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Midwifing a Science of Consciousness: the Role of Kuhnian Paradigms

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Abstract

It is argued that in terms of Thomas Kuhn's analysis of how different fields of science develop and progress, consciousness research is still in the pre-paradigm or pre-science phase that precedes the advent of any universally accepted paradigm. At present, the three mini-paradigms that drive experimental research on the nature of consciousness are: (1) the cognitive-science process theory mini-paradigm ("consciousness is a process, not a thing"), (2) the neurophysiologists' preferred psychoneural identity theory mini-paradigm ("consciousness is brain activity") and (3) the EMF field theory mini-paradigm ("consciousness is a 4-D electromagnetic pattern generated by brain activity"). In established science, paradigms shift when enough 'anomalies' – falsified predictions or largely unrecognised but once-recognised-unacceptable consequences – build up to make the existing paradigm uncomfortable for those who operate within it. At this point, a sudden paradigm shift occurs, ushering in another long period of 'normal science' during which the new paradigm drives experimentation. With regard to the three existing mini-paradigms on the nature of consciousness, it is argued that (1) recognition that *processes* are abstract entities –and that this renders the "consciousness is a process, not a thing" mini-paradigm dualist – makes this mini-paradigm unacceptable to practitioners who regard dualism as unscientific and who prefer to see themselves as staunchly scientific, and therefore as monists. (By definition, monists equate consciousness with physical entities, while dualists equate it with abstract entities). (2) The strong prediction of the "consciousness is brain activity" mini-paradigm – that conscious experiences should invariably correlate with the firing of either particular single neurons or groups of single neurons in the brain – has now been falsified often enough to make this mini-paradigm unacceptable to its practitioners. And this leaves intact only the "consciousness is a 3-D electromagnetic field" mini-paradigm – the idea that conscious experiences *are* particular 3-dimensional (or, given that they change in time, strictly speaking 4-dimensional) patterns in the electromagnetic field generated by brain activity. And as a result, it is suggested that this third mini-paradigm might usefully become the first universally accepted full paradigm, which would finally allow announcement of the birth of a Kuhnian science of consciousness.

Key Words: Kuhn, science, paradigm in consciousness, neural correlates of consciousness, NCC, neuron doctrine

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Introduction

Thomas Samuel Kuhn's book *The Structure of Scientific Revolutions* is one of the few works in the philosophy of science that is actually read by working scientists. A contributing factor here may be that Kuhn himself qualified as a scientist and thus never presumes to prescribe how science should be done – he simply describes, in a particularly novel and interesting way, how it is done. In particular, the experience of earning a PhD in physics from Harvard in the 1940s taught him about the peculiar combination of factual knowledge and tacit tribal attitudes that all graduate students must internalize if they are to survive the rigors of scientific training. Kuhn first names this complex a 'disciplinary matrix', meaning "the entire constellation of beliefs, values, techniques and so on shared by members of a given [scientific] community" (Kuhn, 1996; p.175). But this meaning is also one of the two senses in which he then famously redefines the term 'paradigm'. The other, subtly different, sense denotes an 'exemplar', meaning "one sort of element in that constellation, the concrete puzzle solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science" (Kuhn, 1996; p.175).

The fact that throughout *The Structure of Scientific Revolutions* Kuhn uses these two meanings of 'paradigm' indiscriminately has generated a great deal of argument among philosophers (Preston, 2008), but has generally been ignored by scientists.

Perhaps the core thesis of the book – that science proceeds by means of a series of relatively sudden paradigm shifts separated by long periods of 'normal science' – has seemed attractive enough to make it unimportant exactly which kind of paradigm does the shifting. However, one of the things I suggest in this essay is that the precise meaning of the term 'paradigm' may matter more during the once-only period before a new area of study acquires its first universally accepted paradigm, and thereby (according to Kuhn) becomes a science.

Preston (2008) distills Kuhn's views about this uniquely wild-west, anything-goes time as follows: "The first phase in the typical pattern of the history of a science … is the pre-paradigm or pre-consensus period. Such periods are marked by a certain kind of thorough-going diversity or fragmentation. There are multiple views of the phenomena in question, amounting to different concepts of these phenomena. There are no agreed canons of explanation, no fixed methodology, no acknowledged scientific authorities. Instead there's a plurality of competing 'schools' of doctrine, each deriving strength from its relation to some particular metaphysic, but none having the upper hand. The activity of the researchers involved allows for unlimited disagreement and criticism of each and every assumption. But this debate over fundamentals is directed against other researchers, not towards nature."

Scientists interested in elucidating the nature of consciousness (and I use the word here to mean sensory experience – the sound of a flute, the feel of silk moving over the skin, the aroma of baking bread) may recognize the description above as a startlingly accurate description of the situation in which our field still finds itself, over a century after the first psychologists (Wilhelm Wundt in Europe and William James in America) started studying consciousness scientifically and slightly more than twenty years after the start of the modern series of annual conferences called "Toward a Science of Consciousness". This seems an extraordinarily long gestation period. The most determined of optimists might be forgiven for starting to wonder whether 'a science of consciousness' will ever be born.

How are we to emerge from this lengthy gestational phase? Can anything be done to induce labour? For once, Kuhn doesn't help much here, saying only "History suggests that the road to a firm research consensus is extraordinarily arduous." (Kuhn, 1996; p.15). While this is obviously and painfully true in the present case, I suggest that we may be able to make some useful progress by: deciding exactly what characteristics the first universally accepted paradigm of consciousness research would need to have – in other words, clarifying the definition of the word 'paradigm' in this regard – and perhaps applying to this pre-science period Kuhn's analysis of how science proceeds later on – by slow accumulation of anomalies that cannot be explained on an existing paradigm, until eventually a seismic shift in thinking occurs and a new paradigm is born. Application of this analysis to our present situation would involve thinking of each pre-paradigmatic school as operating according to its own mini-paradigm and seeing whether the means by which Kuhn deems paradigm shifts to occur during 'normal science' might also be applied to the question of how one or other of these mini-paradigms might emerge as the first universally accepted paradigm in this field.

What characteristics would the first universally accepted paradigm in consciousness research need?

Which of Kuhn's two definitions of 'paradigm' is the more relevant here? In order to decide this, we need to look at the present situation in consciousness research. I contend that the present situation has been brought about almost entirely by Francis Crick's admonition in the early 1990s to forget about theories of the nature of consciousness and concentrate on measuring its "neural correlates" (Crick and Koch, 1990; Crick, 1994).

Among the neuroscience community, many of the ears on which this suggestion fell were initially deaf to it. I vividly recall the Society for Neuroscience plenary talk in which Crick first put forward this proposal, ending with a hand-drawn slide saying "NCC NOW". The presentation took place in a huge barn of an auditorium, packed with several thousand expectantly buzzing neuroscientists, all come to breathe a little of Crick's Nobel dust. By the time the great man was half way through his talk, 50% of them had walked out. It was astonishing. The arrogance of youth takes no prisoners.

However, some of those who stayed did go home and start measuring NCCs (neural correlates of consciousness). As a result, quarter of a century later we have a veritable flood of nominal NCCs, most of them measured without the explicit direction of any specific theory and using whatever technique or methodology was readily available to the experimenter.

Quite apart from the fairly serious problem that such activity has so far generally conflated the correlates of conscious experience per se with the correlates of various processes like attention and memory, which usually go with conscious experience (an issue independently identified in Ch.1 of Pockett, 2000 and a decade later by Aru *et al.,* 2012b and Aru & Bachmann, 2015), the lack of any underlying theory to dictate what kinds of measurements should be made in this regard has inevitably produced what Kuhn uncharitably calls a “morass” of data (Kuhn, 1996; p. 16). In Kuhn’s words, “in the absence of a paradigm or some candidate for a paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar.” (Kuhn, 1996; p.15).

Once again, this observation is extraordinarily relevant to our present situation. Crick’s proposal that scientists should forget about theory and concentrate on measuring neural correlates was undoubtedly useful in a heuristic sense, in that it enabled people to start treating consciousness as a tractable scientific problem rather than just an occasion for fruitless philosophizing. But I contend that this usefulness has now run its course. We now have all too many randomly-collected NCCs. What we need to induce the birth of a science of consciousness is theory, to tell us which of the huge body of existing NCC observations “will later prove revealing” and which “will for some time remain too complex to be integrated with theory at all” (Kuhn 1996 p.16). As Kuhn (ibid) puts it: “no natural history can be interpreted in the absence of at least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation and criticism. If that body of belief is not already implicit in the collection of facts … it must be externally supplied”.

Hence, I suggest that the main feature we are looking for in our projected first paradigm of consciousness research is an acceptable theory about the ontological nature of the beast – a theoretical underpinning that will allow selection, evaluation and criticism of existing data and direct the collection of more. At this stage we are not looking for the ‘exemplar’ kind of paradigm, which is based on a particular empirical achievement that provides a model for how things should be done in the future. We are looking for a paradigm characterized by (in Kuhn's terminology) a strong underlying 'metaphysic' – a well-supported theory about the nature of consciousness.

Existing classes of theory about the nature of sensory experience

What, if any, big-picture theories have underpinned data collection in the existing schools of consciousness research? Apart from the inchoate idea that consciousness has something to do with quantum mechanics (which has so far not produced any actual experimentation at all) there are presently two broad theoretical schools in this matter: the psychology /cognitive science /computational school and the neurophysiology/ neuroscience school. The former group operates on the metaphysic "consciousness is a process, not a thing". The latter assumes that conscious experiences are physical things.

In my experience, both of these groups find the other's viewpoint incomprehensible, to the extent that workers in either tradition are unable to believe that any sensible person could possibly hold the other view. Nobody has yet done the sort of sociological survey that might quantify the truth of this observation, but to illustrate it anecdotally, two representative examples of verbatim statements from real people of my acquaintance are (from a cognitive scientist) "hardly anyone today would seriously believe that subjective experience is a "thing" (well, perhaps with few exceptions…)" and (from a cellular neurophysiologist) "What's wrong with saying 'consciousness is the brain' and leaving it at that?"

The deep mutual incomprehension revealed by such statements supports the notion that the two schools should be treated as operating under what Kuhn calls incommensurable paradigms. Incommensurability is only fuzzily defined in *The Structure of Scientific Revolutions* and in the interests of accuracy the paradigms I propose as influencing each current school are probably better called mini-paradigms, in that neither presently holds sway over the whole field of research. Nevertheless, the following discussion is based on the idea that each broad school of consciousness research does operate according to a particular mini-paradigm.

The "consciousness is a process, not a thing" school

The widely stated dictum "consciousness is a process, not a thing" has been attributed (Tononi and Edelman, 1998) to William James, although I have never been able to find a clear statement of this view in the two volume book cited (James, 1896). At this point I should probably reveal that my own trade ticket is in neurophysiology (though I could now be considered something of a lapsed neurophysiologist, in that I no longer hold many of the tribal beliefs I grew up with), so I personally find the mantra “consciousness is a process not a thing” incomprehensible. It took me quite a while to figure out exactly why I feel that way, but eventually I concluded that for me, the biggest problem is that this view is quintessentially dualist. I am constitutionally inclined to assume that consciousness is something physical, so I am unable to get to grips with the idea that it is an abstract entity (a process), rather than a physical entity (a thing). Oddly enough it is my personal observation that most of the scientists who do espouse the idea that 'consciousness is a process not a thing' also prefer not to think of themselves as dualists, however, so I should probably expand a little on my contention that they are.

Definitions are needed. First, dualism is currently defined by Wikipedia as “the position that mental phenomena are, in some respects, non-physical (Hart, 1996), or that the mind and body are not identical (Crane and Patterson, 2001).” Secondly, abstracta and concreta are defined by the Stanford Encyclopedia of Philosophy as follows: “Some clear cases of abstracta are classes, propositions, concepts, the letter ‘A’, and Dante's Inferno. Some clear cases of concreta are stars, protons, electromagnetic fields, the chalk tokens of the letter ‘A’ written on a certain blackboard, and James Joyce's copy of Dante's Inferno.”

From these definitions it seems clear that (a) equating consciousness with a non- physical entity is dualism and (b) processes per se are non-physical entities, in that they are members of the class abstracta, which stands in opposition to the class concreta a.k.a. physical things. Processes per se are not physical things. Therefore, equating consciousness with a process per se is dualism.

One common response to this analysis is to try dismissing it by saying something like “processes are ... conceptually abstract but can be grounded in the physical” – in other words, processes are instantiated by things. This is of course true – and if members of this school just said “consciousness is a process”, they could be taken as meaning that consciousness is the ever-changing spatiotemporal combination of things that make up any particular example of a brain process. That position would definitely not be dualist – but it would not be substantively different from the position espoused by the opposing school described in this essay, either. But members of the school we're talking about here do not say “consciousness is a process”. They say “consciousness is a process, not a thing”. The addition of the last three words specifically repudiates the idea that consciousness may be some temporal or spatial arrangement of the concreta or things that instantiate any given brain process (the neurons, the glia, the moving ions, the resulting electric and magnetic fields). No, members of this school are saying quite explicitly that consciousness is not these – or any other sorts of – things. Their position is clear – consciousness is NOT a physical thing. And going by the definition of dualism cited above, that is dualism, pure and simple.

As an aside, essentially the same analysis also applies (Pockett, 2014) to the distinct but related position (e.g. McFadden, 2013) that consciousness is information. Once again, information is an abstract entity. Once again, most of the people who equate consciousness with information prefer not to think of themselves as dualists. An honorable exception is David Chalmers, who as a good philosopher clearly recognizes and admits his dualism, without shame (Chalmers, 1996).

OK, so what if members of the 'consciousness is a process not a thing' school are unwitting and/or unwilling dualists – does this really matter? In one sense, no of course not. In philosophical terms, it is perfectly acceptable to be a dualist. In scientific terms, if we leave out the all-important rider “not a thing”, the idea that “consciousness is a process” is sufficiently non-specific to mandate the collection of any kind of data at all, which makes all of the massive number of NCC-type observations accumulated by members of this school perfectly valid on their own terms. (Whether these observations are all equally important is a different question: for example, it is technically easy and therefore much done to filter ongoing EEG and ECoG oscillations into arbitrary frequency bands and measure the relationship of these to consciousness, but given the complete lack of consensus about how any EEG frequency band is physiologically generated (Pockett, 2000; Ch 2) and the extremely basic disputes over whether consciousness as opposed to unconsciousness correlates with higher or lower gamma power and synchrony (Vanderwolf, 2000; Imas *et al.,* 2005; Pockett & Holmes, 2009) and whether local gamma power does or does not correlate with subjective sensation (Aru *et al.,* 2012a; Bachmann & Hudetz, 2014), NCC-type observations involving the frequency of ongoing electromagnetic oscillations probably fall into the category “will for some time be too complex to be integrated with theory at all” (Kuhn, 1996; p.16).

But where the dualism of the position that consciousness is not a thing does matter is that this firm (yet largely unexamined) belief clearly leads its holders to refuse to do – or even allow anyone else to do – the methodologically difficult experiments mandated by the hypothesis that consciousness IS a particular kind of thing. When all the referees for all the granting agencies in the world firmly believe that consciousness is NOT a thing – and worse, that only a fool could even countenance such heresy – it becomes impossible to obtain the financial backing necessary to do the critical experiments dictated by the alternative view. It is my hope that recognition of the dualism of their present belief-set might convert at least a few members of the “consciousness is not a thing” school to a more permissive stance.

The ‘consciousness is a thing’ school

Unlike the ‘consciousness is a process not a thing’ school, nobody in the ‘consciousness is a thing’ school actually goes around saying “consciousness is a thing”. On the contrary, the very word 'consciousness' is unofficially banned among much of this group. 'Perception' and 'subjective experience' are (marginally) acceptable terms, but for a neuroscientist or neurophysiologist, blatant use of the word 'consciousness' still carries professionally near-suicidal connotations of new-age flakiness.

However, this rather odd tribal prejudice notwithstanding, there is among at least the neurophysiological subset of this school a strong, if largely tacit tradition of assuming identity between subjective experience and the brain. This tradition originated with the neural (aka psychoneural, aka mind/brain) identity theories of philosophers Place (1956), Feigl (1958) and Smart (1959), but the first person to dare publish essentially this view in a neurophysiology journal was Horace Barlow (1972). Barlow’s central proposition was: “our perceptions are caused by the activity of a rather small number of neurons selected from a very large population of predominantly silent cells. The activity of each single cell is thus an important perceptual event and it is thought to be related quite simply to our subjective experience.” Barlow's circumspection here is obvious. Like his successor Crick (1994), he stops short of explicitly proposing identity between subjective experience and the activity of particular single cells – the intellectual and intuitive difficulties inherent in that position are too great. In fact, Barlow quite openly acknowledges that not all single neural events are even “related quite simply to” conscious perception, that it is not at all clear why some are and others are not and that the concept of the pontifical or grandmother cell, which appears on the face of it to be a logical extension of the single neuron doctrine, is untenable (a case made in detail much later by Gross (2002) and Connor (2005)). Nevertheless, Barlow’s “first dogma” specifically asserts that the “significant level of description” for the study of how the brain represents sensory information is the individual cell and his paper “Single units and sensation: A neuron doctrine for perceptual psychology?” essentially defined the paradigm adopted by a generation of neurophysiologists for the study of perception.

Used in this sense, the word ‘paradigm’ better fits the second of Kuhn’s definitions: the one where paradigm means ‘exemplar’. The examples of successful experimental accomplishments deemed to provide a model for future work in this case were the later-to-be-Nobel-winning work of Hubel and Wiesel (e.g. Hubel and Wiesel, 1959; 1965) and Barlow’s own similar studies (e.g. Barlow and Levick, 1965). This methodologically ground-breaking work used single unit recording to track the influence of visual stimuli on the firing of action potentials by a series of different kinds of neuron in the visual system of cats. Thanks to the results, Barlow thought the rare neurons whose firing would turn out to be "related quite simply to" conscious experience were the complex and hypercomplex cells at the top of the visual feedforward hierarchy.

However, a series of later empirical findings have now made this assumption untenable, to the extent that I suggest a mini-paradigm shift away from the single neuron doctrine may be quietly taking place within the "consciousness is a thing" school. These anomalous empirical findings are described in the next section.

Anomalies upsetting the single neuron doctrine

The buildup of findings that cannot be explained on the original single neuron doctrine has been slow but steady. The main anomalies (to continue with Kuhn's terminology) are two-fold. One major problem for the single neuron doctrine arose with the discovery that late, recurrent (aka reentrant, aka feedback) activity in V1 is necessary for visual experience (Lamme *et al.,* 1998; Lee *et al.,* 1998; Pollen, 1999; Lamme and Roelfsema, 2000; Pascual-Leone and Walsh, 2001; Supèr *et al.,* 2001; Juan and Walsh, 2003; Pollen, 2003; Ro *et al.,* 2003; Juan et al 2004; Lamme 2004; Boyer et al. 2005; Silvanto et al 2005; Fahrenfort *et al.,* 2007; Pollen, 2008; Koivisto et al,. 2010).

Bachmann (2014) disputes the conclusion that feedback activity in V1 is required for subjective visual experience by claiming that the experiments purported to demonstrate this fail to distinguish between subjective experience and various non- specific concomitants thereof, like arousal and attention. Bachmann's argument may or may not explain the particular unusual case he cites, but in my view it would require extraordinarily unconvincing contortions to explain in these terms the results, for example, of Boyer et al (2005), who show that blindsight can be induced by brief TMS inactivation of V1 at 100 ms post-stimulus, or Silvanto et al (2005), who report that consciousness of motion can be ablated by TMS delivered to any of (i) V1 at 40– 60 ms, (ii) V5/MT at 60–80 ms, or (iii) V1 at 80–100 ms post-stimulus. While it is a priori reasonable to expect that arousal and attention do contribute to the generation of subjective experience, it is similarly reasonable to expect that normally their contributions would occur much earlier than 100 ms post-stimulus. There is also abundant evidence from other types of experiment that conscious sensation correlates with synaptic activity in primary sensory cortex at around this time – for example repeated reports show that the small, early waveforms of auditory (Pockett, 1999) and somatosensory (e.g. Cauller and Kulics, 1991; Porkkala *et al.,* 1997) evoked potentials persist under clinical anaesthesia, while loss of consciousness during increasing concentrations of general anaesthetic correlates with reduction of the 'middle latency' waves around 80–100 ms post-stimulus in humans (50 ms in monkeys, which are physically smaller) and at least with regard to somatosensory evoked potentials there is good evidence that the waves in question do arise in primary somatosensory cortex (Cauller and Kulics,1991; Allison *et al.,* 1992). Obviously more work needs to be done to elucidate mechanisms, but it seems to me quite clear at this stage that late synaptic activity in primary sensory cortex is necessary for sensory experience, not only in the visual but also the somatosensory and auditory modalities.

If subjective visual experience does depend on late activation of V1, this finding is a problem for the single neuron doctrine in that it cannot be reconciled with a number of reports that the firing of single units in V1 generally does NOT correlate with consciousness. Such reports include the following: (a) 90% of neurons in the temporal cortex predict perception during binocular rivalry, while only 18% of units in V1 modulate their firing in line with perception (Logothetis and Schall, 1989, Leopold and Logothetis, 1996; Sheinberg and Logothetis, 1997); (b) individual neurons that fire only in response to pictures of specific people are found in the human temporal lobe, but not in V1 (Quiroga *et al.,* 2005); (c) single cell firing rate and high frequency LFPs (local field potentials) in V1 do not correlate with consciousness, while low frequency LFPs and fMRI signals do (Wilke *et al.,* 2006; Maier *et al.,* 2008); (d) contrariwise, there is a great deal of evidence that V1 does contain single unit representations of information that can not be perceived consciously: for example (i) we can not consciously perceive which eye a stimulus is presented to, even though V1 contains representations of this information (Blake & Cormack, 1979); (ii) if a grating has a very high-frequency or is crowded by surrounding gratings its orientation cannot be perceived, even though this information is represented in V1 (He *et al.,* 1996; He and MacLeod, 2001); (iii) single cells in V1 differentiate between local depth cues even when those cues do not give rise to an overall depth percept (Cumming and Parker, 1997); (iv) blinks (Gawne and Martin, 2000) and microsaccades (Martinez-Conde *et al.,* 2000) are not usually perceived, but are reflected in the activity of cells in V1; individual striate cells follow rapid chromatic flickering of a coloured grating when the flickering is too fast for humans to resolve individual colours (Gur and Snodderly, 1997). More such examples are summarized by Rees (2007), who points out that most of them may result from the feedforward pass of activity through V1. To summarize the problem all of this poses for the single neuron doctrine: if visual perception requires late synaptic activity in V1, but the firing of single units in V1 generally does not correlate with consciousness, it is a reasonable conclusion that the firing of single units may not be the best marker of consciousness.

Another dataset that fails to fit the single neuron doctrine is the association between consciousness and the synchronous firing of neurons in widely separated areas of brain (Roelfsema *et al.,* 1994; 1997; Fries *et al.,* 1997). These data led first to an ad hoc modification of the single neuron doctrine proposing that binding can be explained not by the firing of particular consciousness-related cells, but by the simultaneous firing of multiple single neurons forming a 'cell assembly'. However, evidence (Lamme and Spekreijse, 1998) and arguments (Shadlen and Movshen,1999) against this idea soon appeared. Later, the synchrony and other observations (for example that phenomenal experiences were not reported without 'ignition' of an extensive prefrontal network (Dehaene and Naccache, 2001; Del Cul *et al.,* 2009, Dehaene and Changeux, 2011) were claimed as evidence for the concrete existence of a hitherto only functionally defined 'global workspace'. These claims in turn were disproved or at least weakened by the findings that (a) when subjects do not have to report their experiences, there is no difference in prefrontal activity between conscious and non-conscious stimuli (Tse *et al.,* 2005) and (b) magnetically stimulating prefrontal cortex affects voluntary control of bistable stimuli, but not passively experienced bistable stimuli (de Graaf *et al.,* 2011), suggesting that prefrontal activity is needed for report and voluntary control, but not for phenomenal experience per se. On a related theme, signal detection theory deconstruction of the basis of stimulus reportability showed that V1 activity correlated with stimulus processing but not decision or report, parietal/temporal activity correlated with decisions but not stimulus presence and activity in motor regions correlated with report (Hulme *et al.,* 2008).

Again to summarize the problem all of this poses for the single neuron doctrine, synchronous firing of single units in widely separated areas of brain does appear to be a correlate of conscious experience, but the single neuron doctrine not only does not predict this, it cannot even be plausibly modified to explain it. Things are not looking good for the single neuron doctrine.

A new mini-paradigm for the "consciousness is a thing" school?

I am now about to make a claim the truth of which is not immediately obvious and detailed justification of which will therefore be supplied later in this section. The claim is that the major empirical observations constituting bothersome anomalies on the single neuron doctrine collapse into tame predictions of the hypothesis that conscious experiences are identical not with the firing of single units, but with the EM (electromagnetic) fields associated with spatial patterns of population EPSPs (excitatory postsynaptic potentials) in primary and/or secondary sensory cortex.

This hypothesis is called the EM field theory of consciousness (Pockett, 1999) and is discussed in some detail in Pockett (2000) and Pockett (2012). The relationship between the patterns of voltage that for technical reasons are the only things we can presently measure in this regard and the EM fields that give rise to these voltage patterns is discussed by Hales and Pockett (2014).

I have promised to justify the core claim above and again promise to do so later in this section. However, in order to understand the justification it is first necessary to understand the mechanisms by which population EPSPs are generated. The mechanisms described next have long been enshrined in standard neuroscience texts (e.g. Kandel *et al.,* 1991) and are well explained in more modern terms by, for example, Buzsaki et al (2012), but for the benefit of readers whose existing disciplinary matrix does not include this material I summarize it in Figure 1 and the next few paragraphs, with apologies to those for whom this is unnecessary overkill.

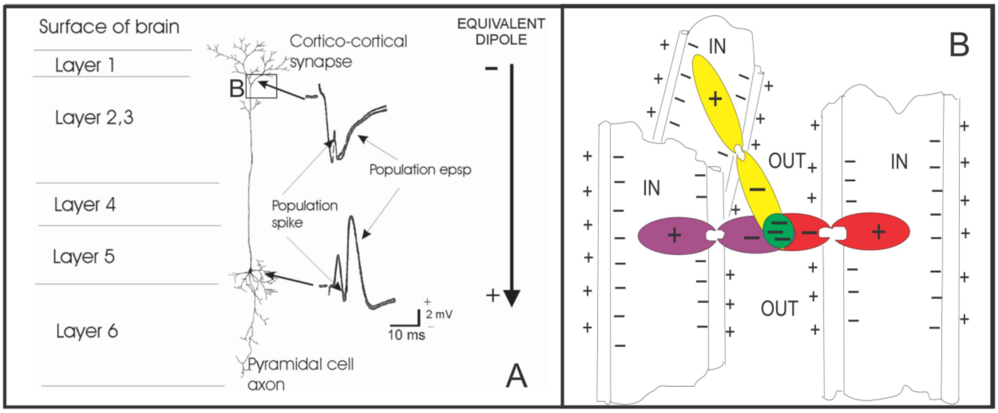

Figure 1. Generation of population excitatory postsynaptic potentials (EPSPs). A: Generation of translaminar dipole. B: Mechanism by which summation of negative poles of the transmembrane dipoles involved with multiple individual EPSPs produces a population EPSP.

When an excitatory synapse onto the apical dendrite of a cortical pyramidal neuron (shown in Figure 1A) becomes active, neurotransmitter is released from the presynaptic axon terminal (not shown in Fig 1A) and diffuses across the synaptic cleft to bind to receptors on the postsynaptic dendrite. As a result, ion channels in the postsynaptic dendrite open and positively charged ions to flow into the dendrite, leaving a transient negative 'hole' in the extracellular fluid around the synapse. The internal positivity and concomitant external negativities resulting from this process are depicted as transmembrane dipoles in Figure 1B. The external negative holes are soon filled and electrical status quo restored by an influx of positive ions more or less simultaneously released from other regions of the postsynaptic cell (the ion flow achieving this used to be called a loop current), but for a few tens of ms, an extracellular negativity is measurable near the synapse (top of Figure 1A) and a concomitant external positivity outside other regions of the cell (bottom of Figure 1A). The negative/positive pair is sometimes depicted as a translaminar equivalent dipole (right of Figure 1A). The transient peri-synaptic areas of internal positivity and external negativity resulting from this process are depicted as the purple, yellow and red transmembrane dipoles in Figure 1B. Both internal and external voltage transients are called excitatory postsynaptic potentials (EPSPs). The kind that can be measured between two electrodes in the extracellular fluid is called an extracellular EPSP and the kind that can be measured between an electrode inside the post- synaptic neuron and an electrode in the extracellular fluid is called an intracellular EPSP.

So far, so basic. But wait, there's more. Because the apical dendrites of all neocortical pyramidal cells are physically aligned (all of them extending perpendicular to the surface of the cortex), more or less simultaneous activation of many synapses at a similar level on many neighboring apical dendrites allows many individual extracellular EPSPs to summate, forming a large voltage transient known as a population EPSP. Such summation is crudely illustrated by the green area in the cartoon in Figure 1B.

The measured voltage transients shown in Figure 1A also reveal that if individual intracellular EPSPs are large enough, action potentials will be initiated at the initial segment of each individual pyramidal cell and the extracellular manifestations of these may also summate to produce a 'population spike' in the middle of the population EPSP. Recent work (e.g. Anastassiou *et al.,* 2011; Kajikawa and Schroeder, 2011; Linden *et al.,* 2011; Buzsáki *et al.,* 2012; Destexhe and Bedard., 2012; Einevolle *et al.,* 2013; Reimann *et al.,* 2013) suggests that various additional mechanisms may also sculpt the LFP, but population EPSPs are generally accepted as the dominant contributors.

One final point needs to be made. Large population EPSPs only occur when synapses on the anatomically aligned apical dendrites of pyramidal neurons are activated. Synapses on stellate cells do not produce large population EPSPs. This is because of the intrinsically dipolar nature of extracellular EPSPs (right hand side of Figure 1A). For every negative-going extracellular EPSP around a synapse, there has to be a positive-going extracellular EPSP, generated by the exit of positive ions from other regions of the post-synaptic cell. The dendrites of stellate cells project from the cell body in all directions, so the positive and negative poles of each extracellular EPSP dipole tend to cancel each other out. It is only when many dendrites are aligned that enough extracellular EPSPs can summate to produce a very large population EPSP.

With this information in mind, we can finally come to the promised justification of the statement that the two major findings constituting anomalies on the single neuron doctrine of perception are merely obvious predictions of the EM field theory of consciousness. The two major anomalies inexplicable on the single neuron doctrine were (1) feedback aka recurrent activity in primary sensory cortex is necessary for consciousness and (2) synchronous firing of action potentials in widely separated areas of brain is involved in consciousness. How does the EM field theory predict each of these?

How does the EM field theory predict the requirement for feedback aka recurrent activity? The EM field theory equates subjective sensory experiences with EM fields that are not only spatially patterned in a diagnostic (but presently unspecified) way, but also large enough in amplitude. Such fields will not arise during the first, feed- forward pass of neural activity from the thalamus through primary sensory cortex, because feed-forward activity traverses primary cortex by way of the stellate cells of Layer 4 (Salami *et al.,* 2003). As we have just seen, stellate cells do not produce large population EPSPs. Large population EPSPs are generated only at recurrent synapses (Garrido *et al.,* 2007), because summation of many individual extracellular EPSPs is necessary to achieve significant amplitude and such summation is only possible when many simultaneously active areas of post-synaptic dendrite are aligned in close anatomical proximity. This situation only occurs during feedback activity. Therefore the EM field theory predicts that feedback activity is necessary for consciousness.

The second anomaly inexplicable on the single neuron doctrine was the empirically observed requirement for synchronous firing of action potentials in widely separated areas of brain. How does the EM field theory predict this? It predicts this simply because roughly synchronous firing of action potentials in widely separated areas of brain is an inherent feature of feedback activity. Feedback activity involves roughly synchronous firing of action potentials first in the cell bodies of the neurons sending the feedback projections to primary sensory cortex and almost immediately afterwards at the axon terminals of those cell bodies in primary sensory cortex.

Single unit recording cannot distinguish which segment of any given neuron is firing an action potential. All it 'sees' is that an action potential has fired somewhere close to the electrode. But (and this is important) synchronous firing of temporally random action potentials at soma and axon terminal of individual feedback neurons is unlikely to be picked up by single unit recording. In order to record a single unit, an electrode has to be very close to the membrane firing the action potential. It is highly unlikely that two randomly placed electrodes in widely separated areas of brain would accidentally end up very close to both the soma and the axon terminal of any given neuron. Thus in order to see the roughly synchronous firing of cell body and axon terminal that is inherent in feedback activity, it is necessary to have multiple cell bodies (and their axon terminals) all firing more or less synchronously. Fortuitously, this is also exactly the condition necessary for the production of large population EPSPs. Multiple individual extracellular EPSPs have to summate in order to produce a large population EPSP, which means that multiple soma/axon-terminal pairs have to fire more or less synchronously. Only when multiple neighboring axon terminal/soma pairs fire more or less synchronously is it possible to record synchronous firing between the axon terminal of one neuron and the soma of another.

The reasoning here is somewhat dense, but richly rewards the effort required to follow it through.

The EM field theory of consciousness really does not only explain, but actually predict, both of the major experimental findings that constitute inexplicable anomalies on the single neuron doctrine. The EM field theory also explains a number of empirical observations that are simply outside the scope of the neuron doctrine. For example: The EM field theory proposes that particular conscious experiences are particular 3-D EM patterns – and empirically, the meaning a sensory stimulus has for an animal correlates with the tangential (intercolumnar) spatial EM pattern it evokes in sensory cortex (Barrie *et al.,* 1996; Freeman and Baird, 1987; Freeman and Grajski, 1987; Freeman and van Dijk, 1987; Pockett *et al.,* 2007).

The EM field theory proposes that the difference between sensory experiences in different sensory modalities lies in the radial (interlaminar) axis of the 3-D pattern – and empirically, it has been known for over a century that different primary sensory areas have different radial (interlaminar) cytoarchitectonic patterns (Campbell, 1905; Brodmann, 1909). The EM field theory proposes that, as well as the right spatial pattern, a conscious field has to have sufficient EM amplitude – and empirically, consciousness during binocular rivalry correlates with more intense EM patterns (Tononi *et al.,* 1998) and local minima at which EM broadband analytic power falls below 50 μV2/Hz do occur in conscious brains at intervals similar to the duration of the psychologically measured frames of consciousness (Pockett *et al.,* 2011). Further supporting evidence is laid out by Pockett (2000) and Pockett (2012).

The birth of a science of consciousness?

Kuhn's view of the transition from pre-science to science is delivered in what would now be regarded as the sexist terms typical of his times: “in the early stages of the development of any science, different men confronting the same range of phenomena ... interpret them in different ways. What is surprising, and perhaps also unique in its degree to the fields we call science, is that such initial divergences should ever largely disappear. For they do disappear to a very considerable extent and then apparently once and for all. Furthermore, their disappearance is usually caused by the triumph of one of the pre-paradigm schools, which, because of its own characteristic beliefs and preconceptions, emphasized only some special part of the too sizable and inchoate pool of information to be accepted as a paradigm a theory must seem better than its competitors, but it need not, and in fact never does, explain all the facts with which it can be confronted.” (Kuhn, 1996; pp16-17). Kuhn then goes on to talk about the sort of 'normal science' that is enabled by the emergence of the first paradigm in any field of science: “we must recognize how very limited in both scope and precision a paradigm can be at the time of its first appearance. Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute. To be more successful is not, however, to be either completely successful with a single problem or notably successful with any large number. The success of a paradigm is at the start largely a promise of success discoverable in selected and still incomplete examples. Normal science consists in the actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself. Few people who are not actually practitioners of a mature science realize how much mop-up work this sort of paradigm leaves to be done or quite how fascinating such work can prove in the execution." (Kuhn, 1996; pp23-24).

As perhaps predicted by the historical analyses of Barber (1961) and Kuhn (1962), the response of other scientists to the EM field theory of consciousness has so far been largely to ignore or reject it – at least initially. However, interest in producing a scientific account of the genesis of LFPs per se is certainly increasing (Anastassiou *et al.,* 2011; Kajikawa and Schroeder, 2011; Linden *et al.,* 2011; Buzsáki *et al.,* 2012; Destexhe and Bedard, 2012; Einevolle *et al.,* 2013; Reimann *et al.,* 2013; Hales and Pockett, 2014; Friston *et al.,* 2015) – and according to Kuhn most of the scientists who are active during a paradigm shift do not explicitly recognize at the time what is happening. So perhaps we presently are on the cusp of a recognition that electromagnetic fields are the key to the first universally accepted paradigm in a new science of consciousness. Certainly Nicolelis (Cicurel and Nicolelis, 2015) flirts with this idea, saying "phenomena like pain perception, phantom limb sensation, auditory and visual illusions … are all manifestations of analog brain processing". Equally certainly, some onward movement from the mini-paradigms underpinning respectively the dualist and the 'neuron doctrine' schools of consciousness research is needed, if only to make sense of existing data.

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