

PAPERS FROM THE 2008 JOINT SESSION

PHENOMENAL AND ACCESS CONSCIOUSNESS

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CONSCIOUSNESS AND COGNITIVE ACCESS

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This article concerns the interplay between two issues that involve both philosophy and neuroscience: whether the content of phenomenal consciousness is 'rich' or 'sparse', whether phenomenal consciousness goes beyond cognitive access, and how it would be possible for there to be evidence one way or the other.

I

Introduction. I will begin with an illustration that raises the issue of whether phenomenal consciousness could be so divorced from cognitive access that a subject can have an experience that he does not and cannot think about. Then I will mention a puzzle about whether there could be evidence one way or the other on that issue, and suggest a solution to the puzzle. The issue of sparse versus rich experiences will be introduced and connected to the puzzle, then some empirical evidence will be described that casts some light on the issue.¹

II

An Illustration. Nancy Kanwisher (2001) and her colleagues (Tong, Nakayama, Vaughan and Kanwisher 1998) have found impressively robust correlations between the experience of faces and activation at the bottom of the temporal lobe, usually in the subject's right hemisphere in what they call the 'fusiform face area'. One method that has been used to investigate the neural basis of face perception exploits a phenomenon known as 'binocular rivalry' (see Koch 2004, ch. 16). If a face-stimulus is presented to one eye and a house stimulus to the other, the subject experiences a face for a few sec-

¹ Most of the material presented here derives from Block (2007a, 2007b, forthcoming).

onds, then a house, then a face, etc. If the visual processing areas of the brain are examined while the face/house perceptual alternation is ongoing, much stronger alternations in sync with the percept are found in the fusiform face area than in other areas. The fusiform face area lights up when subjects are experiencing seeing a face and not when subjects are experiencing seeing a house, despite the fact that the stimuli are unchanging. The fusiform face area also lights up when subjects imagine faces (O'Craven and Kanwisher 2000).

No one would suppose that activation of the fusiform face area all by itself is sufficient for face-experience. I have never heard anyone advocate the view that if a fusiform face area were kept alive in a bottle, that activation of it would determine face-experience, or any experience at all (Kanwisher 2001). The *total* neural basis of a state with phenomenal character *C* is—all by itself—sufficient for the instantiation of *C*. The *core* neural basis of a state with phenomenal character *C* is the *part* of the total neural basis that distinguishes states with *C* from states with other phenomenal characters, for example, the experience as of a face from the experience as of a house. (The core neural basis is similar to what Semir Zeki (Zeki 2001; Zeki and Bartels 1999) has called an essential node.) So activation of the fusiform face area is a candidate for the core neural basis—not the total neural basis—for experience as of a face (see Block 2005; Chalmers 2000; Shoemaker 1981).

There is some evidence that a total neural basis of a kind of experience is the core neural basis of that kind of experience plus active reverberations involving that core neural basis and the upper brain stem including the thalamus. (The most convincing evidence is that disabling the thalamus seems the common core of what different general anesthetics do (Alkire and Miller 2005). But for some doubts, see Alkire 2008; Tononi and Koch 2008.)

Here is the illustration I have been leading up to. There is a type of brain injury which causes a syndrome known as 'visuo-spatial extinction'. If the patient sees a single object on either side, the patient can identify it, but if there are objects on both sides, the patient can identify only the one on the right and claims not to see the one on the left (Aimola Davies 2004). (With competition from the right, the subject cannot attend to the left.) However as Geraint Rees has shown in two fMRI studies of one patient (known as 'GK'), when GK claims not to see a face on the left, his fusiform face area (on the right—which is fed by the left side of space) lights up almost as

much as—and in overlapping areas involving the fusiform face area—when he reports seeing the face (Driver and Vuilleumier 2001; Rees et al. 2000, 2002).

III

The Self. Should we conclude that the fusiform face area is not the whole of the core neural basis for the experience as of a face? Or that activation of the fusiform face area is the core neural basis for the experience as of a face but that some other aspect of the total neural basis is missing in GK? However, another possibility is that GK genuinely has face experience that he doesn't know about and cannot know about. Wait—is that really a possibility? Does it even make sense to suppose that a subject could have an experience that he doesn't and can't know about? What would make it *his* experience?

The last question about GK can be answered by thinking about his visual field. What is the visual field? If you look straight ahead and hold a rod out to the side and slowly move it forward, you will be able to see it at roughly 100° from straight ahead. If you do the same coming down from the top, you will see it at roughly 60°, and if you do it from the bottom, you will see it at roughly 75°. This is a rough way of measuring the visual field. For more precision, the visual field is measured with gratings. Subjects are asked whether at a given eccentricity the grating looks like a grating or like a uniform grey field. The shape of the visual field in normal people is oval, elongated to the right and left, and slightly larger on the bottom. The Humphrey Field Analyser HFA-II-I can measure your visual field in as little as two minutes. The United Kingdom has a minimum visual field requirement for driving (60° to the side, 20° above and below); US states vary widely in their requirements (Peli and Peli 2002). Of course one can measure something without knowing what it is. But it will be too much of a digression to go further into that issue now. The point I wish to make is that it is known that visuo-spatial extinction arises from a difficulty attending to one side of space when there is a competing target of attention on the other side of space. Some may say that GK's lack of attention to the left prevents him from having any experience of a face stimulus on the left, but another possibility is that GK's lack of attention to the left

prevents his genuine experience of a face on the left from reaching the machinery of cognitive access. On that hypothesis, we can understand the face experience as *his* experience by noting that it is in *his* visual field. One could meaningfully ask, for example, whether it is the same half of his visual field in the vertical dimension as his experience on the right, or which is closer to the periphery, the one on the left or the one on the right.

In what follows, I will assume it is meaningful to suppose that GK has an experience that he does not and cannot know about, turning to the question of whether it would be possible to empirically investigate that meaningful possibility.

IV

The Methodological Puzzle. How could the issue be investigated? A natural methodology is to find the neural basis of face experience in clear cases and apply it to problem cases where for some reason there is no cognitive accessibility of the experience. However, in order to apply this methodology to the case of GK, one would first have to ask the question as to whether the activation of the fusiform face area (the core neural basis) plus its connections to the upper brain stem is sufficient for consciousness (i.e. is a total neural basis), or whether the total neural basis of face experience includes the frontal activation that underlies the cognitive access that itself underlies our ability to report our experience. But that question raises the issue of whether there can be face experience without cognitive accessibility, the very issue that we started with. So it looks as if the obvious methodology eats its own tail.

Another variant of the problem: how can we find out whether there can be conscious experience without the cognitive accessibility required for reporting conscious experience, since any evidence would have to derive from reports that themselves derive from that cognitive access?

Note that the problem cannot be solved by stipulating a definition of 'conscious'. Whatever definition one offered of this and other terms, the puzzle could be put in still other terms: for example, there would still be the question of whether what it is like to have that experience includes whatever cognitive processes underlie our ability to report the experience.

The problem does not arise in the study of, for example, water. On the basis of the study of the nature of accessible water, we can know the properties of inaccessible water, for example, in environments outside our light cone—that is, environments that are too far away in space and time for signals travelling at the speed of light to reach us. We have no problem in extrapolating from the observed to the unobserved and even unobservable in the case of water because we are antecedently certain that our cognitive access to water molecules is not part of the constitutive scientific nature of water itself. In homing in on a core neural basis of phenomenal consciousness, we have a choice about whether or not to include *within* the core neural basis the aspects of those neurological states that underlie reportability. If we do, then unreportable phenomenally conscious states are ruled out; if we do not, unreportable phenomenally conscious states are allowed. Few scientifically minded people in the twenty-first century would suppose that water molecules are partly constituted by our cognitive access to them (Boghossian 2006), but few would be sure whether phenomenal consciousness is or is not partly constituted by cognitive access to it. It is this asymmetry that is at the root of the Methodological Puzzle of phenomenal consciousness.

V

The Solution. The solution to the Methodological Puzzle is the method of inference to the best explanation, that is, the approach of looking for the framework that makes the most sense of all the data, not just reports (Harman 1965; Peirce 1903, vol. V, p. 171).

The reader may feel that I have already canvassed inference to the best explanation and that it did not help. Recall that I mentioned that the best explanation of all the data about observed water can give us knowledge of unobserved—even unobservable—water. I said that this approach does not apply straightforwardly to consciousness. We are antecedently certain that our access to information about water molecules is not part of the natural kind that underlies water molecules themselves. But we are not certain (antecedently or otherwise) about whether our cognitive access to our own consciousness is partly constitutive of the consciousness. Without antecedent knowledge of this—according to the reasoning that

leads to the Methodological Puzzle—we cannot know whether whatever makes a phenomenal state cognitively inaccessible also renders it non-phenomenal.

Here is the fallacy in that argument: the best theory of *all* the data may be one that lumps consciousness together with water molecules as something whose constitutive nature does not include cognitive access to it. To hold otherwise is to suppose—mistakenly—that there are antecedent views—or uncertainties, in this case—that are not up for grabs.

Perhaps an analogy will help. It might seem, offhand, that it is impossible to know the extent of errors of measurement, for any measurement of errors of measurement would have to be derived from measurement itself. But we can build models of the sources of measurement error and test them, and if necessary we can build models of the error in the first-level models, and so on, stopping when we get a good predictive fit. For example, the diameter of the moon can be measured repeatedly by a number of different techniques, the results of which will inevitably vary about a mean. But perhaps the diameter of the moon is itself varying? That can be investigated by simultaneously building models of sources of variation in the diameter itself (for example, the temperature of the moon) and models of error in the various methods of measurement. Those models contain assumptions which can themselves be further tested.

The puzzle of how it is possible to use measurement itself to understand errors of measurement is not a deep puzzle. As soon as one sees the answer, the problem of principle falls away. I do not believe that the same is true for the Methodological Puzzle. One reason is the famous ‘explanatory gap’ (Levine 1983; Nagel 1974). There may be reasonable doubt whether the method of inference to the best explanation can apply in the face of the explanatory gap. A second point is that with the demise of verificationism (Uebel 2006), few would think that the nature of a physical magnitude such as length or mass is constitutively tied to our measurement procedures. The mass of the moon is what it is independently of our methods of ascertaining what it is. But verificationism in the case of consciousness is much more tempting—see Dan Dennett’s ‘first person operationalism’ (Dennett 1991) for a case in point.

VI

Rich versus Sparse Experience. As I explained at the outset, this paper concerns the interplay between two issues, the Methodological Puzzle just canvassed and the issue of ‘rich’ versus ‘sparse’ conscious contents, the source of which, in current controversies, is a phenomenon called ‘change blindness’. As illustrated in figure 1, a drawing or photograph is presented briefly to subjects, followed by a blank, followed sometimes by an identical photograph but other times by a similar but not identical photograph, followed by another blank. Then the cycle starts over.

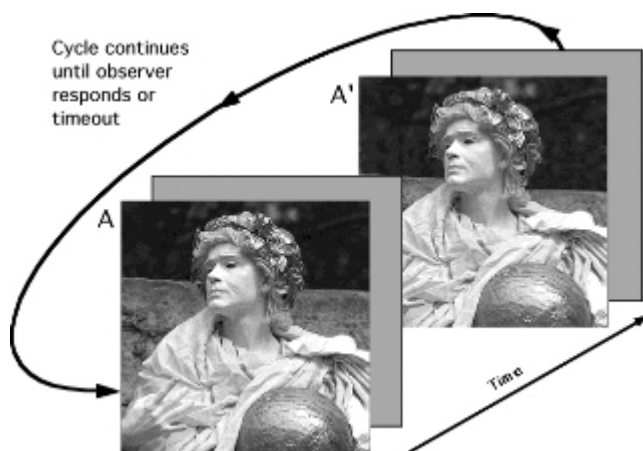


Figure 1. The ‘change blindness’ paradigm. A picture is presented briefly, then a blank screen, then another picture which may or may not differ in some respect from the first one, then another blank, then the cycle starts over. The result is that subjects are surprisingly unable to tell what changes between the two pictures, even after watching the cycle many times. I am grateful to Ron Rensink for redrawing this figure and for permission to use it.

You can experience something of the phenomenon if you look at figures 2 and 3 without looking at them side by side. When the two photographs differ, they usually differ in one object that changes colour, shape or position or appears or disappears. The surprising result is that subjects are often unaware of the difference between the two pictures, even when the changed region takes up a good deal of the photographic real estate. Even with fifty repetitions of

the same change over and over again, people are often unaware of the change. It is widely agreed that the phenomenon is an attentional one. The items that change without detection have been shown to be items that the subjects do not attend to. But the controversial question is whether the explanation of the surprising inability of subjects to notice the change is due to inattentional blindness or inattentional inaccessibility.



Figure 2. Compare this with figure 3 without looking at the two figures side by side. There is a difference between the two pictures that can be hard to be aware of, a fact that motivates the appellation (a misnomer in my view) ‘change blindness’. I am grateful to Ron Rensink for supplying this figure.

The idea of the inattentional blindness view of the phenomenon is that subjects do not actually see the features that change (Noë 2004, O’Regan and Noë 2001). By contrast, the inattentional inaccessibility view (Block 2001) says that subjects may see the features that change, but fail to notice the difference, because although much of the detail in each picture is phenomenally registered, it is not conceptualized at a level that allows cognitive access to the difference. As Fred Dretske (2004) has noted, the difference between the two stimuli in a change blindness experiment can be one object that appears or disappears, and one can be aware of that object that consti-

tutes the difference without noticing that there is a difference. As will be explained below, there appears to be a limit in 'working memory' of about four items. (The existence of such a limit is not part of the dispute between the proponents of inattentional blindness and inattentional inaccessibility.) Proponents of inattentional blindness typically take this limit to show that despite the appearance of a rich visual world, one only sees about four items at a time, and thus that conscious experience is surprisingly 'sparse'. The impression that one takes in many more things is supposed to derive from a 'refrigerator light illusion' in which subjects mistake the easy accessibility of all sorts of detail for actually seeing that detail (O'Regan and Noë 2001). (The analogy invokes a fool who thinks that the refrigerator light is always on because it is on when he looks.) Dehaene and his colleagues put the point as follows:

The change blindness paradigm demonstrates this 'discrepancy between what we see and what we think we see' (Simons and Ambinder 2005). In this paradigm, viewers who claim to perceive an entire visual scene fail to notice when an important element of the scene changes. This suggests that, at any given time, very little of the scene is actually consciously processed. Interestingly, changes that attract attention or occur at an attended location are immediately detected. Thus, the illusion of seeing may arise because viewers know that they can, at will, orient attention to any location and obtain conscious information from it. (Dehaene et al. 2006, p. 210)

The upshot is a disagreement about whether perceptual consciousness is 'rich' or 'sparse'. The advocates of 'sparse' visual experience argue that the limits of working memory are the limits of experience, whereas the advocates of rich experience can allow that experience 'overflows' cognitive accessibility.

Now we can get a glimpse of the relevance of the sparse/rich issue to the Methodological Puzzle. The argument of this paper is that we already have some evidence for the 'rich' view, and the upshot is that the capacity of the perceptual consciousness system is much larger than the capacity of the 'working memory' system that underlies the cognitive access that itself underlies reportability. And this difference in capacity shows that consciousness and cognitive access are to some extent based in different systems with different properties.

I have mentioned the results about working memory, but I will now address that issue in more detail.



Figure 3. Compare this with figure 2 without looking at the two figures side by side. There is a difference that can be hard to see. I am grateful to Ron Rensink for supplying this figure.

VII

Working Memory. At a neural level, we can distinguish between memory that is coded in the active firing of neurons—and ceases when that neuronal firing ceases—and structural memory that depends on changes in the neural hardware itself, for example, change in strength of synapses. The active memory—which is active in the sense that it has to be actively maintained—is sometimes described as ‘short term’—a misdescription, since it lasts as long as active firing lasts, which need not be a short time if the subject is actively rehearsing. The term ‘working memory’ is used differently by different people (Cowan 2007), but in this article, the active memory buffer is called ‘working memory’.

You may have heard of a famous paper by George Miller called ‘The Magical Number Seven, Plus or Minus Two: Some Limits on Our Capacity for Processing Information’ (Miller 1956). Although Miller was more circumspect, this paper has been widely cited as a

manifesto for the view that there is a single active memory system in the brain that has a capacity of seven plus or minus two ‘items’. What is an item? There are some experimental results that fill this notion in a bit. For example, Huntley-Fenner et al. (2002) showed that infants’ visual object tracking system—which, there is some reason to believe, makes use of working memory representations—does not track piles of sand that are poured, but does track them if they are rigid. One constraint on what an item might be comes from some experiments that show that although we can remember only about four of them, we can also remember up to four features of each one. Luck and Vogel asked subjects to detect changes in a task somewhat similar to the Landman et al. task already mentioned. They found that subjects could detect changes in four features (colour, orientation, size, and the presence or absence of a gap in a figure) without being significantly less accurate than if they were asked to detect only one feature (Luck and Vogel 1997; Vogel, Woodman and Luck 2001).

In the fifty years since Miller’s paper, reasons have emerged to question whether there really is a single active memory system, as opposed to a small number of linked systems connected to separate modalities and perhaps separate modules—for example, language. For example, some brain injuries damage verbal working memory but not spatial working memory (Basso, Speinler, Vallar and Zanolio 1982), and others have the opposite effect (Hanley, Young and Pearson 1991). And evidence has accumulated that the capacity of these working memories—especially visual working memory—is actually lower than seven items (Cowan 2001; Cowan, Morey and Chen 2006).

The suggestion of seven items was originally made plausible by such facts as that if you read people lists of digits, words or letters, subjects can repeat back about seven of them. Of course, you can repeat more items if they can be ‘chunked’. Few Americans will have trouble holding the following nine letters in mind: FBICIAIRS—because they can be chunked into three acronyms.

More relevantly to our discussion, visual working memory experiments also come up with capacities in the vicinity of four—or less than four items. (For work that suggests less than four, see McElree 2006.) Whether there is one working memory system that is used in all modalities or overlapping systems that differ to some extent between modalities, this result is what is relevant to the experiments

discussed above. Another paradigm that comes up with the same number involves the number of items that people—and monkeys—can effortlessly keep track of. For example, at a rhesus macaque monkey colony on a small island off Puerto Rico, Marc Hauser and his colleagues did the following experiment. Two experimenters find a monkey relaxing on its own. Each experimenter has a small bucket and a pocket full of apple slices. The experimenters put down the buckets and one at a time, they conspicuously place a small number of slices in each bucket. Then they withdraw and check which bucket the monkey goes to in order to get the apple slices. The result is that for numbers of slices equal to or smaller than four, the monkeys overwhelmingly choose the bucket with more slices. But if either bucket has more than four, the monkeys choose at random. In particular, monkeys chose the greater number in comparison of one versus two, two versus three, and three versus four, but chose at random in cases of four versus five, four versus six, four versus eight and, amazingly, three versus eight. The comparison of the three versus four case (where monkeys reliably chose more) and the three versus eight case (where they chose at random) is especially telling (Hauser, Carey and Hauser 2000). The eight apple slices simply overflowed working memory storage. Infant humans show similar results, although typically with a limit more in the vicinity of three rather than four (Feigenson, Carey and Hauser 2002). Using Graham crackers instead of apple slices, Feigenson et al. found that infants would crawl to the bucket with more crackers in the cases of one versus two and two versus three, but were at chance in the case of one versus four. Again, four crackers overflows memory storage. In one interesting variant, infants are shown a closed container into which the experimenter—again conspicuously—inserts a small number of desirable objects (for example, M&Ms). If the number of M&Ms is one, two or three, the infant continues to reach into the container until all are removed, but if the number is more than three, infants reach into the container just once (Feigenson and Carey 2003).

I should mention that the picture of working memory that I have been describing is a topic of active dispute. The two opposed models are the ‘slot’ model that I am relying on, in which working memory stores a small number of fixed-resolution representations, and a model according to which working memory is a pool of resources that can be allocated to many lower resolution representations. The

'slot' model that I have been describing is described even by proponents of the 'pool' model (Bays and Husain 2008) as the 'dominant model'. A recent article in *Nature* (Zhang and Luck 2008) purported to settle the issue. According to an accompanying editorial, Zhang and Luck's paper 'resolves the matter in favour of the "high resolution" option'. However, a duelling article in *Science* (Bays and Husain 2008) purports to show the opposite, explaining away the Zhang and Luck result.

One possible resolution is the argument of Xu and Chun (2006) that there are *two different working memory systems* with somewhat different brain bases, one of which fits the 'slot' model and the other of which fits the 'pool' model. The system that fits the slot model is spatial and has a limit of four spatial locations or objects at four different spatial locations, independently of complexity. The system that fits the pool model is not spatially based.

This dispute raises the difficult methodological issue of what philosophers are supposed to think when the scientists disagree and whether it makes sense for a philosopher to rely on a controversial scientific claim. My approach is to use scientific conclusions that are well confirmed and widely accepted by scientists even if there is some disagreement. Of course, a single experiment can sometimes turn the tables. The effect on the reasoning of this paper if the tables are turned would be a retreat from the claim that the Methodological Puzzle has actually been empirically resolved to showing how empirical data could in principle resolve it.

VIII

The Global Workspace Model. I will be assuming the global workspace model of cognitive access. The account presupposes a neural network approach in which there is competition among neural coalitions involving both frontal and sensory areas (Koch 2004), the winning coalitions being conscious. Sensory stimulation causes activations in sensory areas in the back of the head that compete with each other to form dominant coalitions (indicated by dark elements in the outer ring in figure 4). Some of these dominant coalitions trigger central reverberations via long range connections to frontal cortex, setting up activations that help to maintain both the central and peripheral activations. The idea that some brain areas control acti-

vations and reactivations in other areas is now ubiquitous in neuroscience (Damasio and Meyer 2008), and one instance of reciprocal control is one in which workspace networks in frontal areas control activations in sensory and spatial areas (Curtis and D'Esposito 2003). It is useful in thinking about the account to distinguish between suppliers and consumers of representations. Perceptual systems supply representations that are consumed by mechanisms of reporting, reasoning, evaluating, deciding, and remembering, which themselves produce representations that are further consumed by the same set of mechanisms. Once perceptual information is 'globally broadcast' in the frontal cortex this way, it is available to all cognitive mechanisms without further processing.

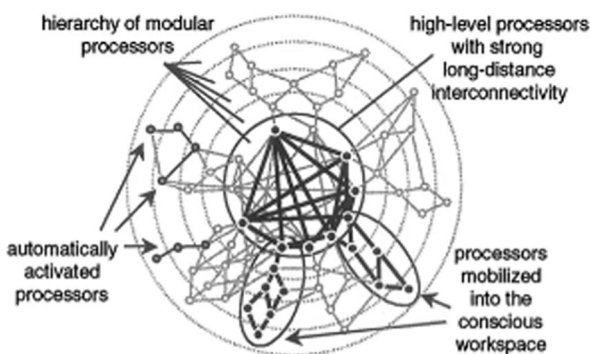


Figure 4. Schematic diagram of the global workspace. Sensory activations in the back of the brain are symbolized by dots and lines in the outer ring. Dominant sensory neural coalitions (dark lines and dots) compete with one another to trigger reverberatory activity in the global workspace (located in frontal areas) in the centre of the diagram. The reverberatory activity in turn maintains the peripheral excitation until a new dominant coalition wins out. I am grateful to Stanislas Dehaene for permission to use this drawing.

One important feature of the global workspace model for philosophical purposes is that it makes it very easy to see a problematic ambiguity in the term 'accessibility'. What is it for a representation to be cognitively accessible? Is it a matter of actually being in the global workspace, or is it a matter of potentially being in the global workspace? Merely potentially in the global workspace would be too weak for a functional notion that has any hope of being identi-

fied with consciousness, as endorsed by many functionalists these days (for example, Dehaene and Changeux 2004; Dehaene and Naccache 2001; Dennett 2001). For example, a completely unconscious representation can be potentially in the global workspace because it would be in if attention were shifted slightly.

An experimental demonstration that shifting attention affects phenomenal consciousness to a degree sufficient to change a sub-threshold stimulus into a supra-threshold stimulus is to be found in a series of papers by Marisa Carrasco (Carrasco 2007; Carrasco, Ling and Read 2004) in which she asked subjects to report the orientation of the one of a pair of gratings that had the higher contrast. She presented an attention-attracting dot on one side of the screen or the other slightly before the pair of gratings. She showed that attention attracted to one side or the other could make a grating that was lower in contrast than the comparison seem higher in contrast. In subsequent work, Carrasco (2007) has been able to show precisely measurable effects of attentional shifts on other phenomenally conscious qualities, for example, perceived colour saturation.

Dispositional notions like accessibility are notoriously flexible, depending on context. In this article I will require not just potential broadcasting for accessibility but actual global broadcasting. Actual global broadcasting does not itself require that any 'consuming' machinery actually process the broadcast representation, so it is a notion involving potentiality.

Jesse Prinz (2007*b*) distinguishes between three categories of representations: those that are not broadcast to working memory, those that are broadcast to working memory, and those that are encoded in working memory. He links consciousness to broadcasting. The picture he uses is one of a TV station broadcasting operation in which a movie can be waiting to be broadcast, actually broadcast but not encoded by any receiver, and encoded. But as attention to the model of figure 4 shows, there is no distinction between a representation having been broadcast and being encoded. What it is for a representation to be broadcast is for it to be part of a reverberating circuit including frontal areas and perceptual areas, and if that happens it is automatically encoded; and further, according to the model, there is no other kind of encoding in working memory.

IX

Overflow. In this section, I will present the key empirical datum for my argument, an experiment by Landman et al. (2003). The subject is shown eight rectangles for half a second (or, in some versions, one second) as in A of figure 5. There is a dot in the middle which the subject is supposed to fixate, that is, keep looking at. The array is replaced by a blank screen for a variable period. Then another array appears in which a line points to one of the objects—which may or may not have changed orientation. In the example shown in figure 5, there is an orientation change. Using statistical procedures that correct for guessing, Landman et al. computed a standard capacity measure showing how many rectangles the subject is able to track. In A, subjects show a capacity of four items. Thus, the subjects are able to deploy working memory so as to access only half of the rectangles despite the fact that subjects reported seeing all or almost all of the rectangles. This is a classic ‘change blindness’ result. In B, the indicator of the rectangle that may or may not change comes on in the first panel. Not surprisingly, subjects can get almost all right: their capacity measure is almost eight. The crucial manipulation is the last one: the indicator comes on during the blank after the original rectangles have gone off. If the subjects are continuing to maintain a visual representation of the whole array and reading their answers off of it—as subjects say they are doing—the difference between C and B will be small, and that is in fact what is observed. The capacity measure in C is between six and seven for up to 1.5 seconds after the first stimulus has been turned off, suggesting that subjects are able to maintain a visual representation of the rectangles. This backs up what the subjects say and what William James said about the phenomenal consciousness involved in this kind of case. What is both conscious and accessible is *that there is a circle of rectangles*. What is conscious but in a sense not accessible, is *all the specific shapes* of the rectangles.

There is some reason to think that the longest lasting visual representations of this sort come with practice and when subjects learn to ‘see (and not look)’. Sligte, Lamme and Scholte (2006) did a more elaborate version of the Landman experiment, finding long persistences, up to four seconds with lots of practice and instructions to relax and let it happen. Others (Long 1980; Yang 1999) have noted that practice in partial report paradigms makes a big difference in subjects’ ability to make the experience last.

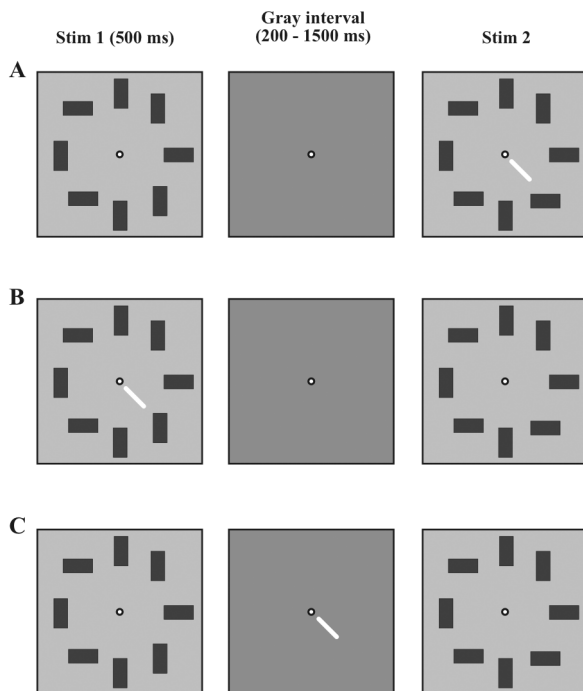


Figure 5. Landman et al.'s change blindness paradigm. The rectangles are displayed here as line drawings, but the actual stimuli were defined by textures. From Lamme (2003). I am grateful to Victor Lamme for providing the drawing, and to Elsevier for permission to reprint it.

The main upshot of the Landman and Sligte experiments (at least on the surface—debunking explanations will be considered later) is that the subject has persisting experiences as of more specific shapes than can be brought under the concepts required to report or compare those specific shapes with others. They can all be brought under the concept ‘rectangle’ in the Landman experiment or ‘letter’ in the Sperling experiment, but not the specific orientation-concept which would be required to make the comparisons in Landman or to report the letters in Sperling.

Why are subjects able to gain access to so few of the items that they see in the first condition of the Landman experiment (i.e. as described in A of figure 5)? I am suggesting that the explanation is that the ‘capacity’ of the visual phenomenal memory system, is greater

than that of the working memory buffer that governs reporting. The capacity of visual phenomenal memory could be said to be at least eight to thirty-two objects—at any rate for stimuli of the sort used in the described experiments. This is suggested by subjects' reports that they can see all or almost all eight items in the Landman experiment and up to thirty-two items in the Sligte experiment in the presented arrays, and by the experimental manipulations just mentioned in which subjects can give reports which exhibit the subjects' apprehension of all or almost all the items. The lines of converging evidence mentioned above suggest that the 'working memory' system—the 'global workspace'—has a capacity of about four items (or less) in adult humans and monkeys and three (or less) in infants.

Given that the capacities of these systems are different, it follows that the systems themselves cannot completely coincide, and in particular, there must be something more to the consciousness system that explains its greater capacity. So it is indeed possible to investigate the difference between consciousness and cognitive accessibility. This is the basic empirical argument of the paper.

There are two crucial features of the Landman phenomenon for my purposes. The most obvious one is the high capacity phenomenal consciousness just mentioned, but it is also important that this high capacity phenomenal consciousness occurs after the stimulus is long gone, because it is no news that there are lots of things in the world and high capacity representations in the retina! A crucial part of my argument—left out here for reasons of space—is to detail the evidence that the high capacity depends most proximally on conscious representations themselves. See Block (2007a, 2007b).

X

Generic versus Specific Phenomenal Consciousness. A key concept in the argument alluded to above but not explicitly is the distinction between generic and specific contents of phenomenal consciousness (Block 2007a, 2007b; Burge 2007; Byrne, Hilbert and Siegel 2007; Grush 2007; Levine 2007; Papineau 2007). In the Landman experiment, the relevant generic phenomenal content would be the phenomenal presentation *that there is* a circle of rectangles. The relevant specific phenomenal content would be a phenomenal presentation that specifies for each of the rectangles (or anyway, most of them)

whether it is horizontal or vertical. My argument was that before the cue, there is specific phenomenal content for all or almost all items. (I also think there is generic phenomenal content before the cue, but that does not figure in the argument.) This specific phenomenal content is what justifies the claim that the capacity of the phenomenal system is more than four, whereas the capacity of the access system is four or less, and thus that the two systems cannot completely coincide. But critics (Byrne et al. 2007; Papineau 2007) have challenged the premiss that there are more than four items of specific phenomenal content before the cue. It is important to recognize that the objectors have to agree that before the cue there are *specific* (not just generic) visual representations of all or almost all of the eight to thirty-two items. There *have* to be such specific representations, given that any location can be cued with high accuracy of response. The locus of controversy is *whether those specific representations are phenomenally conscious*.

A number of critics (Byrne et al. 2007; Naccache and Dehaene 2007; van Gulick 2007) have alleged that subjects are subject to a kind of illusion that they have rich specific phenomenal consciousness. The illusion could be said to be a product of two factors, that the subjects have generic phenomenal content (to the effect that there are five to ten rectangles arranged in a circle) and that when they attend to a specific location, they find that they have specific phenomenal content for that location. According to the objection, the 'refrigerator light illusion' leads subjects to suppose that they have rich specific phenomenal content even though their specific phenomenal content is really sparse.

What shows that there is rich specific phenomenally conscious content (in addition to the agreed-on generic phenomenally conscious content)? First, subjects consistently say that they are simply basing their answers on their visual experience. In the Sligte version of the Landman experiment, the visual experiences last for four to five seconds, and it is plausible that subjects would be likely to be accurate about something they see for such a long period. Second, subjects are attending to arrays in full view in good viewing conditions to stimuli that last between half a second and a second, more than enough time for specific phenomenal content. Third, it is not easy to understand what the alternative is. Is the idea that there is *literally zero specific phenomenally conscious content of the rectangles before the cue*, and then *one item of specific phenomenally conscious content after the cue*, as indicated in figure 6? It does seem as

if the phenomenal content were as depicted in figure 6, subjects could report that, which they very much do not do. Or perhaps the phenomenal content is as depicted in figure 7. Before the cue, a random collection of half the rectangles are specifically conscious. However, that random collection could be expected to contain the item that is cued only half the time. So half the time, one of the specifically conscious items would have to disappear, replaced by the cued item as depicted on the right side of figure 7. I have never heard of any subject reporting any such thing in any of the myriad experiments of this type. There are other hypotheses as well, but I can't think of any—even ones that are not easily picturable—that fit at all well with reported phenomenal consciousness of subjects. A better account is that subjects should be believed in saying that they have experiences of all or most of the rectangles, that is, specific phenomenal contents. Fourth, there is evidence that the subjects' representations are a kind of mental image. In one variation, Landman et al. did the same experiment as before but changed the size of the rectangles rather than the orientation, and then in a final experi-

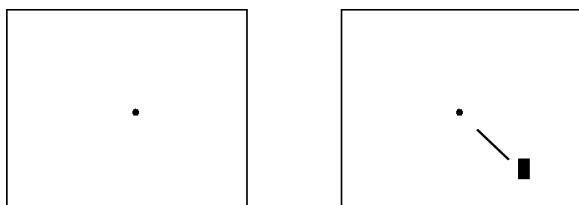


Figure 6. Hypothesis 1 concerning specific phenomenal consciousness in the Landman et al. experiment.

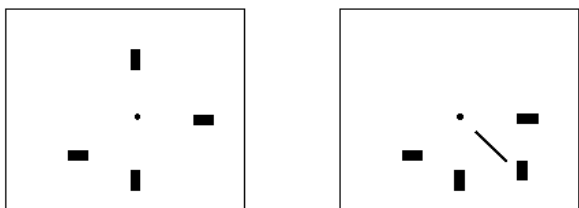


Figure 7. Hypothesis 2 concerning specific phenomenal consciousness in the Landman et al. experiment.

ment changed either the size *or* the orientation. The interesting result is that subjects were no worse at detecting changes in either orientation or size than they were at detecting changes in size alone. That suggests that the subjects have a representation of the rectangles that combines size and orientation from which either one can be recovered with no loss due to the dual task, again backing up subjects' reports that indicate a kind of visual imagery.

Here is a fifth consideration: Vincent Di Lollo (1980) originated a paradigm using a five-by-five grid in which all but one of the squares is filled with a dot. Subjects see a partial grid with twelve of the dots filled in, then after a delay, another partial grid with a different twelve dots filled in. The subjects' task is to report which square has a missing dot, something they can do easily if they have a visual impression as of the whole matrix of dots. Loftus and Irwin (1998) show that subjects' ability to do the task correlates nearly perfectly with their phenomenological judgements of whether there appears to be a whole matrix rather than two partial matrices. Brockmole, Wang and Irwin (2002) delayed the appearance of the second partial grid for as long as five seconds, telling subjects that a good strategy was to 'imagine the dots still being present after they disappeared' (2002, p. 317). Subjects had to remember the first partial grid and superimpose it on the second partial grid (which was still on the screen) to see where the missing dot was. The subjects' memory capacity for the twelve dots in the first grid can be computed by the type of errors made. When the delay between the first and second partial grids is 100 ms, the subjects' retention capacity falls from twelve to 4.1 of the dots in the first partial grid (see figure 8). The striking result was that with delays over 100 ms, subjects' capacity then *increased*, asymptoting at a delay of about 1.5 seconds, at which time their capacity was more than ten of twelve dots, and the capacity *stayed that high for delays up to four to five seconds*. (See the dotted line in figure 8, which represents the percentage of dots remembered from the first partial grid.) Independent estimates of the time to generate a mental image by Kosslyn (Kosslyn, Thompson and Ganis 2006) are between one and two seconds, and the authors argue that the subjects were following instructions, generating a visual image of the first array, and integrating that visual image with the percept of the second array. This result constitutes converging evidence for high capacity specific phenomenal consciousness: the subjects say they have an image, and what they say is

confirmed by their performance. The upshot is that there is a completely different paradigm in which the evidence favours high capacity specific phenomenal consciousness.

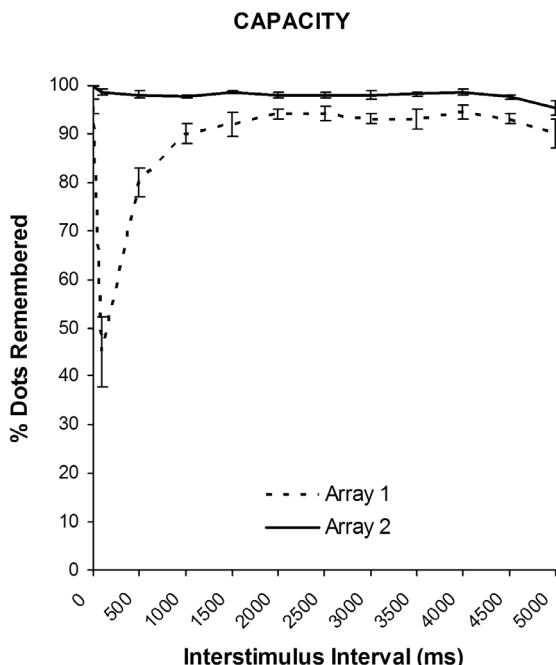


Figure 8. In Brockmole et al.'s experiment, a twelve-dot partial grid is presented briefly, then a period of time elapses (the interstimulus interval), then a second twelve-dot partial grid is presented and stays on the screen. The subjects are asked to report the missing dot. This graph shows that for interstimulus intervals between one and five seconds, subjects are very accurate in reporting the missing dot. From Brockmole et al. (2002). With permission of the American Psychological Association. I am grateful to James Brockmole for providing me with a redrawn figure.

Of course, my eventual conclusion is that people can be dramatically mistaken about their own experience, so some critics may use that as a reason to say that subjects in the Landman and Sligte and Brockmole experiments are dramatically mistaken about the contents of their own phenomenal consciousness. But there is no reason to distrust these subjects, whereas the subject GK, mentioned earlier, does have brain damage that prevents attention to the left side of space

when there is a competing stimulus on the right, so there is a real question as to whether he might see something on the left that he cannot report because of lack of attention. Further, in visuo-spatial extinction generally, the subject's claim not to see the object on the left is often combined with the ability to make comparisons between the thing on the left and the thing on the right (Verfaellie, Milberg, McGlinchey-Berroth, Grande and D'Esposito 1995; Volpe, LeDoux and Gazzaniga 1979). For example, when asked to make a 'same/different' comparison between objects on the left and right, two of the subjects in the Volpe et al. experiment asserted 'that the task was "silly"', since there was no stimulus on the left, but these subjects were nonetheless more than 88% correct on the same/different judgements.

XI

Panpsychic Disaster. A number of commentators argued that once you give up the special authority of reports, you will have no way of avoiding attributing consciousness to lampposts. Papineau (2007) notes that I regard some states as uncontroversially unconscious and wonders 'what makes a state "uncontroversially unconscious" if it is not that subjects tell us so'. He argues that once we allow that a state can be conscious even though normal subjects systematically deny it, there may be no uncontroversially unconscious states. Prinz (2007a) says, 'Block must either concede that reports are authoritative or deny that we can rule out the possibility of conscious states in V1, the LGN, and the retinae.'

However, it is obvious that reports fail to be authoritative. In Anton's syndrome, subjects are blind but think and report that they see. More generally, anosognosics deny their perceptual and motor disabilities, making all sorts of false reports about their own experience. Generally, in cognitive neuroscience, aspects of phenomena found in brain-damaged patients can be produced in some degree in normal subjects with stimuli that are degraded or speeded or in other stressful conditions. Introspective reports do have a certain priority: we have no choice but to start with reports in investigating consciousness. But reports can be overridden on the basis of theory—that is itself based on other reports. One very notable form of empirical evidence that can conflict with reports is evidence about subjects' deci-

sion process evaluated according to signal detection theory (Snodgrass and Lepisto 2007). The signal detection theory perspective dictates that there is no such thing as a raw report uncontaminated by decision processes.

XII

Conclusion. I started with a methodological puzzle which in one form is: how could we possibly find out whether there can be conscious experience without the cognitive accessibility required for reporting conscious experience, since any evidence would have to derive from reports that themselves derive from that cognitive accessibility? I then argued that the method of inference to the best explanation can in principle allow for evidence that separates phenomenal consciousness from cognitive accessibility, since the overall model in which phenomenal consciousness goes beyond cognitive accessibility might turn out to be better supported than the alternatives. I then described the controversy over rich versus sparse phenomenal consciousness and argued that the sparse phenomenal consciousness point of view fits naturally with the view that phenomenal consciousness does not go beyond cognitive accessibility. Finally, I presented evidence for a point of view that combines rich phenomenal consciousness with a picture of phenomenal consciousness as depending on at least somewhat different machinery from cognitive accessibility. Of course, this does not show that there can be phenomenal consciousness without cognitive accessibility as entertained in the case of patient GK, but it does take a step in that direction.

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