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What Is a Brain State?

Richard Brown

Philosophers have been talking about brain states for almost 50 years and as of yet no one has articulated a theoretical account of what one is. In fact this issue has received almost no attention and cognitive scientists still use meaningless phrases like 'C-fiber firing' and 'neuronal activity' when theorizing about the relation of the mind to the brain. To date when theorists do discuss brain states they usually do so in the context of making some other argument with the result being that any discussion of what brain states are has a distinct en passant *flavor. In light of this it is a goal of mine to make brain states the center of attention by providing some general discussion of them. I briefly look at the argument of Bechtel and Mundale, as I think that they expose a common misconception philosophers had about brain states early on. I then turn to briefly examining Polger's argument, as I think he offers an intuitive account of what we expect brain states to be as well as a convincing argument against a common candidate for knowledge about brain states that is currently "on the scene." I then introduce a distinction between brain states and states of the brain: Particular brain states occur against background states of the brain. I argue that brain states are patterns of synchronous neural firing, which reflects the electrical face of the brain; states of the brain are the gating and modulating of neural activity and reflect the chemical face of the brain.*

Keywords: Brain State; Identity Conditions; Identity Theory; Mind-Body Problem; Single Cell Recording

Philosophers have been talking about brain states for almost 50 years and as of yet no one has articulated a theoretical account of what one is. In fact this issue has received almost no attention and cognitive scientists still use meaningless place-holder phrases like 'C-fiber firing' and 'neuronal activity' when theorizing about the relation of the

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mind to the brain. Though the issue first arose in the context of identity theory, having such a viable theoretical account is vital to the success of cognitive science. For, whether you prefer correlation, supervenience, causation, or identity as an account of how the mind and brain relate, you will need to provide an account of what states of the brain this relation is to hold between. To date when theorists discuss brain states they usually do so in the context of making some other argument for or against one of the aforementioned mind-brain relations with the result being that any discussion of what brain states are has a distinct *en passant* flavor. In light of this it is a goal of this paper to make brain states the center of attention by providing some general discussion of them.

As an interesting historical note, Feigl (1967), Place (1956/2004), and Smart (1959/1991)—the identity theory trinity—do not use the phrase 'brain state' in their seminal works of the late 1950s. Instead they prefer the phrase 'brain process', although Feigl seems to have preferred 'neural process'. 'State' terminology comes into the literature with the second wave of identity theorists in the late 1960s (i.e., Armstrong, 1968; Lewis, 1966), although Sellars uses brain state terminology when discussing the identity theory in a lecture he delivered in 1963 (published in 1965) and he appears to attribute it to Feigl. When characterizing the identity theory he wants to discuss in his lecture he says it is the view that "brain states" are identical to "raw feels" or, "as Feigl puts it, 'raw feel' universals are identical with certain 'brain state' universals" (Sellars, 1965, p. 9). So Feigl (1967) must have used state terminology early on and he does occasionally use 'neural state' in *The "Mental" and the "Physical."* There is no clear agreement as to the correct terminology to use and often writers hedge their bets by using 'brain state/process/event'. I believe that there was a reason that 'process' terminology was preferred early on but 'state' terminology is firmly implanted in the cognitive science tradition and I propose to keep using it.

So, what are these brain states supposed to be? Feigl (1967) is clear that 'neural process' is a dummy phrase that will need to be replaced by a mature neuroscience, and I am sure that Place and Smart would agree. Feigl guesses that the neuroscience of the year 3000 might be sophisticated enough to do so. I contend that we are in a position to do so now. I believe that neuroscientists discovered the identity conditions for brain states about 15 years ago. A full thousand years ahead of schedule! However, no one has articulated the theory *as such*. Doing so is the second goal of this paper. Most philosophers take a much dimmer view of the matter. For instance, Bechtel and Mundale (1999) say, "The notion of a brain state is a philosopher's fiction" (p. 177); and more recently Thomas Polger (2004) has argued that "we don't really even have a clue what such things are" (p. 51).

My strategy will be as follows. I want to briefly look at the argument of Bechtel and Mundale, as I think that they expose a common misconception philosophers had about brain states early on. I then turn to briefly examining Polger's argument, as I think he offers an intuitive account of what we expect brain states to be as well as a convincing argument against a common candidate for knowledge about brain states which is currently "on the scene." As it is my goal to try and bring the issue of what brain states are to the forefront of the discussion, I want to try as much as possible to abstract the arguments specifically about brain states from the context in which they appear. I will mention the role that brain states play in the larger argument but shall not spend too much time pursuing them. I then introduce my candidate and argue that it meets the intuitive requirements for being a brain state. Let us now briefly go through each argument.

In their article "Multiple Realizability Revisited: Linking Cognitive and Neural States" Bechtel and Mundale (1999) argue that the intuition that psychological states are multiply realizable, in the way that Putnam suggests, loses its force when the notion of a brain state is spelled out. They begin by noting that "when Putnam characterizes brain states, he treats them as physical-chemical states of the brain" (p. 177). It is this notion of what brain states are that they call a "philosopher's fiction," and they argue that neuroscientists don't think of brain states in this way. What is important to neuroscience is function and function leads to an interest in "parts of the brain." They conclude that "a notion closer to what a neuroscientist would use is activity in the same part or conglomerate of parts [of the brain]" (p. 177). Thus, to identify brain states we need to identify functional brain areas. They then spend the rest of the article trying to show that brain areas are considered to be roughly similar across species. As long as two species have activity in the "same" area of the brain these two species exhibit the same brain state.

Putting aside the issue of multiple realizability, it seems to me that a lot of philosophers, following Putnam, did have 'physical-chemical state of the brain' in mind when they thought about brain states. It is, as Bechtel and Mundale point out, an intuitive first stab (for a philosopher) at what such things might be. Using the word 'state' tends to make one think of something static and so a natural first try would be some physical or chemical state of the brain. But I think they are right that it is a mistake to think of brain states in this way for just the reason they give: neuroscientists don't think of them that way. In fact this may have been the reason that the trinity preferred talk of 'processes' as opposed to 'states'. However, 'neuronal activity in the same part or conglomerate of parts', which they offer as the substitute, will not work as a theoretical account of what brain states are either. It is this that Polger (2004)[1] argues against.

Polger (2004) develops his argument that we (currently) don't know how to individuate brain states in the context of trying to offer an account of why some identities seem contingent (but aren't) that differs from the account Kripke (1980) gives in *Naming and Necessity*. His basic contention is that an identity will seem contingent when we can't individuate (at least) one of the proposed relata. The argument starts with explicating what sort of thing a brain state ought to be. He writes, "the philosophical idea of a brain state is that of a mechanism that will play an explanatory role in our science of the brain," (p. 52). Brain states are the kinds of things that figure in explanations of how the brain works. Polger then argues that we have no way to individuate brain states such that they could play this explanatory role.

The notion of individuation that is at play in Polger's discussion turns out to be Alan Sidelle's notion of "identity conditions." Elaborating on Sidelle's account, Polger (2004) writes: “The identity conditions for a thing or kind of thing are the boundary conditions for that thing (token-identity) or that kind of thing (type-identity). They are the conditions for *being* a particular thing or kind of thing” (p. 45). The identity conditions for a thing do not need to be exact; they need not specify the unique characteristic that distinguishes this object, or kind of object, from all others. Polger is after something much more modest. Just knowing “what sorts of properties are relevant to being a thing of such-and-such kind” (p. 47), or having some general ways of telling if something belongs to the kind in question or not, will do. It is this that we lack with respect to brain states. We can’t even generally say where the boundaries of one are or even generally say what the conditions are for being one.[2] Without such conditions Polger suggests that “some identities may appear contingent . . . because we don’t know the relevant criterion of identity to apply” (p. 49).

Again abstracting from the context in which this argument appears, Polger’s is an intuitive starting point in our search for brain states. Whatever they turn out to be they should be the kinds of things that allow us to explain how the brain works. The notion of ‘explanatory role’ is, admittedly, vague and might have to be spelled out more formally, but for the time being I think we can make do with an intuitive understanding of ‘explanation’ and the kinds of things that play roles in one. Given that we take the brain to be performing mental tasks, I would also suggest that brain states must be the types of things that will aid in our understanding the mind as brain. They should allow us to not only explain how the brain works but how it performs mental tasks. An account of what brain states were that did not allow us to understand how those things (brain states) let the brain accomplish mental tasks would not be worth anything at all.

Both these considerations appear to underlie Bechtel and Mundale’s (1999) argument. They looked at what played explanatory roles in neuroscientific theories and explicated brain states in terms of what they found. Also, the way neuroscientists identify brain areas is partly by identifying brain function and it is a general assumption that the relevant functions are mental. So both of these requirements strike me as intuitive requirements any candidate for being a brain state should meet. Also, if a brain state is to play an explanatory role in our science of the brain, we must at least be able to say, in general, where one ends and the next begins. We must at least be able to list the sorts of properties that are relevant to determining if a candidate is one or not. So, technical issues about the nature of individuation aside, we should be able to provide the kinds of loose identity conditions that Polger has in mind.

Some may be tempted at this point to think that fMRI images allow us to do this. After all, they allow us to see which areas of the brain are (differentially) active while performing cognitive tasks and if one is inclined to believe Bechtel and Mundale this is all that is needed. Seeing which parts of the brain are active just is seeing a brain state. Polger (2004) argues that thinking “that an fMRI shows how to individuate brain states would be like thinking that the identity conditions for cricket matches are to pick out only those features that, statistically, differentially occur during all the cricket games of the past year'' (p. 56). The obvious difficulty with this is that it leaves out things that may be important for cricket matches but unique (injuries, unusual plays), as well as includes things that are irrelevant to them (number of fans, snack purchasing behavior) (p. 57). The same problems hold for fMRIs: they may include information that is irrelevant and exclude information that is important but unusual. Irrelevant information may be included because fMRIs show brain areas that are statistically active during a task, while they may exclude relevant information because researchers subtract out patterns of activation observed in control images.

I would add that *at most* what we should expect from fMRI images are pictures of *where* the brain states we are interested in can be found, not pictures of the brain states themselves. They tell us that there is something in *that* area of the brain that would figure in an explanation of the task, but they don't offer us any insight into what that mechanism might be. Knowing that a particular area of the brain is (differentially) active does not allow us to explain how the brain performs the function we associate with that brain area. We need to know more about the activity. Consider an analogy: we have a simple water pump and want to know how it works. We know that pumping the handle up and down gets the water flowing but ''activity in the handle area'' does not explain how the pump works. Finding out that the handle is active every time water flows out of the pump would lead us to examining the handle with an eye towards trying to see how and why moving it pumps the water.

Granted that fMRI doesn't offer pictures of brain states, the natural conclusion would seem to be to investigate the activity they indicate. One may then think that more fine-grained imaging techniques could give us what we are looking for. And Polger is of course aware that there are other imaging techniques that are available, but he feels that none of these are able to give pictures of singular brain states that could play an explanatory role in our science of the brain. Single cell recordings, for instance, are too local. They ''don't tell us what is going on elsewhere in the brain. One or a few isolated neurons or ensembles of neurons do not a brain state make'' (2004, p. 53). It is here that Polger and I part ways. I will argue that the technique known as multi-unit recordings has identified something that is a general criterion for being a brain state and at the same time allows us to specify their boundary conditions. Before turning to that there is one last preliminary issue.

I want to introduce a distinction between ''brain states'' and ''states of the brain'' (cf. Chalmers', 2000, specific and background neural correlates of consciousness). Particular brain states occur against, and only against, background states of the brain. By 'states of the brain' I intend to include such states as being awake or being asleep; although there may turn out to be more than these two once we fully explicate what one is. However many there are, intuitively a state of the brain is the overall state that the brain is in and this strikes me to be a very different kind of thing from such local states like perceptual or motor representations. In the framework I am advocating, trying to explain brain states in terms of states of the brain (as Putnam wanted to do) is to conflate these two kinds of states. Any theoretical account of the brain and its states should explain both kinds, as it seems natural to expect each kind of state to play a distinct explanatory role. It may, of course, turn out that the distinction ultimately collapses, but absent such an argument I propose to see how far I can get with it. I now want to argue that certain chemical states are good candidates for states of the brain, whereas certain electrical states are good candidates for brain states. I will start with brain states first.

The "neuron doctrine"—the view that the neuron is the computational unit of the brain—has come under attack in the last 15 years (for an introduction to and defense of this view see Barlow, 1995). Among the leaders in this attack is Wolf Singer (1996, 2000), who has argued that the functional unit of the brain is not the neuron but rather neuronal assemblies. The intuitive idea behind Singer's work harks back to Hebb's (1949) notion of a cell assembly: "a diffuse structure comprising cells . . . capable of acting briefly as a closed system" (p. xix). The main difference is that whereas Hebb postulated "reverberation" as the crucial factor in cell assembly formation, Singer has discovered that it is synchrony.

By 'synchrony' he means not only that the cells fire in unison, but also that their firing is phase locked, i.e., they fire at the same time in the same frequency. Besides the theoretical reasons for opting for synchrony—e.g., increasing processing speed in the brain by allowing excitatory postsynaptic potentials to "trigger action potentials with minimal delay" (Singer, 2000, p. 125)—there is also a lot of experimental evidence that supports Singer's theory (reviewed in Singer, 1996). Perhaps his best evidence comes from an experiment showing that we can distinguish between two groups of neurons that are representing different objects (1996, p. 112, Figure 6.3), and thus showing that synchrony allows the brain to detect and discriminate assemblies formed at the same time without ambiguity.

In this experiment they took multi-unit recordings from four distinct orientation columns (numbered arbitrarily 1, 2, 3, and 4) in area 17 of the cat's visual cortex. A multi-unit recording involves taking single cell recordings of several spatially segregated neurons at the same time. Each orientation column was separated by 4 mm. While recording from these orientation columns, the experimenters presented visual stimuli to the animal. The stimuli used were moving light bars at various orientations (0, 112, and 135 degrees). The orientations were chosen because they matched the preferences of the orientation columns. The first stage of the experiment involved presenting the bars to the cat individually. Each bar selectively elicited synchronous activity among different orientation columns. For instance the vertical bar (0 degrees) elicited a response from orientation columns 1 and 3 that was synchronous, while eliciting no response from the other two columns. For the other two stimuli we see the same response pattern but involving columns 2 and 4 and columns 2 and 3, respectively.

Two bars (0 + 112 degrees) were then presented at the same time and arranged so that they overlapped. This elicited graded responses in all four orientation columns, which means that the stimuli activated all cells that were being recorded. But what is really interesting is that as the two objects move apart:

> The activated cells split into two independently synchronized assemblies. Cells whose feature preference match better with stimulus 1 [the vertical bar] form one synchronously active assembly, and those matching better with stimulus 2 [the bar tilted at 112 degrees] the other. Thus, although the two stimuli now evoke graded responses in all recorded groups, cells representing the same stimulus remain distinguishable because their responses have synchronized response epochs while showing no consistent correlations with responses of cells activated by different stimuli. (Singer, 1996, p. 110)

This experiment suggests a variety of things about assemblies—e.g., that neurons coding for the same stimulus will fire in synchrony relative to each other (this just is the hypothesis that synchrony is the crucial factor in cell assembly formation), and that a single neuron may participate in more than one cell assembly. Cells in columns 2 and 3 participated in multiple assemblies. For example, cells in orientation column 3 were synchronously active during presentation of the vertical bar as well as the 135-degree bar. But, as already mentioned, the really interesting thing is that it suggests that synchrony allows us to distinguish assemblies, even ones formed at the same time.

Singer theorizes that synchrony is the signature of cell assemblies as well as a general code for relatedness in the brain. Putting this hypothesis together with the generally accepted assumption that "individual neurons . . . are tuned to particular constellations of input activity" (2000, p. 123), he proposes that synchrony in a frequency can code for "a common feature or a common perceptual object or for a particular motor act" (1996, p. 106). The idea is that neurons with different preferences will fire in synchrony and this is the brain's way of knowing that all of the individual features that the individual neurons making up the assembly respond to, belong to one thing. So, while he does not put it this way, this is evidence that relative synchrony in the gamma frequency (40–100 Hz) allows us to individuate and enumerate brain states that are representing distinct objects, common features and particular motor acts.

Singer's theory is by no means established but it offers us a viable theoretical account of what brain states might be. Brain states are groups of neurons firing at the same time in the same frequency.[3] If synchrony in a frequency is a way of individuating brain states then, as was discussed earlier, there are some things we should expect this to do for us. It should be the kind of thing that gives us, in general, some conditions for whether or not a thing is a brain state or not. And it should at least allow us to roughly say what the boundaries of one are. And it does. Synchronous firing in a frequency is a general way of telling whether or not some pattern of activity is a brain state. Does it also allow us to specify boundary conditions for them? We can't do it now for purely practical reasons. Multi-unit recording, as a technique, is still in its infancy and the surgeries required for successful recordings are very difficult. But in theory synchrony would allow us to completely delimit the boundaries of brain states. This would entail "recording from a sufficiently large fraction of neurons actually participating in a particular assembly" (Singer, 2000, p. 135), which is difficult but not impossible. As multi-unit technology advances so will our understanding of the boundaries of brain states.

The next natural questions about synchrony are: Is it powerful enough to serve as a general model of what brain states are? Do brain states individuated in this way play an explanatory role in our science of the brain? Do they aid us in understanding the mind as brain? I suggest that the answer to all these questions is "yes." We have already seen the kind of explanatory work that Singer has brain states perform. He tries to explain how it is that the brain solves the so-called "binding problem" and to offer a general account of neuronal representation. These are no small matters! But one may wonder if brain states individuated by synchronous firing can play a *general* explanatory role in brain science. In order to show how powerful this model is I will present the work of Gyorgy Buzsaki and James Chrobak (Buzsaki, 1989, 1996; Buzsaki & Chrobak, 1995; Chrobak & Buzsaki, 1998) on rat memory, which, while more controversial than Singer's, is a clear example of the explanatory potential of synchrony in a frequency. Most of their results were also obtained using multi-unit recordings.

Before beginning it will be useful to briefly review the anatomy of the hippocampus and related areas (the hippocampal formation). The hippocampal formation consists of the entorhinal cortex (EC), the subiculum, and the hippocampus, which itself comprises the dentate gyrus (DG), the CA1, and CA3 regions. The entire hippocampal region receives subcortical input from the medial septum (MS). What we see in the hippocampal formation is an orderly flow of information in "a unidirectional feedforward excitatory pathway from the entorhinal cortex to dentate granuale cells to CA3 to CA1 pyramidal cells and subicular neurons and back to the deep layers of the entorhinal cortex" (Buzsaki, 1996, p. 81). The neocortex sends input to layers I–III of the entorhinal cortex and receives input from layers V–VI of the entorhinal cortex. So in essence what we have is a one-way neocortical-hippocampal information-processing loop.

With this anatomy in mind we are ready to move on. Chrobak and Buzsaki (1998) begin by noting that "the operational activity of the [hippocampal formation], just as forebrain-thalamocortical circuits, is organized around two macrostates. These macrostates are theta and sharp wave" (p. 304). Theta waves (4–12 Hz) are seen when the animal is alert and exploring its environment, while sharp waves are seen only when the animal is sleeping or otherwise inactive (p. 304). Sharp waves are slow (5–20 ms) arrhythmic population bursts that are thought to comprise the activity of 50,000–100,000 neurons firing in synchrony. They go on to say: "These patterns seem to serve companion processes. Theta synchronizes the input pathway into the hippocampus, whereas sharp waves synchronize the output pathway from the hippocampus back to neocortical structures" (p. 304). What this means is that theta and sharp wave activity each serve to put the appropriate parts of the hippocampal formation into the appropriate "context" so that specific content can be conveyed (Buzsaki, 1989). Each waveform induces a companion waveform that is thought to carry the content. In the case of theta it is our old friend gamma, and in the case of sharp waves it is fast (200 Hz) wave forms known as "ripples." The gamma activity, following Singer's work, is hypothesized to "reflect the fusion of currently perceived and stored attributes of objects and events," and may even serve to "store immediate memories" (Buzsaki & Chrobak, 1995, p. 507).

Besides inducing gamma activity in the dentate and CA3 regions of the hippocampus, theta activity seems to serve several other functions: (a) it enhances the sensitivity of appropriate neurons because "the intrinsic 'resonant' properties of hippocampus neurons are 'tuned' to theta frequency"; (b) it decreases the amount of noise in the signal "by silencing most principle cells and keeping their membrane voltage close but below the firing threshold," which allows these cells to be activated by a very small amount of entorhinal cells; and (c) theta is thought to serve as a timing device that helps the animal keep track of where it is in space, which is implied by "the observation that the discharge of hippocampal pyramidal cells advances to progressively earlier phases of the theta cycle as the rat passes through the cells spatial field" (Buzsaki, 1996, p. 83). This is to say that the gamma activity of the pyramidal cells is modulated by the theta activity and so this pattern of activity is known as "theta-modulated-gamma."

Sharp waves induce 200 Hz ripples in the CA1 and deep layers of the entorhinal cortex. The relationship between the sharp waves and the ripples is analogous to that of theta-gamma with the exception that theta activity is rhythmic and sharp waves are arrhythmic (Chrobak & Buzsaki, 1998). Thus ripples are not modulated by sharp waves as the gamma is modulated by theta. The sharp wave induced ripples are thought to: "(1) Consolidate the connectivity changes brought about by the exploratory . . . behaviors, and (2) transfer the stored representations to neocortical networks by way of the CA1 region and the entorhinal cortex" (Buzsaki, 1996, p. 89). It is via this mechanism that we get an "understanding of how [long-term potentiation] and [long-term depotentiation] can be achieved, concurrently, at distinct subsets of synapses in association with naturally occurring population dynamics" (Chrobak & Buzsaki, 1998, p. 307). If it is true that long-term potentiation and long-term depotentiation are the cellular mechanisms that underpin memory formation, storage, and transportation, then what we have just seen is an explanation of how rat memory works solely in terms of brain states!

So, in essence, the theory is as follows. While the animal is exploring its environment, which is characterized by theta-modulated-gamma, it is learning about its environment, forming new memories and perhaps comparing new information to what it already knew. It is in effect jotting down notes but much too busy to consolidate the learned behavior to long-term memory. When the animal is asleep or has some time to relax, the theta-modulated-gamma is replaced by sharp wave induced ripples, which indicates that the notes are sent for long-term storage in the neocortex. As Buzsaki puts it, "in essence the information gathered during the exploratory stage is 'replayed' during the SPW [sharp wave bursts] in reversed order" (Buzsaki, 1989, p. 564, as cited in Buzsaki, 1996, p. 89), which means that the cells of the CA1 region now fire in the reverse order from which they were originally activated. Buzsaki calls this the "two stage" model of memory (Buzsaki, 1989, p. 562). Thus each brain state has an explanatory role to play in the theory as well as aids in our understanding the mind as brain. So it looks like 'synchronized neural activity in a specific frequency' meets the requirements for replacing the dummy phrase 'brain state'. Now I turn to states of the brain.

Earlier I said that I thought certain chemical states are good candidates for being states of the brain. Won't synchrony do? I think not, because synchrony in a frequency does not distinguish between the waking and dreaming states of the brain. In their well-known article "Conscious and Pre-Conscious Processes as Seen from the Standpoint of Sleep-Waking Cycle Neurophysiology," Pare and Llinas (1995) argue that "in electrophysiological terms, waking and paradoxical sleep are fundamentally identical states" (p. 1155). What they mean is that both states are characterized by gamma synchrony. Putting this into the framework I have been developing, their point is that we see the very same kinds of brain states in both the waking and sleeping states of the brain. Granted that we think gamma synchrony is important for perceptual representation and binding, it makes sense that while dreaming we would find gamma synchrony. There is, however, something that does distinguish these two states from each other.

Kahn, Pace-Schott, and Hobson (1997) say that "even though both REM [rapid eye movement sleep] and waking states are physiologically similar in having high frequency oscillations, the chemistry underlying waking consciousness is very different from that of dreaming" (p. 24). They go on to say that: "in the awake state the brain is aminergic and cholinergic, in the REM state it is only cholinergic, and in NREM the milieu is chemically intermediate between the two. The source of the chemical differences is determined by several brainstem areas" (p. 24). An aminergic brain is one that is using neurotransmitters derived from monoamines, in this case the neuromodulators norepinephrine and serotonin, while the cholinergic brain uses the choline derived acetylcholine. The brainstem is (mostly) responsible for controlling the chemical state of the brain and so I suggest is (mostly) responsible for controlling the state of the brain.

The rhythmic, tonic bathing in chemicals that the brain stem controls serves two important functions. The first is modulating the brain between its awake and sleeping states. The second is suppressing the input from sensory systems and output to motor systems. These facts suggest a way to quantify the state of the brain. Kahn et al. (1997) call it the AIM model which "allows the state of the brain to be mapped by three parameters A, I, and M," where "A" is the amount of activation (measured by global EEG), "I" is the strength of input-output gating (measured by spinal motor neuron excitability), and "M" is neuromodulatory effects (measured by firing level of aminergic neurons in the brainstem) (p. 27). Though these measurements are all electrical, they are all diagnostic and reflect the chemical state of the brain. With values for these three parameters we are able to define a three-dimensional state space and to map various states of the brain depending on the individual values.

> For example, for high values of A the brain could be awake and interactive with the world, awake but hallucinating, or in [rapid eye movement (REM) sleep], depending upon the values of the other coordinates, I and M as follows: For high values of I which are an indication of input predominately from external sources, and for high values of M which describe the aminergic regime, the brain is awake. For low values of I which indicate an internal input . . . and low values of M which describe the cholinergic regime, the brain is in REM. For low values of I and high values of M the awake brain is hallucinating. (Kahn et al., 1997, p. 27)

This suggests that besides waking, sleeping, and dreaming we should perhaps add hallucination, coma, hypnagogic reverie (the borderline state between waking and dreaming), and perhaps even so-called "lucid dreaming" (Kahn et al., 1997, p. 28) as states of the brain. Thus the state of the brain looks to be the gating and modulating of neuronal activity, while brain states are synchronized neuronal activity in a specific frequency. Before I conclude there are a few loose ends that need to be taken care of.

First, one may wonder why it is that we see the specific frequencies that we do. It may seem to be a mystery why theta, gamma, and 200 Hz waveforms occur and play the roles that they do as opposed to some other waveforms. But there is no mystery here, as "the frequency of the oscillations depends largely on the intrinsic properties of the neurons such as their conductance and propensity to oscillate at particular frequencies, as well as on network properties, for example thalamocortical connections" (Kahn et al., 1997, p. 22). This sentiment is echoed by Steriade, McCormick, and Sejnowski (1993) who say that "despite the complexity of brain rhythms, we are beginning to understand how they arise from the intrinsic behavior of single neurons and the propensity of coupled neurons to form large-scale oscillatory states" (p. 683). So there appear to be two factors that determine the particular frequencies that make up the brain states that we see. The first are the intrinsic properties of individual neurons and the second are the network connections of these neurons.

We have already seen an example of the first of these with Buzsaki and Chrobak, who tell us that hippocampal neurons are "tuned" to theta frequency. Network connections are important because "in the intact nervous system sub-cortically derived neuromodulators maintain cortical activity in a relatively narrow operating range" (Buzsaki, 1996, p. 83), and what this brings home is the fact that brain states do not occur except against a background state of the brain. We should not expect to see brain states in the isolated hippocampus and in fact what we find out is that "the isolated hippocampus operates extremely close to the transition point between a quiescent state and an abnormally active, epileptic state" (Buzsaki, 1996, p. 83). No state of the brain, no brain states.

Second, one may wonder if it is synchrony in a frequency that is really doing the individuation of brain states. Recall that in Singer's experiment a bar was presented to the cat and in response we saw an assembly of neurons form. So one may think that it is actually the bar the cat sees that is the "authoritative" factor in individuation rather than the distinctive pattern of activity that we see in the cat's visual cortex. It seems to me that the distinctive pattern of activation is actually what is doing the individuation, as how else could we say that we see the very same brain states in both the waking and dreaming brain? This becomes even clearer when we consider the work of Buzsaki and Chrobak, who make it very clear that it is synchrony in a frequency that plays the individuating role when they say:

> our analysis of these temporal dynamics is entirely independent of the specific "sensory" phenomena that may be activating hippocampal circuits.... Knowing that neurons within the hippocampus discharge in a particular way when an animal moves through or views a specific environment, or when specific sensory stimuli are presented, or some combination of the above is important for deciphering neuronal representation. However, such information does not provide a complete description of how cortical circuits actually work. Rather, we describe the spatiotemporal relationships between large populations of neurons in anatomically interconnected circuits that allow them, working as an ensemble, to serve as a substrate for the encoding of information or the translation of information into perceptual/behavioral/motoric endpoints. The discharge of specific neurons, or ensembles, responding to specific sensory signals . . . must discharge in relation to the temporal dynamics described. (Chrobak & Buzsaki, 1998, p. 303)

This strikes me as the right way to go. Specific sensory stimuli are important for deciphering neuronal representations, and this appears to be exactly what Singer was engaged in. Once deciphered, we find that the crucial factor for the representation was a distinctive pattern of activity and that we find this pattern of activity in various states of the brain. Also, once deciphered, the theory generalizes and we see in Buzsaki and Chrobak's work an account that proceeds entirely independent of any specific sensory input. Whatever sensory representations the hippocampus is processing, it will do so using the various brain states that have been described.

Finally, what about the arguments of Bechtel and Mundale (1999) and Polger (2004)? Does anything I have argued for impact their arguments? Bechtel and Mundale were concerned about multiple realizability and their basic argument seemed to be that since brain parts are pretty much similar across species, brain states aren't multiply realizable. If brain states turn out to be patterns of synchronous neural activity, their basic intuition seems unthreatened. Synchronized neural activity is seen in humans, cats, rats and monkeys as well as by many researchers working independently and using several different methods (for reviews, see Kahn et al., 1997; Singer, 2000). It appears to be a general strategy utilized by evolved brains. So, if brain states are synchronized neural activity and we see the same synchronous activity in most brained species on earth, the intuition that brain states are multiply realized loses its intuitive appeal.

What about Polger? His account of the seeming contingency of mind-brain identity statements hinges on his claim that we do not know how to individuate brain states. If I am correct and synchrony in a frequency is such a way to individuate them, he may well wonder if his argument is in trouble. It seems to me that the answer to this is no. It may be the case that Polger can accept synchrony as the identity condition for brain states and then argue that, since this is a recent development which has not yet received universal approval or acceptance from the scientific community, it still seems to us as though these identities are contingent, in much the same way as it must have still seemed so when the molecular theory of water first appeared. So, all having the correct theory does is shape our intuitions in the appropriate way.

I have tried to develop a viable theoretical account of what brain states are that meets the requirements that one would reasonably expect a brain state to meet: they play an explanatory role in our science of the brain and they aid our understanding of the mind as brain. To do this I argued that we need to make a distinction between *brain states* and *states of the brain* and I offered what I think, in light of experimental evidence, are good candidates for each. I should not be taken as saying that any of the experimental findings presented in this paper are "proven"—they aren't. Everybody involved is quite aware that these are just theories and as with any theory they need to be subjected to the experimental method. A lot more work needs to be done in order to verify or falsify my account of what these things are. My goals have been (a) to offer some general discussion devoted specifically to brain states, and (b) to articulate an account such that it can begin to be subjected to the kind of testing and debate that ultimately determines whether any of our theories are viable or not.

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Notes

[1] I should mention that Polger and I are in basic agreement as to the metaphysics of mind. Even though we arrived at our views independently we both defend an identity theory (for my version see Brown, 2005), we both maintain that thoughts have a distinctive qualitative feel, and that having such a feel is the mark of the mental. It was reading Polger's (2004) book *Natural Minds* that awoke me from my "dogmatic daydream" and made me realize that we had a candidate for what brain states are and that, while people were using it in their theory construction, no one had come out and made it explicit.

[2] He, of course, thinks that this is something that we can have. It's just that we don't have it now, or even know what would qualify.

[3] It has recently come to my attention that Uriah Kriegel (2003) suggested synchrony as a way of individuating brain states for the same kinds of reasons presented here.

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