

# I Cannot Tell You (Everything) About My Dreams:

## Reply to Ivanowich and Weisberg

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One of the main problems for the scientific study of consciousness is methodological. At least *prima facie*, the kind of knowledge we have of our own experiences is direct and not mediated by an inference process. This kind of knowledge contrasts with the kind of knowledge we have of others' experiences, which relies on the observation of their behavior and their reports.

Collecting data for the scientific study of consciousness requires scientists to go beyond their own personal experiences and study others' states. This, in turn, requires that subjects report or act a certain way depending on their experiences. Although we can, at least typically, report on our own experiences, there are two important methodological worries if:

1. There are experiences on whose content we cannot report.
2. There are experiences in circumstances in which we cannot report.

Theories that I have called *Higher-Order Cognitive*, like Rosenthal's HOT theory, maintain that the mechanisms that render a state phenomenally conscious depend on the kind of cognitive access that underlies our ability to report the content of the state. These theories deny that –in beings like us, with our reporting abilities unimpaired– 1) is possible. The question at this point is how we can empirically falsify this kind of theory. In order to do so, we would need a case of an experience on which the subject cannot report; but, if this were the case, how can we know that the subject is undergoing an experience? The paper I have presented offers a possible reply to this question.

The results of Lau and Passingham's experiment suggest that the neural correlate of the cognitive access that underlies our ability to report lies in the dlPFC, making this area

the most plausible candidate to implement the required kind of HOTS. Rosenthal endorses this later idea. Such a commitment is an empirical one and can be empirically falsified. We shouldn't –by no means– think of this as a weakness of the theory; quite the opposite, because the connection between reportability and consciousness is, in any case, a posteriori. Now, if there are circumstances in which a subject undergoes experiences without the corresponding activity in the dlPFC, then the kind of cognitive theories under consideration would be jeopardized. Dreams seem to present such a case.

The problem is that dreams are instances of 2) and someone might raise doubts on whether dreams are conscious experiences. In “Not a HOT Dream” I presented the case of lucid dreams in favor of the reality of dreams as conscious experiences, given that subjects are able to make some simple reports during these episodes. Surely, as Weisberg notes and I make clear in the paper, it is an open possibility for my opponent to accept that lucid dreams are conscious but not so ordinary dreams. First of all, I guess that most would not find this possibility really plausible and I think that this is a desperate move. But, more importantly, there is some empirical evidence suggesting that ordinary dreams are accompanied by mental imagery. In these experiments (I mention Roffwang (1962)'s one in the paper), subjects (whose eye movements are monitored during sleep) are awakened during REM sleep; they report their dreams, and scenes requiring a determining control of gaze are selected. It has been observed a correlation between the movement of the eyes and the movements required to motorize these scenes. For example, in an experiment by Dement and Kleitman (1957), a sleeper looked up and down during REM sleep followed by his report that he dreamed of climbing up a series of ladders looking up and down as he climbed. Similar results have been found in studies with REM sleep behavior disorder. This condition is characterized by a loss of muscle atonia (paralysis) during REM phase. Leclair-Visonneau et al. (2010) showed that when rapid eye movements accompanied goal-oriented motor behavior during REM sleep behavior disorder (e.g. grabbing a fictive object, hand greetings, climbing a ladder) the great majority were directed towards the action of the patient (same plane and direction) and they suggest that, when present, rapid eye movements imitate the scanning of the dream scene.

I find the second of Weisberg's proposals to block the argument more appealing. He acknowledges the low level of activity in dlPFC, but he rightly stresses that this doesn't

mean that there is no activity at all. It might be the case that the remaining activity corresponds to a few HOTS which would account for dreams. Weisberg's interesting suggestion here is that it may be the case that the phenomenology of dreams is much less rich than the phenomenology of waking experience. In favor of this proposal, Weisberg appeals to his own dreams and hold that they are not especially vivid, at least in the sensory domain. It would be of no help to contrast the content of my dreams with Weisberg's ones, for they might easily differ. However, it is possible to explain Weisberg's claim that his dreams' content is sparse rather than rich and that "the content of most dreams is intuitively sparser than the content of waking experience" (??) as a problem of memory. It is a well known fact that we tend to quickly forget the content of our dreams (some people even think of themselves as not having dreams at all), something that scientists know and try to avoid controlling the waking up conditions in the lab and recording reports directly upon awakening in the REM phase.

Weisberg also suggests the possibility that we may confabulate the phenomenological richness of our dreams. It might be the case that our dream experiences are sparse and that we enrich our conscious memory of dreams beyond what was present in the actual event. I think that Weisberg is right and this is a serious possibility, but a possibility for any kind of post-presentational report, not only in reports about the content of our dreams. In any case, given the low level of activity in dlPFC during REM sleep, the content of our dreams would have to be dramatically sparser than the content of our awaken experience. This kind of speculative reply is especially problematic for the kind of theories we are considering to a point where it is doesn't seem plausible. The reason is that HOT theories already claim that awaken phenomenology is not as richer as it might seem to be. Let me elaborate.

Based on Sperling (1960)'s experiment and some more recent results (Landman et al 2003; Sligte et al. 2008), Ned Block (2007, see also Block 2011) argues that phenomenology overflows cognitive access. Roughly the insight of Block's *mesh argument* is the following:

When presented with a 3x4 array of letters quickly flashed on a computer screen, subjects in Sperling's experiments report having seen a bunch of letters arranged in a block but they are unable to report the identity of most of them. The reason for this result is the

limited capacity of the working memory, the memory buffer that encodes the information we can report on. The interesting case comes from a second condition where a tone is played after the array ceases to be visually present. This tone cues subjects to report one single row. In this case, subjects are able to report the identity of all the letters in the cued row. Block concludes that the best explanation for this result is that the content of experience overflows what we have cognitive access to, because subjects report having seen all the letters and they were able to report the letters when they were cued, in spite of the fact that the letters were not visually present.

In reply to this argument defenders of some form or other of HOT theory (Rosenthal 2007, Brown 2011, Brown and Lau forthcoming) have maintained that the content of phenomenology might not be as rich as some might have thought. In the Sperling's case presented above, our experience would represent an array of alphanumeric characters without thereby representing any determinate character. Furthermore, it has been theorized that something similar usually happens in our everyday experience. For instance, Lau and Brown (forthcoming) suggest that despite our thinking that we see color in the periphery of our visual field we might not experience any determinate color in this area. Independently on whether we can make sense of a color experience which is not an experience of any particular color or of an experience that represents alphanumeric characters without thereby experientially representing any particular alphanumeric character, this line of reply maintains that the content of our experiences lacks all the details that it, at least *prima facie*, might seem to have. Now, in reply to my argument, defenders of HOT might claim that the phenomenology of dreams is "thin" rather than "thick"; the problem is that according to their theories the content of awoken experiences is, arguably, already "thin".

Ivanowich takes a different route. He argues that what I call Higher-Order Cognitive position can be consistent with the lack of expected activity in dlPFC during dream because one can resist, Ivanowich argues, the idea that required HOTs are realized in dlPFC. Ivanowich claims that it is possible that dlPFC activation reflects a confidence judgment about the categorical identity of stimulus, which is then itself the target of a HOT. In Ivanowich's interpretation of Lau and Passingham's experiment, subject's reply to the question on whether they had seen the target or they were just guessing their reply reflects a

judgment about their experiences. This kind of interpretation would be committed to the idea that in order to reply to a question about our perception some kind of additional judgment is required, but it seems to me that we reply to these questions solely in virtue of our experience, without the need of any further judgment. Imagine you are lying in a beach with a friend. He suddenly asks you: “have you seen that plane?”, referring to a plane that just crossed over your heads. In order to reply this question there is no need to make any judgment about the categorical identity of the stimulus, in case there was one, and you can reply to this question solely in virtue of the experience you have undergone. Be that as it may, Ivanowich interpretation is, I think, untenable precisely because of the problems that he foresees. Let me comment on them.

The first one is that performance capacity is matched between the long and the short SOA condition in the experiment. Ivanowich mentions a study by Heekeren et al. (2004) in favor of his interpretation, where it is suggested that the function of the dlPFC is to decide what the subject is seeing on the basis of the strength of the responses of sensory information. In particular, as Ivanowich puts down, they noted that dlPFC activity correlated with the difficulty in the decision task. The problem is that, in the Lau and Passingham's experiment, in both –the short and the long SOA– conditions the performance capacity is the same. This suggests that the “strength of the responses of sensory information” is the same –for otherwise we would expect a variation in the performance capacity as it happens when we modify the SOA– and, therefore, that the activity of the dlPFC seems not to correspond to a “more difficult” decision judgment as Ivanowich following Heekeren would predict.

The second problem is also pressing. Ivanowich seems to concede that there is a phenomenological difference in the experiences of the subjects during the short and the long SOA conditions. However, the only region that shows a difference in activity in the fMRI study that Lau and Passingham performed is dlPFC. Both Ivanowich and Weisberg stress that there might be a whole bunch of other experiences that the subjects undergo while performing the task: subjects are still conscious of the background, the monitor screen, their proprioceptive sensations, the sound of the lights and the AC, etc. If this is the case, one might suggest, adding a visual experience as of a square or as of a diamond would not make much of a difference in the overall experience; we would not expect much of a

change in the brain activity and it might be the case that fMRI technology is not fine-grained enough to find further differences in areas that implement HOTs. There are two important considerations that should be remarked in reply at this point. The first one is that we should assess empirical theories in the light of our current scientific research; the claim that dlPFC encodes HOTs fits the data whereas the claim that there might be other areas encoding them and that fMRI measurements are not fine-grained enough to capture the expected changes remains in the speculative domain. The second one is that subjects are focusing their attention in a certain point in the screen where the stimulus will appear. It is well known that attended objects are more phenomenologically salient than unattended ones (just move your attention away from this paper to the proprioception of your toes). The stimulus is neither like an element in the periphery nor like an unattended stimulus, which might present *defused phenomenology*. Even if elements like proprioception, the light noise or the monitor screen are part of the content of the subjects' phenomenology (a not very plausible assumption, according to the theories we are dealing with, given the capacity restrictions of the kind of memory that underlies our ability to report), the square or the diamond would be the most salient ones, because they appear in the position the subject is gazing at and they occupy the locus of attention. I do not find it very plausible the claim that we cannot find any brain difference that matches these differences in phenomenology. On the contrary, we would expect to see differences in the brain areas responsible for making some information and not other available to the working memory (and therefore to report) and, according to the theories under consideration, making the content conscious.

Finally, in the last section of his paper, Weisberg rightly notes that my argument targets only HOT theories that rely on the cognitive access that underlies our ability to report and that the insight of HOT theories can still be kept while giving up on cognitive access. I agree with him; my only aim in this paper was to undermine the idea that cognitive access is required for having an experience, a thesis that is clearly endorsed in Rosenthal's HOT theory. Weisberg mentions two alternatives: one that relates consciousness and a theory of mind (Carruthers 2000), according to which higher-order representations would be realized in the medial prefrontal cortex and Damasio (2000)'s proposal which links activity in sensory cortex with representations of the current states of the organism. Weisberg notes that both are “in the spirit of the HOT theory, which holds

that mental states are conscious when we are conscious of ourselves as being in them”(??). Although I agree with this, it is doubtful, however, that one needs to appeal to higher-order representations to account for this idea.<sup>1</sup>

## Conclusions

Higher-Order Cognitive theories, like HOT, maintain that phenomenal consciousness depends on the cognitive access that underlies our ability to report. Lau and Passingham's experiment suggests that such an access depends on the dlPFC. Against this conclusion Ivanowich offers an alternative interpretation of the results –in keeping with Heekeren et al. theory about the role of dlPFC– but this interpretation leaves the match in the performance capacity of subject in the short and long SOA unexplained.

The dlPFC is highly deactivated during dreams. This fact jeopardizes HOT theories on the assumption that dreams are phenomenally conscious experiences. Empirical evidence in favor of the reality of this later fact comes from lucid dreams. One can theorize, as Weisberg does, that it might be the case that ordinary dreams radically differ from lucid ones (the former but not the later be phenomenally conscious experiences), but common sense and empirical evidence do not recommend this alternative. Weisberg also notes that, even if conscious, the content of our dreams might be sparser than what we thought, so that the remaining activity in dlPFC account for these experiences. However, in the light of our current knowledge, this doesn't seem to be a satisfactory reply at all given the low level of activity in the dlPFC during REM phase and the commitments of Higher-Order Cognitive theories.

The argument I have presented advocates that Higher-Order Cognitive theories like HOT are wrong. As Weisberg notes, there are other Higher-Order theories in the spirit of Rosenthal's HOT theory that remain untouched. This is true insofar as they are not committed to the idea that phenomenal consciousness depends on the cognitive access that underlies our ability to report.

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<sup>1</sup> See Sebastian (forthcoming) for an account of this transitivity principle unpacked as self-ascription of properties in first-order terms. Such a self-ascription makes use of Damasio's proto-self but without any need to postulate higher-order representations; in other words, the relation between, say, ACC activity and activity in the sensory cortex, is causal but not representational. It links and modulates the connection between the proto-self and the sensory cortex.

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