DISCUSSION:
THE TIMING OF SENSATIONS: REPLY TO LIBET*

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Evidently Libet would prefer that philosophers just accept as correct his hypothesis that there is backwards referral of experiences, and that we restrict our attention to sorting out the 'philosophical' consequences for the mind-body problem. But matters cannot be divided and despatched with such simplicity, for if we are to understand the implications of the hypothesis, we must first understand the hypothesis itself, which in turn necessitates understanding how it is supported, whether the data are better explained by alternate hypotheses, and so forth. The boundaries between science and philosophy cannot be sharply drawn, and *qua* philosopher, one is sometimes obliged to decline to simply take the word of a scientist that his hypothesis is so well supported as to be accepted, and hence obliged to examine carefully and thoroughly the basis of the case. My analysis (1981, pp. 165–81) of Libet's work focused both on his experimental results, and also on whether his results, if interpreted as showing backwards referral, imply anything significant for the mind-brain identity thesis. My evaluation was harsh, in that I found much to criticize on both counts. In studying Libet's reply (Libet 1981, pp. 182–97) to that analysis, I do not find anything to assuage my misgivings, nor anything which would incline me to revise my judgment. There are however, several matters raised in his reply to which a response is necessary. In what follows, I shall discuss briefly his results from medial lemniscus stimulation, and the matter of "adding 100msec." to establish 500msec. as the period of neuronal adequacy for skin stimuli. In the appendix is the data table for the reaction time experiment conducted by David Martin and me. Accompanying the table is a brief description of the methods.

The version of my paper presented *viva voce* in Ann Arbor contained a discussion of Libet's data from medial lemniscus (LM) stimulation, and of why, in my view, it did not lend significant support to the retroactive

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referral hypothesis. Since I had provided substantial objections to his hypothesis quite apart from these criticisms, I judged that the final version of the paper would not be appreciably strengthened by their inclusion, and that, on the other hand, the paper might be more readable without them. However, in his reply to my published criticism, Libet emphasizes the LM data (his reply, p. 186ff., and p. 192) and accordingly, it behooves me to present my reservations now.

Libet explains that the backwards referral hypothesis makes a "startling prediction" and that the prediction is borne out, therewith confirming the hypothesis. (his reply, p. 187; Libet et al. 1979) The prediction is this: if the skin and the LM are stimulated at the same time, the subject will report them as felt together. This should happen, says the hypothesis, because (1) the LM had been found to require a 500msec. pulse train before neuronal adequacy for the conscious sensation is achieved, (2) skin stimuli are alleged to have a similar period of neuronal adequacy, and (3) the wave forms resulting from LM and skin stimulation are sufficiently similar to suggest that both are referred backwards in time. The prediction is thought to be startling inasmuch as it is unlikely that the prediction will be true unless both sensations are referred backwards in time. As described, the prediction is rather risky, but as actually tested, it was not, and indeed, as I shall show, the results obtained are more straightforwardly explained on the commonplace hypothesis that skin stimuli have a neuronal activation period of roughly 100msec., and neither the LM nor the skin sensation is referred backwards in time.

Two points are important in determining how startling the results were. First, the test was conducted not with the stimulus intensity for a 500msec. pulse train, but with an intensity such that a *200*msec. pulse train would produce the sensation. Technical reasons are cited for this adjustment. (his reply, p. 187) Now the choice of stimulus intensity makes a considerable difference to the interpretation of the data, for, and this is the second point, a close look at the data shows subjects to be highly inaccurate in temporal ordering of sensations when the stimuli are separated only by some 100msec., and this is so even when both stimuli are cutaneous. (Libet et al. 1979, p. 210) At 100msec. ISI subjects typically report the sensations as felt together. This implies that the subjects are poor at distinguishing sensations separated by only 100msec. intervals, and consequently, that reports of simultaneity do not entail simultaneity. As to the test then, assuming with Libet that the LM sensation starts roughly 200msec. after the stimulus onset, how significant is it that when the LM and skin stimuli are presented together, they are reported as felt together? It certainly cannot be inferred that the sensations were felt at exactly the same time—but at most, that they were felt somewhere within 100msec. or so of each other. Is it possible then that the LM and skin sensations were separated by 100msec. or so, and neither was referred backwards in time? Indeed it is. Libet's interpretation of the data assumes that skin stimuli have a long neuronal activation period, yet there is no sound evidence that this is so. (Churchland 1981, pp. 169–76, and see below) Other data (Churchland 1981; and appendix this article) suggests a much shorter neuronal activation period. On the hypothesis that skin stimuli have a neuronal activation period of some 50–150msec., then simultaneously presented LM and skin stimuli are *likely* to be reported as felt together—without any backwards referral of sensations. On this hypothesis, the skin sensation would be produced first, at about the 100msec. mark, and the LM sensation produced later, at about the 200msec. mark; the two sensations are separated only by 100msec., and intervals of that magnitude tend to go undetected, hence the reported simultaneity. This is the simpler and less exotic explanation of Libet's results with LM stimulation. If he *had* run the test with 500msec. intensities, and *had* got similar results, that might have been startling. But he did not run that test, and we do not know what the results would have been if he had.

One final remark on the LM results. Even if it be supposed that the sensations arising from the simultaneous skin and LM sensations are felt at exactly the same time, the delay in neuronal adequacy for skin stimuli may well be an artifact of the setup. That is, it is entirely possible that direct electrical stimulation of the LM gums things up slightly for incoming signals from the periphery, so that when the skin and LM are stimulated together, there is abnormal delay for the skin stimulus. In sum, close examination of the LM experiments reveals that they cannot be accorded the significance Libet claims, and they do not provide the confirmation of the backwards referral hypothesis which Libet seeks.

The question of the delay between skin stimulus and neuronal adequacy for awareness of the sensation is nettlesome. In making his case for backwards referral, Libet adopts 500msec. as the delay, though he readily admits (Libet 1973) that there is considerable variation between subjects, with some subjects giving a much smaller figure. The ordering test has a major design flaw because it assumes the 500msec. figures for all subjects, but to be significant, the test should have a within-subjects design. That is, in setting up the ordering test, the relevant period of neuronal activation for skin stimuli must be the one established *for the individual being tested*—not an average, or the figure for the slowest, or the figure for the fastest subject. Otherwise the results from the ordering test are meaningless.

Further to this point is the matter of adding 100msec. to the ISI's in the masking test. (See Libet's reply, p. 190) I had noted with concern (1981, p. 174) that though Libet adopts 500msec. as the standard latency for skin stimuli, in fact he had just one subject who got masking effects at 500msec. ISI. Libet acknowledges that this is true, but complains that it is a ''distasteful distortion'' (his reply, p. 190) because 100msec. should be added to the ISI's of the 'faster' subjects, and that when this ''correction'' is made, it is acceptable to claim 500msec. as the standard latency for skin stimuli. So far as I can determine, the argument for this adjustment is not presented in Libet's published work, but in any case, he did indeed tell me to make the adjustment in our exchange of letters. While willing to oblige, I finally could not find his reasons persuasive.

The first and simpler point is this. For the masking tests, Libet wants to discount the first 100msec. in the pulse train of the second stimulus (cortical), thereby extending the actual ISI to a longer, ''effective'' ISI. For example, the actual interval between the first stimulus (skin) and the second stimulus (cortical) may be only 200msec., but it gets extended by the adjustment to 300msec. But the first 100msec. of the cortical stimulus cannot just be discounted — they are certainly necessary for masking the skin stimulus, even if, on their own, they are not sufficient. It is not as though nothing is happening during the first 100msec. of cortical stimulation; the first 100msec. of stimulation is part of the stimulation, not part of the inter-stimulus interval.

The second and related point is that his rationale for adding 100msec. to the ISI's assumes that the masking by cortical stimuli is not a short-term memory-attention effect, and my considered view is that we do not know anything like enough about the matter to make this assumption. Libet rejects the STM-attention hypothesis on grounds that he gets retroactive enhancement by cerebral stimulation (Libet's reply, p. 190ff., and also 1978) and he claims it is ''obvious'' *that* cannot be an STM-attention effect. Reason counsels otherwise. First, the enhancement effect was obtained in subjects in whom masking could not be obtained (Libet 1978, p. 73), and it was apparently achieved by stimulating a different part of the cortex than that stimulated for masking. So it could well be, for all we know now, that masking by cortical stimulation is an STM-effect, while enhancement is not, and in any event, given the differences in experimental setup, we certainly cannot assume that a similar explanation underlies both effects. Moreover, there is no reason in principle why enhancement cannot be an STM-attention effect. Recall that subjects were reporting after the fact, and it may be that their recollection of the comparative vividness of sensations was affected by the intervening cortical stimulation. It may be obvious that enhancement is not an *amnesic* effect, but amnesic effects are not the only effects of short term memory and attention.

Accordingly, these reasons inveighed against acceding to Libet's instruction that 100msec. should be added to the masking ISI's, and thus what Libet sees as a "distasteful distortion" is finally a substantive disagreement between us concerning the adequacy of the justification for adding 100msec. Lastly, it should perhaps be asked whether adding 100msec. to the masking ISI's is *enough* to yield 500msec. as the acceptable figure? Since Libet has not published data tables for the masking tests, no precise answer can be given. However, insofar as he admits most subjects gave an actual figure of 200msec. (his reply, p. 185 and 1978, p. 72) adding 100msec. still gives most subjects only 300msec., substantially in arrears of the claimed 500msec.

In conclusion, the hypothesis that there is standardly a backwards referral in time of conscious experiences remains for me an infirm and unconfirmed hypothesis. My original objections have not been met in Libet's reply; e.g., there is no sound evidence for the claim that the neuronal activation period for skin stimuli is 500msec., and in the ordering test it is undetermined when the cortically induced sensation begins. Besides the serious design flaw in the ordering test, there are too many contestable and problematic assumptions, too many thin-spun inferences, too many long-reach and tenuous interpretations of the data to make Libet's case convincing. Some of the disagreement between Libet and me might be resolved by further experiments. However, as argued at length (Churchland 1981), even if the hypothesis should be confirmed, I see no especial problem for physicalism.[1]

APPENDIX

Table 1. Mean verbal reaction time to electrical pulses given to the skin on the back of the hand. (s = 9)

Shock Level	*R. Time (msec.)*	*Stan. Dev.*
Very faint	578	265
Faint but definite	358	113
Strong	303	50

We used nine right-handed subjects, seven males, two females. We attached electrodes to the skin on the back of the right hand, and the stimulus consisted of brief electrical shocks. By means of shock work-up, we determined for each subject a voltage at which the sensation was faint but the subject was not uncertain about whether or not he felt it. Judging from Libet's discussion of his methods (Libet 1973, 1978, Libet et al. 1979) this intensity is comparable to what he used. Nonetheless, for comparative purposes, we included two additional levels of intensity, one slightly lower intensity at which the sensation was very faint and the subject sometimes failed to detect its presence, and one at a level higher than the first but short of being painful. The three levels we called, respectively,

[1]While neither my paper nor my reply is the appropriate place for a thorough discussion of the significance of introspection for the mind-body problem, perhaps some bibliographical suggestions might be useful for those who, like Libet, wish to pursue the question. See, e.g., Armstrong (1968), P.M. Churchland (1979) and (forthcoming), Dennett (1978) and (1979).

"faint but definite", "very faint", and "strong". The stimuli were delivered in groups of ten for any given intensity, and were presented at randomly timed intervals. The order of presentation of intensity-type was varied across the subjects. The subject was instructed to say "go" as soon as he felt the sensation, and was allowed one practice trial at each intensity. The vocal response time was picked up by a microphone placed close to the subject's mouth and was recorded by the computer which then calculated the time. On our data, Tukey's test indicates that the difference in reaction time between 'faint but definite' and 'strong' stimuli is clearly not significant, though the difference between these and 'very faint' is substantial ($P < .01$) This suggests that the longer response time for very faint stimuli may be due to uncertainty and hesitation on the part of the subject.

REFERENCES

Armstrong, D. M. (1968), *A Materialist Theory of The Mind.* London: Routledge and Kegan Paul.

Churchland, Patricia Smith (1981), "On the alleged backwards referral of experiences and its relevance to the mind-body problem". *Philosophy of Science 48*: 165–81.

Churchland, Paul M. (1979), *Scientific Realism and the Plasticity of Mind.* Cambridge: Cambridge University Press.

Churchland, Paul M. (forthcoming), "Introspective knowledge and the mind-body problem", in a volume edited by J. Hick and C. Hookway.

Dennett, Daniel C. (1978), *Brainstorms.* Vermont: Bradford Books.

Dennett, Daniel C. (1979), "On the absence of phenomenology", in *Body, Mind and Method.* Edited by D. F. Gustafson and B. L. Tapscott.

Libet, Benjamin (1973), "Electrical stimulation of cortex in human subjects and conscious sensory reports" in *Handbook of Sensory Physiology,* Vol. II. Edited by A. Iggo. Berlin: Springer-Verlag: 743–790.

Libet, Benjamin (1978), "Neuronal vs. subjective timing, for a conscious sensory experience", in *Cerebral Correlates of Conscious Experience.* Edited by P. A. Buser and A. Rougeul-Buser. Amsterdam: Elsevier/North Holland: 69–82.

Libet, Benjamin (1981), "The experimental evidence for subjective referral of a sensory experience backwards in time: reply to P. S. Churchland", *Philosophy of Science 48*: 182–97.

Libet, Benjamin, Wright, E. W. Jr., Feinstein, B., and Pearl, D. K. (1979), "Subjective referral of the timing for a conscious sensory experience: a functional role for the somatosensory specific projection system in man", *Brain*, 102: 191–222.