human activity" (Marr, p. 213). He accuses me of adopting a "mutated" (read "mutant") form of methodological behaviorism. Perhaps he is right, but that depends on one's definition of methodological behaviorism. He apparently intends to damn me with the label (and the "mutated" part), and likely doubts that my views agree with Skinner's original definition (hence the "mutated" part), but I stand by my molar view, regardless of pejorative labeling.

Hineline thinks that the molar paradigm would better be called the *multiscale* paradigm, because that term conveys better the support for analysis of behavior at a variety of time scales, ranging from fractions of a second to months or years, whereas *molar* might seem to imply only long time scales. Perhaps he is right. That might explain Marr's erroneous claim that the molar view cannot address the evolution of skilled performances, like dance, that require rapid feedback between behavior and environment on a small time scale. In the molar or multiscale view, activities are always extended, whether at short scales or long scales, and one chooses the scales for analysis based on one's purposes and the orderliness of the data (Baum, 2002, 2003, 2004, 2010).

Rachlin explains our molar views of behavior as the result of our similar histories of reinforcement in graduate school and beyond. My recollections match pretty well with his, although I was never in awe of Skinner. Like Rachlin, I admired the contributions Skinner had made to the science, but was more influenced

by Herrnstein. Skinner made a good start, but had stopped short of following out the implications of his ground-breaking concepts, like the generic nature of stimulus and response, response rate as a datum, and the concept of stimulus control. Rachlin and I are both what I would call molar behaviorists. I avoid the term *teleological behaviorism* because it seems to make more trouble than it is worth. Philosophers tend to dismiss teleology as wrong or naive and stop listening. The one time I referred to final causes in a talk, half the people in the room (mostly philosophers) objected that final causes had been discarded long ago, for the mistaken reason that they imply cause by future events (which they do not). After that, I saw no reason to make the presentation of behaviorism harder than it already is, in the face of a vast sea of prejudice that holds that behaviorism is dead and, if it isn't, should be.

My objection to the phrase *teleological behaviorism* is practical, however. I do not think Rachlin is wrong, because I agree that in naming patterns of behavior we include what would traditionally be called the goals of the activities. When we say that a rat is pressing a lever or that Tom is going home, we are including the goals of lever depression and arriving home. This inclusion is unavoidable; it underlay Skinner's concept of the operant and is essential to distinguishing behavior from physical movements.

Rachlin and I differ in the way we express the relation between mental terms and extended behavioral patterns. Rachlin says that Tom's belief that the bus will take him home *is* his getting on the bus day after day. I prefer to say that Tom's getting on the bus day after day occasions an observer's (possibly Tom himself) saying that Tom believes the bus will take him home. This approach emphasizes the role of verbal behavior and the culturally received nature of

categories like *belief* and *desire*. The words *belief* and *desire* are helpful shortcuts in our verbal exchanges about behavior, but no belief, believing, desire, or desiring exists apart from the labeling, the verbal behavior. A culture could well exist in which no one spoke of believing or desiring, but in which people only spoke of repeated actions as characteristic of individuals or groups.

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