- avoiding confounds in the interpretation of the behavior and physiological data derived from them.

« 95 » Therefore, the aim and process of our pilot study presented here are in line with a constructivist horizon, because of the active role that participants' experiences of the paradigm had in modeling the experimental design and paradigm itself.

« 96 » Finally, including the first-person in a systematic way during the development and performance of classic cognitive paradigms is a natural step in the attempt to refine the explanation of these multilevel integrative processes (subjective attributes and their objective scientific counterparts). The inclusion of first-person accounts in the study of subtle changes of cognitive functions that ultimately start in, and only make sense in, subjective experience, is becoming mandatory. Mild-neurophenomenology is an inspiring framework for this integration. A wider critical mass of researchers willing to expand cognitive science is required to discuss, enrich, systematize and apply neurophenomenological approaches. Overall, this will help improve the production and interpretation of information, generate new hypotheses, and lead to more rigorous cognitive neuroscience research.

Acknowledgements

This work was supported by the National Committee of Science and Technology of Chile (CONICYT), through Beca Nacional de Doctorado N° 1070761 given to CB. CB acknowledges support by Iniciativa Científica Milenio from the Ministerio de Economía, Fomento y Turismo, Project IS130005: Millennium Institute for Research in Depression and Personality-MIDAP.

Received: 2 September 2016
Accepted: 10 February 2017

Open Peer Commentaries

on Constanza Baquedano & Catalina Fabar's "Modeling Subjects' Experience While Modeling the Experimental Design"

A Newcomer to the Neurophenomenological Family?

Jean-Michel Roy
East China Normal University, China & Ecole Normale Supérieure de Lyon, France
jean-michel.roy/at/ens-lyon.fr

> Upshot • Demonstrating the relevance of collecting first-person data and of establishing reciprocal constraints between this these data and behavioral data to overcome the issue of behavioral data replication is an interesting result. However, this result, as such, falls short of offering any theoretical reorientation of the neurophenomenological project, strictly understood.

The phenomenological tribe and the neurophenomenological family

« 1 » In my opinion, the central purpose of the target article is to introduce a newcomer to the already extended neurophenomenological family. This raises two questions: Who is this newcomer exactly? And how much does it deserve to be treated as a family member? To answer these questions we first need to provide a brief characterization of the present situation of the neurophenomenological family, as this situation is far from being a clear one.

« 2 » From my viewpoint, Francisco Varela's original neurophenomenological project is only one specific version of a more general claim, which I have called the "phenomenological claim" (Roy 2000, 2004). It can be formulated as follows: a *bona fide* phenomenological level of investigation must be introduced in the explanatory framework of contemporary cognitive science in order to overcome the explanatory gap problem. Nearly every word in this formulation counts and there are as many ways to make sense of the claim as there are ways to understand each of them. That Varela's neurophenomenological project is just one of these many ways is perfectly corroborated by the fact that the bulk of its initial statement (Varela 1996) was integrated into the collective essay "Beyond the Gap: An Introduction to Naturalizing Phenomenology" (Roy et al. 1999), which Varela co-authored. Indeed, this introductory chapter to *Naturalizing Phenomenology* (Petitot et al. 1999) contains the first presentation (although it is not without ancestors, such as Nagel 1974 and Marbach 1993) of the phenomenological claim so formulated and refers to some of the specificities of the neurophenomenological approach only as one of the possible directions to pursue in order to turn it into a full-blown research program. However, one distinctive feature shared by all possible versions of the phenomenological claim – and therefore of Varela's neurophenomenological one as well – is that the introduction of a

bona fide level of investigation in the explanatory framework of contemporary cognitive science is motivated by, and geared toward, solving the explanatory gap problem. It is this motivation and finality that distinguish it from the even more general claim that a *bona fide* level of investigation must be introduced in the explanatory framework of contemporary cognitive science, without any specific reference to the explanatory gap problem. The difference is important as this more general claim had been put forward before and independently of the emergence of the phenomenological one, although not necessarily with the appropriate terminology and degree of explicitness (as emphasized in Roy 2004). In this sense the labeling I have proposed is in fact inadequate, since it is no less deserved by versions of this more general claim. Accordingly, one might want to distinguish between a broad sense of the phenomenological claim and a narrow one, of which Varela's original neurophenomenological project is again nothing more than a specific version. My additional contention is nevertheless that, in both cases, the notion of phenomenological investigation is appropriately defined as a first-person and purely descriptive investigation of phenomenological properties. A definition that is deliberately very liberal, as it contains no intrinsic connection with a specific methodology, tool or tradition, even though one might consider that only a phenomenological investigation operating certain choices regarding methodology, tool or tradition can be considered *valid*. When considered at its most general level, the definition of the very idea of a phenomenological claim, be it in a broad or a narrow sense, should include no specific reference to Husserlian or any other phenomenology. So, in this respect my original labeling was not fully adequate either as it did not sufficiently emphasize the difference between a narrow phenomenological claim at large and a narrow phenomenological claim of a Husserlian kind. This inadequacy was corrected in later publications (Roy 2000, 2004) and was due to the context in which the phenomenological claim was initially formulated, namely the examination of the general problem of the relevance of the Husserlian tradition to contemporary cognitive science.

« 3 » In spite of their intrinsic importance and their helpfulness for delineating the precise meaning of the original project of Varelian neurophenomenology, these various distinctions have not always been respected. The resulting confusions have advantages and disadvantages. On the infelicitous side, one could cite the illusion of refuting the phenomenological claim at large when only the unacceptability of its neurophenomenological version has at best been established. In my opinion Tim Bayne (2003) committed such a fallacy, when he stated that the phenomenological claim does not provide a solution to the explanatory gap problem, but as a matter of fact focused his attention only on its restricted Varelian version. On the felicitous side is essentially the fact that these confusions 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. It is for instance arguable that in many hands the connection with the explanatory gap problem got lost and that the expression of neurophenomenology became synonymous with the sheer introduction of a level of first-person investigation (at times not even purely descriptive) of cognitive phenomena. Though they consider the seminal 2002 study of Lutz et al., usually seen as the first concrete attempt to implement the neurophenomenological program, as an "excellent example of the tailored use of introspective evidence," Anthony Jack and Andreas Roepstorff (2002), for example, motivate their plea in favor of such a use only by its usefulness for reaching a better neurocognitive explanation of higher-order cognitive processes.

Which neurophenomenological baby is born?

« 4 » From one perspective the authors' proposal belongs to the core of this neurophenomenological family as it explicitly intends to pave the way for a "subsequent neurophenomenological study" (§79) of the "dereification phenomena," (§7) and also because it conceives of the nature of a neurophenomenological study in rather strict accordance with its original Varelian formulation (§2). In particular, it does not ignore its essential connection either with the explanatory gap problem or with the strategy of establishing mutual constraints in order to solve it. Even espousing that which the authors call a "mild" approach to neurophenomenology is, according to my reading of Varela, faithful to this original conception, even though one might suspect Varela of a certain inconsistency that may have favored diverging interpretations.

« 5 » From another perspective, however, the authors clearly take a step away from Varelian neurophenomenology as they explicitly intend to offer a "neurophenomenological-inspired approach" (§79) that in several respects is "divergent to neurophenomenology" (§83). But wherein lies the difference exactly? And what does it exactly retain from Varelian neurophenomenology?

« 6 » On the most general level, the authors seem to locate this difference primarily in the fact that their proposal is one that addresses the process itself of setting up the experimental design of a neurophenomenological study, and that also defends an approach to this process that incorporates within certain limits "some of the principles of [Varelian] neurophenomenology" (§81). On a more specific level, they seem to locate the difference also in the fact that it demonstrates how such a neurophenomenologically inspired way of setting up an experimental design can be instrumental in overcoming the difficulty of replicating experimental results. A difficulty that affects science at large and is faced head on when pursuing, as the authors do, the objective of investigating the neural correlate of a certain feature of derealisation previously established by other scientists working in a purely behavioral perspective.

« 7 » There are, essentially, two neurophenomenological principles that they borrow within certain limits from Varelian phenomenology. The first one is the principle that makes neurophenomenology phenomenological, namely taking into account the point of view of the experimental subjects themselves about what they experience during the experimental task. A principle that, by being implemented through debriefing interviews that involve open and semi-structured questions and supposedly incarnate a "front-loading" type of description, is different from the one recommended in the original Varelian project. The second principle is that of mutually constraining the subjective data collected in this way and the objective features of the experimental task, but focusing only on the behavioral dimension of

these objective features while – somehow paradoxically – leaving aside their neurobiological dimension.

Is this a genuinely neurophenomenological baby?

« 8 » If the above account of the authors' proposal is correct, I see two possible ways of understanding their claim of having introduced a newcomer to the neurophenomenological family, and both are equally problematic.

« 9 » According to one possible interpretation, the authors propose a new way of carrying out a neurophenomenological investigation that deviates from Varela's original project by retaining only some elements of its defining principles, and decide to demonstrate the usefulness of this alternative way only at the level of the preparatory phase of the neurophenomenological investigation. In this perspective, assessing the degree of neurophenomenological innovation of the authors' proposal comes down to determining whether their alternative form of neurophenomenological investigation is genuinely an alternative and indisputably qualifies as a neurophenomenological one, and if so, whether it is in addition correct. In this respect, I think that it is firstly unclear whether the interviewing strategy that they propose is innovative with regard to previous neurophenomenological studies. Also, it is unclear what its front-loading character actually consists in as the descriptions seem to be largely produced, although dialectically, by the experimental subjects themselves – without mentioning that the mindful condition is one very close to Varelian style *epoché*. Secondly, the fact that the mutual constraints involve only behavioral and no neurobiological data is quite problematic, since consequently a crucial element is missing from the alternative framework without which it can hardly qualify as neurophenomenological. Therefore, this framework can be called a partial and incomplete neurophenomenological one at best. Although, in my opinion, one could further object that the relations it establishes between subjective and behavioral data do not even fully qualify as mutual constraints of the required sort. According to this first interpretation, the proposal is therefore consistent and, as such, certainly belongs to the broad phenomenological family, but much less so to the narrow one, let alone to the neurophenomenological one. So, no new neurophenomenological baby has been born with it.

« 10 » However, this first interpretation ignores the idea undeniably suggested by the authors that their modified neurophenomenological approach is only to be applied to the preparatory phase of the neurophenomenological investigation. Nevertheless, taking this suggestion seriously raises a concern about its consistency. How, indeed, could the principles of investigation applied in the preparatory phase of a neurophenomenological study legitimately differ from the ones operating during this study itself? A piloting phase is a tuning-up one, when one adjusts all the various components that will be part of the full investigation. Consequently, 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. However, if this is what is meant by the authors no new neurophenomenological baby is born either with their proposal. What this proposal only shows – and this is a significant contribution – is how a certain take, already on offer, on the specifically phenomenological component of the Varelian neurophenomenological framework can help take care of the preliminary problem of replicating behavioral data, when there is one, during the setting-up phase of the neurophenomenological study.

Jean-Michel Roy collaborated with Francisco Varela at the time Varela laid the grounds of his neurophenomenological project. A co-founder of the Paris research group *Phenomenology and Cognition*, he organized the 1995 Bordeaux conference that gave birth to the collective volume *Naturalizing Phenomenology*, of which he is a co-editor (Stanford University Press, 1999). In a series of subsequent papers he developed his own view of the possible relevance of a phenomenological investigation to contemporary cognitive science, and of Husserlian phenomenology in particular.

Received: 3 March 2017
Accepted: 4 March 2017

Plurality of Consciousness Appearances – Plurality of Methods

Konstantin Pavlov-Pinus
Institute of Philosophy RAS, Russia
pavlov-koal/at/ya.ru

> Upshot • Baquedano and Fabar's provoking article highlights several difficulties of neurophenomenology, and brings into light the necessity of further clarification of its basic concepts such as human experience, first-person perspective, phenomenological validation, explanation, adequate measurement and so on. Particularly, it becomes more and more clear that the "explanatory gap" cannot be liquidated by means of explanation procedures alone, for the unavoidable variety of modes and forms of (mutual and individual) human understanding cannot be exhaustively reduced to just any one of these modes.

« 1 » Constanza Baquedano and Catalina Fabar's excellent target article leaves almost no room for internal critiques, as the goal, the context of research, the criteria of theoretical success and methodology are stated with sufficient precision, and the conclusion looks very convincing. The very concrete goal, as it is formulated by Baquedano & Fabar, is to achieve the replication of results by Esther Papies, Lawrence Barsalou and Ruud Custers (2012) in their article "Mindful Attention Prevents Mindless Impulses" (§§7f), and then, on the basis of this example, to discuss general reasons for the "replication crisis" in modern psychology as well as ways of resolving this difficulty. Baquedano & Fabar attribute this amazingly low replication rate in experimental psychology to the fact that even slight changes in experiment design lead to great divergence in the observed results (§91). They show via experiment that the integration of a subject's experience into the experimental design becomes crucial. Authors purposely narrow down their search to purely methodological aspects, avoiding ontological and epistemological speculations (§3). They clearly state that their study "is not a neurophenomenol-