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Consciousness and Action: Does Cognitive Science Support (Mild) Epiphenomenalism?

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ABSTRACT

Questions about the function(s) of consciousness have long been central to discussions of consciousness in philosophy and psychology. Intuitively, consciousness has an important role to play in the control of many everyday behaviors. However, this view has recently come under attack. In particular, it is becoming increasingly common for scientists and philosophers to argue that a significant body of data emerging from cognitive science shows that conscious states are *not* involved in the control of behavior. According to these theorists, *nonconscious states* control most everyday behaviors. Andy Clark ([2001]) does an admirable job of summarizing and defending the most important data thought to support this view. In this paper, I argue that the evidence available does not in fact threaten the view that conscious states play an important and intimate role in the control of much everyday behavior. I thereby defend a philosophically intuitive view about the functions of conscious states in action.

1 Introduction

Epiphenomenalism about consciousness is not an intuitively satisfying position.[1] This fact registers our natural tendency to suppose that conscious

[1] Which is not to say that no one believes or defends the view. Nor is it to say that there are no reasonable arguments for the view. I am only noting that it is not a view one is likely to find plausible prior to inquiry. Whether the best reasons ultimately lead one to epiphenomenalism is a matter I leave open here.

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experiences have some causal impact or bearing upon what we do. Of course, it is notoriously difficult to say precisely what conscious states do for us—what their role is in the behavioral process. In a recent paper, Andy Clark ([2001]) suggests that much of our commonsense thinking about the functional role of conscious visual states reveals a commitment to the following principle, which he dubs the Assumption of Experience-Based Control (EBC). According to EBC:

> Conscious visual experience presents the world to the subject in a richly textured way, a way that presents fine detail (detail that may, perhaps, exceed our conceptual or propositional grasp) and that is, in virtue of this richness, especially apt for, and typically utilized in, the control and guidance of fine-tuned, real world activity. (Clark [2001], p. 496)

I agree with Clark that something like EBC underlies our thinking about the functional role of conscious visual experiences—to the extent that we take a position, upon reflection, on this issue. Yet, plausible though EBC may seem, it is becoming increasingly common for scientists and philosophers to argue that recent empirical studies suggest that it is false.[2] According to these theorists, the incoming evidence suggests that much of our everyday behavior is actually controlled by *nonconscious* mental states. Clark ([2001]) presents the most important evidence in favor of this view and argues that we should drop EBC in favor of another hypothesis, the Hypothesis of Experience Based Selection (EBS), according to which:

> Conscious visual experience presents the world to a subject in a form appropriate for the reason-and-memory-based selection of actions. (Clark [2001], p. 512)

'Reason-and-memory-based selection of action' refers to the process of forming a decision about what to do, what course of action to take, where this decision is determined, in part, by one's beliefs and desires. Clark's view, then, is that conscious states interact with reason and memory (perhaps in a faculty of practical reason) to allow a creature to settle on a course of action. Once a creature has made a decision about what to do, however, nonconscious states take over, controlling the execution of the selected action.

I am not opposed to EBS *per se* (for I do not deny that conscious states play a role in reason-and-memory-based selection of action). However, one can grant that conscious states play the roles specified in EBS without denying EBC. And indeed, I believe it would be a mistake to reject EBC outright. To

[2] The most important survey of the evidence, and the definitive contemporary statement of the argument for the view I will be discussing, is (Milner and Goodale [1995]).

reject EBC is to adopt what I call mild epiphenomenalism about conscious experience—the view that a great number of our conscious experiences play no role in controlling and guiding most of our everyday engagements with the world. In this paper, I will argue that the evidence from cognitive science, which Clark surveys, poses no threat to EBC and, consequently, does not justify even mild epiphenomenalism. In particular, I distinguish two readings of EBC, a restricted and a general version. I argue that, at best, the evidence Clark presents is evidence against EBC in its restricted form only. But even this case is weak, I argue, as Clark ignores plausible interpretations of the evidence compatible with EBC, even in its restricted form. Nevertheless, refuting EBC in its restricted form is not sufficient to realize Clark's polemical aims. For, first, the restricted version does not capture the intuitively plausible view about the functions of conscious states that EBC is supposed to be. And second, the falsity of the restricted view does not even remotely suggest that EBS is true. The intuitively plausible version of EBC—the general version—thus emerges unscathed by the evidence. As such, it is a mistake to restrict the functions of conscious states to those roles identified in EBS. Conscious visual states do sometimes play a role in the control of behavior—under any plausible understanding of 'playing a role in the control of behavior'.

Retaining EBC (properly understood) allows us to maintain intuitively plausible ideas about the roles of conscious states in behavior. But there are larger issues at stake here. For one thing, Clark suggests that the rejection of EBC casts some doubt on (though, he admits, does not refute) the view that conscious states have nonconceptual content ([2001], pp. 516–7). Furthermore, various philosophers of mind appeal to the evidence I will be discussing to support some version of the so-called higher-order theory of consciousness, and do so precisely because they take the evidence to cast doubt on EBC (see Carruthers [2000], especially Chapter 6, for a particularly clear case of this strategy). Higher-order theories of consciousness seem to me implausible, largely because they make consciousness too sophisticated a cognitive achievement.[3] There are also (related) consequences for views about the evolution of consciousness—when it appeared, and which animals have it. For, according to Clark's EBS, only animals with *reason* possess conscious states. As such, consciousness will be a fairly recent achievement (in evolutionary terms), confined perhaps to higher primates and maybe dolphins. This seems to me doubtful. It is more likely that consciousness is quite widespread in the animal kingdom, found in most mammals, and probably birds and some reptiles. In defending a version of EBC, not only can we retain

[3] For arguments against such higher-order theories of consciousness—arguments with which I am fully sympathetic, see (Dretske [1995]; Güzeldere [1995]). For a comprehensive defense of higher-order theories, see (Carruthers [2000]).

a common view of the functional roles of conscious states, we can also retain attractive views about the content and evolution of consciousness.

2 Clarifying EBC

To assess the empirical case against EBC properly, we need to get a clearer understanding of EBC. In this section, I identify the part of EBC that Clark argues against (he does not argue against all parts of the assumption) and scrutinize some of the key notions at play in EBC, especially the notions of 'control' and 'fine-tuned activity'. The latter phrase will be especially important, for depending on how we understand it, we get significantly different versions of EBC. This will be important when we consider Clark's evidence against EBC, for we shall have to consider which version (if any) the evidence counts against and whether casting doubt upon this thesis achieves Clark's polemical aims.

Return now to EBC (see above). It incorporates three main claims:

> Phenomenological claim (PC): Conscious visual experience presents the world to the subject in a richly textured way, a way that presents fine detail (which may exceed our conceptual or propositional grasp).
>
> Explanatory/causal claim (ECC): In virtue of the presentation of fine detail, conscious states are especially apt for the control and guidance of fine-tuned, real-world activity.
>
> Frequency claim (FC): Conscious visual experiences are typically utilized in the control and guidance of fine-tuned, real-world activity.

PC and ECC are plausible enough, and Clark does not attempt to cast serious doubt on either of these claims in his paper (though he does, as noted above, think that adopting EBS rather than EBC, lends at least *some* support to views that reject nonconceptual content). Clark's real target is the third part of EBC, FC. Against FC, Clark argues that the best empirical evidence available suggests that it is *nonconscious* visual states—not conscious states—that 'control and guide' most of our everyday behavioral engagements with the world. Let us now turn a critical eye to some of the key notions used in formulating EBC, particularly the ideas of 'control and guidance' and 'fine-tuned activity'.

2.1 Control and guidance

The idea of a mental state's being 'utilized in the control and guidance' of a behavior is an important part of EBC, yet Clark offers little analysis of this key idea. Although I cannot go into detail here, it will be helpful to bear the following points about control and guidance in mind when we turn to the evaluation of the case against EBC.

First, a mental state can be utilized in, or play a role in, the control and guidance of a behavior even if it is not the sole cause, or even an immediate cause, of a behavior. The causation of most behavior is a complex affair, involving not just the senses and the brain, but events in the spinal cord and muscles—events that are closer to being the immediate causes of behavior than any mental state (conscious or not). Indeed, visual states can be utilized in the control and guidance of behavior even if they are not the immediate *mental* cause of a behavior. For we often develop *motor plans* for action—instructions developed by motor control centers that cause (say) limb movements—on the basis of visual information (see, e.g., Hollerbach [1990]). These motor programs can be—and typically are—fully nonconscious. So, a conscious state can play a role in the control and guidance of a behavior even if (i) there are other causal factors in bringing about the behavior and (ii) some of those factors are nonconscious mental states. Therefore, it is fully consistent with EBC that nonconscious (mental) phenomena also play an important role in the guidance/control of behavior. One cannot refute EBC, therefore, by merely providing evidence that nonconscious mental phenomena play *some* role in the 'control/guidance' of some behavior.[4]

In a positive mode, we can say that a mental state—particularly a sensory/perceptual state—plays a role in the control/guidance of some behavior when the state provides the information used by the motor system in developing motor instructions (e.g., information about joint movements) that bring about the behavior in question. Consider, for instance, reaching for one's coffee cup. You glance over at your cup and reach for it, extending your arm, orienting your hand in a particular way, and configuring your fingers in a particular way. According to the present suggestion, your visual experience of the cup will have been utilized in the control and guidance of this behavior if it provides the information your motor system uses in causing your hand to move in the relevant direction and distance, to take on the appropriate

[4] Some of Clark's claims seem to suggest that he overlooks this point. For instance, Clark at one point notes that, 'When [. . .] we continue to look as we bring our finger down onto the center of the printed cross (an act that we naturally suppose to involve conscious states), we may be looking so as continuously to feed visual information to a thoroughly nonconscious action guiding system—a kind of vision using zombie' ([2001], p. 505). Perhaps. But strictly speaking, this is beside the point. Whether the *systems that use* visual information are nonconscious is not the issue; the question is whether or not the *information those systems use* is encoded in a conscious state. Both may be true. Conscious states may well be (indeed, they certainly are) *inputs* to a wide variety of nonconscious systems. That does not show that conscious states do not play a role in the processes those systems govern! However, the balance of textual evidence—some of which I note in the next note—suggests that Clark is simply speaking a bit loosely here, and that he actually intends to be asserting that the inputs to the action-guiding system (not just the system itself) are nonconscious.

orientation, and in configuring your fingers appropriately. This seems to me to be both a plausible hypothesis about how visual states play a role in the behavioral process and a sufficiently important role to count a state as having been utilized in the control and guidance of behavior.[5]

2.2 Fine-tuned activity

Clark uses the notion of 'fine-tuned activity' in his official formulation of EBC (see above). As we shall see, this notion is crucial to the scope and plausibility of EBC. It requires some analysis, however, as Clark says little to specify what 'fine-tuned activity' is supposed to be. I will consider two interpretations of 'fine-tuned activity', each of which finds some textual support. These interpretations lead to two versions of EBC, which I call EBC(res) and EBC(gen). I shall show that Clark can accomplish his argumentative aims only by casting doubt on EBC(gen). It will be crucial to keep this in mind when we turn to the evidence Clark presents against EBC.

Some of Clark's examples suggest that he intends 'fine-tuned activity' to refer to fine, slight, or minor adjustments to behavior, adjustments made to behaviors during their execution. For instance, suppose you are trying to touch the center of a cross printed on a piece of paper (Clark [2001], p. 496; the example is due to Brain O'Shaughnessy). As you bring your finger close to the mark, you will make minor adjustments to the speed and position of your finger, so as to bring your finger down on the very center of the cross. These minor adjustments count as 'fine-tuned activities' on the present understanding. This way of understanding 'fine-tuned activity' also makes sense of the fact that Clark uses, as a further example of such activity, the minor adjustments to speed that a motorcyclist makes in response to changing road conditions ([2001], pp. 498–9; example due to Adrian Cussins).

On the other hand, perhaps Clark is simply using 'fine-tuned activity' to refer to goal-directed behavior, behavior that is 'tuned' with respect to an aim, or directed at a target. Such behaviors would include intentionally bringing one's finger down upon the center of a printed cross, a motorcyclist's speed adjustments, and one's reaching for a cup of coffee. 'Fine-tuned activities', so construed, contrast with 'aimless' behaviors such as random flailing of the limbs, unintentional behaviors such as slips of the tongue, and involuntary movements such as reflexes. Although this interpretation of 'fine-tuned activity'

[5] Indeed, several of Clark's comments suggest that he assumes a similar view about what it would be for a mental state to be utilized in the control and guidance of behavior. For instance, in discussing an example from O'Shaughnessy, Clark writes, 'this assumption—that conscious visual *experience* provides the very information continuously used for visually based motor control [i.e., EBC]—is precisely the one shortly to be challenged' ([2001], p. 496).

is somewhat odd to the ear, there is some reason to suppose that it captures Clark's intent, as I will show below. At any rate, call this the 'general' reading of 'fine-tuned activity', the former the 'restricted' reading.

These readings lead to two versions of EBC, EBC(gen) and EBC(res). (And since Clark's argument is directed only against claim FC embedded in EBC, I will present only the corresponding versions of FC in EBC(gen) and EBC(res).)

> EBC(gen): Conscious visual experiences are typically utilized in the control and guidance of voluntary/intentional behaviors.
>
> EBC(res): Conscious visual experiences are typically utilized in the control and guidance of the fine/minor adjustments made to behaviors during their execution (i.e., 'on the fly').

Which version of EBC does Clark have in mind? As I noted above, most of Clark's examples suggest the restricted reading of 'fine-tuned activity', and hence support EBC(res). Despite the textual support, however, this interpretation faces serious problems in the context of Clark's argument against EBC. First, these slight adjustments are only a part—a relatively minor part—of the behavioral process. As such, even if they constitute part of every behavior, there are other aspects of each behavior that need to be controlled. For instance, in bringing one's finger down upon the center of a printed cross, one not only needs to make minor adjustments to the position of one's finger, one needs to move one's finger in the general direction of the cross, and the appropriate distance, so as to get one's finger in the general vicinity of the cross. These latter, grosser, aspects of behavior need to be controlled every bit as much as the minor adjustments one makes to bring one's finger down on the very center of the cross. Hence, even if it is true that conscious states do not enter into the control/guidance of minor adjustments to behavior, they may well be utilized in the control/guidance of behavior as a whole. Moreover, it is surely part of our commonsense view of the functional role of conscious states that they enter into the control of such grosser aspects of behavior. In other words, it is no part of commonsense that conscious states enter *only* into the control/guidance of the fine adjustments of behavior that we make 'on the fly'. EBC, so understood (i.e., EBC(res)), does not fully reflect/exhaust commonsense about conscious visual states, contrary to Clark's claim that EBC is a 'commonsensical' assumption ([2001], pp. 495, 499). But, if EBC(res) is not an intuitively plausible view, evidence against it is not as philosophically interesting as Clark suggests. Finally, Clark's EBS is neither implied nor supported by the falsity of EBC(res). For, since fine adjustments do not exhaust the aspects of behavior requiring control, there is plenty of room for conscious states to play a role in control and guidance of *every* behavior *even if* EBC(res) is false. Hence, it would be an error—a drastic leap, to say the least—to conclude that conscious states function only in one's deciding what to do, that is, in the 'reason-and-memory-based selection' of behaviors. EBS, therefore, is not supported by the falsity of EBC(res).

Perhaps, then, Clark intends the general interpretation of 'fine-tuned activity', and hence EBC(gen). Despite a relative dearth of positive textual support for this interpretation, several considerations speak in its favor. First, Clark does sometimes drop the 'fine-tuned' modifier in favor of speaking simply of the role of conscious states in the control of motor activity in general (see, e.g., [2001], pp. 496, 499, 500, 505). Second, this interpretation of 'fine-tuned activity' leads to a version of EBC that, unlike the former, does gel well with our commonsensical or intuitive conception of the functions of conscious vision. For, as I commented above, it is surely part of our commonsense view of the functional role of conscious states that they enter into the control of such gross aspects of behavior as the direction and general location to which one moves one's hand when reaching for something. Since Clark contends ([2001], pp. 495, 499) that EBC is an 'intuitive' or 'commonsensical' assumption, this supports interpreting EBC as EBC(gen). Third, as suggested above, Clark needs just such a general interpretation of 'fine-tuned activity' for EBS to be supported by the falsity of EBC. For, if the falsity of EBC is to support the view that conscious states are involved only in the 'reason-and-memory-based selection' of actions, then EBC must cover all aspects of the control/guidance of behavior. Otherwise, it is perfectly consistent with the falsity of EBC that conscious states are involved in some aspects of behavioral control, contrary to what EBS suggests. So, given Clark's polemical aims, he needs a general understanding of 'fine-tuned activity' such as occurs in EBC(gen).

So, some considerations favor each interpretation of 'fine-tuned activity'. Which does Clark actually accept? Textual evidence does not settle the question, and I propose to leave it open. What is important here is that we have uncovered two possible readings of EBC. Keeping both versions in mind, we will need to consider closely which version (if either) Clark's evidence counts against. For, if Clark hopes to show that 'our experiences *don't* control our behavior', that 'experiential content and action-guiding content [are] quite distinct' ([2001], p. 499), he must provide a case against EBC(gen), and not simply EBC(res). And again, for EBS to be even remotely suggested by the falsity of EBC, the latter thesis must be construed as EBC(gen). Otherwise, Clark has not established his 'main moral', namely, that 'conscious visual perception is part and parcel of a cognitive system dedicated to recall, reason, and imagination and only indirectly associated with systems controlling the detailed execution of selected actions' ([2001], p. 517).[6]

Before considering the case against EBC, I want to make one more point about our intuitive view about the role of conscious states in the control/guidance of behavior. This is, that we do not (nor should we) think that conscious states play any role in controlling/guiding reflexes (e.g., the

[6] It might be objected that I have overlooked at least two other initially plausible ways to understand 'fine-tuned activity'. But, in addition to facing other problems (which I will comment upon shortly), textual evidence shows that they do not capture Clark's intent. The first is that 'fine-tuned activity' is highly learned, or automated activity, activity that has been 'fine-tuned' through repetition and practice. Such behaviors would seem to contrast with 'poorly tuned' behaviors, that is, behaviors poorly manifested by us. The second interpretation is that, 'fine-tuned activities' are slight or minor movements. This interpretation is suggested by the fact that Clark sometimes speaks of the control/guidance of 'fine motor activity' ([2001], p. 499). Both of these interpretations, however, appear to be ruled out by Clark's use of O'Shaughnessy's example of moving one's finger down onto the center of a printed cross ([2001], p. 496). For this movement is neither highly practiced/routine nor a very slight/minor movement (at least, it need be neither of these things).

Lack of textual support aside, these interpretations of 'fine-tuned activity' face difficulties similar to those facing the idea that 'fine-tuned activities' are minor adjustments to ongoing behavior. Consider first the idea that a fine-tuned behavior is one that has been 'fine tuned' through repetition and practice. Examples include the hand/finger movements of the skilled typist or musician. There are two problems, if this is what Clark has in mind by 'fine-tuned activity'. First, I am not convinced that it is part of commonsense that conscious visual states play much of a role in the control of these movements. I doubt that many really suppose that, for instance, skilled typing is controlled by conscious visual states. When I was learning to type, I was taught *not* to look at my fingers, and I typically do not do so now (unless the key is one that I do not use very often, like the number 7 key). Indeed, for the standard letter keys, the only role my visual states typically play in my typing is informing me of mistakes. Still, often enough, I do not even need the visual feedback. I can often tell I have made a mistake simply by knowing that my finger moved to the wrong place. Similar remarks apply to the musician's finger/hand movements. The trouble is that highly learned behaviors are things that—as we sometimes say—can be done 'on autopilot', 'in my sleep', and so forth. Once we have decided to perform some routine task, we can often perform the task without feedback from visual states, conscious or not. We can do these things in the absence of information from conscious visual states. As such, I doubt that EBC captures an intuitive, or commonsensical view about the functional roles of conscious states if it is restricted to these sorts of behaviors. Consequently, evidence against this form of EBC is not as philosophically interesting as Clark suggests—it is evidence against a view that we do not really take very seriously.

More significantly, if EBC is restricted to highly learned behaviors, then even if we do learn something interesting in finding evidence that it is false, we do not thereby have much evidence against the broader idea that EBC was intended to capture, namely, the idea that conscious visual states are often important for the control of behavior. This is because there are many behaviors that are not highly practiced/learned in which conscious states may play a control/guidance role. But then it may still be true that conscious states are important to the control of behavior even if EBC (restricted to skilled behavior) is false. Consequently, Clark's EBS does not follow from the falsity of EBC restricted to highly practiced behavior.

Precisely analogous remarks apply to the view that 'fine-tuned activity' refers to slight, or minor, movements. First, EBC simply is not a commonsensical view if it restricted to slight/minor movements. For, commonsense suggests that conscious visual states are utilized in the control/guidance of larger movements (such as reaching for one's coffee cup) too. Second, even if EBC (restricted to slight/minor movements) is false, conscious states may still be utilized in the control/guidance of just such larger movements. As such, the falsity of EBC so restricted does not imply that conscious states are not, in general, important to the control and guidance of behavior, and it does not imply that EBS is true. But, as I said above, textual evidence speaks against both of the present interpretations.

patellar reflex). Reflexes are generally hardwired, fixed, and controlled by (relatively) simple neural circuits. There is no need to invoke conscious states in the explanation of the occurrence of a reflex. So, whatever EBC claims, it should not be understood to include reflex behaviors. I believe Clark recognizes this point—for his formulation of EBC says that conscious states 'typically' are utilized in the control and guidance of behaviors. This formulation allows that some sorts of behaviors, such as reflexes perhaps, need not be mediated by conscious states. Nevertheless, it will be important to keep in mind the present point about reflexes when we consider the evidence against EBC, a task to which I now turn.

3 The empirical case against EBC

Clark presents several lines of experimental evidence against EBC. Drawing upon points made in the previous section, I will show that, at best, they only provide evidence against EBC(res). EBC(gen) is untouched. I do not deny that the results of the experiments Clark discusses tell us something interesting about the functional roles of some mental states in behavioral control/guidance. I only deny that they show that conscious states play no role in these aspects of behavior. Clark fails, therefore, to show that conscious vision is unimportant to the ongoing control of most of our real-world motor behavior.

Clark begins with a discussion of a series of experiments, by Lee and Lishman ([1975]), designed to test subjects' postural response to visual information about motion. In the studies, subjects stand on the floor of a large room. Unbeknownst to them, they are enclosed within a smaller, floorless, room, which is suspended from the ceiling of the large room. In the experiments, the small room would sway slightly, and, even though the floor on which they stood was motionless, it was shown that the subjects would make slight postural adjustments to compensate for the apparent movement of the room.[7] This, even though the subjects were apparently unaware of any movement in the room—that is, they did not report being aware of any movement, nor would they report awareness of making postural adjustments. Clark takes this to be evidence that a 'great deal of our daily, fine-tuned motor activity proceeds quite independently of the current contents of conscious visual experience' ([2001], p. 499). This is a mistake for three reasons.

First, although it is perhaps true that these postural adjustments are not controlled by conscious visual experiences of the room's movement, this does

[7] Clark describes the experimental setup somewhat inaccurately, claiming that the subjects stand in the smaller, swaying room. Although this leads him to claim, incorrectly, that the subjects rely on proprioceptive information in correcting their posture, this does not lead him into any significant error in his interpretation of the results.

not provide evidence against EBC since—as I noted above—EBC should not be understood as applying to reflexes. Slight postural adjustments, however, are automated behaviors—a species of reflex (albeit a complex one, sensitive to many sources of information, including information from the muscles and joints, the inner ear, and vision; see Matthews [1998], p. 207).

Second, this is, at best, evidence against EBC(res). For these 'slight adjustments' are a far cry from the larger-scale movements that we generally assume conscious states to play a role in, such as reaching for an object or moving one's foot to crush an empty aluminum can.

Third, you simply cannot generalize from what happens in these sorts of cases to what happens in a 'great deal of our daily activities'. The fact—if it is a fact—that conscious states do not play a role in the control of minor postural adjustments does not even begin to suggest that they do not play such a role in more typical movements—goal-directed or intentional movements, in particular. This case does not provide compelling evidence against EBC(gen).

The second case Clark discusses is more interesting. In this series of experiments, subjects were asked to point to a stimulus, such as a small light, that would suddenly appear in peripheral vision. At times, the stimulus would unpredictably move a small amount. As Clark reports ([2001], p. 500), subjects would often accommodate to the movement, by making both a saccade (a rapid, ballistic eye movement) and a corrective hand motion (so that they were both looking and pointing at the stimulus' new position; see Milner and Goodale [1995], pp. 156–60). Nevertheless, subjects typically report no awareness of the stimulus' movement (Milner and Goodale [1995], pp. 157, 160–1). Clark concludes (along with Milner and Goodale) that the subjects' corrective hand movements are guided by nonconscious visual information—contrary to what EBC suggests.

In addition to these results, Clark notes, first, that when the movement of the stimulus was large enough to attract attention, subjects' corrective eye and hand movements were *less accurate* than when attention was not engaged ([2001], p. 500). Second, Clark appeals to the following results from Wong and Mack ([1981]). In their study, subjects were required to fixate on a stimulus (a small light) that would appear surrounded by a 'frame' (a horizontal ellipse). After a brief 'blank', the target and frame would reappear. On certain trials, the target reappeared in the same location, with the surrounding frame displaced slightly to the left or right. The subjects were required to refixate on the target. Interestingly, subjects would report that it was the target, not the frame, that had moved (Wong and Mack [1981], p. 125). Nevertheless, eye-tracking devices reveal that subjects' refixation saccades were to the actual location of the target (rather than the 'perceived' location, which, per their reports, differed from its original location; Wong and Mack [1981], p. 125). In a second series of experiments, subjects again fixated on a framed stimulus, and then refixated upon it as it reappeared. Again, the frame was displaced so as to induce a perceived (though illusory) movement of the target. Fixations, as above, were to actual rather than perceived locations (Wong and Mack [1981], pp. 127–8). This time, however, subjects were instructed to make a final saccade to the target's original location. Wong and Mack showed that this *memory driven* saccade was controlled by the perceived (rather than actual) location. That is, the subjects move their eyes to compensate for the target's perceived movement (a movement it did not in fact make; Wong and Mack [1981], pp. 127–8).[8] Clark contends that the evidence concerning memory-driven responses, the evidence concerning eye/hand adjustments to noticed stimulus movements, and the evidence concerning eye/hand adjustments to unnoticed stimulus movements all converge on a single conclusion: conscious visual states are not utilized in the direct control/guidance of eye and hand movements.

The experimental results Clark discusses are quite interesting, but do they support the conclusion Clark draws? Do they form a strong case against EBC? I think not. Let me begin with the initial results, concerning unnoticed hand and eye movements made in response to a slight movement of the target stimulus. These results, at best, count only against EBC(res), for they concern minor adjustments to behavior alone. It remains compatible with the results of the experiments that conscious visual states are involved, for instance, in the control/guidance of the *initial* eye movements—the first saccade subjects make towards the target stimulus. For it may well be that conscious vision provides the spatial information used to generate the initial saccade. If so, then conscious states are 'utilized in the control and guidance' of the behaviors under investigation (eye and hand movements). If this is true,

[8] Those who have read Clark's paper will note that my discussion of Wong and Mack's experiments differs significantly from Clark's. This is because Clark omits important details of the experiment, which leads him to inaccurately report the results and to (misleadingly, in my view) suggest continuity with the experiments described in the previous paragraph. Here is what Clark says in connection with the present experiment: 'Wong and Mack ([1981]) showed that subjects who automatically and unconsciously accommodate the smaller displacements will, if subsequently asked to point to the remembered location of the (now removed) target, actually point to the original (nondisplaced) location' ([2001], p. 500). A few comments. First, when describing evidence that is supposed to support a claim about what subjects can do 'unconsciously', you do not get to describe what they are doing as unconscious. Second, the use of 'the smaller displacements' here suggests that they are the same displacements as occurred in the experiments Clark has just described—namely, those I described in the previous paragraph of text. Not so. The experimental setups are quite different. Furthermore, in the Wong and Mack experiments, the subjects are always aware of a movement—hence the claim that their accommodations are 'unconscious' is unwarranted. Finally, when the subjects do *look* to a location (they did not do any pointing) it is usually *not* to the 'original' location of the target. On the contrary, they usually look to a position that the stimulus *never was*, though *would have been*, had their 'perception' of its movement been accurate. So, although Clark's presentation of the experimental results seems to provide a persuasive case against EBC, the actual experimental results (as I argue in the text) are much less convincing.

then it has not been shown that conscious visual states are not important to the control/guidance of behaviors in general. At best, these experiments can show only that nonconscious states guide the minor corrections we make when we fixate upon some stimulus. This is not a serious blow to our commonsense views about the roles of conscious states in action.

But, in fact, it is questionable whether these experiments even cast doubt on EBC(res). For, an important fact about these experiments is that the stimuli initially appear in the peripheral vision (a fact Clark omits). Now, as is well known, spatial resolution in the peripheral field is not as good as foveal vision. As such, subjects are not able to develop a precise representation of the initial location of the stimulus. Moreover, the movement of the stimulus occurs *while the subjects are making a saccade* to foveate on the target (another important fact Clark omits). As is well known in perceptual psychology, perception is diminished during saccades (a phenomenon called 'saccadic suppression'; see Palmer [1999], p. 523; see also the growing literature on change blindness, e.g., Mack and Rock [1998]). As such, it is not very surprising that they fail to notice the movement of the stimulus, for the movement occurs when vision is poor, and they never get a good 'fix' on the target's location in the first place. Therefore, subjects' failure to notice the movements of the stimuli does not provide good evidence against the view that conscious visual states are providing the information used to generate eye and hand movements, for the initial saccade could be caused (in part) by a conscious representation of the general location of the target in the periphery. During the saccade, the stimulus moves, but little to no information is received about the movement. After the saccade occurs, the subject performs a corrective saccade/hand movement *again based on conscious information about the target's position* (this time more accurate information, since the stimulus is closer to central vision). Even if all of this were true, one would not be surprised that subjects fail to notice the movement, since it occurs during a period of poor vision, even though one's eye/hand movements are controlled by conscious visual states. So, these results fail to cast significant doubt on any version of EBC—restricted or general.

Turn now to the second set of results Clark mentions, concerning noticed movements. As Clark notes, corrective eye/hand movements are less accurate, and less fluid, when the target's movement is noticed. This is interesting, but why would one suppose that it casts doubt on EBC(res)? The reasoning seems to be this. Noticed movements produce less accurate responses than unnoticed movements. Noticed movements are consciously perceived. Hence, if conscious states controlled responses to unnoticed movements, we should expect them to be less accurate as well. Therefore, they are not involved in the control of responses to slight, unnoticed movements. Therefore, they are not involved in the control of 'fine-tuned' eye and hand movements. Unfortunately, this reasoning is multiply flawed. First, there are alternative explanations of why the subjects perform more poorly upon noticing the stimuli move. One possibility is that the poorer performance results from subjects' *noticing* that the stimulus has moved, rather than the conscious awareness of movement per se. For noticing *that* something has occurred is a form of conceptual response—a kind of perceptual judgment. Conceptual representations, however, are 'chunky'—they group together similar objects, events, and properties. They contrast, in this respect, with nonconceptual representations, which have much finer grain (they are more discriminating). Since conceptual representations are coarser grained, it is not too surprising that they would give rise to less refined movements. That explains the poorer pointing performance that results when subjects notice that the target stimulus has moved. If this explanation is correct, then it is not the subjects' conscious visual awareness per se that is responsible for their less accurate hand/eye movements. As such, one cannot infer—as the reasoning reconstructed above suggests—that subjects would have performed poorly in the initial task had their hand/eye movements been controlled by conscious visual states.[9]

Of course, one could reject the alternative explanation I just outlined if one could show that there is no difference between one's being consciously aware of something and one's noticing that something is so, or if one could show that conscious visual states have no nonconceptual content. But, on the one hand, it is implausible to suppose that being consciously aware of something is no different from noticing that something is the case. Instances of awareness without noticing abound. For example, suppose you see a friend along the street, and your friend has had her hair cut. You might not *notice* this for some time. Nevertheless, you were *aware* of her hair, and the properties in virtue of which it looks different to you (which explains the thought you are likely to have when you *do* notice her haircut: 'I had a sense there was something different about her today... '). As for the view that conscious states have no nonconceptual content, this too seems implausible (see, e.g., Dretske [1995]), but here I will just note that arguing for this view is not open to Clark, since he admits that conscious states may well have nonconceptual content. As such, Clark has little basis for claiming that these results provide evidence against EBC.

[9] Another possibility, less likely in my view, is that it is indeed subjects' conscious awareness of *the movement* that explains their poor performance. As such, *had subjects been aware of movement in the initial trials* their performance would have been less accurate. Contrary to first appearances, this is compatible with EBC and the other results discussed so far. For subjects were not aware of movement in the initial trials. What may (for all the evidence tells us) explain their performance is not awareness of *movement* but conscious awareness of a stimulus that has in fact moved; that is, their conscious awareness of the stimulus in its new location. So, again, conscious states could control the accurate fine eye/hand movements—just as EBC requires—even if it is true that conscious awareness of movement produces less accurate eye/hand movements.

Even if the alternative explanations for subjects' less accurate hand/eye movements could be got around, the argument against EBC(res) would remain strained. The reason is that there is no basis for inferring that conscious states do not play a role in fine-tuning *any* visually guided behavior from the fact (supposing it is a fact) that conscious states do not play a role in controlling minor adjustments to movements occurring *in peripheral vision* and *during a saccade*. There is no reason to infer, for instance, that conscious states do not play a role in controlling fine eye/hand movements that occur in response to objects and events within central vision, during fixation (as when one brings one's finger down upon a printed cross).

So far, the case against EBC—even in its restricted form—is weak. Let us now address the results concerning memory-driven responses, to see if they strengthen Clark's case. I contend that they do not. In the relevant cases, subjects fixate on a target surrounded by a frame. The frame and target disappear and then reappear after a brief delay, moved slightly. While the target moves slightly, the frame undergoes greater displacement. This induces an illusory movement of the target—subjects report that the target moves more than it in fact does. Nevertheless, their eyes accurately track the target. When asked to move their eyes back to the target's original position, subjects move their eyes consistently with the illusory movement. That is, they saccade to the position that the stimulus would have initially been, had the illusory movement been actual. Clark believes that, since subjects' memory-driven responses are controlled by the illusory movement content, which is no doubt conscious, their accurate eye movements, which track the actual movement of the target, must be controlled by nonconscious visual states. But the results do not support this claim.

First, there is no question that conscious visual states control the subjects' memory-driven saccades to what they believe to be the stimulus' original position. The illusory movement content is part of conscious vision. But to infer that subjects are using nonconscious visual states to control their accurate eye movements—the movements that track the target's actual motion—Clark must assume that the illusory movement contents exhaust the contents of conscious vision—at least with respect to movement. That is, Clark must assume that the veridical motion contents are not part of conscious vision. For, if veridical motion content is part of conscious vision, there is no basis for inferring that conscious states are not utilized in the control of eye movements.

Now, although it is quite natural to assume that the illusory motion content exhausts the relevant contents of conscious vision, it is in fact far from clear that this is so. For one thing, there are many cases where illusory and veridical contents are both parts of conscious experience. Consider the familiar Müller-Lyer illusion. Subjects have a strong impression that the lines are of unequal length. Yet, the illusion can be presented so that—despite the persistence of the illusion—it is quite clear that conscious vision must also present the lines as of equal length. Simply draw the illusion on graph paper, and observe that the vertical lines touching the ends of the Müller-Lyer lines appear perfectly straight and parallel. This would be impossible if conscious vision 'really' presented the Müller-Lyer lines as being of unequal length. In this case, then, conscious vision presents veridical and illusory 'length contents'.[10] Similar examples abound.[11] I suggest that something similar happens in the eye movement experiments. In particular, one's perceptual constancy mechanisms give rise to an illusion of movement, and cause subjects to *think* that the stimulus has moved. As such, when asked to look to the target's original position, they do so in accordance with what they believe. Nevertheless, the target's true movement is still represented in conscious vision. The subjects simply do not notice that their visual states have this content—just as someone subject to the Müller-Lyer illusion does not notice that her visual states accurately present the two lines as being of equal length. But since their states do contain the veridical content, there is no reason to suppose that conscious visual states do not provide the information about the target's location that eye-control centers use to generate a saccade to the target. The view that they do provide such information—and hence play a role in the control of eye movements (see my remarks on control, above)—is not threatened by fact that memory-based saccades draw upon different contents (the illusory contents). Since this interpretation of the present experiments is not only possible, but in my view plausible,[12] and Clark says nothing to cast doubt upon it, I conclude that these experiments do not refute the view that

[10] Clark, incidentally, grants the possibility that this is a correct description of the Müller-Lyer illusion ([2001], p. 506). Indeed, it is because Clark admits this that he grants that some of the evidence he presents against EBC is inconclusive.

[11] To mention just a few: first, so-called impossible figures, such as Escher drawings. The 'objective' content is impossible—no object could have the presented properties. But the 'line content' is perfectly coherent, there are just lines of certain lengths on paper. Second, vision arguably presents perspectival and objective shapes simultaneously. The objective shape of some color patch could be square, while its perspectival shape is a (nonsquare) parallelogram. Finally, the color one takes vision to present will differ depending on whether one considers an object's 'objective' or 'apparent' color to be presented. For instance, suppose one moves a red ball from sunlight to shade. Did it change color? Subjects will say 'no' if they base their response on considerations of objective color, 'yes' (to a darker shade of red) if they are attending to apparent color.

[12] Indeed, there is a way to perform the experiment to show that veridical motion contents are also present in the subjects' visual experience. The method is the same as described in connection with the Müller-Lyer illusion. Simply present the target and field against a grid-like background. Then, even though the illusory movement would persist, one could show the subject that, in fact, the frame moved (by showing how it shifted relative to the grid) and that, in fact, the target moved less than the subject thought (again by indicating its new position against the grid). Since locations against the grid are also represented in the subjects' visual experiences, it would follow that both the veridical and illusory contents characterize the visual experiences. I predict that this is precisely what one will find if these experiments are performed. This would conclusively establish that Clark cannot appeal to these experiments to argue that eye movements are wholly controlled by nonconscious visual states.

conscious visual states are utilized in the control and guidance of eye and hand movements.[13]

To this point, the experimental evidence Clark draws upon does not in fact threaten EBC—in either its general or its restricted form. Let us turn, finally, to Clark's third and most interesting case. This case involves a well-known patient named D.F., who suffers from a condition known as visual form agnosia ('without knowledge of visual form'). Like other patients with this condition, D.F. is not blind; she perceives small regions of color and fine texture normally, and she has a relatively intact visual field (Milner and Goodale [1995], pp. 125–6; this text provides a comprehensive study of D.F.). However, she has severe difficulties with the perception of shape and orientation. She performs poorly at tests designed to detect sensitivity to form. For instance, when shown an object, and asked to identify its shape, she performs at chance. When asked to indicate the orientation of a slot, by appropriately positioning her hand or a card, she performs very poorly. Yet—remarkably—she is able to reach out, grab, and otherwise manipulate objects. She will, for instance, adjust the size of her grip according to the size and shape of the object she is reaching for—even though she is apparently unaware of the very properties to which she adjusts her grip! (Milner and Goodale [1995], pp. 129–32) Furthermore, when asked to 'post a card' through a slot presented at a random orientation in front of her, she performs very well. Indeed, her performance is nearly indistinguishable from that of normal controls (Milner and Goodale [1995], pp. 128–9).

Clark suggests, following Milner and Goodale, that D.F.'s motor skills rely solely upon nonconscious information about shape, size, and orientation (Clark [2001], pp. 500–1). After all, she must possess that information—she must be developing representations of shape, size, and orientation—otherwise she would not be able to perform the behavioral tasks she does. However, her attempts to identify shapes, either verbally or by positioning her hand, suggest that she lacks conscious awareness of shape, size and orientation. If so, then it seems we must conclude that conscious mental states, at the very least, play no essential role in at least some forms of motor control—including reaching to a location, wrist orientation and grip size. These parameters of motor skill can, if Clark's interpretation is correct, be programmed by nonconscious representations. If this is right, we have what looks to be a powerful case

[13] There is a further reason to question the relevance of this whole body of data concerning eye movements, for eye movements to stimuli that suddenly appear—especially in peripheral vision—arguably, are involuntary, reflex-like movements. If so, then they do not fall within the scope of EBC, properly understood, and therefore, cannot provide evidence against it. I hesitate to press this objection, however, since some eye movements are voluntary, or intentional, and hence would be a proper source of data relevant to the truth of EBC. It is, moreover, very difficult to say whether or not the eye movements are voluntary in the present cases.

against EBC—and, importantly, this case would provide evidence against EBC(gen), since the movements that nonconscious states supposedly control are not mere minor adjustments to behavior.

Before we accept Clark's interpretation, however, let us consider the results of experiments performed on D.F. more closely. What the results demonstrate is that D.F. is severely impaired at tasks that require her to indicate—to represent to someone else, be it verbally or by positioning her hand—aspects of an object's form (size, shape, orientation, etc.). Such tasks require D.F. to form a perceptual judgment about aspects of form (indeed, Milner and Goodale often describe D.F.'s performance in terms of her failure to form perceptual judgments, see [1995], pp. 128, 31). Forming a perceptual judgment, of course, requires one to perceptually identify certain aspects of perceptual stimuli, to bring those features under concepts. And this, I grant, is what D.F. cannot do. D.F.'s problem is a conceptual one: she cannot identify shapes, sizes, and orientations, she cannot 'bring them under concepts'. This is why she cannot perform the representational task of indicating to someone else, verbally or by positioning the hand, aspects of an object's form. However—and here is the crucial point—it does not follow that she is not aware, in a nonconceptual way, of the shapes, sizes and orientations of things. Aspects of form may well be phenomenally present to D.F., she may well consciously sense, and hence represent, the shapes, sizes and orientations of things, even if she cannot properly conceptualize these aspects of form. Similarly, if one is distracted, one may be aware of a sound outside without recognizing it, without conceptualizing it, as a particular type of sound (e.g., a car idling). Therefore, the data derived from the study of D.F., while very interesting, do not show that she lacks conscious awareness *of* form. At best, it shows that she lacks awareness *that* things have particular forms.[14] So, *pace* Clark, Milner, Goodale and others, the data do not establish that D.F.'s motor skills rely wholly on nonconscious information about aspects of form. This case, then, does not refute EBC.

Of course, if we have no reason to believe that D.F. is aware, in any sense, of aspects of form, or if we think it is implausible to suppose that she is, then we may still find a serious challenge to EBC here. In fact, however, the more plausible view is that D.F. is (nonconceptually) aware of aspects of form. For one thing, D.F.'s ability to behave appropriately with respect to shape and orientation is very good, much better than is a blindsight patient's—someone who genuinely is unaware of aspects of form. D.F. can, for instance, catch objects tossed at her (Milner and Goodale [1995], p. 128). Another argument stems from reflection upon what D.F.'s visual phenomenology could be like. D.F. has conscious visual experiences—no one questions this. As I noted

[14] On the distinction between awareness-of and awareness-that, see (Dretske [1995]).

above, D.F. experiences color and texture, and she has a fairly extensive visual field (roughly 60 degrees of visual angle across, see Milner and Goodale [1995], p. 125). What I fail to see here is a coherent description of D.F.'s visual phenomenology that is both consistent with these facts and yet denies that shape is present in her phenomenal field. For, if color and texture are present, then it seems that features such as shapes and orientations must be present as well—minimally, as the boundaries of colored, textured regions. Perhaps aspects of form could be absent, even if color and texture are not, if D.F. perceived only very small regions of space at a time. But this is contradicted by perimetry tests establishing the extent of her visual field. Perhaps D.F.'s visual field is constantly shifting, with forms 'melting away', or changing as though she were peering into a kaleidoscope. However, if this were so, on the one hand she *would* be representing shapes, only just not doing so accurately, and on the other hand it is not clear why she would not report this. If one says she does not report it because she has trouble reporting on the relevant aspects of her experience, then we are back to the point that this is perfectly consistent with the fact that she is consciously sensing (hence representing) shapes. The difficulties we encounter in trying to describe what it could be like, visually, to be someone who experiences colors and textures, but not aspects of form (shape, orientation), cast serious doubt on the claim that D.F. does not consciously represent aspects of form, albeit in a nonconceptual way. At the very least, the fact that we cannot provide an even remotely plausible account of what D.F.'s visual phenomenology could be like ought to make us hesitant to accept the hypothesis that D.F. is unaware of shape, form and so on (even nonconceptually). We ought to be all the more hesitant when *there is* a coherent alternative explanation consistent with the data, namely, that she is nonconceptually conscious of aspects of form but cannot visually conceptualize them, hence cannot indicate them to herself or others. On balance, the more plausible hypothesis is that D.F.'s problem is one of conceptualization, not one of consciousness per se. We should take the etymology of 'visual form agnosia' seriously: it is a failure of *knowledge* about shape, not a failure to sense shape. Visual form agnosics need not—and, I have argued, do not—lack conscious sensation of shape.[15]

[15] It is at least worth noting that D.F.'s primary visual cortex (area V1) is thought to be intact. V1 does contain cells that are responsive to form, and is more likely to be associated with low-level (i.e., nonconceptual) sensing, and less likely to be involved in higher level, recognitional and conceptual responses, than 'higher' visual areas, which Milner and Goodale believe to be deprived of data from V1 in D.F. ([1995], p. 134). This provides some additional evidence in favor of my interpretation of D.F.'s deficit. I place less weight on these considerations though, since first, little is in fact known about the precise nature of the damage to D.F.'s brain (Milner and Goodale's hypotheses about D.F.'s lesion are in fact driven by behavioral data and general knowledge of visual processing in normal primate brains) and, second, our knowledge of the correlations between aspects of conscious vision and precise brain regions remains highly speculative.

Since D.F. (probably) does consciously represent such aspects of form as size, shape, and orientation, we cannot rule out the possibility that these representations enter into the control and guidance of her motor behaviors. As such, Clark's appeal to D.F. does not provide compelling evidence against EBC—restricted or general.

We have surveyed several of Clark's attempts to provide an empirical case against EBC. I have argued that none of them is compelling. At best, Clark has only cast doubt upon EBC(res) (based on the evidence about eye/hand adjustments). Even this case, I argued, was far from conclusive. Still, even if it were a good case, refuting EBC(res) is not enough for Clark to achieve his polemical aims. For, on the one hand, Clark claims to cast doubt upon a 'commonsensical' or philosophically intuitive view about the functions of conscious visual states and, on the other, he wants to replace this view with EBS. Unfortunately, the version of EBC that most accurately captures our intuitions about the functions of conscious visual states is not EBC(res), but EBC(gen). And EBC(gen) is compatible with the falsity of EBC(res). Moreover—and this is a related point—EBS is not supported by the falsity of EBC(res). For even if conscious states do not enter into the control and guidance of our 'fine-tuned activities', plenty of room remains for them to enter into the control and guidance of the grosser aspects of our behaviors. Conscious states may still provide, for instance, the information that the motor system uses[16] (nonconsciously) to direct the hand in a particular direction for a certain distance, or to cause the hand to take on some general configuration. Conscious states, that is, may well be utilized in the control of behavior.

Clark discusses a few more cases, but they introduce no further compelling evidence.[17] We might wonder, however, whether there is any better evidence available against EBC. One might suppose that blindsight subjects provide such evidence. For we have good reason to believe that blindsighters genuinely lack conscious visual experience in half of their visual field.[18] Nevertheless, blindsighters have been found to retain a surprising number of capacities

16 It is plausible to suppose that the activities of the motor system are nonconscious, of course. Recall my earlier point that EBC is compatible with the fact that many nonconscious events also enter into the control and guidance of behaviors.

17 Indeed, Clark would admit as much. In what is perhaps his most interesting further case, a discussion of the differential effects of perceptual illusions (such as the Müller-Lyer illusion) on perceptual judgment and motor control, he grants that the content required to guide motor behaviors may well be part of conscious experience (Clark [2001], pp. 503–7).

18 Here, the neurophysiological data are relevant. Blindsight patients have severe damage to a full half of their primary visual cortex. Though no one yet knows which areas are sufficient for having conscious visual experience, nearly everyone in the study of perception agrees that the primary visual cortex is necessary. So, the nature of blindsight patients' brain damage leaves little doubt that they truly lack conscious visual experience—especially in conjunction with their behavior in normal circumstances and the fact that many alternative explanations of their performance in experimental circumstances have been ruled out (Weiskrantz [1997]).

with respect to stimuli that occur in their blind field. Such capacities include accurately 'guessing' the color or shape of a stimulus (when they are given a choice as to what the color or shape might be and are forced to guess; see Weiskrantz [1997] for good discussion). Evidently, nonconscious visual information enables these capacities. In the present context, the following results might appear to be of special significance. Milner and Dijkerman ([2001]) report the results of experiments performed by Perenin and Rossetti upon two blindsight patients. These patients were forced to put a card into a slot—like a mail slot—presented at various orientations in their blind field.[19] Despite their lack of visual awareness, they were 'moderately accurate', indeed 'statistically well above chance'[20] (Milner and Dijkerman [2001], p. 248), at successfully putting the card through the slot. Similar results were obtained when patients were instructed to reach out and grasp objects placed in their blind field. Not only did patients reach to the correct location, they would appropriately adjust their anticipatory grip size 'with some proficiency' (Milner and Dijkerman [2001], pp. 248–50). A further interesting result is that blindsighters are able to 'mimic' the motion of a stimulus traveling in their blind field with a hand gesture (Weiskrantz [1997], pp. 61–2).

These patients provide solid evidence that nonconscious mental states can, in principle, play a role in the control and guidance of behavior. But this does not immediately cast doubt on EBC. The reason is that behavioral capacities preserved in blindsight subjects are often explained by reference to the operation of alternative processing pathways. The visual system is very complex and highly flexible, containing a large amount of redundancy. When one part of the system suffers damage—as does the primary visual cortex in blindsight subjects—backup systems (which may be phylogenetically older) are there to take over. This is precisely what many neuropsychologists believe occurs in blindsight (Weiskrantz [1997], see especially Chapter 6). The upshot is that one cannot infer that, in normal cases, these pathways are operative. Hence, one cannot infer that nonconscious representations are operative in normal cases.[21] Therefore, the capacities of blindsight subjects do not pose a serious threat to EBC.

[19] This is the same task D.F. was asked to perform. Blindsighters' performance (as I will note momentarily) is impressive, but falls well short of D.F.'s more or less normal-level performance. This fact is not unimportant when considering what we should infer about normal behavioral control from studies of blindsight subjects.

[20] I note the performance adjectives here—'moderately accurate', and below, 'with some proficiency'—to emphasize that blindsighters' performance, though impressive, is not at the level of normal subjects or even D.F.

[21] This explains, perhaps, why Clark does not discuss blindsight patients in his paper.

4 Conclusion

We have yet to find a compelling case against EBC(gen), and we found the case against EBC(res) wanting, too. For all that the empirical evidence currently available tells us, conscious vision may well provide the information our motor systems use in developing the motor instructions that give rise to the gross features of most of our goal-directed behaviors. If so, then those states play a crucial role in the control and guidance of motor behavior, just as EBC, broadly construed, suggests. This is not to deny that conscious visual states have other functional roles. In humans, for instance, conscious visual states no doubt have the function of giving rise to a variety of thoughts and beliefs about the world and about our experiences themselves (so-called higher-order thoughts). They, no doubt, play a role in the 'reason-and-memory-based selection of actions' as well (Clark [2001], p. 512). In defending EBC, however, one of my aims has been to argue that it is a mistake to think that the functions of conscious visual states are confined to these high-level cognitive tasks. Conscious states, in my view, also function in lower-level behavioral tasks such as reaching for food, chasing down prey, or dashing for cover—tasks performed by many creatures. I take it that it is a mark in favor of EBC that it is consistent with the common intuition that many animals, including most mammals, birds and perhaps some reptiles, enjoy conscious visual experience.[22]

Clark, Milner and Goodale, and others argue that the bonds between visual experience and motor action are loose, that visual experience provides, at best, merely indirect guidance of motor action. These theorists thereby advocate mild epiphenomenalism about conscious visual experience—epiphenomenalism with respect to crucial aspects of much of our everyday, 'real world' behavior. If my arguments are correct, cognitive science does not support even mild epiphenomenalism. We can accept everything cognitive science tells us and retain EBC too. In so doing, we not only keep hold of intuitive assumptions about conscious experience, we secure a more plausible conception of when consciousness might have evolved, and which of our fellow creatures have it. The bonds between visual experience and action, while not unbreakable, are tighter than Clark and others suggest.

[22] Some readers of drafts of this paper, including one anonymous referee, have worried that my defense of EBC might go too far, precluding me from denying, in a principled way, conscious experience to very simple organisms such as insects (since they too can 'act fluently'). This is a reasonable worry, but I think that there are some principled things to say here. I excluded reflexes from the scope of EBC earlier in the paper, and I would exclude unlearned stereotyped behaviors—or unlearned 'fixed action patterns'—as well. (The principle? The explanation of such behaviors does not invoke representations at all, much less conscious ones.) To the extent that the behaviors of insects can be explained in terms of such behaviors, I can deny them consciousness. But for an interesting argument that we should attribute some conscious states to honeybees, see (Tye [2000], Chapter 8).

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