Author's Response

Multiple Views in Search of Unifying Models

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> Upshot • We respond to three main challenges that the commentaries have raised. Firstly, we clarify our misunderstood intention of introducing a newcomer to the neurophenomenological family. Rather, we situate our approach under the broader umbrella of phenomenology. Secondly, we argue that from our empirical position it is questionable that the strategy we pursued in the target article left the black box of consciousness completely closed. Thirdly, we argue that the subjective fluctuations that may appear as outcomes in an experimental paradigm are not to be considered with a resigned attitude but as valuable information to work with. We conclude our response by agreeing with the concerns of two of the commentators about extending the perspectives and plurality of the methods to investigate the explanatory gap problem.

« 1 » In our target article we aimed to demonstrate, through the example of a pilot study, the usefulness of a mild-neurophenomenological-inspired approach with a front-loaded logic (Gallagher, 2003; Gallagher & Varela, 2003) to adapt and refine the experimental paradigm of Esther Papies, Lawrence Barsalou and Ruud Custers (2012). These adaptations and refinements were necessary to use this paradigm with electroencephalographic (EEG) data in a subsequent neurophenomenological study. We argue that a neurophenomenological-inspired approach allows for a deeper piloting process and better paradigm design, which fosters replicability and phenomenological validity.

« 2 » Throughout this study we explore how the neurophenomenological method can be integrated into experimental settings. Therefore, we do not use neurophenomenology to describe a conscious experience and its mirroring physiological counterpart, but to draw inspiration from this framework to solve a very concrete everyday laboratory problem: the replicability of behavioral results and ensuring of the phenomenological validity of our adapted paradigm for further neurophysiological and neurophenomenological evaluation.

« 3 » In what follows, we shall respond to three main challenges that the commentaries have raised. We will clarify some ideas that may have led to misunderstandings and reflect upon and discuss other ideas raised by our commentators.

The alleged newcomer to the neurophenomenological family

« 4 » In his commentary **Jean-Michel Roy** assumes that we, somehow, want "to introduce a newcomer to the already extended neurophenomenological family"(§1). Later, based on how the borrowed ideas and principles of neurophenomenology are put into practice in our piloting proposal (mainly for the two reasons discussed below), the author concludes that no new neurophenomenological baby has been born (§§9f).

« 5 » The first point he makes mainly refers to the fact that there is no neurobiological data involved in our analysis. The author maintains that our mild-neurophenomenological-inspired approach with a front-load logic "belongs to the broad phenomenological family, but much less so to the narrow one, let alone to the neurophenomenological one" (§9).

« 6 » It is important to clarify that nowhere in our article did we claim that our mild-neurophenomenological-inspired approach constitutes a neurophenomenological baby. The target article discusses ideas of how our methodological approach converges with and diverges from neurophenomenology in §9 and §§79–81). Also, in Footnote 1 we discuss the missing analysis of neurobiological data. There, we also acknowledge the fact that, for this reason, our preparatory study is not a formal neurophenomenological study. However, we are grateful that **Roy** discussed (what he calls) the "phenomenological claim" (§2) in such a clear way, because we agree that the target article presents a methodological adaption that is part of a broader conceptual umbrella than neurophenomenology. In fact, we could not agree more with **Roy**'s claim that our proposal supports the phenomenological claim without constituting a neurophenomenological stance. Nevertheless, it would have been irresponsible not to give a principal role to neurophenomenology when declaring the source of our inspiration, since we borrow and adapt ideas that are constitutive of the neurophenomenological stance and methodology.

« 7 » **Roy** adds a second argument to support his claim that no new neurophenomenological baby has been born. He states:

> "the sheer process of adjusting the behavioral data, in order to reach a good replication rate with respect to a previous behavioral study, through an interaction with subjective ones, can only be made neurophenomenologically consistent if this adjustment process is to be considered as one aspect of the full setting-up process of the neurophenomenological investigation." (§10)

« 8 » We do not completely agree with the aspect of the argumentation referring to the fact that a pilot study that pursues a future neurophenomenological study also has to use strictly neurophenomenology methods, i.e., include the analysis of brain signals or neurological data in general. We think that preparatory steps are necessary to guarantee that the experimental paradigm assesses what it is supposed to assess, i.e., that the experimental paradigm succeeds in manipulating the variable the researchers expect it to. Those steps are required *before* neural activity can be exhaustively analyzed, in particular if it is an exploratory neural approach. So, it might raise doubts as to why EEG measurements are even included in the piloting process, if they were not to be taken into account as a constraining factor. In our case we used them because we needed to ensure the technical quality of such data, and also to implement the procedure in its most complete version possible so that participants underwent exactly the same set-up experience prior to and during the task itself. Different steps of an experimental study have different methodological requirements, even though they all have the same ultimate goal.

« 9 » Another topic addressed by **Roy** is what he perceives as a lack of clarity about the way front-loading phenomenology is incorporated in our target article, since

the experimental subjects themselves produced the experiences' descriptions (§9). Front-loading phenomenology stands by the idea of building the experimental design through an input of previously acquired phenomenological insights (Gallagher, 2003; Gallagher & Varela, 2003). In our case, first-person data corresponding to phenomenological descriptions were extracted from one former pilot to use in the following pilot as an orientation in its construction, creating a front-loading phenomenological loop across pilots.

« 10 » Since we never claimed to give rise to a neurophenomenological baby, we substantially agree with Roy's constructive criticisms and we are thankful for his commentary. Also, we are pleased that he acknowledges that there is a "felicitous side" concerning the potential confusion of having intended to introduce a newcomer, which has "favored the emancipation of the idea of neurophenomenology from its original meaning and made possible the blooming of a family of different projects under a unique terminological umbrella" (§3). We allude to this issue in our article in §§9 and 96, where we claim that other measures have to be integrated with the objective third-person measurements that are going to connect with the phenomenological dimension, which is in agreement with Bockelman, Reinerman-Jones & Gallagher (2013) and Gallagher & Varela (2003). See also the commentary of Konstantin Pavlov-Pinus, who makes a similar point in §§5f.

The black box of consciousness

« 11 » In his commentary, Pavlov-Pinus states that we adopt the "classical scientific 'black box' methodology" to treat consciousness during the pilot procedure (§1), meaning that we treat conscious experience of our participants as a single and closed unit. We can only partially agree with this statement. While we did not put emphasis on the qualia aspect or on the process of emergence of the cognitive acts, for us the black box was very present in the laboratory. It was specifically situated in each subject watching certain stimuli on a screen while following our instructions and subsequently reacting behaviorally to their perceptions. We explored participants' experience of the task, and subsequently used distinctions arising from their conscious experience to interpret the behavioral results and construct the ensuing paradigm. Would this not count as an attempt to partially open the black box?

« 12 » Regarding the question of whether there is a gap, for us the answer is: it does exist in the explanatory framework, but not necessarily in our perceptual experience itself. However, whether the explanatory gap can be solved or whether neurophenomenology is a remedy for it remain open questions for us. Our interest has always been on the experimental side regarding the explanatory gap and consciousness, so we only link our results to the conceptual discussion in the scientific literature to a limited extent. Clearly, more reflection on methodology and epistemology (in particular on the limits of explanations) is needed.

The need for better designs and the modeling of "real world" settings

« 13 » In her commentary, Anna Ciaunica argues that our findings oblige us to choose between two options:

> "either we (a) accept that subjective confounds are inevitable and stronger than we think, but in this case, why should we continue trying to measure subjective experience in the first place?; or (b) strive at designing better experiments in order to control for these fluctuations." (§3)

« 14 » Later she claims that in the target article we go with the first option without considering the second option, which Ciaunica believes to be the most suitable one from a scientific perspective (§4).

« 15 » We disagree with this last statement because we are convinced that our piloting process precisely pursues a better experimental design in order to control and characterize subjective fluctuations. It results in what we call a "first-person enriched cognitive science paradigm" (abstract) that could account for phenomenological validity, which in our view situates our attempt much closer to option (b) than (a).

« 16 » In fact, we disagree almost completely with option (a). Scientists working with humans and cognitive (consciousness) phenomena should be well aware of the experiential component and the necessity of incorporating this component into their explanations and research programs. Subjectivity and lived experience vary and fluctuate within and between subjects. The possible confusions this situation raises are not to be met with a resigned attitude, as may have been understood (§4), but rather with an active intention of recognizing this difficulty in order to construct better paradigms in cognitive science. In other words, the conscious acknowledgment of researchers of this situation should be translated into an orientation of the cognitive field that aims never to exclude the phenomenological aspect of human cognition, yet also aims to minimize the possible fluctuations of experience that could be crucially detrimental to the testing of a specific paradigm.

« 17 » Finally, we would like to revisit the complexity of subjective experience in an everyday setting, which would correspond to the ideal type of measurement. In our understanding, methods and technology make such a setting very challenging.

« 18 » In accordance with what was stated previously, we think that the most fruitful path forward is to understand that there are many modes of human understanding, which can hardly be grasped by just one model of explanation, as Pavlov-Pinus stated in §6. There are many variations and singularities in everybody's way of experiencing and understanding a particular experimental paradigm, and more than one way to express or study a cognitive process. By choosing a specific theoretical and methodological approach, the research question and the phenomenon under study are enclosed in a particular framework. In this sense, from the very start, researchers bias the outcome of the results to some extent toward their own hypothesis. A partial solution to the question Pavlov-Pinus raises about how to know the extent to which researchers influence the outcomes of their studies (§4) would be to have a reflexive and critical attitude throughout the process of producing scientific knowledge, and to be focused on one's own theoretical position and its influence on the production and interpretation of the experimental results. This should be accompanied by a rigorous triangulation, in which not only are the subject under study and the data carefully discussed with other researchers (Jick 1979), but there is critical discussion and examination of how the phenomena have been outlined and how the

outcomes have been influenced throughout by theoretical and experimental decisions made by researchers. In carrying out this process, an increased awareness of the extent to which outcomes are not entirely independent of the researchers' decisions will emerge. This can offer scientists and science a new perspective when evaluating the limitations of the explanations they provide.

« 19 » The evolution of science is marked by the search for better explanatory models at a certain moment in time, and we share **Pavlov-Pinus**'s (§6) view of a horizon where the complexity will be addressed via multi-dimensional networks of models of understanding. In this way, different approaches, such as the method discussed in the target article, neurophenomenology itself, predictive processing (**Ciaunica** §5) and other models yet to be elaborated, could converge in the search for a coherent synthesis of explanation.

Received: 11 March 2017
Accepted: 14 March 2017

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