

## Real Latencies and Facilitation

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Over the centuries, the mind–body problem has proved to be a hard nut to crack. Benjamin Libet’s laudable and extensive research indicates directions that might prove to be fruitful in this regard. His hypotheses, allegedly based on data derived from experimentation, if true, have immense implications for the mind–body problem, theories of consciousness, and the concept of free will. But these hypotheses have attracted several criticisms, chief of which are those by Churchland (1981a, 1981b), Gomes (1998, 1999), and Dennett and Kinsbourne (1992). On the other hand they have been endorsed by several others such as Penrose (1989, 1994) and Eccles (Popper and Eccles, 1977). In this issue Dr. Susan Pockett presents another interpretation of Libet’s data. Following are a few comments on Pockett’s analysis of Libet’s data and hypotheses.

Pockett’s reinterpretation is simple and cogent. The main theme running through her article is that a better, simpler, and more plausible interpretation is possible if we consider the effects of facilitation, which in fact was demonstrated by Libet himself in one of his earlier articles (Libet et al., 1964). This single effect, if taken into account, has the potential, according to her, of explaining all or at least most of Libet’s data without having to resort to complex and counter intuitive theories like “backward referral of sensations.”

Before going to Pockett’s reinterpretation of Libet’s hypotheses, I would like to say that it has been pointed out (Churchland 1981a, 1981b; Breitmeyer, 1985; Bridgeman, 1985) that the data on which Libet has based his hypotheses are inadequate or do not easily yield to those hypotheses. Klein (this issue) has presented a statistical analysis supporting this view.

I would like to concentrate on her reinterpretation of the data on sensory phenomena, i.e., the data on which Libet based his “backward referral of sensations” hypothesis. First, Pockett argues that the train of cortical stimulus is not effective until the very end. Initially it is subthreshold and, due to facilitation, it reaches or crosses the threshold for sensory awareness at the end of the stimulus train. This very same interpretation was provided by Gomes (1998) and Churchland (1981a). The difference between these views was the reason cited for considering the end of stimulus train as being the point of onset of the “effective” stimulus. Pockett’s explanation is based on a well-established neurophysiological effect. Whereas Gomes (1998) ar-

Commentary on S. Pockett (2002). Commentary on Shevrin, Ghannan, and Libet, “A neural correlate of consciousness related to repression,” *Consciousness and Cognition*, 11, 342–344.

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rived at this conclusion on the basis of a finding noted in one of Libet's (Libet, Wright, Feinstein, & Pearl, 1979) experiments. Subjects were asked to compare the sensations produced by both cortical and peripheral stimuli for the purpose of making them as similar as possible to control for any unwanted biases. It was reported that the duration of the cortical sensation was felt to be equal to that elicited by a skin stimulus lasting for a time equal to the difference between the total Train Duration and the Minimum Train Duration (MTD) of the cortical stimulus. This means that a cortical sensation does not last for the entire duration of the train stimulus but only for the interval between the end of its MTD and the end of the total Train Duration. This implies that the duration of MTD is some sort of a preparatory phase for the stimulus to become *effective*. The meaning of the stimulus becoming effective is it is hypothesized that it then activates a mechanism that results in the achievement of awareness. We can say that Pockett's hypothesis provides a possible neurophysiological explanation for an interpretation that was already put forward by Gomes.

Second, the chief difference between earlier interpretations and the one proposed now is the explanation of the Peripheral stimulus–Medial Lemniscus stimulus coupling experiments (Libet et al., 1979). Gomes (1998) argues, rather convincingly in my view, that the data can be explained if one recognizes that “real latencies” (described by Gomes as the duration required for the stimulus to reach awareness once it becomes effective) for the various stimuli (cortical, lemniscal, and peripheral) could be different from each other. Moreover it can be shown that these latencies vary with the intensities of the stimuli. These premises, taken either singly or together, are sufficient to explain the data (for further details see Gomes, 1998). On the contrary, Pockett relies on facilitation to explain the same. According to her, the MTD of the lemniscal stimuli would have shortened to a large extent. This, she says, is because the lemniscal stimuli were suprathreshold and hence facilitation would have lasted for 1–4 min after the end of each stimulus train. For her, as is implicit in her discussions, the post-MTD (see below) latencies remain the same. This too easily explains the data. In summary, the difference between the two interpretations is that Pockett believes that the MTD changes due to facilitation whereas Gomes opines that it is the post-MTD latencies that are different among the various stimuli.<sup>2</sup> Both explain the data well. Only further experimentation can decide between the two. A simple experiment that could help us decide is, as suggested by Pockett herself, determining the MTD of lemniscal stimuli before and after a series of stimulus administrations at a constant suprathreshold intensity with an interstimulus interval of about a minute. Since this particular experiment is difficult to perform, we could substitute lemniscal stimulation with, if it were possible, stimulation of the sensory cortex by means of trans-cranial magnetic fields (Noordhout et al., 1989; Hess & Ludin, 1988). If the MTD decreases significantly, then Pockett's hypothesis is strengthened. On the other hand, if it does not then we cannot say much about Gomes' hypothesis but Pockett's proposal will certainly hold less water.

Third, a note on “real latencies.” This was the term coined by Gomes (1998)

<sup>2</sup> The reason given by Gomes is that the stimuli are sufficiently different from each other physiologically that it is quite possible that the post-MTD latencies are different.

and as mentioned above it is the duration required by a putative “consciousness mechanism” to result in awareness once a stimulus becomes effective. I prefer to call it “post-MTD latency,” as it is effectively the time interval between the end of MTD and the time of onset of awareness. Also, since it is the only relevant latency that is experimentally important, the term “post-MTD latency” better describes its reliance on an experimental setup and not on a theoretical position. Pockett’s analysis of the evoked potentials led her to believe that the duration of this latency could be around 80 ms. Therefore she supposed that there could be a delay of 0–80 ms from the end of the MTD until the achievement of awareness. Churchland’s (1981b) experiments, involving a rapid verbal response to awareness of sensations, resulted in her postulating a latency of around 150–175 ms. Gomes (1998) arrived at a similar figure. His estimate of the latency for peripheral stimuli was  $\geq 230$ ms. This was based on his analysis of data from peripheral–cortical stimuli coupling experiments (Libet et al., 1979). He also suggests that the latency for cortical stimuli is about the same duration as that for peripheral stimuli and that the latency for lemniscal stimuli is shorter. If the backward masking experiments are considered indicative of the duration of the neuronal activity leading to a sensation, which is indeed highly debatable, then it can be calculated that the peripheral latency is of the duration 125–200 ms (Gomes, 1998). Thus, there are several estimations of the latencies. All these are based on different approaches to Libet’s data. Which of these reflect reality cannot be decided without further experimentation.

Finally, I would like to add a point regarding Pockett’s reinterpretation of the data on motor experiments. When she talks about the usage of the oscilloscope to time events in the motor experiments, she argues that a person noting the clock-time that he feels to be simultaneous with a given event (like the awareness of movement) is actually noting a clock position that is about 80 ms in the past. This is because it takes time for the visual cue to reach awareness, which probably takes 80 ms of processing (see above). This 80 ms has to be deducted from the noted clock-time to obtain the actual time of occurrence of the event. My point is that Pockett is ignoring the fact that the proprioceptive impulses from the activated muscle also require a similar, if not the same, amount of time to reach awareness. So, the durations of the two events—the visual stimulus of the clock and the awareness of movement—cancel each other out (or leave a deficit of a few milliseconds). In other words, suppose the muscle contracts at time  $t$ ; it would reach awareness at  $t + 80$  ms (say). The person notes the time at  $t + 80$  ms. But since visual awareness also requires around 80 ms of neuronal processing, the actual time noted would be  $t + 80 - 80$ , i.e., at time  $t$ ! Therefore Pockett’s theory does not explain the observed discrepancies in Libet’s data on motor experiments (the movement being “felt” 85 ms before actual movement and the peripheral sensation being felt 50 ms before administration). A possible solution, apart from Pockett’s observation that it could be due to a large variation in the data, is postulating a different latency for proprioceptive impulses (which is possible because they are physiologically different from other exogenous stimuli). If it were short (say, 20–30 ms) then the data would be explainable. For example, if the visual stimulus takes about 120 ms (as Pockett points out, visual processing is more complex and could take a longer time) to reach awareness and if proprioceptive impulses require a latency of 30 ms, then the movement will be

felt at  $30 - 120 = -90$  ms, i.e., 90 ms before the movement actually takes place. This is exactly what was observed in Libet's (Libet, Gleason, Wright, & Pearl, 1983) experiments.

In summary, Pockett's simple and elegant hypothesis provides a possible neurological explanation of a part of already existing interpretations of Libet's data. The differences among the proposals are the explanation of the peripheral–lemniscal stimuli coupling experiment and the duration of the latencies. Only experimentation can adjudicate between these alternate hypotheses. On the whole, these interpretations, including Pockett's, are naturalistic and avoid postulating radical and conceptually unsatisfactory theories such as the “subjective backward referral” hypothesis.

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