

The Interpretation of Libet's Results on the Timing of Conscious Events: A Commentary

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A commentary on articles by Klein, Pockett, and Trevena and Miller, in this issue, is given. Average shift in the point of subjective equality (PSE), calculated by Klein on Libet's data, and corresponding change in mean shift, calculated by Libet et al. (1983), may be "corrected," taking as a reference point the end of the minimum train duration. Values obtained, if significant, indicate a latency for conscious sensation of the skin stimulus of at least 230 ms. Pockett's main conclusions are favored, but her explanation of peripheral–lemniscal couplings is found to be unconvincing. Trevena and Miller's article unsuccessfully tries to rescue a dualist interactionist view. Libet's method of timing intentions is thoroughly criticized. © 2002 Elsevier Science (USA)

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The articles by Klein (2002), Pockett (2002), and Trevena and Miller (2002), in this issue, bring new fuel to the debate over the proper interpretation of the results of Libet's research on the timing of conscious events.

I

Klein's article represents an important contribution to the subject, showing again, with the aid of a different method of analysis, the weakness of the available data. Here we learn that the shifts in the point of subjective equality (PSE) [and certainly the difference in shift between peripheral–cortical (P–C) and peripheral–lemniscal (LM) couplings as well] are not impressive in relation to two measures calculated by Klein. One is the discrimination threshold (as measured by the slope of the psychometric functions) and the other is PSE uncertainty (as measured by the separation between the two criterion PSEs, which reflects the occurrence of many experiences of simultaneity with different asynchronies) (Klein, 2002).

One of the problems encountered by Klein was, for subject CJ, the impossibility of adequately measuring PSE₁, since with no tested asynchrony did the subject perceive the cortex stimulus before the skin stimulus. The Minimum Train Duration (MTD) was about 300 ms in this case. If we consider that the most important reference point should be the end of the MTD (Gomes, 1998; Pockett, 2002), the asynchronies to be tested should be –300 ms and points before and after –300 ms.

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TABLE 1
Asynchrony Change Necessary to Keep Simultaneity of Sensations When the Second Skin Stimulus
Is Replaced by a Cerebral Stimulus (C or LM)

	Subject	Libet's change in mean shift (-1)	Klein's average PSE shift	Libet's values "corrected" for end of MTD	Klein's values "corrected" for end of MTD
P-LM coupling	HS	12	56	262	306
	GS	20	-11	195	164
P-C coupling	JW	-220	-148	-20	52
	CJ	-454	(-343)	-154	(43)

Note. "Corrected" values use the end of the MTD (when there is a MTD) instead of onset as the reference point for asynchrony. Values indicate ms to be added to the reference point of the second stimulus.

Instead, Libet tested -200 , 0 , and $+200$ ms, none of which produced perception of the cortical stimulus first (Libet et al., 1979). This shows a failure of the experimental design, which was biased by Libet's previous hypothesis of a similar time of "neuronal adequacy" for cortical stimuli at liminal intensity and peripheral stimuli.

The average PSE, according to Klein's calculations, corresponds to Libet's "mean shift" and Klein's "average PSE shift" corresponds to Libet's "change in mean shift." In my 1998 article, I suggested a "correction" of Libet's figures for the "change in mean shift" by subtracting the cerebral MTD (and adding the peripheral MTD present in P-LM couplings) (Gomes, 1998, p. 577). In this way, we take the end of the MTD as the reference point for the comparison of cerebral and peripheral stimuli. The same can be applied to Klein's numbers. Table 1 shows Libet's original and corrected values for the "change in mean shift" and Klein's original and corrected "average PSE shifts." [For CJ, due to the problem mentioned above, Klein estimates $PSE_1 < -400$ ms. If we use -400 ms as PSE_1 , we arrive at an average PSE of -343 ms, indicated in parenthesis in Table 1. Subject MT was not considered here since in his case a completely different stimulus (a flash) was used.]

We can certainly see a difference between P-LM and P-C couplings, which was explained by Libet with the backward-referral hypothesis. However, this difference, even if it is significant, can be explained by two alternative hypotheses, as mentioned below. "Corrected" values for JW indicate simultaneity of sensations when the peripheral single pulse stimulus coincides approximately with the end of the MTD of the cortical stimulus. According to my interpretation of the data, "corrected" values for P-LM couplings may indicate (if significant) that the latency for conscious sensation of the skin stimulus used in this case is about 230 ms longer than any latency for conscious sensation occurring after the MTD of the LM stimulus. (The mean of Libet's "corrected" LM values is 228.5 and the mean of Klein's "corrected" LM values is 235.)

Klein (2002) argues that, although Libet's data do not support it, backward referral in time is a sound idea and useful for the organism to implement. Since the subject has no absolute time reference, I think it is inadequate to speak of either backward or forward referral. For the subject, as regards the timing of his or her conscious

sensations, succession or simultaneity of sensations is all there is.¹ Klein argues that synchronization of sensations caused by simultaneous events, even if they would otherwise have different latencies for conscious sensation, should be useful for the organism. This may well be so, but we do not know if the mechanism for such synchronization exists. If it does, it is not always effective, as prior entry and flash-lag² effects show. Anyway, this synchronization mechanism must operate before sensations become conscious. If the subject has had conscious sensation *a* and then has had conscious sensation *b*, such synchronization is no longer possible.

A different situation involves some cases mentioned by Baars (Psyche-B on-line discussion, August 20, 2000, available at <http://listserv.uh.edu/archives/psyche-b.html>) in which cognitive processing attributes a property that was discerned later, to an event that was perceived earlier and is either retained in working memory or retrieved from long-term memory. Regarding these cases, one may wish to use the term “*backward referral*.”

II

Pockett’s article gives a clear and didactical presentation of criticisms and reinterpretations already put forward by other authors (Churchland, 1981a, 1981b; Glynn, 1990, 1991; Gomes, 1998) and adds some new points as well. Its main point, with which I fully agree, is that there is no good reason for accepting either Libet’s backward-referral hypothesis or his hypothesis that it takes about 500 ms to become aware of sensations.

Pockett adopts the view that most of the Minimum Train Duration of the cerebral stimuli used by Libet should be considered as a preparatory part (Gomes, 1998). As her Fig. 3 and my Fig. 4 (Gomes, 1998) show, when the skin stimulus is presented 200 ms *after* onset of the cortical stimulus (having a Minimum Train Duration of 500 ms), it occurs in fact 300 ms *before* the effective part of the cortical stimulus. Consequently, there is no reversed order of sensations and no need for backward referral. Pockett goes further than this in assuming that the physiological mechanism of facilitation is the basis for what I called “the building up of a brain state that makes subsequent stimulation effective in producing conscious sensation” (p. 566). She assumes that each pulse simply creates a facilitation for the next one, up to the point when the threshold for conscious sensation is reached.

¹ The concept of spatial referral from the brain to some part of the body is no more justified than that of temporal referral. It derives from a confusion between features of representings and features of representeds (Neumann, 1990, cited in Dennett & Kinsbourne, 1992). When we touch something with our finger, what we feel is represented as occurring on the tip of our finger. The representing occurs in our brain, but there is no need to “refer” this representing to the represented. (The medical concept of *referred pain* involves a different situation, since here the referral is from one part of the body to another, both represented by the conscious subject.)

² The flash-lag effect has been interpreted by Nijhawan (1994) as due to an extrapolation of the position of moving objects in order to favor perceptual–motor synchronization. However, the phenomenon has also been explained as the effect of differential latencies, postdiction, or attentional effects. Anyway, the fact remains that the perception of the flash is not synchronized to that of the moving object. Two adjacent positions that occur simultaneously in reality are perceived one after the other. (See Baldo, Kihara, Namba, & Klein, 2002.)

Coincidence of sensations when the skin stimulus is presented at the end of the cortical Minimum Train Duration is explained, following Churchland and Glynn, by supposing roughly the same delay after the skin stimulus and after the cortical Minimum Train Duration.

However, Pockett's explanation of the difference in results between the coupling of skin stimuli with cortical stimuli and the coupling of skin stimuli with lemniscal stimuli is not convincing. In relation to the latter, there seems to be coincidence of sensations when the skin stimulus is presented at the beginning of the lemniscal one (Libet et al., 1979). Pockett explains this as a result of intertrial facilitation that could have happened since the intensity used was higher than liminal. This would have greatly reduced the lemniscal Minimum Train Duration without Libet's knowledge. I find it unlikely that Libet could have missed the presence of such intertrial facilitation. Anyway, any intertrial facilitation, if present, would have also affected the coupling of skin stimuli with cortical stimuli, since the experimental design was similar in both cases. Results would have also had to be similar, but they were not.

This difference (if really significant) can be easily explained, however, without appeal to backward referral, by the two alternative hypotheses I have presented. One involves a shorter latency after the lemniscal Minimum Train Duration and the other a longer latency of the skin stimulus, since its intensity was weaker (Gomes, 1998).

III

Trevena and Miller's article seems designed to rescue a Cartesian interactionist view of the mind-brain relation, according to which the mind acts on the brain to initiate voluntary actions. This position begs the question of whether the mind supervenes on complex sets of brain activities. If it does, to say that the mind acts on the brain is merely to say that some brain activities act on other brain activities to initiate voluntary action. If it does not, mentally caused brain activities should not be systematically preceded by other specific brain activities, since it is the immaterial mind that causes them.

However, Kornhuber and Deecke (1965) found specific brain activity that precedes voluntary movements by a whole second or more. Libet et al. (1982) found that even with instructions that favored spontaneity, the *Bereitschaftspotential* (or Readiness Potential, RP) preceded movement by at least 200 ms, typically by about 600 ms. Since people usually do not have the subjective experience of an interval of such an order of magnitude between their conscious decision to move and the movement itself, this poses a problem for the dualist view that a nonneural conscious decision causes the movement (Gomes, 1999). Libet found he needed an objective measure of this subjective experience, so he devised a method of timing the conscious intention to move (Libet et al., 1983a). This method has been criticized by many and should be considered as essentially unreliable (Breitmeyer, 1985; Rollman, 1985; Scheerer, 1985; Stamm, 1985; Davis, 1987; Gomes, 1998). Anyhow, his results supported the idea that a specific brain activity precedes the conscious decision.

Now Trevena and Miller (2002) try to find arguments against this conclusion. They accept Libet's method of timing the decision to act, despite the criticism it received. However, they raised the possibility that a smearing artifact might account for the

conclusion that the RP precedes the conscious decision (timed by this method). Previous studies had confronted onset of the RP with the mean timing of the decision. However, since the RP reflects a large number of trials, it might be that the timing of the decision had preceded the neural events underlying the RP in each case, even if the *mean* timing of the decision is later than the RP *onset*. So, in their replication of Libet's study, Trevena and Miller examined the times of the earliest reported decisions. Results, however, did not confirm this suspicion: The RP was present before all timings of the decision to move.

They also examined a second alternative explanation that seemed to rescue the Cartesian view. Following Haggard and Eimer (1999), they argue that perhaps the Lateralized Readiness Potential (LRP), and not the RP, is the specific brain activity related to voluntary movement. The LRP (not really a potential, but a difference between potentials) is based on the contralateral preponderance of negativity observed by Deecke et al. (1976, pp. 99, 103, 110) over the motor cortex. Haggard and Eimer had found that the LRP also precedes the mean timing of the decision to move. However, the smearing artifact could perhaps explain this. In fact, Trevena and Miller found that about 20% of the decision timings were earlier than the LRP. This is compatible with the possibility that the timing of the conscious decision preceded in every trial the neural events underlying the LRP.

However, this does not seem enough to save the interactionist view. Even if we admitted the validity of the decision timings, it is still true that the RP precedes these timings. Trevena and Miller argue that the RP might reflect "some general preparatory activity" or "general anticipatory processes." But this is still problematic for the dualist view. If the mind does not supervene on brain activity, how can the brain prepare or anticipate a decision that the mind has not yet taken? How could this anticipatory activity occur, even if, as the authors argue, it is not specifically motor? The authors say the RP might be "an indicator of a movement that is being contemplated in the future." But the subject is not conscious of starting a specific contemplation of the movement some time before the decision. Thus why not consider this unconscious "contemplation of a future movement" as its unconscious neural initiation?

IV

Libet's method of timing the conscious intention (or decision) to move (Libet et al., 1983a) has been used in at least three other studies (Keller & Heckhausen, 1990; Haggard & Eimer, 1999; Trevena & Miller, 2002). However, it presents many problems, as outlined below.

1. Libet's rationale for the method includes acceptance of his backward-referral hypothesis (Libet, 1985b, p. 559). Latency for conscious perception of clock position would be automatically corrected by this mechanism. However, if there is no good reason to accept this hypothesis (Churchland, 1981a, 1981b, Glynn, 1990, 1991; Gomes, 1998; Pockett, 2002), this rationale must be revised.

2. Mean timing of the intention to move (*W*) varied from about 200 ms before EMG in Libet et al. (1983a) and Keller and Heckhausen (1990) to 122 ms before keypress in Trevena and Miller's Experiment 1, about 80 ms before keypress in their

Experiment 2, and about 350 ms before keypress in Haggard and Eimer (1999) [corresponding to approximately 80, 40, and 310 ms before EMG, respectively, if we allow 40 ms between EMG and keypress (Trevena & Miller, 2002)]. This wide variation indicates that small differences in instructions may lead to different timings and suggests that the event to be timed was artificially produced by instructions. There was also wide individual variation in timings in all studies. In Libet et al. (1983a), W varied from -54 to -422 ms and in Keller and Heckhausen (1999) from $+806$ to -362 ms. Trevena and Miller (2002) report that 40% of W s in Experiment 1 were after keypress, whereas many were 400 ms before, one subject having given W s of more than 1 s before keypress in 33 trials. Haggard and Eimer (1999) separated W s into two groups (early and late), obtaining mean W values of -530 ms for the early W group and -179 ms for the late W group. This wide variation across subjects (and across sessions with the same subject) is also suggestive against the validity of the measure.

3. Latency for becoming conscious of a certain clock position must be taken into account and alters in an unknown way the timings obtained by observation of the clock (Rollman, 1985).

4. Possible latency for becoming conscious of the intention to move must also be considered. If consciousness is considered as an intrinsic property of the intention, it is present as soon as the intention occurs. However, consciousness *of* the intention may also be considered as distinct from the intention itself (Davis, 1987; Bittner, 1995; Gomes, 1999). This would be in accordance with a higher order theory of consciousness (Rosenthal, 1986). If so, it takes some unknown amount of time to become conscious of the intention.

5. Libet's situation in timing intention may be compared to the flash-lag phenomenon (Nijhawan, 1994; Baldo et al., 2002), in which an instantaneous visual stimulus is perceived as lagging behind the simultaneous position of a continuously moving stimulus. As a control for W timings, Libet et al. (1983a) had their subjects time a single pulse applied to the skin (S), using the same revolving spot as a clock. Results were the opposite of what is found with visual stimuli: It is the revolving spot that lags behind the timing of the skin stimulus. In fact, for the flash-lag experiment to be strictly comparable to Libet's, the flashed stimulus should be presented at the center of the circle described by the moving one, and subjects would then have to tell the position of the moving stimulus at the moment the spot was flashed. Would one obtain the same flash-lag? Why is there a pulse-lead in the case of Libet's pulse to skin?

We can see that the situation is complicated. Perhaps the difference is due to observation and reporting conditions. Perhaps it is due to different latencies for the two sensory modalities. We do not know. And we know still less about what the temporal relations may mean when a conscious intention is substituted for the instantaneous peripheral stimulus.

6. Three different types of intentions should be distinguished, all of which may be conscious. The first is the prior intention (an intention to do something at some time *in the future*). This is certainly present in the Libet situation (the intention to comply with instructions), as many commentators have noted, but this does not

exclude that *the moment* of performing the movement was freely chosen by the subject.

The second is the intention to act now. This intention may be involuntarily aborted in some cases, as Libet's subjects have reported. Sometimes, they experienced the intention to move now, but the movement just did not occur (Libet, 1985a, p. 538). This seems to be related to the observation by Fried et al. (1991) that weak stimulation of the frontal cortex in Brodmann's area 6 caused patients to report an urge to move a specific body part or a feeling that they were about to move, while a stronger stimulation caused actual movements of the same body part (Haggard, in Haggard & Libet, 2001, p. 53).

This intention to act now may also be voluntarily vetoed by the subject. Libet et al. (1983b) instructed their subjects to perform a sudden hand movement at preset times. In other series of trials, subjects were told to prepare to move at the preset time and veto the intention just before this moment. Despite the strangeness of this request, subjects reported experiencing the intention and the first part of the RP was recorded from such trials.

It seems that one may also program oneself to interpose a certain time interval between this intention to act now and actual motor performance. I may "say" a mental "go" and make the movement after a shorter or longer moment if I wish to do so. In all four studies using Libet's method of timing intentions, it seems likely that the task of noticing the spot position at the moment of the intention that precedes the movement may have induced the intercalation of a small interval between the intention and the performance of the movement.

The third type of intention is what I have called "the irrevocable decision to act now" (Gomes, 1999). This is considered to be the immediate cause of the motor commands that lead to muscular contraction. Once it occurs, movement can no longer be normally avoided. In actions that involve a continuous movement or a series of movements, it is likewise extended in time and lasts as long as the action. In this case, it may be likened to Searle's (1983) concept of intention-in-action.

The need for the concept of this third type of intention is given by the fact that a simple conscious intention to act now (of the second type) is neither sufficient nor necessary for the initiation of the movement. Examples have been given of cases in which this conscious intention to act now is not followed by actual performance of the act. On the other hand, we often start voluntary actions that are not preceded by a conscious intention to do so. Searle (1983) gives the example of someone who is thinking about a difficult philosophical problem and suddenly starts walking across the room. The action is clearly voluntary and conscious, but there was no conscious intention preceding its start. Many (perhaps most) of our actions are so spontaneous that we are not at all conscious of a preceding intention. This is very clearly so in the case of impulsive acts or utterances that we sometimes regret having carried out.³

³ One may hypothesize that an intention of this second type, as a representation of the act that immediately precedes its performance, is present in all voluntary acts, albeit unconsciously. Consciousness of it might be precluded by attentional or emotional factors or simply by the fact that the act was so quickly performed that consciousness of this immediately previous representation of it merges with consciousness of the act itself.

We do not experience this irrevocable decision to act now as a separate mental event. We experience it as part of the experience of the action itself. In a sense, this kind of intention is *in the action*. However, we may clearly be conscious of this third type of intention in the sense that we can clearly distinguish an intentional movement from an involuntary one, such as a tic or a muscular spasm. Even some movements that might be seen as “purposeful,” such as stepping on an imaginary break when riding in a car that comes close to an obstacle, are experienced as automatically performed reflex acts and we are not conscious of the presence of an intention in them. By contrast, an impulsive act, which we will perhaps later regret, is nevertheless experienced as “our own,” as something we did because we wanted to. We are conscious of an intention that accompanies the act, or is present in the act itself, and we feel that this intention *caused* the act.

Now which type of intention is involved in Libet’s method of timing conscious intentions? If the experimenters had simply asked the subjects to perform the movement *as soon* as they wanted to, in a sudden and abrupt way, they would probably report that they were just conscious of performing an intentional movement and that they were not clearly conscious of an intention to move before they were conscious of the movement itself (Breitmeyer, 1985). Even if they had asked the subjects to notice the position of the revolving spot at the time of the movement, the same would probably occur. Conscious intentions of the first and third types described above would be present, with no conscious intention of the second type. However, when the experimenters ask the subjects to pay attention to the position of the revolving spot at the moment of “the intention to move that precedes the movement,” they artificially produce some consciousness of the second type of intention.

Besides, since subjects have to identify the exact moment in which this intention occurs, they tend to create a sort of mental “go,” an introspectable mental order to themselves that would not be normally present. This was reported by several people whom I asked to perform a similar task. Moreover, since the intention must be clearly distinguished from the act itself (or else subjects would not be complying with instructions), they will probably tend to introduce a shorter or longer interval between this mental “go” and the movement. This “go” is what will be timed by noticing the perceived position of the revolving spot. The interval between the time of the real position of the revolving spot corresponding to this perceived position and the time of the movement will then be considered as the “timing” of the conscious intention.

In conclusion, all these problems cast doubts on the validity of this method of timing the intention to move. We may view it as giving an unreliable measure of the time of an artificially created mental event relative to a movement that is itself “timed” by the subject to occur at a certain indeterminate interval after the mental event to be timed.

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