Blockheads!

Essays on Ned Block's Philosophy of Mind and Consciousness

Edited by Adam Pautz and Daniel Stoljar

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12 Empirical Science Meets Higher-Order Views of Consciousness: Reply to Hakwan Lau and Richard Brown

Ned Block

Much of the battle over first-order versus higher-order theories of conscious experience concerns intuitions concerning the word "consciousness"—or more charitably, the concept of consciousness. First-order perceptual representations are representations of the world, whereas higher-order representations are representations of one's representations, including but not limited to first-order representations. First-order theories of the sort I advocate locate conscious perception in interacting representations of the world and perhaps interactions between representations of the world and subcortical activations that do not represent anything.

Advocates of higher-order theories focus on the founding intuition that a state that one is in no way conscious of being in is not a conscious state. In recent years, I have suggested that first-order theories can accommodate this intuition via a same-order account in which conscious states include a kind of awareness of themselves. I have also taken seriously the idea that the founding intuition is too superficial to give us any insight. We say that a song is something one sings, that a dance is something one dances, and a jump is something one jumps. These relations are superficial in that they do not provide any substantive insight into what singing, dancing, or jumping is. Similarly, saying a conscious state is one we are conscious of is a superficial relation.

In some past writings I have focused on a bizarre feature of higher-order views, such as those of Rosenthal (2005) and Weisberg (2010), in which a conscious visual experience as of the green color of the grass on my lawn can occur without any first-order visual representation of the green color of the grass (Block 2011a, 2011b). These authors are forced into this position by consideration of cases in which first-order and higher-order representations disagree and cases in which there are no first-order representations for the higher-order representations to be about.

I have found discussions on this topic rather unproductive—even more so than most intuition-based disagreements in philosophy. So I welcome that Hakwan Lau and Richard Brown have focused their discussion on actual cases that raise genuine empirical issues. By discussion of these and other real cases, we stand a better chance of adjudicating between first-order and higher-order theories.

Lau and Brown describe three cases for which they think there is conscious perception (or at least perception-like experience) but no relevant first-order perceptual representation is present—or where there may be a first-order representation, but it is too weak to account for the conscious perception. I will go through these cases one by one.

Rare Charles Bonnet Syndrome

Lau and Brown define "Rare Charles Bonnet Syndrome" as a syndrome of visual hallucination with destruction of the first visual cortical area, V1. In the cases they describe, *partial* damage to early visual cortex including area V1 is accompanied by vivid visual hallucinations. These hallucinations often occupy the whole visual field, including the part of the visual field served by the damaged part of V1. According to Lau and Brown, this phenomenon poses a problem for first-order theories because there is no first-order physical state that can realize the conscious experiences. Lau and Brown are adverting to an idea I and others have emphasized, that the first-order realizer of conscious visual percepts involves feedback loops from higher visual areas to V1 (Block 2007a; Lamme 2003).

My response appeals to the distinction between causal and constitutive factors in conscious experience (Adams and Aizawa 2008; Block 2005). To take a nonmental example, dry fuel causally contributes to fire but is not constitutive of fire. What is constitutive is rapid exothermic oxidation. If you have rapid exothermic oxidation, you have fire whether or not the fuel is wet. Moving to consciousness: Blood flow in the brain is a causal factor in the production of conscious experience because blood carries oxygen necessary for neural processing. Neural processing itself is constitutive of consciousness—what conscious experience is. Even if blood flow stops, there can be brief consciousness before the neurons die for lack of oxygen.

My account of consciousness is biologically based and has always focused on activations in the circuits that process the relevant contents. For example, we know that neural activity in MT/V5 is part of the circuit that underlies (and is the constitutive basis of) conscious experience of motion. We know that neural activity in the fusiform face area and other face patches is part of the constitutive basis of conscious face experience.

I have entertained the idea that recurrent loops from content areas such as MT/V5 or the fusiform face area to lower visual areas may be necessary for conscious experience. But I have not committed to whether these loops are causally necessary or constitutively necessary. Further, I have never said that an intact V1 was necessary for conscious experience. I normally refer to "lower visual areas"; for example, I said, "However, mere activation over a certain threshold in V5 is not enough for the experience as of motion: the activation probably has to be part of a recurrent feedback loop to *lower areas*" (Block 2007a, 496; italics added). In the same paper, I suggested, "Perhaps V2 or other lower visual areas can substitute for V1 as the lower site in a recurrent loop" (499).

I considered other cases of conscious experience with damaged V1. I said,

> Blindsight patients who have had blindsight for many years can acquire some kinds of vision in their blind fields despite lacking V1 for those areas. One subject describes his experience as like a black thing moving on a black background (Zeki and Ffytche 1998). Afterimages in the blind field have been reported (Weiskrantz et al. 2002). Stoerig (2001) notes that blindsight patients are subject to visual hallucinations in their blind fields even immediately after the surgery removing parts of V1. (Block 2007a, 499)

I suggested another possibility, due to Petra Stoerig (2001, 190), keyed to the damage to V1 in these cases being only partial: "This may be due to a high level of excitation that spreads to other higher cortical areas that have their own feedback loops to other areas of V1 or to other areas of early vision such as V2" (Block 2007a, 499). Here is how Stoerig puts it: "In the case of hallucinations, the spontaneous extrastriate cortical activation is quite strong, and may therefore spread to other structures, subcortical and cortical, in the ipsi- and contralesional hemisphere" (2001, 190; if damage is on the left, the ipsilateral hemisphere is the left one and the contralateral hemisphere is the right one).

Importantly, the cases of Rare Charles Bonnet Syndrome that Lau and Brown describe are all cases of partial destruction of V1. Two of them are descriptions of damage to one side of the visual cortex. The third describes damage to "most of the primary visual cortex" (or V1), suggesting that some of V1 was preserved (Duggal and Pierri 2002, 291). Suppose, as in one of Stoerig's cases, we have visual experience of motion in the left visual field in the absence of the right half of V1, the part that processes the left visual field. That activation could spread from the right part of the motion area to the left part of it and then down to the intact part of V1.

To summarize: My biological theory of consciousness has focused on content areas such as MT/V5 for motion content. I have also speculated that loops to lower areas are important, but I have usually marked those references as somewhat speculative. So Rare Charles Bonnet Syndrome does not go counter to anything I and other advocates of recurrent loops have said, because we have not required V1. Further, in cases of partial damage to V1, other pathways to the remaining part of V1 may take over.

Lau and Brown note that I am not committed to feedback to V1, but they think that feedback to V1 fits with my philosophical position:

> On Block's view, it is the *biological substrate* of the first-order representation that is critical for conscious phenomenology. Presumably, the feedback-to-V1 view is attractive to him because the recurrent processing reflected by the feed-forward and feedback waves of neural activity seems to give a flavor of a specialized biological phenomenon. If Block is to abandon this view, he would need to specify what is special about extrastriate activity that allows it to support conscious phenomenology. Is it not just normal neural coding, which sometimes can reflect unconscious processing too?

Of course, they are right that the biological point of view should seek an account of the difference between conscious and unconscious perception. One proposal is that it is just a matter of level of activation. Another proposal is that feedback to lower visual areas is required but not necessarily feedback to V1.

The descriptions of Rare Charles Bonnet Syndrome in the articles that Lau and Brown refer to reveal vivid hallucinations, but it is not clear that they are as fine grained as normal perception. One description is that "[the patient] usually referred to seeing coloured 'Lilliputian' figures of women and children, either static or moving, but usually running in meadows or even lying in bed with her, or, occasionally, brightly coloured countryside scenes" (Contardi et al. 2007, 272). Another description is "[the patient] noticed vivid images of lions and cats in the right visual field. Over the next few days he described… seeing flock of birds, pack of hounds, chessboards and brightly coloured scarves in the same area" (Ashwin and Tsaloumas 2007, 184). Similar descriptions are given elsewhere (e.g., Duggal and Pierri 2002). Nothing in these descriptions suggests, for example, that the hallucinators have experiences as of different shades of color of the sort one finds in a paint store, despite the claim of Joseph LeDoux and Richard Brown (LeDoux and Brown 2017) that the experiences of these hallucinators are "rich"(E2020).

Of course, hallucinations may always be caused by higher-level cognitive activity. Presumably the Lilliputian figures are not something the patient had ever seen before. But that does not mean that they are constitutively cognitive. The cognitive activity may have its effect by causing activations in the perceptual areas that are themselves constitutive of the hallucinatory experience.

It has often been noted that our experiences are more finely differentiated than our concepts of those experiences as indexed by our ability to identify the experiences (Evans 1982; Peacocke 1992; Tye 2006). For example, even people with perfect pitch can recognize fewer than a hundred pitches but can distinguish among more than a thousand pitches (Raffman 1995).

One problem for cognitive theories of consciousness of the sort that Lau and Brown are advocating is that the cognitive system that according to them generates conscious experience is simply too coarse grained to explain normal human perceivers consciously seeing a million colors even though they have concepts of only a tiny fraction of those colors. Potentially, cases of hallucination can provide further evidence for the coarse grain of purely top-down experiences and so add to the growing evidence against cognitive theories of conscious experience.

Peripheral Vision

Lau and Brown put the criticism of my view on the basis of peripheral vision as follows: "In Peripheral Vision, it is not clear how the relevant first-order representations can exist, because even at the retinal level the relevant input is not rich enough. One can perhaps argue that the color sensation and vividness of details in the first-order representation are created from top-down mechanisms, but one needs to substantiate such empirical claims. In our own introspective experience, even if we open our eyes for a brief period to a new scene, we get the phenomenological feeling that the periphery is not exactly monochrome and devoid of details." And later they say that "under rigorous laboratory testing, it seems that we do not actually experience any determinate color in the periphery."

I responded to much the same argument made by Dehaene, Naccache, and Van Gulick (Block 2007b, 534), but I amplify those remarks here.

First, it is a myth that there are insufficient color receptors in the periphery of the retina to see vivid colors. Discrimination of one hue from another is as good at 50° as it is in the fovea if the color stimuli are large enough (Mullen 1992). And there is some color sensitivity out to 80° to 90°. I called this a myth (Block 2007b, 534), and a recent article describes it as a "widespread misconception even among vision scientists" (Tyler 2015). This misconception was recently repeated yet again (Cohen, Dennett, and Kanwisher 2016).

Christopher Tyler (2015) estimates that one-third of the cells in the peripheral retina are color-sensing cone cells. However, vision in the periphery involves integration over wider areas (Block 2012, 2013; Pelli and Tillman 2008). Integration over wider areas could produce vivid color experience. See figure 12.1 for an illustration of the size of the integration windows.

Tyler argues that color perception in the periphery is more vivid than in the fovea. Of course, that depends on how large the items are in the fovea and the periphery. Figure 12.2 is a display in which the colored circles have been increased in size in the periphery to more than match the decrease in cone density. (See also the discussion in Haun et al. 2017.)

One of Lau and Brown's claims is "in Peripheral Vision, it is not clear how the relevant first-order representations can exist, because even at the retinal level the relevant input is not rich enough." But sparse retinal input can, at the level of visual cortex, be averaged over many receptors to produce a vivid representation of color.

Second, it is well known that there is integration of color information over time within visual cortex. Indeed, there is some evidence that colors are processed one at a time so that perceiving the multicolored scenes we are aware of requires integrating many different color-processing episodes in a brief period (Huang and Pashler 2007; Huang, Treisman, and Pashler 2007). Seeing peripheral colors over time could be due to previous less-peripheral fixations. Again, these are first-order effects.

Third, memory color effects are well known. In one interesting paradigm, subjects presented with a picture of a common object colored at random (a banana might be purple) are asked to adjust the color to look a neutral gray. They twiddle two knobs, one of which controls the red-green axis and the other of which controls the blue-yellow

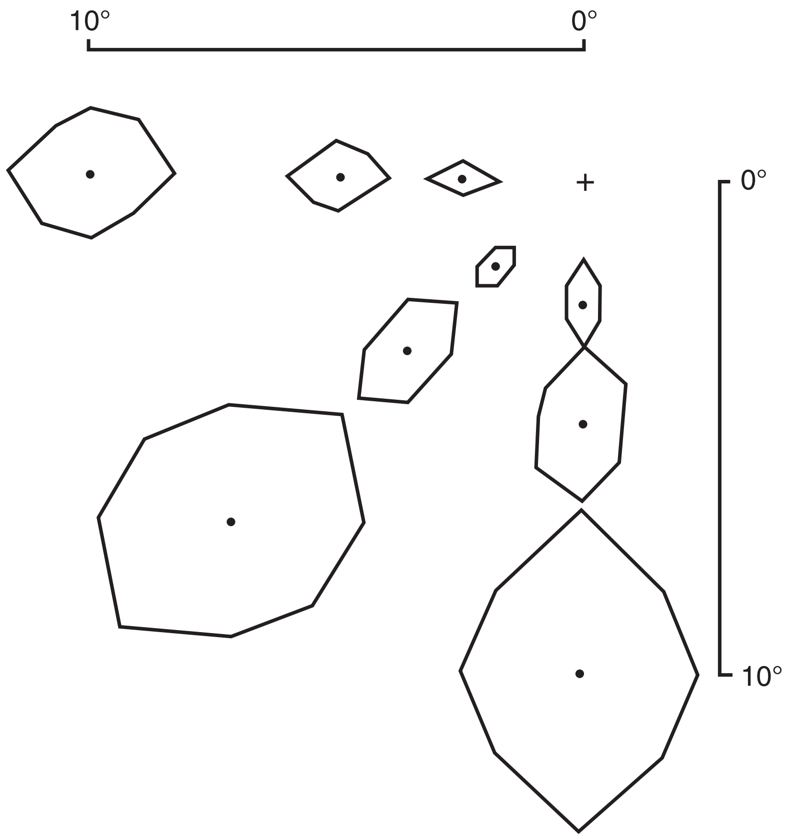

Figure 12.1

The fixation point is the +. The diagram illustrates the size and shape of windows within which stimuli are integrated. These integration windows increase in size with eccentricities. From Pelli and Tillman 2008. Reprinted by permission of Macmillan Publishers Ltd.

axis. The memory color effect is revealed by subjects moving the dial 15–20 percent toward the blue direction to make a banana look gray (Witzel and Hansen 2015; Witzel, Olkkonen, and Gegenfurtner 2017; Witzel et al. 2011). These are not general effects of knowledge on perception, as revealed by the fact that the effect works for yellow and blue but not for paradigms of red and green. For example, there is a negative effect on pictures of hearts (Witzel and Hansen 2015). That is, subjects adjust a picture of a heart so as to be slightly reddish rather than greenish to make it look gray. There are near-zero effects for the classic red Coke insignia, the typical red strawberry, and the classic red fire extinguisher, and only a weak effect for green ping-pong tables. But there are strong positive effects for bananas, the classic yellow German mailbox, blue Smurfs and Nivea tins, and the purple Milka container (Witzel et al. 2011).

Further, even though subjects believe that both gray photos and gray drawings represent bananas, the effect is larger for fully textured photos. One hypothesis the experimenters discuss is that the effect is based on associations within the visual system

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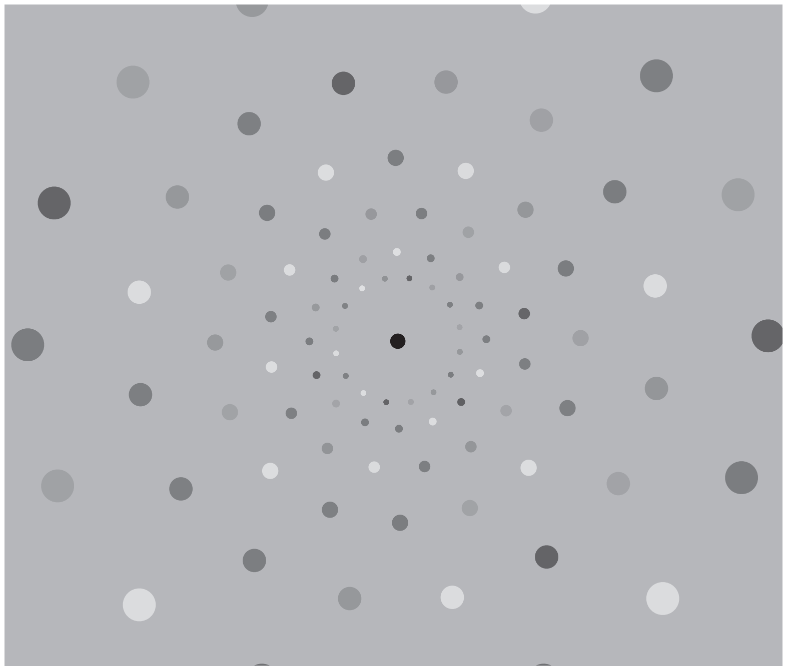

Figure 12.2
Fixate the central black disk at about twelve inches away. In the color version of the figure, the outer disks should be as vivid in color as the inner ones. From Tyler 2015, Creative Commons.

between, on the one hand, shape and texture and, on the other hand, color. The shapes of lemons and bananas are shapes the subjects would know are meant to depict lemons and bananas, (See Deroy [2013] on this point, and see Brogaard and Gatzia [2017] for a different view.) In sum, there is no reason to think that the memory color effects are effects of cognition on perception, and there is some reason to think these effects are a result of associations within vision itself.

The points that I have been making are illustrated dramatically in figure 12.3 (from Lau and Rosenthal 2011). In the caption, Lau and Rosenthal speak of low spatial and color sensitivity in the periphery of vision along with the main attention being devoted to the center of the visual field. They say, “Based on these findings [the ones just mentioned], one might expect our conscious visual experience to be similar to what is shown in (a). However, there is a compelling subjective impression that peripheral vision is less impoverished: in particular, subjective vision is more similar to what is depicted in (b) rather than (a)” (2011, 369). However, after taking into account (1) visual integration over time and space and (2) effects of prior experience as reflected in memory color and top-down filling-in effects of cognition on perception, we can see that this reasoning is defective.

Lau and Rosenthal themselves appeal to top-down filling-in to argue for the higher-order view. “Furthermore, the subjective richness of qualitative character in peripheral

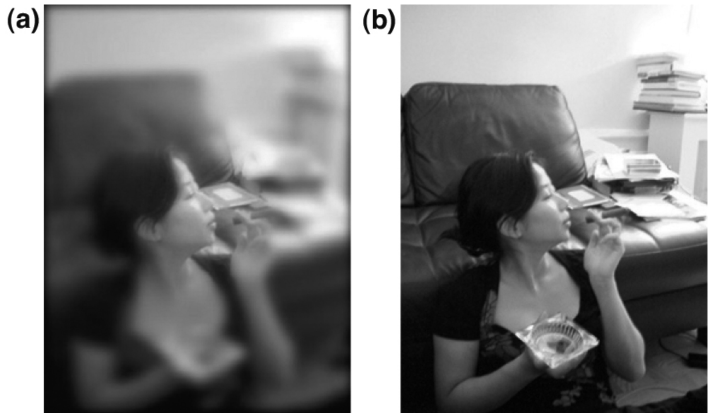

Figure 12.3
This figure from Lau and Rosenthal 2011 is supposed to illustrate the difference between what we would experience if the first-order view were right (on the left) and what we actually experience (on the right). Reprinted from Lau and Rosenthal 2011 with the permission of Elsevier.

vision could be due to memory from previous visual fixations at such locations. If so, the apparent richness of qualitative character is due to top-down 'filling in,' rather than detailed first-order representations at the moment of perception. This again fits well with the higher-order view" (2011, 369). Their mistake in this passage is inattention to the distinction between causal and constitutive factors. (See the discussion above for an explanation of this distinction.) The efficacy of top-down filling in for increasing the level of detail in first-order representations in the visual system is well documented (Churchland and Ramachandran 1996; Spillman et al. 2006). This is a causal effect of cognition on first-order conscious representation and does nothing to promote the higher-order approach.

Inattentional Inflation

Rahnev, Maniscalco, and colleagues (2011) created an experimental situation in which subjects were just as good at discriminating the orientation of an unattended high-contrast grating (a Gabor patch) as discriminating an attended low-contrast grating. (They boosted the contrast of the unattended grating compared to the attended grating.) Despite equal discrimination ability in the two cases, subjects gave higher visibility ratings to the unattended grating than to the attended grating. They explain this result in terms of the increased variability in the perception of the unattended grating. (More on this explanation below.) Lau and his colleagues were also able to achieve similar results using the comparison between central and peripheral vision. They argue that peripheral vision is more variable than central vision.

Apparently, their argument is this:

1. The first-order states were about the same in strength as evidenced by the equal performance on discriminating the gratings;
2. But as reflected in the differing visibility judgments, the unattended case was higher in consciousness;
3. To explain the higher degree of consciousness in the unattended case we cannot appeal to a first-order difference since there is no such difference (see premise 1). So the only available explanation has to appeal to the higher-order difference in judgments of visibility.

Premise 2 says that the unattended case was higher in degree of consciousness. The meaning of this phrase is not entirely clear. When I am driving in low-visibility conditions, I am at least as conscious of the scene as when I am driving on a clear day. Subjects rated the stimuli as more visible, but judgments of visibility can reflect how sure the subjects are about their judgments or alternatively the content of perception—for example, in acuity or precision rather than anything that could be called degree of consciousness. Cars on the highway are less visible on a hazy day than on a sunny day, but I know of no reason to think the experience of the driver is less conscious for all that. The difference is in determinacy of content. To be clear: I'm not saying that the unattended case is higher in determinacy. Lack of attention *decreases* determinacy, other things being equal. But other things are not equal, because the sensitivity (d′) in the two cases is equal (due to the unattended stimulus being higher in contrast). The results give us no indication of whether content differs or not.

The apparent reason for believing premise 2 is that there was a difference in higher-order thought about the first-order state. *But to use that as a reason just presupposes the higher-order theory and cannot be used to argue for it.* Lau and Brown say that using changes in visibility ratings as evidence for changes in phenomenology is standard in the field: if "higher visibility ratings is not good enough evidence that phenomenology changed, what else can count as good evidence? To deny that is to deny the common standard of interpretation of experiments in this field." But their procedure exploits a problematic aspect of taking visibility ratings as a perfect guide to consciousness. I am not denying that the difference in attention brings a phenomenology difference with it. Attention may have its own phenomenology. What I am denying is that the difference in visibility ratings shows a difference in anything that could be called "level of consciousness."

In another paper by the same group of researchers (Rahnev, Bahdo, et al. 2012) that yielded similar results, the subjects' judgments were confidence judgments rather than visibility judgments. They used a 4-point scale, in which 1 represented low confidence and 4 was high confidence. Are these judgments supposed to show a difference in degree of consciousness? In another paper by this group titled "Direct Injection of Noise to the Visual Cortex Decreases Accuracy but Increases Decision Confidence" (Rahnev, Maniscalco, et al. 2012), an electromagnetic pulse was delivered to the visual cortex. As the authors note, electromagnetic pulses of this sort increase variation in neural activity without increasing strength. The effect was to decrease perceptual accuracy but increase confidence. Is that increase in confidence supposed to be an increase in consciousness of the state? It may be that the pulse changed phenomenology but the increased confidence does not constitute an increase in consciousness, whatever that would be.

It is often said that when you give subjects a 4-point rating scale it doesn't much matter whether you ask them to rate visibility or to rate confidence in their judgment. The pragmatic situation dominates the responses independently of exactly what the ratings are supposed to mean.

My point here is confirmed by the explanation by Rahnev et al. (2011) of the effect. The explanation is that attention decreases variation in the visual representation so that the representation of the unattended stimulus is more variable. But higher variability promotes more crossing of a threshold of confidence than lower variability, and hence there is a higher likelihood of judging the stimuli to be visible. This is an effect on the criterion for judgment—the judgment that one has seen the grating—not on perception itself.

Here is an analogy: Suppose there are two political demonstrations, one on your left, the other on your right. You want to judge which is louder. You listen for really loud noises. Unknown to you, the average noise level is the same, but the one on the right is more unruly—that is, there is more variability. So the demonstration on the right is more likely to cross the threshold for really loud noises, and you are more likely to judge it to be louder even though the average noise levels are the same.

Here is my point: Rahnev's model of the phenomenon shows it is a judgment phenomenon, a phenomenon of judgments crossing a confidence line. I would say—from my first-order point of view—that it is a phenomenon in which the judgments in the unattended case differ without any known difference in actual consciousness. A higher-order theorist would say that the difference in judgments shows the consciousness differs, backing up premise 2. But this little dialogue shows that the justification of premise 2 presupposes the higher-order conclusion and cannot be used to argue for it.

The question-begging nature of the argument is apparent in Lau and Brown's justification for premise 2: "However, as in standard models of perception (Green and Swets 1966), subjective perception happens when the signal crosses a threshold or criterion." What they are calling "subjective perception" is just the subject's opinion of the quality of the perception—for example, its degree of visibility. That is not what I would call subjective perception. Note that I am not saying that my view of subjective perception is right and theirs is wrong. That would be just as question-begging as premise 2. Rather, I am saying that their argument presupposes the conclusion.

In conversation, Richard Brown notes that Rahnev et al. (2011) gave subjects rewards for more optimal metacognitive judgments but that the rewards failed to put a dent in the inattentional inflation. (A metacognitive judgment of perception is optimal insofar as one's confidence tracks whether one's discriminations are right.) In the Supplementary Information for the article, the authors describe variants of the experiment in which they encourage subjects to respond optimally by a number of means: they reward subjects for responding optimally, they explain that the payoff structure promotes unbiased responding, and they give subjects trial-by-trial feedback, a procedure that is said to diminish suboptimal decision biases.

As I understand Brown, he is saying that showing subjects that an effect is an illusion does not change how the illusion looks, whereas reward is known to affect criteria for judgment. The authors describe these manipulations as showing the robustness of the result. They do not in any way use this result to distance themselves from their model, showing as it does that the result is an effect on criterion. Is the upshot of Brown's remark supposed to be that the Rahnev effect is perceptual rather than a criterion effect? If that idea were accepted, then it would be difficult to see what to make of the paper, given that it shows that the results can be explained by a model based on attention shifting the criterion in a more conservative direction. The title of the paper is "Attention Induces Conservative Subjective Biases in Visual Perception" and that is what the paper shows. Rahnev, Maniscalco, and colleagues put their result by saying that an "important idea of the model is that attention reduces the trial-by-trial variability of an internal perceptual signal, which subsequently reduces the probability with which the signal exceeds the decision criterion" (2011, 1514). Perhaps they think that shifting the criterion for responding just is shifting the conscious perception, but that would be a question-begging claim that presupposes the higher-order view.

Further, the way to get a criterion to shift via reward is to slant rewards toward a more liberal kind of response or else to a more conservative kind of response. For example, if you want subjects to lower their criterion for saying they saw something, you just reward them for correctly saying they saw it without punishing them for incorrectly saying they saw it; that is, you reward hits but don't punish false alarms (Snodgrass 2002; Visser and Merikle 1999). However, the rewards in the Rahnev, Maniscalco, et al. (2011) experiment were for optimal responding, not for a liberal or conservative bias.

Returning to the main point of this section, the question-begging form of argument infects the discussion of temporary inactivation of frontal cortex due to electromagnetic pulses from transcranial magnetic stimulation (TMS) in Lau and Rosenthal (2011). They say,

> [Rounis et al. 2010] reported that TMS targeted at the dorsolateral prefrontal cortex both lowered subjective reports of visual awareness and impaired metacognitive ability ... the effect was salient in the subjective reports, whereas task performance was unimpaired.... However, because subjective reports are more direct measures of visual awareness compared to task

> performance [Lau 2008, Figure 2], this is in agreement with the claim proposed by the higher-order view that the prefrontal cortex is crucial for conscious awareness, and in particular the subjective aspects (i.e., not only task performance). (Lau and Rosenthal 2011, 369)

From the point of view of a first-order theory such as mine, these results are unsurprising. Dorsolateral prefrontal cortex is a known cognitive and metacognitive area, so disturbances to it having a negative effect on metacognitive accuracy and subjective reports is to be expected. From my point of view, these are effects on reports without requiring effects on conscious perception, and I take the unchanged task performance as an indication of this. Thus, this result is neutral as between their view and mine, and it is just question-begging for them to treat it as supporting their view.

In discussing Rare Charles Bonnet Syndrome and peripheral experience, we were talking about phenomena that could be discussed independently of theory. In this case, however, the interpretation of the results is relative to theory. As a result of this, Lau and Brown's argument is straightforwardly question-begging. I have treated these three cases differently, but Lau and Brown say that there is an advantage to giving the same treatment to all of them, and they think this gives an advantage to the higher-order approach. But this argument is question-begging too. The only similarity between the cases is one that appeals to a higher-order point of view: absent or weakened first-order state plus a present higher-order state. That reason for treating the cases alike presupposes the higher-order view and cannot be used as an argument for it.

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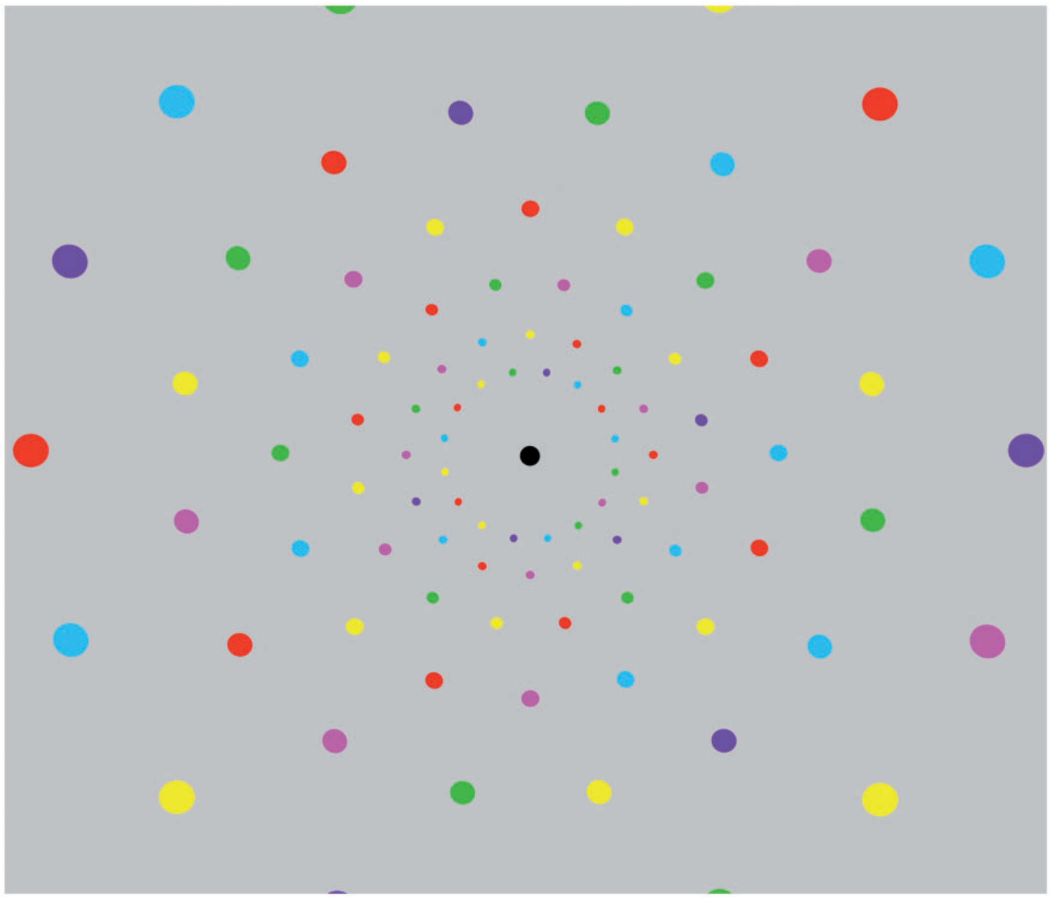
The figures in this article were printed in grayscale so I have included the color pictures here.



(a) (b)