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Useful Review Marked by Conceptual Vagueness

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To this day, the experimental design of Libet and colleagues (1983) is commonly applied in neuroscience research on intended action. Therefore, the review by Saigle, Dubljevic, and Racine (2018) is most welcome as it provides a useful tool for checking the consistency across the various results of these Libet-type studies. In addition, the authors construct a taxonomy of Libet-type experiments (see their Table 2 and Supplementary Tables 1 and 2), which provides a useful overview of existing variations within the Libet design. Moreover, their review highlights the inconsistencies within and between publications: Often only averages across participants are reported, no standard deviations are provided, or certain measurements are recorded but not reported. These findings emphasize the importance of concise research reports that allow valid and complete comparisons between related studies.

The main goal of the authors is to reflect on what Libet-type studies can tell us about the complex phenomenon of free will. A review of the scale they deliver provides an excellent opportunity to detect the core results that are found consistently across studies and are in some way relevant to our understanding of free will. However, in our view, the authors failed to fully seize this opportunity. First, they did not provide their definition of free will. Without such a foundation, the discussion is marked by conceptual vagueness. Second, the authors seem to take for granted that Libet studies can tell us something about the neuromechanism of free will. However, why such a mechanism would exist and what it could look like remain unspecified, which makes it harder to see exactly why and in what way Libet-type studies are relevant to our understanding of free will.

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Most importantly, in our view, it is unfortunate that the authors do not clearly specify what criteria a successful replication of the original Libet-study should meet. They seem to require an exact replication of brain signals in terms of timing and location. However, if that is the case, the comparison of all studies within their review seems a bit off the mark. In their review, they include both electroencephalogram (EEG) and functional magnetic resonance imaging (fMRI) studies. Reported timings and locations between these studies are bound to differ because fMRI studies have a much lower temporal resolution than EEG studies. Therefore, the brain signals measured in fMRI studies will be much earlier (relative to movement onset) than those reported by EEG studies. Furthermore, the authors seem to require exact replication of reported intention timings in order for a replication to be deemed successful. However, fMRI studies allow for a temporal resolution of 500 to 1000 msec for the intention reports, whereas behavioral or EEG studies allow for one of about 20 msec. This means that the authors are effectively comparing apples to pears. If they really require an exact replication of brain signals and intention reports in terms of timing and location, they should have only considered those studies that use the exact same methods as Libet and colleagues (i.e., the studies that compose Group 1).

Furthermore, we regret that the authors missed an opportunity to include a concise review of the instructions that are provided in Libet-style experiments. A difference between instructions might explain a difference in the timing of intention reports. In contrast to what the authors argue, the comparisons between different groups within a single experiment (e.g., patient and control groups) might be the strongest comparisons one could make. Within these experiments participants received the exact same instructions and performed the exact same task.

In general, the most important finding of the original Libet study is considered to be that brain signals arise before the intention reports. The reviewed studies convincingly show that the readiness potential (RP) and lateralized RP consistently occur prior to the reported intention onsets. Therefore, contrary to the authors' conclusion, they do replicate Libet's main result. The exact timing of brain signals and intention reports may differ between studies, but their order does not: Brain signals consistently precede intention reports (this is very clear from their Figure 3). One could argue that such replications, indicating the sequential occurrence of brain activity followed by the awareness of an intention, can be considered inconsistent with a metaphysical definition of free will (i.e., a will that is independent of the brain). However, in their discussion, the authors suggest that Libet-type studies may not yet be quite relevant for our understanding of free will. The main reason for this is that Libet-type studies usually investigate only simple finger flexions, which are not the main example of a layperson's definition of a voluntary action. To be sure, we ourselves have been critical of Libet-style experiments (Verbaarschot, Farquhar, and Haselager 2015; Mecacci and Haselager 2015). But the exact reasoning behind the authors' statement remains unclear. If we were to investigate complex actions in a Libet-type setting and find the same result, that is, brain signals that precede the conscious intention to act, would this be relevant to our understanding of free will?

In summary, although the authors provide a useful review of Libet-type studies, their discussion is marked by conceptual vagueness. A clear definition of the concept of free will and a specification of the criteria that a successful replication should meet are missing. Without these clarifications, the review lacks a sufficient foundation for any conclusions to be drawn. ■

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