The timing of conscious experience: a critical review and reinterpretation of Libet's research

Gilberto Gomes

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Current address (2006):
Laboratory of Language and Cognition
UENF, 28013-602 Campos, RJ, Brazil
ggomes@uenf.br

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Abstract: An extended examination of Libet's works led to a comprehensive reinterpretation of his results. According to this reinterpretation, the Minimum Train Duration of electrical brain stimulation should be considered as the time needed to create a brain stimulus efficient for producing conscious sensation and not as a basis for inferring the latency for conscious sensation of peripheral origin. Latency for conscious sensation with brain stimulation may occur *after* the Minimum Train Duration. Backward masking with cortical stimuli suggests a 125-300 ms minimum value for the latency for conscious sensation of threshold skin stimuli. Backward enhancement is not suitable for inferring this latency. For determining temporal relations between stimuli that correspond to subjects' reports, the *end* of cerebral Minimum Train Duration should be used as reference, rather than its onset. Results of coupling peripheral and cortical stimuli are explained by a latency after the cortical Minimum Train Duration, having roughly the same duration as the latency for supraliminal skin stimuli. Results of coupling peripheral stimuli and stimuli to medial lemniscus (LM) are explained by a shorter LM latency and/or a longer peripheral latency. This interpretation suggests a 230 ms minimum value for the latency for conscious sensation of somatosensory near-threshold stimuli. The backward referral hypothesis, as formulated by Libet, should not be retained. Long readiness potentials preceding spontaneous conscious or nonconscious movements suggest that both kinds of movement are nonconsciously initiated. The validity of Libet's measures of W and M moments (Libet et al. 1983a) is questionable due to problems involving latencies, training and introspective distinction of W and M. Veto of intended actions may be initially nonconscious but dependent on conscious awareness.

Keywords: conscious sensation; conscious voluntary movement; consciousness; event-related chronometry; introspection; latency; mental timing; mind-brain; nonconscious processes; readiness potentials.

1. Introduction

Benjamin Libet has developed an extended research program on the timing of conscious experience, investigating first the conscious experience of sensory stimuli, then of voluntary action. His results have led to startling conclusions, which have aroused a great deal of controversy (P. S. Churchland 1981a, 1981b; "Open peer commentary" in Libet 1985; Glynn 1990, 1991; Dennett & Kinsbourne 1992). First, he has suggested that we may be conscious of sensory stimuli only after rather long latencies of up to half a second or more (Libet et al. 1964). Secondly, he has made the hypothesis of a mechanism of referral of conscious awareness of sensory stimuli backwards in time, and obtained experimental results that he considered supportive of this hypothesis (Libet et al. 1979). This backward referral has been interpreted by some in a literal way, and taken to indicate either a reversal of the direction of physical time (Penrose 1989), or the independence of mental in relation to physical reality (Eccles, *in* Popper & Eccles 1977). The other possible interpretation is "cognitive", and consists in considering that it is only in the subject's representation of time that the experience is referred backwards. Thirdly, he has found evidence suggesting that spontaneous voluntary movements are nonconsciously initiated and that conscious experience of the intention to act comes only some 350 milliseconds (ms) after the beginning of the cortical activity that corresponds to it (Libet et al. 1983a).[1] Fourthly, he has maintained that, although voluntary action is nonconsciously initiated, the intention to act may be consciously vetoed before its motor accomplishment. This might indicate that "conscious control functions can appear without prior initiation by unconscious cerebral processes, in a context in which conscious awareness of intention to act has already developed" (Libet 1985).

Libet's conclusions have often been taken to be in contradiction with the hypothesis of identity between mental and neural states. This has granted him support from some (Eccles, *in* Popper & Eccles 1977) and opposition from many (Churchland 1981a and 1981b; Glynn 1990; Danto 1985; Nelson 1985; Wood 1985). Besides this general question, the specific points raised above are by themselves debatable enough, and have raised unsettled controversy. While I believe that some of his experimental results are highly important, and that his efforts to scientifically investigate these difficult questions are praiseworthy, I also think that some of his conclusions are mistaken and that alternative interpretations are not only possible but more plausible. In relation to one specific point, two experimentally testable alternative hypotheses will

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[1] For a philosophical discussion of these results, see Bittner 1996.

be offered. It is also hoped that the proposed reinterpretations will inspire other experimental approaches. It must be noted, and regretted, that (to my knowledge, and with one exception, Keller & Heckhausen, 1989) only Libet and his group have conducted experimental investigations of the specific points that raised the greatest controversy. It seems highly desirable that other researchers should make replications and variations of these experimental studies.

2. Conscious sensations evoked by electrical stimuli applied to the brain

2.1 The minimum duration of a train of electrical pulses, needed to produce a conscious sensation

Up to now, there is no way of objectively and directly determining the exact moment when a conscious experience takes place. If you simply ask the subject to indicate this moment through a motor reaction, the time needed to prepare and perform this motor reaction will make it come later than the conscious experience itself. But with a perceptual conscious experience, a distortion in the opposite direction is also possible, since the subject may react to an expected stimulus with a preprogrammed response before becoming conscious of it (Fehrer and Biederman 1962; Taylor & McCloskey 1990). Libet has tried to devise indirect methods enabling him to infer the moment of conscious experience and thus study the relation of conscious experience to cerebral events.

His starting point was the fact that it takes a certain time of electrical stimulation of the cortex to produce a conscious sensation in an awake patient undergoing neurological surgery. In a series of very careful experiments, Libet and his associates (1964) studied the various parameters of electrical stimulation of somatosensory cortex that affect the threshold of conscious sensation. They found that a stimulus train of about 500 ms is frequently needed to provoke a conscious sensation and this led Libet to the hypothesis that a similarly long period of cortical activity might also be needed to form the conscious experience induced by peripheral sensory stimuli. Our ordinary consciousness of the world, then, would always be about half a second late in relation to real events.

In order to be able to discuss this hypothesis, however, we must go into some details of the experimental methods and results. Libet and his associates found that the principal parameters that affected the threshold of conscious sensation were Train Duration (duration of the train of electrical pulses applied to the cortex), Pulse Frequency (number of pulses per second) and Intensity (peak current measured in mA). The Minimum Train Duration is *the minimum duration* *of the train of pulses that is necessary to produce a conscious sensation*. With Pulse Frequency above 15 pulses per second and keeping Intensity as low as possible (Liminal Intensity), the Minimum Train Duration varies between 500 ms and 1 s. Libet calls this Minimum Train Duration when Intensity is liminal the ‘utilization’ Train Duration. If the Intensity is higher than the liminal one, the Minimum Train Duration becomes shorter. On the other hand, if Train Duration is made longer than the ‘utilization’ Train Duration, no further reduction of Intensity is possible if one wants to obtain a conscious sensation, and this is what defines the Liminal Intensity. We should therefore distinguish between *threshold intensity*, which varies with the Train Duration employed (for Train Durations shorter than the ‘utilization’ Train Duration) and *Liminal Intensity*, which is the minimum intensity to produce a conscious sensation when Train Duration is made as long as necessary.

However, Liminal Intensity itself varies remarkably as a function of Pulse Frequency. The higher the Pulse Frequency (up to 240/s), the smaller the Liminal Intensity (Libet et al. 1964, p. 558). Libet says there is a comparative absence of effect of Pulse Frequency on ‘utilization’ Train Duration (when Pulse Frequency is higher than 15/s) (p. 576) but results given show a slight drop in ‘utilization’ Train Duration as Pulse Frequency is increased (p. 557). With stimuli of supraliminal Intensity, the effect of Pulse Frequency on Minimum Train Duration is much greater. For these stimuli, an increase in Pulse Frequency seems to have a considerable facilitatory effect, that leads to a much shorter Minimum Train Duration (p. 559). On the other hand, if Pulse Frequency is diminished below 15/s, with Liminal Intensities, there is a striking rise in ‘utilization’ Train Duration. With 8 pulses per second, ‘utilization’ Train Duration goes up to 5-10 seconds.

2.2 The hypothesis of a latency for sensory awareness lasting as long as the Minimum Train Duration of brain stimulation at Liminal Intensity

Libet has attached a special significance to the ‘utilization’ Train Duration, that is, the Minimum Train Duration when the Intensity is at liminal level. "The relatively long cerebral utilization TD [Train Duration] leads to inferences about a latency for conscious awareness of sensory input, at least at the near-threshold levels, which may have important general implications" (Libet et al. 1964, p. 576). Although he includes the qualification about near-threshold levels, we can see that he is ready to jump to the general implications. Here is another statement of his inference: "such long utilization TDs [Train Durations] (...) indicate that not until 0.5 sec. or so after the arrival at the cortex of the initial impulses generated by a near-threshold sensory stimulus, will a subjective awareness of this stimulus take place" (Libet et al. 1964, p.

574). Of course one could object that the cortical effect of sensory stimuli is not necessarily equivalent to the effect of near threshold electrical pulses applied on the cortical surface. With higher intensities, the Minimum Train Duration is much shorter and we might just as reasonably take a shorter Minimum Train Duration as a reference for an estimation of the latency for conscious awareness of sensory stimuli. But Libet had an argument to counter this objection. According to him, "the more intense though briefer repetitive input could conceivably give rise to some appropriate after-activations which continue for some tenths of a second" (Libet et al. 1972, p. 159). So he maintained that "it is (...) possible that the requirement for about 0.5 sec of actual activation holds even for these supraliminar, brief train inputs" (ibid.). However, this hypothesis has been disproved by some results of Libet's own later research (Libet et al 1979).[2] These have shown that if the Minimum Train Duration is briefer, we must also admit a proportionately shorter duration of the period of cortical activity required for eliciting a conscious sensation (even if its absolute value is unknown). That is to say we can no longer suppose that the same period of cortical activity is required with stimuli of different Intensities and Minimum Train Durations. Suppose the Minimum Train Duration were accepted as a basis for inferring the latency for consciousness of sensory input. Even so, after these later experimental results, there would no longer be any reason to privilege the special case of Minimum Train Duration that is the ‘utilization’ Train Duration. (There would also be little reason to exclude from consideration cases of ultra-long ‘utilization’ Train Durations — up to 5-10 seconds — that are obtained when Pulse Frequency is lower than 15/s. And these are certainly not suitable for an inference regarding latency of normal conscious sensations.)

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[2] This involves the research that will be described in a later part of this article (Libet et al. 1979, pp. 203, 214, 220). The authors paired skin stimuli and cortical stimuli. When the cortical stimulus was presented 200-300 ms before the skin stimulus, if there were in fact a requirement of 500 ms of cortical activation (in spite of the Minimum Train Duration of 200-300 ms that resulted from the intensities employed), the conscious sensation of the cortical stimulus could only appear at least 200-300 ms after the skin stimulus. The subjects should in this case perceive the skin stimulus before the cortical one. No consistent theoretical interpretation (that is, no interpretation capable of explaining at the same time the different experimental observations) would then explain the fact that, in general, they perceived them as simultaneous. (See specially subjects J.W. and M.T., experiments B.) See also Libet (1982, p. 239), where he states: "neuronal adequacy for the C-experience [experience induced by cortical stimulation] is achieved at or near the end of the required stimulus TD [Train Duration], whether this be 500 or 200 ms or other tested values".

I will argue, however, that the inference itself is ill-founded. Libet seems to have been surprised, at the start, that even a high-intensity single pulse applied to cortex was often unable to provoke a conscious sensation, while a low-intensity train of repeated pulses did it. The requirement of a certain time of cortical activity (induced in the experimental case by repetitive stimulation) then seemed to him to be the condition for conscious experience in general. The fact that this time was almost constant within a certain parametric region (Liminal Intensity; Pulse Frequency $>$ 15/s) seems to have led him to believe he had discovered the approximate value of this temporal condition. With peripheral input, no repetitive stimulation (or very little) is needed, so he hypothesized that the isolated peripheral stimulus would provoke a period of autonomous repetitive cortical activity that must be equivalent to the period of artificially induced repetitive cortical activity needed in the experimental case. He found supportive evidence for this hypothesis in the late components of the evoked potential recorded after peripheral stimuli, lasting 500 ms or more (Libet 1965; Libet et al. 1967).

If we consider this as a mere tentative hypothesis, there is nothing wrong with it. On the contrary, it indicates a subtle and intelligent reasoning. But intelligent hypotheses are often false and it must be recognized that the data thus far available were merely suggestive of this possibility and could not be considered as supporting evidence for it. According to this hypothesis, all our conscious experience of the world would be about half a second late in relation to real events, and this is a very strong supposition. Specific evidence for it was badly needed.

An alternative hypothesis was just as plausible at this point. As the neural mechanism responsible for conscious experience is unknown, one could simply assume that the Minimum Train Duration (‘utilization’ Train Duration or other) is just a necessary condition for activating this mechanism by way of electrical stimulation of the cortical surface. The same mechanism could be differently activated in the case of normal sensory perception, with different time requirements. This alternative hypothesis considers that the time required to activate the mechanism that leads to the production of conscious experience is distinct from the time that this mechanism itself, once activated, requires to produce it.

2.3 Experimental latency and real latency

Libet treats the Minimum Train Duration as a latency. "If a rather long period of activation, e.g., 0.5-1 sec., is a requirement for conscious experiences at near liminal levels, this would constitute a 'latency' between the onset of activation and the 'appearance' of the conscious experience" (Libet 1965, p. 84). However, we have no guarantee that conscious experiences arises exactly at the end of the ‘utilization’ Train Duration, as Libet suggests. It may arise later. Besides, we should here distinguish between what we may call ‘experimental latency’ and ‘real latency’. From the practical point of view of the experimenter, it must be permissible to speak of a latency (or synonymously, latent period) in relation to the interval between the *onset* of a stimulus and the onset of its effect. But from a theoretical point of view, the real latency must be considered as the interval between the onset of the *effective* stimulus and the onset of the effect. More precisely, I propose that 'real latency' be defined as the interval between the moment when a continuous stimulus becomes effective and the onset of its effect. In usual cases of supraliminal peripheral stimuli, the distinction is irrelevant, since the stimulus is effective since its beginning, or its minimum effective duration is so short that we can safely ignore it. (In relation to this usual type of stimulus, the latency for conscious awareness was called 'perceptual onset delay' by Efron, 1970.) In these cases, the experimental latency and the real latency are identical. However, when the minimum duration of the stimulus is not negligible, one may consider the *real latency* as the interval between the *end* of this minimum duration and the onset of the effect. In fact, the end (or rather, the very last moments) of this minimum period of stimulation may be considered as *the beginning of the effective stimulus*. So the Minimum Train Duration of brain stimulation may be viewed as a part of the experimental latency, but not as a part of the real latency. Most of it may be considered as just a preparatory period that puts the brain in a state suitable to produce conscious sensation if new pulses arrive. Real latency would then be considered as the interval between the onset of the *effective* stimulus (at the end of the Minimum Train Duration) and the onset of conscious sensation.

No such period of repetitive stimulation is usually needed when the skin is stimulated with electrical pulses. One single electrical pulse of suitable intensity applied to skin is typically sufficient to produce a conscious sensation. Libet explained this difference by assuming that a period of cortical activity preceding the conscious sensation (comparable to the cortical ‘utilization’ Train Duration) would be induced by the peripheral (skin) stimulus. But with liminal intensities there is also a Minimum Train Duration for the electrical stimulation of the skin. Libet and his colleagues found a skin ‘utilization’ Train Duration of 33-100 ms (2-4 pulses at 30 pps) (1964, p. 573). This skin ‘utilization’ Train Duration is certainly much shorter than the cortical ‘utilization’ Train Duration, but the difference may be considered as a consequence of the two very different sources of stimulation. This difference in source of stimulation may be also considered sufficient to explain the fact that no repetitive skin stimulation is usually needed to produce a conscious sensation, while, on the other hand, even very strong single pulses applied to the cortex (or to the thalamus) are usually unable to do it.[3] The physiological stimulation of the brain through neural pathways starting at the skin receptors may be much more efficient in activating the unknown mechanism responsible for conscious experience than is the electrical stimulation of cortical surface or of other points of the somatosensory system. It is probably more reasonable to suppose an equivalence between the neural processes underlying the skin 'utilization' Train Duration and those underlying the cortical 'utilization' Train Duration than to suppose an equivalence between the cortical 'utilization' Train Duration and a latency for conscious sensation of supraliminal skin stimuli. This does not exclude, of course, the existence of a latency (of unknown duration) for the conscious sensation of a skin stimulus, extending well beyond the time needed for the excitation arising from this stimulus to reach the cortex. (When there is a peripheral Minimum Train Duration, the real latency would start at the end of it.) What I am questioning is not the existence of this rather long latency, but its equivalence to the cortical 'utilization' Train Duration (or to any other cortical Minimum Train Duration). There probably is a rather long latency for conscious experience of a peripheral stimulus and there certainly is a cortical 'utilization' Train Duration, but they are most likely two different things.

Two hypothetical cases may help make clear the difference between a real latency and a period that is needed to build up a brain state that enables a stimulus to be effective. (1) Suppose an electrical pulse, applied to some neural structure, produces a muscular contraction after 200 ms. Say we apply a train of pulses lasting 500 ms. We will have a train of responses lasting 500 ms and starting 200 ms after the onset of the stimulus train. In this case, we can speak of a *real latency* of 200 ms separating the stimulus train and the train of responses. (2)

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[3] There is an ambiguity in Libet's reports regarding the ability of single pulses to cortex or thalamus to produce a conscious sensation. Fig. 3 in Libet et al. 1964 (p. 558) indicates liminal intensity values of cortical stimuli required to elicit a sensation *and includes single pulses*. The text does not make clear whether the effect obtained in this case is still a sensation or a muscular contraction. In relation to the ventro-postero-lateral nucleus of the thalamus (VPL), the authors state explicitly that stimulation "*elicited sensations* without any visible motor responses, even when using pulse frequencies below 20 pulses/sec.(namely, 15 pulses/sec., 8 pulses/sec., *and single pulses*)" (p. 570, my italics). In Libet et al. 1967, however, we find that, in VPL, single pulses "were completely inadequate to elicit a conscious sensory experience (...), even with peak currents which were as much as 20 times liminal Intensity" (p. 1599). Libet also asserts that "[t]he minimum train duration that can elicit awareness, when the intensity is raised as high as possible, has not been firmly determined, although it would appear to be in the order of 100 ms" (Libet 1993, p. 131).

Now suppose that a train of 11 pulses separated by 20 ms-intervals (thus making a 200 ms stimulus train), applied to some other neural structure, is needed to produce a single muscular contraction, that appears 50 ms after the 11th pulse. Suppose also that if the stimulus train is continued, at the same frequency, other contractions follow, each 50 ms after each new pulse. Say we applied a stimulus train lasting 500 ms. We would have in this case a train of responses lasting 300 ms and starting 250 ms after the onset of the stimulus train. The interval of 250 ms between the onset of the stimulus train and the onset of the response train would then be the experimental latency. From a theoretical point of view, however, the real latency is in this case 50 ms, and there is a preparatory period of 200 ms for the building up of a necessary excitatory state. The first 10 pulses should be considered as what creates this excitatory state that, once formed, enables the response to the succeeding pulses. The first contraction is not a response to the first pulse, delayed by 250 ms. It would be better conceived as a response to the 11th pulse, made possible by the preparatory effect of the preceding ten pulses.

Now is the conscious sensation elicited by electrical stimulation of the brain analogous to the first hypothetical case above, or to the second? We have evidence from Libet's own studies that it is analogous to the second. The onset of conscious experience cannot be directly determined, but its duration can be compared by the subject to the duration of the sensation evoked by another stimulus. Libet and his colleagues asked their subjects to compare the duration of sensation evoked by a stimulus to brain to that evoked by a stimulus to skin. They found that the durations matched when brain Train Duration was equal to skin Train Duration plus brain Minimum Train Duration. They give the example of a stimulus to brain requiring a period of 200 ms to produce a conscious sensation. When they applied a stimulus train lasting 500 ms, the subjects said the conscious sensation had the same duration as that produced by a skin stimulus train lasting 300 ms (Libet et al. 1979, p. 197). *I attach a great theoretical importance to this finding*, which the authors mention somewhat in passing (as a methodological procedure for making brain stimuli and skin stimuli appear subjectively similar). The duration of the sensation corresponds to the part of the stimulus train that starts *at the end* of the Minimum Train Duration. This seems to indicate that most of the Minimum Train Duration is in fact a period for the building up of a brain state that makes subsequent stimulation effective in producing conscious sensation, rather than a real latency (as defined above) or part of it (see Figure 1). We will see that this distinction is important for interpreting Libet's later research involving the coupling of skin stimuli and brain stimuli.

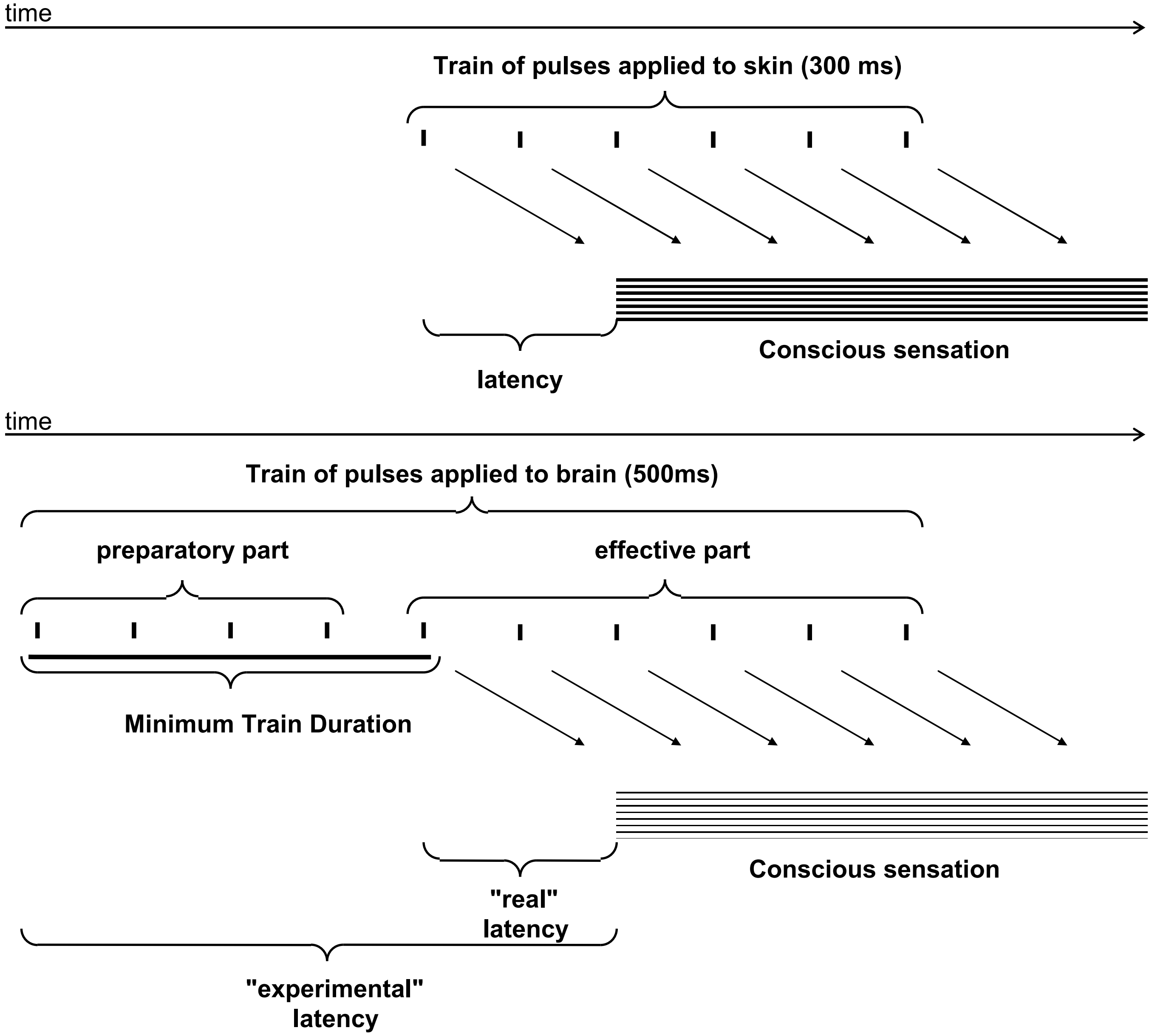

Figure 1 - Latency for conscious sensation - Sensations experienced by the subject as having the same duration are produced by a 300 ms pulse train applied to skin and by a 500 ms pulse train applied to brain. Latencies and pulse frequency shown are merely illustrative. (New interpretation of experimental data from Libet et al. 1979.)

If the Minimum Train Duration is the period needed to form an efficient stimulus for conscious sensation, then after this stimulus is formed, a real latency may still occur. Just as in our hypothetical example (2), above, a latency of 50 ms separated the first *efficient* pulse from the first response, we may suppose that a certain time is needed, after the Minimum Train Duration, for conscious experience to appear. After all, the experiments show that a certain time of electrical stimulation of the cortex is required for a conscious sensation, but not that this conscious sensation is immediately present at the end of this period. Such a latency after the Minimum Train Duration is up to this point a mere theoretical possibility, and we have no indication of its possible duration, but we will see that this hypothesis has its place in the interpretation of Libet's later experiments.

2.4 From subliminal detection to conscious sensation of a thalamic stimulus

In 1967, Libet and his colleagues were able to elicit Evoked Potentials with cutaneous single pulse stimuli well below the threshold for conscious sensation (Libet et al. 1967). These Evoked Potentials resembled, with smaller amplitude, the initial components of those produced by stimuli above the threshold for conscious sensation. The later components of the latter, however, were absent.

Information coming from skin receptors (of epicritic exteroceptive sensation) is processed first at the medulla oblongata (nuclei gracilis and cuneatus), where the medial lemniscus has its origin, and then at the ventro-postero-lateral nucleus of the thalamus **(VPL)**, before reaching the cortex (Delmas 1970). Libet and his colleagues had already shown that electrical stimulation of VPL at Liminal Intensity requires a ‘utilization’ Train Duration comparable to the cortical ‘utilization’ Train Duration (300-2000 ms) (Libet et al 1964, p. 557). They now showed that the ability of stimuli below the threshold of consciousness to elicit the initial components of the Evoked Potential is even more striking when the stimuli are applied to VPL than when applied to skin (Libet et al. 1967). A single pulse with intensity 20 times stronger than the Liminal Intensity used with pulse trains of suitable duration was still insufficient to elicit a conscious experience, but the primary Evoked Potential was very marked. Indeed, the amplitude of the primary Evoked Potential elicited by a strong single pulse applied to VPL (that did *not* evoke a conscious sensation) could be greater than that recorded after a skin stimulus well above the threshold for consciousness. This clearly shows that neither the presence nor a high amplitude of the initial components of the Evoked Potential is a sufficient condition for conscious sensation.

In a more recent research project, Libet and his colleagues investigated the detection of stimuli applied to ventrobasal thalamus that were too short to be consciously perceived (Libet et al. 1991) They found that, in a forced-choice situation, subjects were in fact able to identify, with greater-than-chance accuracy, which of two periods (indicated by two different lights) the stimulus had been presented in, even when they thought they were merely guessing. The study is one more to show the reality of subliminal identification and the adequacy of the forced-choice paradigm to evidence such phenomena.

Libet's discussion of the results of this research, however, is confusing. He acknowledges the effect of intensity in the creation of a cerebral stimulus that is efficient for producing conscious awareness. "Increase of intensity of an input to the cortex can reduce the 'time-on' required for awareness" (p. 1753). At the same time, he argues *against* "the possible suggestion that any integrative mechanism sensitive simply to intensity and duration produces awareness, instead of some more specific role in this for 'time-on' per se" (p. 1753). He cites three facts as evidence against such a mechanism of temporal integration. The first is that there is a Liminal Intensity for cerebral stimuli, so that stimuli with lower intensity do not produce conscious awareness even if duration is prolonged to 5 s or more. But there may be a minimum intensity to activate the process of temporal integration. The second is that no progressive alteration in electrophysiological activity was recorded from the cortex during the application of cerebral stimuli, nor any unique event at the end of the Minimum Train Duration. But Libet himself recognizes that not all neuronal activities were recordable – possibly not the relevant ones in this case. According to him, "this evidence at least offers no support for a progressive integrative factor". But no support is not evidence against.[4] The third fact he cites is that a single pulse to the medial lemniscus does not elicit a conscious sensation even if its intensity is 20-40 times Liminal Intensity. But this does not exclude a mechanism of temporal integration. There may simply be an upper limit of utilizable stimulation energy per unit of time, and so, a minimum duration even with the highest intensity. The mechanism may not be so simple, but still one of temporal integration. Otherwise, how would we explain shorter Minimum Train Durations with higher intensities (within a wide range)?

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[4] Modern neurophysiological hypotheses concerning conscious and preconscious processes often invoke the synchronization of oscillatory discharges of a set of neurons discovered by Gray & Singer (1989). If the establishment of such a synchronized neuronal activity is necessary to activate the mechanism that leads to the production of a conscious sensation, then no progressive alteration nor any unique event at the end of the Minimum Train Duration should be expected merely with surface recordings.

All these considerations, however, are only relevant to the question of what is needed to build up a cerebral stimulus that elicits a conscious sensation. As we argued above, no inference of the latency for conscious awareness of peripheral stimuli is warranted by these experiments.

3. Backward masking and backward enhancement

3.1 Backward masking

We have seen that there is no direct way of determining the latency for conscious experience of a sensory stimulus, and I have criticized an inference of the duration of this latency based on the Minimum Train Duration of electrical stimulation of the cortex. Experiments of backward masking, however, may provide an estimation of the minimum duration of this latency. In backward masking, consciousness of a stimulus is prevented by presentation of a subsequent stimulus under certain conditions. If the masking stimulus is able to prevent consciousness of the first stimulus, it may be concluded that this consciousness had not yet taken place at the moment of presentation of the mask. (An alternative interpretation is that the mask prevents not the awareness but the memory of the first stimulus.) If the interval between the two stimuli is increased to a certain value, the masking effect disappears. It may then be concluded that either consciousness had already taken place at this moment or its later occurrence could no longer be prevented by the action of the second stimulus. So the latency for conscious experience of the first stimulus is inferred to be equal to or greater than this interval (see Figure 2).

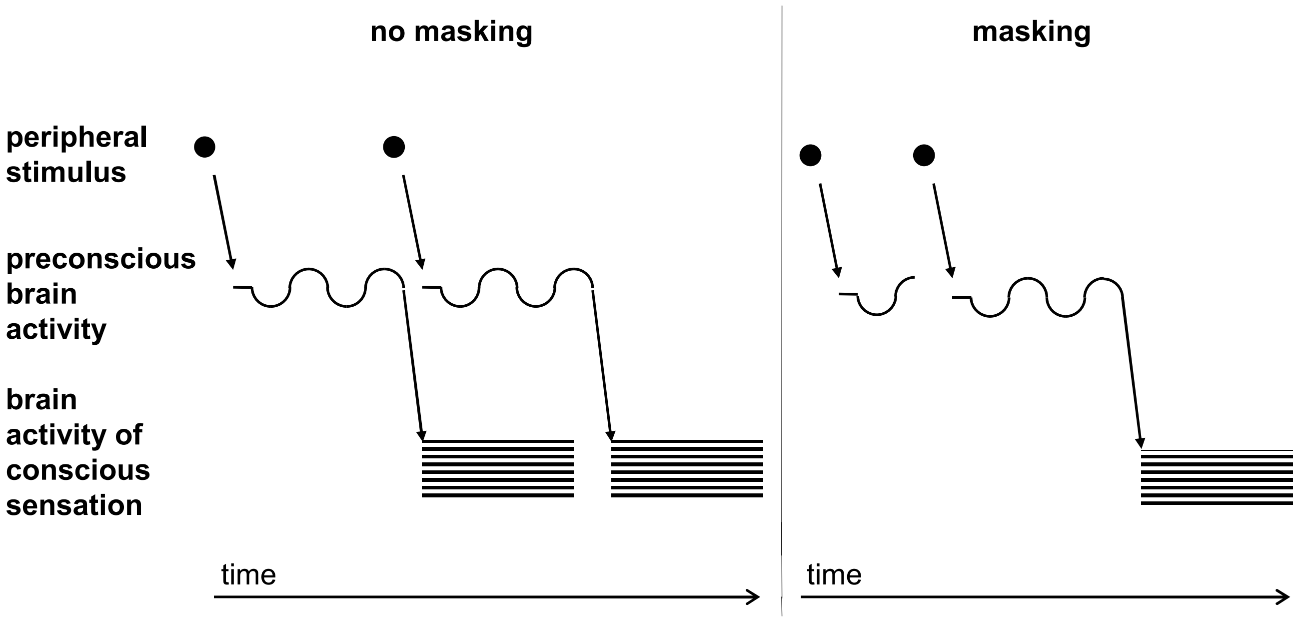

Figure 2 - Backward masking
Interpreted as giving an estimation of a minimum value
of the latency for conscious sensation

Backward masking of a peripheral stimulus by a second peripheral stimulus has been studied by various authors with stimuli of different sense modalities (Fox 1978). For visual stimuli, backward masking can usually be produced with an interval of up to 100 ms. It has been found that the maximum interstimulus interval that produces masking varies according to the intensity of the first stimulus (among other factors). The higher the intensity, the shorter the effective interstimulus interval. This seems to indicate that *stronger stimuli take shorter to produce conscious experience*.

Libet has conducted backward masking experiments in which the masking stimulus was a train of electrical pulses applied to cortex. The test stimulus was a single electrical pulse applied to skin, with Intensity at the threshold for conscious sensation, at the place to which the sensation caused by the cortical stimulus was referred. A formal report of these experiments was not published, but an account of their results was included in a number of papers (Libet et al. 1972; Libet 1978; Libet 1982). Backward masking was obtained with cortical stimuli with intensity 1.3-1.5 times Liminal Intensity (Libet et al. 1972; 1.1-1.2 times according to Libet 1978; 1.2-2 times according to Libet 1982). A single cortical pulse was not effective to produce masking. In 1972, Libet et al. say the minimum cortical Train Duration needed to produce masking had not yet been determined, but in 1978 and 1982 the value of ~100 ms is given for this minimum Train Duration. Is the minimum Train Duration needed to produce masking the same as the Minimum Train Duration needed to produce conscious sensation of the mask, with the Intensity and Pulse Frequency used in this case? The authors are not explicit on this point. Masking was obtained with intervals between the test stimulus and the beginning of the cortical stimulus of up to 125-200 ms for most subjects, and up to 500 ms for one subject (Libet et al. 1972). It must be recalled that the test skin stimulus was at the threshold for conscious sensation. "When the interval was greater, or when the strength of S_1 [the skin stimulus] was raised sufficiently, the subject experienced both of the sensations in the same temporal order as the responsible stimuli" (Libet et al. 1972).

What inference can be made regarding latency for consciousness on the basis of these data? One may conclude that conscious experience did not occur less than 125-200 ms after the skin stimulus at threshold intensity, for most subjects. Since a cortical train of 100 ms was needed, one may add 100 ms to this estimate. One may add these 100 ms if one supposes, as Libet does, that only at the end of this minimum Train Duration is the cortical stimulus able to interfere with the forming of the conscious sensation of the skin stimulus, and prevent its completion. We might as well suppose that, since its beginning, the cortical stimulus disturbs and retards the forming of the conscious sensation, and at last, if allowed to proceed for 100 ms or more, irrevocably prevents its completion. On this hypothesis, we would not be allowed to add the 100 ms to our estimate. A longer latency is suggested by one case, in which, Libet tells us, backward masking was obtained with a 500 ms interstimulus interval. But, on the other hand, we must not forget that, with intensities of the skin stimulus higher than the threshold for consciousness, the interstimulus interval must be shorter, so that for most stimuli a shorter latency for consciousness must be admitted. Anyway, backward masking only allows us to estimate the *minimum* value of the latency for consciousness, for the intensity employed − this latency can of course be longer.

3.2 Backward enhancement

Backward masking was produced by cortical stimuli applied through a large 10-mm disk electrode. A cortical stimulus given through a small 1-mm wire electrode sometimes produced backward enhancement (Libet et al. 1992; preliminary report in Libet 1978). This was evidenced by the use of two skin stimuli, separated by a 5 second interval, and followed by the cortical stimulus. Even though the two skin stimuli were of equal intensity (near threshold), subjects tended to evaluate the second as having been stronger than the first, when it was followed by the cortical stimulus up to a certain time. The effect was more pronounced when the interval between the second skin stimulus and the cortical one was between 25 and 400 ms, but occurred also with intervals ≥ 500 ms.

The inference regarding the duration of the latency for conscious sensation, however, is less certain in the case of backward enhancement than in the case of backward masking. Subjects here are not simply asked to report a conscious sensation but to report on the comparison of this conscious sensation with the memory of another conscious sensation that has taken place a considerable time (5 seconds) before. What I am questioning is *not* the ability of a subject to report both of these conscious sensations some seconds after the second one. What I am saying is that the comparison of the two stimuli is a mental operation that probably comes after the subject becomes conscious of the second stimulus. If the cortical stimulus comes before this mental operation of comparison is completed, it may influence differently the memory of a very recent second stimulus and the memory of a not so recent (more than 5-s old) first stimulus. The sensation of the cortical stimulus may become associated with the sensation of the second skin stimulus that has taken place just a little before, and so favor its evaluation as stronger. That this comparison is not so easy (and probably not so rapid) is shown by the fact that in control tests in which no cortical stimulus was given, subjects failed to correctly evaluate the intensity of the two stimuli as equal in 35% of trials. The intervals between the second skin stimulus and the cortical one thus only allow us to infer the minimum duration of the latency for conscious sensation *plus* the duration of the mental operation of comparison. They give us no information on the latency itself. So we cannot agree with Libet's conclusion that "the observation of retroactive enhancement even with delays of 400 ms or more (...) provides further support for the postulated delay [up to 500 ms or more] in sensory awareness" (Libet et al. 1992, p. 372).

4. Coupling of peripheral and cerebral stimuli

4.1 The end of the Minimum Train Duration as a reference for determining a minimum value of the latency for conscious sensation of a peripheral stimulus

We have seen (2.1) that there is at present no way of directly determining the moment when a conscious sensation takes place. However, Libet had the idea of using the cerebral Minimum Train Duration as a reference to determine a minimum value of the latency for conscious sensation of peripheral stimuli **(P-latency)**. Since conscious awareness does not occur with stimuli shorter than the Minimum Train Duration, for those with Train Duration ≥ Minimum Train

Duration, conscious awareness cannot begin *before* the end of the Minimum Train Duration. (On the other hand, Libet himself recognizes it might begin afterwards; Libet et al 1979, p. 199.) Suppose a peripheral stimulus **(P)** is presented at the beginning of a cerebral stimulus. If the subject perceived P and the cerebral stimulus as simultaneous, since conscious awareness of the cerebral stimulus can only occur after its Minimum Train Duration, this would mean that P-latency would have been at least equal to the cerebral Minimum Train Duration (see Figure 3).

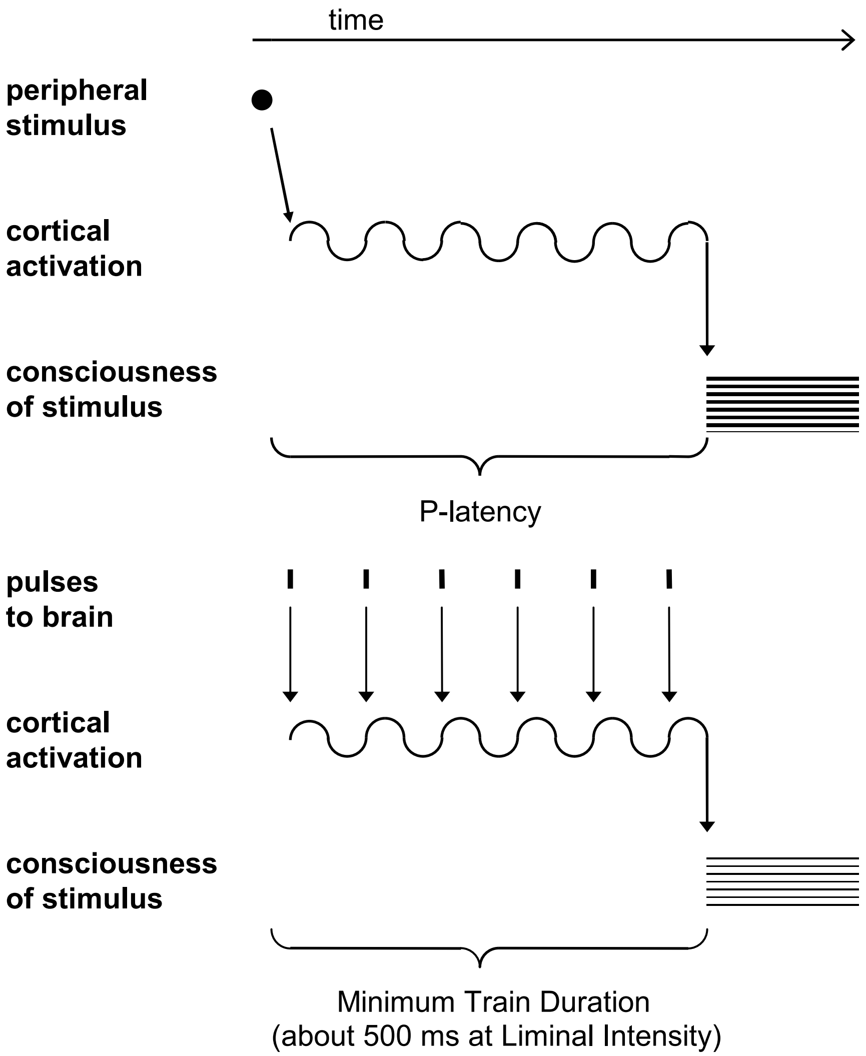

Figure 3 - Schematic representation of Libet's original hypothesis
(P-latency: latency for conscious sensation of the peripheral stimulus)

Suppose now, for the sake of argument, that simultaneity of sensations was obtained when P was presented 200 ms before the end of the cerebral Minimum Train Duration. This would make one infer a *minimum* value of P-latency of 200 ms. If Libet had started the experimental testing of these temporal relations using the stimuli to the medial lemniscus **(LM)** he used later, he might in fact have inferred a minimum value of about 200 ms for the P-latency of the stimuli he used[5]. As we will see, the Minimum Train Duration was in this case about 200 ms (due to an Intensity stronger than liminal), and simultaneity of sensations was obtained when P and LM started approximately at the same time (Libet et al. 1979, pp. 209-211). However, Libet first tested the coupling of peripheral and cortical **(C)** stimuli and here results were different.

4.2 P-C coupling

4.2.1 Libet's expectation, negative results and the pseudo-problem of a reversed order of sensations.

P and C were coupled at different time intervals. In these tests, P was usually applied "on the side opposite to that for the referred cerebrally-induced sensation", so the subject could report simply 'right first', 'left first' or 'together' (Libet et al 1979, p. 195). Results showed that the simultaneity of sensations was generally obtained when P coincided with the *end* of the cortical Minimum Train Duration. This demonstrated unequivocally that Libet's original hypothesis (Libet et al 1979, p. 199), illustrated in Figure 3, had to be abandoned. From these results, the experimental latency for conscious sensation of C appears to be much longer than that for conscious sensation of P.

Libet had supposed a P-latency lasting as long as the cortical ‘utilization’ Train Duration (Minimum Train Duration with Liminal Intensity, lasting about 500 ms with the Pulse Frequency used). He had also supposed no real latency after this cortical Minimum Train Duration. So he had predicted that conscious sensations would be simultaneous if P was presented at the *beginning* of C (Figure 3). If P was presented *after* the beginning of C, then P should be perceived as coming *after* C. The experimental results, however, did not confirm what he had expected. They showed that, even if the peripheral stimulus came 450 ms *after* the start of C, subjects reported awareness of P *before* C (Libet et al. 1979, p. 200).

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[5] Intensity of P was adjusted so that subjective intensities induced by P and by LM match (Libet et al. 1979, pp. 197, 212). P was a train of pulses that when presented singly were sub-threshold (p. 205).

It must be emphasized, however, that presentation of these temporal relations in terms of the *start* of C may be misleading. As we saw in section 2.3, C only becomes an efficient stimulus at the end of the Minimum Train Duration. So even if P is presented *after* the start of C, it can still be *before* the efficient part of C. Patricia S. Churchland, when she describes the experiment in which P is presented 200 ms *after* the start of C, says stimuli were reported as felt "in the reverse order", that is, P *before* C (Churchland 1981a, p. 167). This is not in fact a suitable description because the *start* of C is not the reference point we should adopt. It would be better to say that P was presented 300 ms *before* the end of the Minimum Train Duration of C (in this case, about 500 ms), and was also perceived *before* C. In fact, there is no reversed order (as shown in Figure 4). The expectation that it should be perceived *after* C would only be justified if we supposed a very long P-latency (for example, 500 ms as Libet supposed) and no latency for consciousness after the Minimum Train Duration of C. Dennett and Kinsbourne also speak of a reversed order. In their description of these experiments, they say: "(...) a subject's left *cortex* was stimulated *before* his left *hand* was stimulated (...)", missing the difference between the start of C and the moment when C becomes effective (Dennett & Kinsbourne 1992, p. 187). And they conclude: "In fact, however, the subjective report was reversed: 'first left, then right'", posing the problem of a reversed order that in fact does not exist.

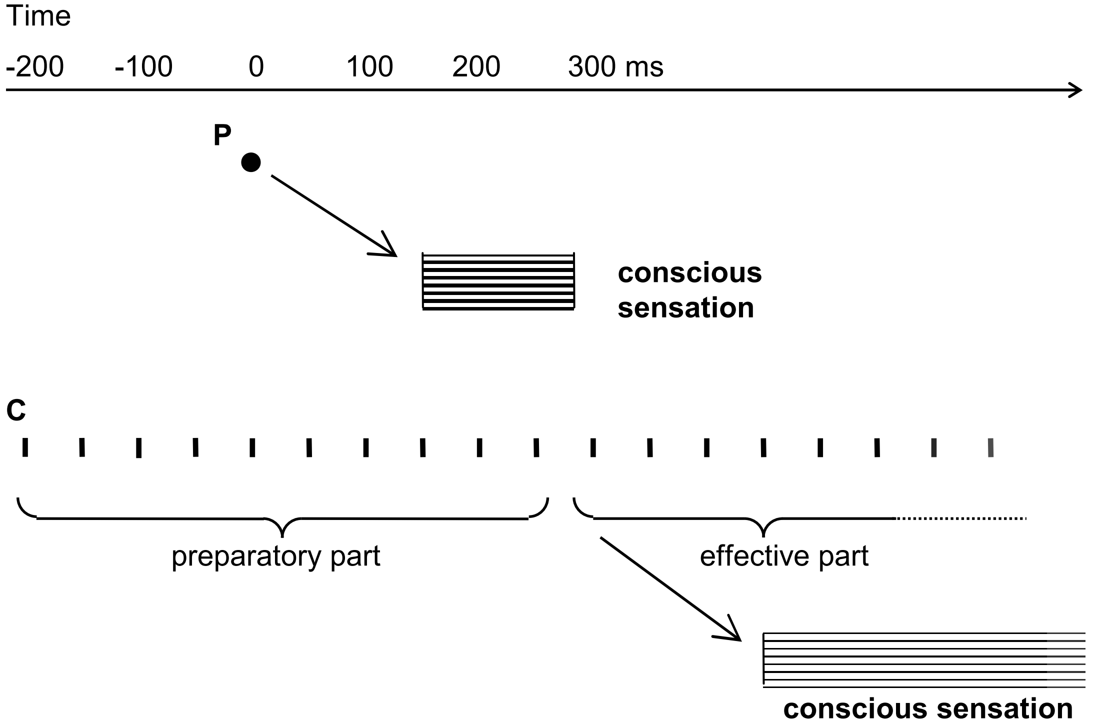

Figure 4 - P 200 ms after the start of C but 300 ms *before* the effective part of C: sensation of P *before* sensation of C: no reversed order
(Length of P- and C-latency shown is merely illustrative)

4.2.2 Three hypotheses for explaining results.

What possibilities are there, then, for explaining these results? The first possibility would be to conclude that P-latency is in fact very short. Consciousness of C would be formed at the end of the Minimum Train Duration and consciousness of P at the arrival of the P input at the cortex. But this is contradicted by the data of backward masking. These, as we have seen in section 3.1, indicate a P-latency of at least 125-300 ms for stimuli at threshold level. And this explanation is also contradicted by the results of P-LM coupling.

The second possibility was not considered by Libet at first. We can suppose that there is a real latency (as defined in 2.3) between the end of the cortical Minimum Train Duration and consciousness of the sensation (call it **C-latency**), and that the duration of this latency is the same as the duration of P-latency (Figures 4 and 5). In this case, coincidence of the conscious sensations of P and C is obtained if P coincides with the end of C-Minimum Train Duration, no matter what the duration of these latencies is. The length of the Minimum Train Duration (determined by the intensity used) is also indifferent. This hypothesis is in agreement with the experimental results, that showed simultaneity of the sensations when P coincided with the end of the C-Minimum Train Duration, whether this was the ‘utilization’ Train Duration (500 ms) or a shorter Minimum Train Duration (200-300 ms).

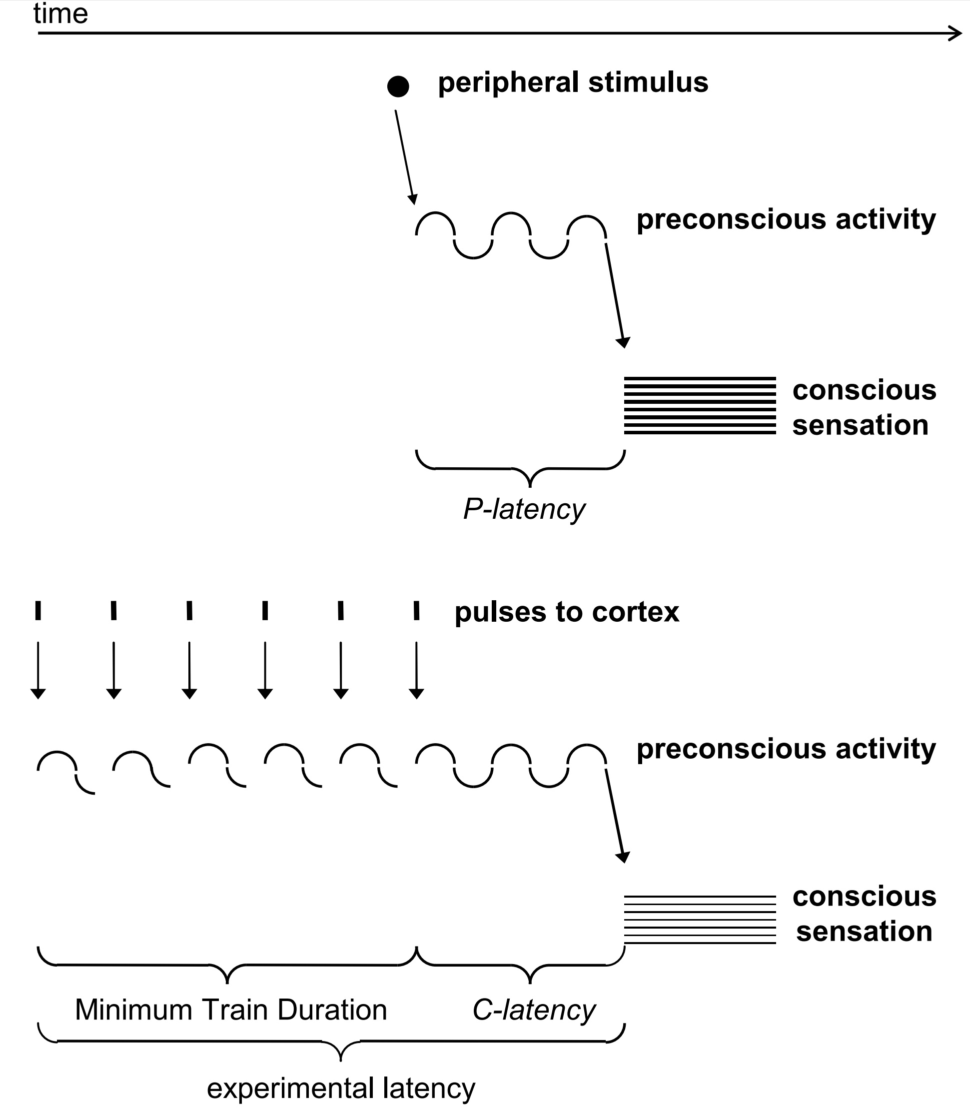

Figure 5 - An alternative interpretation of the experimental results of P-C coupling (Sensations experienced as simultaneous when P is presented at the end of C-Minimum Train Duration): C-latency equal to P-latency

C-latency: latency for conscious sensation after the cortical stimulus has become effective for producing it.
P-latency: latency for conscious sensation of the peripheral stimulus.

The idea of a latency after the Minimum Train Duration was suggested by two critics of his papers (Churchland 1981a; Glynn 1990). Libet seems not to have examined the suggestion and its implications very carefully (Libet 1981, 1991). He tries to refute it with the following argument: "the difference between the subjective timings for the skin-induced and the cortically-induced sensations was approximately equal to the actual minimum duration of the cortical stimulus train

(...) [S]election of different minimum durations for the cortical stimulus cannot be expected to influence the latency of the sensation elicited by the *skin*-stimulus (...)" (Libet 1981, p. 192). But no such expectation derives from the hypothesis in question! All it requires is that C-latency *(after the C-Minimum Train Duration)* be equal to P-latency. (Look at Figure 5 and imagine a longer or shorter P- and C-latency and a longer or shorter Minimum Train Duration.) The "subjective timings" mentioned by Libet are only relative (P perceived before, together with or after C). The *beginning* of the C pulse train is taken by Libet as the reference point for measuring the temporal relations with P. This means that the "difference in subjective timing" includes the C-Minimum Train Duration. If this C-Minimum Train Duration varies, the "difference" will vary as well. On the other hand, if we take the *end* of the C-Minimum Train Duration as the reference point, there is no "difference" in subjective time at all. P and C will be perceived as simultaneous when P is simultaneous to the end of C-Minimum Train Duration. From a practical point of view, it is natural to measure times from the beginning of the stimulus. But for theoretical interpretation of results, what matters is the end of the C-Minimum Train Duration – starting point of that part of the cortical stimulus that corresponds to the conscious sensation. Indeed, from the matching of subjective durations of sensations elicited by P and C (see section 2.3 and Figure 1), we may say that the end of the Minimum Train Duration is the moment of C that corresponds to the moment of the arrival at the cortex of the influx induced by P. This assumption is radically different from Libet's.

The problem with this second hypothesis is that it may be thought that, if we admit a latency for conscious awareness after the Minimum Train Duration for cortical stimuli, we should also assume the existence of a similar latency for LM stimuli. The results of P-LM coupling, however, seem to preclude this assumption.

Libet has proposed a third hypothesis, that of a "subjective referral of conscious experience backwards in time". He has based this hypothesis on the fact that P elicits, after a very short interval (about 15 ms), a primary cortical Evoked Potential, that is absent with cortical stimuli. According to the hypothesis, conscious awareness of a peripheral stimulus occurs later (after about 500 ms), but is referred backwards to the moment of this primary Evoked Potential. This primary Evoked Potential would thus act as a temporal marker for sensory experience. So, if the primary Evoked Potential induced by P coincides with the end of the C-Minimum Train Duration – and admitting consciousness of C occurs at this moment – P will be perceived as simultaneous to C, even if consciousness of P occurs in fact later. Consciousness of P would be "moved" backwards in real time, as some have interpreted Libet's results, or "subjectively referred" to an earlier moment, as Libet himself says.

Before considering the problems involved in this hypothesis, that has had a considerable impact, let us examine the results of P-LM coupling, briefly mentioned in the previous section.

4.3 P-LM coupling

Stimulation of the medial lemniscus (LM) or VPL at Liminal Intensity also requires a Minimum Train Duration of about 500 ms with the usually employed Pulse Frequency. In P-LM couplings, however, as in some of the P-C couplings mentioned above, intensity of the stimulus to brain was higher than liminal, in order to obtain more consistent responses. This gave rise to a Minimum Train Duration of 200-300 ms. (It was not possible to determine if the precise structure being stimulated was LM or VPL. Stimulation was monopolar, the other electrode being a large metal armband. Libet et al. 1979, p. 195). As the stimulation of LM or VPL elicits a primary Evoked Potential in the cortex, Libet hypothesized that it would produce backward referral, just as peripheral stimulation. Seeming to confirm his hypothesis, *results showed that simultaneity of sensations was obtained when P coincided approximately with the beginning of LM.*

Let us consider the first hypothesis we examined in relation to P-C coupling, that of a very short P-latency. According to it, if P is presented at the beginning of LM, it would be consciously perceived a very short time after this moment and so well *before* LM, since LM can only be consciously perceived after the Minimum Train Duration. But results were different, so this hypothesis must be discarded.

Suppose we admit the existence of an **LM-latency** (a real latency, as defined in 2.3, for conscious sensation of LM, beginning at the end of its Minimum Train Duration). If this LM-latency must have the same duration as the C-latency, then the second hypothesis considered in the previous section seems to be also refuted by the results of P-LM coupling. Simultaneity of sensations when P coincides with the beginning of LM would not be possible. Remember that this hypothesis says C-latency and P-latency have equal duration. To arrive at the conscious sensation of LM we would have first the Minimum Train Duration, then a latency equal to P-latency, so conscious sensation of LM would necessarily come *after* the conscious sensation of P. Conscious sensations would only be simultaneous when P coincided with the *end* of the Minimum Train Duration, as it happens in P-C coupling.

Churchland, in her first paper on the subject, proposes the hypothesis of a C-latency (what she calls the 'postponement hypothesis')[6], but does not consider the results of P-LM coupling, which seem to exclude a similar postponement for LM (Churchland 1981a). Glynn, however, does consider P-LM coupling, and offers an explanation of the results. The sum of the hypothetical LM-latency and the LM-Minimum Train Duration would be equal to the sum of the P-latency and the Minimum Train Duration of the peripheral stimulus (Glynn 1990, p. 479). In fact, a train of very weak skin pulses was used in P-LM couplings, instead of a single pulse (as in P-C coupling), in order to produce a sensation of intensity and duration matching those elicited by LM stimuli. But the duration of the P-Minimum Train Duration was only 17-33 ms (2 or 3 pulses at 60 pps) (Libet et al 1979, p. 205). (For this reason I have up to now neglected the P-Minimum Train Duration involved in P-LM coupling.) So, if the latency is supposed to be equal for P and for LM (see above), Glynn's suggestion would not be tenable: the sum of Minimum Train Duration and latency could not be the same in the two cases (because Minimum Train Duration was 200-300 ms for LM and 17-33 ms for P in these experiments).

Reacting to Libet's reply, in which he stresses the difficulty that the results of P-LM coupling present to the postponement hypothesis, Churchland proposes a new hypothesis to account for the data (Churchland 1981b). She supposes a P-latency of about 100 ms and no latency after the LM-Minimum Train Duration. When P coincides with the beginning of LM, since LM-Minimum Train Duration was 200 ms, conscious sensation of P would in principle arrive 100 ms *before* conscious sensation of LM. But subjects are usually unable to detect intervals of only 100 ms, hence the reported simultaneity. Churchland points out that, according to Libet's own data, subjects are highly inaccurate in temporal ordering of sensations when the interval is 100 ms, even when both stimuli are cutaneous. One may wonder why Churchland has chosen to suppose a P-latency of 100 ms, since a value of 200 ms would give perfect instead of only approximate simultaneity of sensations. Perhaps she thought that supposing the absence of an LM-latency (as she did) would imply also supposing the absence of a C-latency. In this case, she would be dropping her postponement hypothesis about C. If so, when P is presented at the end of the C-Minimum Train Duration, the interval between the conscious sensations of P and of C should also, in principle, be of 100 ms, this time conscious sensation of P being predicted to come *after* conscious sensation of C (and not *before*, as in the case of LM). But the same incapacity of detecting the interval (this time in the opposite direction) would explain the reports of simultaneity. This hypothesis, however, would not be tenable. Libet and his colleagues have not only registered responses of simultaneity with P at the beginning of LM and at the end of C-Minimum Train Duration. They have studied various temporal relations between P and LM and between P and C. If we suppose the absence not only of an LM-latency but also of a C-latency, the pattern of responses should be the same for P-LM couplings and P-C couplings, but they are not. In both cases,

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[6] Her figure 3 indicates a C-latency of 300 ms (Minimum Train Duration 500ms; conscious sensation 800 ms after the beginning of C) against a P-latency of 500 ms. This would explain the case in which P comes 200 ms after the beginning of C but not that in which the interval is 400 ms.

reports of simultaneity should be more frequent when P is presented 100 ms after C or LM. But in fact they are more frequent around an interval of 0 ms, in the case of P-LM couplings, and around an interval of -200 ms, in the case of P-C couplings (Libet et al. 1979, tables 2A and 3A, pp. 210, 214-215).[7]

I have said that, if we admit the existence of a C-latency, it may be thought that we must also admit the existence of an LM-latency with the same duration. Indeed, stimulation of LM and of C may be seen as quite similar, since both require similar Minimum Train Durations. However, it must be recognized that the hypothesis of a C-latency is really independent of what may happen with LM stimulation. Besides, there is an important known difference between the

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[7] For those well acquainted with Libet's study, some details and particulars are given here. Libet and his colleagues have first assessed the bias that subjects might have in evaluating the temporal relation between stimuli perceived at the right and at the left sides of the body. They did it by calculating the estimated 'mean shift' in timings of two peripheral stimuli (right and left). They have then calculated the estimated 'change in mean shift' that took place when the second peripheral stimulus was replaced by C or LM. According to the hypothesis of a P-latency of 100 ms and of the absence of either an LM-latency or a C-latency, the change in mean shift (as calculated by Libet et al.) should be of about +100 ms in both cases (P-LM and P-C). In fact, changes in estimated mean shift were, for P-LM couplings: -42, +18, -10 and -30 ms; and for P-C couplings: +220, +454 and +95 ms (Libet et al. 1979, tables 2B and 3B, pp. 211, 215). This confirms the difference between the two cases. – If we adopt the end of the Minimum Train Duration as the reference point for stimuli to brain, as my interpretation suggests, we will obtain different values for the estimated 'mean shift' of such stimuli and consequently for the estimated 'change in mean shift'. From the value of the estimated 'change in mean shift', as calculated by Libet et al., we should subtract the value of the cerebral Minimum Train Duration. This correction would have the advantage of permitting to take into account the fact that the Minimum Train Duration was not the same in all tests. In the case of P-C coupling, we would then have 'corrected' estimated 'changes in mean shift' of +20, +154 and -155 ms. (For subject M.T., Minimum Train Duration was 200-300 ms, so the intermediate value of 250 was used for calculating correction.) In the case of P-LM coupling, we would not only have to subtract the LM-Minimum Train Duration but also add the value of the P-Minimum Train Duration (mean 25 ms) and so we would have -267, -257, -185 and -205 (mean: -228.5). For P-C, the first value (consistent with other observations by Libet) indicates simultaneity of conscious sensations when P coincides approximately with the end of Minimum Train Duration. This may be interpreted as indicating that the durations of P-latency and of C-latency, whatever their value, are equal, in this case. For the other two values, see sections 4.5 and 4.7. For P-LM, the values obtained indicate simultaneity of conscious sensations when P precedes the end of the Minimum Train Duration by approximately 230 ms. This may then be interpreted as indicating that P-latency is about 230 ms longer than LM-latency. Since LM-latency must be ≥ zero, we conclude that P-latency is ≥ ~230 ms.

two: the presence of cortical Evoked Potentials in the case of LM stimulation. With direct cortical stimulation, there is no similar electrophysiological response of the cortex. Libet takes the first primary Evoked Potential to be a time-marker serving the hypothetical backward referral. But the significance of the presence of the cortical Evoked Potential in the case of LM stimulation may be of another order. It should be noted that there is not only one primary Evoked Potential during an LM pulse train, occurring immediately after its beginning. In fact, "[e]ach pulse in the medial lemniscus puts a primary evoked response on the sensory cortex" (Libet 1993, p. 140). So, the responses of the cortex to LM stimulation and to direct cortical stimulation are in fact very different. The former is characterized by repeated Evoked Potentials that are absent in the latter. These Evoked Potentials might effect a preparation of the cortex for conscious experience, concomitant with the building up of an efficient stimulus, so that conscious sensation might occur with a much shorter real latency (after the Minimum Train Duration).

Just as the C-latency is a hypothetical fact, not a logical necessity, its having the same duration as the P-latency is also a hypothetical contingency, not a necessary supposition. Supposing a C-latency with the same duration as the P-latency does not imply any similar supposition regarding LM-latency. The latter may well have a much shorter duration. And the presence of the Evoked Potentials in the case of LM stimulation would possibly be the cause of the difference (or be related to it).

Libet would probably say the hypothesis of an LM-latency much shorter than the C-latency is an *ad hoc* hypothesis made to accommodate the data. But in fact it is more parsimonious than backward referral hypothesis and it should have been considered before the experimental comparison of P-LM coupling and P-C coupling was made. In planning these experiments, Libet assumed that LM-latency and C-latency (as defined above) would be equal. (In fact, he supposed them to be null.) He also assumed, as we will consider soon, that differences in the intensity (and modality) of P would not affect the comparison. But both these assumptions are unwarranted, so these experiments cannot be considered as a suitable test of his hypothesis. The hypothesis of a C-latency much longer than LM-latency is as justified as the hypothesis of a backward referral, before or after the results of these experiments. Besides, as we will see, it is experimentally testable. The fact is that we know too little about the physiological mechanism of conscious sensations to be able to decide, and it is only reasonable to seek the interpretation that is most parsimonious and most plausible, to explain the data available.

We conclude that the second hypothesis, that of a C-latency approximately equal in duration to the P-latency, is tenable, along with the supposition of a much shorter (or nonexistent) LM-latency. I think that, before any new experimental data is available, we should adopt this hypothesis, instead of Libet's, or else another one we will consider in 4.5, or both. We may make it more precise by tentatively proposing also that the duration of P-latency (for the stimulus used in P-LM coupling) is approximately equal to the duration of the LM-Minimum Train Duration used by Libet (about 200 ms) plus the duration of LM-latency (Figure 6)[8].

Libet's hypothesis, the third one, supposing the existence of a backward referral, seems less plausible and less parsimonious.

When Libet tested P-LM couplings, he had already adhered to the backward referral hypothesis, and took results to be confirmatory of it. So these results could not give him any indication of the duration of P-latency. This is because conscious sensations of both P and LM are supposed to be referred backwards to the moment of the initial Evoked Potentials. But if we adopt the hypothesis described above, of a P-latency approximately equal in duration to the sum of the LM-Minimum Train Duration used plus LM-latency, and no backward referral, then the results of P-LM couplings do give us an indication about the duration of P-latency, as Libet had expected from his first P-C pairings. Since the LM-Minimum Train Duration used in these tests was 200-300 ms and P-Minimum Train Duration was 17-33 ms, we conclude that P-latency at near-threshold intensity is estimated to be $\geq$ ~230 ms.[9]

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[8] At the very low Intensity used, the skin stimulus also had a Minimum Train Duration, but this was very short and was not represented in Figures 6 and 7, for simplicity.

[9] For details, see end of note 7.

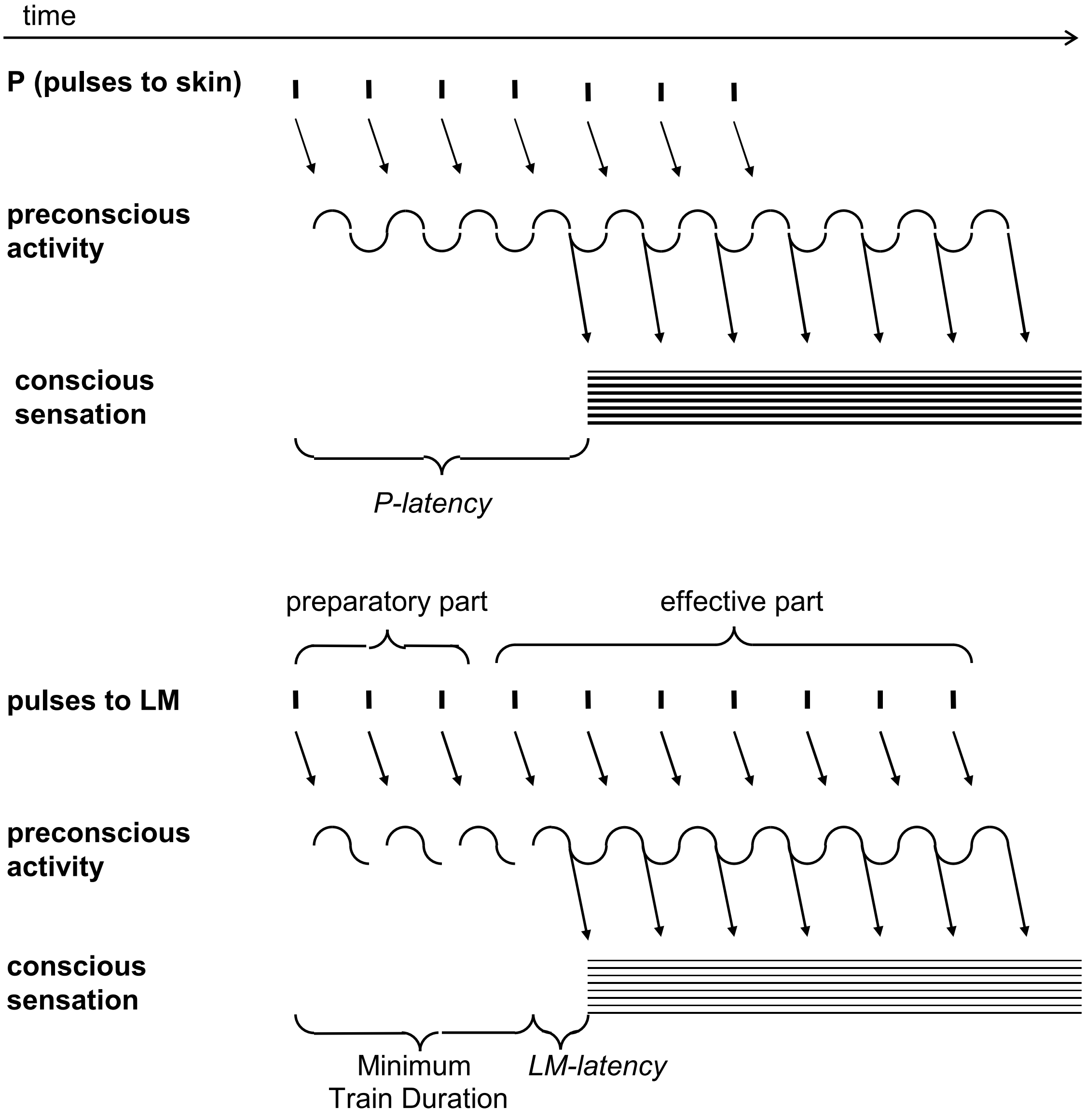

Figure 6 - First interpretation of the experimental results of P-LM coupling: very short LM-latency

LM-latency: latency for conscious sensation after the stimulus to the medial lemniscus has become effective for producing it.

P-latency: latency for conscious sensation of the peripheral stimulus.

4.4 Plausibility of backward referral hypothesis

4.4.1 A "time-travel machine" in the brain?

The phrase 'backwards in time', used by Libet, has been interpreted in a literal way by some, who took it to indicate a backward displacement of conscious sensation in real time. This would either show that consciousness is an immaterial reality that escapes physical laws altogether (Eccles, in Popper & Eccles 1977) or that a new physical theory is needed to explain the physical processes that mediate consciousness (Penrose 1989) . Others, worried by the mind-brain dualism or at least the violation of the temporal succession of cause and effect that backward referral seemed to imply, have tended by all means to attack Libet's data, methods and conclusions.

Libet himself is not very clear on this point. He says that "a dissociation between the timings of the corresponding 'mental' and 'physical' events" might raise serious difficulties for the mind-brain identity theory, but he does not state them explicitly. At the same time, he believes these difficulties are "not insurmountable", but does not say why, either (Libet et al. 1979, p. 222).

However, he never describes conscious sensation as occurring before the achievement of the state of neuronal adequacy that makes it possible, as the effect of a backward flowing of physical time, that makes the effect precede the cause. He speaks of a *'referral'* backwards in time, and qualifies this referral as *'subjective'*. So it seems he considers this referral to take place in *represented time*, not in real time. On the other hand, in a figure often reproduced in his papers, he represents backward referral on the same time scale (that of physical time as measured by the experimenter) used to indicate the moments of presentation of stimuli and the presumed moments of occurrence of sensations. This may suggest an anticipation in real time and is at least misleading, since it blurs the distinction of real time and represented time.

It seems clear that we should dismiss the concept of an improbable time-travel machine in the brain, that is, the hypothesis of a transference of conscious sensations backwards in physical time. But the concept of a backward referral in the conscious representation of temporal succession also brings its own problems.

4.4.2 The succession of conscious sensations.

If we admit the correspondence of mental and cerebral events, and the real existence of consciousness, then there must be a precise moment in physical time when a conscious experience occurs (*pace* Dennet & Kinsbourne 1992). At this moment, two sensations may be experienced as simultaneous or as coming one after the other. We can admit that, prior to this moment, some temporal adjustments may be made. Thus two sensations, originating from the same event through different sense modalities, may be made to be experienced as simultaneous even if they would otherwise have different latencies for awareness. Libet speculates that such a synchronization, taking the primary Evoked Potential as a reference, would prevent the occurrence of a "subjective jitter" (Libet, 1982, p. 241). According to him, there is a referral of both sensations to the moment of their primary cortical Evoked Potentials. I think it makes little sense to speak of referring an experience to a certain moment, for there is no experience of moments *per se*. We do not experience time in itself (Efron, 1967), we only experience the duration of and the temporal relations among the different things we experience. Anyway, the Evoked Potentials could serve as a reference to assure the simultaneity of the sensations in conscious experience. However, the attentional pre-entry effect, studied by Wundt in the 19th century, shows that two simultaneous stimuli can lead to sensations that are not simultaneous. The attended stimulus is experienced as coming before the unattended one, although the primary Evoked Potentials they produce are certainly simultaneous. So the mechanism of synchronization hypothesized by Libet does not work in this case.

Libet's hypothesis of backward referral, however, would make us admit much more than such a mechanism of temporal normalization, occurring prior to the conscious experience. Backward referral would in fact alter the experienced time relations between sensations *after* these conscious sensations have already taken place. The represented sequence of sensations would not correspond to the real sequence *of these sensations*, and this would lead to situations that are rather incompatible with the phenomenology of conscious experience. Let us suppose P is presented at the end of the C-Minimum Train Duration. At this moment, according to Libet's assumption of no C-latency, the subject would have the conscious sensation of C, only C. After some hundreds of milliseconds, the conscious sensation of P would occur, but that sensation, due to backward referral, would be represented as having occurred at the same time as the conscious sensation of C. Now how could this representation not be in conflict with the representation of C as having already occurred alone? Why would the subject not have the memory of the preceding sensation of C?

Due to the instructions given before the experiment, the subject expects the stimuli and has already in mind the question he must answer: Did the two stimuli come together or, if not, which one came first? When the conscious sensation of one of them is formed, he immediately answers this question to himself. How could a later sensation change this answer without his noticing the change?

Let us compare this situation with that of the perception of a Necker cube. When the subject perceives the cube in a new position, this representation replaces the previous one completely. But the subject notices the change. He keeps the memory that he has perceived the cube in another position earlier, even if the lines on the paper have not changed. This means we are not only conscious of things in the world but also conscious of being conscious of them. And we keep a memory of this. We know the drawing has not changed, but that our perception did. In Libet's experiment with P and C mentioned above, the subject should report the curious sensation of having first perceived C alone, and then C and P as having occurred at the same time.

If P is presented 200 ms after the beginning of C, as illustrated in Libet's Fig. 1 (Libet et al. 1979), we would have an even stranger situation. In this case, the subject would feel C before he feels P, but when he does feel P, due to backward referral, he would perceive it as having occurred *before* C! Libet gives no explanation why the subject reports no memory of a previous perception of the cortical stimulus as coming first.

4.4.3 Different senses of 'backward referral'

Of course, the term 'backward referral' can be used in different senses. For instance, a conscious perception can be placed in the context of a previous conscious perception, and this can be called 'backward referral'. In this sense, we can say that, in speech perception, a later part of a long sentence is referred backwards to earlier parts of the sentence. Another situation may also invite the use of the expression 'backward referral'. You may have already had the experience of hearing something that someone has said without understanding it. And then, as you are asking the person to repeat it, the repetition is no longer necessary, because you are now aware of what she or he has said. Of course, you do not have the experience of hearing a second sentence when you realize what has been said. So we can say that the words that you now identify are *referred backwards* to the uttering you have just heard. But in this case, you have the memory of your first impression. Now you know what the person said, but you also know that at first you were not able to discriminate these words. So these cases are not equivalent to what Libet has described as 'backward referral'. Rejection of Libet's backward referral does not imply rejection of these other processes of 'backward referral'.

4.5 Differences in latency

Individual differences in latency and differences in latency for different sense modalities are also possibilities that have not been considered by Libet. With some of the subjects tested for P-C coupling, we are surprised to find a visual stimulus (a weak flash of light) replacing the cutaneous shock. Statistical analysis of results of subject M.T. indicate that simultaneity of conscious sensations of P and C would theoretically occur if P (flash) was presented 155 ms *before* (and not at the same time as) the end of the C-Minimum Train Duration (see note 7). This may indicate that *visual* P-latency is longer than C-latency. For subject C.J., on the other hand, theoretical simultaneity of conscious sensations would occur if P (cutaneous shock) was presented 154 ms *after* the end of the C-Minimum Train Duration. This may indicate that cutaneous P-latency was *in this patient* shorter than C-latency.

Differences in P-latency due to intensity of P must also be considered. Backward masking suggests that P-latency is shorter when P is stronger. Now, intensity of P was stronger in P-C coupling than in P-LM coupling. In P-C coupling, P was a single pulse, with intensity "distinctly above the threshold" (Libet et al. 1979, p. 213). In P-LM coupling, intensity of P was set at a level that made single pulses subliminal, so it was significantly weaker (Libet et al. 1979, pp. 205, 212). *This difference alone could account for the difference in relative timing between P-C and P-LM couplings*. LM-latency might after all have the same duration as C-latency (see Figure 7). The observed difference would have resulted from the intensity parameter of P in the actual experiments (causing a different P-latency) and not from a difference in cerebral real latency, as I have hypothesized in 4.3 (nor from the respective absence and presence of backward referral, as Libet supposes).

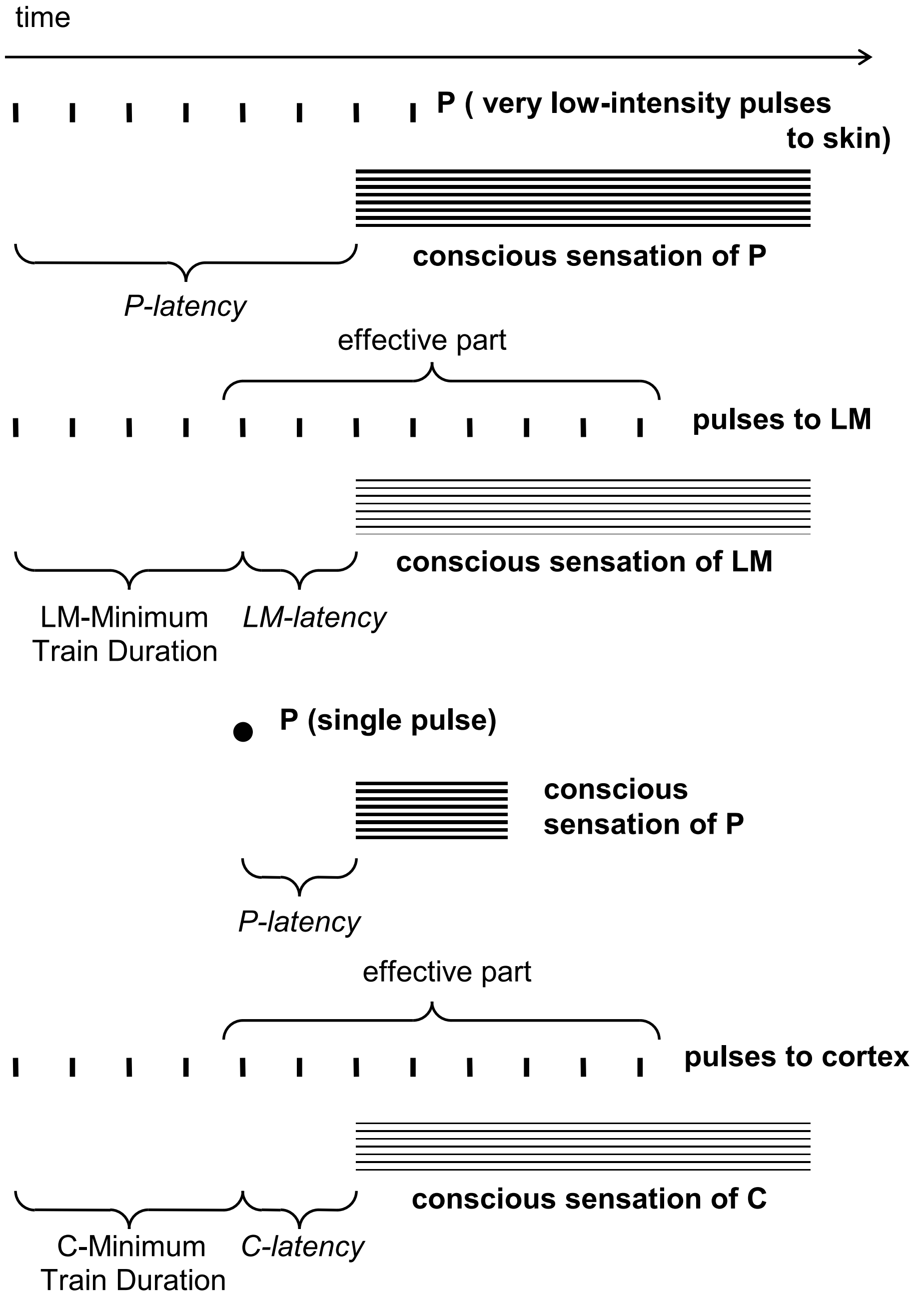

Figure 7 - Second interpretation of the experimental results of P-LM coupling, compared with the interpretation of the experimental results of P-C coupling: LM latency as long as C-latency but P-latency longer in P-LM coupling, due to smaller P intensity.

4.6 Experimental conditions for testing the alternative hypotheses

Both alternative hypothesis I have proposed (4.3 and 4.5) are experimentally testable. Figures 5 and 6 represent the first alternative hypothesis. Look at Figure 5. Conscious sensations of P and C are simultaneous when P coincides approximately with the end of the C-Minimum Train Duration (whatever its value). This is in accordance with Libet's experimental results using two different Intensity levels (that cause Minimum Train Durations of about 200 ms and about 500 ms). Now look at Figure 6. Conscious sensations of P and LM are simultaneous when P starts about 200 ms before the end of the LM-Minimum Train Duration. In Libet's experiments, this coincides with the beginning of LM, but using different Intensities, that cause different Minimum Train Durations, this would no longer be the case. Looking at Figure 6, imagine a shorter Minimum Train Duration. P would then have to be presented *before* the start of LM to obtain coincidence of sensations. Now imagine a longer Minimum Train Duration. P would now have to be presented *after* the start of LM.

These are the predictions of this hypothesis. Its experimental test would then be to check the effect of different Intensities of LM, causing different Minimum Train Durations, on the simultaneity of sensations.

Now look at Figure 7, that represents the second alternative hypothesis. Here LM-latency is supposed to be as long as C-latency. The prediction of this hypothesis is that if we used in P-C coupling a P pulse train as weak as the one used in P-LM coupling, we would also have coincidence of sensations in P-C coupling when P is presented about 200 ms before the end of the C-Minimum Train Duration.

Note that an intermediate situation between these two hypotheses could also obtain. A part of the observed difference between P-C coupling and P-LM coupling could be due to a shorter LM-latency, and the rest to a longer P-latency in P-LM coupling.

4.7 Quality of the evidence

Experiments involving the temporal relation of stimuli to skin and to brain seem to be very difficult ones. The first observations were done during surgical procedures. Others were done in a more favorable situation, with patients with chronically implanted electrodes but even so they require the availability and cooperation of the patients and control of the many variables involved. Technical limitations of the studies are certainly a consequence of such difficulties.

The quality of the experimental results obtained in these tests and the degree to which they may be considered supportive of Libet's conclusions have been the object of criticisms (Churchland 1981a; Glynn 1990; Dennet & Kinsbourne 1992). Libet replies to these criticisms with the argument that the paper passed the rigorous evaluation by the reviewers of *Brain* (Libet 1981, p. 183; Libet 1992a, p.214). I think the editors of *Brain* were certainly right in publishing this difficult and pioneering research, but its technical limitations should be recognized.

The number of subjects studied was really small and many observations were unsuitable for establishing the relevant relations. For P-C coupling, Libet and his colleagues report that a limited number of observations were made with six patients, 5 during the surgical procedure (before 1969) and 1 with implanted electrodes (in 1970). They do not give a table of results for these observations, but only say that "the pooled reports were predominantly" those of P perceived before C when P followed C up to almost the full value of the C-'utilization' Train Duration (Libet et al. 1979, p. 200). For a second group of observations, the authors give a table of results (Libet et al. 1979, pp. 214-217), presenting data that must really be considered poor and little conclusive.

Results of patient J.W., presented first, are in fact the only ones that clearly show the pattern of simultaneity of conscious sensations when P is presented at the end of the Minimum Train Duration. With patient C.J., Minimum Train Duration was ~300 ms, but coupling was only tested with P 200 ms after, at the same time as, and 200 ms before the beginning of C. The most interesting coupling would of course be that with P 300 ms after the beginning of C, that is, at the end of the Minimum Train Duration. Tests with P *after* the end of the Minimum Train Duration (400 ms and more after the start of C) would have also been interesting. Statistical treatment of the available data indicates, however, that simultaneity of conscious sensations would theoretically be obtained in this patient if P was presented 454 ms after the beginning of C (see note 7), which is rather late in relation to the 300-ms Minimum Train Duration. As noted in the previous section, this might be due to individual differences in P-latency and/or C-latency. For patient M.T., Minimum Train Duration was 200-300 ms and conscious sensations were in fact most often simultaneous when P was presented 200 ms after the beginning of C. But there were also many reports of simultaneity when P was presented at the beginning of C. Statistical treatment indicates theoretical simultaneity of conscious sensations with P 95 ms after the beginning of C (see note 7), that is, rather early in relation to the Minimum Train Duration (200-300 ms). But with this patient P was a visual stimulus, as noted above. With patient O.K., results were not statistically treated because coupling involved stimuli perceived in the same side of the body while control coupling of two P stimuli involved different sides. The authors say that "results for subject O.K. (...) are in qualitative agreement with those for J.W., C.J., and M.T." (p. 216). I cannot see, however, much agreement, since most of O.K.'s reports indicated P sensation first, with P presented up to 500 ms after the beginning of C, Minimum Train Duration being 400 ms in his case. Other tests involving one of these and two other patients used a different type of cortical stimulus (polarity reversal instead of cathodal pulses). Results with these stimuli were not conclusive, since subjects tended to report P and C sensations 'together', regardless of the coupling interval. New experiments are certainly needed to make the situation clearer.

Results of P-LM couplings were more consistent, but they involved only two subjects.

5. The timing of conscious intention to act

5.1 Readiness potentials

In 1964, using a new method of reverse computation of stored electroencephalographic (EEG) data, Kornhuber and Deecke discovered that self-paced voluntary hand or foot movements are preceded by a slow negative cortical potential (Kornhuber & Deecke, 1965). The potential was called *'Bereitschaftspotential'* or 'readiness potential' (RP), this term implying a process of preparation for the movement. It occurs bilaterally, mainly over parietal and precentral cortex. It is impossible to observe this potential in the EEG tracing that precedes a single movement. It is necessary to average tracings from at least 20 trials to distinguish it from the background 'noise'. In a later series of experiments, average onset of RPs was 750 ms prior to finger flexion, and the authors distinguished two other potentials superimposed on the RP: the pre-motion positivity (PMP), starting on the average 90 ms before movement, and the motor potential (MP), starting on the average 50 ms before movement (Deecke et al. 1976). They also observed a contralateral preponderance of negativity (CPN) over precentral cortex, starting about 400 ms before movement. This extra negativity over the contralateral precentral cortex increases at first slowly; it seems to be responsible for the reduction or absence of the PMP over this area; and then it rises abruptly as the MP.

Searle believes "there's no mental reality to the formation of a readiness potential" and objects to its being considered as indicative of an unconscious mental phenomenon ('Discussion', in Libet 1993). However, a voluntary movement is undoubtedly the result of mental events and since the readiness potential is unequivocally correlated to voluntary movements, I believe it must be considered to be a consequence of neural processes that correspond to mental events, even if these go beyond what we are introspectively aware of. And the long interval between RP onset and the movement indicates then that the mental events that cause a voluntary movement have an initial nonconscious phase.

Kornhuber discussed the readiness potential in terms of volition, and argued that the process involved is not one of attention (Kornhuber 1984). Libet had the merit of recognizing the relevance of these findings for the question of consciousness. After all, the movements studied were performed after a voluntary decision that was conscious. It is somewhat surprising to find a specific cortical activity preceding voluntary movements by such long intervals. One has no conscious experience of such a long interval between deciding to move and the movement itself. Subjects were not told to decide to move, wait half or one second and then do it. They were asked just to make the movement at the moment they decided to. One has no consciousness of a specific mental activity preceding a movement such as this. So what does this neural activity correspond to? It is no response to some stimulus. It is not the *effect* of any specified event. It *precedes* an event with which it has a constant relation, that is, the movement, so one would be inclined to consider it as its *cause*. But the cause of a voluntary movement is usually considered to be a conscious decision. So where should we situate temporally this conscious decision? And what can be its relation to the development of the readiness potential? These were the questions that Libet asked and tried to answer by experimental investigation.

First, Libet and his colleagues tried to improve the experimental situation for the study of spontaneous voluntary movements. They tried to minimize the time constraints imposed on the subjects: (1) by making each trial an independent event, preceded by a relaxation period; (2) the subject was allowed to blink, he/she was simply asked to wait for at least another indicated period of 2.56 s before performing the movement; (3) there was no limitation on the time in which to perform the act. Furthermore, an additional instruction was introduced after some experiments, asking the subjects "to let the urge to act appear on its own at any time without any pre-planning (...) i.e., to try to be 'spontaneous'" (Libet et al. 1982).

Among the RPs obtained, the authors distinguished 3 types, according to the form and the onset time. Type I had onset prior to –700 ms, and in some an inflection at about –500 ms appeared to be discernible. Type II had onset between –700 and –400 ms. Type III had onset about –250 to –200 ms. In some cases, the introduction of the instruction for spontaneity induced a change from type I to type II in the same session. After a series of 40 trials, subjects were asked whether they had been aware of 'pre-planning' to act in some of the trials. In those series in which no pre-planning was reported, RPs were type II or III, that is, they were shorter. In 8 out of 9 series in which pre-planning was reported, even if only in a minority of the 40 acts, RPs were type I, that is, longer. These results indicate that the initial phase of longer RPs may represent a phase of pre-planning of the act or of expectation of the moment of deciding to move. Even with Libet's procedure, subjects are asked in advance to make a specific movement within certain time constraints (after a certain moment, during a non-blinking period). The intention to move is formed in advance, only the moment of the act is left relatively undetermined. Even the requirement that the moment of the act should not be pre-determined may be regarded as a pre-determined condition. The subject may be considered as being in a state of expectation of being able to fulfill the pre-determined conditions. So the longer RPs may be considered as corresponding to a process of preparation to comply with the experimental instructions.[10]

5.2 Nonconscious motor acts

In Kornhuber and Deecke's experiments and in Libet's as well, conscious voluntary acts were studied. It was natural to suppose that the neural processes evidenced by RPs were specific of such conscious and voluntary acts. However, this assumption was refuted by a very interesting study by Keller and Heckhausen, in which it was shown that RPs also precede nonconscious movements (Keller & Heckhausen 1990). Subjects were asked to perform a mental counting down task and when the EMG indicated a finger movement, they were asked whether they were conscious of having moved. 301 trials in which they were not conscious of having moved were averaged, and RPs quite similar to usual ones were obtained. This shows clearly that RPs are not exclusive to conscious voluntary acts. So it seems that the fact of becoming conscious of a decision to act is not what is primarily reflected in the RP. The RP seems to primarily reflect the process of generating a spontaneous act, whether conscious or nonconscious.

Libet argued that subjects might have been conscious of the intention and of the movement but might have forgotten this because of the distracting task (Libet 1992b). This is hard to accept because subjects were inquired immediately after the movements (delay not greater than 10 s; Keller 1992). It is more reasonable to think that the distracting task in fact prevented the consciousness of the act, as was the authors' intention, and not the remembering of it. Moreover, very similar RPs were obtained with nonconscious movements in the absence of any distracting task. Libet dismisses the latter results with the allegation that movements in this condition were reported to be rare (Libet 1992b), but even so there was a total of 102 movements in 8 subjects (Keller & Heckhausen 1990).

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[10] The subject need not have any conscious experience of this preparation process. However, there may be some conscious experience. And it is interesting to note that one of the subjects described what happened within him before some of the movements as "pre-tensions" rather than pre-plannings (Libet et al. 1982).

Keller and Heckhausen's study point to the important conclusion that conscious and nonconscious acts share more than what a certain conception of conscious voluntary acts would be ready to admit. Not only the final motor command and its immediately prior programming but also the preparation and initiation of both kinds of act seem to have much in common. Consciousness of the intention and consciousness of the act itself seem to be processes that are added to the process of generating and performing a nonconscious motor act. This is consistent with a concept of consciousness I have elsewhere advocated (Gomes 1995).

There were some differences, however, between RPs obtained with conscious and nonconscious motor acts. Keller and Heckhausen replicated Libet's experiment with the same subjects tested for nonconscious movements. Mean onset of RP was –420 ms for conscious movements (Libet's situation) and –462 ms for nonconscious movements. Amplitude of the RP was greater with conscious movements. Amplitude distribution for the various electrode positions was different for conscious and nonconscious movements. A difference that was not mentioned by the authors can be seen on the tracings presented in their figure 1. RPs obtained with conscious movements, as is often the case with RPs obtained in other studies, present (at electrode positions Fz, FCz and Cz) a peak of negativity 10-20 ms *after* 0-time, that is, after the start of muscle contraction indicated by the EMG. This peak is almost completely absent in RP tracings for nonconscious movements. This difference and/or some of the ones previously mentioned may reflect the neural processes that lead to consciousness of the decision and/or of the preparation for the movement. Anyhow, there is a striking similarity between the two sets of RPs for nonconscious movements (with and without distraction) and between the two sets of RPs for conscious movements (Libet's situation and resting situation); and an observable difference between the former and the latter (compare figs. 1 and 4 in Keller and Heckhausen 1990).[11]

5.3 Timing of consciousness of intention to act

The word 'intention' may denote the volitional state in which one has decided to do something at some future time. Libet uses it to rather indicate the decision or urge that immediately precedes the act itself. I prefer to use the word 'decision' in this sense.

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[11] This similarity involves not only the amplitudes for each electrode position (see fig. 5 in their paper) but also the general form of the potential, even after the movement.

There is a part of the research conducted by Libet and his colleagues that I have not yet described. Subjects were asked to look at the center of a circular screen. After the relaxation 'get-ready' period, a light spot started revolving on the screen in a clockwise circle. Each revolution took 2.56 s instead of the usual 60 s taken by the second-hand of a clock. Numbers indicated each '5 s' position and lines indicated each '2.5 s' space. So the smallest subdivision to which the subject could refer corresponded to 106 ms of real time. Subjects were asked to perform an abrupt flexion of the fingers of the right hand, at any moment they chose to do it, after at least one full revolution of the spot without eye-blinks. In some series of trials, named M, subjects were asked to note and report (after a few seconds) the time of conscious awareness of actually performing the movement (Libet et al. 1982, 1983a). In other series, named W, they were asked to note and report the time of appearance of conscious awareness of 'wanting' to move. In still other series, named S, the subjects were not asked to perform any movement. Instead, an electrical near-threshold pulse was applied to their hand and they were asked to note and report the time of conscious awareness of the sensation (Libet et al. 1983a). Times reported by the subject were then related to the time of movement onset, in series M and W, and to the time of the stimulus, in series S.

This amounts to a classical experiment in psychophysics, in which introspective reports are related to a physical measure (in this case, time) of a stimulus or movement. At the same time, RPs were recorded, and the authors investigated the relation of subjective timings to RPs. But before addressing this relation, it is necessary to examine the psychophysical experiment itself. Problems with this psychophysics of intention (decision) were pointed by specialists in experimental psychology (Breitmeyer 1985; Rollman 1985; Scheerer 1985; Stamm 1985).

A first problem concerns the possibility of distinguishing W and M moments. Breitmeyer says that "the actions investigated by Libet have been performed (by myself and several of my colleagues) without awareness of intent to act. By requiring subjects to attend to awareness of intent, Libet may have imposed intention artificially (...)" (Breitmeyer 1985). I invite the reader to perform quick finger flexions at irregular intervals. One usually does not have first a distinct awareness of wanting to move and then a distinct awareness of moving. We usually have just a unitary awareness of voluntarily moving. Of course, we also have the feeling that if we wanted to avoid performing the movement, we could have done so. Occasionally, we may have the awareness of having had 'the thought' of making the movement without in fact performing it. Possibly, we may sometimes have the impression of having been 'taken by surprise', as if the movement had arrived 'on its own', without our having voluntarily caused it. (These two possibilities sometimes occurred to Libet's subjects.) But this does not mean that the decision to act and the act itself are usually experienced as discrete events with different assignable times of occurrence. All we usually know is that the decision *must* precede the movement, or, at most, that it *immediately* precedes the movement.

I tried to put myself in the subjects' situation, performing the finger movements while looking at a target that moved around a clock at about the same speed used in the experiments. In order to be able to assign a different time to the intention, I tended to give myself a mental verbal order, such as 'Now' or 'Go'. Even without such mental verbal orders, I had in a certain sense to 'create' a mental motor command, separating it artificially from the movement itself, in order to be able to assign a special timing to it. Other people I inquired had similar feelings. It seems that Libet exaggerates when he says that subjects had no difficulty in distinguishing W and M moments. I guess they have tried to do their best to comply with the experimenter's presuppositions, manifested in the instructions.

According to Libet and his colleagues, "[t]he fact that instances of 'surprises' were reported increases confidence that the reports of timing prior to the act represented endogenous experiences not defined or induced by the instructions" (Libet et al. 1983a, p. 627). But there are various elements suggesting that these reports were in fact induced by the instructions. "It was *emphasized* that the reported time should refer to the *earliest* appearance of the W awareness that might *precede* an actual movement" (Libet et al. 1983a, p. 627, italics added). The suggestive force of these instructions is evident. "In M series, the time of the subject's *awareness* that he/she *'actually moved'* (...) was to be noted and reported. This was intended to provide an instruction for timing an experience *related to but different from that of W*" (Libet et al. 1983a, p. 627, last italics added). It seems that the introspective effort induced by the requirement of reporting a W time different from M was noted by some subjects: "Some subjects stated, on their own, that their mental set differed somewhat in W *vs* M series. In a W series there was a feeling of *active attention to* or *'watching for'* the awareness of wanting to move, *so as to be able to note the time of its appearance* (...)" (Libet et al. 1983a, p. 627, italics added). The problem is that, as Scheerer notes, "in introspection we find only those events that we have been led to expect" (Scheerer 1985). Libet and his colleagues obtained a difference of about 115 ms between W and M (W preceding M) (Libet et al. 1983a, table 2D, pp. 631-632). The significance of this numerical value, however, must be questioned considering the suggestive effect of the instructions, leading to an expectation of W earlier than M.

A second problem has to do with *training*. Subjects' average assessments of clock position for S and for M might have more value if Libet and his colleagues had abstained completely from interfering with the subject's responses. However, the authors state: "After each S series, whether for training or experimental purposes, the subject was given a rough indication of how close he was to the actual times for the stimuli, as an aid in improving accuracy in all the experiments" (Libet et al. 1983, p. 627). The first (and, in some cases, the second) half-day session was purely for training purposes. The 4 subsequent sessions started with a training S series of 25 trials (in which, after each 5 trials, the subject was told the actual 'clock-times' of stimuli), followed by the three 40-trial experimental series (in the order W, M, S or M, W, S). It is obvious that this training may have altered the subjects' assessments in a variable way, for the subject may learn (more or less, and forget after some time) to correct, at least in part, a systematic error (bias) in his assessments. This error may be due, among other factors, to the situation itself of having to compare an instantaneous event with the position of a very rapidly moving spot. This correction may have also affected M and W assessments. This seems to have really happened. Assessments of S presented the systematic error of anticipation relative to real time of stimulus deliverance in most sessions. Assessments of M were usually also anticipated relative to movement time. Sessions in which the M series was done first yielded smaller anticipation for M in relation to sessions in which it was done after the W series (table 2D in Libet et al 1983a). This means that anticipation in the assessment of M was less pronounced when the M series was done immediately after the training S series with feedback. The interval between the timing given for W and EMG time was also smaller in sessions in which the W series was done immediately after the training series.

A third problem concerns latencies for conscious sensation. In S series, the subject compares the moment of conscious sensation of a near-threshold cutaneous stimulus with that of a bright visual stimulus. If we accepted Libet's hypothesis of backward referral (section 4.2.2), we would admit that both conscious sensations would be referred to the time of arrival of stimulation at the cortex, that is, about 15 ms after the occurrence of the peripheral stimuli. Libet reasoned that, if the subject gave timings for S that coincided approximately with the real times of the stimulus, this would validate timings given for M and W. If they did not, the 'shift' in S timings could be used to correct values given for M and W, since the reporting situation was the same in both cases (Libet et al. 1983a, pp. 630-631, 637-638). However, we have reason to reject backward referral hypothesis (sections 4.3, 4.4, 4.5). So, latencies must be considered. Latency for conscious sensation is probably longer for weaker stimuli (section 3.1) and it may also be different for different sense modalities. Complexity of the stimulus should also be taken into account. It probably takes longer to consciously perceive the position of a spot relative to a frame of reference than to consciously perceive the simple occurrence of a cutaneous shock. The effects of different latencies was very adequately discussed by Rollman (1985). Libet's answer to his commentary is biased by his adherence to the backward referral hypothesis (Author's response, in Libet 1985, pp. 559-560). If there is no backward referral, 'clock-times' reported for S or M do not correspond to real times of S or M conscious experience. To arrive at the real time of S or M conscious experience one would have to add the unknown latency for conscious clock reading to the reported clock-time. And to compare reported S time with real S stimulus delivery time, we must consider (in addition to difficulties inherent to the simultaneous observation of clock and skin stimulus) that two (probably different) latencies are involved: latency for conscious clock reading and latency for conscious perception of the skin stimulus.

Since the reported clock-times are somewhat unprecise assessments done by the subject, to which we should add an unknown value, we conclude that we can attach no value at all to such reports as an indication of the real time of occurrence of the conscious experience. Regarding S, we are also unable to conclude anything from the observed discrepancy between reported times and times of stimulus deliverance. It may result from difference in latencies (as Rollman suggests) and/or from the difficulty in comparing the moment of the skin stimulus with the position of the spot.

Libet emphasizes as an experimental finding of his research that the onset of the RP precedes awareness of intention to move by substantial amounts of time. But there was no need of the unconvincing "W" timings to conclude this. As noted above, voluntary movement is normally perceived to come so *immediately* after the decision to act that no interval between them is usually discernible. RP duration can be directly compared with the phenomenology of consciousness to conclude that voluntary and conscious movements are nonconsciously initiated.

5.4 Conscious control of voluntary action

Once an RP is initiated, can the movement still be avoided? Libet and his colleagues asked the subjects to prepare to perform a movement at a preset time and "veto" this intention 100-200 ms before this time (according to the clock they were observing) (Libet et al. 1983b). This produced RPs, even though there was no movement. So, development of an RP does not indicate that a movement will necessarily occur. Of course, the "veto" was also pre-set and we can imagine that it may have been preceded by its own nonconscious preparatory phase.

Since voluntary movements are nonconsciously initiated, does consciousness have any function in voluntary action? First of all, there may be a prior conscious intention (as in RP experiments) even though the immediate initiation of an act is nonconscious. But is the belief that voluntary acts immediately follow a *conscious* decision just an illusion? I think it is important to distinguish here two senses of 'conscious decision'. A decision may be called 'conscious' (1) because it results from conscious awareness of something or (2) because one is conscious of it. (Usually both things occur and the two senses are merged in one.) Can the fact of becoming conscious of a decision to act have any influence on the act itself? Libet argues that if it can lead the subject to veto the act, this would mean that there is conscious control of conscious acts, even though they are nonconsciously initiated. He takes this veto to be conscious in sense (2) but I think the relevant sense here is sense (1). The veto is conscious because it is the result of consciousness of the decision (among other factors). It is not necessary that the subject be conscious of the veto itself *before* inhibiting the act. That is, the veto will also become conscious, but it can exert its effect before this. And even if the veto is initially nonconscious (in sense 2 above), it is evidence of *conscious* control (in sense 1), since it is a result of conscious awareness. The important point is that this veto could not occur in the absence of consciousness (of the decision), so consciousness has a function in conscious voluntary acts. We can picture the sequence of events as: (1) nonconscious decision to act → (2) consciousness of decision to act → (3) nonconscious veto ('conscious' control since it is a consequence of step 2) → (4) inhibition of movement; (5) consciousness of veto. (Besides, step (1) itself may be a result of conscious awareness, so it may be called 'conscious' in sense 1.)

So, we need not agree with Libet when he says: "For *control* of the volitional process to be exerted as a *conscious initiative*, it would indeed seem necessary to postulate that conscious control functions can appear without prior initiation by unconscious cerebral processes, in a context in which conscious awareness of intention to act has already developed" (Libet 1985). Conscious (sense 1) control functions may occur at first nonconsciously (sense 2), after conscious awareness of intention to act, and then become conscious (sense 2).

6. Conclusions

6.1 Duration of P-latency

Libet emphasizes 500 ms as an estimate of P-latency (for stimuli at threshold intensity). According to him, "conscious sensory awareness can lag behind the real world by as much as 0.5 s (depending on intensity of input)" (Libet et al. 1991). We have questioned this value itself and we must also question the emphasis on threshold intensity. We have seen that the inference of P-latency from cortical ‘utilization’ Train Duration (or any other Minimum Train Duration) is not justified (sections 2.2 and 2.3). Results of backward enhancement are also unsuitable for such an inference (section 3.2). Libet's results of cortical backward masking of a peripheral stimulus indicate 125-300 ms as a minimum value for the usual P-latency for somatosensory stimuli at threshold intensity (section 3.1). Results of P-LM coupling suggest, according to my interpretation of them, a minimum value of 230 ms for somatosensory P-latency, for stimuli consisting of pulses that are individually subliminal and near the threshold for consciousness when repeated a few times (section 4.3). We should emphasize that for most usual stimuli (of stronger intensity) P-latency is probably shorter than that for stimuli near the threshold for consciousness.

6.2 Backward referral hypothesis

Pending further evidence, backward referral hypothesis, as formulated by Libet, should be abandoned (sections 4.3, 4.4, 4.5). P-C coupling results are explained by the hypothesis of a latency after the end of the cortical Minimum Train Duration having roughly the same duration as the P-latency for skin stimuli of supraliminal Intensity (section 4.2.2 and Figures 4 and 5). P-LM coupling results are explained by a shorter LM-latency (section 4.3 and Figure 6) and/or by a longer P-latency due to the weak intensity used for P (section 4.5 and Figure 7).

6.3 Conscious awareness and voluntary movements

The long duration of RPs preceding spontaneous movements, whether conscious and voluntary or nonconscious, indicate that both kinds of movements are nonconsciously initiated. The validity of measures of W and M moments is questionable due to problems involving latencies, the effects of training and the suggestive influence of instructions on introspective distinction of W and M. Veto of intended actions may be initially nonconscious but dependent on conscious awareness of the decision to act.

7. Suggestions of experimental research

To further elucidate the issues involved, the following investigations would be desirable:

(1) to study P-C and P-LM couplings with a greater number of subjects, to compensate for and evaluate the role of individual differences;

(2) to study P-C coupling with the same intensity levels, relative to the threshold for conscious sensation, and the same duration of P used in P-LM coupling, to enable a better comparison of P-C and P-LM coupling results;

(3) to perform cerebral backward masking experiments in the same patients tested in P-C coupling and (if backward masking can also be obtained with LM stimuli) in P-LM coupling, to evaluate and correct for individual differences in P-latency;

(4) to study P-LM coupling with different intensity levels of the LM stimulus, and so different Minimum Train Durations, to test the hypothesis that simultaneity of conscious sensations will be obtained when P precedes the end of the LM-Minimum Train Duration by a fixed interval (~230 ms according to Libet's data).

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