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.

Plurality of Consciousness Appearances – Plurality of Methods

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> 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-

ogy study itself since it does not involve neural activities or extensive phenomenological experience description" (Footnote 1). In fact they adopt a classical scientific "black box" methodology enriched by first-person data, recursively incorporated into the experimental moves (for they treat consciousness as a "black box" in order to produce certain predictions about its behavior). The authors do not seek to predict new patterns because even the replication of previously observed patterns appears to be a substantial problem. All these carefully explicated restrictions and presumptions, together with the final interpretation of results, make this work highly consistent and self-sufficient.

« 2 » Another positive side of the target article is in that the difficulties described by the authors provoke further questioning. The latter is the only point where I feel myself competent in joining the discussion raised by the authors. So my goal is to become one more drop in a potential sea of "researchers willing to expand cognitive science" in terms of neurophenomenological approaches (§96). I plan to make a few philosophical comments, aimed at the interpretation of such fundamental terms as (a) first-person reports, (b) human experience, (c) explanation, measuring, replication, modeling and analytical description as theoretical tools of research.

« 3 » The very fact that first-person data could be successfully adopted for the third-person framework shows that there is already a gap between the *experience* itself and publicly announced first-person *reports* about it. In many cases the latter appear in the form of objectivized messages, generated under different external and internal pressures, and many times they should be considered more like citizens of the third-person framework (or at least as the citizens of inter-subjective space). It is nowhere near clear enough how and where to draw a borderline between *genuine expressions* of human experience (which are an essential constituent of consciousness) and just "patterns of talking." It is not accidental that Baquedano & Fabar are pointing to the necessity placing "the impetus of training on the interviewer, not the participant, so that the interviewer acts to support the participant in precise experiential re-

porting"(§83). One more distinction must be mentioned here: the difference between the first-person reports themselves and the third-person view on (and usage of) the first-person reports. To illustrate the significance of these two distinctions, assume that (instead of humans) we have a number of specifically designed computers involved in the same experiment. As of today, the internal life of extremely complex computer programs (especially those that deal with "big data") is no less a "black box" for us then our own consciousness (see, for example, Napoletani, Panza & Struppa 2011 and Pietsch 2016). If we were interested in a diagnostic search of the inner life of such programs we would face exactly the same conceptual difficulties since the same conceptual and terminological apparatus (except the concept of "experience"!) would be applicable here due to the self-learning and self-correcting statistical nature of the corresponding programs. It is perfectly fine to talk about first-person reports here. However, the computer-simulated first-person reports could not be considered an expression of genuine experience due to the absence of such (as it is usually assumed so far). The concept of a "first-person report" is wider then the concept of "experience expression." Here is a big problem. Neither science nor philosophy has worked out any satisfactory idea of such a language (more precisely, of appropriate speech-generating procedures), which could guarantee us that (human) experience is manifesting itself via the results of correctly organized speech acts. The analysis of Turing Test (e.g., Hofstadter 1995) shows that the problem lies neither in the choice of an appropriate vocabulary nor in grammar competence. It is rooted in deeper layers of our experience and language competence. Some researchers such as Douglas Hofstadter (1995) and Daniel Dennett (1990) argue that this difference will never be found, as it simply does not exist (i.e., the nature of human brains and that of artificial brains is the same, there is no hidden difference between the two).

« 4 » One more shade of doubt. I agree that we have to be sure that "the paradigms and instruments we employ are measuring what they claim to measure" (§94). Practically it means that researchers have to play

an active role in the measuring process (§§83, 89). In some sense, this implies that researchers *force* subjects' experience to correspond to a certain theoretical framework. But the question then is: how could the amount of researchers' influence on the final outcome be measured, and how far might it go (in general)? We can consider the following example: assume you want to investigate the nature of the market economy. You want to measure certain *replicating* aspects of it, which presumably should be in line with your initial hypothesis or initial plan. So you design an appropriate experiment to allow you to force certain aspects of the market evolution to be adequate to the initial conditions of your hypothesis. The question then is the following: are we still studying the market economy, or we are already in the territory of the planned economy? Indeed, it may well happen that for the sake of replicability and in order to get rid of a certain "market noise," in fact we will force a tiny segment of the economical process (covered by the experiment) to obtain certain features of a planned economy. I am not sure that this could be addressed in the context of Baquedano & Fabar's experiment, but in general this consideration definitely has to be taken into account.

« 5 » I cannot fully agree that "psychology is the discipline with the lowest replication rate" (§89). I think that among humanity studies the lowest replication rate belongs to history. By definition, history deals with unique events with a zero replication rate, which makes it highly problematic to apply classical scientific methods to *the original content of* historical process. This conceptual puzzle was recognized more than 100 years ago, and its study resulted in a couple of significant distinctions. It was argued that the essence of the natural sciences is constituted by the generalization methodology, while methods for the humanities (Geisteswissenschaft) could be understood in terms of individuation procedures. It is important to take this into account, because history is one of the most significant arenas of human consciousness manifestation. To a great extent phenomenology is the phenomenology of historical appearances of human experience. From this point of view psychology and (neuro) phenomenology are somewhere in between

the above-mentioned extremes, with experimental psychology being closer to natural sciences, while descriptive psychology and phenomenology are closer to the other pole. This could mean that neither of them can just stick to one specific methodological paradigm. Both of them are constituted by the pressure of (at least) two counter-oriented, mutually contradicting theoretical approaches. Baquedano & Fabar show that first-person enrichment of generalization methodology will bring up a great deal of fruitful dividends to the studies within the third-person framework. We must now understand how the phenomenological dimension (of neurophenomenology) could benefit from the progress in its other dimension.

« 6 » Finally, I would like to make one more rather speculative comment. Science and analytical philosophy tend to talk about "explanations" of consciousness (as they do about any subject under consideration). However, an explanation is a very specific epistemic procedure, with a certain internal structure and specific criteria of success. Particularly it means that it is a specific type of *mode of understanding*, which is quite different from other modes and acts of human understanding (say, when one finds something funny or intriguing, or from such a mode as description). Therefore, one may ask: if we are guided in our theoretical inquiries by such a "sample of understanding" as explanation, then what kind of evidence could be provided in support of the idea that *any* kind of human understanding could be "grasped" totally by such a particular mode of understanding as "explanation"? Can a particular type of a mode of understanding exhaustively cover all other types of modes of understanding? For example, it is correct that while the language of grown-ups can successfully predict, model and explain the behavior of children it does not mean that this language will adequately serve the needs of children's expressions of *experience*. So I assume that the "explanatory gap" cannot be eliminated solely by means of explanations. Any improvement in our understanding of the "hard problem of consciousness" will depend on our ability to synthesize different *patterns of understanding* and different *samples of compre-*

hension into a multi-dimensional network of approaches, recursively intertwined and equipped with certain rules of translation of one into another.

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Modelling Subjectivity and Uncertainty in "Real World" Settings

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> Upshot · The authors show in their pilots how open it is to participants not to obey the instructions during an experiment. Their findings leave 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?; or (b) strive at designing better experiments in order to control for these fluctuations. I will argue for option (b) and propose an alternative model to go beyond the first- and third-person data gap, namely "predictive processing."

« 1 » One perennial concern within both philosophical and cognitive science is to identify the most suitable method for bridging the gap between first- and third-person data, in other words between subjective, qualitative experiences on the one hand, and objective, scientific measurements on the other. In their target article Constanza Baquedano and Catalina Fabar argue that a mild-neurophenomenology-inspired approach (Gallagher 2003; Galla-

gher & Varela 2003; Bitbol & Petitmengin 2017) might prove useful in implementing better means of handling first-person data in a reliable and productive way, providing thereby clarifications and classifications that are not captured by typical cognitive science approaches. Ultimately, this new approach is designed to address the "replication crisis" in cognitive science research (Schooler 2014) by allowing for a deeper piloting process and better paradigm design, to foster replicability and phenomenological validity. The authors have run a set of pilots in which they asked the participants to tell them how they were completing the task. Hence, they identified a number of potential confounds whereby the participants were executing the task somewhat differently from the way the experimenters had intended.

« 2 » While I applaud their attempts to tackle the very difficult question of how the neurophenomenological method can be integrated into experimental settings, I suggest that an alternative explanation of their findings is that one simply needs a better designed and controlled experimental setting in order to get rid of the confounds. In other words, while taking the fluctuations of the first-person subjective reports into consideration is an important step in achieving scientific measurements, the main challenge for a scientist remains that of setting up an experimental design that overcomes these fluctuations. Whether the study by Esther Papies, Lawrence Barsalou and Ruud Custers (2012) is the best example to follow up with a replication study is questionable.

« 3 » Consequently, the point made by Baquedano and Fabar would be stronger if they were targeting at least two studies from different research paradigms and then comparing them. Their findings leave 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.

« 4 » The Baquedano and Fabar article presents us with option (a) without considering option (b), which I consider to be the most suitable one from a scientific perspective. To sum up: the "replication crisis" in