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ON THE ALLEGED BACKWARDS REFERRAL OF EXPERIENCES AND ITS RELEVANCE TO THE MIND-BODY PROBLEM*

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A remarkable hypothesis has recently been advanced by Libet and promoted by Eccles which claims that there is standardly a backwards referral of conscious experiences in time, and that this constitutes empirical evidence for the failure of identity of brain states and mental states. Libet's neurophysiological data are critically examined and are found insufficient to support the hypothesis. Additionally, it is argued that even if there is a temporal displacement phenomenon to be explained, a neurophysiological explanation is most likely.

There are many ways of tricking one's nervous system such that false perceptual judgments are made about the perceived world. Children know how to monkey with their vestibular system and make the world spin after they have stopped spinning, and to monkey with their stereoscopic apparatus to make floating sausages appear between their fingers.[1] More sophisticated and cunning trickery is practised by psychologists attempt-

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[1]This is fun and easy to do. Bring the index fingers in front of the face about a foot or so, at eye level, so that the tips are pointing at each other but are separated by about an inch. Then focus straight past the fingers on a wall or something distant. The 'sausage' will appear between the fingers, and gradually you will be able to focus on it.

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ing to pry into the manifold secrets of the brain, by getting it to reveal something of itself in illusion-producing conditions. In saying that we fool the nervous system, I am not in the least demeaning the method, for inspired trickery has been wonderfully illuminating. Thus, to take but two from a vast array of examples, Kolers' (1972) work on illusory motion, and Gregory's (1977) work on spatial illusions, have been revealing of the complexity of processing in the visual system. Now it may be argued that some illusory experiences are of such a nature that they provide clues indicating that the phenomenon could not be owed to the nervous system *at all,* but rather must be seen to be the result of operations of a non-physical system, where the mind may be thought to be one such system. This is a stout claim indeed, and this is the claim Libet tenders as a result of his investigations in the somatosensory system. Whilst Libet's own conclusions are relatively guarded, in that he argues that his results may cause problems for the identity of mental states and brain states, Eccles (1977), on the other hand, sees Libet's work as major empirical support for mind-brain dualism. Eccles' enthusiasm for Libet's hypothesis regarding retroactive dating is revealed in the following remarks, taken from *The Self and Its Brain:*

> This antedating procedure does not seem to be explicable by any neurophysiological process. Presumably it is a strategy that has been learnt by the self conscious mind . . . the antedating sensory experience is attributable to the ability of the self-conscious mind to make slight temporal adjustments, i.e., to play tricks with time. (Eccles and Popper 1977, p.364)

These are surely quite remarkable claims to make for certain empirical results, and it was with great curiosity that I turned to the details of Libet's work to find what had so moved Eccles.

I

Because the data are quite complex, I shall provide first a thumbnail sketch of Libet's case, and where the finer points become relevant, I shall introduce them. Here is the sketch: suppose that for a certain type of stimulus, namely direct electrical stimulation of the cortex[2], it is known that up to 500 milliseconds worth of neuronal activity must take place before the subject feels the sensation. Suppose additionally, that the same thing is known about a stimulus applied to the skin on the back of the

[2]The area stimulated is the postcentral gyrus (somatosensory cortex) in the area receiving projections from the hand. The intensities are liminal. See Libet (1973), p. 762 and Libet et al. (1979).

hand. However, when the stimuli are *presented* in the order 'direct cortical stimulus followed by skin stimulus' (with an interval of about 200 msec.) they are sometimes *reported as felt* in the reverse order, namely, 'skin stimulus followed by direct cortical stimulus'. (Hereafter, call this experiment "the ordering test". (see Figure 1; modified from Libet 1979)) Given these premises, Libet apparently takes the following hypotheses to be reasonable: (Libet et al. 1979, p. 221, Libet 1978, pp. 80–81)

(1) There is a temporal displacement of one sensation such that it appears earlier in the order of experienced events than it should.
(2) The skin sensation is felt earlier than the brain states necessary for its production.
(3) Subjective time is different from physical time.
(4) Mental states cannot be identical to physical states.

Not all hypotheses are advanced with equal conviction, and I must acknowledge the possibility that Libet may wish to describe his conjectures somewhat differently. My uncertainty here stems from uncertain interpretation of the centrepiece hypothesis, the one he calls the retroactive dating hypothesis, and has otherwise expressed thus:

> . . . there is an automatic subjective referral of conscious experience backwards in time . . . after the delayed neuronal adequacy has been achieved. (Libet 1978, p. 75)

(See also Libet et al. 1979, pp. 217–222). The only reading of this hypothesis which could conceivably connect it to dualism, as he wishes to do, requires (2) and/or (3).

The timing of events is of the essence for Libet's case. The important evidence concerns the duration of certain processes in the brain before sensations intrude into awareness, and the order in which sensations are reported as felt. Before addressing the case itself, I want to draw attention to the fact that the timing of the experienced sensation is not established on the basis of the subject's immediate response, such as pressing a button or saying "go" as soon as he feels a sensation. Libet (1973, pp. 755–6; Libet et al. 1979, p. 193) considers it a virtue that he eschews all such data. The reason given is that it is possible that the subject's response may occur in advance of the awareness. He therefore chooses to rely exclusively on the subject's reports of the relative order of sensations given after the conclusion of the trial. The delay between trial end and subsequent report is apparently on the order of several seconds. The exclusion of immediate reporting data does not, I think, lend rigor to the methods; indeed, it bespeaks a rather question-begging selectivity of the

Fig. 1

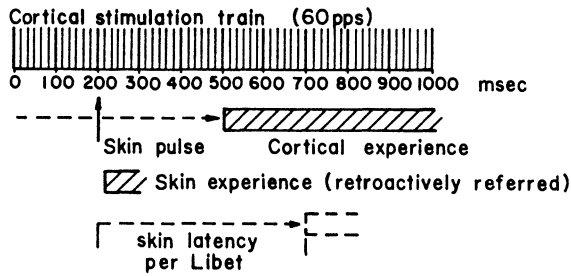


Fig. 2

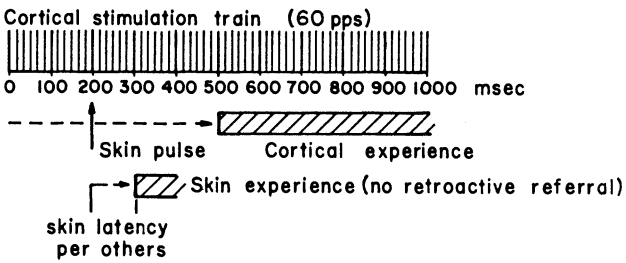
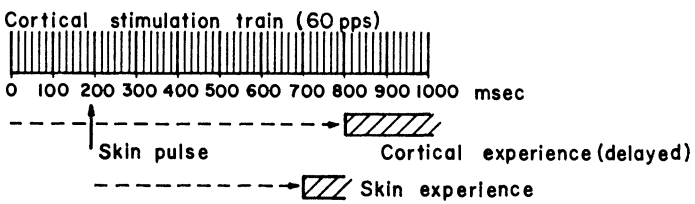


Fig. 3



Ordering Test:

Stimuli: skin pulse administered 200 msec after pulse train to cortex is begun.

Reports: after trial end, some subjects report that the skin sensation was felt before the cortical sensation.

data. If the immediate reports should conflict with the retroactive dating hypothesis, this may be because, as Libet suggests, the response is in advance of the experience. Equally, it may be because the hypothesis is false. (See below, p. 174ff.) In any event, there is not the slightest reason for thinking that the verbal response may antedate the awareness.[3] It will become evident in the ensuing discussion that the exclusive reliance on

[3]Libet cites the work of Fehrer and Biederman (1962) and Fehrer and Raab (1962) as the basis for rejecting immediate response data. A reading of these papers reveals, however, that there is nothing in their results which impugns verbal response data in the way Libet suggests.

after-the-trial reporting is an error in the methods, depriving us of useful data.

My criticism of Libet's thesis essentially falls into two parts: (1) the evidence that there is a temporal misordering of experiences is entirely inadequate, and (2) even if there is a temporal misordering of experiences in the ordering test, this provides no evidence for a non-material mind. The first criticism is quite technical, in that it addresses some of the finer points of Libet's experiments. The second is more general, but less technical.

II

According to Libet, in the ordering test there is a temporal displacement of experiences, in that the skin sensation is felt before it should be. This of course implies that we know that the skin sensation should not have been felt before the direct cortical sensation, and I am critical of the premises which bid us so assume. Those premises say that (a) it takes some 500msec. worth of neuronal activity before the threshold skin stimulus is felt, and (b) the neuronal activation time for cortically induced sensations is also about 500msec.[4] (Libet 1978, p. 75) The arithmetic in Libet's argument is crucial, for if the cortical latency should be *longer* than postulated, or if the skin latency should be *shorter,* then the temporal displacement phenomenon becomes a mere figment of miscalculation, for in that event, the sensations would be felt in the correct order in the ordering test. As it happens, both possibilities appear to be alive and well rather than remote and dismissable.

As for the first possibility, Libet (1966, 1978; Libet et al. 1964) establishes the latency for cortically induced sensations by applying pulse trains (at liminal intensity) for varying durations, and querying the subject at the trial end to determine whether he felt a sensation. Libet found that at pulse trains shorter than 500msec. the subject felt nothing, but at pulse trains 500msec. and longer, the subject reported that he had felt a sensation. Notice however, that these results tell us only that at least 500msec. of pulses are needed, not that the sensation is *produced* at the 500msec. mark. The most that can be gleaned from these results is that the sensation is produced *somewhere between the 500msec. mark and the query at the trial end.* It is perfectly compatible with the data that the sensation is produced at, e.g., 800msec., or at 1.5sec. This slack notwithstanding, the latency figure Libet (Libet et al. 1979, p. 199) employs in inferring temporal displacement is the smallest in the range, to wit,

[4]Libet sometimes (Libet et al. 1979, p. 202) uses 200msec. as the latency for both skin and cortical sensations. My arguments apply *mutatis mutandis*.

500msec. Whilst this figure does make the arithmetic come out right for temporal displacement, there is no reason to single it out from the range as established. In view of the imprecision as to when the subject felt the cortically induced sensation—somewhere between 500msec. and trial end—it becomes highly questionable whether Libet can reasonably infer the existence of temporal displacement from the results of the ordering test. Certainly much more work needs to be done to narrow down the interval within which the subject can be assumed to have felt the sensation before the inference can be taken seriously.

The second possibility which threatens to undo Libet's calculations is that the skin latency may be shorter than he reckons. Libet really has just one method[5] to fix the figure for latency of skin stimuli at 500msec. According to this method, a cutaneous stimulus can be masked[6] by a direct cortical stimulus when the latter is begun up to 500msec. after the onset of the cutaneous stimulation. (Libet 1973, pp. 773–5) Therefore, it is concluded, the cutaneous stimulation must require up to 500msec. of neuronal activity for awareness. This is perhaps a *prima facie* case for cutaneous latency, but as I shall show, it is a case beset with such troubles as render it hopelessly weak.

To begin with, the abnormality of direct electrical stimulation of the brain must be factored in, for as Mountcastle (1966) remarks in comparing the latency of direct cortical stimuli with the alleged latency for skin stimuli, ". . . the cortex has a very difficult job weeding out the conscious perception from the abnormal train of events set in motion by the electrical stimulus." The abnormality of such stimulation may perpetrate abnormal activity elsewhere in the brain. Libet (1973, p. 749) notes Penfield's (1958) caution that direct cortical stimulation produces responses with abnormal characteristics, but seems satisfied with the idea that by using very low intensity stimulation, the sensation elicited is somewhat like the sensation elicited by electrical stimulation of the skin. This is unconvincing. For even if the resulting effects are somewhat similar, such similarity may be but the surface manifestation of substantial dissimilarities in the underlying neuronal activity. The fact is, no one knows anything much about what happens when the brain is directly stimulated, about how close or remote the overall effect is from the normal effect.

It is easy to conjecture why the direct cortical stimulation might con-

[5]Libet also discusses evoked potential data in this connection, but the data do not begin to support his claim. For a thorough discussion of this point, see Churchland (unpublished).

[6]This is usually called "retroactive masking" or "backwards masking" in the literature, but that means only that the first stimulus is masked by the second, not that there is backwards referral in time.

trast with peripheral stimulation in requiring an unusually long latency before the stimulus becomes conscious. The reticular core of the brain stem appears to have a most important role in consciousness. As Scheibel and Scheibel (1968) report:

> . . . the system is now known to exert a remarkable range of control upon the neuroaxis and upon the organism as a whole. These functions include: determination of operational modes; gating of all sensory influx; participation at all levels of cortical function, including read out for cortical differentiative and comparative processes; gain manipulation of motor output; multilevel control over most visceral functions; and the active manipulation of a spectrum of states of consciousness from deep coma to maximal vigilance. (p. 261)

A signal entering the system directly from the cortex enters at a highly unusual point, and if, as the Scheibel and Scheibel work suggests, it needs to find a route to the reticular core, that route may well be a devious, circuitous, and ill-trodden path. For peripheral stimulation, on the other hand, the signal will travel up the spinal cord and pass through the reticular core in the usual and established way *en route*.[7]

Apart from these general reservations, specific reasons abound for suspecting abnormality here. To begin with, other work (Halliday and Mingay (1961), and Melzack and Wall (1963)) involving pairs of cutaneous stimuli puts the maximum inter-stimulus interval (ISI) for retroactive masking at about *50–100msec*. Now Libet appears not to have balanced his skin-cortical masking data with data from skin-skin masking, and I would have thought this necessary to underwrite his confidence that the 500msec. figure is accurate. He explains (Libet 1973, pp. 773–4) his decision by noting that masking may be of two types, peripheral and central, and that by using direct stimulation as a mask, he can avoid the complications of peripheral masking. This will not do. As Turvey (1973) points out in his work on masking in the visual system, peripheral masking can be avoided by sending the signals to distinct hemispheres. Whilst this is a fussy business in the case of the visual system, it is straightforward in the somatosensory system. Simply stimulate contralateral hands.[8] In fact, the 50–100msec. figure of Halliday and Mingay (1961) and Melzack and Wall (1963) was obtained in just this way, and a much shorter ISI (5–10msec.) is obtained for ipsilateral masking.[9] (Schmid 1961) Now

[7]See also Dixon (1971) who argues that in order for awareness to occur there must be activation of the non-specific reticular system.

[8]As Gazzaniga and LeDoux remark (1978), there is homolateral representation of some parts of the limbs, but not the hands.

[9]The longer ISI for masking in contralateral stimulation suggests central masking, and the shorter ISI is more plausibly linked to peripheral masking.

the beauty of using the stratagem of stimulating contralaterally at the periphery is that it avoids the use of a stimulus which likely causes eccentric and unpredictable interference. The cutaneous stimulation and the direct cortical stimulation may be as different as chalk and cheese in their *overall* effects on the nervous system.

Furthermore, in the skin-cortex pairs, unlike the skin-skin pairs, masking appears to be order relative, for backwards masking does not seem to occur when the stimuli are presented in the reverse order; i.e., cortex-skin. Instead, that is when we allegedly get temporal displacement. *Prima facie,* this order-relative feature is an oddity which betokens the abnormality of the stimulation method. Clearly it is possible that the alleged masking may not in fact be masking in the usual sense at all, but rather blanking in short term memory. It should be remembered that the subject is not reporting until after trial end, and no attempt was made to see if the subject could initiate an immediate report before the 500msec. had elapsed. The suggestion that the alleged masking may really be owed to interference with memory or attention is a pregnant one. There are believed to be step-wise connections from the primary and secondary cortex to the hippocampus (Van Hoesen, Pandya and Butters (1972), and O'Keefe and Nadel (1978)), and certain limbic structures, particularly the hippocampus, have been implicated in short term memory and attention. (O'Keefe and Nadel (1978)). The pattern of activity induced in the cortex by means of electrical stimulation is not identical to that induced in the cortex by cutaneously applied stimulation, and, as Brodal (1969) observes, "[i]t is generally agreed that the hippocampal responses are complex, labile, and easily modified by various factors . . ." (p. 258) Now I hasten to say that these remarks on the hippocampus certainly do not add up to anything like a proof of interference with hippocampal function when the cortex is directly stimulated, and indeed the many outstanding puzzles about hippocampal function impede breezy conviction. Nevertheless, what remains after all the hedges and caveats is a suggestion which serves to illustrate my point that it is necessary to weigh the skin-cortex masking data against the skin-skin masking data.[10,11]

[10]See also Olton, Becker and Handelmann (1979) who say: "Thus fimbria-fornix damage may interfere with the ability to maintain the temporal order of stimuli accurately in memory." (p. 318)

[11]Libet's reply (1978 and at the symposium) to the criticism that his masking data may be queered by the abnormality of the stimulation method is to point out that retroactive enhancement of skin sensations at ISI's of 200–500msec. is obtainable when stimulating a *different* part of the cortex. He described his set-up as follows:

> This type of conditioning effect was demonstrable when the test consisted of two separate pulses applied to the same site on the skin but delivered about five seconds apart, and the subject asked to compare their subjective intensities. When the two

A distinct line on the time taken for a sensation to become conscious would of course be to determine the verbal response time to a skin stimulus. This would not tell us exactly when the awareness began, because mobilization of the verbal system and of the motor neurons to activate the muscles to provide the verbal response would account for a substantial proportion of the response time. Nonetheless, the verbal response time data would be valuable, for they would put an upper limit on the time required for the stimulus to reach consciousness, and hence if the *response time* was 500msec. or less, it would be reasonable to expect that the *time taken to reach consciousness* was far less. As I noted earlier, (pp. 167–68) Libet decided against such data, and since I could not find the relevant data in the psychological literature, I sought the help of an experimental psychologist, David Martin, and together we undertook to measure verbal response time to skin stimuli.[12]

We stimulated the skin on the back of the hand with electrical pulses, adjusting the intensity such that the sensations were faint but the subject was not uncertain about whether or not he felt the stimulus. We asked the subject to say "go" as soon as he was aware of the skin sensation. The *mean response time across nine subjects was 358.22msec.* Evidently this figure is well below the 500msec. Libet claims it takes for the stimulus to reach consciousness, and it should be emphasized that this includes not only the time it takes for the stimulus to become conscious, but also the time taken in the production of the verbal response. A conservative guess at the proportion of the 358msec. consumed by the mobilization of the verbal response is placed at about one-half. This puts the time for neuronal activation for conscious awareness of skin stimuli far

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> delivered skin pulses were electrically equal or unequal (by about 10%), subjects reported them respectively to be subjectively equal or unequal (in the appropriate direction). However, if a conditioning cortical stimulus was begun at any time up to 200–500msec. following the second of two equal skin pulses, some subjects consistently reported that the second test pulse to the skin felt distinctly stronger than the first. (These same subjects never exhibited retroactive inhibition by the conditioning cortical stimulus. . . .). (1978, p. 73)

Again, there are no data tables presented, and no indication of how many subjects "some" are, nor how many reported the effect at which ISI's and on how many trials, nor how many false reports were recorded. In answer to my queries as to the precise number of subjects, trials and ISI's, Libet (in correspondence) said only that he recalled there were three or four subjects, and the trials and intervals were roughly similar to those used in cases where retroactive masking was obtained. Does this mean he had but one subject who got the effect at ISI's of 500msec.? It is evident that this is a very slender thread on which to hang the defense of the stimulation method for the masking results, and it is entirely insufficient to justify exclusion of skin-skin masking data and verbal response data.

[12]For a complete account of the methods and results, see Churchland and Martin (unpublished).

and away below Libet's figure of 500msec.[13]

The foregoing objections to Libet's claim that threshold skin stimuli standardly require 500msec. of neuronal activation before they become conscious are, I think, decisive, but as a final and telling point, it must be mentioned that there was in fact only *one* subject for whom Libet got backwards masking (skin-cortex) at 500msec. (Libet 1973, p. 774, and Libet et al. 1972, p. 161) Nor is it clear that there was a significant number of trials for this one subject, for on that question Libet has replied (in correspondence) only that his "recollection was that this was repeated more than once." Although not wishing to sermonize on methodological matters, I should be remiss if I failed to remark on the hazard of concluding much on the basis of *one* subject and a few trials. As far as the remaining subjects are concerned, it is not clear what the figures are on skin latency, because *nowhere in the presentation of his hypothesis does Libet provide data tables on skin-cortex masking*. We simply do not know for how many subjects, for what ISI's and on how many trials Libet got results. From some of Libet's general remarks, it is evident that most subjects gave figures of *200msec. or less* in skin-cortex masking tests. (Libet 1978, p. 72)

Now the reason why the milliseconds arithmetic matters so much is this: if the normal latency for cutaneous stimuli is on the order of 50–200msec. (indeed, if it is less than *300msec.*) then in the crucial ordering test, *the stimuli are felt in the order they should be;* i.e., the skin stimulus before the cortical stimulus. Hence even on Libet's own figure of 200msec. or less which he allows he got for most subjects, the skin stimulus should be felt before the cortical stimulus in the ordering test. In that event, there is no temporal displacement, and the case against physicalism breaks down at the very first step. Now I am not here asserting that there is no temporal displacement, but only that given the palpable reservations regarding Libet's hypothesis for normal latency for skin stimuli, *it is very much an open question whether there is any temporal displacement at all*. Compare Figures 1 and 2. Figure 1 is Libet's representation of the events in the ordering test, based on the assumption that the latency for the skin stimuli is about 500msec. and the skin sensation is retroactively referred. Figure 2 is based on the different assumption that the latency for the skin stimulus is roughly 50–100msec.

The question of the neuronal activation period for skin stimuli is even

[13]At the symposium, Libet dismissed the data on grounds that the verbal responses can come in advance of the conscious experience, though it will not *seem* so to the subject because the conscious experience will be referred backwards in time. Whilst this possibility is conceivable, it remains a *mere* possibility because there is no evidence for it, and in the absence of evidence, it is a question-begging rescue of the hypothesis to say that verbal response data are queered by retroactive referral. (See also footnote 3.)

more pressing when we turn from Libet's general description of his procedure in the ordering test to an examination of his results from those tests. For here, at any rate, data tables are presented, but it is plain that the data tables from the ordering tests are completely meaningless in the absence of the specification of the skin latency for the particular subject tested, and utterly misleading if 500msec. skin latency is assumed for all and sundry. (See above, p. 174) Consider, for example, the subject C.J. The intensity of the cortical stimulus was set such that the subject should feel the cortical sensation at 300msec. *or later*. Here are the results for C.J.:

	ISI	No. of trials	*S* first	*T*	*C* first
	200	9	5	4	0
Cortex-Skin	0	7	5	2	0
	−200	9	9	0	0

Now if C.J. is assumed to have a skin latency of 500msec., these data might be taken to show a temporal displacement. The question therefore is this: what is the skin latency for C.J.? Even the results from the flawed skin-cortex masking test would be better than nothing, yet nothing is provided. Despite the fact that even on his own masking results, 500msec. is extravagant for most subjects, Libet makes it clear that he has determined to go with that figure for all subjects (Libet et al. 1979, p. 199). Notice, however, that on a more modest estimate, even as large as 100–200msec., these results would not give the least support to a temporal displacement hypothesis. Indeed, they are pretty much what one would expect. Moreover, a certain amount of inaccuracy is bound to show up, especially given the fuzzy onset of cerebrally induced stimuli (Libet et al. 1979, p. 197), the abnormality of events so initiated, and the faintness of the sensations. Some misordering by subjects can be confidently predicted, particularly when the ISI is under 200msec.

To add to the difficulties with Libet's data, the number of his subjects is very small. The data tables for the ordering test (Libet et al. 1979, pp. 214–5) list six subjects, but Libet himself discounts three. Of these three, two are rejected because the subjects were cortically stimulated with anodal rather than cathodal current, and these subjects did not give results which could be interpreted as showing retroactive referral. (Libet et al. 1979, pp. 216–7) They do exhibit some inaccuracy, but not the kind which is biased in favor of the skin stimulus regularly occurring before it should. Libet does undertake (Libet et al. 1979, p. 217) to justify the exclusions on grounds that the anodal current excites deeper layers of the cortex and thereby excites neurons involved with retroactive timing. This is unacceptable. Since the data under consideration are the data on which one is to determine *whether or not there is a temporal displacement phe-*

nomenon at all, Libet's exclusion of the untoward results unfortunately has all the earmarks of begging the question. His explanation of why anodal and cathodal current should give different sorts of inaccuracies, even supposing there is a temporal displacement unique to cathodal stimulation, is sheer speculation.

On the other hand, giving Libet the exclusion he desires, this does shrink his data base to a mere four. Of these four, Libet rejects a further one because for this subject the cortical stimulus and the skin stimulus involved the same hemisphere. And then there were three. One of these, M.T. was stimulated not on the hand, but on the face, and Libet gives no latency data whatsoever for facial stimulation. Including M.T. nonetheless, his data like that of C.J. are readily explicable without resort to a temporal displacement hypothesis, if given a skin latency even as large as 100–150msec. Only one subject, J.W. has results which are not easily accounted for in this straightforward way. Yet even the case of J.W. is unimpressive. If he has a latency for skin stimuli of 100–125msec., then his results are unexceptional, save for one condition. That is where the onsets of the two sensations should be separated by about 200msec., and on 11 trials J.W. 8 times reports the sensations as felt together. This is rather thin gruel for so grand an hypothesis as retroactive referral. As remarked earlier, inaccuracies in ordering judgments, even on the part of normal subjects, are plentiful, and one subject can have an eccentric run of judgments. I discovered this myself in testing graduate students with pairs of skin stimuli, and it is evident too from Libet's data tables for pairs of skin stimuli (1979, p. 210). Additionally, against a too ready willingness to assume temporal displacement on the basis of results from one subject in one condition, it is pertinent to point out that Libet's subjects were neurological patients, the tests were made during surgical procedure (under local anaesthetic) and, as Libet acknowledges (Libet et al. 1979, p. 198) the subjects appeared to weary easily in the task of attending to batches of faint stimuli.

The findings of this section are simply put: Libet's evidence for the hypothesis that there is standardly a temporal displacement of conscious experiences is exceedingly vapid. Until Libet specifies skin latency for each individual, his results from the ordering test are entirely inconclusive, and unless he determines that magnitude in a more comprehensive fashion than hitherto, his hypothesis is mere guesswork.

III

Nevertheless, in the second stage of my analysis, I wish to put aside these reservations. Rather, let us suppose that the temporal displacement hypothesis is correct, and that the skin sensation should have occurred

after the sensation induced by direct cortical stimulation. Granting all that, can we go further and say that the skin sensation is felt earlier than the brain states necessary for its production; that the sensation is referred backwards in time? I think not. The ordering test and the latency assumptions are also compatible with a quite different, less spectacular explanation; to wit, there is a *delay* in the works, such that the cortical sensation is felt *later* than predicted. Rather than suppose one sensation is referred backwards in time, explain the phenomenon by saying that one stimulus is put on hold, so to speak, until it can be 'admitted' to consciousness. I shall dub this the ''postponement hypothesis'', and it immediately suggests itself because it is simpler than the retroactive hypothesis, and also because, quite apart from Libet's research, there is evidence of delaying operations in perception, particularly in the visual system. (Turvey 1973, pp. 37–9) (See Figure 3 for the diagram representing the postponement hypothesis in the ordering test, and compare it to Libet's representation in Figure 1 based on the retroactive hypothesis.) The two hypotheses differ essentially on just when the respective sensations were felt, but in the absence of any immediate reporting data that might bear on the question, the postponement hypothesis is certainly a most reasonable contender. Indeed, it must be said that an hypothesis which sees the cause happening before the effect is, *ceteris paribus,* preferable to one which sees the effect happening before the cause. The retroactive hypothesis invites circumspection because it needlessly ushers in quite daunting complexity. Consider that the primary evoked response is taken to signal the unconscious receipt of information from the periphery, and precedes the neuronal activation time necessary for awareness. Hence the evoked potential wave precedes the awareness, but additionally it is alleged to provide the ''time marker'' for the sensation to be referred back to. Thus *the awareness is simultaneous with what precedes it;* an event *A* both precedes and is simultaneous with an event *B*. The postponement hypothesis is, by comparison, simplicity itself, requiring nothing more elaborate or esoteric than the delay of a neuronal effect.

Still, it might be argued, even on the postponement hypothesis, there is a temporal displacement phenomenon which has to be explained, since the order of sensations is not the order of stimulation onsets. According to this view, we do not yet have any idea how the brain could accomplish such a feat, so the explanation must lie elsewhere, in the machinations of a non-physical mind. Thus we find Eccles averring that it is the mind which plays tricks with time. The argument is a spectacular *non sequitur*. True enough, there is indeed a phenomenon to be explained, but unexplained events are never evidence *for* anything; they are merely unexplained events. Moreover, it is worth pointing out that to suppose the

mind does the temporal re-ordering is not, anyhow, to provide an explanation, any more than it would be to say, baldly, that the brain does it. To say the mind does it is at best a negative characterization of the sort of explanation one is looking for; specifically, one is *not* looking for a neurophysiological one. No one has the slightest *positive* idea of what kind of explanation is being sought.

Perhaps it is thought that because the illusion is a temporal one, it is therefore special to the extent that it cannot be explained by the affairs of something purely physical. Eccles' remarks concerning the mind's ability to play tricks with time engender such a conjecture. But intriguing as temporal illusions are, there is no reason to suppose there is something preternatural about them, and certainly there is nothing which distinguishes them from spatial illusions or motion illusions as uniquely bearing the benchmark of a non-physical origin. The idea that there are physical mechanisms controlling access to awareness of items, and that such mechanisms may operate on the basis of complex priority principles, ordering items according to a complex function, is not in the least farfetched. A computer can be programmed with a time-sharing algorithm which determines output order. To a naive observer, not knowing the program, the computer's order of output may prompt amazement, and the suspicion that something non-physical in the computer is "playing tricks with time". Nor can I discern anything in Libet's particular temporal illusion which singles it out for non-physical explanation, unless it is the claim that one sensation is felt before the neuronal conditions causing it. But as I have shown, this claim is totally unsupported by the evidence.

Although spatial illusions have received more intensive investigation than temporal illusions, the existence of the latter has been known for a long time. In the *Principles,* William James notes that anticipation can affect the order of perceptions, and thus the nervous surgeon may see blood before he sees the lancet cut, and the blacksmith will occasionally see sparks before he sees the hammer strike. Anticipatory errors were the bane of early astronomers bent on precisely timing the transit of a planet across the mid-line of a telescope. One man's heralding the mid-way point was often many milliseconds in advance of another man's. More recently, Kolers (1966) reports that subjects to whom the letters *a, b, c, d, e, f,* had been serially flashed would report the experienced order as, for example *b, a, d, c, e, f.* Divenyi and Hirsh (1975) found a temporal mis-ordering of sounds. Alice Healy (1974) has also provided evidence that temporal mis-orderings occur even when there is no confusion concerning *what* items were presented. In their studies on speech perception, Fodor, Bever, and Garrett (1974, ch. 3) found that when low information noises—clicks—were presented simultaneously with high information

signals such as speech, the *heard* position of the click was in advance of its *presented* position. I do not know of any comparable studies for the somatosensory system, but it would be worth finding out whether order illusions can be produced by stimulating at the periphery. In none of the aforementioned cases of temporal mis-ordering should we feel compelled to invoke esoteric explanations of the phenomena, foxing though they may be in our present state of profound ignorance. The timing capacities of the brain are certainly not inherently more intractable or mysterious than many other capacities the nervous system enjoys. The absence of spatial "jitter" when watching movies is puzzling. (Kolers (1972)) After all, the action in the movie is seen as continuous, rather than as the series of discrete, discontinuous stills that it is. Does a non-physical mind fill in the gaps? Probably not. But how does it happen? How does the brain do other things, such as store information and learn, how does it coordinate movement and regulate temperature and growth? In all of these questions neuroscience has made exciting progress, but for none do we have the full story. If, in our ignorance and frustration we invoke non-physical mechanisms to explain what we cannot yet explain physically, we stand to thwart satisfaction of our curiosity.

IV

In concluding my discussion of Libet's retroactive timing hypothesis, I want to make two fast observations. First, evidence for a dualist theory of the mind cannot be expected from isolated phenomena so far unexplained. Rather, if the dualist is to make a serious challenge, he must show that a dualist theory is, all things considered, a better theory than what neuroscience and psychology can offer. Accordingly, in order to assess what sort of showing dualism can make in this challenge we need to examine a distinctly dualist theory of the mind. We need to examine its generalizations, to evaluate its research program and its distinctly dualist experimental paradigm. My second observation is that so far as I can tell, there is no theory to examine, there are no counter-explanations, there is no worked out research program nor distinctly dualist experimental paradigm, and no real idea of what dualist explanations would actually look like. At most, dualists have a program-by-contrast, describing what *not* to look for, namely, physical explanations. This is surely a research program by courtesy only.

REFERENCES

Brodal, A. (1969), *Neurological Anatomy, 2nd. Edition*. New York: Oxford University Press.

Churchland, Patricia Smith (forthcoming), "In defense of physicalism" in *Proceedings of the Birmingham Colloquium on the Mind-Brain*. Edited by John Hick and Christopher Hookway. Oxford: Blackwells.

Churchland, Patricia Smith and Martin, David (unpublished), "Retroactive referral of conscious experiences: some counter-evidence".

Divenyi, P. L. and Hirsh, I. J. (1975), "The effect of blanking on the identification of temporal order in three-tone sequences" in *Perception and Psychophysics* 17, 3: 246–252.

Dixon, F. (1971), *Subliminal Perception: The Nature of a Controversy*. London: McGraw-Hill.

Eccles, J. C. and Popper, K. R. (1977), *The Self and Its Brain*. Berlin: Springer-Verlag.

Fehrer, Elizabeth and Biederman, Irving (1962), "A comparison of reaction time and verbal report in the detection of masked stimuli" in *Journal of Experimental Psychology* 64, 2: 126–130.

Fehrer, Elizabeth and Raab, David (1962), "Reaction time to stimuli masked by metacontrast" in *Journal of Experimental Psychology* 63, 2: 143-147.

Fodor, J. A., Bever, T. G. and Garrett, M. F. (1974), *The Psychology of Language*. New York: McGraw-Hill.

Gazzaniga, Michael and LeDoux, Joseph E. (1978), *The Integrated Mind*. New York: Plenum Press.

Gregory, Richard L. (1977), *Eye and Brain, 3rd. Edition*. London: Weidenfield and Nicolson.

Halliday, A. M. and Mingay, Rosemary (1961), "Retroactive raising of a sensory threshold by a contralateral stimulus" in *Quarterly Journal of Experimental Psychology 13:* 1–11.

Healy, Alice F. (1974), "Separating item from order information in short-term memory", *Journal of Verbal Learning and Verbal Behavior 13:* 644–55.

Kolers, Paul A. (1966), "Naming sequentially presented letters and words", *Language and Speech 9:* 84–95.

Kolers, Paul A. (1972), *Aspects of Motion Perception*. London: Pergamon Press.

Libet, Benjamin (1965), "Cortical activation in conscious and unconscious experience", *Perspectives in Biology and Medicine 9:* 77–86.

Libet, Benjamin (1966), "Brain stimulation and the threshold of conscious experience" in *Brain and Conscious Experience*. Edited by J. Eccles. New York: Springer-Verlag: 165–81.

Libet, Benjamin (1973), "Electrical stimulation of the cortex in human subjects and conscious sensory aspects" in *Handbook of Sensory Physiology*. Edited by A. Iggo. Heidelberg: Springer-Verlag, 2: 743–90.

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

Libet, Benjamin, Alberts, W. W., Wright, E. W. Jr., Delattre, L. D., Levin, G. and Feinstein, B. (1964), "Production of threshold levels of conscious sensation by electrical stimulation of human somatosensory cortex" in *Journal of Neurophysiology 27:* 546–578.

Libet, Benjamin, Alberts, W. W., Wright, E. W. Jr., and Feinstein, B. (1972), "Cortical and thalamic activation in conscious sensory experience" in *Neurophysiology Studies in Man*. Edited by G. G. Somjen. Amsterdam: Excerpta Medica: 157–68.

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 in man" in *Brain 102:* 193–224.

Melzack, R. and Wall, P. D. (1963), "Masking and metacontrast phenomena in the skin sensory system" in *Experimental Neurology 8:* 35–46.

Mountcastle, Vernon (1966), comments in discussion of Libet (1966).

O'Keefe, John and Nadel, Lynn (1978), *The Hippocampus as a Cognitive Map*. Oxford: Clarendon Press.

Olton, David S., Becker, James T., and Handelmann, Gail E. (1979), "Hippocampus, space, and memory" in *The Behavioral and Brain Sciences* 2,3: 313–365.

Penfield, Wilder (1958), *The Excitable Cortex in Conscious Man*. Fifth Sherrington Lecture. Liverpool: Liverpool University Press.

Scheibel, M. E. and Scheibel, A. B. (1968), "The brain stem reticular core—an integrative matrix" in *Systems Theory and Biology*. Edited by M. D. Mesarovic, New York: Springer-Verlag.

Schmid, Ethel (1961), "Temporal aspects of cutaneous interaction with two-point electrical stimulation" in *Journal of Experimental Psychology 62:* 400–409.

Turvey, M. T. (1973), "On peripheral and central processes in vision: inferences from information-processing analysis of masking with patterned stimuli" in *Psychological Review* 80, 1: 1–52.

Van Hoesen, G. W., Pandya, D. N. and Butters, N. (1972), "Cortical afferants to the entorhinal cortex of the rhesus monkey" in *Science 175:* 1471–3.