Chapter 13
Free Will and Neuroscience: Revisiting Libet's Studies

Alfred R. Mele

Abstract Benjamin Libet contends both that "the brain 'decides' to initiate or, at least, prepare to initiate [certain actions] before there is any reportable subjective awareness that such a decision has taken place" and that "If the 'act now' process is initiated unconsciously, then conscious free will is not doing it." Elsewhere, I have argued that the claims I just reported are not justified by the data Libet and others offer in support of them. Here I review some of the problems one encounters in attempting to move from Libet's data to his conclusions.

Keywords Libet's experiments • Free will • Awareness • Readiness potential • Proximal decisions and intentions • Distal decision and intentions • Conscious decisions

13.1 Introduction

Benjamin Libet contends both that "the brain 'decides' to initiate or, at least, prepare to initiate [certain actions] before there is any reportable subjective awareness that such a decision has taken place" (Libet, 1985, p. 536)[1] and that "If the 'act now' process is initiated unconsciously, then conscious free will is not doing it" (Libet, 2001, p. 62; see 2004, p. 136).[2] He also claims that once we become conscious of our decisions, we can exercise free will in vetoing them (1985, 1999, 2004, pp. 137–149). Some people follow Libet part of the way: they accept

[1] Elsewhere, Libet writes: "the brain has begun the specific preparatory processes for the voluntary act well before the subject is even aware of any wish or intention to act" (1992, p. 263).

[2] For a useful discussion of what the initiation of an action might amount to and of connections among action initiation, Libet's data, and free will, see Bayne (2011).

A.R. Mele (✉)
Florida State University, Tallahassee, FL, USA
e-mail: amele@fsu.edu

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his claims about when and how decisions to act are made but reject the window of opportunity for free will as illusory (Hallett, 2007; Wegner, 2002, p. 55).

Elsewhere, I have argued that the claims I just reported are not justified by the data Libet and others offer in support of them (Mele, 2009). Here I review some of the problems one encounters in attempting to move from Libet's data to his conclusions.

13.2 Libet's Studies

Libet's findings are based on a creative series of studies (for summaries, see Libet, 1985, 2004). In some of the studies, subjects are regularly encouraged to flex a wrist whenever they wish. In subjects who do not report any advance planning of their movements, electrical readings from the scalp (EEGs)—averaged over at least 40 flexes for each subject—show a shift in readiness potentials (RPs) beginning about 550 milliseconds (ms) before the time at which an electromyogram (EMG) shows relevant muscular activity to begin. These are type II RPs. Subjects who are not regularly encouraged to aim for spontaneity or who report some advance planning produce RPs that begin about half a second earlier—type I RPs. The same is true of subjects instructed to flex at a prearranged time (Libet, Wright, & Gleason, 1982, p. 325). (According to a common use of "readiness potential," it is a measure of activity in the motor cortex that precedes voluntary muscle motion and, by definition, EEGs generated in situations in which there is no muscle motion do not count as RPs. Libet's use of the term is broader. For example, because there is no muscle motion in the veto experiment I describe later, some scientists would not refer to what Libet calls "the 'veto' RP" (1985, p. 538) as an RP.)

Subjects are also instructed to recall where a dot was on a special clock when they first became aware of something, x, that Libet variously describes as a decision, intention, urge, wanting, will, or wish to move. (The dot on this Libet clock makes a complete revolution in less than 3 s.) On average, the onset of type II RPs preceded what subjects reported to be the time of their initial awareness of x (time *W*) by 350 ms. Time *W*, then, preceded the beginning of muscle motion (a muscle burst) by about 200 ms. The results may be represented as follows:

Libet's results for type II RPs

−550 ms	−200 ms	0 ms
RP onset	Reported time *W*	Muscle begins to move

(Libet finds evidence of what he regards as an error in subjects' recall of the times at which they first become aware of sensations (1985, pp. 531, 534). Correcting for it, time *W* is −150 ms.)

Again, in Libet's view, consciousness opens a tiny window of opportunity for free will in his subjects. If a subject becomes aware of his decision or intention at −150 ms, and if by −50 ms his condition is such that "the act goes to completion with no possibility of its being stopped by the rest of the cerebral cortex" (Libet, 2004, p. 138), his window is open for 100 ms. Libet writes: "The role of conscious free will [is] not to initiate a voluntary act, but rather to control whether the act takes place. We may view the unconscious initiatives as 'bubbling up' in the brain. The conscious-will then selects which of these initiatives may go forward to an action or which ones to veto and abort" (1999, p. 54).

13.3 Conceptual Background

Some conceptual background will prove useful for the purposes of assessing the implications of Libet's findings. I start with the concept of deciding to do something—practical deciding. (Deciding that something is true is a distinct phenomenon.) Like many philosophers, I take *deciding* to *A* to be an action—as I see it, a momentary action of forming an intention to *A* (Mele, 2003, ch. 9). Deliberating about what to do is not a momentary action, but it must be distinguished from an act of deciding that is based on deliberation.

This conception of practical deciding does not entail that all intentions are formed in acts of deciding. In fact, many intentions seem to be acquired without being so formed (see Mele, 2003, ch. 9). If, as I believe, all decisions about what to do are prompted partly by uncertainty about what to do (2003, ch. 9), in situations in which there is no such uncertainty, no decisions will be made. Even so, intentions may be acquired in these situations.

Some decisions and intentions are about things to do straightaway. They are *proximal* decisions and intentions. Others—*distal* decisions and intentions—are about things to do later. Ann's decision to phone Al now is a proximal decision; her decision to phone Bob tomorrow is a distal decision. Libet's attention to decisions and intentions is focused on the proximal kind.

Deciding to do something should be distinguished from wanting (or having an urge) to do it. Sometimes people want to do things that they decide not to do. And often, when people want to do each of two incompatible things—for example, meet some friends for lunch at noon and attend a lecture at noon—they settle matters by deciding which one to do. Just as deciding should be distinguished from wanting, so should intending. Intending to do something is more tightly connected to action than is merely wanting to do it.

For critiques of alternative accounts of deciding, see Mele, 2003, ch. 9. A virtue of the account just sketched, for the purposes of this article, is that it is consonant with Libet's apparent conception of practical deciding.

13.4 What Happens at −550 ms?

One inference Libet makes on the basis of his findings is that the brain produces a proximal decision or intention to flex about one-third of a second before the subject becomes aware of that decision or intention. Is this inference warranted?

An alternative hypothesis is that what the brain produces around −550 ms is a potential cause of a subsequent proximal decision or intention to flex and the decision or intention emerges significantly later.

How might one get evidence about whether the onset of the type II RPs at −550 ms is correlated with unconscious proximal decisions or intentions to flex or instead with potential causes of decisions or intentions? An apt question to ask in this connection is how long it takes a proximal intention to flex to generate a muscle burst. If, in fact, the brain produces proximal decisions or intentions in Libet's study about 550 ms before the muscle burst, then, in his subjects, it takes those decisions or intentions about 550 ms to produce a muscle burst. Is this a realistic figure?

Some reaction-time studies provide relevant evidence. In a study in which subjects were watching a Libet clock, the mean time between the sounding of the go signal and the muscle burst was 231 ms (Haggard & Magno, 1999, p. 104). Subjects were instructed to respond as rapidly as possible to the go signal by pressing a button. If detection of the go signal produced a proximal intention to press the button, then the mean time between a subject's acquiring a proximal intention to press and the muscle burst was less than 231 ms. (Detecting a go signal takes time.) And notice how close this is to Libet's time *W*—his subjects' reported time of their initial awareness of something he variously describes as an intention, urge, wanting, decision, will, or wish to move (−200 ms). Even without putting much weight on the exact number (−231 ms), one can fairly observe that if proximal intentions to flex are acquired in Libet's studies, the finding just reported makes it look like a much better bet that they are acquired around time *W* than that they are acquired around −550 ms.

Someone might object that in reaction-time studies of the kind described, muscle bursts and actions are not produced by proximal intentions but by something else. It may be claimed, for example, that the combination of subjects' *conditional* intentions to press whenever they detect the go signal together with their detection of it produces muscle bursts and pressings without the assistance of any proximal intentions to press. (A typical conditional intention has this form: "if [or when] x happens, do y.") But if this claim is accepted, a parallel claim about Libet's studies should be taken seriously. The parallel claim is that, in Libet's studies, the muscle bursts and actions are not produced by proximal intentions but by the combination of subjects' conditional intentions to flex whenever they detect a conscious proximal urge to flex together with their detection of such an urge. Someone who makes this claim may hypothesize that the onset of the type II RPs at −550 ms is correlated with a potential cause of a conscious proximal urge to flex. Libet's findings do not contradict this hypothesis.

Even if Libet is wrong in claiming that the brain produces proximal intentions or decisions to flex at about −550 ms, his claim about the 100 ms window of opportunity for free will merits attention. Libet's idea is that free will can only be exercised consciously and, therefore, can only be exercised after his subjects become conscious of proximal intentions, decisions, or urges to flex (and before it is too late to stop what is in place from generating a flex). He contends that free will can be exercised only in vetoing the decision, intention, or urge of which the person has become conscious. An alternative hypothesis is that Libet's subjects exercise free will in making conscious proximal decisions to flex rather than after they become conscious of such decisions (or intentions or urges). Given that Libet's findings do not justify the inference that proximal decisions to flex are made before the subjects are conscious of any such decision, they do not contradict the present hypothesis.

13.5 What Happens Between −550 and 0 ms?

Libet's findings are sometimes said to support the thesis that conscious intentions and decisions play no role in producing corresponding actions. It is claimed that they are caused by the same brain events that cause actions and that they are not themselves in the causal chain that results in action (Lau, Rogers, & Passingham, 2007; Wegner, 2002, pp. 55, 67–70, 317–318). Sometimes the following assertion is offered in support of the preceding one: Subjects' conscious proximal intentions to flex cannot be among the causes of their flexes because those intentions are caused by unconscious brain events (Pockett, 2006, p. 21; Roediger, Goode, & Zaromb, 2008, p. 208). This assertion is misguided, as attention to the following analogous assertion shows: Burnings of fuses cannot be among the causes of explosions of firecrackers because the burnings are caused by lightings of fuses. Obviously, both the lighting of its fuse and the burning of its fuse are among the causes of a firecracker's explosion in normal scenarios. Other things being equal, if the fuse had not been lit—or if the lit fuse had stopped burning early—there would have been no explosion. There is no reason to believe that the more proximal causes of firecracker explosions cannot themselves have causes. Analogously, there is no reason to believe that items that are among the relatively proximal causes of flexes cannot themselves have causes and cannot be caused by unconscious brain events.

Is the brain activity registered by, for example, the first 300 ms of type II RPs—*type 300 activity*, for short—as tightly connected to subsequent flexes as lightings of firecracker fuses are to exploding firecrackers? In fact, no one knows. In the experiments that yield Libet's type II RPs, it is the muscle burst that triggers a computer to make a record of the preceding brain activity. In the absence of a muscle burst, there is no record of that activity. So, for all anyone knows, there were many occasions on which type 300 activity occurred in Libet's subjects and there were no associated flexes.

Libet mentions that some subjects encouraged to flex spontaneously report that they sometimes suppressed conscious proximal urges to flex (1985, p. 538). As he points out, because there was no muscle activation, there was no trigger to initiate the computer's recording of any RP that may have preceded the veto (2004, p. 141). So, for all anyone knows, type 300 activity was present before the urges were suppressed.

It is the urges that these subjects are said to report and suppress. Might it be that type 300 activity is a potential cause of conscious urges to flex in Libet's subjects and that some subjects make no decision about when to flex—unconsciously or otherwise—until after the conscious urge emerges? And might it be that prior to the emergence of the conscious urge, subjects have no proximal intention to flex? That our urges often are generated by processes of which we are not conscious is not surprising. And if we sometimes make effective decisions about whether or not to act on a conscious urge, so much the better for free will. Moreover, as I have explained, Libet's data do not show that subjects have unconscious proximal intentions to flex before they have conscious ones. The data do not contradict the hypothesis that what precedes these conscious proximal intentions is a causal process that includes no unconscious proximal decisions or intentions to flex.

Libet offers two kinds of evidence to support his claim that subjects have time to veto proximal conscious urges to flex. One kind has already been mentioned: subjects say they did this. The other kind is produced by an experiment in which subjects are instructed to prepare to flex at a prearranged clock time but to refrain from actually flexing and "to veto the developing intention/preparation to act . . . about 100 to 200 ms before [that] time" (Libet, 1985, p. 538).

The results of Libet's veto study suggest an interpretation of type I and type II RPs that is contrary to his own interpretation. To begin to see why, notice that Libet's claim that the subjects in this study veto "*intended* motor action" (1985, p. 38; emphasis added) is implausible (Mele, 1997, p. 322, 2009, pp. 52–53). These subjects were instructed in advance *not* to flex, but to prepare to flex at the prearranged time and to "veto" this. The subjects intentionally complied with the request. They intended from the beginning *not* to flex at the appointed time. So what is indicated by what Libet refers to as "the 'veto' RP" before "about 150–250 ms before the preset time" (Libet, 1985, p. 538)? Presumably, not the acquisition or presence of an *intention* to flex; for then, at some point in time, subjects would have both an intention to flex at the prearranged time and an intention not to flex at that time. And how can a normal agent be in this condition?[3]

A segment of "the 'veto' RP" resembles segments of type I RPs in cases in which subjects do flex, as Libet observes (1985, p. 538). Given that this segment of "the 'veto' RP" is not correlated with a proximal intention to flex, perhaps the similar segments of type I RPs (and of type II RPs) also are not correlated with proximal intentions to flex. Even so, they might be correlated with potential causes of such intentions.

This idea is developed in Mele, 2006 and 2009. The shape the idea takes there is based partly on the following possibilities about subjects in the veto experiment:

> perhaps a subject's wanting to comply with the instructions—including the instruction to prepare to flex at the appointed time—together with his recognition that the time is approaching produces an unconscious urge to flex soon, a pretty reliable causal contributor

[3] Try to imagine that you intend to eat some pie now while also intending not to eat it now. What would you do? Would you reach for it with one hand and grab the reaching hand with your other hand? People who suffer from anarchic hand syndrome sometimes display behavior of this kind. Spence and Frith suggest that these people "have conscious 'intentions to act' [that] are thwarted by . . . 'intentions' to which the patient does not experience conscious access" (1999, p. 24).

> to an urge to flex soon, or the motor preparedness typically associated with such an urge. Things of these kinds are potential causal contributors to the acquisition of proximal intentions to flex. A related possibility is suggested by the observation that "the pattern of brain activity associated with imagining making a movement is very similar to the pattern of activity associated with preparing to make a movement" (Spence & Frith, 1999, p. 27 . . .).[4] The instructions given to [subjects in the veto experiment] would naturally elicit imagining flexing very soon, an event of a kind suitable, in the circumstances, for making a causal contribution to the emergence of a proximal urge to flex (Mele, 2009, p. 55).

The suggestion is that these same items—as opposed to proximal intentions to flex—are candidates for what the pertinent segments of type I RPs signify and that *proximal intentions* to flex emerge later, both in the case of flexes associated with type I RPs and in the case of flexes associated with type II RPs (Mele, 2009, ch. 3). And again, the reaction time study discussed earlier provides independent evidence about when proximal intentions emerge that places their emergence much closer to the muscle burst than −550 ms.

Trevena and Miller conducted a study involving an "always-move" and a "sometimes-move" condition (2010, p. 449). In both the conditions, participants were presented with either an "L" (indicating a left-handed movement) or an "R" (indicating a right-handed movement) and responded to tones emitted at random intervals. In the sometimes-move condition, participants were given the following instructions: "At the start of each trial you will see an L or an R, indicating the hand to be used on that trial. However, you should only make a key press about half the time. Please try not to decide in advance what you will do, but when you hear the tone either tap the key with the required hand as quickly as possible, or make no movement at all" (p. 449). In the always-move condition, participants were always to tap the assigned key as quickly as possible after the tone. Trevena and Miller examined EEG activity for the second preceding the tone and found that mean EEG "amplitudes did not differ among conditions" (p. 450). That is, there were no significant differences among pre-tone EEG amplitudes in the following three conditions: always-move; sometimes-move with movement; sometimes-move without movement. They also found that there was no significant lateralized readiness potential (LRP) before the tone (p. 450). Trevena and Miller plausibly regard these findings as evidence that no part of pre-tone EEG represents a decision to move. The mean time "from the onset of the tone to a key press . . . was 322 ms in the always-move condition and 355 ms in the sometimes-move condition" (p. 450).

Trevena and Miller conducted a second study in which it was up to the subjects which hand to move when they heard the tone. As in the first study, there was an always-move condition and a sometimes-move condition. Trevena and Miller found that, as in the first study, pre-tone EEG "did not discriminate between" trials with movement and trials without movement, "LRP was absent before the tone," and LRP "was significantly positive after the tone for trials in which a movement

[4] Kilner et al. produce evidence that, as they put it, "the readiness potential (RP)—an electrophysiological marker of motor preparation—is present when one is observing someone else's action" (2004, p. 1299).

was made" (p. 453). They conclude, reasonably, that pre-tone EEG "does not necessarily reflect preparation for movement, and that it may instead simply develop as a consequence of some ongoing attention to or involvement with a task requiring occasional spontaneous movements" (p. 454).

Even if Libet's data do not warrant his claim that his subjects have proximal intentions to flex before they think they do, his idea that we have unconscious proximal intentions should not be lightly dismissed. Such intentions may be at work when, for example, experienced drivers flip their turn indicators to signal for turns they are about to make. In a study in which subjects are instructed to flex whenever they feel like it without also being instructed to report after flexing on when they first became aware of an intention, urge, or decision to flex, would they often be conscious of proximal intentions, urges, or decisions to flex? Might unconscious proximal intentions to flex—and, more specifically, proximal intentions of which they are *never* conscious—be at work in producing flexes in the imagined scenario?

Imagine that someone conducts the experiment just sketched and discovers (somehow) that the subjects were never or rarely conscious of proximal urges, intentions, or decisions to flex. Could it legitimately be inferred that, in Libet's own experiment, conscious urges, intentions, and decisions had no effect on the flexing actions? No. One possibility is that some of Libet's subjects treat their initial consciousness of an urge to flex as a go signal. If they do, the conscious urge seemingly has a place in the causal process that issues in the flexing. Another possibility is that some subjects treat the conscious urge as what may be called a *decide signal*—a signal calling for them consciously to decide right then whether to flex right away or to wait a while. If that is so, and if they consciously decide to flex and execute that decision, the conscious urge again seemingly has a place in the causal process, as does the conscious decision. (Notice that the tone in the sometimes-move conditions in Trevena and Miller's studies apparently functions as a decide signal. In the first study, it signals participants to decide whether or not to press the designated key right then; and in the second, it signals them to decide both whether or not to press right then and which key to press, if they decide to press.)

Perhaps it will be suggested that even if a subject treats a conscious urge to flex as a go or decide signal, that urge has no place in the causal process that issues in a flex because an unconscious brain event caused the conscious urge. But the inference here has the same form as the misguided assertion about conscious intention discussed earlier. An x can be among the causes of a y even if the x itself is caused (recall the firecracker example). Possibly, it will be claimed that by the time the conscious urge emerges it is too late for the subject to refrain from acting on it (something that Libet denies) and that is why the conscious urge should not be seen as part of the process at issue, even if subjects think they are treating the urge as a go or decide signal. One way to get evidence about this is to conduct an experiment in which subjects are instructed to flex at a time *t* unless they detect a stop signal—for example, a change in the color of the clock from white to red. (On stop signal experiments, see Logan, 1994.) By varying the interval between the stop signal and the mean time of the completion of a full flex when there is no stop signal, experimenters can try to ascertain when subjects reach the point of no return. (Time *t* can be a designated point on a Libet clock, and brain activity can be measured backward from *t*.) Perhaps it will be discovered that the point is reached significantly later than time *W*.

13.6 How Accurate Are Subjects' Awareness Reports?

Libet contends that subjects in his main experiment become aware of their proximal intentions well after they acquire them. His primary evidence for the average time of the onset of this awareness comes from the reports subjects make after each flex—reports about where they believe the dot was on the clock when they first became aware of their decision, intention, urge, or whatever, to flex. How accurate are these reports likely to be?

The following labels facilitate discussion:

P-time: The time at which a proximal decision is made or a proximal intention or urge is acquired.
C-time: The time of the onset of the subject's consciousness of an item of the kind just specified.
B-time: The time the subject believes to be *C*-time when responding to the experimenter's question about *C*-time.

Libet contends that average *P*-time is −550 ms for subjects who are regularly encouraged to flex spontaneously and report no "preplanning." And he arrives at an average *C*-time of −150 ms by adding 50 ms to his average *B*-time (−200 ms) to correct for what he believes to be a 50 ms bias in subjects' reports. (For alleged evidence of the existence of this bias, see Libet, 1985, pp. 534–535, 2004, p. 128.) One connection in which *C*-time is important to Libet is his position on veto power. Whether subjects in Libet's studies are ever conscious of relevant proximal urges or intentions early enough to veto them, as he claims, depends partly on what their *C*-times are. The same is true of the question whether, on average, his subjects become conscious of proximal intentions to flex about 400 ms after those intentions emerge in them.

There is an interesting body of work on how accurate *B*-times are likely to be—that is, on how likely it is that they closely approximate *C*-times. This is not surprising. Reading the position of a rapidly revolving dot at a given time is a difficult task, as Wim van de Grind observes (2002, p. 251). The same is true of relating the position of the dot to such an event as the onset of one's consciousness of a proximal intention to flex a wrist. Patrick Haggard notes that "the large number of biases inherent in cross-modal synchronization tasks means that the perceived time of a stimulus may differ dramatically from its actual onset time. There is every reason to believe that purely internal events, such as conscious intentions, are at least as subject to this bias as perceptions of external events" (2006, p. 82).

One fact that has not received sufficient attention in the literature on accuracy is that individuals display great variability of *B*-times across trials. Haggard and Eimer (1999) provide some relevant data. For each of their eight subjects, they locate the median *B*-time and then calculate the mean of the premedian (i.e., "early") *B*-times and the mean of the postmedian (i.e., "late") *B*-times. At the low end of variability by this measure, one subject had mean early and late *B*-times of −231 and −80 ms and another had means of −542 and −351 ms (p. 132). At the high end, one subject's figures were −940 and −4 ms and another's were −984 and −253 ms; and, as I mentioned, these figures are for means, not extremes. These results contribute to grounds for serious doubt that *B*-time closely approximates *C*-time. If there were good reasons to believe that *C*-times vary enormously across trials for the same subject, we might not find enormous variability in a subject's *B*-times worrisome in this connection. But there is good reason to believe this only if there is good reason to believe that *B*-times closely approximate *C*-times; and given the points made about cross-modal synchronization tasks in general and the cross-modal task of subjects in Libet-style experiments, there is not.

Another factor that may make it difficult for subjects to provide *B*-times that closely approximate *C*-times is their uncertainty about exactly what they are experiencing. As Haggard observes, subjects' reports about their intentions "are easily mediated by cognitive strategies, by the subjects' understanding of the experimental situation, and by their folk psychological beliefs about intentions" (2006, p. 81). He also remarks that "the conscious experience of intending is quite thin and evasive" (2005, p. 291). Even if the latter claim is an overstatement and some conscious experiences of intending are robust, the claim may be true of many of the experiences at issue in Libet-style studies. One can well imagine subjects wondering occasionally whether, for example, what they are experiencing is an intention (or urge) to act or merely a thought about when to act or an anticipation of acting soon. Lau and coauthors say that they require their subjects to move a cursor to where they believed the dot on a Libet clock was "when they first felt their *intention* to press the button" (Lau et al., 2007, p. 82; emphasis mine). One should not be surprised if some subjects given such an instruction were occasionally to wonder whether they were experiencing an intention to press or just an *urge* to press, for example. (Presumably, at least some lay folk treat intentions and urges as conceptually distinct, as dictionaries do.) Subjects may also wonder occasionally whether they are actually *feeling* an intention to press or are mistakenly thinking that they feel such an intention.

I argued elsewhere that results reported by Lau et al. (2007) "suggest that reports of *B*-times are reports of estimates that can be influenced by events that follow action" (Mele, 2009, p. 128).[5] A study by Banks and Isham provides confirmation for this claim. They asked subjects in a Libet-style experiment to report, shortly after pressing a response button, where the cursor was on a numbered Libet clock "at the instant they made the decision to respond" (2009, p. 18). "The computer registered the switch closure and emitted a 200-ms beep . . . at 5, 20, 40, or 60 ms after closure." Obviously, subjects were not being asked to report on unconscious decisions; conscious decisions are at issue.

[5] I did not suggest that the estimates are influenced *only* by events that follow action. For evidence that the estimates are also influenced by events that precede action, see Haggard (2011, pp. 19–22).

Banks and Isham found that although the average time between the beep and *B*-time did not differ significantly across beep delays, the following two average times did differ significantly across delays: (1) the time between EMG onset and *B*-time; (2) the time between switch closure and *B*-time. The data display an interesting pattern (see 2009, p. 19):

Beep delay	*B*-time to EMG	*B*-time to beep	*B*-time to switch closure
+5	−21	−127	−122
+20	+4	−124	−104
+40	+4	−135	−95
+60	+21	−137	−77

The beep affected *B*-time, and the beep followed switch closure.

Return to the issue of great variability in *B*-times in the same subject. One way to seek to reduce it is to give the subject a way of conceiving of, for example, making a conscious proximal decision that is easily grasped and applied. Subjects in a Libet-style experiment may be given the following instructions:

> One way to think of deciding to press the button now is as consciously saying "now!" to yourself silently in order to command yourself to press the button at once. Consciously say "now!" silently to yourself whenever you feel like it and then immediately press the button. Look at the clock and try to determine as closely as possible where the dot is when you say "now!" . . . You'll report that location to us after you press the button (Mele, 2009, p. 125).

Subjects can also be regularly reminded to make their decisions "spontaneously"—that is, to make them without thinking in advance about when to press. If, as I predict, subjects given these instructions individually show much less variability in *B*-times than subjects given typical Libet-style instructions, we would have grounds for believing that their reports about when they consciously said "now!" involve *less guesswork* and, accordingly, additional grounds for skepticism about the accuracy of *B*-times in typical studies.

I asked how accurate subjects' reports about when they first became aware of a proximal intention or urge are likely to have been? *Not very* certainly seems to be a safe answer. But there may be ways to improve accuracy.[6] If such *B*-times as have actually been gathered are unreliable indicators of *C*-times, little weight can be put

[6] Would subjects' conscious, silent "now!"s actually express proximal *decisions*? Perhaps not. To see why, consider an imaginary experiment in which subjects are instructed to count—consciously and silently—from 1 to 3 and to press a button just after they consciously say "3" to themselves. Presumably, these instructions would be no less effective at eliciting pressings than the "now!" instructions. In this experiment, the subjects are treating a conscious event—the conscious "3"-saying—as a go signal. (When they say "3," they are not at all uncertain about what to do, and they make no *decision* then to press.) Possibly, in a study in which subjects are given the "now!" instructions, they would not actually make proximal decisions to press but would instead consciously simulate deciding and use the conscious simulation event as a go signal. However, the possibility of simulation is not a special problem for studies featuring the "now!"-saying instructions. In Libet's own studies, some subjects may be treating a conscious experience—for example, their initial consciousness of an urge to flex—as a go signal (see Keller & Heckhausen, 1990, p. 352).

on them in arguments about whether or not there is time enough to veto conscious proximal urges and the like; and the same is true of arguments about whether or not *C*-time is too late for conscious proximal intentions and the like to play a role in producing corresponding overt actions.

13.7 Conclusion

Libet's data do not warrant the claim that his subjects make decisions to move before they are aware of those decisions. Nor do his data warrant the claim that conscious decisions and intentions play no role in generating corresponding overt actions. It is fair to conclude that, on any reasonable conception of free will, the studies and data reviewed here pose no threat to it.[7]

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