



Reply

Theories and measures of consciousness develop together [☆]

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Persaud, McLeod, and Cowey (2007b) dispute my claim that “post-decision wagering [PDW] cannot supply a direct behavioral measure of consciousness” (Seth, 2007), in which a “direct measure” is one that transparently reflects its target property, as a ruler directly measures length. Here I defend this claim and argue in favour of a virtuous circularity among measures, theories, and data.

Behavioral measures of consciousness come in two flavours, subjective and objective. Subjective measures leverage introspective capabilities, for example by a person verbally reporting the content of her conscious experience or expressing her level of confidence in a previous decision or discrimination. Introspection always involves metacognition because introspective reports consist of judgments about mental states. Therefore, subjective measures are always indirect and can be vulnerable to many biases (e.g., reluctance to report uncertain experiences). Also, because metacognitive conscious content assumes primary (sensory) consciousness but not *vice versa*, subjective measures risk incorrectly rejecting the presence of sensory consciousness based on the absence of metacognition.¹

Objective measures do not require introspection and instead use some other behavior, for example forced-choice decision accuracy, as a proxy. Although objective measures need not involve metacognition they are also always indirect, for two reasons. First, they still require a response criterion, for example whether to push a button or not. Second, they may not measure consciousness at all because many behavioral proxies—forced-choice decision accuracy being a good example—are capable of being learned unconsciously.

So the simple reason why PDW cannot supply a direct measure of consciousness is that *no* behavioral measure, subjective or objective, is up to the job. Indeed, the fact that consciousness is ontologically subjective precludes direct behavioral access to conscious content. This does not however mandate pessimism: A science of consciousness is made possible in the first place by adopting epistemologically objective attitudes towards ontologically subjective phenomena (Searle, 1992). Importantly, both subjective and objective measures are epistemologically objective because both produce data that can be shared publicly.

Persaud et al. raise two other issues in support of PDW being a direct measure: its intuitiveness for subjects, and evidence that it doesn’t affect the conscious states that it supposedly measures, at least not as much as some subjective measures do. However, both conscious and unconscious behaviors can be intuitive and indeed the phenomenology of intuition is often associated with unconscious knowledge (Dienes & Scott, 2005). And having a measure of X that doesn’t affect X is a prerequisite for a good measure of X, one which may be necessary but is by no means sufficient for establishing directness.

[☆] Reply to Commentaries on Persaud, N., McLeod, P., & Cowey, A. (2007b). Reply to Note by Seth: Experiments show what post-decision wagering measures. *Consciousness and Cognition*. doi:10.1016/j.concog.2007.05.008.

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¹ Higher Order Thought (HOT) theorists may demur here. According to HOT theory (Rosenthal, 2005), a mental state is a conscious mental state in virtue of there being a (metacognitive) HOT to the effect that one is in that mental state.

Given that PDW is indirect, how can it best be characterized? Apparently, PDW is objective because it doesn't explicitly require introspection. However, unlike many objective measures, PDW is explicitly metacognitive because placing a wager is a decision about a decision. There are three important consequences of these features for interpreting data generated using PDW. First, because PDW is objective there is the possibility in principle that advantageous wagering could be learned unconsciously. Second, because PDW depends on metacognition it may be vulnerable to metacognitive biases just as subjective measures are; for example, individual differences in risk aversion may lead to variations in wagering performance even with the same underlying conscious phenomenology. Last, as noted above, because PDW involves metacognition the absence of advantageous wagering does not unequivocally establish a corresponding absence of primary consciousness (Seth, 2007).

Empirical progress on these issues can be furthered by comparisons of different measures within single studies. For example, Szczepanowski and Pessoa (2007) have recently dissociated subjective and objective measures of fear perception using signal detection theory [see also (Tunney & Shanks, 2003)]. Even better, experiments probing the neural basis of metacognitive access could shed much-needed light on the distinction between primary consciousness and (metacognitive) higher-order consciousness. An improved understanding of this distinction will greatly enhance the explanatory power of all metacognitive measures of consciousness; it is exciting that several such experimental programmes are now underway [(Baars, Ramsoy, & Laureys, 2003; Goldberg, Harel, & Malach, 2006); note that (Dienes & Scott, 2005) does not directly the above distinction because both 'structural knowledge' and 'judgment knowledge' are metacognitive].

In the absence of measurements and data, theory is lame. But without theory, measures are blind and data are ambiguous. Persaud, McLeod, and Cowey (2007a) conclude that variations in subject GY's wagering performance track variations in awareness. This seems reasonable, *not* because PDW provides a methodological silver bullet directly revealing the contents of GY's visual consciousness (it doesn't), but instead because GY's data fit nicely into a rich context comprising previous experimental findings and theoretical accounts of blindsight. It is to leverage this context that Persaud et al. (2007b) note that GY "is well-known to exhibit good performance without awareness when shown subthreshold stimuli in his affected field" and that "based on these and previous findings with GY, we concluded that the difference in post-decision wagering reflected the difference in awareness."

Persaud et al. suggest that having a direct objective measure of consciousness is a pre-requisite for a science of consciousness. Without such a measure, they wonder, how can one know whether consciousness has any of the neurophysiological, neuroanatomical, or behavioral properties of the sort proposed in Seth, Baars, and Edelman (2005)? I continue to believe that a reasonable approach consists in a consensus of experimental methods buttressed by explicit theoretical frameworks. Although this is certainly challenging, in the best case there can be a strong virtuous circularity in which putative measures and theoretical advances mutually inform, validate, and refine each other. For example, subjective reports currently remain the most reliable source of evidence about conscious contents. Possessing such evidence allows us to procure and evaluate other more objective evidence in the form of behavioral and physiological correlates of conscious contents and conscious states. These correlates then serve as constraints to inform the development of theories that suggest necessary and sufficient mechanisms for consciousness. These theories can in turn suggest new measures and contextualize existing measures, both subjective and objective.

In summary, it is premature to herald a direct objective measure of consciousness in the absence of a satisfactory theoretical foundation justifying such a measure.

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