Consciousness and Cognition **11,** 144–161 (2002)
doi:10.1006/ccog.2002.0549

On Subjective Back-Referral and How Long It Takes to Become Conscious of a Stimulus: A Reinterpretation of Libet's Data

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The original data reported by Benjamin Libet and colleagues are reinterpreted, taking into account the facilitation which is experimentally demonstrated in the first of their series of articles. It is shown that the original data equally well or better support a quite different set of conclusions from those drawn by Libet. The new conclusions are that it takes only 80 ms (rather than 500 ms) for stimuli to come to consciousness and that ''subjective back-referral of sensations in time'' to the time of the stimulus does not occur (contrary to Libet's original interpretation of his results).

INTRODUCTION

Benjamin Libet is deservedly honored as a pioneer in the field of consciousness research and his corpus of work has had a significant influence on current thinking in the area. The two most widely quoted ideas that have come out of his research are (a) that it takes around 500 ms for the sensation evoked by any given external stimulus to reach conscious awareness, but that the time of the sensation is subjectively ''back-referred'' to the time of the stimulus (with the result that there does not seem subjectively to be a half-second lag in our awareness of the world); and (b) that supposedly voluntary motor actions are actually generated unconsciously, about 500 ms before we become consciously aware of any intention to act.

These conclusions are important to consciousness researchers precisely because they seem to deny to consciousness any major role in the conduct of our day-to-day affairs. If we cannot become conscious of external stimuli until half a second after they happen, then any even mildly fast reaction to a stimulus has to be unconsciously generated. Worse, not only are reactions necessarily unconscious, but even proactive or ''voluntary'' actions must be unconsciously generated. So if consciousness is important at all, it can only be in the generation of long-term plans, which are then carried out largely unconsciously.

While it is not impossible for such a counterintuitive state of affairs to be the case, it would certainly do less violence to our commonsense notions of how the world works if Libet's interpretations of his experimental data were shown to be mistaken. Consequently, there have been a number of attempts to show that Libet interprets his findings wrongly (Churchland, 1981a, 1981b; Glynn, 1990; Gomes, 1998). However, these attacks must be balanced against the enthusiastic and sometimes lengthy discussion of Libet's conclusions as if they were correct by a number of influential

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1053-8100/02 $35.00

investigators, including some remarkably big names (Popper & Eccles, 1977; Penrose, 1989; Dennett, 1991; Freeman, 1999; McCrone, 1999). One of these workers, a renowned physicist, even suggests that we should rethink our concept of time on the basis of Libet's conclusions (Penrose, 1989). Thus a further critique of Libet's results and the conclusions he draws from them would appear to be justified.

In this article I take a slightly different approach from that of previous critics. Rather than attempting to show that Libet's conclusions are wrong, I simply point out that the original experimental observations equally well or better support a set of conclusions quite different from those he draws. These revised conclusions are that it takes only about 80 ms for the sensation evoked by a stimulus to enter awareness (not 500 ms) and that no subjective backdating takes place (i.e., that our conscious awareness of the world does run about 80 ms behind events).

No extra data or recent findings by others are introduced in order to justify these new conclusions. The aim of the present article is purely to show that the actual data produced by Libet and his colleagues are capable (and always were capable) of supporting a different set of conclusions from those espoused by their authors. To demonstrate this I divide the data into sections, corresponding to the main lines of evidence adduced for each of Libet's conclusions. For each section I first summarize the data themselves, as Libet and colleagues report them in the original experimental articles; then describe Libet's interpretation of the data, as summarized in the prologue to his 1993 book (Libet, 1993); and finally offer and justify a viable reinterpretation of the data.

THE DATA, LIBET'S INTERPRETATIONS, AND SOME REINTERPRETATIONS

Time Necessary for Stimuli to Enter Awareness

The experimental findings which lead to the widely quoted conclusion that it takes half a second for stimuli to enter awareness can be summarized under three headings.

''Cerebral Time-on'' or ''Minimal Train Duration''

The data (Libet, Alberts, Wright, Delattre, Levin, & Feinstein, 1964). When the postcentral gyrus, i.e., the somatosensory cortex, of volunteer human neurosurgery patients was directly stimulated with a train of electrical pulses, reports of bodily sensations could be elicited from the subjects (who for clinical reasons were awake during the surgery, with only local anesthesia around the scalp incisions). A number of stimulus parameters were important in determining whether a given stimulus resulted in a report of experienced sensation. These included peak current passed, train duration, pulse frequency, polarity of pulses, and area of electrode contact with the cortex. In general, the lower the current, the longer the train of pulses necessary to elicit a report of conscious sensation. However there was a liminal or threshold current, below which no report of sensation could be elicited, even if the train of pulses was extended to 5 s. At this liminal current, the length of train necessary to cause a sensation was generally 0.5 to 1 s. If the stimulus intensity was turned up by a small amount, the threshold for sensation was achieved with a train duration of only

0.1–0.5 s (at a pulse frequency of 15 Hz or 0.5–5 s at a pulse frequency of 8 Hz). The quality of the sensation changed at this higher stimulus intensity from ''tingling'' to feelings of ''quiver,'' ''pull,'' or ''drawing in the hand.'' If the stimulus intensity was turned up still further, to 2 or more times the liminal current, trains as short as <0.1–0.3 s, or even single pulses, elicited reports of sensation. Stimuli at this intensity often caused a muscle twitch (possibly through spread of current to the adjacent Rolandic cortex) and subjects sometimes reported a sensation like a ''jerk'' or ''twitch'' in these cases. However, they sometimes said they felt no sensation even when a twitch was objectively observable and they sometimes did experience a high intensity single shock to the somatosensory cortex as a ''tingle'' like that caused by long trains of the lowest stimulus intensities. The locus of the sensation is reported to have shifted around somewhat from test to test, irrespective of stimulus duration and intensity.

Facilitation is also reported to occur. This means that the threshold current necessary to elicit sensation was lower for a second train of pulses if these were delivered shortly after an initial train. The time for which this facilitatory effect lasted decreased with decreasing pulse intensity. With strong and medium intensity pulses, facilitation lasted between 4 and 1 min; in other words, with very strong pulses, the effect was seen if the second train was delivered at any time up to 4 min after the first. With stimuli of liminal intensity, the facilitatory effect usually lasted less than 30 s and so facilitation was usually not seen with the stimulus parameters used.

Subcortical stimulation gave similar results to cortical stimulation. The minimum train length necessary to evoke a sensation when the thalamus (see Fig. 1) was stimulated at liminal intensity was 0.3–0.5 s in one subject and 0.5–2 s in the only other

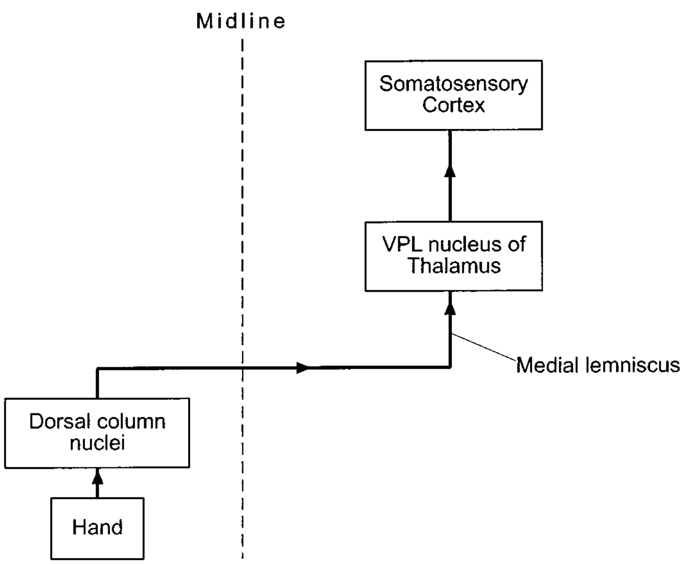

FIG. 1. The dorsal column–medial lemniscal system. Schematic showing anatomical relationship of stimulation sites in Libet's experiments.

subject described. However, when the skin of the hand was stimulated, a single suprathreshold stimulus was sufficient to cause sensation, at least in normal subjects. It should be pointed out that all of the subjects in the cortical or thalamic stimulation experiments were neurosurgical patients and therefore were neurologically abnormal, usually because of Parkinson's disease, epilepsy, or chronic pain syndromes. How much effect this had on the experiments described is unknown.

Libet's interpretation. By 1993 (Libet, 1993) the originally reported data have transmuted slightly. Libet now states that

> We concluded that a single pulse stimulus is simply unable to elicit conscious sensation, when applied in the cerebral somatosensory system. {Author: cf original report}. Sensations have been reported with as little as 2 to 3 sufficiently strong pulses (Libet, Alberts, Wright & Feinstein, 1967; Libet, 1973; Libet, Alberts, Wright, Lewis & Feinstein, 1975). But with more modest and presumably closer to ''normal'' inputs, rather than the high intensity levels required for 2 to 3 pulses, it seems clear that substantial train durations up to 0.5 sec or even 1 sec are necessary to elicit conscious sensation (Libet, Alberts, Wright & Feinstein, 1967; Libet, Alberts, Wright, Lewis & Feinstein, 1975; Libet, Pearl, Morledge, Gleason, Hosobuchi & Barbaro, 1991).

In other words, the liminal stimulus intensity, which requires a train duration of half a second to elicit a sensation, is now taken as being ''normal.'' From here it is a short step to the statement that ''to elicit even a normally induced conscious sensation requires substantial cerebral durations (hundreds of msec).'' This statement, which is in fact merely an interpretation of the data (in other words a hypothesis), is thereafter taken by Libet as fact, e.g., in Libet, Wright, Feinstein, & Pearl (1979): ''Previous studies had indicated that there is a substantial delay, up to about 0.5 s, before activity at cerebral levels achieves 'neuronal adequacy' for eliciting a conscious somatosensory experience (Libet, Alberts, Wright, Delattre, Levin and Feinstein, 1964; Libet 1966).''

Reinterpretation. The truth is that these data do not show that ''there is a substantial delay up to about 0.5 s before activity at cerebral levels achieves 'neuronal adequacy' '' at all. In fact, good evidence is presented that a train of direct cortical stimuli of moderately high intensity do not have to be left on for nearly as long as 0.5 s to elicit a sensation, and there is absolutely no reason to label these stimuli as any more abnormal than lower intensity stimuli which do have to be left on for 0.5 s.

Any direct electrical stimulation of the cortex is highly abnormal, whatever its intensity. The effects of ''normal'' stimulation would arrive at the cortex as a highly spatially organized pattern of neural activity resulting from the stimulation of peripheral receptors, located in this case in the skin of the hand. There is no way that electrical stimuli delivered as rectangular pulses through a Ag/AgCl electrode connected to the exposed surface of the cortex by a glass tube of physiological saline with a tip diameter of 2 mm can be imagined to reproduce the spatial activity pattern evoked by stimulation of peripheral receptors. None of the direct stimuli used by Libet can be considered as ''normal.''

So on this interpretation, nothing at all can actually be concluded from the data about how long it takes for a ''normal'' stimulus to become conscious. All that can actually be said from these experiments is that direct cerebral stimulation with a low

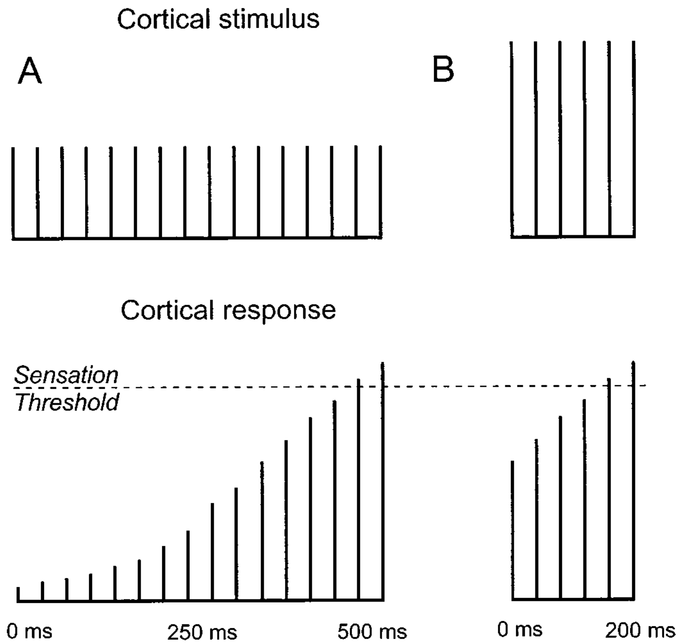

FIG. 2. The facilitation hypothesis to explain why low-intensity stimuli take longer than high-intensity stimuli to cause a sensation. The top half of the figure represents stimulus trains of (A) liminal and (B) supraliminal intensity. The lower half represents the proposed neural responses to these trains showing increases due to facilitation.

stimulus intensity elicits a sensation after a certain number of pulses, while direct cerebral stimulation with higher stimulus intensities elicits a sensation after fewer and fewer pulses as the stimulus intensity increases.

Because the point becomes important in later discussions, it should be noted at this stage that the above finding clearly suggests the operation of some kind of facilitation mechanism, whereby the first pulses in a low-intensity train are subthreshold for sensation, but with repetition the neural response to each pulse increases until by the end of the train each pulse has become suprathreshold for sensation (see Fig. 2). Such a facilitation would only need to last for the time between the individual pulses in a train (which at 30 Hz is 1/30 s) to be effective in this sense. On this hypothesis, the length of train necessary before the facilitation or potentiation raised the neural response to each stimulus pulse to sensation threshold would depend on the pulse intensity. Starting with a stronger pulse (which is already closer to threshold) would mean that a shorter train was needed (see Fig. 2B). This is exactly what was found experimentally.

Libet originally reported these findings in 1964. It is largely since that time that the study of various forms of activity-induced synaptic facilitation/potentiation and depression (which have now been shown to occur in all parts of the central nervous system) has become a major industry in neuroscience. But actually the existence of facilitation in the system under study was clearly demonstrated by Libet's own data

(Libet, Alberts, Wright, Delattre, Levin, & Feinstein, 1964). As described under ‘‘The Data (Libert, Alberts, Wright, Delattre, Levin, & Feinstein, 1964)’’ above, among the many results reported in the original article is the finding that if a second pulse (or train of pulses) was delivered shortly after an initial stimulus, the threshold current necessary to elicit sensation was lower for the second train. The time for which this facilitatory effect lasted decreased as the individual pulse intensity decreased. With strong- and medium-intensity pulses, the facilitation lasted between 4 and 1 min. With stimuli of liminal intensity, it usually lasted less than 30 s and so was often not seen with the stimulus parameters used. However, by extrapolation it seems highly likely that facilitation lasting for at least the length of time between the individual pulses in a train would still have occured at liminal stimulus intensities.

In fact the implications of his own findings concerning facilitation were not lost on Libet, who did at the time consider the facilitation or summation hypothesis sketched in Fig. 2 as the explanation for his data (Libet, 1964). However, at that stage, for no reason other than that he felt it to be ‘‘more intriguing,’’ he stated that he preferred the alternative hypothesis that there is a minimum cerebral activation period for the development of a sensory experience (Libet, 1964). This then became the hypothesis that informed all of his subsequent work.

In my view, however, Libet’s 1964 data supply little or no support for the hypothesis that ‘‘to elicit even a normally induced conscious sensation requires substantial cerebral durations (hundreds of msec)’’ and even less support for the hypothesis that this minimum cerebral activation period is 500 ms. While it is true that application of very weak stimuli for periods longer than 500 ms produced no sensation at all, it was also entirely possible to produce a sensation with trains as short as 1 or 2 ms. Therefore I can see no reason whatsoever for concluding at this stage that there is a minimum cerebral activation period for the development of a sensory experience, let alone that this period is of the order of half a second.

Evoked Potentials

The data (Libet, Alberts, Wright, & Feinstein, 1967). Stimulation of the skin or the VPL nucleus of the thalamus induced evoked potentials which could be recorded at the surface of the cortex. These evoked potentials were different in shape from the so-called direct cortical response caused by direct stimulation of the cortex. Early components of the evoked potentials were present whether or not a sensation was felt in response to the stimulus. When sensation threshold was reached, a later negative peak in the evoked potential became evident. In subject B from Fig. 1 of the *Science* article (Libet, Alberts, Wright, & Feinstein, 1967), the poststimulus latency of this peak is around 70 to 90 ms. In Subject A from Fig. 1, sensation threshold coincides with the appearance of a second hump at about 80 ms, superimposed on the larger negative peak already present; a new positive peak at about 220 ms also appears. In Fig. 2 of the same article, the latency of the new negative peak that clearly appears when sensation threshold is reached is about 80 ms.

Libet’s interpretation. In the prologue to his 1993 book (Libet, 1993) Libet does not remark specifically on the latencies of the peaks in the evoked potential that appear at sensation threshold. He simply points out that early peaks do not covary with conscious sensation, but later components ‘‘extending for 500 ms or more’’ are already present when stimulus intensity is just at or above threshold for conscious sensation. He then writes, ‘‘neuronal activities represented by the later EPs seem to be associated with eliciting conscious sensation.’’

Reinterpretation. It would seem to be a reasonable hypothesis that simple conscious sensation may be represented by the earliest of the many evoked potential components that appear only when the stimulus reaches sensation threshold. From the limited data supplied, it appears that these occur at about 80 ms post stimulus. While it cannot be regarded as a strong conclusion that these 80-ms peaks represent simple conscious sensation, it is a reasonable conclusion, which is consistent with the data.

As Libet points out, later peaks also appear in concert with perception. These are likely to be concerned with various judgments and comparisons that may be made between the immediate stimulus and preceding ones and so might be expected to be enhanced if the sensation is coupled with a particular task.

Retroactive Effects of a Conditioning Stimulus

The data (Libet, Alberts, Wright, & Feinstein, 1972; Libet, 1973; Libet, Wright, Feinstein, & Pearl, 1992). A full experimental article on Libet and colleagues’ findings about the retroactive effects of a masking stimulus is described in Libet, Alberts, Wright, and Feinstein (1972) as being in preparation, but it never appeared. Thus only the sketchy descriptions of retroactive masking given in two review-type articles containing a mixture of old and new data (Libet, Alberts, Wright, & Feinstein, 1972; Libet, 1973) are available. According to these, Libet and colleagues found that if the skin of the hand was stimulated with a suprathreshold stimulus, the sensation that would ordinarily be caused by this could be prevented, or masked, by stimulating the relevant bit of cortex directly with a large electrode *at any time up until 200–500 ms after the skin stimulus*. Alternatively, the sensation caused by the skin stimulus could be enhanced by stimulating the same bit of cortex with a smaller electrode. This effect was found to occur over a similar time frame. A full statistical treatment of the data from the four patients in whom the enhancement effect was found was finally published 20 years after the experiments were done (Libet, Wright, Feinstein, & Pearl, 1992).

Libet’s interpretation. The data show that reports of a sensation can be affected by direct cortical stimulation occuring up to 500 ms after the sensation-causing stimulus. Libet interprets this as meaning that the sensation could not have come to consciousness until 500 ms after its stimulus. He agrees that retroactive *masking* of a sensation (i.e., obliteration of a sensation by a later stimulus) ‘‘may be thought of as due to a partial or complete extinction of a memory’’ rather than interference with the sensation itself (Libet, Wright, Feinstein, & Pearl, 1992). But he argues that explaining the retroactive *enhancement* of a stimulus by saying the memory was in this case simply altered, rather than completely obliterated, is ‘‘a gratuitous ad hoc construction.’’

Reinterpretation. In order for a subject to report on a sensation, he or she has to remember it. If it is accepted that memory can be interfered with in the interval between experience and reportage, there appears little reason to suppose that this interference could not take the form of enhancement rather than degradation. If such a proposition is accepted, these data could simply reflect the fact that it is possible to interfere with memory traces by direct stimulation of the cortex where they are encoded.

Subjective Referral Backwards in Time

The data that supposedly demonstrate subjective back-referral of experiences to the time of the stimulus can be divided into two parts.

Cortical Stimulation and Skin Stimulation

The data (Libet, Wright, Feinstein, & Pearl, 1979). A 500-ms train of stimuli was delivered to a stimulating electrode on the hand area of the somatosensory cortex, at such an pulse intensity that a sensation was felt in the hand only when the train had been on for the full 500 ms. A skin stimulus (to the hand that projected to the somatosensory cortex on the other side of the brain from the directly stimulated cortex) was delivered 200 ms after the start of the cortical train. The subjects reported that the skin-elicited sensation began before the cortically elicited sensation.

At least, this is the simplified version of events that is promulgated by Libet in the introduction to this 1979 article and in Fig. 1 of that article (which is reproduced in several subsequent articles). However, a somewhat different perspective on the experiment is given in the article under Methods and Results. First of all, the cortical stimulating ‘‘electrode’’ itself is reported to be an array of between five and seven wires (insulated to within about 1 mm of the tip), in an unspecified geometrical arrangement, with the wires spaced between 3 and 10 mm apart. So according to these specifications, the stimulating electrode array could be anything from about 6 or 7 mm in diameter (if there were five wires spaced 3 mm apart in a circular arrangement) to 70 mm in diameter (if there were seven wires in a line spaced 10 mm apart). This is a huge difference in size in relation to the SI cortex, but it is never made clear exactly which array was used in any particular experiment. Second, the temporal characteristics of the cortical stimulus are specified in a similarly vague fashion. In what is described as ‘‘most’’ experimental series, the cortical stimulus train is actually not the liminal 500-ms train discussed in the simplified version referred to above. In fact, because the subjects’ responses to cortical stimulus trains are reported in these experiments to be unstable at liminal intensity (i.e., at an intensity such that the pulse train had to be left on for 500 ms for a subjective sensation to be felt) the stimulus strength was actually turned up to a strength at which sensations were (sometimes) reported when the train had been on for only 200 or 300 ms. However, because ‘‘there were often instances in which adopted minimum TD (of 200 ms) produced an inconsistent and uncertain subjective response’’ the train duration was turned up to ‘‘500 or 600 msec’’ at the 200-ms intensity. So we are left uncertain both as to whether the cortical stimulus trains were left on for 500 or 600 ms and as to whether the intensities were such that a sensation could be reported after the train had been left on for 500, 300, or 200 ms. The time at which the skin stimulus was delivered was also varied radically in a manner which is not adequately reported.

When we turn to the one paragraph in the article under Results and Discussion, we find the following statement:

> Actual tests of this kind, with S [the skin stimulus] delayed for variable times after the start of the C [cortical stimulus] train, were carried out with six patients. In each of these subjects only a limited number of observations could be made. However the pooled reports were predominantly those of sensory experience for the C (cortical) stimulus beginning *after*, not before, that for a delayed threshold S pulse; this was true even when the delivery of the S pulse was delayed from the start of the C stimulus train by almost the full value of the U-TD (that is, by up to 400 to 500ms when the U-TD was 500ms).

And that is absolutely the only report we are given on the actual results of these much-quoted experiments.

Libet's interpretation (Fig. 3). Libet's interpretation of these results, as shown in his oft-reproduced Fig. 1 and in the text of the rest of the 1979 article refers only to the highly simplified version of the data presented in the first paragraph under ''The Data (Libet, Wright, Feinstein, & Pearl, 1979)'' above. It is as follows. The cortically induced sensation occurs at the end of the stimulus train. For the purposes of this description we define this as time 500 ms (taking the start of the stimulus train as time 0) because in Libet's interpretation he uses an example where stimulus intensity was such that the train had to be left on for 500 ms. The skin stimulus is delivered at time 200 ms (in the simplified version of events). Since this stimulus is also assumed to require a 500-ms cortical ''on-time'' to elicit a sensation, it is predicted that the skin-induced sensation will occur 200 ms after the cortically induced sensation, i.e., at time 700 ms.

Unexpectedly, however, this prediction fails: the skin-induced sensation was reported by the subjects as occuring before the cortically induced sensation. Therefore there must be some mechanism whereby skin sensations (which actually become neuronally adequate only after 500 ms) can be backwardly referred so that they are felt to begin at the time of the stimulus. This is hypothesized to be a cortical time marker that occurs close to the time of the stimulus, whether or not the stimulus causes a sensation—the early components of the evoked potential (see ''Evoked Potentials''). Direct cortical stimuli demonstrably do not induce these early components. Skin stimuli do.

Reinterpretation (Fig. 3). These experiments were undeniably terribly hard to do in a practical sense and also it is fair to point out that the research program was adversely affected by the premature illness and death of the neurosurgeon, Bertram Feinstein. Therefore I leave aside for the moment the question of whether the experimental data reported are actually adequate to allow any conclusions at all to be drawn or whether they should perhaps have been used only as a pilot study to leverage a grant for doing the thing properly. Even if we accept the veracity of the simplified version of data on which Libet based his far-reaching conclusions, it has to be said that another interpretation of these data is possible.

This reinterpretation is shown diagrammatically in Fig. 3. There are two major points of difference between it and Libet's interpretation. The reinterpretation hypotheses that: (1) Libet's assumption that the skin sensation requires 500 ms of cortical activity to enter consciousness is simply wrong. Actually there is either no delay or some equal but unknown delay (like 80 ms) between either a suprathreshold skin

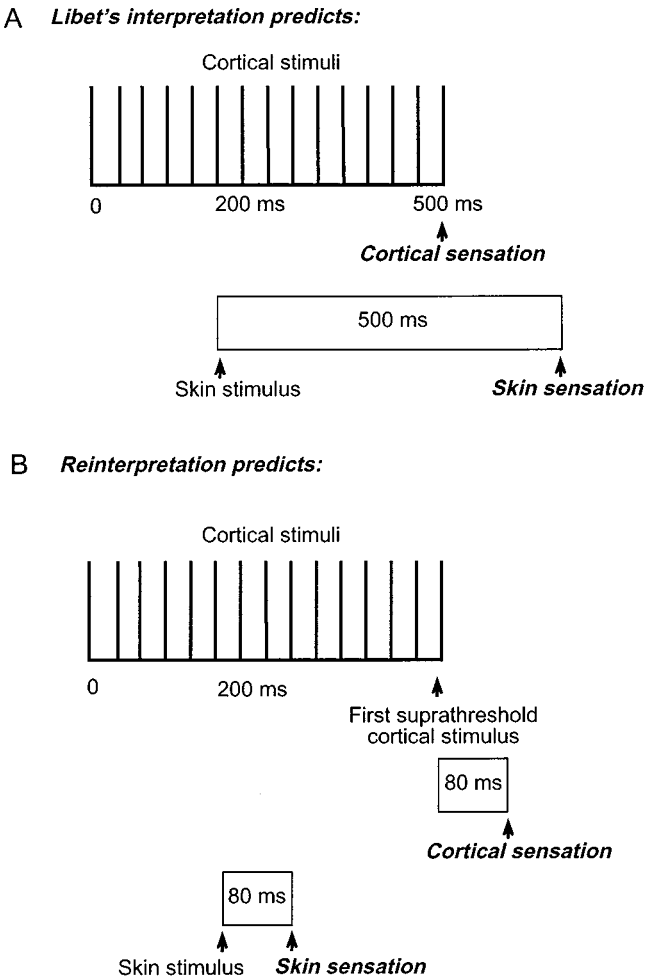

FIG. 3. Two predictions as to the results of experiments in which a skin stimulus is presented 200 ms after the start of a 500-ms train of liminal pulses to the cerebral cortex. (A) Libet's prediction is that the sensation due to the skin stimulus will be reported as occurring *after* the sensation due to the cortical stimulus, because it takes 500 ms for the skin stimulus to evoke a sensation. (B) The reinterpretation suggested in the present article predicts that the skin stimulus will be reported as occurring *before* the sensation due to the cortical stimulus. This is because the respective sensations occur 80 ms after both (i) the first suprathreshold cortical pulse, which occurs only at the end of the 500 ms train as shown in Fig. 2, and (ii) the skin stimulus. The experimental result was that the skin stimulus was reported as occurring before the cortical stimulus, as in Prediction B. Libet's response to the failure of Prediction A was not to accept that his assumptions in making the Prediction A were wrong, but to introduce a new postulated mechanism—subjective backwards referral of the skin stimulus from the time at which he still proposed it was experienced (200 ms after the end of the cortical train) to the time at which it was delivered.

stimulus and its sensation or a suprathreshold cortical stimulus and its sensation. (2) Intratrain facilitation occurs, as shown diagrammatically in Fig. 2 of the present article. If this is true, we can see that the critical point of the experiment just described is that it is set up so that the first stimuli in the cortical train are not suprathreshold. Only the last few are.

Now if assumption (2) is correct, it is obvious that the skin stimulus was actually *delivered* a full 300 ms *before* the first *suprathreshold* cortical stimuli. And since we are postulating that there is either no delay or some constant common delay between any suprathreshold stimulus and its resulting sensation, we would now predict that the skin-induced sensation should also *occur* about 300 ms *before* the cortically induced sensation. The empirical results are that the subjects did report the skin-induced sensation as beginning before the cortically induced sensation. So on this reinterpretation of what is happening, the data do fit the prediction. There is no problem. There is certainly no need to postulate ''subjective backwards referral.''

For completeness, we should probably ask how the messiness of the actual experiments (as opposed to the clean lines of the simplified version) impacts on Libet's interpretation and the present reinterpretation. The answer is that it is impossible to say. We are not told exactly which cortical stimulus trains were used in any given experiment, we are not told exactly which delays between the start of the cortical stimulus and the skin stimulus were used in any given experiment, and in fact we are not told exactly what the results were for any given experiment, for any of the total of six subjects studied. All we can do is accept Libet's summary of the results as given at the end of ''The Data (Libet, Wright, Feinstein, & Pearl, 1979).''

This is basically that the skin stimulus, although delivered after the start of the cortical train, was always felt before the cortical stimulus. My interpretation of what is going on predicts this (for any of the stimulus parameters used). Libet's interpretation does not. The fact that Libet's interpretation fails to predict the actual data is not taken by him as evidence that his prediction was wrong, however. Instead he explains the data by sticking to his original hypothesis (that it takes of the order of half a second for any stimulus to come to consciousness) and making the additional, ad hoc postulate that subjective backward referral then occurs.

The question must now be which is more unbelievable—the hypothesis that is necessitated by Libet's interpretation, or the hypotheses on which the reinterpretation is based. The hypotheses on which the reinterpretation rests are unremarkable. They are (a) that intratrain facilitation occurs, so that only the last stimuli in a ''threshold'' train actually elicit sensation; and (b) that there is either no delay or a delay of about 80 ms between any kind of suprathreshold stimulus and its sensation. The hypothesis put forward by Libet to explain the data is a completely novel and radical proposal: ''subjective backwards referral.''

Medial Lemniscus or Thalamic Stimulation and Skin Stimulation

The data (Libet, Wright, Feinstein, & Pearl, 1979). When a similar experiment to that described under ''Cortical Stimulation and Skin Stimulation'' is done with stimulation in the medial lemniscus or thalamus instead of the cerebral cortex, the result is different. Now a skin stimulus delivered at the start of the thalamic train is reported to begin at the same time as the thalamic train, even though the pulses in the latter are of such an intensity that the train has to be left on for 500 ms to elicit a sensation.

At least this is the way the data are presented by Libet in the prologue to his book (Libet, 1993). The data reported in the orginal experimental article (Libet, Wright, Feinstein, & Pearl, 1979) again give a slightly different picture. Here Libet reports that ‘‘the technically most satisfactory experimental series of this type’’ was done on two subjects, both of whom had had electrodes chronically implanted in their medial lemniscus for several years, for the purpose of relieving chronic pain of central origin by self-stimulation. The treatment had been successful and both patients’ pain was well controlled at the time of the experiments, so the presumption is that the electrodes had been used regularly over a period of years. The experimental protocol was as follows. Trains of current pulses were delivered to the medial lemniscus. At the start of the experiment, the intensity of the pulses was set by the experimenters to a current value that reliably elicited a report of experience after the train had been on for 200–300 ms. In the jargon of the 1964 article (Libet, Alberts, Wright, Delattre, Levin, & Feinstein, 1964), these were supraliminal stimuli. In the experimental series, 500- or 600-ms trains of these stimuli were used to produce sensations, which were felt in one of the subject’s hands. The skin of the other hand was stimulated directly, using a stimulus intensity that required a minimum of two to three pulses at 60 Hz to elicit a sensation. Trains of these pulses lasting 300 ms were delivered, with the aim of producing sensations that resembled as much as possible the leminiscally evoked sensations in the first hand. A large number of pairings of skin stimuli with lemniscal trains were then delivered (50 were reported for most experimental sessions) with the intervals between the skin and lemniscal stimuli being varied randomly between zero and ±200 ms. For each pairing the subjects were asked to say which hand felt the sensation first. As a control, pairs of skin stimuli to either hand were also delivered, with the same interhand stimulus intervals.

The results were as follows. For both hand–hand and hand–lemniscal pairings, subjects were unable to tell accurately which stimulus began first if the interval between the beginnings of the two stimuli was anything up to 150 ms. In this situation they reported either that the stimuli began at the same time or that one or the other began first, but with no better than chance relationship to the actual order of stimulus delivery. This result was repeated with normal subjects for hand–hand pairings, so it probably had nothing to do with the neurological status of the two subjects with lemniscal electrodes. When the interstimulus interval was lengthened to 200 ms, subjects reliably reported the stimulus which actually was delivered first as being experienced first, both for hand–hand and hand–lemniscal stimulus pairings.

Libet’s interpretation. Since the pulse intensities were set so that it required a lemniscal pulse duration of 200 ms before any experience was reported, a lemniscal stimulus which began 200 ms before a skin stimulus should have been *experienced* as starting at the same time as the skin stimulus. In fact it was reported as starting before the skin stimulus. Likewise lemniscal stimuli that began at the same time as the skin stimuli should have been reported by the subject as starting after the skin stimulus (because the lemniscal stimuli were of such an intensity that they could not be experienced before the stimulus train had been on for 200 ms). In fact, however, such stimuli are reported as starting at the same time as the skin stimuli.

The best hypothesis to explain all this is that the experience caused by the lemniscal stimuli was somehow subjectively referred back to the time of the beginning of the lemniscal stimulus (probably using the time marker of the earliest evoked potential components). Direct cortical stimulation does not produce the early evoked potential components (the putative timing signal), so sensations induced by direct cortical stimulation are not subjectively back-referred. This explains the difference between the results under ‘‘Cortical Stimulation and Skin Stimulation’’ and those under ‘‘Medial Lemniscus or Thalamic Stimulation and Skin Stimulation.’’

Reinterpretation. At first sight Libet’s logic seems inescapable. If the lemniscal sensation could not have been actually experienced any sooner than 200 ms after the start of the lemniscal stimulus train, then a lemniscal stimulus train starting 200 ms before a skin stimulus *should* be reported as being experienced at the same time as the skin stimulus. One explanation for why this was not the case would be that some mechanism like back-referral was operating. (Another reasonable explanation would be that there was no back-referral and the skin sensation was actually experienced 500 ms after *its* stimulus; but this interpretation seems inexplicably to have been neglected.)

However, further thought reveals a major difficulty with either of the above interpretations. This is that, again, no account is taken of facilitation. If we accept that the facilitation earlier described by Libet (Libet, Alberts, Wright, Delattre, Levin, & Feinstein, 1964) would still have occurred in the same way in this later series of experiments, it can reasonably be predicted that such facilitation would rapidly have changed the efficacy of the lemniscal pulses so that a much shorter train than 200 ms would have been adequate to elicit a sensation.

The evidence that facilitation would have occurred is persuasive. The time interval between successive skin–lemniscal pairings is not specified, but it can be inferred that in order to have completed each experimental series within a reasonable period, successive pairings would probably have been delivered not more than a minute or two apart. Going by the results in the 1964 article, where it is reported that facilitation caused by trains of supraliminal pulses such as those used here lasted up to 4 min (Libet, Alberts, Wright, Delattre, Levin, & Feinstein, 1964), this sort of repetition rate should have caused significant facilitation.

By definition, facilitation reduces the minimum train duration necessary to cause a sensation. There is no indication in the 1979 article (Libet, Wright, Feinstein, & Pearl, 1979) that the experimenters checked at the end of each session for whether the lemniscal trains they were using still had to be left on for 200 ms before a sensation was reported. It seems highly likely that had they done so, they would have found the time the lemniscal trains had to be left on in order to elicit a sensation was significantly reduced. If the efficacy of the stimuli in the leminiscal trains were increased by facilitation to a point where only the first few stimuli in the train were sufficient to cause a sensation, as might relatively easily have happened, there would be no need to invoke subjective back-referral to explain the results. No hypothesis, with or without back-referral, would predict any subjectively observable difference with regard to timing between skin trains and lemniscal trains. A lemniscal train starting 200 ms before a reference skin train *should* be perceived as starting before the skin train, just as another skin train starting 200 ms before the reference skin train should be perceived as coming first. The data reported simply bear out these predictions.

If it is accepted that a significant degree of facilitation is very likely to have occurred during these experiments, there are two implications. First, as described above, there is no need to postulate subjective back-referral to explain the experimental findings. To be fair, the idea that subjective back-referral might occur is not *excluded* by the data; it simply is no longer necessary to make sense of them. In light of this, Occam's Razor might suggest that the hypothesis of subjective back-referral, while not disproven, should not be adopted at this stage. Second, nothing can now be concluded from these data about how long it takes for a stimulus to become conscious. The results would be quite compatible with the proposal that both lemniscal and skin stimuli become conscious 75 or 80 ms (or indeed any other uniform period of time one chooses to postulate) after the first suprathreshold stimulus pulse, which on this interpretation would have occurred shortly after the start of the respective stimulus trains.

It should probably be noted here that, as reported in a later article (Libet, Pearl, Morledge, Gleason, Hosobuchi, & Barbaro, 1991), forced-choice experiments showed that a thalamic stimulus could be detected unconsciously and then brought to consciousness simply by extending the duration of the stimulus train. This in no way invalidates the reinterpretation above, since in the 1991 article liminal (not supraliminal) stimulus pulses were used, with intervals of ''a minute or two'' between successive trains. According to the facilitation data in the 1964 article, no intertrain facilitation would be expected under these conditions. So all that need be postulated to explain the forced choice results according to our revised interpretation is that the sub-sensation-threshold pulses in Fig. 2 could still be detected by an unconscious mechanism.

Conscious Intention in Voluntary Action

The Data (Libet, Gleason, Wright, & Pearl, 1983)

The aim of the experiments was to examine the relative times of onset of (a) the readiness potential which can be recorded from the scalp before spontaneous motor actions, (b) the subject's awareness of wanting to move, (c) the subject's awareness of the actual movement, (d) an objective measure of the actual movement (using EMG electrodes on the muscle in question), and (e) the subject's awareness of a randomly delivered stimulus pulse to the back of the hand. The timings of (b), (c), and (e) were established using a spot of light which revolved in a circle on an oscilloscope screen at 1 revolution/2.56 s. The subject was asked to remember and report the ''clock position'' of the spot when they became aware of wanting to move, when they were aware of actually moving, and when they were aware of the random skin stimulus.

The results showed that the readiness potential always began an average of 350 ms before the subjects became aware of wanting to move. The awareness of wanting to move came an average of 200 ms before the objectively recorded movement. The subjects always reported being aware of actually moving an average of 85 ms before the movement was objectively recorded. They were aware of the random skin stimulus an average of 50 ms before it was actually delivered.

Libet's Interpretation

With apparently voluntary actions, the conscious intention to act appears before the act is carried out, but after an unconscious initiating process. No interpretation is offered for the observation that the experience of moving apparently occurs before the movement and the experience of a randomly delivered skin stimulus occurs before the stimulus.

Reinterpretation

It seems clear that the cortical readiness potential already discovered by others (Kornhuber & Deecke, 1965; Deecke & Kornhuber, 1978) does precede the awareness of wanting or intending to move by a significant margin. This stands as a major finding, the full implications of which are yet to be worked out. It is also clear from Libet's data that a conscious intention to act does precede the act itself (as might be expected).

However, the findings that are most relevant for the present discussion of how long it takes for a stimulus to enter awareness and whether back-referral occurs are (a) that subjects reported being aware of actually moving 85 ms before they did actually move and (b) that they reported being aware of the skin stimuli (which were randomly delivered by the experimenter at times completely unpredictable by the subject) 50 ms before they actually were delivered. The concept of subjective back-referral cannot help us here. The only nonmystical interpretation of these findings would appear to be that there is a 50- to 85-ms error built into the method used to report subjective timings.

Such an error could be introduced as follows. In the clock-position method of reporting time, no correction is made for the time it takes for the stimulus of the spot of light to enter awareness. This is because the presumption is that this time lag would be cancelled out by subjective back-referral. But assume for the moment that no subjective back-referral to the objective time of the stimulus occurs and that it actually takes around 80 ms (not 500 ms) for a light stimulus to enter consciousness. In this case our perception would continuously be running 80 ms behind events. If this were so, when the light spot reached clock-position *x* the subject would still be seeing it at position *x* minus 80 ms. This could well provide an explanation for the finding that subjects said they experienced moving 85 ms before they actually did move. The explanation would be that they actually experienced moving exactly when they did move, but at that time they were still seeing the revolving light spot where it had been 85 ms earlier.

The additional experimental finding that randomly delivered skin stimuli were reported by this method to be experienced 50 ms before they actually occurred appears not quite to fit this reinterpretation. According to our assumptions (80 ms lag to awareness, no back-referral), the skin stimuli should have been experienced 80 ms after they occurred, so that factoring in the putative −80-ms time lag in the reporting method should have meant that they were reported exactly when they did occur. In fact they were reported an average of 50 ms early (which, allowing for an 80-ms error in the reporting method, would put them as being experienced only 30 ms after the event).

One explanation for this discrepancy may be that there was actually a very large variance in the experimental results, indicating that the task was too difficult to allow any great accuracy. An alternative explanation could be that light stimuli and somatosensory stimuli take different times to enter awareness. Perhaps somatosensory stimuli take around 70 ms (see ''Evoked Potentials'') and light stimuli take 120 ms because the visual cortex is more extensive and complicated than the somatosensory cortex. If these figures were right, then the built-in error in the clock-position method of reporting would be −120 ms. Thus the predicted appearance of skin sensations as reported by the clock-position method would be at −50 ms (as observed) and the experience of moving would have actually begun 35 ms after the movement itself started (so that with an error of −120 ms it would have been reported at −85 ms). Another possibility is that the times taken for both light and somatosensory sensations to enter consciousness vary between individuals, which would account for the variance in the observed results. Much basic work on the appearance of evoked potential components coincident with subjective sensation thresholds remains to be done.

The major point is, however, that the hypotheses (a) that it takes of the order of 80 ms (rather than 500 ms) for stimuli to enter consciousness and (b) that there is no subjective back-referral do at least as well and probably better than Libet's hypotheses (a 500-ms time lag and back-referral) at predicting the experimental results described under ''Conscious Intention in Voluntary Action''). This is also the case for all the rest of Libet's experimental results.

DISCUSSION

The main underlying reason for the differences between Libet's interpretations of his data and the reinterpretations offered here is that in all of his chains of reasoning, Libet ignores the existence of facilitation. This is odd, since he makes a point of demonstrating in the first of his long series of articles in the area that facilitation did occur in his system. However, once having rejected this mechanism as the underlying cause of his initial series of observations on how trains of high intensity pulses produced a sensation after less time-on than trains of low intensity pulses, he never refers to facilitation again. The idea took root that a minimum duration of at least several hundred milliseconds of neuronal activation was necessary for conscious experience, and thereafter all further data were interpreted in terms of this idea.

In the service of this idea, clear indications that conscious sensations coappeared with evoked potential components at around 80 ms were glossed over. The explanation that retroactive enhancement of sensations by a later stimulus (which, by the way, is not quite as novel a finding as claimed, at least in the visual system; Donchin & Lindsley, 1965; Standing & Dodwell, 1972) was due to memory retouching rather than enhancement of the original sensation was dismissed out of hand, with no reason given. The likelihood that repetitive stimulation of the medial lemniscus at supraliminal pulse intensity would cause facilitation was overlooked, thus giving rise to the idea of subjective backwards referral. The idea that subjective backwards referral must be operating led to neglect of the time taken for a light stimulus to enter consciousness when interpreting results of the clock-position method of timing subjective events in the last series of experiments.

This cascade of interpretations (or as time may show, misinterpretations) is easy enough to follow. What is perhaps more difficult to understand is ease with which these interpretations have been accepted by many in the consciousness research community. This may be partly due to the complexity of the articles in which they are put forward. There is a temptation not to struggle with the data and the chains of reasoning, but simply to accept the conclusions. However, there may also be a deeper reason in operation. There seems almost to be a feeling abroad that if backward referral did not exist, it would be necessary to invent it. An off-the-cuff remark by Baars in an on-line discussion group (''Psyche-B,'' 23 August 2000) perhaps exemplifies this line of reasoning. Referring to the problem of localizing stimuli in space, Baars concludes: ''the order of arrival of the parts of a stimulus cannot be rigidly interpreted in the brain in the same way that stimuli arrive at the sensory receptors. That's why the ability to do things like backward referral are necessary in principle for any living brain.''

This may or may not be so. In any case, the reinterpetations offered here notwithstanding, it remains true that backward referral *might* exist (although the reinterpretation under ''Conscious Intention in Voluntary Action'' of the present article would tend to suggest that, at least under the conditions studied by Libet, it does not). The only strong message of the present article is that Libet's data, as they stand, do not prove the matter one way or the other. Neither do they prove that it takes of the order of 500 ms for a stimulus to enter consciousness.

Libet's pioneering work has revealed new territory, which is something it is given to few scientists to achieve. It remains for his successors to chart that territory accurately.

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Received November 6, 2000