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Author's Response
Does Naturalistic First-Person Research Need Methodological Pluralism?

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> Abstract • Addressing the methodological issues raised by the commentators, I argue that the disagreement among them regarding the optimal method to gather phenomenal data (micro-phenomenology or descriptive experience sampling) points to the constructive nature of consciousness. Then, I discuss the idea of naturalistic cognitive science (i.e., cognitive science that is relatively free of laboratory constraints). I conclude that if we are to engage in naturalistic first-person research, we must embrace methodological pluralism in order to (a) contend with the constructive nature of consciousness; and (b) account for demand characteristics.

« 1 » I am grateful to my commentators for raising concerns about various aspects in my target article. To start with, I address the methodological issues they point out (notably, favoring one of the two dominant methods in contemporary first-person research) and argue that they reflect a property of phenomenal data, which changes depending on how we observe it. I then discuss naturalistic cognitive science, and finally defend the need for methodological pluralism in naturalistic approaches to first-person research specifically, and cognitive science generally.

Methodological considerations

« 2 » The commentators raise a number of diverse issues regarding the methodological soundness of my study:

- **Bryony Pierce** in in §2 and §11, and **Katrin Heimann**, in general, suggest that investigation of the lived experience of performing a drawing task should follow the micro-phenomenological approach (MPI);
- **Cody Kaneshiro & Russell Hurlburt** suggest it should follow the descriptive experience sampling (DES); whereas
- **Urban Kordeš** focuses on the epistemological stance of the study presented in the target article. He argues that naturalistic studies may be less productive than an epistemological shift that aims at bracketing the natural attitude.

« 3 » A major problem is cleaving to specific moments. In §5, **Kaneshiro & Hurlburt** identify the problematic nature of the prompt to report on experience (i.e., "okay"), arguing that a more unambiguous call with a fast rise time would be more appropriate. The use of a language-based prompt was made to establish a rapport between the two co-researchers, and reduce the sense that the reporting co-researchers were passive subjects from whom we were extracting data. I justify the decision to use a language-based prompt by arguing that my co-researchers knew that a prompt to report on their experience was coming on account of the same epistemic stance being afforded to the principal investigator and the co-researcher. The epistemic equality of all the co-researchers informed other aspects of the study, as well. While **Kaneshiro & Hurlburt** (§21) argue that each individual session be considered anew, I aimed at engaging the co-researchers in deepening their understanding of their own experience. For instance, as their understanding of their own phenomenology increased, they created their own shorthand for aspects of their experience. Rather than seeing them attempting to classify their experience as a negative, I hold that this reflects the co-researchers' interest in investigating their experience, which may be an essential criterion for the validity of phenomenal data.

« 4 » Further, a clarification on the method is necessary. In §7, **Kaneshiro & Hurlburt** point out that the interval between memorizing and motif was too long (about 4 minutes). The entire drawing time was not the period that was investigated in the interview. In the interview, co-researchers reported on the experience from the last glance at the motif until the prompt: in other words, a time period they subjectively experienced as consisting of a single memorization and reconstruction. The preceding period of time (on the order of a few minutes)

simply allowed them to become immersed in the task at hand.

« 5 » Both Kaneshiro & Hurlburt and Heimann (in §6) point out the necessity of either "cleaving to the moment" or gathering phenomenal data while co-researchers are in the state of evocation (i.e., in contact with the lived experience under investigation). I deviated from cleaving to the moment for the reason that, within those constraints, working memory simply cannot be researched from the first-person perspective. DES and MPI are informed by a culture-specific view on temporality: the hegemony of the present moment.[1] The hegemony of the present moment is the idea that has been brought into cognitive science by the popularity of mindfulness. It refers to the claim that any experience that is epistemically significant has to have occurred in the present moment. It has even been suggested that we can draw parallels between meditation and phenomenological reduction (Kordeš et al. 2019). What these arguments lack is sensitivity to the observation that meditation amounts to a psychotechnology, a tool that allows us to modify the structure of consciousness (Hanson & Gueulette 1988). I propose that we cannot yet make a principled claim that adhering to the present moment changes how one perceives one's experience in an unbiased way. Furthermore, focusing on the present moment may preclude investigations of any number of phenomena that constitute one's experience but do not inhere in the moment, for example, existential feelings (Ratcliffe 2008) and, as I tried to show in my article, working memory.

« 6 » To explain further: the study presented in the target article was guided by the principle that a researcher should fit her method to the research question. Both DES and MPI operate on extremely small timescales. For example, MPI operates in milliseconds (Petitmengin & Lachaux 2013). By contrast, working-memory tasks may span seconds. Deviating from the established methods may yield data that is of a lower quality from the point of view of a specific approach (Kaneshiro & Hurlburt §17). However, the lower quality may be a necessary trade-off if we are to bring phenomenology into a discussion with other disciplines of cognitive science (Luhrmann 2020). The trade-off is most obvious in certain suppositions informing the construction of higher-order categories (e.g., how mental commentary was constructed as a category, Kaneshiro & Hurlburt §18). The theory-informed construction of higher-order codes was deliberate. Lower-order codes were induced from the raw data (and may reflect a pre-theoretical view of experience). In axial coding, I aimed at constructing theory-informed higher-order categories that would allow me to discuss working memory (cf. §26 of the target article).

« 7 » Moving away from methodological considerations, Kordeš (§8) suggests a more robust deployment of phenomenological reduction, i.e., divorcing oneself from the natural attitude. Per Edmund Husserl (1983: §60), the scope of concepts that are to be bracketed is vast. While an epistemological shift of the magnitude asked by Kordeš in his Q2, is a laudable goal, it may be too broad to be tenable outside of theoretical discussions. It is possible to imagine that an individual observing her own experience can bracket her natural attitude. However, one has to wonder whether it is possible to construct phenomenal data using second-person methods under such an attitude. By necessity, dyadic communication is a process of negotiation of meaning (Steels 2008). The pragmatics of a second-person interview require that meaning be grounded at the very least in naive language-based descriptors or structures of consciousness assumed by a specific phenomenological approach, such as reducing categories to bodily feelings in MPI, or mental gestures, as suggested by Kordeš in §15.

Visual representation… in a park: On naturalistic cognitive science

« 8 » The second point raised by the commentators pertains to naturalistic cognitive science, i.e., exploring cognition relatively free of laboratory-based psychological tasks. Davood Gozli suggests specifying research designs along three dimensions:

- whether activities used to measure a cognitive function amount to everyday activities or are contrived;
- whether participants are on-task or off-task; and
- whether the participants are made privy to the research goals of the study.

The three dimensions are helpful for understanding the concern raised by Pierce, in Q1. Based on her own experience with painting, she reports what she refers to as *imagined drawing*, which can be seen as an *object of remembering* whereby a planned reconstruction itself forms a mnemonic representation.

« 9 » In an earlier (and as of yet unpublished) study on the experience of performing a visual-spatial working-memory task, an experiential dynamics similar to *imagined drawing* has been observed. Often, individuals reported on an experience, whereby they memorized the initial stimulus by planning out the movements of the hand required to reconstruct the stimulus. One co-researcher reports:

> "So, there was a haptic feeling to it. […] It definitely had more of a visual element insofar as the tactic was present to me and I thought about there was this visual presence of subcomponents and scanning around them and protrusions and so there was definitely a visual part to the thought. But that visual part seems to relate to the tactics of how I will reconstruct the stimulus."

« 10 » As the above-mentioned study chronologically precedes the study presented in the target article, I was aware of the experiential dynamics whereby co-researchers memorized a stimulus by planning out its reconstruction. As there were no clear examples of *imagined drawing* (or as we could refer to it more generally in working-memory research, *imagined reconstructing*), I felt it would be intellectually dishonest to force such a category onto the data. One explanation for the reported discrepancy is that scaling down from a purely naturalistic observation of experience (i.e., Pierce observing her experience while watercoloring, being guided, perhaps, by spontaneous interest) to a scientific investigation of experience (i.e., being prompted to draw and reflect on experience by a principal investigator) obscured *imagined reconstructing*.

1| Cf. presentation "Cultural neurophenomenology of hypnosis and meditation," by Michael Lifschitz and Samuel Veissière at the FPR-McGill social and cultural neuroscience workshop at McGill University in Montreal, Canada, June 2019. https://www.youtube.com/watch?v=g9di_ZFFepQ

« 11 » Relatedly, **Toma Strle**, in Q1, wonders how constraints from the research design of the target article differ from a laboratory-based psychological task. To adequately address that question, **Gozli**'s proposed framework has to be extended when it comes to the question of whether a study employs a contrived or everyday activity. According to recent discussion in naturalistic neuroscience put forward by Pawel Matusz et al. (2019), we can analyze how artificial or life-like a given research design is according to three types of research environments:

- Naturalistic research that is entirely unconstrained, providing an insight into how a given construct functions in everyday life (e.g., research on naturalistic decision-making, explored in some detail by **Strle** in his commentary);
- Laboratory research that is highly constrained, making it possible to infer causal effects between variables;
- Naturalistic-laboratory research as an attempt to bridge the two by introducing complex stimuli or tasks not specifically designed to measure a given construct.

« 12 » Reflecting on **Kaneshiro & Hurlburt**'s elaboration on pristine experience in §9, I see that my target article falls under the category of naturalistic-laboratory research, i.e., it is exploring visual representation in a park, rather than in the wild. In order to address the question of working memory directly (rather than rely on serendipitous findings), it was necessary to move away from the idea of pristine experience. To be able to gather data that provides insight into subjective aspects of working memory in a naturalistic task, some constraints still had to be imposed on the co-researchers. A completely naturalistic (rather than naturalistic-laboratory) account of working memory would indeed have to be based on sampling experience. I am also in agreement with **Kaneshiro & Hurlburt** when, in §12, they point out that working memory is not a phenomenon that can be directly apprehended. Because it is not clear how phenomenology informs psychological constructs (such as working memory), any phenomenological investigation of psychological constructs has to make clear how it makes the connection between psychological constructs (traditionally defined according to function) and experience. In the target article, this mapping of experience to function is explained in §35.

« 13 » In responding to **Strle**'s Q1 regarding the differences between naturalistic-laboratory and laboratory research into working memory, the first difference is the non-sequential nature of the drawing task. Psychological tasks – in particular working-memory tasks – employ the block design. The participant is exposed to a rapid succession of trials that take the structure of the to-be-remembered stimulus, delay period, and the test stimulus. The temporal dynamics of these blocks is predictable, creating a reliable rhythm of task performance. In combination with the speed of presentation and limited time available for response, I argue that such tasks create pressure that is rarely felt in everyday situations (§4 of the target article). The second difference between a naturalistic-laboratory approach and laboratory research is the nature of the stimuli. In psychological tasks, stimuli are typically simplified, reduced to one or two sensory modalities (usually, a geometric shape). In everyday life, we are rarely faced with purely geometric shapes. Stimuli – in the target article, conceived of as motifs – in naturalistic-laboratory tasks are complex, such as may be encountered during everyday activities.

« 14 » The reliance on manipulating the external environment to investigate consciousness, however, is not unproblematic, as pointed out by both **Strle** (Q2) and **Kordeš** (§6). **Kordeš** notes that defining experience by external environmental constraints (as I did in §22 of my target article) amounts to a commitment to realist epistemology. However, in the target article, the motifs are merely an element of the naturalistic-laboratory set-up. Beyond forming a part of the research context, the motifs did not play a part in how the experiential categories were formed. In the coding process, I relied solely on the phenomenal data. For an illustration, consider sample VWMW-05-03-01 in the supplementary materials (https://constructivist.info/data/15/3/codebook.doc), where a bug flew onto the screen, becoming the most apparent aspect of the co-researcher's visual experience, and yet it still represented an admissible sample. However, if we are committed to the goal of making phenomenal data consistent with the natural sciences, one proposal amounts precisely to varying external demands upon the cognizer and measuring her lived experience:

> "[W]hy not henceforth conceive these situations in a quasi-experimental way by controlling, even manipulating the physical parameters of the situation? In other words, why not have recourse to what might be called 'an experimental phenomenology,' which would certainly continue to describe experience, or its lived content, but circumstances whose parameters would be controlled?" (Pachoud 1999: 211)

« 15 » Relatedly, **Strle** in Q2 points out the possibility that individuals may employ different cognitive strategies to solve the same task, if they perceive the task in different ways. While not explicitly discussed in the target article, but present in the supplementary materials, the category *attentional dispositions* may help us understand how the salient aspect of the visual field and its memorization were not unilaterally determined by the motif. Rather, they were (co) determined by the co-researchers' attitudes. *Attentional dispositions* refer to different attitudes with which co-researchers attend to an object of their awareness. In the target article, three attentional dispositions were detected (see also Table 3 in the article):

- *geometric gaze*, i.e., viewing the motif as an array of geometric shapes;
- *conceptual gaze*, i.e., viewing the motif as the object it represents; and
- *spectacular gaze*, i.e., viewing the motif as an artwork that is to be enjoyed.

« 16 » Under the attitude of the spectacular gaze, co-researchers were not engaged in solving a particular task; they were merely enjoying the motifs in front of them. The aesthetic appreciation under the spectacular gaze brings me to the question raised by **Gozli** in Q1. He wonders whether allowing participants to engage in leisurely activities would be informative for our understanding of working memory. I agree that such a deviation from using tasks (whether naturalistic or contrived) could be a productive methodological decision. Edwin Hutchins, for example, in his study of social decision-making aboard a ship, reports that for some sailors, the main focus of the job was not to aid in navigating the ship (even though they still played a role in the collective navigation

task). Rather, it was to enjoy conversations with their co-workers (Hutchins 1993). Such episodes emphasize the disconnection between the functional level of description (i.e., the task) and the phenomenological level of description (i.e., the experience). Exploring the leisurely dimension of operationalized activities might be particularly revealing in working-memory research, as the object of inquiry is traditionally defined by some sort of behavioral data reflecting task performance (Cowan 2005).

« 17 » An important point to consider when planning out naturalistic research designs is also what activity we will choose to operationalize. In Q2, **Pierce** wonders whether individuals who are more or less skilled at drawing notice and report on different aspects of their phenomenology. Upon reviewing the transcripts, I cannot point to any explicit mentions of more experienced drawers noticing different aspects of their phenomenology. However, a chi-square test of association between the co-researchers' self-reported level of drawing skills (i.e., were they classically trained artists, naive drawers or hobby drawers), and the relevant experiential categories was significant: $\chi^2(10, N=68)=27.498$, $p=0.002$. Classically trained artists and naive drawers primarily rely on *mental imagery* to draw. Hobby drawers, however, deploy the most varied array of strategies. A possible interpretation of these findings is that classically trained artists rely on *mental imagery* to project various geometric structures and ratios onto the motif (as seen in §41 of the target article), while naive drawers attempt to imagine the entire motif. Hobby drawers may attempt to develop more nuanced drawing strategies by drawing on various aspects of their life-world.

« 18 » Of course, a systematic investigation of how environments differently constrain cognition would necessitate a phenomenological re-examination of psychological tasks, as well. **Kordeš** (Q1) and **Heimann** (§2) point out that an investigation of the subjective aspects of working memory may be premature as we do not yet understand the phenomenology of working-memory tasks. I am in agreement with **Kordeš** and **Heimann**. More empirical work is needed to examine the phenomenological aspects of working memory.

A defense of methodological pluralism

« 19 » After considering the issues mentioned in the previous subsections, it is important to defend the necessity of methodological pluralism in first-person research. I will argue for methodological pluralism from three perspectives:

- that different methods yield different accounts of the same phenomenon;
- that to account for demand characteristics, we may have to step outside of a given method; and
- that methodological pluralism is in the interest of critical and democratic science.

« 20 » It may very well be that the discussion present in the commentaries and in the section *Methodological considerations* reflects a property of phenomenal data: it changes depending on how we acquire it (Kordeš & Demšar 2018). I agree with **Heimann** (§7), who points out that different methods prompt individuals to turn towards different aspects of their experience. This can be further demonstrated with empirical examples from synaesthesia research. In an ongoing case study of synaesthesia at the Sackler Centre for Consciousness Science, we are using a number of different methods to gather subjective reports. When using a closed-form questionnaire, the synaesthete reports her synaesthetic color associations (concurrents) as purely automatic. When using a broadly qualitative approach (not unlike the one presented in the target article), she reports on concurrents being tied to a specific mental gesture. When using MPI, she reports that the gesture of becoming aware of the concurrent is processual and characterized by an ever-increasing awareness of the color, accompanied by an embodied gesture of turning towards the experience. When the gesture is complete, she experiences a bodily feeling of "becoming the color." And finally, when using DES, she again reports the concurrents being automatic.

« 21 » While these findings could be interpreted as there not being any "correct" way of gathering phenomenal data, I hold that different methods' yielding of different descriptions shows that the object that is of interest for cognitive science is the constructive nature of consciousness. We should not focus only on the specific contents of consciousness, as they may be (to an extent) arbitrary. Rather, we should focus on the processes that give rise to individual objects of awareness. A deeper understanding of these constructive processes might be achievable through methodological pluralism and subsequent comparison of findings.

« 22 » Varying environmental constraints along the dimensions proposed by **Gozli** (everyday vs. contrived activity; being on-task vs. off-task; and explicitness of research goals) may not be as easy as simply changing independent variables. Such manipulations may require more drastic manipulations in research design, in turn, necessitating different methods. MPI may reveal the experience when reflectively attended to, DES may reveal experience as measured in an ecological setting, and a variety of other methods may be necessary to make phenomenal data comparable across different environmental constraints (Luhrmann 2020).

« 23 » The second reason in favor of methodological pluralism is that it may help us address the problem of demand characteristics. One has to wonder if accounting for demand characteristics is achievable by relying on method-specific approaches (as seemingly suggested by **Pierce** in §10, and **Kaneshiro & Hurlburt** in §17). Can we state *a priori* that the evocation state of MPI does not reflect a suggestion effect similar to the one seen in hypnosis research (Lush, Naish & Dienes 2016)? Can we claim that subjunctification-free speech is related solely to the iterative engagement of co-researchers (as suggested by **Kaneshiro & Hurlburt** in §13) and not also to the good-subject effect (Nichols & Manner 2008)? The commentaries have revealed that the use of the closed-form debriefing was not an adequate method to account for the validity of the phenomenal data. Further, **Gozli** points out in Q2 that there may have been a subtle element of instruction present in my style of questioning, which could have led to my co-researchers either reporting on what they considered to be desirable data or indeed caused them to experience it in earnest. In the study, I aimed to account for that by repeatedly explaining that there was no such thing as desirable data and that I

was interested in their genuine experience. However, transparency before co-researchers does not preclude a suggestion effect. It may be necessary to step outside of first-person methods, deploying quantitative instruments such as the Sussex-Waterloo Scale of Hypnotizability (Lush et al. 2018) to account for demand characteristics of this nature.

« 24 » Another approach might be giving the same epistemic status to all the co-researchers. As pointed out in Q3 by Gozli, the implementation of equal status of both co-researchers has to be further developed. One way to do it is to design a method of validating phenomenal data using participatory sensemaking, as has been suggested in Oblak (2020). Following this method means that *all* the co-researchers, rather than just the principal investigator, engage in the final stage of data analysis. This can be done by having the entire group of co-researchers jointly establish a vocabulary with which to describe their experience.

« 25 » Finally, adhering to a limited set of methods in qualitative research may lead to a social problem that Adele Clarke (2019) refers to as "methodological tribalism": concentrating epistemic value on a limited set of approaches. Indeed, methodological pluralism is not only desirable in qualitative research, but psychology, as well (Luhrmann 2020), as relying on a limited set of methods may lead to a scientific image of experience that is grounded in specific conceptions of consciousness and particular cultural ideas of what experience is. We can all agree that such an outcome is not broadly desirable.

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