Difficulties with the Electromagnetic Field Theory of Consciousness: An Update

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Abstract

Two previously identified difficulties with the electromagnetic field theory of consciousness are discussed. The first difficulty is that, although spatiotemporal electromagnetic patterns co-varying with conscious experiences have been identified in rabbits and cats, no analogous patterns have yet been found in humans. Evidence is cited that this is very likely because the relevant patterns are inaccessible from the scalp. Recording from the surface of the human brain will be necessary. Such electrocorticography (ECoG) recordings are feasible in the context of localizing epileptogenic foci, but logistical difficulties have so far prevented their being done with a view to identifying spatial patterns co-varying with conscious sensations. The second difficulty is that, although electromagnetic fields certainly can cause neural firing, the same mathematical calculations that show the need for ECoG reveal that the spatial patterns proposed as being conscious become unidentifiable such a short distance away from their source that they are ill suited to causing behavior by activating neurons in other areas of the brain. This difficulty is rendered unimportant by an accumulation of empirical evidence that consciousness is actually not causal for behavior. A number of philosophical objections to this evidence are dissected. The conclusion is arrived at that the electromagnetic field theory of consciousness is still very much alive and kicking.

Key Words: electromagnetic field, consciousness, mind-brain theory

Seven years ago, after a number of unsuccessful attempts at publishing the idea in a peer-reviewed forum, I used a demand publisher to put out a relatively long exposition of the electromagnetic field theory of consciousness (Pockett, 2000). The essence of the hypothesis was that conscious experience (a.k.a. sensation) would prove to be identical with certain spatiotemporal patterns in the electromagnetic field. These patterns are at present generated only by living brains, but in principle they could be generated by hardware instead of wetware. The characteristics of the patterns were largely unspecified, except that they would probably be transiently occurring, brain sized, spatial patterns of electromagnetic intensity or amplitude (i.e. voltage). One of the points which I then thought to be in favor of the theory was that such localized electromagnetic fields are known to be capable of causing neurons to fire, which in principle offers a mechanism by which consciousness could cause behavior.

Two years later, McFadden (2002) published exactly the same idea, in exactly the same journal that had repeatedly rejected it from me between 1995 and 1997. In response, I fired off a short paper called “Difficulties with the electromagnetic field theory of consciousness” (Pockett, 2002), in which I listed three problems with the original idea that had
been revealed by further thought and experimentation. The present invited paper represents an update on the status of those three problems.

Difficulties 1 and 2

The original Difficulties 1 and 2 have turned out to be intertwined, so I will treat them together. The first problem I saw in 2002 was a technical one. The only means of testing the theory that I could (or can) see is to measure the characteristics of the brain-generated spatial electromagnetic pattern that co-varies with some simple sensation, then reproduce that pattern artificially and stick my head in it. The theory predicts that I should then re-experience the original sensation. The technical problems involved in doing this are legion, but the first hurdle (Difficulty #1) is simply to measure the relevant patterns.

My initial attempts at recording spatial patterns that co-varied with particular sensations used EEG recorded from the scalp. They failed dismally. In the 2002 paper I specified this as Difficulty #2, separated from Difficulty #1 because at the time it was not clear whether the failure was merely due to inadequate experimental technique (at the time I thought the inadequacy might be on the subjective, rather than the recording side) or whether there really is a general, in-principle, lack of one to one correspondence between spatial electromagnetic patterns and sensation.

I am now convinced that at least a large part of the problem is simply technical. The issue turns out to be that the human scalp is, in all likelihood, just too far away from the cortical site of pattern generation for the patterns of interest to be resolved in scalp EEG records.

How have we shown this? The mathematics describing the spatial spread of electromagnetic activity generated by a dipole point source have been well worked out for over a century. More recently, Walter Freeman and colleagues (Freeman and Viana Di Prisco, 1986; Freeman and Baird, 1987; Freeman and Grajski, 1987; Freeman and van Dijk, 1987) showed that rabbits can be successfully classified as either experiencing or not experiencing particular sensations using spatial patterns measured from the surface of the rabbits’ brains. The frequency of these spatial patterns was approximately 0.2 cycles/mm. We have now applied the standard math of electromagnetic fields to show that, if the patterns we are looking for in humans have a spatial frequency similar to that of the rabbit patterns (which is not unlikely, given that both the anatomical types of neuron in the brain and the diameter of the columnar unit of cortico-cortical afferent terminations remain roughly the same over a wide range of species (Szentagotthai, 1977)) then the patterns we are looking for are likely to be generated by dipoles spaced approximately 3 mm apart in the human cortex (Pockett et al., 2007). Further calculations show that in order to resolve the spatial electromagnetic patterns produced by dipoles spaced 3 mm apart, the array of recording electrodes has to be less than 2.5 mm above the dipoles. High-pass spatial filters extend this distance to 4.5 mm. Since the human scalp is at least 15 mm above the cortical dipoles that are active in sensation, it is very unlikely that the patterns we seek will appear in scalp recordings. In order to measure electromagnetic patterns that co-vary with human sensation, it will be necessary to record from the surface of the awake brain.

This imposes yet another layer of difficulty on the project of testing the electromagnetic field hypothesis - a layer which at first sight seems impenetrable. To record from the surface of the brain, one first must breach the skull. Breaching the human skull is an activity likely to be seriously frowned upon in most modern-day cultures. Remarkably however, it turns out that recordings from the surface of the awake human brain are actually made on a routine basis in epilepsy clinics around the world.

Most cases of epilepsy can be well controlled by drugs. But when seizures persist in the face of all non-invasive treatment, surgical removal of the epileptogenic focus (the bit of brain where the seizures start) is an effective cure for the disorder. The main prerequisite for such surgery is accurate localization of the focus. Sometimes this localization is achievable using scalp-recorded EEG. But in a few cases, it is clinically advisable to anesthetise the patient, insert a grid of recording electrodes on the surface of the brain, in what has been determined by scalp EEG as the general vicinity of the focus, wake the patient up again, install them in a hospital room for several days and wait until they have a seizure. The epileptogenic focus can then be
accurately localized using records from the grid. In other words, in the occasional rare case, it is clinically responsible to make exactly the sorts of recordings we need to make from the surface of the awake human brain.

Unfortunately though, all is still not straightforward. There remain two big problems, which (fortunately or unfortunately) are of a political rather than a technical nature. First, there are very few suitable patients and altogether too many researchers wanting access to them. Secondly, the standard grids in clinical use have inter-electrode spacings of 10 mm, while our calculations show that we need inter-electrode spacing of 1 to 1.5 mm. At least in the United States, FDA regulations require ECoG (electrocorticography) grids for human use to be made by licensed manufacturers and used once only. While suitable grids have occasionally been made, they are extremely costly. The upshot is that, while the technology for measuring electromagnetic patterns that co-vary with sensations clearly is available, it is still at this point not available to me. I very much wish that it were.

Difficulty 3

The third difficulty with the electromagnetic field theory of consciousness as outlined in Pockett (2002) is that, while it is certainly possible for localized electromagnetic fields of the sort proposed as being conscious to affect voltage-dependent ion channels and thus cause neuronal firing, it is very difficult to see how they could do so in the exquisite detail necessary to cause detailed motor acts. Neurons, with their precise, micrometer-level, point to point communication capabilities, are much better suited to the control of fine motor behavior than are fields. In fact, the more I learn about motor control, the less likely it seems that motor acts could be controlled in any way at all by an instrument as blunt as a field. As shown by the calculations mentioned above (Pockett et al 2007), the spatial pattern characterizing a local field smears and spreads with distance according to Maxwell’s Laws so rapidly that the pattern is unrecognizable even a few mm away from its neuronal source. Certainly there is zero chance that a spatial electromagnetic pattern generated in the frontal lobes (where decisions are made) could directly affect motor neurons in the spinal cord, as suggested by McFadden (2002). So if one takes as gospel the folk-psychological assumption that consciousness really does cause voluntary behavior, it seems to me increasingly unlikely that the electromagnetic field theory of consciousness could be right.

In view of this, it is something of a relief to watch the steady build-up of empirical evidence suggesting that, at least most of the time, folk-psychology is actually wrong in this regard. Consciousness does not either initiate or control so-called voluntary behavior. Some of this accumulating empirical evidence is detailed by the contributors to the Neuroscience section of Pockett et al (2006). The unpalatability of the obvious conclusion from the evidence is demonstrated in the Philosophy section of the same book. Without exception, the contributors to the Philosophy section of Pockett et al (2006) argue that the science reported in the Neuroscience section is faulty, or incomplete, or at the very least irrelevant to the concept of free will.

Could this be merely a power play between philosophy and science, a sort of intellectual demarcation dispute - or is there something in the philosophers’ complaints? Let us consider their arguments in a little more detail. The most popular device among the philosophers represented in the book is to suggest that the empirical evidence is wrong and/or irrelevant because [insert any or all of] (a) the experiments described involve “abnormal” situations (b) the experiments involve introspection (which, the argument goes, everyone knows to be unreliable) (c) even if the apparently voluntary acts studied in these particular experiments are pre/unconsciously generated, this evidence does not show that all apparently voluntary acts are pre/unconsciously generated.

It is immediately apparent that, if taken seriously, this set of arguments would invalidate not only any possible scientific experiment on consciousness, but any possible scientific experiment at all. All experiments involve situations that could be described as abnormal. All experiments on consciousness involve introspection. And the Problem of Induction (ie the problem that no matter how many swans one observes, one can never prove by induction that swans are white, because there is always a chance that tomorrow one will meet a black swan) is a hardy perennial which
again is applicable to scientific experimentation on any topic you care to name.

Should these arguments be taken seriously? It is undeniable that science – the proposal of hypotheses and the testing of these by experimentation - has a remarkably good record of successfully explaining how the world works. So whatever the formal accuracy of reasons (a) and (c) for disbelieving in the efficacy of experimentation in general, it could be argued that in practice, these two arguments simply lack significant force. Reason (b) is specifically applicable to the study of consciousness - perhaps it invalidates the whole idea of doing scientific experiments on consciousness? I think not. Argument (b) stands or falls on the contention that introspection is unreliable. This surely depends on what introspection is being asked to do. The original introspectionists were trying to determine by introspection how the brain works. They were unsuccessful in this, because the task is impossible. We can not determine by introspection the workings of, for example, the visual system, because these workings are not accessible to consciousness. But the experimenters in the Neuroscience section of Pockett et al (2006) are not asking their subjects to introspect on the workings of the subjects’ brains – all they are asking for is reportage of the end product of all the subconscious processing, the experiences that are accessible to consciousness. Where was the rotating spot when you became conscious of the urge to move your finger? Do you believe that you caused this cursor to stop? While these questions may be difficult to answer, they are not impossible. And if we are to study consciousness scientifically at all, raw reports of subjective conscious experience must be given credence and taken at face value.

Other arguments put forward by the philosophers contributing to Pockett et al (2006) are:

- **The experiments described in the Neuroscience section are irrelevant to free will, because the concept of free will does not apply to the initiation and control of movements.** This argument basically represents an attempt to rescue the concept of free will by the simple expedient of redefining it. But as far as I can see, while the concept of free will may well not apply to the control of movements, it applies exactly to the initiation of movements. It is not much use willing something to happen if you never do anything about it, and doing something about it by definition involves initiating some sort of bodily movement.

- **Both the mental phenomenology and the neuroscience of action are still far from well understood, and until we have a fuller understanding of these matters we should not make pronouncements one way or the other about “the will”.** There is, of course, a good deal to recommend this traditionally scientific, humble-toiler-in-the-vineyard sort of position (although in this case it does seem to me to fail to do justice to what has been shown). Amusingly, though, the moral high ground on which this particular vineyard is planted eventually proves an irresistible pulpit from which to make the author’s own final pronouncement, that it is “highly unlikely that the phenomenology of agency is systematically misleading. We experience ourselves as agents who do things for reasons, and there is little serious reason to suppose that we are mistaken on either count.” What evidence is adduced for this? None at all. If one refuses the other guy permission to make pronouncements because too little evidence is available to support any conclusions, surely it is only fair (and sensible) to remain silent oneself.

- **The start of the readiness potential [the brainwave preceding voluntary movements] might represent only an unconscious urge to move and the actual decision to move might not take place until the [later] time at which the subject says it does - which might mean that the decision to move is caused by consciousness after all.** This argument begs the questions of (a) who gets to assign the labels urge and decision to particular neural events and what justification for doing so should be required, and more specifically (b) whether there could even be such a thing as an unconscious urge – personally I would have thought that urges by definition were consciously experienced. This author’s only argument in favor of his hypothesis is that it is possible to react to external stimuli in less time than the 500 ms that elapses between the start of the RP and the movement. Quite why this should be thought relevant, given that there is good experimental evidence that
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actions and reactions are subserved by different neural pathways, escapes me.

- There is no evidence that “will” actually is a folk concept and “the notion of free will is at best vague and at worst meaningless”. While this may or may not be the case with regard to folk concepts, many generations of philosophers would object to the idea that it is the case with regard to philosophical discourse. When it comes to discussing the thesis that “the real causes of human action are unconscious”, this author does not address the experiments in the Neuroscience section of the book at all. What he does is bring up a whole series of other experiments - experiments which nobody, or at least nobody in the present debate, has even suggested are arguments against the idea that consciousness causes behavior - and show that these are not good arguments against the idea that consciousness causes behavior. He then picks holes in the arguments in the Neuroscience section as if those arguments were directed not to the question of whether or not consciousness causes behavior, but to the separate question of whether or not intentions cause behavior. No wonder the title of this particular paper mentions straw men ...

In summary, to put it mildly none of these arguments seems to me to hold much water. On the other hand, although science has undeniably put some serious dents in the folk-psychological idea that consciousness causes behavior, much work remains to be done. On the basis of the science at this stage, we can not make a definite declaration that “consciousness never causes behavior.” But as I see it, the onus of proof has now subtly shifted, from those who are inclined to believe that consciousness does not cause behavior to those (still the majority) who believe that it does.

Even one experimental demonstration that consciousness actually does cause a behavior – that the model depicted in Figure 2 of the Editors’ Introduction in Pockett et al (2006) is wrong – would go a long way towards settling this debate. Of course by rights, any such demonstration should have to overcome the criticisms that experimental situations are abnormal, that one demonstration does not a conclusion make and that introspection is unreliable. But I suspect that if the boot were on the other foot, such criticisms might fade quietly into the background.

Conclusions

So what, after all that, is the present status of the electromagnetic field theory of consciousness? The theory has, I think, survived the criticism that electromagnetic fields are unsuited to a directly causal role in voluntary movement (a.k.a. behavior). The answer here is that it is becoming increasingly likely that voluntary movements are not directly caused by consciousness. And if behavior is not directly caused by consciousness, there is no requirement for putatively conscious electromagnetic fields directly to cause behavior.

But on the down side of the ledger for the theory, we still have not managed to describe a single empirical example of a spatial electromagnetic pattern that covaries with a particular kind of human conscious experience. The only overt advance in that direction over the past seven years has been to determine the methodology that will very likely be necessary for making such measurements. Basically, we need access to human electrocorticography (ECoG). Ask me again in another seven years.

References

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