

On experimental and philosophical investigations of mental timing: A response to commentary

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Abstract: Reinterpretations of Libet's results have received support from most commentaries. Libet's arguments against alternative hypotheses are contested. Latency depends on intensity. Integration of intensity and duration explains the Minimum Train Duration. Analogies of Libet's timing of intentions with control (flash-lag-like) experiments indicate biases of opposite signs, according to intramodal or intermodal results. Rosenthal's view of nonconscious intentions (or sensations) becoming conscious after a delay is favored. Compatibilist free will is discussed.

Key words: conscious experience; consciousness; conscious will; free will; voluntary action; cerebral timing; event-related chronometry; readiness potential; latency.

Critical points and reinterpretations of Libet's results present in my target article, as well as in my previous paper in *Consciousness and Cognition* (Gomes, 1998), have generally received support from the commentaries about the various target articles (with the exception of **Libet's** own commentary).

I. Response to Libet

Libet implies that one should not put forward hypotheses and speculative interpretations if one has not done an experimental test of them. However, in physics, which is a paradigm within scientific disciplines, the role of theoreticians is traditionally

recognized. Many physical theories and hypotheses have been published before they could be subject to experimental test.

On the other hand, if an experimenter has not considered simpler alternative hypotheses and has not controlled for them in his experimental design, the conclusions he draws from his data are as speculative as those of someone who has found these data to agree with a different interpretation. Only a suitably designed crucial experiment can decide between two alternative hypotheses.

Libet's only evidence for his backward referral hypothesis is the difference in results between peripheral-cortical (P-C) and peripheral-lemniscal (P-LM) couplings. However, he failed to observe some elementary precautions for controlling the variables. In P-LM coupling, the peripheral stimulus was a weak train of skin pulses, while in P-C coupling it was either a relatively strong single skin pulse or a visual flash! How can one consider these stimuli as commensurate?

Additionally, if he had systematically varied the intensity of the cerebral stimuli (leading to different Minimum Train Durations, MTD), he would have controlled for the effect of a possible shorter post-MTD latency¹ in P-LM couplings, relative to that in P-C couplings, when the parameters are those he used in the reported tests (Gomes, 1998, p. 584 and Fig. 6).

Contrary to what Libet states, the main alternative hypotheses I have proposed are fully testable, and the conditions of such tests were explicitly discussed in my 1998 paper (Gomes, 1998, sections 4.6 and 7, pp. 583-584, 593).

I concede that the possibility of repeated evoked responses (EP) accounting for a shorter LM latency is speculative, but the hypothesis of a shorter LM latency itself could have easily been tested. The argument of the possible role of repeated EPs was simply meant to counter the assumption that LM-latency should be similar to C-latency, by

¹ A term suggested by Ramakrishna Chakravarthi in his dissertation on Libet's findings.

showing that the two conditions of stimulation produce very different effects on the somatosensory cortex.

As in his previous reply, Libet insists that the use of weak skin stimuli in P-LM coupling would only justify increasing the sensory latency by the 30 ms of their MTD. It is a well-known psychophysical fact that latency in general depends on stimulus intensity (see **Breitmeyer's** commentary). The point is not the presence of a MTD. Suppose two supraliminal single pulses differ in intensity. In this case, there is no MTD, but the latency of the weaker stimulus may be longer, simply as a consequence of the difference in intensity (Roufs, 1963; Wilson & Anstis, 1969). Libet not only ignores this fact but also calls it a “nonsensical proposal”.

He clearly evades the methodological flaw of comparing couplings (P-LM and P-C) in which very different peripheral stimuli were used.² He also ignores my suggestion that my two alternative hypotheses might combine their effects to produce the observed result (if it is really significant; see Klein, 2002a).

II. Response to the other commentaries

The need for considering the latencies involved in the timing procedures used by Libet is supported by all commentators. The integration of intensity and duration is highlighted by **van de Grind**, **Breitmeyer** and **Bolbecker et al.**. **Van de Grind** shows that an integrator mechanism and facilitation are alternative or complementary explanations for the Minimum Train Duration (MTD) of the stimulus. The effect of stimulus intensity on latency is also emphasized by **Breitmeyer**.

The analogy of the procedure used by Libet for timing intentions with the flash-lag paradigm is also explored by **Klein** and **van de Grind**. Interestingly, **Joordens et al.** present the results of an experiment that is similar to the one I have independently suggested in my own commentary. Instead of a flashed stimulus at the center of the

² For the difference in latency between stimuli of different modalities, see Rutschmann & Link (1964).

clock, they used a change in the color of the clock as the instantaneous stimulus to be timed.

Their results (consistent with flash-lag results) go in the opposite direction of Libet's own control experiment with the timing of a skin stimulus by means of the visual clock. Whereas Libet's subjects indicated a clock position at the moment of the stimulus that was *before* its real position, **Joordens et al.**'s subjects indicated a position that was *after* the real one. This difference shows the importance of considering *intramodal vs. intermodal* paradigms, since the *visual* clock was compared to a *visual* stimulus in one case and to a *somatosensory* one in the other.³ In the case of the intention to move (Libet's attempt at an *internal psychophysics*, as noted by **Breitmeyer**), we just do not know in which direction we should expect a bias.

Joordens et al. suppose that conscious experience of initiating the movement might coincide with the onset of the readiness potential (RP), being reported as occurring later due to a bias. However, this hypothesis does not explain why we are not aware of such a long time interval between this experience and the experience of the movement itself, as there is between onset of the RP and onset of the movement. Why two events separated by about half a second or more would be experienced as occurring one immediately after the other?

Alongside the excellent neuroscientific commentaries, we are fortunate to have an illuminating philosophical analysis of the timing of conscious states by David Rosenthal. **Rosenthal** argues that there is good reason not to view consciousness as an essential property of some mental states (as Libet does). In relation to a conscious intention, we should thus distinguish the intention *of which* we are conscious from

³ The situation is even more complicated, since **van de Grind**'s comments on Zeki & Moutoussis' findings remind us that different visual events (color change or flashed form, for instance) may have different latencies for conscious perception.

being conscious of it. These are distinct mental states that occur one after the other. According to this view, there must be a delay between the intention (or sensation) and consciousness of the intention (or sensation).

Once we admit the existence of nonconscious mental states, we may consider that some become conscious while others do not. Thus **Rosenthal** can assume that the initial part of the RP corresponds to a preparatory volition that does not become conscious, while a later part of it (possibly the lateralized readiness potential, LRP) corresponds to a volition that initiates the action and becomes conscious.

Concerning free will, **Rosenthal** defends its compatibility with the neural determination of voluntary actions. His version of compatibilism, however, seems to me a bit deflationary in relation to the freedom of the will. We are often conscious of many mental antecedents of our conscious volitions (our reasons), but we feel that although these reasons may have a role in their causal determination, they are not sufficient. We feel that, if an action is free, we must have chosen it. This means that we feel *ourselves* as a necessary cause of our free actions.

The whole problem is that we are not usually inclined to view our own agency as a property of a certain neural system in our brain. If we admit this, however, we can keep our intuition that our free actions are caused “by us” and not by any external or internal determinants that do not belong to this mental/neural system (our *self*).

We may also admit that we do not become conscious of all that occurs in this system. From this perspective, we may consider even the onset of the RP as a consequence of the activity of this system, though not yet corresponding to a conscious intention to act now or to the irrevocable decision that causes the action. In this way, we can save our intuition of being the true initiators of our voluntary actions.

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