Consciousness and mental causation: Contemporary empirical arguments for epiphenomenalism

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Forthcoming in Oxford Handbook of the Philosophy of Consciousness

1. Introduction: Varieties of epiphenomenalism

In its classical form, epiphenomenalism is the view that conscious mental events have no physical effects: While physical events cause mental events, the opposite is never true (Huxley 1874). Stepping on a thorn causes an experience of pain, but the pain is not what causes the person's shriek; a picture of a loved one causes a feeling of longing, but the longing is not what causes the person's sigh. It might be fair to say that epiphenomenalism is a view adopted under duress, its adherents having been philosophers who wanted to hold that phenomenal properties (i.e., the "what-it's-like" that accompanies conscious mental states; see Nagel 1974) are non-physical[1] while not abandoning the naturalistic view that physical events never have non-physical causes (Jackson 1982; Chalmers 1996).[2] But to many philosophers it is plainly absurd that a thought to raise one's hand is not what caused one's hand to rise, or that a feeling of sadness is not what caused one's tears, and so classical epiphenomenalism has never enjoyed much popularity.

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[1] The motivation for adopting this kind of property dualism was the failure of phenomenal facts to logically supervene on physical facts; see Chalmers 1995 for a brief treatment of this idea, Chalmers 1996 for one in-depth and technical.

[2] Even this advocacy has been short-lived or tentative: Jackson later recanted epiphenomenalism in favor of materialism (see, e.g., his 2003), and Chalmers just considers the view a live possibility, given the truth of property dualism.

The same is not true of contemporary forms of epiphenomenalism, which are based not on *a priori* reasoning, but rather results in neuroscience and psychology, and so have not been so casually dismissed. Unlike classical epiphenomenalism, these contemporary forms do not hold conscious mental states always lack causal efficacy, only that they are epiphenomenal relative to certain kinds of action, ones we pre-theoretically would have thought consciousness to make a causal contribution to. It is two of these contemporary, empirically based challenges to the efficacy of the mental that are the focus of this chapter.

The first originates in research conducted by the psychologist Benjamin Libet and his colleagues. This work has been interpreted by many as showing that the neural events initiating voluntary actions precede our conscious willing of them, meaning the conscious will cannot be what causes them. The second challenge originates in research carried out by vision scientists David Milner and Mel Goodale. These studies, which consist of instances in which the content of visual consciousness and motor action dissociate, have been thought to cast doubt on the intuitive view that visual consciousness guides visually based motor action. Below, we look at each challenge in turn.

2: Epiphenomenalism about conscious will

It is seemingly hard to question the causal efficacy of conscious will, given the numerous and easily available examples in which the conscious will appears to produce action: I decide to drink my water, and my hand reaches for it; I intend to order a burger instead of salad, and hear myself say "the burger" to the waiter. However, the last few decades have seen this apparently safe assumption come under increasing empirical pressure, with several lines of evidence suggesting that feelings of conscious willing are but by-products of unconsciously initiated decisions to act (see, e.g., Wegner 2003a, b; Haynes 2011). Most prominent among these lines of evidence would be experiments conducted by Libet and colleagues, ones in which a subject's awareness of an incipient action seems to come only after her brain has initiated it. This section examines these experiments, along with challenges that have been made to the methodology and significance of them.

2.1 The Libet experiments and free will

For two millennia, the debate over free will was the exclusive province of philosophers, the issue remaining largely insulated from empirical data. This changed in a dramatic way in the early eighties when Libet and colleagues (1983) conducted an ingenious series of experiments. The idea was to compare the time at which the brain initiates a voluntary action with the time at which subjects felt that they had consciously willed it. Libet and his collaborators did not explicitly discuss the philosophical implications of their research (but see Haggard & Libet 2001), but the implications were clear enough: If a subject's brain initiated the voluntary action before the subject knew it, then it would look like her conscious intention was not the source of her action; and this in turn might perhaps call into question the very idea of free will.

In the experiments, subjects were asked to perform a simple voluntary task, this being to spontaneously ("freely and capriciously") flex their wrist at a time of their choosing.[3] The timing of three events were measured: First, the time at which the wrist flex occurred was measured using an electromagnetogram (EMG), a device that detects the electrical charge created when a muscle contracts. Second, the time at which the subject consciously willed the wrist flex was determined by having the subject watch a computer image in which a dot orbited a central disk, having the subject note the dot's precise position when she decided to flex. The final event whose time needed to be measured was that of the brain initiating the wrist flex. To determine this, the experimenters employed an electroencephalogram (EEG), a device that consists of numerous electrodes taped to a subject's head that are used to measure the electrical activity created when populations of neurons fire. Prior experiments had discovered that voluntary motor actions like those that Libet's subjects were asked to carry out are reliably preceded by something known as a *readiness potential* (Kornhuber & Deecke 1965), a distinctive negative shift in electrical activity in frontal parts of the brain. Libet used this readiness potential as an index of when the brain initiated the wrist flex.

The results were astonishing. While the readiness potential appeared an average of 535ms before wrist flex, subjects reported deciding to flex an average of just 192ms beforehand: The brain initiated the motor action before subjects thought that they had consciously willed it. If the results were as they seemed—i.e., if the readiness potential was the initiation of the wrist flex,

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[3] More exactly, the subject was asked to "perform [a] quick, abrupt flexion of the fingers and/or the wrist of his right hand" (p.625).

and if subjects were correct about the time of conscious willing—then the conscious willing could not have caused the wrist flex. And if there was reason to think that the results of the Libet experiments generalized to all or most instances of intentional action, then it looked like there was also reason to question the existence of free will.

It is important to note that the conclusions to be drawn from the Libet experiments are potentially wide-ranging, posing a threat to even less demanding interpretations of free will. Few modern philosophers subscribe to *libertarian* theories of free will, according to which people enjoy a freedom of choice unconstrained by the (more or less)[4] deterministic laws prevailing elsewhere in the universe (e.g., Campbell 1957; Kane 2009). Libertarianism is clearly hard to reconcile with the Libet results, given that they imply that these allegedly unconstrained actions are not authored by the agent. But the Libet results even threaten the less stringent *compatibilist* theories of free will, those that do not require an agent's actions to be undetermined. Such theories vary in what they take to be required for an agent's action to be free, with some saying that the agent must be responsive to reasons for acting or not acting (e.g., Dennett 1984; McKenna 2013), others saying that it requires that one's second-order desires mesh with her first-order desires in the proper way (Frankfurt 1988). But a plausible common denominator of the various compatibilist theories would be that an action is free only if it is caused—in some fashion or another—by one's conscious will (cf. Sinnott-Armstrong & Nadel 2010). And if it is true that the Libet experiments show the initiation of the wrist flex to temporally precede the conscious willing, the conscious will cannot be causing the flexion. So the Libet results appear to threaten free will even if we are considering less stringent notions of it.

However, Libet's lab did employ another paradigm the results of which Libet took to save a measured role for conscious will (Libet 1985). Subjects were asked to initiate a wrist flex at a pre-arranged time, but then decide to not follow through with it, instead "vetoing" the wrist flex just before (100-200ms) it would have occurred. That subjects were capable of doing this was taken by Libet to mean that, while we cannot consciously choose to initiate an action, we can consciously choose to abstain from performing an act that was unconsciously initiated. While we lack free will, we do have "free won't" (as it is sometimes jocularly put). However, one would

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[4] Of course, according to contemporary quantum theories of physics, microphysical events do not happen deterministically, but only with a certain probability. But few philosophers or scientists think that these microphysical events "percolate" up to the macrophysical level in such a way as to support libertarian theories of free will.

expect that this vetoing action itself has a readiness potential, one that precedes the conscious willing of the veto (Dennett & Kinsbourne 1992). In fact, this veto-readiness potential appears to have been recently discovered (Filevich, Kühn & Haggard 2013).

Of course, methodological challenges have been raised to the Libet experiments; these are surveyed shortly. But some commentators argue that even if the Libet results are valid, they have little bearing on the free will debate. It has been argued, for instance, that the actions subjects were asked to perform ("Libet-actions") are not paradigmatic acts of free will, since flexing one's wrist at some arbitrary time has little in common with the kind of reason-based and morally significant choices usually associated with free will (Pockett & Purdy 2010; Roskies 2010; see also Breitmeyer 1985; Bridgeman 1985). Put another way, Libet-actions are more like choosing which pant leg to put on first, and less like deciding whether to steal money from a register that one is running. There is the further worry that a Libet-action is not properly considered an instance of conscious willing, it instead being the automatic component of an action that was willed earlier, that of forming the intention to follow the experimenter's instructions to flex her wrist some time in the next few seeconds (Breitmeyer 1985; Flanagan 1996, Chap. 4; van de Grind 2002; Bayne 2011). Going forward, however, we will put aside questions concerning what the Libet results might mean for the free will debate, instead focusing only on what they show (or do not show) about the efficacy of conscious will.

The significance of the Libet experiments comes from their appearing to show the initiation of the wrist flex to occur before subjects think that they consciously willed it. Accordingly, there are two general strategies for undermining the validity of the Libet results, the first being to contest the timing of the initiation of the wrist flex, the second being to contest the timing of the conscious willing. Each of the next two subsections are dedicated to looking at one of these two strategies.

2.2 Questioning the time of action initiation

In the Libet paradigm, it is assumed that the moment that the brain initiates the wrist flex can be determined by looking for the readiness potential (hereafter RP), the distinctive type of electrical activity that is thought to precede the wrist flex. Before looking at doubts that have been raised about this assumption, some background is needed on how RP is determined.

Because of the messy nature of neural activity and poor spatial resolution of EEG, it might be difficult to detect the neural signature of some cognitive event on any individual trial. A distinctive pattern of neural activity such as RP is usually found only after analyzing and averaging data from numerous trials, a technique known as "back-averaging." In back-averaging, some observable event (like a report or wrist flex) is used as a reference point, and electrical activity preceding the event is analyzed so as to identify the neural signature of the cognitive event that the experimenters want to track. A consequence of using back-averaging to find RP is that trials in which high neural noise would mask an individual RP are treated the same as trials where there is low neural noise but still no detectable RP. This means that it is possible that many individual wrist flexes are not preceded by an RP.

To investigate this possibility, Pockett and Purdy (2010) adopted the unconventional technique of "eye-scoring" individual trials, searching for low-noise trials that seemed to also lack an RP. 12% of the 390 trials analyzed turned out like this. Upon averaging these trials, there was still no discernable RP preceding the wrist flexes. These are interesting data, but also contentious, since scoring individual trials is frowned upon because of the highly variable nature of neural activity. But if Pockett and Purdy's method were valid, it would mean that a significant portion of wrist flexes are not preceded by an RP, suggesting that RP does not mark the moment of action initiation.

Back-averaging creates another potential problem (Mele 2009; Roskies 2010). Since back-averaging requires some reference point from which to analyze the electrical activity, data are gathered only from those trials in which the wrist flex is carried out. This means that the brain could frequently generate RPs even when no voluntary motor action is performed, leaving open the possibility that RPs are the neural signature of some cognitive process other than the initiation of flexion.

It is because of this that Mele (2010) wonders whether RPs are present on trials where subjects are asked to form the intention to flex but then veto it at the last moment (i.e., trials supposedly exemplifying "free won't"; see 2.1), since such trials will be among those not recorded for purposes of back-averaging. An explanation as to what RP alternatively might be the neural signature of is offered by Pockett and Purdy (2010), who have pointed out how slow-going negative waveforms similar to RP precede the perception of a stimulus. Pocket and Purdy have argued on the basis of this that RP might be the neural signature not of action-initiation, but rather of the *anticipation* of some event, be this a decision to move, the appearance of the stimulus, or something else. But if RP is not the neural signature of the decision to flex, what neural event is? The beginning of an answer might be found in a study conducted by (Haggard & Eimer 1999). Here, experimenters asked subjects to choose to flex either their left or right wrist. Analysis of the EEG data revealed a *lateralized readiness potential* (LRP), one appearing in the hemisphere contralateral to the hand, and which was preceded by W in about 20% of trials. Such results are suggestive, but at this time it remains unclear what neural event the LRP represents, and whether similar neural activity can be found in the standard Libet paradigm (but see Haggard 2011).

In sum, there is an at least tentative case to be made against RP being necessary and/or sufficient for the wrist flexes that subjects are asked to perform in a Libet paradigm, in which case we cannot be sure that RP reliably signifies the initiation of the wrist flex. This leaves open the possibility that the initiation of the wrist flex does not actually precede the subject's conscious willing of it, potentially undermining the main result of the Libet experiment.

2.3 Questioning the measured time of conscious willing

The time at which subjects report having consciously willed the wrist flex is known as "W." One might wonder whether it is worth questioning the validity of W, given that if there is anything one knows, it would be the events in one's own mind, particularly conscious willings. There is, however, a half-century tradition of experiments in cognitive science showing not as much is available to introspection as one might expect (for reviews, see Nisbett & Wilson 1977; Carruthers 2010), research to be revisited below. For now, it is enough to note that the results of these experiments provide ample reason to question the validity of W.

One worry about W comes from the well-documented way in which subjects' judgments about a stimulus can be affected by the conditions under which it is perceived (van de Grind 2002). While it is not immediately clear whether we should think that introspection is subject to distortions the same way that the perception of external events is, some commentators think the processes analogous enough to merit concern (Haggard 2006). One potential problem comes from the "prior entry effect" (Sternberg & Knoll 1973). This can occur when a subject is asked to compare the relative timing of events in two modalities (e.g., vision and audition), and causes the attended event to appear to occur earlier than it actually did. Given that attention was not controlled for in the Libet experiments, this appears worrying. However, Libet discounts the prior entry effect on grounds that it has been shown to move the perceived temporal location of a stimulus no more than 70ms, far short of closing the roughly 300ms gap between W and RP (Haggard & Libet 2001).

Nonetheless, there are still grounds for wondering whether the Libet paradigm suffers *some* confound or another, given that W displays high variability both between and within subjects (Mele 2010). Haggard and Eimer (1999) calculated each of their subjects' premedian and postmedian average Ws, finding them to be anywhere between 984 and 4ms before flexion. Unless we have reason to think that the temporal relation between the conscious decision and RP is highly variable, this is worrying. However, Trevena and Miller (2002) found mean RP to be not only earlier than mean W, but also earlier than the earliest W that occurred on any individual trial; in which case the essential point of the Libet studies is probably retained. Hopefully future research reveals the reasons for high variability, and whether it is cause for concern.

The possibility must be entertained, however, that W is not a meaningful measure of anything at all. Consider this study by (Banks & Isham 2009). Instead of flexing their wrist, subjects were asked to press a button that responded with a "beep." In some trials the tone sounded immediately after the button was pressed, but in others it was delayed by varying amounts. It was found that the longer the interval there was between the button press and the tone, the later W would become.[5] On some trials, W even came after the button press! Banks and Isham take this to mean that subjects were using the tone as a reference for the time of the conscious willing. Going further, Banks and Isham hypothesize that when subjects in Libet paradigms judge the time of conscious willing, it is not an act of introspection at all, but rather a process of "retrospective inference," one in which the subjects construct the time of conscious willing from whatever cues are available (2009, 2010; see also Hallett 2007).[6]

That this would be the case would comport with the tradition of cognitive science research mentioned earlier, the one consisting of an extensive collection of experiments in which subjects appear to not know the workings of their own minds. Subjects, for instance, have been shown to confabulate reasons for why they prefer one of two identical pairs of pantyhose (Nisbett & Wilson 1977), or to be unaware that the presence of others was a factor in their

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[5] The delayed W-judgments were not quite time-locked to the auditory cue, in that 1ms of delay in the tone produced only an average of .77ms of delay in the perceived time of the decision.
[6] Since in the normal Libet paradigm, there is no tone available as a reference point, Banks and Isham hypothesize that it might be kinesthetic feedback that is determining W.

deciding not to seek aid for someone in distress (Latané & Darley 1970). Such experiments are often taken to show that one comes to know what mental states and/or processes she is in in roughly the same way that we come to know those of another person, which is to infer them by applying a folk psychological theory to the person's overt behavior and her surrounding circumstances. In fact, another experiment by Banks and Isham (2010) indicate that this might be precisely what is going on in Libet-style experiments: The experimenters had subjects observe another person perform the button-pressing version of the Libet paradigm, asking the subjects to report the time at which they thought that the person had decided to press the button. Subjects' judgments of the timing of the observed person's decisions closely tracked what W had been in the experiments where the subjects themselves were pressing the button; again, their temporal judgments moved with the tone as it was delayed.

Perhaps the parsimonious explanation of these data—the high variability in the timing of W, the susceptibility of W to the timing of related cues, the symmetry between judgments of W for oneself and for others—is that there is nothing for W to track (Banks & Isham 2010). That is, perhaps the idea that there are such things as conscious willings is just another mistaken tenant of folk psychology, making the Libet experiments a misbegotten attempt to measure something that does not exist.

In this section, we have examined the Libet experiments, which seem to show the time at which one consciously flexes her wrist to come after the brain has already initiated it. These experiments have sometimes been taken to show that free will does not exist, but as seen above, there are probably a number of methodological and philosophical barriers to overcome before this sweeping conclusion could be arrived at. Next we look at another current debate in cognitive science, one concerning the extent to which visual experience guides motor action.

3: Epiphenomenalism about visually guided motor action

Within visual experience, one seems to find very precise information about the location of objects. A glass of water sitting before you, for instance, appears represented in your experience as not merely in some general location or another (e.g., on the table), but rather *right there*, some exact distance and direction from you. Similar observations could be made about the precision with which an object's shape and orientation are represented within visual experience. Notice now that this metrically precise information found within visual experience seems well-suited for use in fine-grained, online motor operations, such as efficiently picking up the water without spilling any. Similar reflections could be made about other precision movements, like catching a Frisbee or hiking a rock-strewn trail.

These observations have been thought by many philosophers to lead rather naturally to the idea that visual experience must play some important, perhaps ineliminable role in visually guided motor action (O'Shaughnessy 1992; Peacocke 1992; Grush 1998; Briscoe & Schwenkler 2015). Such a view is plausibly also part of the commonsense conception of how visual experience and motor action are related (Clark 2001; Wallhagen 2007; Mole 2009; Kozuch 2015). Intuitive as it seems, however, this view has lately come under increasing empirical pressure, in favor of the idea that it is only rarely (if ever) that visual experience guides motor action (Milner & Goodale 1995/2006; Clark 2001, 2007, 2009; Kozuch 2015; cf. Wu 2013). This section surveys the more important arguments and data that have been brought to bear in the currently active debate concerning how visual experience and motor action are related.

3.1 The Dissociation Argument

The primary argument for visual experience not being involved in motor action is based on a collection of experiments in which visual experience appears to come apart from motor action. Before looking at this argument, some neuroscientific background is necessary. In the human brain, more advanced kinds of cognition such as perception, memory, or planning take place in the *neocortex*, the brain's outermost layer. Visual information first enters the neocortex in an area known as the *primary visual cortex*, then proceeds via two distinct pathways, a superiorly located *dorsal stream*, and an inferiorly located *ventral stream* (Morel & Bullier 1990; Young 1992; but see Prinz 2012, Chap. 6). Building on this neuroanatomical division, vision scientists Milner and Goodale (1995/2006; cf. Jacob & Jeannerod 2003) have proposed a *dual visual systems theory*, according to which the two pathways are functionally distinct, with the dorsal stream preparing visual information for use in motor action, and the ventral stream providing the identity of objects for use in goal-oriented cognition. More controversially, the theory also includes the idea that visual consciousness is confined to the ventral stream.

The most well-developed argument for visual experience not guiding motor action is due to Clark (2001, 2007, 2009; see also Milner & Goodale 1995/2006), who builds a case for this by appealing to experiments in which the content driving motor action appears missing from visual experience. We can refer to this as the "Dissociation Argument." The Dissociation Argument mainly consists of two lines of evidence, the first consisting of studies of lesions to the ventral or dorsal stream, the second consisting of psychophysical experiments involving visual illusions. We examine each in turn.

Damage to the ventral stream can result in *visual form agnosia*, an inability to consciously perceive things like the shape, size, orientation, or position of an object (Heider 2000). The most extensively studied case of visual form agnosia is patient DF,[7] a Scottish woman who suffered ventral damage when a water heater leaked carbon monoxide as she showered. The case of DF is remarkable because her deficits of visual consciousness apparently do not impede her ability to perform visually guided motor actions: In the "posting task," the subject is asked to place an envelope-shaped object into a slot whose orientation varies from trial to trial. While DF is at chance when asked to report the orientation of the slot, she effortlessly fits the envelope into the slot each time (Goodale *et al.* 1991; Milner *et al.* 1991). DF's ventral damage also resulted in numerous other dissociations between visual experience and motor action, such as picking up objects that differ in shape without being able to tell them apart (Goodale *et al.* 1994), or stepping over obstacles the height of which she cannot report (Patla & Goodale 1996).[8] On the other hand, *dorsal* lesions sometimes result in converse deficits: Damage here can produce *optic ataxia*, a disorder of motor action unaccompanied by deficits of visual consciousness (Perenin & Vighetto 1983; Perenin & Vighetto 1988). Those suffering from optic ataxia are, for instance, unable to succeed at the posting task, but have no difficulty identifying things like the orientation, position, or shape of an object.[9]

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[7] To preserve anonymity, lesion patients are usually called by just their initials in psychological studies.

[8] For further examples of the dissociations that DF displays, see Milner and Goodale 1995/2006:128–33.

[9] I am presenting a somewhat simplified picture here, since optic ataxics found it difficult to identify some of these properties in (Pisella *et al.* 2006, 2009); however, these difficulties are probably the result, not of deficits in consciousness, but rather of attention (see Kozuch 2015).

The other line of evidence to which the Dissociation Argument appeals comes from experiments in which a consciously experienced visual illusion appears to leave motor action unaffected. The most widely discussed experiment in context of this debate is due to Aglioti, Desouza, and Goodale (1995), and involves the Ebbinghaus-Titchener illusion, an illusion in which two disks of identical size are made to appear different by the addition of a ring of small circles to the first disk, and a ring of large circles to the second (Haffenden & Goodale 1998). In an interactive version of this illusion created with plastic disks placed on a table, Aglioti and colleagues demonstrated that the visual illusion had significantly less of an effect on subjects' motor actions than it did on their conscious perception, since their grip width would conform much more closely to the interior disks' actual size than did their conscious perception of them. Other visual illusions, such as the Ponzo (Brenner & Smeets 1996; Ellis, Flanagan & Lederman 1999) or hollow face illusion (Króliczak *et al.* 2006) have been used to demonstrate similar dissociations. I note that some of these experiments are still considered controversial (see, e.g., Franz & Gegenfurtner 2008); this, however, is a technical issue that we lack space to go into in this short piece.

As we will see below, both lines of evidence are contested. Putting this aside, however, the brain lesion and visual illusion data just surveyed are naturally interpreted as showing visual experience to not have the kind of relationship with motor action that we might have pre-theoretically thought: As observed earlier, the information found in visual consciousness seems to contain the kind of richness and detail required for the fine-grained, online guidance of motor action. In the experiments just described above, however, the content driving motor action seems to be either missing from (DF) or mismatched with (the visual illusions) the content of visual experience, in which case visual experience cannot be what is driving motor action. From this one might infer, as Clark does, that "a great deal of our daily, fine-tuned motor activity proceeds quite independently of the current contents of conscious visual experience" (2001:499).

Shortly, we look at how some commentators attempt to resist this conclusion. First though, we explore the issue of upon what precise claim the Dissociation Argument should be taken to cast doubt.

3.2 The Hypothesis of Experience-Based Control

The Dissociation Argument seeks to overturn a certain conception how visual experience relates to motor action, but what precisely is this conception? In Clark's original salvo, he called into question what he referred to as the "Hypothesis of Experience-Based Control"; for short, "EBC." EBC, along with other, similar theses at play in the present debate, has been understood in various ways. The key issues at stake can be encapsulated in the following formulation (Kozuch 2015):

> EBC: The content of visual consciousness is what is typically used to directly guide visually based motor action

A first thing to notice about this formulation is that it contains both a *typicality clause*, and a *directness clause*; i.e., for EBC to be considered true in its entirety, visually guided motor actions would need to be both typically and directly guided by information within visual experience. My formulating EBC in this way is motivated by how commentators involved in the debate have described the view that they mean to attack or defend. However, EBC can be understood either as a philosophical thesis, or as an attempt to express the commonsense view of the relationship between visual experience and motor action. In the case of EBC as a philosophical hypothesis, formulating it as it appears above is of course not contentious, since the formulation merely adheres to the stipulated view that commentators in the debate intend to engage with. More controversial is whether the above formulation precisely captures the commonsense view of the relationship between visual experience and motor action, and whether the impression that visual experience guides motor action is something that can be derived from the phenomenology of performing motor actions (Shepherd 2016, 2015). However, these latter two issues are set aside in this short review so that we might focus on whether EBC as a philosophical thesis passes muster.[10]

Let us further precisify what is at issue. Something to note is that opponents of EBC do not hold that visual experience is *wholly* epiphenomenal toward motor action, it instead being the case that visual experience mostly only has high-level, broadly specified influences on motor

[10] But see Kozuch 2015 for some justification for what is probably the more contentious of the two, the directness clause.

action. More specifically, it is thought that the function of visual experience (and of the ventral stream) is to merely identify and select the objects that are to be acted upon (e.g., a mug of coffee) and perhaps provide general parameters for the action based upon context or background knowledge (to pick the mug up by its handle since it is hot); once these general goals and parameters are set, the task is handed off to non-conscious dorsal processes for implementation. Detractors of EBC usually also grant that there are times where information from visual experience might be called upon to more directly guide motor action. For example, it is commonly thought that, because visual information in the dorsal stream decays quickly (it has a "short memory"), certain delayed motor actions are guided by conscious information in the ventral stream (e.g., ones performed after lights have been shut off for a few seconds; Rossetti *et al.* 2005). However, according to the opponent of EBC, this kind of fine-grained guidance by visual experience is very much the exception rather than rule.

Above, we looked at how the original challenge to EBC was raised in the form of the Dissociation Argument. Now we move on to examine how commentators have subsequently attempted to argue for or against EBC. The discussion is organized according to the directness and typicality clauses of EBC, starting with directness.

3.3 Does visual experience directly *guide motor action?*

According to Milner and Goodale's dual visual systems theory, just the dorsal stream produces those representations guiding visuomotor action, and just the ventral stream produces conscious representations. Were this true, it would mean that, even if it could be shown that visual experience has a significant causal influence on motor action (an issue taken up later), any such contributions would first be routed through the dorsal stream, making them indirect; i.e., the *directness* clause of EBC would be false. The question arises then as to whether the relevant empirical data support this tenant of Milner and Goodale's theory. The most systematic argument for this is due to Kozuch (2015), who argues that a close examination of available evidence shows it to be neither the case that the ventral stream ever directly guides motor action, nor that the dorsal stream ever produces conscious representations.

To support the first idea, Kozuch appeals to a variety of neuroimaging and lesion data. A first line of evidence comes from a set of neuroimaging studies in which increased visuomotor task demand produced increased dorsal activity without a corresponding increase in ventral activity (Binkofski *et al.* 1999; Culham *et al.* 2003; Prado *et al.* 2005); these studies include one experiment in which DF's neural activity was measured while carrying out the posting task discussed above (James *et al.* 2003). The results of damage to ventral and dorsal areas are thought to lend further support to the idea that the ventral stream never directly guides motor action, since while it is the case that dorsal damage produces profound, pervasive visuomotor problems, those brought about by ventral lesions manifest only under unusual circumstances, such as when the motor target is viewed monocularly (Dijkerman, Milner & Carey 1996, 1999), or is removed shortly before the action is performed (Milner, Dijkerman & Carey 1999);[11] and even in those cases where these ventrally caused motor deficits manifest, the available neuroimaging evidence implies that it is not the ventral stream directly guiding the motor actions, since it is still dorsal areas showing increased activation (see, e.g., Himmelbach *et al.* 2009).

In arguing for the idea that the dorsal stream does not produce conscious representations, Kozuch mostly appeals to the results of dorsal lesions: While dorsally damaged patients will suffer from those deficits in visuomotor action that constitute optic ataxia, their conscious perception appears unaffected, since dorsal patients can successfully report upon things such as the shape, direction, orientation, position, and identity of objects (Perenin & Vighetto 1983, 1988). But if the dorsal stream produced conscious representations, one would think that dorsal damage would bring about deficits in at least some kinds of conscious visual perception. Additionally, Brogaard (2011b) argues that we should think that dorsal representations are not conscious since processes such as changing one's grip aperture happen too quickly to be cognitively accessed, and such access is necessary for consciousness (Brogaard 2011a).

However, Wu recently appealed to neuroimaging (Committeri *et al.* 2004) and lesion (Berryhill, Fendrich & Olson 2009) evidence in order to argue that dorsal areas V3A and V7 might produce conscious representations of object distance (2014; see also Prinz 2012, Chap. 6), although it is unclear whether these representations feed into visuomotor action. Additionally, brain area MT (mediotemporal) is often classified as being part of the dorsal stream, but is also

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[11] Himmelbach, Boehme, and Karnath (2012) argue that DF does not actual perform at the level of controls in even some tasks carried out under normal conditions, for example, actions performed toward objects in the periphery (see also Briscoe & Schwenkler 2015), but these deficits could be attributed to dorsal damage more recently discovered in DF (James *et al.* 2003; Bridge *et al.* 2013).

thought by some to produce conscious motion representations (Zeki 2003; Block 2007; Schenk & McIntosh 2010; Prinz 2012, Chap. 6); however, Milner and Goodale (1995/2006:219) argue that MT is instead more aptly considered an early processing area (i.e., not part of the dorsal stream), one akin to the primary visual cortex, and that the representations of MT are probably not conscious until they arrive in the ventral stream (ibid., Chap. 8). Additionally, Brogaard (2012) argues that dorsal representations must sometimes be conscious, since according to one gloss of dual visual systems theory it is only the dorsal stream that produces the kind of viewer-centered representations that we find in visual experience, but Foley, Whitwell and Goodale (2015) argue that it is only because of a misreading of the dual visual systems hypothesis that some commentators suppose that the the ventral stream does not contain these kinds of "egocentric" representation.

According to the directness clause of EBC, the representations found in visual experience are—at least sometimes—the same as those driving fine-grained, online motor actions. If this were the case, then the ventral stream should directly guide motor action, or the dorsal stream should produce conscious representations. As just seen, the first condition seems rather unlikely to obtain, but whether the second obtains is more controversial. Now we move on the issue of how frequently the information in visual experience is what—directly or indirectly—guides motor action.

3.4 Does visual experience typically *guide motor action?*

The Dissociation Argument proceeds by citing experiments in which the content driving motor action appears mismatched with or missing from the content of visual experience, the idea being that these present instances in which visual experience cannot be driving motor action. One strategy that commentators have used to respond to the Dissociation Argument is to argue that the experiments appealed to in it do not actually present instances of content mismatches, since there is reason to think that subjects' reports about what content appears in their visual experience cannot be taken at face value. If such a strategy succeeds, it would help support the idea that visual experience guides motor action more typically than is often supposed by the critic of EBC.

For an example of this approach, we again consider visual form agnosia.[12] The case of DF is troubling for EBC because DF can perform motor actions that require information about a target object's form, though she is apparently unable to consciously perceive form. Wallhagen (2007), however, argues that we should think that DF actually consciously experiences form, since DF is still able to experience colors, and it would be incoherent to suppose that her experience could contain colors without them being bounded in some fashion (e.g., in areas where two different colors adjoin), in which case she *de facto* has conscious experiences of form (but see Mole 2009). Wallhagen claims that we should instead think that DF merely has a deficit in bringing conscious form representations under concepts, as would be necessary for their report. This is the more natural explanation, argues Wallhagen, since the facility with which she motorically interacts with objects is hard to explain if she cannot experience object form.

Clark's main response to this is to argue that whatever form content DF allegedly enjoys cannot be considered conscious, as it lacks the kind of connection to personal agency required for this. Clark points out some distinctive limitations on how DF uses form content, such as her inability to sketch an object, or to determine the proper end to pick it up from (in the case of, e.g., a screwdriver). But one would think that content lacking these kinds of connection to an agent and her practical goals does not deserve to be called "conscious" (see also Evans 1982). Criticizing Clark's response, Kozuch (2015) points out how Clark's argument appears to appeal only to *conceptual* considerations, i.e., ones concerning how the term "conscious" is or should be used, but that it is unclear how conceptual considerations are supposed to bear on the issue of whether DF, as a matter of fact, does or does not have (phenomenally)[13] conscious experiences of form.

In a vein similar to Wallhagen, Mole (2009, 2013) argues that DF's experience contains *embodied demonstrative* content, content representing the slot's orientation in the posting task as something like "*this* way round." This form content, claims Mole, is difficult to experimentally discover since it arises only in course of performing motor actions. To support this hypothesis,

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[12] Critics of the Dissociation Argument also contest the visual illusion evidence, doing so in a similar manner, but we lack space for considering these objections here.
[13] The word "phenomenally" is inserted parenthetically to highlight that we are talking about a notion of conscious experience according to which there is no entailment from some mental content being conscious to its being accessible for report; this is in contrast to the idea of so-called access consciousness (see Block 1995, 2002).

Mole points out how DF sometimes correctly identifies an object's shape if she attempts to do so while or just before reaching for it (Schenk & Milner 2006). Mole also claims that it would be hard to explain the confidence with which DF performs visually guided motor actions if she lacked conscious form content. In response to this, Wu (2013) argues that DF's ability to identify object form in these situations might come from proprioception and not vision, the result of an "efference copy" (Wolpert & Ghahramani 2000), i.e., a simulation of an incipient motor action generated by the motor system to predict and correct errors. However, Schenk and Milner (ibid.) take this scenario to be unlikely since the accuracy of DF's grip fails to correlate with the accuracy of her verbal report, something that would be expected if her report was based on a simulation of the motor action about to be preformed.

Another strategy adopted in response to the Dissociation Argument is to appeal to studies or data in which visual experience appears to be guiding or influencing motor action. Briscoe and Schwenkler (2015), for instance, call attention to how in some visual illusion studies that are cited against EBC (e.g., Aglioti, Desouza & Goodale 1995) it is not that the visual illusion has *no* effect on motor action, but rather that the effect on motor action is significantly less than the effect on visual experience, something suggesting that even in these cases "consciously encoded spatial information will make measurable contributions to motor programming" (2015, p. 21). Briscoe and Schwenkler argue, furthermore, that this difference between motor action and visual experience presents under only very specific circumstances, viz., when the motor action is well-practiced (Gonzalez *et al.* 2008), performed rapidly (e.g., Kroliczak *et al.* 2006), right-handedly (Gonzalez, Ganel & Goodale 2006), *and* with binocular vision (Marotta *et al.* 1998); Briscoe and Schwenkler claim that outside of these conditions, the motor systems are "fully susceptible to the effects of visual illusions" (ibid., 21). Overall, Briscoe and Schwenkler take the data to which they appeal to make a good case for visual experience playing a far larger role in motor action than detractors of EBC usually allow.[14]

Using a similar strategy, Shepherd cites experiments that he believes show visual experience to make a "critical causal contribution" to motor action (2015b). One is a study in which the experimenters altered the visual illusion while subjects were midway through performing a motor action toward it, the result being that the visual illusion had a greater effect

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[14] Briscoe and Schwenkler also make several notable points about the case of DF, but we lack space for considering them here.

on motor action than it would have otherwise (Caljouw *et al.* 2011). Another study showed that the amount of time that a golfer visually fixates the hole while putting was positively correlated with success (Vine *et al.* 2013); since this longer "quiet eye duration" (Mann *et al.* 2007) is plausibly construed as successful allocation of attention (something probably closely associated with consciousness), this might present an instance where visual experience is guiding motor action. Both studies Shepherd cites are of particular interest, since they are both instances in which visual experience might be involved in the *online* guidance of motor action (i.e., guidance of motor action in the course of its being performed), something detractors of EBC have expressed particular skepticism about ever happening (cf. Milner & Goodale 2010).

The studies cited by Briscoe and Schwenkler and Shepherd indeed seem important to introduce into the debate over EBC. However, the idea that these experiments might lend support to visual experience typically driving motor action is open to several criticisms.

A first criticism applies to Briscoe and Schwenkler's idea that a visual illusion having any effect on motor action could act as evidence for visual experience playing a role in guiding motor action, even if this effect is less than the one had on visual experience. However, it seems that all one would need to show that visual experience is not guiding a motor action is that there is a *mismatch* between the content of visual experience and the motor systems: Plausibly, visual experience could be guiding some given motor action only if the representations appearing in visual experience are the *same* as those guiding the motor action (more carefully, only if the two have type-identical content), something precluded by their being mismatched.

However, even in studies where there is a purported match in content, this falls short of showing that it is visual experience guiding motor action, since it fails to exclude the possibility that the observed motor effects arise because the visual illusions affect representations in early visual areas (e.g., the primary visual cortex), areas tributary to both the dorsal and ventral streams. In such a case, the effects on motor action would arise because it is the (unconscious) representations of early visual areas that are providing the visual information used by the dorsal stream, the conscious ventral stream having been completely bypassed. This might occur because the nature of the visual illusion is such that it is prone to arise within early visual areas (Dyde & Milner 2002; Milner & Dyde 2003), or because of top-down effects from higher areas (Murray, Boyaci & Kersten 2006; Fang *et al.* 2008), ones possibly the result of modulation by attention (Tootell *et al.* 1998; Ito & Gilbert 1999; Somers *et al.* 1999; Fischer & Whitney 2009). And so many of the studies to which Briscoe and Schwenkler and Shepherd appeal cannot yet be considered instances in which visual experience is driving motor action.

One last criticism applies to the Briscoe and Schwenkler contention that motor actions are less affected by visual illusions only under narrow circumstances. A first difficulty here is that the number of studies used to support this claim is rather small. Putting this aside, there is the further problem that they often interpret these studies in an idiosyncratic fashion. For instance, what Briscoe and Schwenkler regard as a slowly performed motor action in (Kroliczak *et al.* 2006) is not taken by the study's authors to be a measure of the content of the motor systems, but rather a "perceptual" measure, i.e., a measure of the content of visual experience; similarly, the motor actions in (Rossetti *et al.* 2005) are not taken by the study's authors to be *slow* motor actions, but rather *delayed* motor actions: In the experiment in question, subjects are not asked to perform the action slowly, but rather are not allowed to perform the action until several seconds after the target is presented, the lights having been shut off in the interim. Similar critiques can be made about the manner in which Briscoe and Schwenkler appeal to other experiments to support the claim under consideration.

As discussed earlier, the character of visual experience strongly suggests that it is closely associated with motor action, given that the kind of fine-grained and detailed information found in visual experience seems well-suited for use in precision movement. This view, however, has been thought to be belied by results in vision science, including the way that DF is able to perform motor actions in absence of normal visual experience, and the way that motor actions appear resistant to the influence of visual illusions. But, as just seen, whether such results make the view that visual experience rarely guides motor action mandatory is an active area of debate, one perhaps not resolved soon.

4: Conclusion

These days, most philosophers are materialists, and therefore worry not about the type of epiphenomenalism that might arise from dualism about phenomenal properties. The same is not true of contemporary empirically driven cases for epiphenomenalism: Whether the conscious will lacks causal efficacy became a worry when the Libet experiments were first performed, and remains so; whether visual experience only rarely (if ever) drives our motor actions became a worry when DF's residual motor abilities were first discovered, and remains so. Likely these will continue to be open questions until we have not just more data, but also further philosophical argumentation, where the latter plays an integral role in the proper interpretation of the former.

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