

## **METHODOLOGICAL DUALISM AND MULTI-EXPLANATION FRAMEWORK: REPLIES TO CRITICISMS AND FURTHER DEVELOPMENTS**

Sam S. Rakover  
*Haifa University*

**ABSTRACT:** First I comment on the reasons that motivated me to develop the approach of Methodological Dualism (MD) and Multi-Explanation Framework (MEF) and present a brief summary of its main ideas; second, I respond to the commentators' criticisms; finally, I present further developments that compare my approach to other relevant psychological approaches, and develop certain arguments as to why one should employ MD and MEF.

*Key words:* methodology, explanation, philosophy of science, mind, and psychology

I am indebted to the commentators for their thought-provoking reviews. For different reasons, they do not accept my plea for including mentalistic explanations under the umbrella of the scientific methodology (except perhaps **Foxall**, 2011/2012, who welcomes my methodological pluralism approach). I will attempt to show that their arguments against my approach are incorrect.

The paper is organized in the following way: first I comment on the reasons that motivated me to develop the approach of Methodological Dualism (MD) and Multi-Explanation Framework (MEF) and present a brief summary of its main ideas; second, I respond to the commentators' criticisms; and finally, I present further developments.

### **MD and MEF: Motivation and Brief Summary**

In Rakover (2011/2012) I reviewed the literature and reached the following conclusions: (a) there is *not yet* any theory of the mind/body relationship [T(m/b)], (b) it is hard to conceive mentalistic explanations in terms of mechanistic explanations, and (c) it is hard to provide a complete account of behavior by appeal to mechanistic explanations only. These conclusions motivated me to develop an alternative approach: MD, which leads to the construction of MEF for developing specific theories in psychology. This approach does not suggest a new way to solve the consciousness/brain problem, but attempts to circumvent it by taking a methodological route that provides a comprehensive and coherent explanation of behavior by appeal to two kinds of explanations: mechanistic (e.g.,

---

**AUTHOR'S NOTE:** I am grateful to Editor J. Moore for his insightful and helpful comments. Please address correspondence to Sam S. Rakover, Department of Psychology, Haifa University, Haifa, Israel 31905; Email: rakover@psy.haifa.ac.il

neurophysiological, cognitive-computational processes) and mentalistic (e.g., will, belief, purpose, intention).

The present article provides me with an opportunity to say something more (perhaps something more personal) about the reasons that motivated me to develop MD and MEF.<sup>1</sup> A major reason for developing MD and MEF is my cumulative impression that the attempts to solve the mind/body problem, after all these years, seem to be going nowhere. We are going in circles, or perhaps more accurately, oscillating between two opposing pulls: one pull is our certainty in our conscious experiences, and the other is our certainty in the knowledge provided by science. Apparently we are unable to reconcile these two pulls because their several implications contradict. Let me illustrate this with the following line of thought (based on Kim, 2011).

Many philosophers and psychologists are committed to the view that understanding of the world and of behavior is achieved by appeal to physical and neurophysiological events. Furthermore, these researchers believe that these phenomena cannot be explained or affected by appeal to events that do not belonging to the physical and neurophysiological domain. Since it seems that mental states, conscious experience (will, belief, etc) do not belong to this domain (and the attempts to conceive them neurophysiologically encounter impassable obstacles), it follows that consciousness cannot affect the events in this domain—our brain and behavior (i.e., they are epiphenomenal). If this is true, it is impossible for one to report on one's own conscious experience. But this conclusion seems self-contradictory, as the very fact that we are trying to understand consciousness attests that conscious experience does have an effect on behavior. Otherwise, how could we report on our conscious experience? This situation strikes me as a paradox, call it the "consciousness paradox," and it reminds me of Zeno's famous paradox of Achilles and the turtle. The two run a race, with the turtle starting to run ahead of the Greek warrior. The argument ingeniously convinces us that Achilles will never overtake the turtle! But we all know what really happened: the truth. Apparently, something is wrong with this argument. Apparently, something is wrong with the consciousness paradox, with the argument that consciousness is epiphenomenal.<sup>2</sup>

In fact, I was so impressed by the knowledge that the T(m/b) has not yet been discovered that I begin to conceive of psychology as an associational science. I have attempted to show that the following observations on the nature of psychology as a scientific discipline can be viewed as expressions of the mind/body problem (see Rakover, 2012). Unlike the sciences, psychology has not developed in any areas of research a top-theory like Newtonian theory or the theory of relativity. Psychological research is fragmented and fractionated, and it

---

<sup>1</sup> This may be viewed as a partial reply to **Allen**, 2011/2012, and **Malone**, 2011/2012, who do not see the lack of a solution to the mind/body problem as sufficient grounds for my approach.

<sup>2</sup> While some believe that Zeno's paradox can be solved by calculus, I don't believe that calculus can help us solve the consciousness paradox.

## REPLIES TO CRITICS

has not developed measurement techniques like the sciences (e.g., measurement units similar to those in physics have not been discovered). These differences between the sciences and psychology have led me to view psychology as an associational science, one whose observations are based primarily on two kinds of association: stimulus and response, and one type of response and another type of response. These associations are explained by different and rival theoretical approaches.

Furthermore, the knowledge that the T(m/b) has not yet been discovered has influenced my thinking in another way: as mentioned above, I have attempted to circumvent the mind/body problem and develop the approach of MD and MEF to improve the explanatory ability of a psychological theory. Here are the major ideas of this approach:

1. A distinction is made between mechanistic and mentalistic explanations. While the former include explanations used in the natural and social sciences and reject explanations based on mental states, the latter consist of explanations based on one's inner worlds.
2. Purposive, will/belief-specific mentalistic explanations are conceived as generated by the following model/scheme of explanation: [Will/Belief]: if X wants G and believes that behavior B will realize X's will, then X will perform B.
3. Although [Will/Belief] cannot be reduced to mechanistic explanations, it fulfills the scientific methodological requirements for explanation. Hence, one can include purposive explanations as part of the general methodology used in psychology.
4. MEF suggests a methodological way to integrate coherently mechanistic and mentalistic explanation in a specific psychological theory. This is achieved by making an appropriate behavior/explanation match, i.e., a match between behavior (and its components) and the two kinds of explanations—mechanistic and mentalistic.
5. The behavior/explanation match is achieved by employing empirical methods and theoretical considerations, and by applying the principle of Explanation-Matching: while the components of a mechanistic behavior (which has mechanistic explanation) cannot be explained mentalistically, the components of a mentalistic behavior (which has mentalistic explanation) can be explained mechanistically and mentalistically.

### Responses to the Commentators' Criticisms

The comments refer to different aspects of my proposal: **Allen** (2011/2012) criticizes the paper from the perspective of the philosophy of science and mind, **Malone** (2011/2012) and **Branch** (2011/2012) from the perspective of radical behaviorism, and **Foxall** (2011/2012) from the perspective of intentional behaviorism. Given this diversity, I decided to respond to each one individually.

**Allen** (2011/2012) criticizes my approach from the following main points.

*A pessimistic approach:* Could my pessimistic attitude, which emanates from the unsuccessful attempt to discover T(m/b), block researchers from finding a solution to the mind/body problem, and also from using mechanistic explanations for behavioral phenomena? My answer is no. First, in contrast to several researchers who abandoned hope of solving the mind/body problem (see Rakover, 2011/2012) or who were convinced that this problem is unsolvable in principle (like my teacher, the late Professor Y. Leibowitz), I only stated that the problem *still* has not been solved. I have no idea if it will be solved in the future or if it will be shown that it is an unsolvable problem.

Second, the very essence of my approach, which is based on a coherent integration of mechanistic and mentalistic explanations to account for behavior (see major ideas above), does not dispirit but rather encourages a researcher to develop all possible aspects of mechanistic explanations as well as mentalistic ones. This is also exhibited by the principle of Explanation-Matching (see (5) above). Accordingly, one has to match mechanistic and mentalistic explanations to behavior as a whole and to its behavioral components.

*Functional and qualitative aspects of mental states:* **Allen** maintains that many philosophers distinguish the functional from the qualitative properties of conscious experiences (qualia). While the former properties can be understood by developing computational and neurophysiological theories (hence are called the “easy” problem), the latter properties cannot (hence are called the “hard” problem; see Chalmers, 1996; Kim, 2011). He also proposes that the difficulty in explaining qualia does not necessarily lead to capturing [Will/Belief] dualistically. I cannot agree with these positions.

The hard problem is indeed hard, but the easy problem is no less hard (see Chalmers, 1997). The main reason is that purposive, will/belief behavior is saturated with consciousness; it is a consciously meaningful and organized action. Furthermore, in many cases one’s ultimate goal is to change one’s own states of consciousness (as in reading, travelling, and meeting friends). Now, if conscious experiences (qualia) cannot be captured by computational and neurophysiological theories, what are we explaining with these theories? I propose that without conscious experiences the computational and neurophysiological theories account for behavior that is no more than motor movements, no more than a robot’s responding (i.e., they account for a zombie’s will/belief behavior; see also Further Developments). Harnad (2000) wrote:

The functional stuff would all go through fine—behaviorally, computationally—  
if we were all just feelingless zombies. But we’re not. (p.56)

By contrast, MD and MEF propose that (a) scientific explanatory methodology can incorporate [Will/Belief], and (b) this explanation scheme is appropriate to handle behavior saturated with conscious experiences.

*Is [Will/Belief] an explanation scheme?* **Allen** criticizes the justifications for placing [Will/Belief] under the umbrella of scientific methodology as follows: first,

## REPLIES TO CRITICS

he suggests that my criterion of *Empiricism* is not scientifically rigorous, and second, he proposes that my criterion of *Empirical irrelevance* is too strong.

First, consider *Empiricism*. **Allen** suggests that the fact that David's selection of Ruth's photograph out of ten does not provide a strong empirical test to the explanation that David waved his hand to bid farewell to Ruth. It hardly suffices to support David's intention to bid farewell by waving his hand, and it does not fulfill the scientific standards of repeatability and independent confirmation. I don't take this criticism as severe.

It is well known that a behavioral description per se does not capture one's intention, one's conscious experience of will and belief. Therefore, an observation can provide only indirect empirical support for an explanation based on one's inner world. If one takes **Allen's** criticism as crucial, then psychological experiments in recognition memory are overshadowed by distrust. The reason is simple: the farewell episode is no more than a popular description of a standard Yes/No procedure in a face recognition experiment, and, as such, it satisfies the methodological requirements of repeatability and independent confirmation (e.g., David takes his leave of Ruth every time in the same way, other observers can also confirm it, the face-recognition experiment can be repeated, etc).

Now let us consider *Empirical irrelevance*. **Allen** proposes that while a general explanatory scheme is not directly testable, accumulations of negative experimental results can still disconfirm such a scheme. As an example, he presents the theory of evolution (a general explanatory scheme), which generates different specific explanatory models whose accumulated empirical results can have an important implication for the theory of evolution.

I have no dispute with this example per se. However, the point I would like to emphasize is that the methodological status of [Will/Belief] is different from the methodological status of the theory of evolution or, as another example, of the information-processing theoretical approach in cognitive psychology. While these two are general theoretical schemes (which allow the development of different specific models; indeed, cognitive psychology has dozens of such models based on the general information processing scheme), [Will/Belief] is not such a theoretical scheme. It is an explanation scheme (model), that is, a *procedure* which tells one how to provide specific purposive explanations for specific behaviors. In this respect [Will/Belief] is similar to Hempel's D-N model of explanation (i.e., both are *procedures* that tell researchers what to do in order to offer a scientific explanation). While scientific theories are evaluated by empirical tests (which confirm/disconfirm a theory), procedures are evaluated differently. A procedure is evaluated by what I call the "appropriateness" criterion, which asks whether the application of the procedure is appropriate. For example, Hempel's D-N model was criticized as being inappropriate for handling causal phenomena. This led, among other things, to the development of several alternative explanation models (see Salmon, 1989). To take a different example, consider the rules of social conduct. One does not argue that such rules are right (or wrong) but only discusses whether they are appropriate for achieving their goal—to organize the social life of a certain group of people.

In his satirical, patronizing, zealous, but nevertheless zappy style, **Malone** (2011/2012) criticizes not only my approach (MD and MEF) but also me as a radical-behaviorism ignoramus.<sup>3</sup> For example, **Malone** suggests that radical behaviorism is a complete mystery to me since I conceive it in terms of the three-term contingency (stimulus-response-consequence), whereas he maintains that “Radical behaviorism is the important view that intervening variables don’t *explain* anything, though their use is common in every conversation” (**Malone**, 2011/2012, p. 94). It is pity that **Malone** did not pay attention to the entire sentence, in which I refer to radical behaviorism “. . .and also stimulus-response-consequence relations proposed by radical behaviorism (*which avoids using mental terms in explanations*). . .” (Rakover, 2011/2012, p. 25; italics added here). Clearly, the phrase in parentheses accords with **Malone’s** view of radical behaviorism (and therefore I cannot help but quote here **Malone** himself: “Oh, my goodness,” maybe even “Horror of horrors,” or perhaps “my brain is signaling ‘low battery’”). But now, having amused ourselves with this “wordy-duel,” let us examine **Malone’s** major arguments against my approach.

*Mind/Body problem and mental states.* **Malone** views the mind/body problem as “phony (bogus, artificial, contrived, fictitious, and even meretricious)” (p. 95). Stated otherwise, the mind/body problem is not a problem for a radical behaviorist. All behaviors, overt as well as covert, are explained by appeal to environmental factors with no need for explanation by using one’s inner world or mediating mental states (will, belief, etc), which do not belong to the behavioral dimension (see also Moore, 2008). That is, overt behavior is not explained by appeal to covert behavior. **Malone** maintains that intervening variables such as the concepts of will, belief, intention, and purpose are only words that refer to a relation between independent and dependent variables, and that they themselves need explanations.

In discussing this approach I base myself on Moore (2008), Rakover (1990), and Skinner (1953, 1974). I will attempt to show that **Malone’s** approach rests on a weak argument, and I will also present three cases whose explanation, without the use of mediating hypothetical constructs but with the use of discriminative stimuli, responses, consequences (social, non-social) and history of contingencies of each individual, seems difficult.

*Weak argument:* Given that Skinner allows associating the relation of independent and dependent variables with the relation of cause and effect, I will present the above assertion in terms of what I call the “Eliminative Argument,” which is based on a mentalist and radical behaviorist viewpoints (arrows signify causal relation):

---

<sup>3</sup> In the same vein, he also criticizes several other researchers like Foxall and Dennett, and blames B. F. Skinner himself for contributing to the confusion surrounding the proper understanding of radical behaviorism. Note that Foxall (2008) points out that there are several incommensurable forms of radical behaviorism, and asks: “will the real radical behaviorist please stand up?” (p. 114).

## REPLIES TO CRITICS

- a) Mental states  $\implies$  behavior. (Acceptable by a mentalist. Behavior is explained by appeal to mental states.)
- b) Environmental factors  $\implies$  mental states. (Acceptable by a radical behaviorist. Mental states, private events conceived of as private behavior [and not as mediating theoretical, fictional terms], are explained by environmental factors such as social and nonsocial consequences.)
- c) Hence: Environmental factors  $\implies$  behavior.

According to radical behaviorism, then, the Eliminative Argument shows that the appeal to mental states as explanatory concepts have been eliminated and that they are not needed.

I shall now argue that this argument is weak. First, I suggest that the above causal relations do not always correspond to the relation holding among observable events (which do not always show causal transitivity). Consider the following three events: pressing the gas pedal, car engine operation, car movement. Given these three events, one cannot suggest that since: engine operation  $\implies$  car movement, and pressing the gas pedal  $\implies$  engine operation, it always true that pressing the gas pedal  $\implies$  car movement. (Similarly, pressing the pistol's trigger does not always fire a bullet; a typhoon does not always wreck Tommy's car, although it so happened that the typhoon broke a big tree that wrecked poor Tommy's car; and a sensor does not always open the supermarket's doors.) Furthermore, in many cases the initiating causal event disappears and the mediating event is the one that causes the event to be accounted for. For example, the head of Eastern gang was killed by an explosive device attached to his car the previous day by a hired hit man. In the same vein, one cannot propose that the above relation (Environmental factors  $\implies$  behavior) always holds. There are several cases where this causal relation does not hold, and one has to appeal to mental states to propose a satisfactory explanation. For example, Danny regularly has his lunch at noon, but one day on the spur of the moment he decides not to eat but to listen to Verdi's *Requiem*. We may analyze this episode as follows: Hunger  $\implies$  lunch, and Noon  $\implies$  Hunger, but clearly we cannot infer that Noon  $\implies$  lunch. In this case one has to appeal to Danny's inner world to answer the question why his behavior has changed.

Secondly, one possible reason why mental states are hard to eliminate as explanations for behavioral phenomena arises from the distinction between "intervening variables" and "hypothetical constructs" (see MacCorquodale & Mheel, 1948; Moore, 2008). Accordingly, intervening variables refer to concepts that just describe the investigated phenomena with no theoretical unobserved states or processes, whereas hypothetical constructs have surplus meaning beyond that obtained through observations and measurements. So unlike intervening variables, hypothetical constructs cannot be completely reduced to empirical observations. In my view mental states are appropriately characterized as hypothetical constructs. They have surplus meaning which stems from their being very complex and interwoven with other different mental states, neurophysiological states, and different situations and responses, and they are also interlaced with various

conscious experiences. Hence it is hard to see how they can be reduced completely to empirical observations, and how they can be eliminated as mediating explanatory terms.

I admit that appeal to hypothetical constructs may generate many fallacious explanatory concepts such as the notorious “phlogiston” theory in chemistry in the eighteenth century (see Moore, 2008, Rakover, 1990). But this theory (as well as many other incorrect hypotheses and theories) was refuted and replaced by a better theory (Lavoisier’s oxygen theory) through the employment of the scientific methods, which led to progress in science. Furthermore, in contrast to Mach’s view that atoms are fictional, scientific methods have discovered that they do exist. Hence, I propose that the requirement to reject explanations based on mediating constructs (such as mental states and processes) is too extreme as it throws the baby out with the bathwater. So, in my view, what largely determines whether or not a given explanation is satisfactory is scientific methodology (e.g., confirmation/refutation of a hypothesis) and, to a lesser degree, philosophical worldview.<sup>4</sup>

*Three cases:* First, in the last forty years an interesting kind of explanation, called Neural Nets (NN), has been developed and applied to psychology (to such areas as perception, recognition, and learning). The NN, which have produced various computational models for handling different phenomena, are based on three layers of variables: input layer, hidden (intermediary) layer, and output layer, which are parallels to stimuli, a mediating system of hypothetical constructs, and responses. The main point I would emphasize here is the utmost importance of the role that the hidden, intermediating layer plays in NN’s success in explaining the phenomenon under study. For example, it is well known that a NN without a hidden layer cannot explain the simple XOR function (XOR gate, exclusive or). XOR function refers to the following: given two inputs, each of which can be True or False, the XOR output is T in the cases where one input is T and the other is F (see Hertz, Krogh & Palmer, 1991). For example, we use XOR when we say that a certain job will be done tomorrow or the day after tomorrow. The question is how can the various phenomena that were handled by NN which are founded on hidden layers be treated by the methodological tools of a radical behaviorist? I believe that one can train a rat to press a lever to obtain reinforcement only when one of two lights is turned on. How can this finding be explained? According to the NN approach, one may hypothesize that in the rat’s brain there is a hidden layer that functions similarly to an XOR net and which intervenes between stimulus and response.

Second, Rakover (2013) developed a mediating model to explain the Face-Inversion Effect (FIE) and its related phenomena.<sup>5</sup> This model, called the Face-

---

<sup>4</sup> Note, for example, that the Hypothetico-Deductive method is rejected by radical behaviorism as ineffective. For a discussion see Moore (2008).

<sup>5</sup> According to FIE, perception and recognition of faces are better when faces are presented upright than when inverted, and these differences are greater in faces than in non-face objects.



## REPLIES TO CRITICS

Scheme Incompatibility (FSI) model, has been developed not on the algorithm level (like the NN) or on the implementation level, but on the functional (computational) level. It consists of three intermediating stages which intervene between the stimuli (faces) and response (perception, recognition): stage 1, where the visual stimulus is transformed into a face stimulus; stage 2, where face schemes (and prototypes) are formed from the distributions of the face stimuli; and stage 3, where a meaning-bearing face is formed by means of the schemes and the prototypes. The question is how could a radical behaviorist handle the FIE and its related phenomena?

Finally, consider the following classic and famous experiment done by Shepard & Metzler (1971). Participants were asked to decide whether two geometrical forms were the same or different. The forms were rotated from each other 0 to 180 degrees. The main result showed that reaction time increased linearly as a function of the physical angle of rotation. For example, it took more time to make a decision for forms that rotated 70 degrees than 15 degrees. Based on the participants' introspections the following explanation was proposed: the participants rotated the form-images in their mind until there was (or was not) a match between them; the greater the physical rotation (in degrees) between the two forms, the longer the time needed to reach a decision. This explanation is anchored to certain mediating mechanisms: mental states and an information-processing mechanism. The question arising is how can this finding be accounted for without assuming the hypothetical mental rotation? How would a radical behaviorist provide a satisfactory explanation? (For example, how may this finding be accounted for by appeal to conditioned respondent seeing or operant seeing?)

*Criteria for mechanistic and mentalistic explanations.* **Malone** claims that radical behaviorism is not mechanistic. Since I made it clear that there are two types of explanations—mentalistic and mechanistic [when “. . . mentalistic explanations involve conscious experience whereas mechanistic explanations do not” (Rakover, 2011/2012, p. 25)]—and since I gave many examples for each one of these two types, it seems that there is a certain conceptual misunderstanding: perhaps the phrase mechanistic explanation calls to mind the Newtonian mechanical explanation, reduction to neurophysiology, or S-R behaviorism. At any rate, the concept of mechanistic explanation that I propose is wide and covers different explanations used in the natural sciences, including computational models and explanations provided by radical behaviorism, which reject using mental states, private events, as explanations for behavior.

**Malone** states that my list of requirements for scientific explanations is something otiose like the Vienna Circle's Positivism, and that the utility of these requirements and the Multi-Explanation Framework (MEF) eludes him. I find this critique unsound. It is hard to understand how one can develop psychology as a science if one does not follow certain rules and norms of scientific conduct. For example, do psychologists accept any kind of explanation? Does radical behaviorism accept a neo-behaviorism type of explanation? The answer to both questions is negative. Furthermore, the fact that Positivism failed (although certain

aspects of psychology are still under its influence) simply shows, to my mind, that it is very difficult to understand how science is done and how it should be done.

If one is convinced that to understand behavior one has to appeal to inner mental states and processes, then one has to ask whether mentalistic explanations can be encompassed by scientific methodology. Given a positive answer (see Rakover, 2011/2012), one must go on to ask how to apply coherently these two kinds of explanation—mechanistic and mentalistic—to behavior. The answer to this question is given by the development of MEF. However, if one is not convinced of the importance of mentalistic explanations my whole approach should probably be deemed futile. In fact, radical behaviorism, neo-behaviorism, and cognitive psychology do not see any need to employ mentalistic explanations and have developed different sorts of mechanistic explanations for behavior (see also Further Developments).

**Branch** (2011/2012) criticizes my approach in the following major respects.

*Basis of mentalistic explanations.* Like **Allen, Branch** proposes that the rationale for employing mentalistic explanations is not compelling since the claim that the mind/body is not solved is premature.

The conclusion that one may reach from the six quotations and the review of the literature discussed in Rakover (2011/2012; see also above) is that the mind/body problem has not *yet* been solved. The question is what to do with this *interim* conclusion? Two tactics are possible: decide to wait for additional empirical and theoretical relevant knowledge, or attempt to circumvent the mind/body problem by developing a new approach that will encompass mentalistic explanations scientifically and thereby enhance the ability of psychological theories to provide better explanations. I have tried to do the latter and at the same time watch closely for new developments generated by the first tactic.

*Criteria for scientific explanation.* This issue raised by **Branch** (and also by **Malone**) suggests that I discuss only cognitive and neurophysiological explanations and overlook behavioral analyses, which provide non-mentalistic accounts for teleological behavior. Instead of explaining behavior by appeal to mentalistic accounts, one should appeal to one's past history of stimulus-response-reinforcement contingencies.

Above I argued that it is difficult to eliminate mentalistic explanations, and also suggested as examples experiments that are hard to account for without appealing to intervening (mentalistic, cognitive) concepts. Here, I would like to add the following two points. First, **Branch** suggests that the will/belief explanation can itself be accounted for in terms of one's past history. Similarly, I ask what the explanation of one's past history is. Past history, which is a particular correlation between a discriminative stimulus and a response (established by reinforcements), can be viewed as a phenomenon that itself requires an explanation. One possible way to account for this phenomenon is to appeal to mentalistic or cognitive intervening processes.

Secondly, a discriminative stimulus does not always have complete control over one's response (it presents an occasion for responding). How this may be explained? Consider the following everyday example. After the infant Timmy has

## REPLIES TO CRITICS

learned to say “Timmy nose” when his mother touches his nose, he suddenly says “TV nose” and sometimes “Doggy nose.” One possible explanation for this behavior is to appeal to Timmy’s inner mental life: the child has a terrific sense of humor and will probably one day be a famous stand-up comic. Thus, it may be suggested that whether a discriminative stimulus does or does not control one’s behavior depends on one’s inner world.

*Do mechanistic and mentalistic accounts provide understanding?* **Branch** proposes that an account by a description of a mechanism, such as an engine or a flashlight, does not provide understanding of how these things work, and that a mentalistic account is too vague to provide understanding since “until it is shown how one can identify mentalisms independently from the behavior they purport to explain, they remain mysterious” (p. 92).

*Mechanistic accounts.* In about the last twenty years some philosophers and researchers have developed a relatively new approach to explanation that accounts for behavior by describing the mechanism responsible for the behavior under investigation (let’s call it “explanation by mechanism”; see Bechtel, 2008; Bechtel & Wright, 2009; Rakover, 2007; Wright & Bechtel, 2007). As an example that is relevant to psychology, I briefly describe the information-processing mechanism. It consists of different components that process relevant information (e.g., visual, verbal, etc). The components are organized and interact so as to produce the phenomenon under investigation (e.g., the FIE; see Rakover, 2013). It is proposed that the description of the mechanism (the components and their activity) mirrors the actual activity that occurs in one’s cognitive system when one processes the relevant information. While most of the mechanism’s activity is beyond one’s awareness, the phenomenon to be accounted for involves one’s consciousness.

One of the greatest explanatory powers of the “explanation by mechanism” approach dwells in the organization and the interactions among the components that comprise the whole mechanism. For example, while each component of a flashlight is explained by its appropriate physical and chemical theories, the function of the flashlight (emitting light) is explained by describing the appropriate organization and interactions among the flashlight’s components. In the same vein, a crucial explanatory property of the information-processing mechanism abides in the appropriate organization and interactions among its components.<sup>6</sup>

*Mentalistic accounts.* To the best of my knowledge, no strict definition or exact procedure exists that decides a priori whether a given behavior and its components are to be accounted for by appeal to a mentalistic explanation. Nevertheless, there is a clear description of what a mentalistic purposive scheme,

---

<sup>6</sup> Here the following clarification has to be noted. The literature names explanation by mechanism “mechanistic explanation.” I, however, use the latter phrase in a much wider sense: it refers to a group of explanations that encompass all the explanations applied by modern natural and social sciences which do not appeal to mental states; therefore it also includes an explanation by mechanism. While explanation by mechanism is considered to be an alternative to explanations by appeal to laws and theories (Hempel’s models), I conceive both these kinds of explanation as belonging to the group of mechanistic explanations.

[Will/Belief], is (see Rakover, 2011/2012). According to MD and MEF, research effort must be invested in order to propose an appropriate match between the two kinds of explanation (mechanistic or mentalistic) and the specific behavior and its components. The attainment of such a match involves various theoretical and empirical considerations as well as educated employment of the principle of explanation-matching (see above).

*Dog–Elevator episode.* In Rakover (2011/2012) I describe a true episode where a neighbor’s dog hitched a ride with me on the elevator. I presented this episode to illustrate that the dog’s behavior can be analyzed by employing the principle of explanation-matching (i.e., to suggest an account for this episode by matching mechanistic/mentalistic explanations and the dog’s behavior and its components). **Branch** maintains that my account is inadequate: it is not clear how to get the dog to do it (do we need to train the dog in riding the elevator?) and the explanation looks ad hoc and seems unrepeatable.

I believe it is not a big problem to run in a lab an experiment that will simulate the Dog–Elevator episode, where a dog will learn to use one (a person or another dog) to achieve a certain goal (there are many anecdotes in which dogs bring their owners their leashes to go out for a walk). Given that such an experiment can be performed, one may ponder its appropriate explanation: by appeal to the experimental factors, the experimental stimuli, or by appeal to mechanistic and mentalistic factors (i.e., the dog’s inner mental processes). I prefer the latter explanation, because the former account is a description of a phenomenon (the dog’s behavior in a particular situation)—a description that itself demands an explanation. Furthermore, I tend to speculate that the dog in the Dog–Elevator episode could have been making an interesting practical inference. Assuming that the dog (a) had the experience of entering the elevator tunnel and riding the elevator with his owner, and (b) had seen me entering the tunnel several times, the dog could have figured out that I was going to the elevator (like his owner) and decided to hitch a ride on it with me (Lloyd Morgan would probably raise an eyebrow, but George Romanes would nod and smile).

While **Foxall’s** (2011/2012) approach and mine are based on the understanding that an appropriate explanation of behavior has to take into consideration mentalistic concepts, our approaches differ on several interesting issues. Perhaps the major difference is the following. **Foxall’s** (2007, 2008, 2011/2012) intentional behaviorism is philosophical, linguistic, and a-ontological (i.e., it does not maintain that the ascription of intentional interpretations to a person refers to a certain reality in the mind). The justification for using intentional interpretations is limited to specific groups of cases for which radical behaviorism has failed to propose appropriate explanations: continuity of behavior over time, behavior at the personal level, and delimitation of behaviorist interpretations. These intentional interpretations have to be consistent with evolution, neurophysiology, and behavior analysis.

By contrast, I propose that conscious experiences and mental states and processes are real, are involved in explanation of behavior, and that the goal of explanation is to mirror the activity that occurs in one’s mind. The explanatory

## REPLIES TO CRITICS

function of mental states and processes is not limited to specific kinds of behaviors; rather, it is fixed for any kind of behavior methodologically (i.e., by employing theoretical and empirical considerations that lead to a match between mechanistic/mentalistic explanations and the behavior and its components). This difference can shed light on the following distinctions between our approaches.

**Foxall** suggests that a linguistic distinction and a methodological gap lie between the explanations proposed by extensional accounts (given, for example, by radical behaviorism) and intentional interpretations. These profound differences portray intentional behaviorism as a-ontological and reject the attempt to subordinate intentional accounts to extensional explanatory methodology. As mentioned above, I propose a different approach. Will/belief, which is perceived as real conscious experiences, constitutes the basis for providing mentalistic, purposive, will/belief explanations for behavior. Since *as yet* several unbridgeable differences exist between mental states and neurophysiological states (i.e., *as yet* the T(m/b) has not been discovered), I develop an argument that supports the claim that will/belief explanations do fulfill the scientific explanatory requirements, and therefore can be justifiably used scientifically.

**Foxall** proposes that intentional behaviorism attempts to set limits on the tendency to generate multiple hypothetical fictitious explanations for behavior in two ways: first, by limiting intentional accounts to specific kinds of behavior such as continuity of behavior over time; secondly, by allowing intentional interpretations only when neurophysiology and behavioral sciences have failed to offer satisfactory explanations. He argues that in these ways intentional behaviorism prevents fictitious explanations better than MD and MEF. Furthermore, he proposes that will/belief cannot be manipulated experimentally so as to change behavior.

**Foxall's** intentional behaviorism is inspired by Dennett's (1971, 1987) "intentional stance," which may be characterized not as a realist but as an instrumentalist approach. Dennett's approach accounts for the behavior of any system (e.g., humans, animals, artifacts such as computers) *as if* the system were a rational agent behaving according to will/belief ascribed to him. For example, it does not matter if a chess-playing computer really had beliefs and desires; all that matters is that the ascription of will/belief allows one conveniently and pragmatically to propose prediction and explanation of the computer's behavior. Dennett (1971) wrote: "All that has been claimed is that on occasion a purely physical system can be so complex, and yet so organized, that we find it convenient, explanatory, pragmatically necessary for prediction, to treat it as if it had beliefs and desires and was rational" (pp. 91-92). In addition, it seems to me that intentional behaviorism is influenced by Morgan's (1977/1894) canon, which demands that one should not interpret behavior by appeal to higher mental processes (such as will, belief, intention) when it can be accounted for in terms of simple lower processes (e.g., neurophysiological processes, discriminative stimuli, and reinforcements).

As can be surmised from the above, my approach is different and can be exemplified as follows. **Foxall** argues that radical behaviorism encounters

difficulties in explaining behavior continuity and that intentional behaviorism overcomes these difficulties by offering linguistic intentional interpretations. Nevertheless, I would like to propose an example of behavior continuity that illustrates that my approach seems to handle this particular behavior more easily than **Foxall's**. Here is the example: Dan traveled by train from town A to town B for a business meeting. He was told to get off the train at town B/station 2. Although Dan read a detective story on the train and never once thought about the meeting or town B/station 2, he got off the train at the correct location. While intentional behaviorism can propose an interpretation of Dan's behavior, I feel that it encounters more difficulties than MEF in dealing with the following two questions. First, since during the train ride Dan read a book, where and how was the information about the meeting and town B/station 2 preserved? Secondly, how may one treat explanatorily and coherently the fact that Dan's behavior can be conceived as composed of different components, some of which were in Dan's consciousness and the rest were not? It seems to me that intentional behaviorism would find it difficult to answer these two questions without assuming that Dan's behavior was composed of behavioral components that demand different kinds of explanations: mechanistic and mentalistic.

There are many ways one can manipulate and control one's will/belief so as to affect one's actual behavior; here are some illustrations that one may change his/her own will/belief. Consider Dan's train journey once again. How is it that Dan did not think about the business meeting even once? A possible answer is that he decided not to think about it because it was so important that thinking about it produced uncomfortable feelings. Thus one can increase/decrease and control his/her own will and beliefs. Moreover, one may behave so as to change one's own conscious mental states—for example, when one acquires information, takes a walk, reads a detective story, or goes to the opera. Here are several illustrations of the many ways to affect one's will/belief by external means: strengthening or weakening one's will/belief through presenting arguments or descriptions of possible consequences; using an external symbolic stimulus, as in the case when Iago sparks Othello's envy by means of his wife Desdemona's handkerchief—an envy that drives him to murder her; and by passionate religious and political harangues that incite a believer to perform extreme and terrible behaviors such as blowing oneself up so as to murder innocent people.

### **Further Developments**

Responding to these criticisms aroused the following two questions: first, what are the similarities and differences between my approach and other psychological approaches relevant to the issue of explanation of behavior? Secondly, what are the prospects of MD and MEF being accepted?

***Comparison of MD and MEF and other approaches:*** Two issues are important to psychological explanations: their kind (mechanistic/mentalistic) and the explanatory status of mental states. Based on these, I propose a hierarchy of the

## REPLIES TO CRITICS

five most relevant approaches to our concern (the theme of each approach is outlined briefly):

- a) *Radical behaviorism* conceives of mental states not as explainers of behavior, but as covert private behavior that is explained similarly to overt behavior—by appeal to environmental factors [social (verbal community) and non-social consequences], i.e., mental states do not play any role in the explanation of overt behavior.
- b) *Intentional behaviorism* supplements radical behaviorism with linguistic intentional interpretation in certain cases where the latter has failed to offer an adequate account. Like radical behaviorism, it does not view mental states themselves as explanatory events.
- c) *Neo-behaviorism* conceives mental states as theoretical constructs that are connected to stimulus, response, and other different theoretical terms. These theoretical constructs play a major role in explaining behavior.
- d) *Cognitive psychology* conceives mental states as special theoretical constructs that are interwoven in the whole system of information processing based on the analogy to a computer. The information-processing mechanism carries the function of explanation of behavior.
- e) *MD and MEF* conceive mental states, will/belief, as real, privately observable, and as providing mentalistic accounts, which fulfill the requirements for scientific explanation. Mentalistic and mechanistic explanations are matched to behavior and its parts, and thus offer a more complete account of behavior.

These five approaches generate a hierarchy, the “psychological explanation,” where radical behaviorism is located at one extreme and MD and MEF on the other. Roughly, one may differentiate radical behaviorism from the other four approaches in the following way: while radical behaviorism conceives of mental states as a part of behavior to be explained in terms of environmental factors, the other four approaches conceive of mental states as part of the explainers (e.g., the processes that account for behavior). While neo-behaviorism and cognitive psychology view mental states as unobservable theoretical constructs, and intentional behaviorism as linguistic terms, MD and MEF view mental states as special observable terms, since one can observe one’s own mental states but not others’.

While MD and MEF are an approach that appeals to both mechanistic and mentalistic explanations, the other four (except for intentional behaviorism to some degree) are approaches that appeal to mechanistic explanations. Radical behaviorism is mechanistic since it does not allow mental states to play an explanatory role (mental states are viewed as behavior to be accounted for by environmental factors).<sup>7</sup> Neo-behaviorism is mechanistic since it conceives of mental states as theoretical terms in mechanistic explanatory models used in the sciences (for example, the mental state of expectancy was conceptualized as an

---

<sup>7</sup> In fact, Skinner saw in mediating neurophysiological concepts a possible cover of mentalistic terms; see Moore (2008).

internal chain of fractional anticipatory goal-responses,  $s_g-r_g$ ). Cognitive psychology is mechanistic since it conceives mental states as theoretical terms in the mechanistic explanatory information-processing approach based on the computer analogy. Finally, intentional behaviorism is an approach that appeals to radical behaviorism accounts and (in certain cases) intentional linguistic interpretations (explanations that have to be consistent with neurophysiological and evolutionary approaches). So intentional behaviorism can be viewed as mechanistic and partially mentalistic; it is inspired by Dennett's approach of "intentional stance," and therefore one may conceive of intentional interpretations as based on the as-if view (see above).

Hence, all five approaches use mechanistic explanations for behavior, where only intentional behaviorism and MD and MEF appeal to mentalistic explanations as well.

**Acceptance of MD and MEF:** Given the mechanistic foundation of these five explanatory approaches, I started pondering the bleak chances of researchers accepting mentalistic explanations as a legitimate part of the scientific explanatory apparatus and applying my approach in their work. I examine two main reasons for this doleful possibility and reach an interesting conclusion.

*Constructing mechanistic explanations:* Several years ago I had a discussion with colleagues in the department of psychology at Haifa University about the question of whether conscious experiences, mental states, are needed for an explanation of behavior. They maintained that any behavior can be explained mechanistically without resort to consciousness. I did not agree. I believe that my colleagues were expressing the common approach to this issue. Why? Basically, because of the following "otiose" argument: given that many mathematical functions can be fitted to any set of empirical observations, and given that these functions express different mechanistic theories, it follows that it is possible to construct a mechanistic theory for any set of psychological observations. Hence, one may ask why we should appeal to the complex hybrid MEF (or to intentional behaviorism for that matter) if we can explain successfully a given phenomenon by appeal to a mechanistic explanation. That is, consciousness apparently does not enhance our understanding (predicting, explaining) over and above the understanding that one obtains by a mechanistic theory. In this regard Dawkins (1995) wrote: "There is no prediction we can make that if the animal has consciousness it should do X but not conscious it should do Y" (p. 139). And Flanagan (1992) conceived of "conscious inessentialism" as "the view that for any activity  $i$  performed in any cognitive domain  $d$ , even if we do  $i$  consciously,  $i$  can in principle be done nonconsciously" (p. 129). I don't agree.

First, as I illustrated above, in Rakover (2011/2012), and as has been argued by Foxall (2007, 2008), there are various behaviors that necessitate an appeal to mentalistic explanations because it seems difficult to propose for them purely mechanistic explanations.

Secondly, I believe that the otiose argument is inadequate. In my view, this argument is based on a crucial hidden assumption that many researchers have overlooked: the behavior that is explained in psychology is behavior stripped of



## REPLIES TO CRITICS

conscious meaning. The psychological indexes (such as number of correct responses and reaction time) do not carry any conscious meaning since these indexes are the same as those that represent a robot's behavior: they are public responses. Psychologists treat only those behavioral properties that belong to the public domain, properties that do not carry private conscious meaning. Thus, if the behavior to be explained is devoid of conscious meaning, no wonder that the appropriate explanation is constructed mechanistically—but in this case one does not account for a conscious meaningful behavior.

Now, if one does not accept the argument that psychological indexes are devoid of conscious meaning (since, after all, we are not robots and our behavior is loaded with conscious meanings) then (a) one has to admit that it is necessary to employ MEF to handle both kinds of behavior, mechanistic and mentalistic, or (b) one has to make the questionable suggestion that a mechanistic explanation can account for conscious behavior. Why is it questionable? Because, as yet, the T(m/b) has not been discovered. In view of these things one may suggest that a mechanistic explanation provides only a partial account for behavior and that an appeal to mental states should be recommended.

*Constructing will/belief explanations:* If one accepts the above argument and conclusion, then one may wonder how it is that we continue to provide pure mechanistic explanations for behavior. One possible answer is to suggest that explanations by appeal to will/belief are problematic. Although will/belief seems appropriate to handle conscious, meaningful behavior because will/belief itself bears consciousness, one may wonder how these mental states are causally responsible for behavior. The many answers to this question discussed in the philosophy of mind (see e.g., Kim, 2011) are not satisfactory. That is, as yet there is no answer. Hence, one may object the use of will/belief explanations by pointing out that these explanations are themselves inscrutable, and that it is a bad methodological strategy to explain an incomprehensible phenomenon by appeal to inapprehensible explainer.

I have tried to circumvent this knotty situation by interpreting [Will/Belief] not as a law, a theory, or an empirical generalization, but as an explanation scheme that generates specific purposive explanations for particular actions (see Rakover, 2007, 2011/2012). These actions are predicted and explained, not on the basis of the usual procedures of rationality (such as logic, mathematics, and probability) but on the basis of practical reasoning (i.e., the rational deliberations that one makes when one chooses to perform a specific behavior that one believes will realize one's will). As can be seen, this rationality basis differs from the causal relation employed in the sciences; it is a process of rational reasoning on how to conduct one's own behavior.

Now, if one is not convinced by the latter argument (since it does not meet rigorous scientific standards) one has no other option but to reject my approach—the employment of mentalistic explanations—and continue using mechanistic explanations. But given the above problems that mechanistic explanations encounter, what reason has one to persist in offering pure mechanistic explanations? To be frank, I don't see any reason except for the faith that one day

everything will be accounted for mechanistically—but if this is the only answer I don't see why, meanwhile, one should not employ MD and MEF.

### References

- Allen, C. (2011/2012). Methodological questions begged. *Behavior and Philosophy*, 39/40, 83-87.
- Branch, M. (2011/2012). Comments on Rakover's "A plea for methodological dualism and multi-explanation framework in psychology." *Behavior and Philosophy*, 39/40, 89-92.
- Bechtel, W. (2008). *Mental mechanisms: Philosophical perspectives on cognitive neuroscience*. New York: Routledge.
- Bechtel, W., & Wright, C. D. (2009). What is psychological explanation? In J. Symons & P. Calvo (Eds.), *The Routledge companion to philosophy of psychology* (pp. 113-130). London and New York: Routledge.
- Chalmers, D. J. (1996). *The conscious mind: In search of a fundamental theory*. New York: Oxford University Press.
- Chalmers, D. J. (1997). Facing up to the problem of consciousness. In J. Shear (Ed.), *Explaining consciousness—the 'hard problem'*. Cambridge, MIT: The MIT press.
- Dawkins, M. S. (1995). *Unraveling animal behavior* (2nd ed). Essex: Longman Scientific & Technical.
- Dennett, D. C. (1971). Intentional systems. *Journal of Philosophy*, 8, 87-106.
- Dennett, D. C. (1987). *The intentional stance*. Cambridge, MA: The MIT Press.
- Flanagan, O. (1992). *Consciousness reconsidered*. Cambridge, MA: The MIT Press.
- Foxall, G. R. (2007). Intentional behaviorism. *Behavior and Philosophy*, 35, 1-55.
- Foxall, G. R. (2008). Intentional behaviorism revisited. *Behavior and Philosophy*, 35, 113-155.
- Foxall, G. R. (2011/2012). The intentional and the extensional. A response to Rakover. *Behavior and Philosophy*, 39/40, 103-105.
- Harnad, S. (2000). Correlation vs. causality: How/why the mind-body problem is hard. *Journal of Consciousness Studies*, 7, 54-61.
- Hertz, J., Krogh, A., & Palmer, R. G. (1991). *Introduction to the theory of neural computation*. Redwood City, CA: Addison-Wesley Publishing Company.
- Kim, J. (2011). *Philosophy of mind* (3rd ed). Boulder, CO: Westview Press.
- MacCorquodale, K., & Mheel, P. E. (1948). On a distinction between hypothetical constructs and intervening variables. *Psychological Review*, 55, 95-107.
- Malone, J. C. (2011/2012). Mind, consciousness, will, and belief: Rakover's multi-explanation framework. *Behavior and Philosophy*, 39/40, 93-102.
- Moore, J. (2008). *Conceptual foundations of radical behaviorism*. Cornwall on Hudson, NY: Sloan.
- Morgan, C. L. (1977/1894). *An introduction to comparative psychology* (D. N. Robison, Ed.). Washington, DC: University Publications of America.
- Rakover, S. S. (1990). *Metapsychology: Missing links in behavior, mind and science*. New York: Paragon/Solomon.
- Rakover, S. S. (2007). *To understand a cat: Methodology and philosophy*. Amsterdam/Philadelphia: John Benjamins.
- Rakover, S. S. (2011/2012). A plea for methodological dualism and a multi-explanation approach in psychology. *Behavior and Philosophy*, 39/40, 17-43.

## REPLIES TO CRITICS

- Rakover, S. S. (2012). Psychology as an associational science: A methodological viewpoint. *Open Journal of Philosophy*, 2, 143-152.
- Rakover, S. S. (2013). Explaining the face-inversion effect: The Face-Scheme Incompatibility (FSI) model. *Psychonomic Bulletin & Review*, 20, 665-692.
- Salmon, W.C. (1989). *Four decades of scientific explanation*. Minneapolis: University of Minneapolis Press.
- Shepard, R. N., & Metzler, J. (1971). Mental rotations of three-dimensional objects. *Science*, 171, 701-703.
- Skinner, B. F. (1953). *Science and human behavior*. New York: Free Press.
- Skinner, B. F. (1974). *About behaviorism*. New York: Knopf.
- Wright, C. D., & Bechtel, W. (2007). Mechanisms and psychological explanation. In P. Thagard (Ed.), *Philosophy of psychology and cognitive science*. (pp. 31-77). Amsterdam: Elsevier.